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The impact of lower caseloads in public employment services on the unemployed

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Abstract

In a randomised controlled trial in Austria, lowering caseloads for caseworkers in a Public Employment Office led to more meetings with unemployed clients, more job offers, more programme assignments, and more sanctions for noncompliance with job search requirements. It shortened unemployment spells through faster job entry, but also through more exits from the labour force in the 2 years following treatment. The duration of unemployment was reduced for a number of subgroups of the unemployed, but not all benefited from increased employment. For women and foreigners, lower caseloads led to more time out of the labour force. The quality of jobs after unemployment, measured by wages, did not change. A cost–benefit analysis suggests that lower caseloads not only shorten unemployment but also save public costs.

Keywords Active labour market policy, Public employment services, Caseworkers, Counselling, Job placement, Field experiment, J64, J68

JEL Classification J64, J68

1 Introduction

In OECD countries, active labour market policies are important in tackling unemployment. Compared to the extensive evidence on the effectiveness of specific programmes such as training or subsidised employment (e.g. Card et al. 2010, 2018), relatively little is known about the role of caseworkers in Public Employment Services (PES). In particular, few studies have examined the impact of caseloads for caseworkers on the labour market outcomes of the unemployed. Caseloads refer to the number of unemployed clients a caseworker is responsible for: the caseworker-to-clients ratio. It is a potentially important policy parameter as caseloads determine how much resources caseworkers can devote to the core PES tasks of counselling and placement of the unemployed.

Caseworkers play a crucial role as they work directly with jobseekers and try to help them back into work in a variety of ways. They provide career counselling and guidance. They advise jobseekers on how to search for jobs effectively and help them overcome barriers to employment, such as transport problems and lack of childcare. In personal meetings, they assess individual skills, deficits and needs. Based on this assessment, they match jobseekers with vacancies and assign them to labour market programmes. Caseworkers can motivate their clients to actively search for work, especially if they are at risk of becoming discouraged and might withdraw from the labour market. Moreover, they monitor the unemployed and impose sanctions in case of non-compliance with job search requirements.

Caseloads influence the effectiveness of caseworkers by determining how much time and effort they can devote to each client (Hainmueller et al. 2016). In theory, lower

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caseloads could yield positive returns in terms of better job placement, job retention or earnings, because caseworkers have more resources to do their job better. It could improve communication, facilitate a relationship of trust and enable caseworkers to provide more individualised support that is better tailored to the jobseekers' needs. Moreover, it could improve the quality and speed of service delivery. At the same time, lower caseloads require the recruitment of more staff and more infrastructure. Caseloads are therefore a major driver of PES administrative costs. Finding the right balance between caseload size and resource allocation is a major challenge.

Only a few studies have examined the effects of caseloads on unemployment outcomes. Even rarer are those based on random experiments, and most of the evidence is confined to Germany. The existing studies suggest that lower caseloads reduce unemployment and also increase employment. However, there is still a lack of empirical evidence on the extent to which exits from unemployment are driven by more frequent job entries or exits from the labour force. This is relevant because individuals may also withdraw from the labour market due to increased pressure to work or participate in a programme.

Especially little is known about possible mechanisms through which caseload changes affect labour market outcomes. Hainmueller et al. (2016) have so far been the only ones to shed light on possible impact mechanisms by examining two intermediate outcomes. They show that German employment offices with lower caseloads impose more sanctions and report more new vacancies. Beyond that, nothing is known about possible impact mechanisms. More research is also needed on the heterogeneity of effects across target groups. Studies have shown positive effects for both the less and more disadvantaged unemployed, but beyond this there is little evidence on how lower caseloads affect different subgroups of the unemployed.

We extend knowledge on the effects of lower caseloads for PES caseworkers, exploiting a randomised controlled trial (RCT) conducted in Austria. In 2015, the caseloads of caseworkers for a randomly selected group of unemployed clients in a regional employment office of the Austrian PES were reduced. Based on this experiment and detailed data, we examine multiple outcomes and margins through which lower caseloads may affect the unemployed. First, we examine the effects on labour market outcomes, namely exits from unemployment and, to shed light on job quality after unemployment, the gross monthly wage in the job taken up. To show how each component contributes to the exit from unemployment, we estimate effects on entering employment and withdrawals from the labour force.

Second, we analyse several intermediate outcomes through which lower caseloads could have produce the observed labour market effects: changes in the frequency of meetings, job offers, programme participation, and benefit sanctions for failure to meet job search requirements. We expect intensified counselling and monitoring, reflected in more contacts, job referrals and sanctions imposed, because caseworkers have more time to meet with their clients, find suitable job vacancies, and monitor their job search efforts. More sanctions may be the result of more job recommendations not being followed and stricter monitoring. However, lower caseloads could also lead to fewer sanctions, as better support reduces the likelihood of individuals failing to meet their obligations.

To examine effect heterogeneity, we estimate impacts for several subgroups of the unemployed: existing clients, new clients who joined during the RCT, and subgroups defined by gender, age, educational attainment, health status, nationality and previous duration of unemployment. Furthermore, to shed light on cost-effectiveness, we add a cost–benefit analysis.

We find that lowering the caseloads of PES caseworkers leads to more meetings between the unemployed and their caseworkers, more job offers, more programme assignments, and more sanctions. Hence, it allows caseworkers to be more active in the counselling and placement of their clients. Lower caseloads result in shorter unemployment durations, both through faster job take-ups, and more exits from the labour market, probably in response to tighter monitoring and increased pressure. Over a 2-year period, they significantly reduce unemployment for a variety of subgroups of the unemployed, but do not increase employment for all. For women and foreigners, they only lead to more time out of the labour force. Post-unemployment wages are not affected. We find no evidence of spillover effects to the untreated in the pilot office. A cost–benefit analysis suggests that lowering caseloads not only reduces unemployment but also public costs.

2 Previous evidence

2.1 Effects of lower caseloads

The impact of caseworker caseloads in PES on jobseekers' labour market outcomes has been investigated in few studies, and only some of these studies are based on randomised field experiments. Most of the evidence is available for Germany. Several pilot projects have been conducted there which consistently indicate positive employment effects from lower caseloads. Using matching methods, Jerger et al. (2001) show that a higher caseworker-to-client ratio at the Mannheim employment office contributed to more job placements

of social assistance recipients, but without any effect on employment stability. In the early 2000s, a pilot project was implemented in four offices of the German Federal Employment Agency. On this basis, Schiel et al. (2008) use event analysis to show that the additional staff increased costs, but that the long–term unemployed and those threatened with long–term unemployment were more likely to find unsubsidised employment.

Another pilot project involved 14 offices of the Federal Employment Agency. Hofmann et al. (2010, 2012) estimate via matching that reducing the caseload by hiring more staff reduced the average duration of unemployment. In addition, using a combination of matching and difference-in-differences estimation, Hainmueller et al. (2016) show that the unemployed clients were also more successful in finding a job. Overall, the reduction in caseloads led to lower local unemployment rates, shorter unemployment spells and higher re-employment rates. Similar results are evident from the "Berlin Job Offensive" where a reduction in caseloads at the twelve Berlin Jobcentres increased the transitions of clients close to the labour market into unsubsidised employment (Fertig 2013).

For the Netherlands, Koning (2009) finds that a higher caseworker-to-client ratio increases the exit rate from unemployment only for the short–term unemployed and has no effect on the long-term unemployed. Using propensity score matching and difference–in–differences regression, Ravn and Nielsen (2019) show that a significant reduction in the caseload of Danish social workers in the PES, and increased activation of disadvantaged hard-to-place welfare recipients, increased their clients' number of hours in subsequent employment.

In two other experiments, lower caseloads were one part of an intensification of counselling to job seekers. They were not studied in isolation, and the focus was on a comparison between the public and private provision of counselling to job seekers. Based on a randomised field experiment for two German employment agencies, Krug and Stephan (2016) show that the public mandatory counselling programme with intensified placement services performed significantly better than the private one. Evaluating a large-scale randomised controlled experiment in France, Behaghel et al. (2014) find similar positive effects for the public and private provision of an intensive job search assistance programme for people at risk of long–term unemployment.

In summary, the empirical evidence suggests that lower caseloads reduce the duration of unemployment and also increase transitions to employment. However, there is still a lack of research on the extent to which exits from unemployment are driven by more frequent job entries or exits from the labour force due to increased pressure to take up a job or participate in labour market programmes. Little is known about possible mechanisms that produce the effects on labour market outcomes. Hainmueller et al. (2016) are so far the only ones to shed light on possible impact mechanisms by examining two intermediate outcomes. They show that German employment offices with lower caseloads impose more sanctions and report more new vacancies.

More research is also needed on the heterogeneity of effects across target groups. The studies mentioned show positive effects for both the less and more disadvantaged unemployed, but beyond this there is little evidence of differential effects for different subgroups of the unemployed population.

2.2 Effects of meetings

Maibom et al. (2017) show that 30 out of 37 studies find positive employment effects of more meetings between caseworkers and unemployed clients. In their own analysis for Denmark, they find that early and intensive counselling in the form of more frequent (biweekly) one-to-one meetings between newly unemployed people and their caseworkers substantially increase employment rates. For group meetings and early mandatory activation with the aim of generating threat effects, they observe positive but insignificant effects. Van den Berg et al. (2012) also find large positive effects of meetings on the transition rate to work for Denmark and according to their findings, the more meetings, the greater the effect. Van der Klaauw and Vethaak (2022) find that assigning unemployed people to an additional meeting with their caseworkers significantly increases job finding in the Netherlands, and Vehkasalo (2020) shows that online and telephone counselling is not a perfect substitute for face-to-face counselling in Finland.

The announcement of an upcoming meeting may change behaviour already before the actual meeting. For example, a randomised controlled trial in Sweden showed that an invitation to a meeting dedicated to counselling and monitoring significantly increased the rate of transition to employment (Hägglund 2011). Apparently, individuals wanted to avoid meetings or associated job or programme assignments that they perceived as unpleasant. Maibom (2023) finds such a "threat effect" for Denmark: Unemployed persons give up about 1.5 weeks of unemployment benefit to avoid participating in a mandatory reemployment programme that requires either more caseworker meetings or early participation in activation programmes.

2.3 Effects of job search assistance programmes and sanctions

Evidence of the positive effects of lower caseloads for PES caseworkers is also consistent with evaluations of specific job search assistance programmes. These programmes often focus on the 'activation' of jobseekers, for example through increased early support and mandatory job-search training. Unless they include human capital-intensive training, they have no long-term effects on employment, however, they usually increase the transition to employment in the short-term (Kluve 2010; Card et al. 2010, 2018). For example, Graversen and van Ours (2008), Rosholm (2008), Graversen and van Ours (2011), Vikström et al. (2013), and Gautier et al. (2018) find positive effects of a Danish activation programme on unemployment exit and job findings, but not on post-unemployment job quality.[1] According to Gautier et al. (2018), the programme increased the job-finding rate of participants but lowered the one of the nonparticipants. They conclude that simply comparing unemployment durations of participants and nonparticipants overestimates the programme effects.

The effectiveness of programmes seems to depend on their detailed design, the target groups reached and the economic and institutional context. For example, van den Berg and van der Klaauw (2006) find overall no clear evidence that a job search assistance programme in the Netherlands, consisting of counselling and monitoring, affects the exit rate to work. More specifically, they find evidence that the more intensive the job search assistance, the more likely it is to have an effect, and that the worse the initial labour market prospects of the treated, the more likely monitoring is to be effective. In a follow-up study, van den Berg and van der Klaauw (2019) show, among other things, that the opportunity to move to a better-paid job compensates for the negative long-term effects of search effort monitoring on post-unemployment wages.

Most studies also report positive effects of benefit sanctions imposed for non-compliance with job search requirements on the transition to work. However, sanctions often increase the likelihood of dropping out of the labour market and tend to have negative effects on the quality of the post-unemployment job, such as job stability or earnings (see Pattaro et al. 2022 for a literature review and van den Berg and Vikström 2014 for a concrete example).

