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IZA DP No. 11040

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ABSTRACT

The Role of Caseworkers in Unemployment Insurance: Evidence from Unplanned Absences*

Caseworkers are the main human resource used to provide social services. This paper asks if, and how much, caseworkers matter for the outcomes of unemployed individuals. Using large-scale administrative data, I exploit exogenous variation in unplanned absences among Swiss UI caseworkers. I find that individuals who lose an early meeting with their caseworker stay on average 10 days longer in unemployment (5% relative to the mean). Results show large heterogeneity in the economic value of caseworkers: the effect of a foregone meeting doubles for caseworkers in the highest productivity tercile, while it is zero for caseworkers in the lowest tercile. Finally, absences induce negative spillover effects on the performance of present colleagues, who have to cover additional workload.

JEL Classification: J64, J65, M50

Keywords: unemployment insurance, public human resources, caseworkers

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* I thank Patrick Arni, Marco Caliendo, Arnaud Chevalier, Thomas Dohmen, Benjamin Elsner, Peter Haan, Matthias Heinz, Simon Jäger, Peter Kuhn, Rafael Lalive, Camille Landais, Andreas Lichter, Asako Ohinata, Julia Schmieder and Jeffrey Smith for very valuable suggestions. The paper further benefited from comments received at the European Winter Meeting of the Econometric Society in Edinburgh, the IZA/IFAU Evaluation Conference in Bonn and seminars at the University of Bristol, IZA Bonn and DIW Berlin. I thank the Swiss Secretary of Economic Affairs (SECO) for the data and information provision. Financial support from the German National Academic Foundation is gratefully acknowledged.

1 Introduction

Modern welfare states rely on the human resources of caseworkers to provide social services. In particular, caseworkers are often charged with the labor market reintegration of individuals enrolled in welfare schemes. In the context of unemployment insurance (UI), many OECD countries expose benefit recipients to regular face-to-face interactions with a caseworker, who supports and monitors the transition back to work (OECD, 2015). While a large literature evaluates the effects of assignments made by caseworkers (c.f. Card et al., 2010, 2015, for a survey on evaluations of active labor market programs),[1] less is known on how individuals in the caseworker profession shape the outcomes of unemployed individuals.

In this paper, I ask if, and how much, the human resources of caseworkers matter for the outcomes of unemployed job seekers. I first estimate how the presence of a caseworker affects, on average, the exit from unemployment. In a second step, I study how the effect differs by the caseworker's rank in the productivity distribution. From a policy perspective, these analyses reveal to which extent welfare states can improve the effectiveness of social services by investing into their human resources.

My research design relies on the incidence of unplanned caseworker absences.[2] Importantly, absences are not analyzed as the intervention of interest, but as a source of exogenous variation in the quantity and quality of caseworker interactions experienced by unemployed individuals: on the one hand, absences reduce the average number of caseworker meetings. On the other hand, they may induce the replacement by a different caseworker in the office. In this case, the unemployed individual can experience a loss or a gain in caseworker quality. I first exploit absences to estimate the return to an additional meeting with the average caseworker, including an analysis of spillover effects on present colleagues. In a second part, I exploit heterogeneity in the absent caseworker's productivity to identify the importance of quality differences.

The study is based on administrative data from the Swiss UI, covering the full population of benefit recipients registered between 2010 and 2012. The data provide high frequency information on all planned and realized caseworker meetings. Unplanned absences can therefore be measured through the incidence of meeting cancellations: when a caseworker cancels all scheduled meetings, she most likely planned to come to work, but was retained by an unexpected incidence.

For identification, I exploit that the exact caseworker-specific timing of an absence is as good as random from the job seeker's perspective. To this end, I condition on caseworker and calendar

[1] Further, an increasing literature uses the stringency of caseworkers or judges as an instrument for individual treatments (e.g. Autor et al., 2017; Bhuller et al., 2017; Dahl et al., 2014; French and Song, 2013; Kling, 2006; **?**).

[2] Related research designs include Jäger (2016), who exploits worker deaths as exogenously determined worker separations, and McVicar (2008), who uses benefit office refurbishments as an exogenous source of variation in the job search monitoring intensity. Herrmann and Rockoff (2012) estimate the effect of teacher absences to study the productivity losses induced by absenteeism.

month fixed effects, excluding time-constant productivity differences between caseworkers and aggregate time shocks from the identifying variation.[3] Conditional on the fixed effects, neither job seeker characteristics nor workload predict the incidence of caseworker absences. Placebo tests further corroborate the approach by demonstrating that future absences do not affect current outcomes.

Results show that individuals remain unemployed longer when their caseworker is absent. An absence-induced loss of one meeting (40% relative to the mean over six months) decreases the probability to exit unemployment within six months by 2.8 percentage points (5% relative to the mean). The unemployment duration increases by 10 days. As about half of meetings foregone due to an absence are replaced by meetings with another caseworker, the estimates are a lower bound to the effect of loosing a meeting without replacement possibilities.

I further test for spillover effects of absences on the performance of present colleagues. To this end, I analyze how variation in the office-specific absence rate (leave-out mean) affects individuals with present caseworkers, conditional on caseworker and month fixed effects. I find that individuals with present caseworkers experience less meetings when the absence rate increases. They further stay unemployed longer, which confirms that exogenous increases in caseworker workload translate into economically relevant changes in outcomes.

In the second part of the analysis, I interact the incidence of caseworker absences with the absent caseworker's productivity at work. Provided that replacements are in expectation performed by the average caseworker in the office, absences of caseworkers with average productivity should not cause any quality effect.[4] If the absent caseworker is, however, more productive than the average, the meeting with a replacement will in expectation cause a quality loss. The reverse applies when the absent caseworker is less productive than the average. I estimate a caseworker's relative productivity at work as her fixed effect on the six-months exit probability of job seekers who are not affected by a long absence. By comparing caseworkers within office $\times$ quarter cells, I hold the working environment constant. The assignment of job seekers to caseworkers is based on availability or on observable job seeker characteristics included in the regression, ensuring that caseworker fixed effects can indeed be interpreted as a measure of productivity.

Results confirm that there are large quality differences between caseworker meetings. Strikingly, absences of caseworkers in the lowest tercile cause a zero net effect: the loss in meeting quantity is offset by a productivity gain due to the replacement by a better caseworker. In turn, absences of caseworkers in the upper tercile, who are in expectation replaced by a worse case-

[3] The use of worker-time specific variation in workplace presence is closely related to work by Mas and Moretti (2009) and Herrmann and Rockoff (2012), who study peer effects in the workplace and the effects of teacher absences on student test scores, respectively.

[4] I do not exploit heterogeneity in the replacement's productivity, as it is potentially endogenous whether and by whom a meeting is replaced.

worker, induce twice the average effect. This strong effect is mostly driven by exits to stable jobs, suggesting that productive caseworkers achieve good job matches.