[1] The programme consisted of three parts: participation in a two-week job search assistance programme, followed by weekly or bi-weekly meetings with a caseworker and, if no job was found, another programme.

Table 1 Structure of an Austrian regional employment office. *Source*: Austrian PES

	Info zone	Service zone	Counselling zone
Target group	Public	New entrants	6 months
	Anonymous	"Job-ready"	"Hard-to-place"
Main services	Information	Claims and benefits	Intensive guidance
	Self-service	Placement	Assistance
Mean caseload	–	1:100	1:250

2.4 Caseworker effect heterogeneity

Finally, there is a growing number of studies examining how which caseworkers influence the outcomes of the unemployed. Caseworkers work directly with the jobseekers and have a high degree of discretion. Therefore, differences in their social background, skills, values and working conditions, and consequently in their behaviour, could have an impact on placement success (cf. Hofmann et al. 2014).

Lagerström (2011) shows for Sweden that caseworkers have an impact on jobseekers' future employment and earnings. For Switzerland, Lechner and Smith (2007) compare the allocation of caseworkers to labour market programmes and services with alternatives including random allocation and allocation via statistical treatment rules based on observable participant characteristics. They find that caseworkers achieved roughly the same post-programme employment rates as random assignment, while statistical treatment rules performed significantly better.

If caseworkers have access to a statistical system providing individual predictions of unemployment risk in relation to participation in different programmes, they do not necessarily use this information. In this vein, a large field experiment in Switzerland showed that they did not change their behaviour in any significant way due to having such information access (see Behncke et al. 2009). In line with this, Bolhaar et al. (2020) found considerable heterogeneity in the way caseworkers assign welfare–to–work programmes in a field experiment in Amsterdam. They found that caseworkers do not appear to make optimal use of their discretion in assigning benefits recipients to the most effective programmes. Even learning about the effectiveness of different programmes does not lead them to focus more on the effective programmes. For Germany, Schmieder and Trenkle (2020) found that caseworkers do not optimise their behaviour by treating unemployed people with different lengths of unemployment insurance eligibility differently.

Several studies point to differences in the performance of caseworkers and ask what this depends on. For example, Behncke et al. (2007) found for Germany that

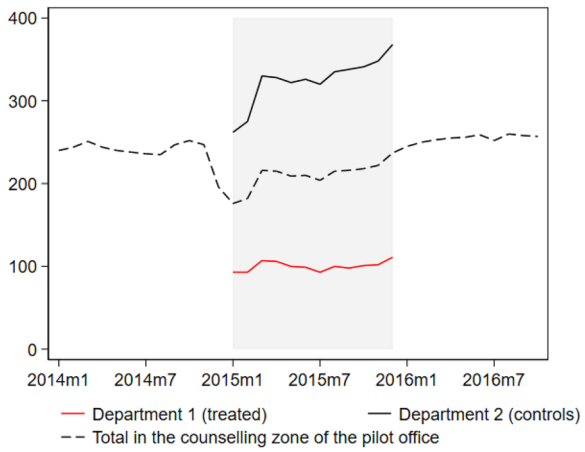

Fig. 1 Caseloads (unemployed clients per caseworker) in the counselling zone of the pilot office. *Source*: Austrian PES. Total: total caseloads in the counselling zone of the pilot office

caseworkers with better contacts to local firms achieved higher reintegration rates. Also for Switzerland, Behncke et al. (2010a) found that those who place less emphasis on a cooperative and harmonious relationship with their clients increase their chances of employment in the short and medium term. Analyses by Huber et al. (2017) suggest that these positive employment effects of less accommodating caseworkers are not driven by a particularly effective mix of labour market programmes, but rather by other dimensions of the counselling process, possibly including the threat of sanctions and pressure to accept jobs. In another study for Switzerland, Behncke et al. (2010b) found that the chances of finding a job increased when the unemployed were counselled by caseworkers who belonged to the same social group, defined by gender, age, education and nationality.

For Germany, Boockmann et al. (2014) found that caseworkers who dee rapid placement as the most important goal tend to achieve better integration outcomes than those who see other goals as more important. The willingness to impose sanctions slightly and significantly increases the probability of leaving unemployment without affecting the probability of finding a job. For Sweden, Granqvist et al. (2017) found that a positive attitude towards public sickness insurance and existing rehabilitation methods increases the return to work. Finally, for Switzerland, Schiprowski (2020) found that individuals remain unemployed longer when they lose a meeting with their caseworker, and that the impact of caseworkers on both the duration of unemployment and the quality of the job match varies significantly with their productivity. Cederlöf et al. (2021) for Sweden and Dromundo and Haramboure (2022) for the French capital Paris also find evidence that caseworkers' performance varies according to their characteristics and attitudes.

3 Institutional background

3.1 PES structure

The Austrian PES ("Arbeitsmarktservice", AMS) is a one-stop shop for the unemployed. It administers unemployment benefits and (means-tested) unemployment assistance. It also provides counselling and placement services. In addition, it is responsible for implementing training programmes and other active labour market policies.

The PES is divided into a federal head office, nine regional offices—one for each of Austria's nine provinces—and 101 regional offices, 12 of which are located in Vienna. The federal office is responsible for management, controlling, evaluation, analysis, and strategic planning. The provincial offices coordinate the regional offices where the unemployed meet their caseworkers. Clients are assigned to the regional office based on the post code of their place of residence, which is usually the nearest office.

Each of the 101 regional offices offers services for the unemployed in three "zones": an information zone, a service zone, and a counselling zone, as shown in Table 1. The *information zone* provides general and anonymous labour market information to the public, including numerous self-service facilities. The *service zone* is for newly registered unemployed and those who need little assistance. Here, applications for unemployment benefits are processed, and the unemployed receive counselling and job offers. The *counselling zone* is for the unemployed who have been unemployed for at least 6 months or are considered difficult to place for other reasons. Here they receive more intensive guidance and support than in the service zone. The average caseload of a caseworker in the counselling zone was about 250 unemployed persons per caseworker, compared to 1:100 in the service zone before the pilot project started.

3.2 The randomised controlled trial

The randomised controlled trial (RCT) was conducted in one of the twelve regional employment offices in Vienna: the AMS Vienna Esteplatz. In this office, there are two departments in the counselling zone with identical tasks. In the RCT, the caseloads of caseworkers in one of the counselling zone departments was changed by an administrative reorganization. Before the year 2015, one department was responsible for unemployed jobseekers born between January and June, while the other department was responsible for unemployed people born between July and December. Each department had an average of about 22 full-time equivalent caseworkers. The

average caseload, or the caseworker-to-client ratio, was about 1:250.

The RCT was implemented in 2015 and consisted of the following two changes. First, each department received four additional caseworkers. Second, the responsibility of department 1 (treatment group) was limited to all unemployed born in January, February or March. Department 2 (control group) was responsible for all other unemployed persons in the counselling zone (i.e., those born in April through December).[2]

These changes lowered the caseload to a ratio of 1:100 for Department 1, while the caseload in Department 2 remained unchanged. At the beginning of 2015, the ratio was about 1:260. Later in the year, the caseload in both departments worsened due to a sharp increase in unemployment (see Fig. 1). This trend was more pronounced in Department 2 for two reasons: Firstly, Department 2 was responsible for three quarters of the unemployed clients (nine out of twelve birth months) and was therefore more affected by the increase in unemployment in absolute terms. Secondly, it is a dynamic consequence of the effectiveness of the treatment in Department 1. The caseload reduction caused unemployed clients to leave unemployment more quickly. This lowered the stock of clients and thus reduced caseloads further over time.

Due to the increase in unemployment, the caseload in the control group did not remain constant but worsened compared to the situation before the pilot project. In this sense, the effects we measure come from both a decrease in the caseload of the treatment group and an increase in the caseload of the control group. However, the experiment itself only changed the caseload in Department 1. Here, the caseload was much lower throughout the pilot, both compared to before the pilot and compared to the control group. Between January and December 2015, the difference between the groups remained broadly constant between 64 and 71%.

Other things did not change. The tasks of the two departments remained the same. With the aim of intensifying contact with these customers, only the staff responsible for the unemployed was increased. The extra capacity was not used for other purposes, such as intensifying contacts with employers or reorganising internal processes.

Randomisation is credible because in one regional office of the PES (the AMS Vienna Esteplatz) and here in one zone (the "counselling zone"), the unemployed clients were strictly divided into treatment and control groups according to their date of birth, i.e. an exogenous factor. Which clients are served in the counselling zone is not only determined by administrative rules, but also by the discretion of the PES staff: if they notice an increased need for support already at the beginning of unemployment, they may refer people directly to the counselling zone. However, this only determined who participated in the pilot project, not the allocation to the departments (i.e. treatment or control group), which was strictly based on the date of birth.[3] Secondly, there is no reason to believe that the pilot changed the allocation to the counselling zone.

Theoretically, it is possible that PES staff referred more clients to the counselling zone because they knew that the number of staff there had increased, but we find no evidence of this. If the increase in staff had led to more referrals to the counselling zone, this should have been reflected in (1) a higher proportion of unemployed clients and (2) a shorter previous unemployment spell of clients in the counselling zone. However, neither was the case.

In the regional employment office where the RCT was conducted ("pilot office"), the proportion of unemployed in the counselling zone developed very similarly to the other offices in Vienna. At the start of the pilot in January 2015, it was 72.7% in the pilot office and 72.1% in the

Table 2 Selected summary statistics by treatment status

	Controls	Treated	Difference
New entrant	0.492	0.497	−0.004
Existing client	0.508	0.503	0.004
Women	0.422	0.420	0.001
Age (years)	38.560	38.890	−0.330
Disabled	0.017	0.014	0.003
Health problems	0.121	0.130	−0.008
Formal education			
Compulsory	0.460	0.477	−0.017*
Apprenticeship	0.196	0.192	0.004
Vocational school	0.042	0.039	0.003
High school	0.159	0.160	−0.001
College or university	0.143	0.132	0.010
Austrian	0.649	0.645	0.004
Elapsed unemployment duration (days)	361.300	339.400	21.856*
Unemployment last 5 years (days)	953.400	938.900	14.450
Observations	9027	3397	

The unit of observation is an unemployment spell

*$p < 0.1$; All variables measured at entry into the RCT

[2] Note that the Austrian cut-off date for starting school is the first of September (in contrast, to e.g., the first of January in the USA). We therefore do not expect that persons born in the first quarter of a calendar year differ substantially from persons born in, say, the second or third quarter. Robustness checks that exclude, for example, persons born in the third or fourth quarter underscore the robustness of our results (see Table 9 in the "Appendix").

[3] There is also no sorting in the regional employment office, as there are clear rules for this: Except for small groups such as homeless people, allocation is strictly by the post code of the home address.

other offices in Vienna, i.e. almost exactly the same (see Fig. 5 in the "Appendix"). The same is true for the average duration of previous unemployment: In January 2015, it was 137 days in the pilot office and 135 days in the other offices in Vienna and has developed very similarly over time (see Fig. 6 in the "Appendix"). Therefore, there should be no sorting into departments.

We also find no evidence of differences in performance between the two departments before the pilot project. Of all persons who entered the counselling zone of the pilot office in the period January to September 2014, 29.1% of those with birth months January to June (Department 1) and 29.2% of those with birth months July to December (Department 2) left unemployment within 3 months. The differences between the two departments are statistically insignificant, both with and without controlling for differences in the groups' personal and labour market history characteristics in an Ordinary Least Squares (OLS) regression.[4]

4 Empirical research design

4.1 Administrative data

We use two sources of administrative data, the Austrian unemployment register (AUR) and the Austrian social security database (ASSD). The AUR contains data collected and processed by the Austrian Public Employment Service (AMS).[5] From this source, we obtain detailed information on all unemployed individuals, such as age, gender, formal education, health restrictions or care responsibilities which may affect individual labour supply. We use information on unemployment spells, benefit receipt, caseworker interventions such as meetings, job referrals or benefit sanctions imposed for non-compliance with job search requirements. Furthermore, we use information from the AUR on participation in active measures such as training or various types of subsidised employment.