To understand mechanisms, I explore whether the differential success of caseworkers can be explained by the active labor market programs (ALMPs) they prescribe. Results show that ALMP assignments decrease, on average, in response to caseworker absences. However, the effects hardly vary by caseworker productivity. The success of a caseworker appears to be mostly driven by unobserved personal qualities and counseling styles, rendering the replacement of productive caseworkers difficult.[5] This intuition is in line with findings from other economic contexts. A large literature documents that teacher quality is a central determinant of student performance (e.g., Chetty et al., 2014a,b; Rivkin et al., 2005; Rockoff, 2004; Rothstein, 2010). Further, individual managers have been shown to be central determinants of firm policies (Bertrand and Schoar, 2002) and worker productivity (Lazear et al., 2015). Jäger (2016) finds that firms face difficulties in finding replacements after exogenous worker separations, in particular when human capital is largely firm-specific. In a final analysis, I show that the value of caseworker presence also increases in experience, suggesting that caseworkers accumulate task-specific human capital which decreases their replaceability.

By identifying a strong role of individual caseworkers, I complement the previous literature on counseling in UI systems. Several experimental studies show that personal counseling can increase the exit rate out of unemployment. For instance, Dolton and O'Neill (1996, 2002) provide evidence on the effects of the British Restart program, which combined stricter eligibility rules with an interview at the public employment service (PES). Graversen and Van Ours (2008) and Rosholm (2008) evaluate a Danish activation program, which included both a two week job search program and an intensified contact with the employment service. Hägglund (2011) estimates the anticipation effect of being invited to a meeting at the PES. Maibom et al. (2017) evaluate several Danish experiments, including different combinations of early meetings, and Van Landeghem et al. (2017) estimate the effect of a collective information session followed by a one-on-one interview.[6] All of the evoked studies suggest that the contact with a caseworker can increase unemployment exit. However, they remain agnostic on how individuals in the caseworker profession shape the returns to the contact. Most related, Behncke et al. (2010a) find that "tough" caseworkers are more successful and and Huber et al. (2017) show that this result cannot be explained by ALMP

[5] In addition, it is possible that productive caseworkers target programs to the right individuals. Previous findings show that, on average, caseworkers do not perform well in targeting active labor market programs. Schmieder and Trenkle (2016) use an RDD design to show that caseworkers do not take individual search incentives induced by the duration of benefits into account when assigning treatments. They conclude that caseworkers apply programs in a bureaucratic way. Lechner and Smith (2007) use propensity score matching and find that the payoffs of treatment assignments made by a statistical program exceed those made by caseworkers.

[6] Card et al. (2010, 2015) provide a comprehensive overview on interventions targeted at unemployment benefit recipients.

assignments.[7] My findings show more generally that policy makers can strongly increase the returns to counseling by investing into the human resources of caseworkers.

In addition, this paper adds to the scarce evidence on the micro-foundations of the job matching function and on the role of public institutions. One part of the literature investigates how (unemployed) workers direct their search to a job match (e.g. Blau and Robins, 1990; Holzer et al., 1991; Marinescu and Wolthoff, 2016). A few studies have pointed out that the efficiency of the PES is a central driver of unemployment exits. Using structural estimation techniques, Fougère et al. (2009) and Launov and Wälde (2016) show that a more productive PES increases outflows from unemployment.[8] My results add an additional layer to these findings, by showing that caseworker productivity is an important input in the job matching process.

Finally, my results reveal substantial economic costs of workplace absenteeism. In the U.S., 1.5% of working time was lost in 2016 due to absences (U.S. Bureau of Labor Statistics, 2017). The causal evidence on the costs induced by absences is, however, limited to Herrmann and Rockoff (2012), who find that teacher absences negatively affect student test scores. The results of my paper confirm the notion that worker absences may induce large costs. They further identify negative spillover effects on present workers and point towards a low replaceability of productive workers.

The remainder of the paper is structured as follows: section 2 lays out the institutional context and the data sources. It further shows how absences are measured and assigned to the job seeker's unemployment spell. In section 3, I discuss the conceptual link between caseworker absences and the exit from unemployment. Section 4 presents the empirical analysis on the average effects of caseworker absences. Section 5 decomposes the effect according to the absent caseworker's productivity, and section 6 concludes.

2 Institutions and Data

2.1 Caseworkers in the Swiss UI

In Switzerland, unemployed individuals are entitled to UI benefits if they have contributed for at least six months during the two previous years. To be eligible for the full benefit period, the contribution period extends to 12 to 22 months. The potential duration of unemployment benefits is usually 1.5 years for eligible prime age individuals, but varies by the job seeker's contribution period, age and family situation. The replacement ratio ranges between 70% and 80% of previous

[7] Evidence on the role of the *match quality* between job seekers and their caseworker is given by Behncke et al. (2010b). Using propensity score matching, they show that caseworkers are more successful when sharing common traits with a given job seeker.

[8] Pissarides (1979) and Jung and Kuhn (2014) reach similar conclusions based on theoretical search-and-matching models.

earnings, depending on the individual family situation and the level of past earnings.

To claim benefits, individuals register at the local Public Employment Service (PES) office. As in most OECD countries, the registration is followed by the assignment to a caseworker. According to a survey realized by Behncke et al. (2010a), the most common assignment criteria are caseload, occupation or industry (all mentioned by about 50% of surveyed caseworkers and PES officials) and randomness (mentioned by 24%).

Individuals are obliged to attend regular meetings with their caseworker. This obligation is enforced through the threat of benefit sanctions. On average, there are two caseworker meetings during the first three months of unemployment. The average meeting lasts 40 minutes. During the meetings, caseworkers provide information, counseling and monitoring. They can also assign training programs and refer vacancies.

By default, meetings take place with the assigned caseworker. If the assigned caseworker is unexpectedly absent on the day of a scheduled meeting, the meeting can be canceled, re-scheduled, or replaced by a different caseworker.

2.2 Data and Measurement of Caseworker Absences

Data Sources The empirical analysis is based on individual level data from the Swiss UI register (so-called AVAM and ASAL data), covering the universe of individuals who entered formal unemployment between 2010 and 2012.[9] The data, which are described in detail by Gast et al. (2004), include extensive information on the entry into and exit from formal unemployment, sociodemographics, potential benefit duration as well as employment and unemployment histories. They report the job seeker's public employment service (PES) office and the assigned caseworker, as well as the type and time of different treatment assignments (e.g., training programs or benefit sanctions). Most importantly for the purpose of this paper, I can link the data to all scheduled meetings on the job seeker-caseworker level, with the exact date and time of each meeting. It is further reported whether a scheduled meeting was realized, canceled, re-scheduled or whether the job seeker did not appear at the meeting.

I restrict the analysis to full-time unemployed individuals aged 20-55, who are eligible for UI and not eligible for disability benefits. I drop 1.11% (N=4,351) of observations because the caseworker assigned to the individual never appears in the meeting database, and additional 2.15% (N=8,520) because the caseworker has less than 30 cases.[10] These have a high likelihood of being mis-classified assignments.

The sample used in the empirical analysis contains 382,123 job seekers assigned to 2,269 case-

[9] Data on earlier entry cohorts are available, but do not systematically report unrealized caseworker meetings. As these are essential to measure caseworker absences, I do not include earlier cohorts in the analysis.