The ASSD is an integrated employer-employee data set that provides a complete record of the labour market histories and earnings of all private-sector employees in Austria on a daily basis since 1972, as well as some information on employee and employer characteristics. From this data source, we obtain detailed information on employment history, including information on wages.[6]

4.2 Sample

Our sample comprises 12,424 unemployment episodes from 11,646 unemployed persons who registered with the counselling zone during 2015.[7] Each person can be observed until January 31, 2018. Of these, 3397 (27.3%) are treated and 9027 are control observations. A comparison of the groups shows that there are few, and only small, differences between the groups[8] (see Table 2 for selected variables and Table 7 in the "Appendix" for the full summary statistics).[9] All characteristics were measured at the time of entry into the RCT. Among the treated, the proportion with at most compulsory education was slightly higher, and they had been unemployed for a slightly shorter period of time than the control group by the time the RCT started.

The sample consists of persons who already received counselling from the departments before the RCT started ("existing clients ") and of new entrants during the period ("new entrants "). As shown in Table 2, 50.8% are existing clients and 49.2% new entrants.

4.3 Empirical strategy

Because of the random design of the RCT, we can directly compare the outcomes of the two departments. However, given that we observe small differences in characteristics

[4] We restrict the comparison to outcomes within three months for people with pilot entry up to September to avoid observing outcomes in the 2015 pilot period. The regression results are available on request from the authors.

[5] The AMS maintains various databases and registers for statistical and administrative purposes, for the provision of its services and to enable informed policy decisions. This includes information on persons who are unemployed, looking for work or receiving unemployment benefits and are therefore registered with the AMS. Access to the individual data of the AMS is subject to strict data protection regulations and is explicitly granted only for specific research projects.

[6] As documented by Zweimüller et al. (2009), the ASSD is administered by the Federation of Social Insurances, which collects individual data from all Austrian social insurance institutions. The data cover more than 200 different types of spells of individuals, which determine the eligibility for and the amount of social security benefits in health, accident, and pension insurance. These spells can be translated into labour market statuses. In addition, social contributions are included in the data as they also determine the level of social benefits. They are derived from individual annual earnings and thus provide information on annual earnings. However, this information on earnings is incomplete. Firstly, the recorded social contributions are top-coded with the maximum contribution base. Second, the data do not provide information on working time. Therefore, we cannot compare hourly earnings. Changes in monthly wages are not necessarily due to changes in hourly wages but may also be due to changes in working time. Access to social security data is also subject to strict data protection rules. They are only made available to a very limited extent to designated research institutions for research purposes.

[7] We observe 10,892 persons with a single unemployment spell (2,891 are treated and 8,001 are controls) and 754 with more than one unemployment spell (248 treated and 506 controls).

[8] Note that there is no clear cut-off to decide when differences between variables indicate proper randomization. For a discussion, see e.g., de Boer et al. (2015).
We use a large set of control variables to control for the remaining differences. In Fig. 5 in the "Appendix", we show that a propensity score estimated with these covariates is balanced across treatment and comparison groups. Moreover, when we apply one-to-one nearest neighbour matching, t-tests reveal no significant differences in the mean values between the treatment and control groups overall after matching.

[9] The time of entry into the RCT is the time when an unemployed person first appeared in the counselling zone in 2015.

Table 3 Effects of lower caseloads on labour market outcomes

	Treated	Controls	Mean difference (SE)	OLS estimates (SE)
All exits within 1 year[a]	60.5	53.0	7.5*** (1.0)	7.7*** (0.9)
Employment within 1 year	34.7	31.1	3.7*** (1.0)	4.8*** (0.9)
Unsubsidised employment within 1 year	25.8	23.9	1.8** (0.9)	2.9*** (0.8)
Subs. empl. 1st labour market within 1 year	2.7	2.1	0.6* (0.3)	0.5* (0.3)
Subs. empl. 2nd labour market within 1 year	3.4	2.1	1.3*** (0.3)	1.3*** (0.3)
OLF within 1 year	25.8	22.0	3.8*** (0.9)	3.0*** (0.8)
Unemployment duration[b]	394	453	−60*** (8)	−62*** (7)
Days employment over 2 years	194	184	10** (5)	16*** (4)
Days unsubs. empl. over 2 years	155	148	7 (5)	13*** (4)
Days unemployment over 2 years	386	420	−34*** (5)	−36*** (4)
Days OLF over 2 years	162	137	24*** (4)	20*** (4)
Employment after 2 years[a]	34.4	33.7	0.7 (1.0)	1.7** (0.9)
Unsubdisized employment after 2 years	27.8	26.8	1.0 (0.9)	2.0** (0.8)
Unemployment after 2 years	41.9	45.3	−3.4*** (1.0)	−3.7*** (0.9)
OLF after 2 years	23.7	20.9	2.7*** (0.8)	2.0** (0.8)
Benefit days[b]	406	453	−46*** (7)	−35*** (5)
Total benefits[c]	10,362	11,390	−1028*** (205)	−755*** (152)
Starting wage[c]	1694	1691	3 (27)	16 (21)

Effects for both existing and new clients. Data are from AUR, ASSD. Observations: 12,424, of which 3397 treated and 9027 controls. (a) %-points; (b) days; (c) Euros; starting wage w/o extra payments (excluding marginal jobs). Censoring date: 31.1.2018. Robust standard errors in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

between the treatment group and the control group, we control for any remaining observable differences using an OLS regression. We estimate the average treatment effect of lower caseloads for unemployment episode i by comparing the outcomes between treated and controls:

$$y_i = \alpha_0 + \alpha_1 Treatment_i + X_i'\beta + \delta_t + \varepsilon_i, \quad (1)$$

where y_i is an outcome indicator, e.g., the unemployment duration after entering the RCT, for unemployment episode i. The indicator "*Treatment* " indicates whether an unemployed person was treated or not. The vector X contains observable characteristics measured at entry into the RCT. As controls, we use gender, age, age squared, indicators for marital status, number of children, age of the youngest child, whether the person was legally disabled or not, whether there were other health problems or not, indicators for formal education, and an indicator for the person's nationality. These personal characteristics are possibly correlated with the chances of finding a job.

Additional control variables describe the person's labour market situation. We use the unemployment duration at the time of entry into the RCT and whether the unemployed had already an employer's promise to be hired at a later date or not.[10] We use indicators for the type and level of past unemployment benefits, including the receipt of a needs-based minimum income (*"Bedarfsorientierte Mindestsicherung"*), whether the previous employment spell ended more than 1 year before entry into the experiment, wages in the last job, detailed employment histories (including sickness benefit receipt), and indicators for the sector of the person's last job. We control for the past contacts between the person and the caseworker, the number of earlier placement proposals, and the participation in active measures before the entry into the RCT. δ_t are monthly indicators which control for the entry month into the experiment.

As shown in Table 3, controlling for observables changes the measured effects only slightly, as the treatment and control groups were already very similar due to random selection.

4.4 Outcome indicators

We compare transitions from unemployment to different exit destinations between the treated and the controls. If the treated unemployed leave unemployment faster than the controls, this could be due to more job take-ups, but

[10] Individuals with a hiring promise search less intensively for a job and are not included in the job placement process for a period of time. This is typically relevant for persons in seasonal sectors who are often temporarily laid off (Böheim 2006; see also Nekoei and Weber 2020).

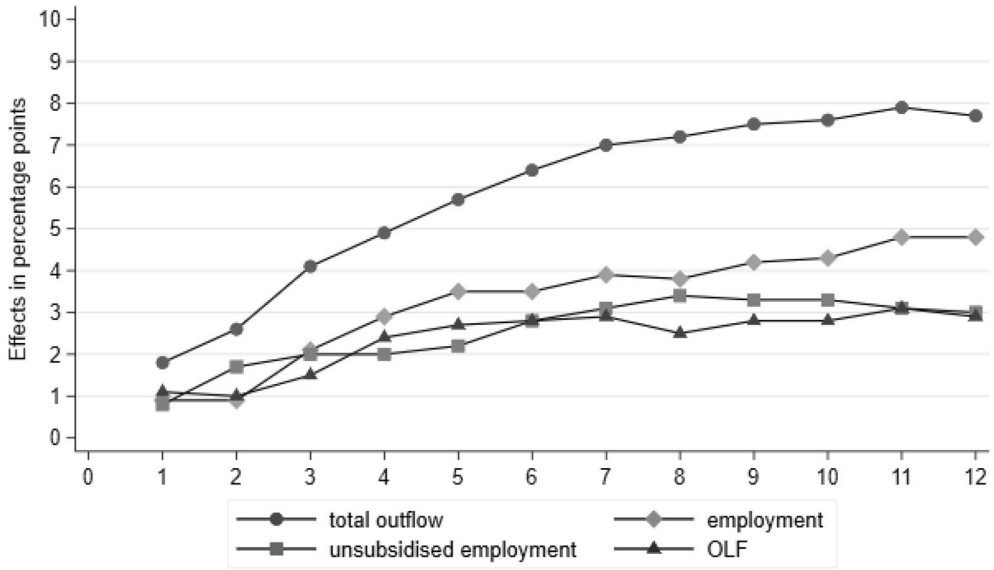

Fig. 2 Effects of lower caseloads on unemployment exits. *Source*: AUR, ASSD. *Note*: Unemployment spells by both existing and new clients. Each dot is an estimated difference between treated and control observations. Each estimated effect is obtained from a separate estimation of Eq. (1) where the dependent variable is a binary indicator of leaving unemployment (all exits, all exits to employment, all exits to unsubsidised employment, and exits from the labour force) within 3 months after entering the RCT. All effects are significant at a level of 10%, almost all at a level of 1%

also to more people leaving the labour force, for example, in response to increased pressure from the caseworkers to take up employment. For this reason, we distinguish between (1) exits to employment and (2) exits from the labour force.

We define an exit to employment when we observe a person entering employment within 2 weeks of leaving the unemployment register. In more detail, we distinguish between subsidised and unsubsidised dependent employment and self-employment. In all other cases, i.e. when an unemployed person leaves the unemployment register but does not take up a job within 2 weeks, we define this as an exit from the labour force.[11] In addition to exits from the initial spell of unemployment, we also analyse the employment status 2 years after the start of the RCT and the cumulative number of days spent in various employment statuses over a 2-year follow-up period. Other indicators of labour market success include the duration of unemployment, the duration and total amount of unemployment benefit and unemployment assistance received, and the gross monthly wage in the next job.

We measure the effect of exits from unemployment to (subsidised or unsubsidised) employment on the monthly entry wage. As an indicator of monthly wages, we use the social security contribution base (excluding extra payments). Three things have to be taken into account: First, the contribution base is top-coded with the maximum contribution base. Second, for reasons of data availability, we can only look at employment in 2015 and 2016. Third, a positive wage is conditional on taking up a job. Thus, treatment status is no longer exogenous in the estimation of wage effects. In the OLS regression, we do not necessarily fully control for all factors that influence job take-up. Therefore, the measured effects could be biased in this case. In order to describe the effect of the treatment on the placement process (intermediate outcomes), we compare the frequency of meetings with caseworkers, the number of job referrals, the number of benefit sanctions (unemployment benefit suspension due to non-compliance with job search requirements), and participation in various active labour market policy measures during the RCT.[12]

[11] The outcomes are competing risks. If persons do not exit to employment, this can either be because they instead exit from the labour force, or because they do not leave unemployment at all. Likewise, exits from the labour force do not occur when an individual instead starts a new job or remains unemployed.

[12] Each estimated effect is obtained from a separate estimation of Eq. (1) where the dependent variables are either binary indicators (such as exit rates, employment status on cut-off dates) or continuous variables (durations, days in different employment statuses, benefits, and starting wage).