[10] Results are robust to modifying this cutoff (c.f. section 4).

workers. Caseworker assignments can be updated during the unemployment spell. As these updates may occur in response to caseworker absences, they are potentially endogenous. I thus retain the assignment made up to one week after the job seeker's entry into unemployment: if the caseworker assignment gets updated during the first week of unemployment, I use the updated assignment. This allows correcting for erroneously made initial assignments.[11]

Table 1 shows summary statistics on how often job seekers interact with a caseworker during different three-months periods of the unemployment spell. In total, job seekers experience about 2.1 meetings during the first three months of unemployment. The meeting intensity decreases over the unemployment spell, down to 1.6 meetings in months 9-12. In particular, meetings with the initially assigned caseworker drop from 1.5 in months 1-3 to 1.0 in months 9-12. In turn, the average number of meetings with a different caseworker increases from 0.6 to 0.8. From anecdotal evidence, it is common practice that caseworkers switch cases after around six months. As only initial caseworker assignments can be considered exogenous to the dynamics of the unemployment spell, I focus on the role of caseworkers during the first six months of unemployment.

[Insert Table 1]

Measurement of Caseworker Absences I exploit the detailed information on scheduled meetings to identify when a caseworker planned to come to work, but was retained by an unplanned incidence. In the data, such absences should translate into a sequence of scheduled, but unrealized meetings.

Therefore, I define an unplanned absence to start on the day during which none of a caseworker's scheduled meetings take place. Unrealized meetings take the status "scheduled", "canceled" or "re-scheduled". The two other possible categories are "realized" or "job seeker did not appear". I apply a conservative approach to ensure that unrealized meetings indeed reflect caseworker absences: I require that during at least two subsequent day entries, at least two meetings were scheduled and not realized.[12] It is highly unlikely that such a sequence of consecutively unrealized meetings by one caseworker is caused by chance. The absence duration is computed as the number of workdays between the first day of the absence and the first day at which the caseworker is reported to conduct a meeting again.

Figure 1 plots a time series of the monthly share of caseworkers who start an unplanned absence. The share fluctuates around 3.5% and peaks during the winter months, most likely reflecting an increased incidence of sickness days. Table 2 shows caseworker-level summary statistics on the number of workdays and the number of absent days. The average caseworker unexpectedly misses

[11] Results are robust to not updating assignments or to using updates made up to week 2 instead (c.f. section 4).
[12] The median caseworker-day cell has two meetings.

2.3% of her workdays. Taken over a year with 230 workdays,this means that, on average, 5.3 workdays are missed due to an unplanned absence. This number appears of reasonable size, as the average Swiss public sector employee missed 63 hours ($\approx$ 7.5 days) per year in 2015 (Swiss Federal Statistical Office, 2016), which also contain anticipated absences (e.g., planned surgeries).

[Insert Figure 1 and Table 2]

To assign absences to the individual unemployment spell, I count the number of workdays during which the job seeker's caseworker was absent in a given three-months period t after entry.[13] Figure 2 illustrates the assignment over the course of the unemployment spell. For instance, the variable $ABS_{j(i),1-3}$ contains the number of days during which caseworker j was absent during months 1-3 after job seeker i's entry (excluding week 1, during which caseworker assignments are allowed to be updated). $ABS_{j(i),t}$ is an intended treatment, as it does not condition on the job seeker's survival in unemployment at the time of the absence. The empirical analysis will scale the effects of the intended treatment to the average treatment effect on job seekers who are still unemployed at the first day of caseworker absence.

The analysis focuses on absences occurring up to month six after unemployment entry. Later absences will be used to run placebo tests. Table 3 reports summary statistics on how job seekers are affected by caseworker absences. The average job seeker's caseworker has 0.45 workdays of unplanned absence during a given three months interval after entry.[14] About 0.9% of job seekers are affected by ten or more workdays of caseworker absence over a given three-months period. Importantly, the largest share of the variation in the exposure to absences comes from within-caseworker variation. This ensures that there exists enough variation to estimate models with caseworker fixed effects.

[Insert Figure 2 and Table 3]

2.3 Caseworker Absences and Unemployment Exit: Raw Data

As a purely descriptive exercise, figure 3 plots the unemployment exit hazard and survival rate over the first 520 days of unemployment.[15] The solid line includes job seekers whose caseworker is absent during at least ten workdays over the first three months after entry into unemployment (0.9%). To avoid dynamic selection, this status is assigned regardless of whether the job seeker

[13] An absence needs to start during period t to be included into the job seeker's treatment status in t.

[14] There are slightly fewer days of absence in months 1-3, as I do not count absences occurring during the first week after unemployment entry.

[15] 520 days is the maximum potential benefit duration in the sample.

is still unemployed at the start of the absence (intention-to-treat). The dashed line includes all other job seekers in the sample.

The graphs reveal that the initial spike of the exit hazard is visibly less pronounced for job seekers with a caseworker absence (panel a). The survival rate (panel b) shows that this decreased probability of early exit goes along with a rather persistent increase in the medium-run probability of surviving in unemployment. Motivated by this descriptive evidence, the following section presents a stylized conceptual framework to discuss how caseworker absences affect the quantity and quality of meetings experienced by job seekers.

[Insert Figure 3]

3 Conceptual Framework

This section provides a simple conceptual discussion on the role of caseworkers in the job search process. In particular, I describe how the variation caused by caseworker absences can inform about quantity versus quality effects of caseworker resources.

Setup I suppose that job finding of individual i depends on resources $c_{j(i)}$, which caseworker j spends on i during the unemployment spell. $c_{j(i)}$ is composed of the number m and productivity $q_j \sim \mathcal{N}(0, \sigma_q^2)$ of meetings with the caseworker:

$$c_{j(i)} = (1+q_j)m = \begin{cases} (1+q_j)\, m^0 & \text{if } A_{j(i)} = 0 \\ (1+\underbrace{\bar{q}_{-j}}_{\approx 0})\, m^1;\ m^1 < m^0 & \text{if } A_{j(i)} = 1 \end{cases} \tag{1}$$

If caseworker j is present at work after being assigned to i ($A_{j(i)} = 0$), i has a fixed number of meetings with j.[16] The meeting quantity affects the job search process through two components: the caseworker-constant component m^0 contains the average content of a meeting, i.e., standard advice and information, but also the potential disutility associated with the obligation of going to a meeting.The second component is the product between m^0 and an additive, caseworker-specific productivity term, $q_j \sim \mathcal{N}(0, \sigma_q^2)$.[17] q_j measures whether a meeting with caseworker j is more or less productive than a meeting with the average caseworker in the office. Variation in q_j can,

[16] In reality, the number of meetings may vary with respect to job seeker characteristics. However, the absence-driven variation in meetings used in the empirical analysis does not depend on job seeker or caseworker characteristics.

[17] The assumption that q_j is about normally distributed reflects the empirical distribution of caseworker productivity.

for instance, stem from differences in job matching skills, counseling techniques or the choice of program assignments.

If the caseworker is absent ($A_{j(i)} = 1$), unemployed individuals do not have any meeting with their assigned caseworker j. Instead, m^1 meetings take place with a caseworker who replaces her absent colleague. As not all meetings are replaced due to transaction costs and capacity constraints, m^1 is lower than m^0. In expectation, replaced meetings have the productivity of the average caseworker present in the office, $\bar{q}_{-j}$, which is close to zero due to the distribution of q_j.[18] Caseworker absences thus cause two effects: (i) a reduction in the average number of meetings, $m^1 - m^0$, and (ii) a loss of the caseworker-specific term $m^0 q_j$.

Therefore, heterogeneity in q_j can be used to reveal the relative importance of quantity versus quality effects: in expectation, individuals whose absent caseworker ranks in the upper productivity tercile experience a quality gain with the replacement. The opposite is true when the absent caseworker ranks in the upper tercile. Absences of caseworkers in the medium tercile induce, in expectation, no quality effect. I do not exploit heterogeneity in the replacement productivity, because whether and by whom a meeting is replaced can be endogenous to the job seeker's situation.

4 Average Effects of Caseworker Absences

The following section estimates the average effect of caseworker absences, without taking into account heterogeneity in caseworker productivity. First, I set up the empirical model and discuss its identifying assumptions. I then present the estimated effects on the exit from unemployment and on the quantity of realized meetings, including a discussion of spillover effects on present colleagues.

4.1 Empirical Model

Estimation Equation (Intention-to-Treat) As discussed in section 2, the main treatment variable of interest, $ABS_{j(i),t}$, contains the number of workdays during which caseworker j is absent over the three-months period t after job seeker i's entry into unemployment. Outcomes y_i include the linear probability to exit unemployment within a given period, the linear duration of unemployment in days and the number of realized caseworker meetings. I estimate the following

[18] The empirical results confirm that the replacement productivity is on average zero and is unrelated to the absent caseworker's productivity. Nevertheless, some job seekers may receive caseworkers with a positive or negative additive productivity as an replacement. It is assumed that the effects of replacements are symmetric.

equation using OLS:[19]

$$y_i = \alpha + \rho_j + \delta_{1-3} A_{j(i),1-3} + \delta_{4-6} A_{j(i),4-6} + \lambda_\tau + X_i' \beta + \varepsilon_i \tag{2}$$

$$A_{j(i),t} \in \{ABS_{j(i),t}, \mathbb{1}(ABS_{j(i),t} \geq 10)\}$$

The coefficients of interest δ_t measure the effect of caseworker j's absence occurring in time period $t \in (1-3, 4-6)$ after i's entry into unemployment. The focus is on absences in months 1-3 and 4-6 after entry, because job seekers are most intensely followed by their caseworker during these initial periods of the unemployment spell. Later absences will mainly serve as placebo tests. The treatment variable $A_{j(i),t}$ either includes the linear number of days during which caseworker j was absent in period t ($ABS_{j(i),t}$) or a binary variable which equals one if $ABS_{j(i),t}$ contains ten or more days ($\mathbb{1}(ABS_{j(i),t} \geq 10)$).[20] As $ABS_{j(i),t}$ does not take into account whether individual i is still unemployed when the caseworker becomes absent, δ_t defines an intention-to-treat effect (ITT).

ρ_j contains caseworker fixed effects. As discussed in section 3, ρ_j measures j's additive productivity during workplace presence. It further controls for all time-constant caseworker characteristics and addresses the threat that the caseworker's productivity while at work coincides with the likelihood of an absence. The empirical model thus compares individuals assigned to the same caseworker during workplace presence versus absence. λ_τ contains fixed effects for the job seeker's calendar month of entry into unemployment. It controls for aggregate time shocks (e.g., health-related) which correlate both with the caseworker's probability of being absent and with the job seeker's labor market conditions. X_i features job seeker characteristics, whose summary statistics are reported in appendix table A.1.

Scaling to the Average Treatment Effect on the Treated In the above specification, δ_t estimates an intention-to-treat (ITT) effect of caseworker absences: the variable $ABS_{j(i),t}$ contains the number of caseworker absence days in period t after the job seeker's entry into unemployment. This measure does not take into account whether an individual is still unemployed when the caseworker becomes absent. In turn, the average treatment effect on the treated (ATT) defines the effect of an absence on job seekers who are still unemployed and therefore actually experience the consequences of the absence. As a job seeker's survival in unemployment results from dynamic selection, the ATT cannot be estimated directly. I therefore instrument the actual treatment (=the caseworker becomes absent in period t of the unemployment spell, and the job seeker is still unemployed at the first day of the absence) by the intended treatment (=the caseworker becomes

[19] Results are robust to specifying the exit from unemployment as a proportional hazard (available upon request). Given the large number of estimated fixed effects, I refrain from estimating logit or probit regressions.

[20] Results will show that absences start mattering if they sum to at least ten days over a three-months period t.

absent in period t after the job seeker's entry). Equivalently, this Wald estimator divides the ITT effects by the share of individuals who are unemployed at the first day of the absence.[21] In the results section, I show both ITT and ATT results. In addition, I present effects of absences on the number of realized meetings, to interpret the effects on unemployment exit in terms of foregone caseworker meetings.

4.2 Identification

Equation 2 exploits variation in absences within caseworkers over time. Its identification relies on the assumption that the exact caseworker-specific timing of an absence is as good as random from the job seeker's perspective – conditional on aggregate time effects.[22] In the following, I whether this is an internally valid strategy to study the role of caseworkers.

Composition of Job Seekers The identification strategy requires that the timing of caseworker absences does not respond to the characteristics of assigned job seekers.

Table 4 tests whether a job seeker's exposure to caseworker absences is influenced by her pre-determined characteristics. In panel A, the outcome is the number of days during which the job seeker's caseworker is absent during months 1-3 and 4-6 after entry ($ABS_{j(i),t}$). In panel B, it is the linear probability that $ABS_{j(i),t}$ contains 10 or more workdays. In columns 1 and 4, no caseworkers fixed effects are included in the regression. The reported coefficients suggest that there is some selection of job seekers to frequently absent caseworkers. The selection may be due to spatial correlations between job seeker characteristics and PES-specific absence rates, or due to the endogenous assignment of frequently absent caseworkers to certain job seekers within PES offices. To test the relevance of these two mechanisms, columns 2 and 5 add PES fixed effects. Absences and observed job seeker characteristics no longer correlate. Within offices, frequently absent caseworkers are thus not systematically assigned to certain types of job seekers. After replacing PES fixed effects by caseworker fixed effects in columns 3 and 6, it remains that absences are unrelated to pre-determined characteristics. This supports the assumption that the caseworker-time specific variation in absences occurs independently of job seeker characteristics.[23]

[Insert Table 4]

[21] The resulting ATT estimate represents a lower bound, as job seekers may exit unemployment during an absence and therefore be only partially affected.

[22] The identification strategy is related to Mas and Moretti (2009), who study the effects of coworker (cashier) productivity using within-worker variation in the composition of coworkers over ten-minute intervals. Herrmann and Rockoff (2012), who study the effects of teacher absences on student test scores, use a similar approach, exploiting variation within teachers over school years.

[23] One of the coefficients is marginally significant at the 10% level. Given the large number of estimated coefficients, this is likely attributable to chance.

Caseworker Workload As a direct reflection of changes in the local unemployment rate, changes in workload could induce a non-causal relation between absences and unemployment exit. I test for the relationship between workload and absences by running regressions on the caseworker-month level. For each calendar month τ, I count the new cases assigned to each caseworker, as well as their share of the monthly PES-level inflow. I then assess whether the new workload received in month τ affects the incidence of absences in the following two three-months periods ($\tau + 1$ to $\tau + 3$ and $\tau + 4$ to $\tau + 6$).