Table 4 Effects of lower caseloads on frequency of caseworker meetings, job offers, sanctions and programme participation. *Source*: AUR, ASSD. Robust standard errors in parentheses

	Treated	Controls	Mean Difference (SE)	OLS estimates (SE)
Share with meeting (%)	93.5	91.7	1.8*** (0.5)	1.7*** (0.5)
Meetings	5.9	3.5	2.3*** (0.1)	2.4*** (0.1)
Meetings per month of treatment	0.6	0.4	0.3*** (0.0)	0.3*** (0.0)
Share with job offer (%)	59.1	49.8	9.3*** (1.0)	10.5*** (0.9)
Job offers	4.7	2.1	2.5*** (0.1)	2.6*** (0.1)
Job offers per month of treatment	0.5	0.2	0.3*** (0.0)	0.3*** (0.0)
Share with benefit sanction (%)	11.9	8.3	3.5*** (0.6)	3.7*** (0.6)
Report failure	9.7	7.2	2.5*** (0.6)	2.6*** (0.6)
Job or training refusal	2.4	1.1	1.2*** (0.3)	1.3*** (0.3)
Share with programme start (%)	47.3	38.5	8.8*** (1.0)	9.1*** (0.9)
Job search programme	5.9	5.5	0.4 (0.5)	0.7 (0.5)
Vocational orientation	4.2	3.2	1.0** (0.4)	1.0*** (0.4)
Training	19.5	17.3	2.2*** (0.8)	2.1*** (0.7)
Course subsidies	5.4	3.3	2.1*** (0.4)	2.3*** (0.4)
Integration subsidy	0.3	0.4	0.0 (0.1)	0.0 (0.1)
Direct job creation	2.0	1.0	1.0*** (0.3)	1.0*** (0.3)
Non-profit temp agency	0.4	0.3	0.1 (0.1)	0.1 (0.1)
External counselling	27.2	19.7	7.4*** (0.9)	7.1*** (0.8)

Effects for both existing and new clients. Interventions during unemployment spell, in period from RCT entry until the end of 2015 (only during RCT duration). Share with meeting (%): Share with at least one meeting in the period from entry into the RCT until the end of 2015. Training is provided by institutions on behalf of the PES. Course subsidies are subsidies for participation in courses of private sector education providers. Integration subsidy refers to subsidised private sector employment. Direct job-creation is temporary subsidised employment in public- or non-profit-sector firms in combination with skills training and socio-pedagogical support

***$p < 0.01$, **$p < 0.05$

5 Main results

5.1 Labour market effects

We estimate that lower caseloads had a significant positive effect on exits from unemployment and consequently shortened the duration of unemployment. The shorter unemployment duration is the result of faster and more frequent exits to employment and of more withdrawals from the labour market.

Figure 2 plots the estimated effects of treatment on exits from unemployment for all destinations. It shows that exit rates are significantly higher for the treated persons than for the control group throughout the observation period. The lower caseloads already had an effect in the first month: The share of persons who left unemployment within 1 month was 1.8 percentage points higher for the treated than for the controls. This effect increased with the length of the observation period. It is likely that part of the effect takes some time to materialise, as more intensive counselling made possible by lower caseloads leads to more training. More training may reduce exits from unemployment in the short-term due to a lock-in-effect but may increase exit rates later. The proportion of persons who left unemployment within 1 year was 7.7 pp greater for the treated than for the controls, corresponding to a 15% greater exit rate.

Distinguishing between the destinations of exit from unemployment, we see that the treatment increased both the exit rate into employment and the exit rate from the labour market. The share of persons leaving unemployment for any job within 1 year increased by 4.8 pp (about 15%), and the exit rate from the labour market increased by 3.0 pp (about 14%). The majority of the jobs accepted by the treated unemployed were unsubsidised jobs. The share of treated persons who started an unsubsidised job within 1 year of entering the RCT was about 2.9 pp higher than for the controls.[13]

The higher exit rate shortened the duration of unemployment. On average, the treated left unemployment about 62 days earlier (14%) than the controls (the estimated coefficients are tabulated in Table 3). During the period from the start of the RCT until January 31, 2018, our censoring date, they accumulated on average 35 fewer days of benefit receipt (8%). On average, each treated

[13] To a lesser extent, the probability of moving from unemployment to subsidized employment also increased. The share of treated who started a subsidized job (mainly integration subsidies) within a year was 0.5 pp greater than for the controls. The share that took up subsidized employment in the "second labour market" (direct job creation in the public or non-profit sector) was 1.3 pp greater for the treated than for the controls.

Table 5 Effects of lower caseloads on unemployment duration and benefit receipt by population group. *Source*: AUR, ASSD

	(1) Unempl. duration[a]	(2) Benefit days[a]	(3) Total benefits[b]
Full sample	−62*** (7)	−35*** (5)	−755*** (152)
Existing clients	−67*** (11)	−37*** (8)	−771*** (235)
New entrants	−54*** (8)	−32*** (7)	−689*** (189)
Men	−63*** (9)	−35*** (7)	−721*** (206)
Women	−60*** (10)	−31*** (8)	−736*** (226)
Age 15–24	−33** (16)	−16 (12)	−238 (276)
Age 25–49	−55*** (9)	−29*** (7)	−622*** (188)
Age 50–64	−84*** (16)	−58*** (12)	−1302*** (367)
At most compulsory school	−57*** (10)	−36*** (8)	−845*** (196)
Apprenticeship	−91*** (16)	−51*** (13)	−1057*** (378)
Intermediate voc. School	−33 (43)	−24 (37)	−617 (1106)
Higher academic or voc. school	−68*** (16)	−31** (14)	−562 (408)
Academic education	−19 (16)	−9 (14)	33 (486)
Disability	−81*** (21)	−61*** (16)	−1287*** (416)
No disability	−59*** (7)	−30*** (6)	−662*** (164)
Austrian nationality	−71*** (9)	−39*** (7)	−794*** (204)
Other nationality	−43*** (11)	−25*** (8)	−618*** (215)
Long-term jobless	−88*** (16)	−53*** (12)	−1179*** (343)
Short-term jobless	−52*** (7)	−29*** (6)	−628*** (166)

(a) days; (b) Euros. Effects for both existing and new clients. Statutory disability status or other health-related placement obstacle according to the PES caseworker. Robust standard errors in parentheses. Long-term jobless: apart from shorter interruptions (maximum 62 days) already more than 365 days unemployed

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

person received about 755 Euro (7%) less in unemployment benefits than a member of the control group.

In the 2 years following the start of the RCT, treated persons spent on average 16 more days (9%) in employment (13 more days in unsubsidised employment), 36 fewer days in unemployment (9%), and 20 more days (15%) out of the labour force than the controls. Thus, the effect of lower caseloads on time spent out of the labour force is even more pronounced than the effect on cumulative employment.

The median duration in treatment was 365 days, and the mean was 276 days. With the end of the RCT after 12 months, the difference in exit rates remains stable. However, even 2 years after entry, the treated are significantly less often unemployed (−3.7 pp) and more frequently employed (+1.7 pp; unsubsidised employment +2.2 pp) than the controls. However, they are also more often out of the labour force (+2.0 pp).

The effect of lower caseloads on post-unemployment job match quality is not clear from a theoretical perspective. More intensive counselling, made possible by lower caseloads, could only improve the job finding rate and have no additional effect on the quality of the match, measured, for example, by wages or post-unemployment tenure. However, it is also possible that more intensive counselling results in better matches, if caseworkers are better informed about labour demand than unemployed jobseekers. Conversely, more intensive counselling may lead jobseekers to accept a worse match, either because of increased pressure or to avoid meeting the caseworker. We find no statistically significant effect of lower caseloads on the starting wages of post-unemployment jobs. The estimated difference in starting wages is about €16, at an average of about €1694 per month, and statistically insignificant.

5.2 Effects on the job placement process

In a next step, we examine the effects of more caseworkers on the job placement process to see which aspects of counselling changed as a result of the lower caseload. Table 4 tabulates the estimated effects of treatment on job placement activities. The share of the unemployed who had a meeting with their caseworker was significantly greater among the treated than among the controls in each of the 12 months of the RCT. From an unemployed person's entry into the RCT until the end of 2015, the treated had on average 2.4 more contacts than the controls, an increase by two-thirds, despite their shorter average duration of unemployment. Accordingly, they

Table 6 Effects of lower caseloads on days spent in employment, unemployment, and OLF over 2 years by population group. *Source*: AUR, ASSD

	(1) Employment	(2) Unsubsidised employment	(3) Unemployment	(4) OLF
Full sample	16*** (4)	13*** (4)	−36*** (4)	20*** (4)
Existing clients	9 (5)	6 (5)	−35*** (6)	27*** (5)
New entrants	24*** (6)	21*** (6)	−35*** (6)	11** (5)
Men	22*** (5)	21*** (5)	−35*** (5)	12*** (4)
Women	6 (7)	1 (6)	−37*** (6)	32*** (6)
Age 15–24	32*** (12)	21* (12)	−26** (11)	−5 (10)
Age 25–49	12** (5)	14*** (5)	−31*** (5)	19*** (4)
Age 50–64	17** (7)	7 (6)	−46*** (9)	30*** (8)
At most compulsory school	12** (5)	5 (5)	−33*** (6)	22*** (5)
Apprenticeship	22** (9)	16* (9)	−45*** (9)	23*** (8)
Intermediate voc. School	42 (28)	35 (28)	−48* (27)	7 (22)
Higher acad. or voc. school	15 (12)	16 (12)	−42*** (11)	27*** (10)
Academic education	13 (14)	26* (14)	−18 (12)	5 (11)
Disability	24*** (9)	14* (8)	−47*** (11)	23** (10)
No disability	16*** (5)	14*** (4)	−34*** (4)	18*** (4)
Austrian nationality	21*** (5)	16*** (5)	−40*** (5)	20*** (4)
Foreign nationality	9 (7)	9 (7)	−27*** (7)	18*** (6)
Long-term jobless	14* (7)	5 (6)	−52*** (8)	38*** (6)
Short-term jobless	18*** (5)	17*** (5)	−30*** (5)	13*** (4)

Effects for both existing and new clients. Statutory disability status or other health-related placement obstacle according to the AMS caseworker. Robust standard errors in parentheses. Long-term jobless: apart from shorter interruptions (maximum 62 days) already more than 365 days unemployed. Robust standard errors in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

had 0.3 more contacts per month during the unemployment spell.

More frequent meetings with their caseworkers resulted in significantly more job referrals. On average, the treated received 2.6 more job proposals during the RCT, an 122% increase, than the controls. On average, the treated received 0.3 proposals more per month of treatment than the controls. Moreover, the treated were significantly more frequently assigned to and participated more frequently in active labour market programmes, such as vocational orientation, training, direct job creation, and external counselling, than the controls.

Lower caseloads also resulted in more sanctions for non-compliance with job search requirements. The treated were 3.7 pp more likely to have their unemployment benefits suspended than the controls.[14]

Thus, having more resources apparently allowed caseworkers to be more active in counselling and placing their clients. This may explain why lower caseloads reduced the duration of unemployment.

[14] This effect is evident for both missing a meeting (2.6 pp), and for refusal to accept a reasonable job offer or training (1.3 pp).

5.3 Differences between groups

We find a considerable heterogeneity of treatment effects for different groups of unemployed persons. Table 5 shows the heterogeneity of impacts on unemployment duration and benefit receipt, Table 6 on days spent in employment, unemployment, and OLF over 2 years. In addition, outcomes on the placement process are tabulated in Table 8 in the "Appendix".

Lower caseloads reduced the duration of unemployment for a number of subgroups of the unemployed, but not all benefited from increased employment. For women and foreigners, the intervention only led to significantly more time out of the labour force. Both "existing clients" who were already in the counselling zone before the RCT and new clients who joined during the RCT experienced a reduction in unemployment, but only the new clients also experienced a significant increase in their employment. For existing clients, exits from the labour force dominated.

The long-term unemployed (unemployed for more than 1 year, with short breaks of 62 days or less) recorded a stronger absolute decrease in unemployment, but the employment effect was weaker for them and the effect on labour market exits stronger than for the short-term

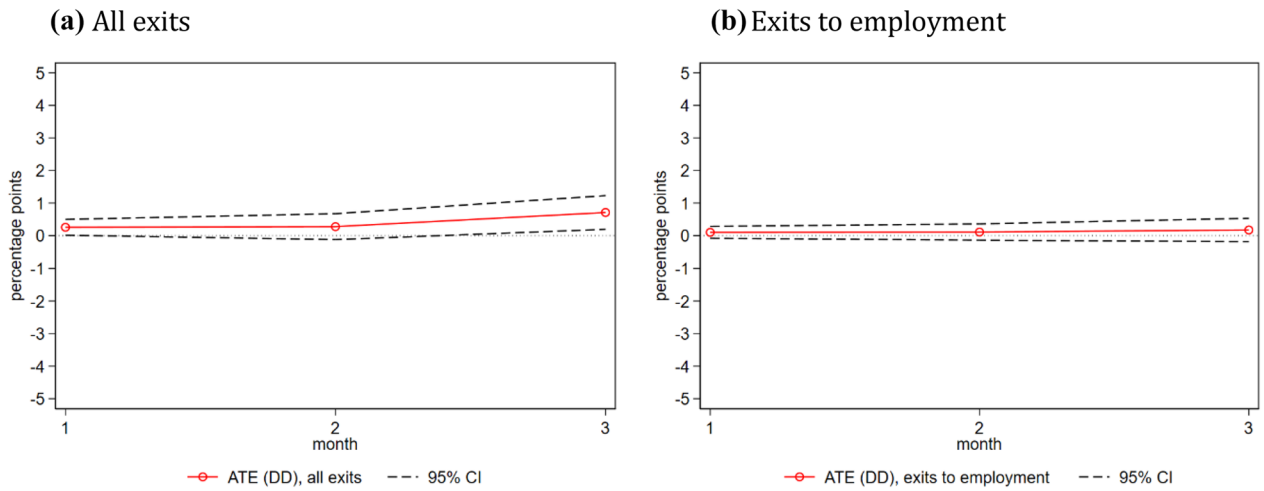

Fig. 3 DD estimates of the effects of being in the pilot office compared to other employment offices in Vienna on exits from unemployment, among untreated jobseekers with birth month April–December. *Source*: AUR, ASSD. *Note*: Unemployment spells by both existing and new clients. Each dot is a difference between the two comparison groups, estimated separately by difference-in-differences (DiD) estimation with a binary indicator for leaving unemployment (all exits, all exits to employment) within 3 months of entering the counselling zone of the regional public employment office

unemployed. Moreover, the absolute impact on unemployment duration increased with age. Unemployment fell the most for older people aged 50 and over, but young people under 25 experienced the largest increases in employment and no increase in exits from the labour market.

In terms of intermediate outcomes (see Table 8 in the "Appendix"), the reduction in caseloads led to an increase in the number of meetings and job placements for all groups. With a few exceptions, the proportion of people receiving benefit sanctions and entering a programme also increased significantly for all groups. In other words, the intensification of counselling and monitoring was not concentrated on specific clients but affected the entire client stock. Only the specific extent and details, such as the types of programmes that were more frequently assigned, varied. For example, more disadvantaged groups, namely the long-term unemployed, older people over 50 and people with disabilities, were particularly often transferred to external providers of labour market related counselling and support.

5.4 Cost–benefit analysis

To assess the cost-effectiveness of the treatment, we compare the costs and benefits of the intervention per unemployed person treated from a fiscal perspective. Our outcome period extends from the start of RCT entry to 31 January 2018, the date of data censoring.

We find that the gains, i.e., fewer unemployment benefits (UB) and unemployment assistance (UA), as well as income tax and social security contributions, exceed the additional costs of treatment. The cost of the additional caseworkers, including overhead, was €613,120 or, alternatively, about €163 per treated person. Additional participation in active labour market programmes resulted in additional expenditures averaging €227 per person. In sum, the additional costs amounted to €390 per treated person.

These additional expenses were offset by savings on UB and UA benefits amounting to an average of €755 per treated person. Due to more exits to employment and a shorter unemployment duration, the treated paid on average €159 more in income tax and €551 more in social security contributions than persons in the control group. Overall, the public sector received €1075 per treated person.

Hence, the comparison of direct costs and benefits suggests that the treatment was not only successful in reducing unemployment durations, but that it was also cost-effective. This cost–benefit analysis assumes that there are no spillover effects, i.e., that the additional employment and subsequent tax and social security payments do not come at the expense of other jobseekers who would otherwise have found employment.

6 Potential spillover effects

Finally, we examine the presence of potential spillover effects, i.e. unintended effects of caseload reduction on untreated jobseekers, which could bias our estimates. In our setting, one concern could be displacement effects: Jobseekers who benefit from a reduced caseload may find jobs more quickly, but at the expense of other

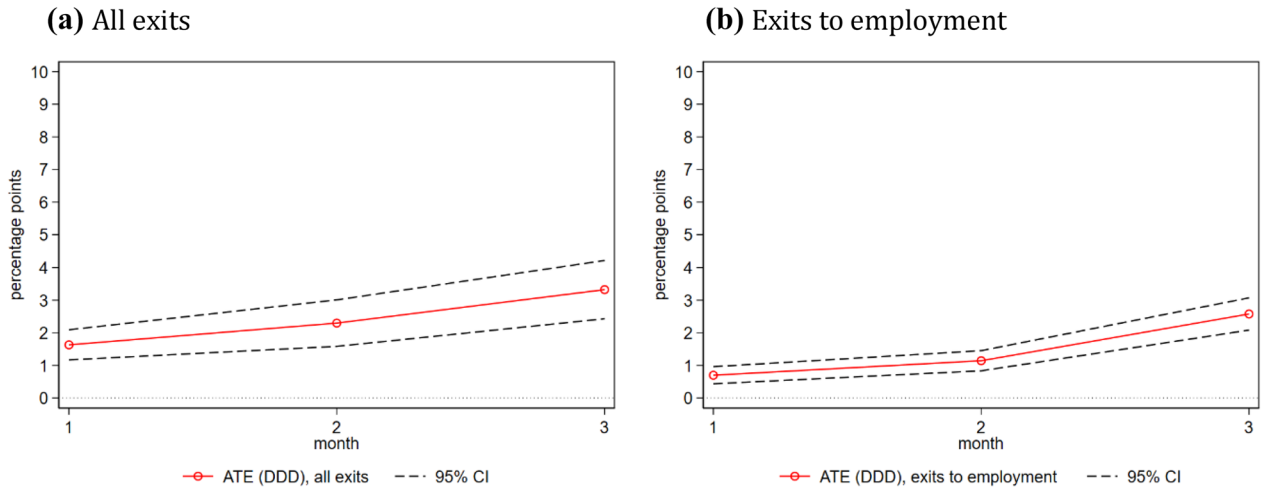

Fig. 4 DDD estimates of the effects of lower caseloads on exits from unemployment. *Source*: AUR, ASSD. *Note*: Unemployment spells by both existing and new clients. Each dot is an estimate from a separate difference-in-differences-in-differences (DDD) estimation with a binary indicator for leaving unemployment (all exits, all exits to employment) within 3 months of entering the counselling zone of the regional public employment office

unemployed people competing for the same jobs. Especially in the short run, nontreated jobseekers could be partially crowded out because vacancies they would otherwise have received are no longer available (see Crepon et al. 2013). Such negative spillover effects should mainly affect the nontreated in the pilot region, because these clients are in the same labour market district.[15]

If such negative displacement effects exist, our comparison of treated and untreated jobseekers in the pilot office overestimates the impact of caseload reduction. The same applies to a second type of possible unintended effect: The experiment may have induced behavioural effects ("Hawthorne effects") in the sense that caseworkers in the pilot office whose caseloads were not reduced showed less effort. Such a response is plausible because the pilot office staff knew about the experiment and may have wanted a general reduction in caseloads. Conversely, however, the experiment could also have had a positive spillover effect if the reduction in caseloads led to the acquisition of more vacancies from which the nontreated also benefited (cf. Hainmueller et al. 2016).

To investigate the possible bias due to spillover effects, we first compare the labour market outcomes of the untreated jobseekers in the pilot office with those of all untreated jobseekers with the same birth month (April–December) from the eleven other employment offices in Vienna. These should be (largely) unaffected by the experiment because they were unaware of it and less likely competed for the same jobs. Any negative externalities of treatment should be reflected in a worse performance of untreated jobseekers in the pilot office. As the pilot office was not selected randomly, we use difference-in-differences (DD) estimation. This means that we compare changes over time to account for unobservable differences between regions that remain constant over time. We estimate the following specification:

$$y_{it} = \alpha_0 + \alpha_1 Location_i + \alpha_2 Period_t + \rho\,(Location_i \times Period_t) + X_i'\beta + \delta_i + \varepsilon_{it}, \quad (2)$$

where y_{it} is the outcome, location is 1 for the pilot office and 0 for the other Vienna offices, and period distinguishes between 2015 (post-treatment period) and 2014 (pre-treatment period). The coefficient ρ of the interaction between location and period reflects the effect of the experiment on the nontreated unemployed in the pilot office.

Our population consists of all persons who were in the counselling zone of the respective regional employment office at the beginning of the year (existing clients) or who entered in the course of the year (new clients). To avoid observing outcomes of the 2014 population in the post-treatment period in 2015, we restrict the sample to jobseekers who entered until the end of September and focus on unemployment exits within 3 months. We compare the frequency of exits from unemployment within 3 months after entering the counselling zone of the regional employment office in the respective year,

[15] Spillovers to jobseekers in other labour market districts of Vienna are less likely, as there is no such direct competition for vacancies. Most of the unemployed are placed in vacancies in the immediate vicinity—especially the less mobile ones, who for example suffer from health limitations or have caregiving responsibilities. Furthermore, the number of treated in the pilot office is very small compared to the total number of unemployed clients in Vienna. They accounted for only 1% in 2015.

observing on the one hand total exits and on the other hand exits into employment. The vector X contains the same large set of control variables that we used in our main effects estimation (specification 1),[16] and δ_i are indicators of the month of entry into the counselling zone. Standard errors are obtained by wild bootstrap, which is especially useful when there are few clusters or few treated clusters (Roodman et al. 2019).

Figure 3 shows the estimated results, on the left total exits from unemployment and on the right exits into employment. We find a modestly higher exit rate from unemployment for the untreated in the pilot office than for the untreated in the other employment offices in Vienna. The difference in the probability of leaving unemployment within 3 months is + 0.7 percentage points, which is statistically significant at the 5% level but very small. The difference in the exit rate to employment is also minimal at + 0.2 percentage points and insignificant. In both cases, the exit rates within 1 or 2 months do not differ significantly either. These results suggest that the reduction in the caseload of the treated in the pilot office had no relevant spillover effect on the nontreated in the pilot office.

In a second step, we examine whether we arrive at a similar estimate of the effect of the caseload reduction if we compare the treated in the pilot office not only with the nontreated in the pilot office, but also with the nontreated jobseekers in the other Vienna employment offices and at the same time make a before-after comparison, i.e. account for various sources of variation. More specifically, we use a difference-in-differences-in-differences (DDD) estimation that includes the following three comparisons: (1) jobseekers born in Jan-March (treated in the pilot office, nontreated in other offices) versus jobseekers born in April-Dec (nontreated), (2) jobseekers in the pilot office versus jobseekers in other Vienna employment offices, and (3) before (2014) versus after the start of the RCT (2015). The comparison between the birth months controls for potential differences in labour market outcomes between these groups, and the before-after comparison is to account for time-constant unobservable differences between the regions.