In table 5, columns 1 and 3 report results from regressions without caseworker fixed effects. They show that absenteeism is unrelated to the overall PES-level inflow, but negatively correlated with the caseworker's share of this inflow. The correlation is most likely mechanical: caseworkers who are overall less present at work are assigned on average a lower share of the inflow. Indeed, the correlation disappears when caseworker fixed effects are included (columns 2 and 4), supporting the assumption that the caseworker- specific timing of an absence is not induced by workload.

[Insert Table 5]

Unobserved Caseworker-Time Trends: Placebo Test A remaining threat is that unobserved time-varying factors influence both caseworker absences and job seeker outcomes. To test for the existence of unobserved caseworker-time trends, future absences can serve as placebo variables in regressions on current outcomes. For instance, caseworker absences occurring in months 4-6, 7-9 and 10-12 after unemployment entry are placebo absences when regressing on an outcome which realizes within the first three months of unemployment.[24] Placebo tests will be shown in the next section, jointly with the main results.

4.3 Results

In the following, I first present effects of caseworker absences on the exit from unemployment. To scale these effects in terms of foregone caseworker interactions, I then show the effects of absences on the quantity of realized meeting. In an additional analysis, I provide evidence that absent caseworkers exert negative spillover effects on their present colleagues.

Unemployment Exit To start, I analyze whether caseworker absence days affects the exit from unemployment in a linear way. To this end, figure 4 plots effects of the number of absence days occurring in months 1-3 of unemployment. Instead of the linear number of days $ABS_{j(i),t}$, the treatment variable used to estimate equation 2 contains five-days categories (e.g., 1-5 absence

[24]The idea to use future workplace presence/absence as a placebo for current outcomes is taken from Mas and Moretti (2009) and Herrmann and Rockoff (2012).

days, 6-10 absence days etc.). Outcomes are the probability to exit unemployment within six months (panel a) and the duration of unemployment in days (panel b).[25] For both outcomes, it appears clearly that absences influence the exit from unemployment if they accumulate to at least 10 days.[26] It is also visible that a linear specification of absence days would not match the observed effect pattern. In the following analysis, I thus use a binary treatment variable, which equals one if the job seeker's caseworker is absent during at least ten workdays over the three month window t after entry. Appendix table A.2 additionally presents coefficients of the linear variable $ABS_{j(i),t}$.

[Insert Figure 4]

Figure 5 reports the short and medium run effect of being exposed to a caseworker absence which accumulates to ten or more days over a three-months period. In particular, the figure shows how the main effects and placebo estimates (effects of future absences on current outcomes) react to the introduction of caseworker fixed effects and individual covariates. Outcomes are the probability to exit from unemployment within $T = 3$ and $T = 6$ months. The x-axis denotes the three-months periods in which the absences occurred; the y-axis shows the size of the estimated coefficient. As described in section 4.2, ATT effects scale ITT effects by the share of job seekers still unemployed at the first day of a caseworker absence. This share is 0.92 for t=1-3 and 0.67 for t=4-6.

Panels a and b present estimates from regressions without caseworker fixed effects and covariates. Clearly, these estimates appear to be biased, as estimated placebo effects of absences occurring after the outcome period are negative and statistically significant. This suggests a negative correlation between the caseworker's general absence probability and productivity at work. Panels c and d show that caseworker fixed effects are able to address the endogeneity problem, as placebo estimates turn zero. Results remain statistically unchanged after the inclusion of covariates in panels e and f.

Table 6 reports the corresponding effects of the preferred specification with fixed effects and covariates. Panel A presents the ITT effects and Panel B the scaled ATT effects. Panel B of column 1 shows that an absence of ten or more days in months 1-3 decreases the probability to exit within three months by 2.4 percentage points, corresponding to a decrease of 8% relative to the mean. The probability to exit within six months decreases by 2.8 percentage points in response to both absences in months 1-3 and absences in months 4-6 (5% relative to the mean, panel B

[25] I cap unemployment spells at 520 days because this is the maximum potential UI benefit duration in the estimation sample. 12.3% of observations are affected by the cap, as job seekers are not automatically de-registered after benefit exhaustion.

[26] As 92% of job seekers are still unemployed at the first day of absence occurring in the first three months after entry, ATT effects are very close to ITT effects.

of column 2). Column 3 reports no significant effect of early absences on the probability to exit within 12 months, suggesting that job seekers with early caseworker absences catch up later in the spell. As shown in column 4, absences in months 1-3 increase the duration of unemployment by 10 days (5% relative to the mean). Absences of months 4-6 show no significant effect on the duration of unemployment, suggesting that the support by a caseworker matters mostly at early stages of the unemployment spell. This intuition is in line with evidence showing that early policy interventions in UI can have large effects. For instance, Black et al. (2003) find that early announcements of re-employment services exert a substantial threat effect on newly unemployed individuals. Similarly, Bolhaar et al. (2016) show that steering the initial search effort of Dutch welfare recipients increases early exits from unemployment.

Appendix table A.3 documents that the main estimates are invariant to several modifications of sampling choices and variable specifications.

[Insert Table 6]

As an additional analysis, I test in table 7 whether the effects differ by observed job seeker characteristics. The table directly presents ATT estimates of absences during months 1-3 of unemployment. It shows a low degree of heterogeneity in the effects. Very early reactions (effects on exits within three months, columns 1 to 4) are significantly more pronounced among women and individuals with a previous income below the median. This is in line with the literature on active labor market programs, which typically finds stronger reactions for women and individuals of low income potential (Card et al., 2010, 2015). However, columns 5 to 8 show that the heterogeneity fades out over the six months outcome window. It appears that in the medium run, all types of job seekers benefit from the interaction with a caseworker.

[Insert Table 7]

Meeting Quantity To interpret presented effects in terms of foregone caseworker interactions, table 8 reports effects on the number of meetings realized over the first $T = 3/T = 6$ months of unemployment.[27] The number of meetings is normalized by the time spent in unemployment over period T.[28] Figure 6 illustrates the effects and additionally presents placebo estimates. To relate

[27]The number of meetings is used here as a proxy for the degree of interaction between caseworkers and job seekers. Other dimensions are, for instance, the duration of meetings or the time gap between meetings.

[28]I.e., I multiply the number of meetings realized during period T by the share of days the job seeker remained unemployed during T. This normalization addresses the concern that the negative effect of absences on unemployment exit translates into a mechanical positive effect on the number of meetings, as job seekers have more meetings when they are unemployed longer. Results hardly differ when not applying the normalization (available upon request).

to the presented effects on unemployment exit, I focus directly on absences which sum to 10 days or more over three months. Linear effects are shown in appendix table A.4.

Columns 1 and 2 of table 8 report estimated effects on the number of meetings realized between the job seeker and her *assigned* caseworker. As expected, this outcome decreases in response to caseworker absences. Absences of at least ten days during months 1-3 of unemployment induce on average a decrease of 0.62 meetings over the first three months and of 1.12 meetings over the first six months (ATTs). For both outcome windows, this corresponds to a decrease of about 40% relative to the mean.[29] Absences in months 4-6 induce a drop of 0.56 meetings over the first six months (25 % relative to the mean). Given that the previously presented effects on the duration of unemployment were driven by absences in months 1-3, I conclude that job seekers who loose one caseworker meeting (or 40% of the average meeting quantity) over the first six months of unemployment stay unemployed 10 days longer.[30] I interpret this estimate as a local effect, as it is likely that caseworker meetings have decreasing marginal returns.