We estimate the following equation:

$$\begin{aligned} y_{it} = {} & \alpha_0 + \alpha_1 Treatment_i + \alpha_2 Location_i + \alpha_3 Period_t \\ & + \rho_1 (Location_i \times Period_t) + \rho_2 \left(Treatment_i \times Period_p\right) \\ & + \rho_3 (Location_i \times Period_t) + \tau (Treatment_i \times Location_i \\ & \times Period_t) + X'_{it}\beta + \delta_{it} + \varepsilon_{it}, \end{aligned} \tag{3}$$

where y_{it} is the outcome indicator. We again compare the total exit from unemployment and the exit to employment within 3 months of entering the counselling zone. As before, we restrict the observations to those from January to September to avoid observing the outcomes of the 2014 population in the post-treatment period. "*Treatment*" indicates whether an unemployed person was born in January to March, "*Location*" whether they were counselled in the pilot office or in another employment office in Vienna, and "*Period*" whether the observation is from the post-treatment period (2015) or the pre-treatment period (2014). The coefficient of the interaction term τ, which combines the three indicators—the treatment indicator (1 if born in January-March, 0 otherwise), the location indicator (1 if pilot office, 0 otherwise) and the period indicator (1 if 2015, 0 for 2014) –, is our parameter of interest. It measures the impact of the treatment. A positive coefficient indicates that the caseload reduction had, on average, a positive effect on the treatment group.

The vector X contains the same observable characteristics as before and δ_i are again monthly indicators to control for the month of entry into the counselling zone. The estimation relies mainly on two assumptions: (1) that in the absence of the treatment, the outcome trends for the treatment and comparison groups would have followed a parallel trend over time, and (2) that there are no unobserved or unaccounted for shocks or events that simultaneously affect the treatment and the outcome.

Figure 4 shows the results of the DDD-estimation. They are very similar to the results of our main estimation (Fig. 2), the cross-sectional comparison between treated and nontreated in the pilot office (see Fig. 8 in the "Appendix" for an illustration of the difference). The effect on the exit rate from unemployment within 3 months is + 3.3 percentage points, which is 0.8 percentage points lower than in our main estimation. The effect on the exit rate to employment within 3 months is + 2.5 percentage points, which is 0.4 percentage points higher than in our main estimation. In both cases, the coefficients are significant on the 1% level and the difference to the main estimation is small.[17] This suggests that the results we derive from our comparison within the pilot office are valid, even when taking into account potential spillovers on the nontreated unemployed. The underlying assumption is that potential spillovers do not affect the other PES offices in Vienna to a relevant extent. For other possible types of spillovers we cannot control for, such as those at the expense of jobseekers who are not registered as unemployed but are employed and willing to change jobs, or economically inactive and looking for work.

[16] All characteristics are measured at the time of entry into the counselling zone.

[17] In both cases it is within the band of 1.96 times the standard error around the point estimate.

7 Conclusions

We analyse a field experiment in which the caseworker-to-client ratio was improved for randomly selected unemployed persons in an Austrian PES. The results clearly show that the number of caseworkers for the unemployed is an important parameter of labour market policy. We find that reducing the caseload of PES caseworkers leads to more meetings between the unemployed and their caseworkers, more job offers, more programme assignments, and moderately more sanctions. It therefore allows caseworkers to be more active in counselling and placing their clients. Lower caseloads lead to shorter spells of unemployment, both through faster job entry and more exits from the labour market. The unemployed are significantly less often unemployed and more often employed and out of the labour force in the 2 years after entering the RCT.

Lower caseloads reduce unemployment for a variety of subgroups of the unemployed, but do not increase employment for all. For example, for women and foreigners, the intervention only led to significantly more time out of the labour market. Distinguishing between existing clients and new clients entering during the RCT shows a reduction in unemployment for both groups. However, employment transitions increased much more for the new clients. This group is particularly relevant in terms of labour market policy, since it is primarily the new entrants who would benefit from a permanent increase in PES staff.

Overall, our results suggest that more counselling can help unemployed people back into employment. Some of those concerned, possibly those with lower labour market attachment, may respond to stricter job search monitoring and thus to more pressure to take up a job or participate in active measures by leaving the labour force. More people entering employment is clearly a success, but more people leaving the labour market is not. The PES saves wage replacement payments and other resources when clients who are not seriously looking for work deregister. However, individuals lose their unemployment benefits and move further away from work, which is an important precondition for social participation (see for example Gundert and Pohlan 2022). From this perspective, only exits from unemployment that are linked to employment are a success.

The reduction in caseloads increases the probability of taking up a job on average. However, we find no evidence of an effect on post-unemployment job quality as measured by initial wages. A simple comparison of direct costs and benefits suggests that more staff not only reduces unemployment but also public costs, even in the short-term.

We conclude with some comments on the interpretation and classification of the results. First, our analyses are limited to a 2-year post-treatment period. While some of the results, particularly the increase in labour market exits, could be a one-time effect, lower caseloads could lead to benefits that are only realised after this period. In this case, the estimated effects are a lower bound of the overall effect. Second, we have no access to other outcomes which might be influenced by shorter unemployment durations, such as health status or criminal behaviour. Other aspects which could be affected are, for example, medium-term effects of training on the skill mix or changes in motivation and effort.

Third, the experiment might have had displacement effects. Jobseekers who found a job earlier as a result of lower caseloads could have crowded out other jobseekers. We find no spillover effects on the nontreated in the pilot office. However, we cannot rule out that other jobseekers, e.g., unemployed who did not register with the PES, were negatively affected. Fourth, we caution that the regional employment office staff knew about the experiment. This could have been associated with "Hawthorne effects", i.e. behavioural changes that affect the external validity of the results. We cannot rule out that caseworkers showed greater effort because they received increased attention. Similarly, increased effort in the treatment group and reduced effort in the control group is possible from the hope of a permanent increase in staff.

However, behavioural effects are not very likely for several reasons. The focus of the experiment was not the individual performance of the caseworkers but the labour market success of the jobseekers. It did not include any changes in target agreements for the caseworkers that could have been associated with increased monitoring of their performance. The caseworkers in the control group were monitored to the same extent as those in the treatment group. Both groups already had performance incentives set by target agreements before the pilot. Furthermore, we find no differences in labour market success between the nontreated clients of the pilot office and the nontreated clients of other employment offices in Vienna.

Fifth, as is typical of field experiments, we cannot claim the external validity of our results because we derived the effects from an RCT in one of about 100 regional employment offices. It is not certain that an effect of the same magnitude would be achieved if caseloads were reduced across the country. Any intervention takes place in a particular context, which is likely to affect outcomes. In particular, the impact of caseload size may vary depending on local labour market conditions (cf. Hofmann et al. 2010, 2012).

Vienna is a specific labour market because it is by far the largest city in Austria and continues to grow rapidly as it attracts jobseekers from different parts of Austria and abroad, especially those from the nearby eastern EU countries. A relatively large number of immigrants and

low-skilled workers live here. In addition, the share of the service sector in total economic output is high and that of industry low—the structural change is thus more pronounced than elsewhere in Austria. Vienna has been particularly affected by rising unemployment in the past due to high growth in labour supply. The specificity of Vienna may on the one hand limit the generalisability of our results. On the other hand, a large part of the more disadvantaged unemployed live here, and these were the focus of the model project. In this respect, our results could be highly representative for this group.

We do not know how the effects of lower caseloads depend on the overall economic cycle. The pilot project under study took place in a period of weak economic growth and rising unemployment. It is possible that changes in caseloads work differently in better economic times. However, our results are consistent with previous studies that found lower caseloads to be effective in periods of more favourable labour market conditions (Hofmann et al. 2010, 2012; Hainmueller et al. 2016; Ravn and Nielsen 2019). Exploring the influence of economic and institutional conditions remains an avenue for future research. Another subject of future research could be general equilibrium effects that a nationwide introduction of lower caseloads might induce (cf. Hofmann et al. 2012).

Sixth, we emphasise that the design of the experiment does not allow us to disentangle the mechanisms that generated the improved labour market outcomes. We shed light on a number of intermediate outcomes that together appear to have produced the observed labour market effects. However, we cannot quantify the respective contribution of the different factors. More research is needed here too.

Appendix

See Figs. 5, 6, 7, 8 and Tables 7, 8, 9.

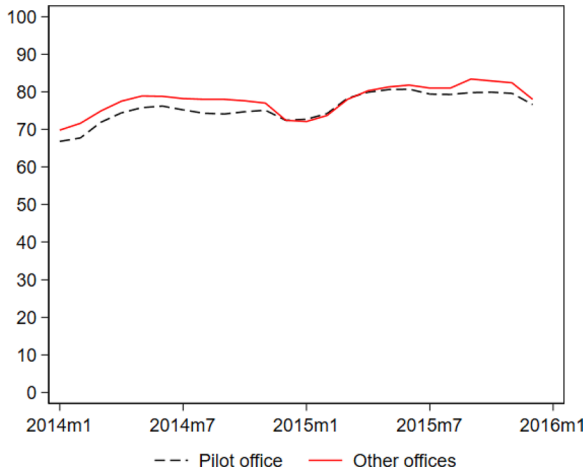

Fig. 5 Share of unemployed (including those in PES training) in the counselling zone (in per cent). *Source*: Official PES data

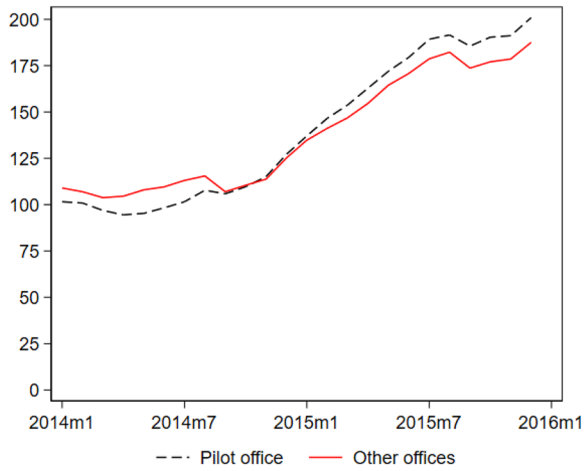

Fig. 6 Average previous unemployment duration of the unemployed (including those in PES training) in the counselling zone (in days). *Source*: Official PES data

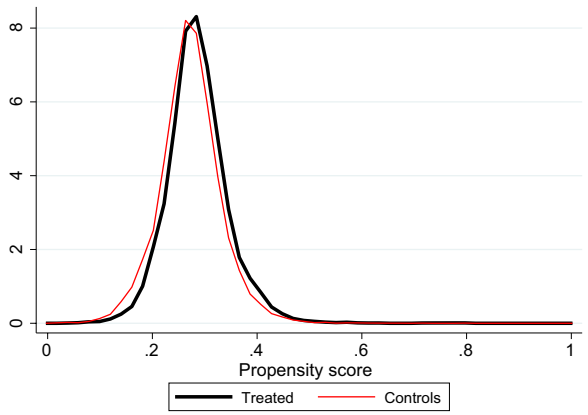

Fig. 7 Propensity score estimated with control variables from OLS regression. *Source*: AUR, ASSD

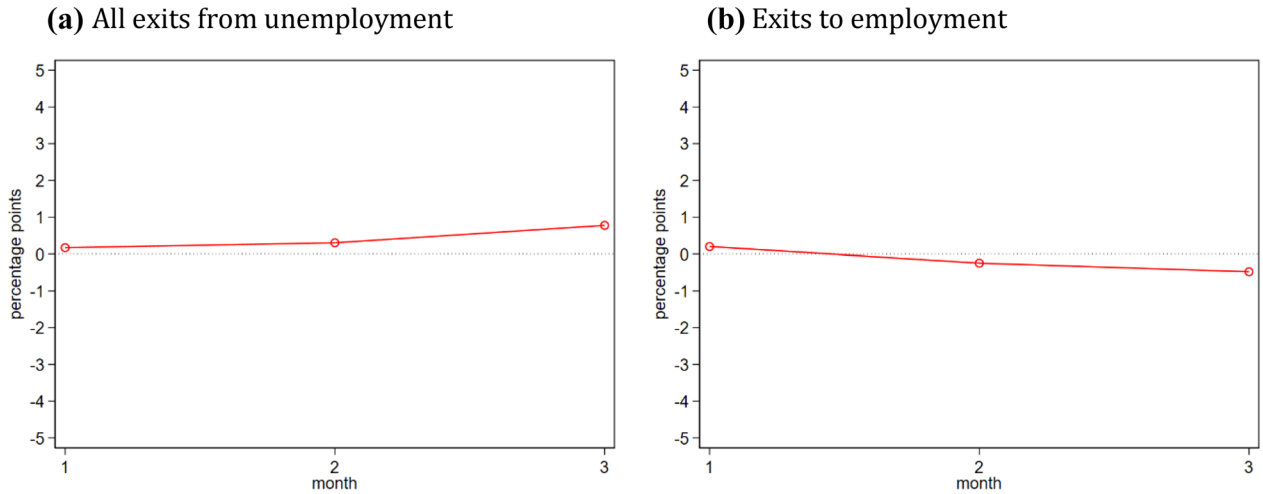

Fig. 8 Differences in treatment effect estimates between main estimate and DDD estimate. *Source*: AUR, ASSD. *Note*: Difference is main estimate minus DDD estimate. Main estimate: Result of the cross-sectional comparison between treated and nontreated in the pilot office via OLS. DDD estimate: Result of the difference-in-differences-in-differences (DDD) estimation. Difference is main estimate minus DDD estimate