It is a priori unclear whether job seekers affected by absences have *overall* less caseworker meetings. If there was perfect replacement, the total number of meetings should hardly react to absences. Therefore, columns 3 to 6 decompose the effect into an increased number of replaced meetings and a reduced total number of meetings. Results show that about half of the reduction in meetings caused by absences in months 1-3 is compensated by replacements (columns 3 and 4).[31] The other half translates into a reduction in the total number of meetings (columns 5 and 6). Column 4 shows that only one third of meetings foregone due to absences in months 4-6 are replaced. This suggests that meetings later in the spell are less prioritized by the PES and therefore less likely to be replaced. Altogether, the existence of replacement suggests that the estimated cost of loosing a meeting with the assigned caseworker is a lower bound to its costs in contexts without replacement mechanisms.

Figure 6 reports that placebo estimates on the effects of future absences on current meetings are equal or close to zero, suggesting the absence of any major confounding trend.[32]

[Insert Table 8 and Figure 6]

[29] The fact that the effect is larger for the six months outcome window can have two reasons: on the one hand, the caseworker's absence can reach into months 4-6 if it starts in months 1-3 of the spell. On the other hand, caseworkers returning after an absence may need time to catch up on their meetings.

[30] There is no exclusion restriction that allows testing whether meetings realized during months 1-3 or during months 4-6 drive this effect, as absences in months 1-3 affect the meeting quantity in both time windows.

[31] Replaced meetings are defined as meetings that take place with a different caseworker than the initially assigned one.

[32] Placebo effects on the number replaced meetings over 6 months are marginally significant and negative (Figure 6d). These effects are however economically very small and work into the opposite direction as the main effects, which contain an increase in the number of replaced meetings.

Spillover Effects on the Performance of Present Caseworkers A caseworker's absence may also affect the performance of present colleagues in the office. The direction of spillover effects from caseworker absences is ex ante ambiguous: on the one hand, job seekers with an absent caseworker search less and decrease the competition for available vacancies in the local labor market (Crépon et al., 2013; Lalive et al., 2015).[33] This is expected to cause positive spillover effects on the exit probability of job seekers with present caseworkers. On the other hand, absent caseworkers temporarily increase the workload of their colleagues, who have to jump in as replacements. Therefore, all job seekers potentially receive less attention from their caseworker. From this second mechanism, I expect negative spillover effects.

Table 9 presents spillover effects of caseworker absences. For each job seeker i, I measure as $\overline{ABS}_{oq-j,1-3}$ the average of $ABS_{j(i),1-3}$ among individuals who enter the same PES office o in the same calendar quarter q as i, and who are not assigned to the same caseworker as i (leave-out mean). $\overline{ABS}_{oq-j,1-3}$ is introduced into equation 2. As the equation contains caseworker and calendar month fixed effects, the identifying assumption is that the exact time-office specific variation in the absence rate is quasi-random from the individual job seeker's perspective.

To start, columns 1 to 3 assess whether there are spillover effects of caseworker absences on the number of meetings realized over the first six months of unemployment. Such effects occur if present caseworkers have to replace their absent colleagues and can therefore meet their own cases less frequently.[34] Indeed, column 1 shows that when the average exposure to caseworker absences increases by 1 day, meetings with present colleagues decrease by 0.06 days. This effect is as large as the linear effect of one absence day of the own caseworker. In turn, the reduction in meetings arising from spillover effects is -as expected- not replaced by a different caseworker (column 2). The effect therefore translates directly into a reduction in the total number of meetings (column 3). Columns 4 further shows that spillover effects increase the duration of unemployment. The ratio between the direct effect of $ABS_{j(i),1-3}$ and the spillover effect equals their ratio when the outcomes is the total number of meetings (column 3).

Table 9 further suggests that the spillovers do not bias the estimated effect of $ABS_{j(i),1-3}$, which does not differ from the estimates reported in table A.2. However, the empirical setting does not allow for conclusions on job search externalities that may arise when changing the number of job seeker-caseworker interactions on a larger scale.

[Insert Table 9]

[33] Crépon et al. (2013) show that job search assistance decreases job finding rates among non-treated individuals. Lalive et al. (2015) find that an extension in the potential benefit duration increases job finding rates among unaffected individuals.

[34] The number of meetings is used here as a proxy for the time spent on each case. Other potentially affected dimensions are, for instance, the duration of meetings or the time gap between meetings.

5 Effects by Caseworker Productivity

Having established the average effect of caseworker absences, I now analyze its heterogeneity with respect to caseworker productivity. As discussed in section 3, absences can cause both a reduction in the quantity of caseworker meetings and a change in the quality of realized meetings. Provided that all job seekers receive on average the same type of replacement, the quality effect depends on the absent caseworker's productivity at work. Therefore, heterogeneity in the effects by the absent caseworker's productivity can reveal the relative importance caseworker quality for the effectiveness of counseling. In the following, I first set up the empirical model and then present results for different outcomes.

5.1 Empirical Model

Additive Caseworker Productivity I estimate additive caseworker productivity by means of fixed effects, in the spirit of the commonly used method to measure teacher or manager value added (e.g., Bertrand and Schoar, 2002; Chetty et al., 2014a,b; Lazear et al., 2015). I consider medium-run unemployment exit as the relevant output,[35] and hold other input factors, such as working conditions, the local employment office's resources or the job seeker's characteristics, constant. To this end, I set up the following expression for s_i, job seeker i's linear probability to exit unemployment within six months:

$$s_i = \alpha_c + X_i'\beta_c + \kappa_{o\times q} + \theta_j + \varepsilon_i \tag{3}$$

The estimation is stratified at the level of canton c. Therefore, α_c includes a canton-specific constant term, and β_c measures canton-specific returns to individual covariates X_i.[36] $\kappa_{o\times q}$ contains interacted PES office $\times$ calendar quarter fixed effects.[37] This ensures that caseworkers face the same workplace conditions, office policies and local labor market conditions. θ_j measures the parameter of interest, caseworker j's additive effect on the probability to exit within six months. As I am interested in the interaction between productivity at work and the effect of absences, I want to avoid that job seekers treated by an absence contribute to the estimated productivity θ_j (c.f. the intuition of a leave-out mean). Therefore, the regression is run without job seekers who are affected by at least ten days of caseworker absence in months 1-3 or 4-6 after entry (i.e., for whom $ABS_{j(i),1-3} \geq 10$ or $ABS_{j(i),4-6} \geq 10$).

The criteria for assigning job seekers to caseworkers are mostly regulated by the cantonal au-

[35] Medium-run exit is only one out of many dimensions through which caseworker productivity could me measured. However, as I analyze the effects of absences occurring during the first six months of unemployment, it appears the most relevant one in the context of this paper.

[36] X_i includes the same covariates as in equation 2. Summary statistics are reported in Table A.1.

[37] I use quarter instead of month fixed effects to avoid small cells in PES with low monthly inflow.

thorities. Running regressions at the cantonal level therefore has the advantage of controlling more flexibly for potential influences of observed job seeker characteristics in the assignment process. As discussed in section 2.1, the assignment is often based on caseworker availability. However, observed criteria included in X_i, such as the job seeker's occupation or education level, may also influence the assignment. Appendix figure A.1 shows the densities of estimated caseworker effects $\hat{\theta}_j$ from regressions with and without covariates X_i. As the shape and variance of the two distributions hardly differ, job seeker characteristics appear to have a minor influence on the productivity measure.Also recall from section 4.2 (Table 4) that within PES offices, frequently absent caseworkers are not systematically assigned to certain types of job seekers. This further supports the intuition that there are no sophisticated assignment rules which map job seeker characteristics to caseworkers.

Effects of Caseworker Absences by Productivity Having estimated θ_j as a measure of the caseworker's additive productivity when present at work, I introduce it as a source of heterogeneity into the main equation:

$$y_i = \alpha + \rho_j + \sum_{k=1}^{3} \gamma_k (A_{j(i),1-3} \times Tk_j^{\hat{\theta}}) + \pi_k + \lambda_\tau + X_i'\beta + \varepsilon_i \tag{4}$$

$$A_{j(i),1-3} \in \{ABS_{j(i),1-3}, \mathbb{1}(ABS_{j(i),1-3} \geq 10)\}$$

The heterogeneity analysis focuses on absence days occurring in months 1-3 of the spell ($A_{j(i),1-3}$). Given that more productive caseworkers cause faster exits, the composition of job seekers who are unemployed at the time of the absence risks to vary with $\hat{\theta}_j$. For instance, productive caseworker may in the medium run remain with less employable job seekers. In the first three months after entry, 92% of job seekers are still unemployed at the first day of an absence.[38] Therefore, compositional changes are a minor issue for absences occurring in months 1-3.

As in the previous analysis, $A_{j(i),1-3}$ either contains the linear number of absence days, $ABS_{j(i),1-3}$, or a dummy variable which equals one if $ABS_{j(i),1-3}$ contains ten or more days. $A_{j(i),1-3}$ is interacted with $j's$ tercile $Tk_j^{\hat{\theta}}$ in the office-specific distribution of estimated caseworker productivity $\hat{\theta}_j$.[39]

The equation further includes π_k, the baseline effect of having a caseworker in a given productivity tercile. π_k is needed in addition to caseworker fixed effects ρ_j because some caseworkers work in more than one office during the sample period (269 out of 2269). These caseworkers can have different ranks in different offices. As before, the specification further includes fixed effects for the calendar month of entry, λ_τ, and individual covariates, X_i.

[38] 93% for caseworkers in T1 and T2, 90% for caseworkers in T3.

[39] I.e., I classify caseworkers into terciles depending on their productivity rank within office cells.

5.2 Results

I first estimate how absences of caseworkers in the three productivity terciles affect the exit from unemployment, including a decomposition into different exit destinations. To ensure that the heterogeneous effects can be interpreted as pure quality effects, I the show that absences of caseworkers in the three terciles equally reduce the meeting quantity, and that the productivity of the replacement is constant across terciles. In a final step, I explore whether the usage of active labor market programs explains the heterogeneous effects of absences and assess the role of caseworker tenure.

Unemployment Exit In line with the previous section, I pool individuals affected by ten or more days of absence over months 1-3 after entry into a binary treatment variable. Appendix table A.5 additionally reports linear effects, which show the same pattern in terms of heterogeneity.

Table 10 how the effect of absences on the exit from unemployment interacts with the absent caseworker's productivity tercile. It documents a largely heterogeneous economic value of caseworker presence. Columns 1 and 2 show effects on the probability to exit within six months, estimated without and with covariates. Caseworkers in the second tercile are on average as productive as the replacement – provided that replacements are in expectation performed by the average caseworker in the office. Their absence should thus mostly induce a reduction in the quantity of meetings, due to imperfect replacement. Results show that absences of caseworkers ranking in the second tercile decrease the probability to exit within six months by 2.6 percentage points (column 2). This estimate is close to the average effect reported in the previous section, and corresponds to a change of 5% relative to the group-specific mean. Absences of caseworkers in the lowest productivity tercile show no effect on the exit from unemployment. For job seekers assigned to one of these caseworkers, the absence-induced loss in meeting quantity appears to be offset by a gain in productivity due to the replacement by a better caseworker. On the contrary, absences of caseworkers in the third tercile induce a negative quality effect, as the replacement is less productive. This expresses in a significantly more negative effect on unemployment exit. For instance, the probability to exit within six months decreases by 6.4 percentage points if the absent caseworker ranks in the third tercile (9% relative to the group-specific mean). Results further show that early absences of highly productive caseworkers have a persistent effect: twelve months after entry, the probability to exit is still lowered by 3.2 percentage points (column 3). In terms of the overall unemployment duration, absences induce an increase by 19 days for caseworkers ranking in the third tercile and by 12.5 days for caseworkers ranking in the second tercile (column 4).[40] Absences of caseworkers in the lowest tercile have no effect on the duration. Appendix table

[40] Effects on the unemployment duration are not significantly different between the upper two terciles.

A.6 further decomposes the heterogeneity by quintiles. It shows that the positive productivity effect of absences is mostly driven by caseworkers in the lowest quintile. The negative productivity effect is equally driven by caseworkers in the upper two quintiles.

[Insert Table 10]

Exit Destinations From a policy perspective, it is of interest to decompose the presented effects into different exit destinations: do productive caseworkers foster the job seeker's own search effort, or do they rely on vacancy referrals? Do they generate sustainable job matches? To answer these questions, table 11 presents heterogeneous effects of caseworker absences on the probability to exit towards different destinations within six months.

In columns 1 and 2, the outcome is split into jobs that are found by the job seeker's own effort and jobs that are found through a vacancy referral. This information is recorded by the UI when job seekers de-register from unemployment. Results show that the relative importance of high productivity caseworkers expresses primarily in job finding through the job seeker's own effort. These caseworkers thus appear to be successful in increasing and directing search.

Effects on job stability are reported in columns 3 and 4, where the outcome is split into stable and unstable job finding. A job match is coded as stable if the job seeker stays out of formal unemployment for at least 12 months after exit.[41] Absences of caseworkers in the third tercile show the most negative effect on the propensity to find a stable job match. In turn, exits to unstable jobs react equally to absences of caseworkers in the second and third terciles. This implies that not only the absolute number, but also the share of stable job matches is higher among the most productive caseworkers. This is remarkable, as treatments commonly used in UI - in particular job search monitoring and sanctions- have been shown to lower post-unemployment job stability (e.g., Arni et al., 2013; Petrongolo, 2009). Finally, column 5 shows that exits to non-employment do not react to caseworker absences.

[Insert Table 11]

Quantity of Meetings and Productivity of Replacement The presented heterogeneities can only be interpreted as pure quality effects if absences of caseworkers in the three productivity terciles equally reduce the meeting quantity.[42] Further, the productivity of the replacement needs to be constant with respect to the absent caseworker's productivity.

[41] The data do not report additional dimensions of job quality, such as post-unemployment wages.

[42] As before, the number of meetings is normalized by each job seeker's duration of unemployment over the outcome period.

Columns 1 to 2 of table 12 shows that in all three productivity terciles, absences lead to a loss of roughly one meeting with the assigned caseworker (ATTs in panel B).[43] It is therefore granted to conclude that the effect of one caseworker meeting on the exit from unemployment strongly depends on caseworker quality.