Table 7 Summary statistics. *Source*: AUR, ASSD

	Controls	Treated	Difference
New entrant	0.492	0.497	−0.004
Existing client	0.508	0.503	0.004
RCT entry month			
January	0.554	0.557	−0.004
February	0.045	0.038	0.007*
March	0.044	0.039	0.005
April	0.043	0.044	−0.002
May	0.035	0.035	0.000
June	0.042	0.043	0.000
July	0.040	0.039	0.001
August	0.036	0.031	0.004
September	0.047	0.046	0.001
October	0.043	0.040	0.003
November	0.039	0.047	−0.008**
December	0.033	0.042	−0.009**
Female	0.422	0.420	0.001
Age	38.560	38.890	−0.330
Marital status			
Single	0.457	0.453	0.004
Married/registered partnership	0.266	0.276	−0.010
Cohabiting union	0.067	0.066	0.001
Divorced	0.165	0.159	0.006
Married/living apart	0.036	0.037	−0.001
Widowed	0.008	0.009	−0.001
Female, family-related returner to the workforce	0.087	0.094	−0.008
Health restriction			
Legal disability status	0.017	0.014	0.003
Other health-related employment limitation	0.121	0.130	−0.008

Table 7 (continued)

	Controls	Treated	Difference
Education			
At most compulsory school	0.460	0.477	−0.017*
Apprenticeship	0.196	0.192	0.004
Intermediate vocational school	0.042	0.039	0.003
Higher academic or vocational school	0.159	0.160	−0.001
Academic education	0.143	0.132	0.010
Economic sector			
Agriculture, mining	0.003	0.003	0.000
Manufacturing	0.039	0.033	0.006*
Energy and water supply	0.002	0.001	0.001
Construction	0.061	0.068	−0.007
Trade	0.133	0.127	0.006
Transport and logistics	0.040	0.043	−0.003
Accommodation and gastronomy	0.121	0.120	0.001
Information and communication, financial and insurance service provider, real estate and housing	0.061	0.062	−0.001
Freelance, academic, technological services	0.060	0.060	0.000
Other economical service	0.216	0.209	0.007
Public service	0.127	0.116	0.011*
Other services	0.047	0.052	−0.005
Other/unknown	0.090	0.107	−0.017***
Last occupation			
Simple/basic services	0.111	0.115	−0.005
Hospitality sector occupations	0.134	0.131	0.003
Health, teaching, cultural occupations	0.098	0.097	0.001
Mandataries, legal, administrative- and office occupations	0.176	0.160	0.015**
Agriculture and forestry occupations	0.008	0.009	0.000
Manufacturing occupations	0.237	0.248	−0.011
Technical occupations	0.060	0.056	0.004
Transport occupations	0.044	0.048	−0.005
Trade and sales professions	0.127	0.131	−0.004
Unknown	0.006	0.004	0.002
Number of children (only women)			
No child	0.862	0.857	0.005
One child	0.076	0.083	−0.007
Two children	0.042	0.043	−0.002
At least three children	0.021	0.017	0.004
Age of the youngest child			
Up to 2 years	0.015	0.010	0.005**
Between 2 and 7 years	0.041	0.041	0.000
Between 7 and 10 years	0.015	0.016	-0.001
Between 10 and 15 years	0.020	0.023	−0.004
Over 15 years	0.048	0.054	−0.006
Nationality			
Austria	0.649	0.645	0.004
Germany	0.021	0.019	0.003
EU15 (without Austria, Germany)	0.019	0.022	−0.003
Turkey	0.034	0.035	−0.001
Former Yugoslavia (without Slovenia, Croatia)	0.077	0.075	0.002
EU2004-member state	0.057	0.055	0.003
EU2007/2013-member state	0.033	0.029	0.004
Other country	0.109	0.121	−0.011*
Nationalised	0.160	0.161	−0.001

Table 7 (continued)

	Controls	Treated	Difference
Job promise	0.020	0.010	0.010***
Unemployment insurance benefit receipt			
Unemployment benefit	0.363	0.355	0.008
Unemployment assistance	0.426	0.406	0.020**
None	0.212	0.239	−0.028***
Unemployment insurance benefit level (per day in €)			
Up to 20	0.192	0.188	0.004
20–25	0.140	0.127	0.013*
26–30	0.239	0.226	0.013
Over 30	0.216	0.219	−0.003
No benefit	0.212	0.239	−0.028***
Needs-based minimum benefit (BMS)—full receipt	0.016	0.012	0.005*
Needs-based minimum benefit (BMS)—partial receipt	0.025	0.016	0.009***
Previous duration in consultation zone (days)	272.800	263.300	9.503
Zone before RCT entry			
Only counselling zone	0.160	0.183	−0.023***
Counselling zone and other	0.357	0.326	0.031***
Only other	0.346	0.335	0.011
None	0.137	0.155	−0.018**
Zone at unemployment entry			
Service zone	0.681	0.646	0.035***
Counselling zone	0.287	0.328	−0.041***
Other	0.032	0.026	0.005
Previous unemployment duration (days)	361.300	339.400	21.856*
Long-term jobless	0.265	0.246	0.019**
Employment statuses during unemployment episode (days)			
Registered unemployment	263.000	243.200	19.802**
PES training	65.110	61.730	3.382
Apprenticeship search	0.450	0.569	−0.119
Health check	4.019	3.850	0.169
Skilled worker scholarship	0.136	0.460	−0.324**
Other unemployment status	20.940	21.510	−0.572
Dependent employed	2.722	2.896	−0.174
Dependent employed (first labour market)	0.336	0.307	0.029
Dependent employed (second labour market)	1.711	1.566	0.146
Self-employed	0.273	0.454	−0.181**
Retired	0.020	0.019	0.001
Out of labour force	0.031	0.001	0.030
Marginally employed	0.117	0.141	−0.024
Other labour market status	2.366	2.676	−0.310
Time since last employment (days)			
Up to 90	0.548	0.520	0.028***
Between 90 and 180	0.027	0.030	−0.003
Between 180 and 366	0.045	0.049	−0.004
More than 366	0.231	0.241	−0.010
No previous employment	0.149	0.159	−0.010
Income (assessment basis without special payment) in last dependent employment (in €)			
Up to 1000	0.213	0.213	0.000
Between 1000 and 1500	0.235	0.243	−0.008
Between 1500 and 2000	0.165	0.156	0.008
Between 2000 and 2500	0.094	0.086	0.008
More than 2500	0.115	0.116	−0.001
Past labour market statuses (days)			

Table 7 (continued)

	Controls	Treated	Difference
Registered unemployment in past 2 years	268.000	261.500	6.549*
Registered unemployment in past 5 years	507.900	493.700	14.190*
Registered unemployment in past 15 years	953.400	938.900	14.450
PES training in past 2 years	64.130	62.410	1.715
PES training in past 5 years	126.400	119.900	6.450*
PES training in past 15 years	228.100	215.100	12.986**
Other unemployment status in past 2 years	41.800	45.820	−4.019**
Other unemployment status in past 5 years	99.330	106.000	−6.696*
Other unemployment status in past 15 years	181.400	188.100	−6.743
Subsidised dependent employment in 1st labour market in past 2 years	4.925	4.737	0.187
Subsidised dependent employment in 1st labour market in past 5 years	13.160	12.740	0.414
Subsidised dependent employment in 1st labour market in past 15 years	32.530	28.990	3.540
Subsidised dependent employment in 2nd labour market in past 2 years	9.386	8.743	0.643
Subsidised dependent employment in 2nd labour market in past 5 years	17.670	16.630	1.047
Subsidised dependent employment in 2nd labour market in past 15 years	27.060	24.970	2.090
Unsubsidised dependent employment in past 2 years	186.900	181.300	5.616
Unsubsidised dependent employment in past 5 years	547.800	531.200	16.620
Unsubsidised dependent employment in past 15 years	1564.000	1541.000	22.540
Self-employment in past 2 years	11.850	13.020	−1.168
Self-employment in past 5 years	38.710	40.050	−1.339
Self-employment in past 15 years	122.200	134.900	−12.720
Secured OLF-status in past 2 years	16.960	15.260	1.695
Secured OLF-status in past 5 years	54.440	54.700	−0.264
Secured OLF-status in past 15 years	155.200	158.600	−3.468
Marginal employment in past 2 years	11.620	11.570	0.053
Marginal employment in past 5 years	37.380	36.390	0.985
Marginal employment in past 15 years	102.100	101.500	0.638
Sick pay during unemployment in past 2 years	21.520	23.100	−1.581
Sick pay during unemployment in past 5 years	40.050	43.000	−2.946
Sick pay during unemployment in past 15 years	66.880	72.710	−5.826*
Sick pay during employment in past 2 years	0.038	0.020	0.018
Sick pay during employment in past 5 years	0.084	0.063	0.021
Sick pay during employment in past 15 years	0.260	0.325	−0.065
Employed on the reference date			
2 weeks before RCT entry	0.483	0.455	0.028***
3 months before RCT entry	0.504	0.474	0.030***
6 months before RCT entry	0.464	0.440	0.024**
1 year before RCT entry	0.434	0.405	0.029***
2 years before RCT entry	0.408	0.394	0.015
Active labour market policy participation in past 6 months (days)			
Active job search	1.304	1.206	0.098
Vocational orientation	0.924	0.663	0.261*
Training and further education	11.960	12.130	−0.169
Course cost subsidies	1.926	1.404	0.522**
External counselling	16.240	19.550	−3.307***
Private-sector wage subsidies	0.537	0.660	−0.123
Active labour market policy participation in past 2 years (days)			
Active job search	4.694	4.218	0.477*
Vocational orientation	3.686	3.185	0.501*
Training and further education	42.430	43.380	−0.947
Course cost subsidies	9.765	8.584	1.182*

Table 7 (continued)

	Controls	Treated	Difference
External counselling	51.210	60.090	−8.879***
Private-sector wage subsidies	4.398	4.321	0.076
Active labour market policy participation in past 5 years (days)			
Active job search	11.780	12.020	−0.244
Vocational orientation	6.689	5.616	1.074**
Training and further education	82.060	81.070	0.987
Course cost subsidies	20.820	17.980	2.839***
External counselling	88.130	103.300	−15.154***
Private-sector wage subsidies	10.620	9.418	1.205
At least one PES meeting during unemployment episode	0.849	0.822	0.027***
Number of PES meetings during unemployment episode	7.794	7.581	0.214
At least one PES job offer during unemployment episode	0.594	0.556	0.038***
Number of PES job offers during unemployment episode	4.880	4.756	0.124
Observations	9027	3397	

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 8 Effects of lower caseloads on the job placement process by population group. *Source*: AUR, ASSD. Robust standard errors in parentheses

	(1) Full sample	(2) New entrants	(3) Existing clients	(4) Men	(5) Women	(6) Age 15–24	(7) Age 25–49	(8) Age 50–64
Share with meeting (%)	1.7*** (−0.5)	1.4** (−0.6)	2.2*** (−0.8)	1.7*** (−0.6)	2.0** (−0.8)	1.8 (−1.5)	1.9*** (−0.6)	1.8* (−1.0)
Meetings	2.4*** (−0.1)	1.5*** (−0.1)	3.2*** (−0.1)	2.4*** (−0.1)	2.3*** (−0.1)	1.8*** (−0.2)	2.5*** (−0.1)	2.6*** (−0.1)
Meetings per month of treatment	0.3*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.2*** (0.0)	0.3*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.2*** (0.0)
Share with job offer (%)	10.5*** (−0.9)	9.4*** (−1.3)	11.4*** (−1.3)	11.2*** (−1.2)	9.7*** (−1.4)	9.1*** (−2.5)	10.4*** (−1.2)	13.1*** (−1.9)
Job offers	2.6*** (−0.1)	2.0*** (−0.2)	3.3*** (−0.2)	2.9*** (−0.2)	2.3*** (−0.2)	3.1*** (−0.4)	2.8*** (−0.2)	2.0*** (−0.2)
Job offers per month of treatment	0.3*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.5*** (−0.1)	0.2*** (0.0)	0.1*** (0.0)
Share with benefit sanction (%)	3.7*** (−0.6)	3.8*** (−0.8)	3.7*** (−0.9)	4.6*** (−0.9)	2.7*** (−0.8)	2.8 (−2.0)	4.2*** (−0.8)	3.0*** (−1.0)
Report failure	2.6*** (−0.6)	2.6*** (−0.7)	2.6*** (−0.9)	3.2*** (−0.8)	2.0*** (−0.8)	1.2 (−1.9)	3.0*** (−0.7)	2.3** (−1.0)
Job or training refusal	1.3*** (−0.3)	1.0*** (−0.3)	1.6*** (−0.4)	1.7*** (−0.4)	0.7** (−0.3)	2.7*** (−1.0)	1.4*** (−0.4)	0.5 (−0.4)
Share with programme start (%)	9.1*** (−0.9)	6.6*** (−1.3)	11.5*** (−1.4)	10.1*** (−1.2)	7.6*** (−1.5)	5.2** (−2.5)	8.6*** (−1.2)	14.7*** (−2.0)
Job search programme	0.7 (−0.5)	1.0* (−0.6)	0.4 (−0.7)	0.5 (−0.6)	1.0 (−0.7)	−2.0 (−1.8)	0.6 (−0.6)	3.1*** (−0.8)
Vocational orientation	1.0*** (−0.4)	0.8* (−0.5)	1.1* (−0.6)	0.5 (−0.4)	1.8** (−0.7)	0.5 (−0.9)	1.1** (−0.5)	0.9 (−0.6)
Training	2.1*** (−0.7)	0.0 (−0.4)	0.7** (−0.3)	3.2*** (−1.0)	0.5 (−1.2)	1.7 (−2.3)	2.7*** (−1.0)	1.9 (−1.2)
Course subsidies	2.3*** (−0.4)	0.3** (−0.1)	0.2*** (−0.1)	1.6*** (−0.5)	3.1*** (−0.7)	5.1*** (−1.6)	2.0*** (−0.5)	1.2* (−0.7)
Integration subsidy	0.0 (−0.1)	0.2* (−0.1)	−0.2 (−0.2)	−0.1 (−0.2)	0.1 (−0.2)		0.0 (−0.1)	0.0 (−0.4)
Direct job creation	1.0*** (−0.3)	0.3* (−0.2)	1.6*** (−0.5)	1.0*** (−0.3)	1.0** (−0.4)	−0.2 (−0.3)	0.8*** (−0.3)	2.2*** (−0.8)
Non-profit temp agency	0.1 (−0.1)	0.0 (−0.1)	0.2 (−0.2)	−0.1 (−0.1)	0.4* (−0.2)	−0.1 (−0.1)	0.1 (−0.1)	0.4 (−0.4)
External counselling	7.1*** (0.0)	2.2*** (0.0)	1.2*** (0.0)	7.8*** (0.0)	5.8*** (0.0)	1.6 (0.0)	6.8*** (0.0)	12.3*** (0.0)

Table 8 (continued)

	(9) At most compulsory school	(10) Apprentice-ship	(11) Intermediate vocational school	(12) Higher academic or vocational school	(13) Academic education	(14) Disability	(15) No disability	(16) Austrian nationality	(17) Other nationality	(18) Long-term jobless	(19) Short-term jobless
Share with meeting (%)	2.0*** (−0.7)	0.2 (−1.2)	0.1 (−2.9)	2.6* (−1.4)	1.5 (−1.8)	5.2*** (−1.2)	1.4** (−0.6)	1.5** (−0.6)	2.0** (−0.9)	0.7 (−1.0)	2.2*** (−0.6)
Meetings	2.4*** (−0.1)	2.5*** (−0.2)	2.6*** (−0.4)	2.2*** (−0.2)	2.1*** (−0.2)	2.8*** (−0.2)	2.3*** (−0.1)	2.5*** (−0.1)	2.1*** (−0.1)	3.7*** (−0.2)	1.9*** (−0.1)
Meetings per month of treatment	0.3*** (0.0)	0.2*** (0.0)	0.3*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.1*** (0.0)	0.3*** (0.0)
Share with job offer (%)	13.5*** (−1.3)	8.5*** (−2.1)	−3.6 (−5.7)	10.9*** (−2.5)	6.6** (−2.7)	9.8*** (−2.6)	10.5*** (−1.0)	10.7*** (−1.2)	10.1*** (−1.5)	14.5*** (−1.8)	9.4*** (−1.1)
Job offers	2.3*** (−0.2)	3.5*** (−0.4)	2.8*** (−0.8)	3.0*** (−0.4)	2.0*** (−0.4)	1.8*** (−0.3)	2.8*** (−0.2)	2.9*** (−0.2)	2.1*** (−0.2)	4.1*** (−0.3)	2.1*** (−0.2)
Job offers per month of treatment	0.2*** (0.0)	0.3*** (0.0)	0.2*** (−0.1)	0.3*** (0.0)	0.1*** (0.0)	0.1*** (0.0)	0.3*** (0.0)	0.3*** (0.0)	0.2*** (0.0)	0.1*** (0.0)	0.3*** (0.0)
Share with benefit sanction (%)	3.7*** (−0.9)	5.2*** (−1.5)	2.2 (−3.7)	3.2** (−1.4)	2.8** (−1.2)	3.8** (−1.8)	3.7*** (−0.6)	4.9*** (−0.8)	1.6* (−0.9)	5.9*** (−1.4)	3.0*** (−0.7)
Report failure	2.2*** (−0.9)	4.4*** (−1.4)	3.4 (−3.6)	2.5** (−1.3)	2.0* (−1.1)	2.5 (−1.6)	2.6*** (−0.6)	3.8*** (−0.8)	0.6 (−0.8)	4.1*** (−1.3)	2.2*** (−0.6)
Job or training refusal	1.7*** (−0.5)	0.9 (−0.7)	0.1 (−1.3)	0.8 (−0.7)	0.8* (−0.5)	0.6 (−0.7)	1.4*** (−0.3)	1.5*** (−0.4)	0.8* (−0.4)	2.6*** (−0.7)	0.9*** (−0.3)
Share with programme start (%)	9.5*** (−1.4)	8.0*** (−2.2)	6.7 (−5.7)	4.9** (−2.4)	12.6*** (−2.6)	15.3*** (−2.7)	8.3*** (−1.0)	10.0*** (−1.2)	7.8*** (−1.6)	10.9*** (−1.9)	8.7*** (−1.1)
Job search programme	0.1 (−0.7)	0.8 (−1.1)	−0.2 (−2.4)	2.6** (−1.1)	0.1 (−1.3)	2.3* (−1.2)	0.4 (−0.5)	1.2* (−0.6)	0.0 (−0.7)	1.1 (−1.1)	0.4 (−0.5)
Vocational orientation	0.4 (−0.6)	0.8 (−0.8)	4.0* (−2.2)	1.3 (−1.0)	2.8*** (−0.9)	1.8 (−1.4)	0.8** (−0.4)	1.2** (−0.5)	0.8 (−0.7)	1.1 (−0.9)	1.0** (−0.4)
Training	1.5 (−1.1)	2.3 (−1.6)	5.4 (−4.8)	0.8 (−1.9)	4.9** (−2.2)	3.3** (−1.7)	1.8** (−0.8)	2.4*** (−0.9)	2.0 (−1.4)	1.0 (−1.5)	2.5*** (−0.9)
Course subsidies	1.8*** (−0.6)	1.7* (−0.9)	7.3** (−3.2)	3.3*** (−1.3)	0.7 (−1.3)	1.0 (−1.1)	2.4*** (−0.5)	2.4*** (−0.6)	1.9*** (−0.7)	2.1** (−0.9)	2.2*** (−0.5)
Integration subsidy	0.1 (−0.2)	−0.1 (−0.4)		−0.1 (−0.2)	−0.4* (−0.2)	0.1 (−0.5)	0.0 (−0.1)	0.0 (−0.2)	−0.1 (−0.2)	−0.1 (−0.3)	0.0 (−0.1)
Direct job creation	1.3*** (−0.4)	1.3* (−0.7)	−2.1 (−1.4)	1.2 (−0.8)	−0.2 (−0.2)	0.3 (−0.8)	1.1*** (−0.3)	1.1*** (−0.3)	0.9** (−0.4)	2.1*** (−0.8)	0.6** (−0.2)
Non-profit temp agency	0.1 (−0.2)	0.4 (−0.3)	−0.2 (−0.3)	0.0 (−0.3)	0.2 (−0.3)	−0.1 (−0.3)	0.2 (−0.1)	0.1 (−0.2)	0.1 (−0.2)	0.1 (−0.4)	0.1 (−0.1)
External counselling	7.9*** (0.0)	6.5*** (0.0)	2.1 (0.0)	2.0 (0.0)	10.1*** (0.0)	13.6*** (0.0)	6.2*** (0.0)	7.5*** (0.0)	6.3*** (0.0)	10.5*** (0.0)	6.3*** (0.0)

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 9 Robustness of labour market effects to exclusion of persons born in different quarters. *Source*: AUR, ASSD

	(1) Full sample	(2) W/o Q2 and Q4	(3) W/o Q3 and Q4	(4) W/o Q4
Unemployment duration[a]	−62*** (7)	−62*** (9)	−61*** (9)	−61*** (7)
Benefit days[a]	−35*** (5)	−32*** (7)	−33*** (7)	−33*** (6)
Total benefits[b]	−755*** (152)	−630*** (189)	−682*** (190)	−666*** (161)
Employment[a]	16*** (4)	14*** (5)	15*** (5)	15*** (4)
Unsubsidised employment[a]	13*** (4)	11** (5)	13*** (5)	12*** (4)
Unemployment[a]	−36*** (4)	−35*** (5)	−34*** (5)	−35*** (4)
OLF[a]	20*** (4)	21*** (4)	19*** (4)	20*** (4)

(a) Days; (b) Euros. Sample in (2) without all unemployed born in the second and fourth quarter of the calendar year. Sample in (3) without all unemployed born in the third and fourth quarter of the year. Sample in (4) without all unemployed born in the fourth quarter of the year. Effects for both existing and new clients. Robust standard errors in parentheses

***$p < 0.01$, **$p < 0.05$

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Author contributions
All authors jointly developed the research question and design. Rainer Eppel took the first steps in various data processing. All three authors were involved in developing the empirical approach and contributed to the writing of the text.

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Availability of data and materials
The data that support the findings of this study are available from the Austrian Federation of Social Insurances and the Austrian Employment Agency (AMS), but restrictions apply to the availability of these data, which were used under licence for the current study, and so are not publicly available. However, we will, of course, comply with anyone wishing to replicate our results.

Declarations

Competing interests
The authors have no competing interests.

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