Columns 3 to 4 show that about half of caseworker meetings are replaced in all productivity terciles. Further, columns 5 to 6 report results from regressions in which the difference between a job seeker's replacement productivity and the average PES productivity is regressed on dummies for the absent caseworker's productivity tercile, conditional on PES fixed effects.[44] This regression only includes job seekers who are affected by an absence and who receive at least one replaced meeting during the outcome period. Therefore, the second tercile is the omitted baseline category. Results show clearly that there is no difference in replacement productivity between the three terciles. This holds for both outcome periods (three and six months after entry).

[Insert Table 12]

The Role of Treatment Assignments The strong heterogeneity in the effect of caseworker absences raises questions about the underlying channels. Interactions between caseworkers and job seekers have mostly two components: first, they contain the unobserved counseling process, in which caseworkers motivate job seekers, generate pressure to actively search for work, provide information and give guidance. Second, caseworker meetings can result in observed outcomes, such as the assignment of active labor market programs, the referral of vacancies and the imposition of benefit sanctions due to job search monitoring. In the following, I analyze how the number of treatments assigned over the first $T = 3/T = 6$ months of unemployment reacts to absences of caseworkers in the three productivity terciles.[45] This can shed light on the importance of observed treatments versus unobserved counseling techniques in the caseworker production function. As in the previous analyses, estimations are based on equation 4 and the binary treatment variable equals one if the caseworker is absent during ten or more workdays in months 1-3 after unemployment entry.

Columns 1 and 2 of table 13 present estimated effects on the number of assigned training programs. These programs mostly include job search trainings or skill classes, such as computer or language courses. While there is a jointly significant negative effect of absences on this outcome,

[43] Column 2 reports a slightly stronger effect on meetings with caseworkers in the third tercile for the six months outcome window (difference between γ_1 and γ_3 at the margin of statistical significance). However, the difference is economically too small to explain that absences of caseworkers in the third tercile increase unemployment by 20 days, while absences of caseworkers in the lowest tercile show no effect.

[44] If a job seeker has more than one replacement during the outcome period, I use the average replacement productivity.

[45] I normalize these outcomes in terms of unemployment duration, as previously done for the number of meetings.

the effect does clearly not differ systematically by caseworker productivity. Columns 3 to 6 show that there is a joint negative effect of absences on the referral of vacancies and on the incidence of benefit sanctions. While it appears from eyeballing that the effects are stronger for absences of caseworkers in the third tercile (columns 3 and 5), the difference between the interaction terms is insignificant or at the margin to significance.[46]

Altogether, I cannot find any strong evidence that the use of labor market programs drives differences in caseworker productivity. As a consequence, unobserved counseling qualities appear to render the replacement of productive caseworkers difficult. This result is in line with Huber et al. (2017), who use mediation analysis to show that the success of tough caseworkers cannot be explained by program assignments. My findings further suggest that "being tough", as proxied by the use of sanctions, is not the only determinant of caseworker performance. A large literature has focused on evaluating labor market programs (c.f. the surveys by Card et al., 2010, 2015), finding mixed results. This paper suggests that the counseling style of caseworkers, which is unobserved in traditional data sources, may be a more important driver of early exits from unemployment. From a policy perspective, this implies that the hiring of high quality caseworkers or investments into the counseling style of existing caseworkers may have large payoffs.

[Insert Table 13]

The Role of Caseworker Tenure In a final analysis, I assess the role of task-specific human capital for the performance of caseworkers. Provided that tenure measures the specificity of human capital (c.f. Becker, 1962), the negative effect of absences is expected to increase with tenure if caseworker human capital is task-specific. The UI registers do not provide information on caseworker characteristics. I observe, however, the full population of caseworker-job seeker matches since January 2008 and can therefore construct proxies of tenure and experience.[47]

Table 14 reports how the effects of early caseworker absences on unemployment exit interact with tenure.[48] For each caseworker $\times$ calendar month cell, I measure tenure as the number of prior months since 2008 during which the caseworker was assigned to job seekers (columns 1 and 3). As an alternative measure, I count for each caseworker $\times$ calendar month cell the number of prior cases assigned since 2008 (columns 2 and 4). I then perform median splits within each month cell. The same caseworker can thus have different levels of relative tenure and experience in different calendar months.

Results show that absences of more tenured and experienced caseworkers cause stronger re-

[46] Further note that only 3.9% of all job seekers find a job through a vacancy referral within the first six months of unemployment (c.f. table 11, column 2.

[47] I do not use the years 2008-2009 for the main analysis because the meeting data is incomplete prior to 2010.

[48] The table directly reports ATT effects. ITT effects are available upon request.

ductions in unemployment exit (statistically significant difference in columns 1 and 4). It thus appears that caseworkers hold largely task-specific human capital, implying that more tenured caseworkers are harder to replace in the case of an absence. In line with this intuition, Lazear and Shaw (2008) show that worker productivity strongly increases with tenure. Further, Jäger (2016) finds that if is difficult for firms to replace a long-tenured worker after an exogenously caused job separation.[49]

[Insert Table 14]

6 Conclusion

This paper exploits exogenous variation in unplanned work absences to estimate how caseworkers affect the unemployment exit of job seekers. I identify substantial economic value of caseworkers: reducing the amount of early caseworker interactions by 40% (≈ one meeting) increases the average duration of unemployment by 10 days. Swiss UI benefit recipients receive on average around 3300 CHF benefits per month. According to a naive back-of-the-envelope calculation, the direct value of 40 minutes working time spent by a caseworker (average duration of a meeting) is thus estimated to be around 1100 CHF (≈ 1100 USD).

As an additional core result, the economic value of caseworkers turns out to be largely heterogeneous. Absences of caseworkers in the lowest productivity tercile show no effect. In turn, the average return of a caseworker meeting would double if all caseworkers had on average the productivity of caseworkers in the upper tercile. Additionally, the negative effects of absences are driven by caseworkers with high tenure and experience, suggesting low replaceability of caseworkers with large task-specific human capital.

The results suggest that investments into the human resources of welfare systems can have high economic payoffs. On the *quantity* side, caseload reductions can increase the time spent on each unemployed individual. The spillover analysis showed that individuals stay unemployed longer if their caseworker has to replace absent colleagues, confirming the economic relevance of caseloads. Investments into caseworker *quality* could target the counseling skills of existing caseworkers (e.g., through training) or the selection of individuals attracted by the caseworker profession (e.g., through higher salaries[50]) Further, reducing the number of job separations among caseworkers may help increasing the amount of task-specific human capital. Lazear and Oyer (2013) review

[49] In addition, a large literature documents that job seniority increases wages and makes job separations more costly for workers (e.g., Topel, 1991).

[50] There is a small literature studying the selection of workers into the public service, mostly in the context of developing countries. For instance, Dal Bo et al. (2013) show that changes in posted salaries change the composition of applicants for public service jobs. Ashraf et al. (2016) find that agents attracted to the public service by career concerns have more skills and ambitions than those attracted by purely altruistic motives.

existing evidence on the determinants of productivity in firms, as offered by research in personnel economics. Future research is needed to understand which interventions and personnel policies work to increase caseworker performance in welfare systems.

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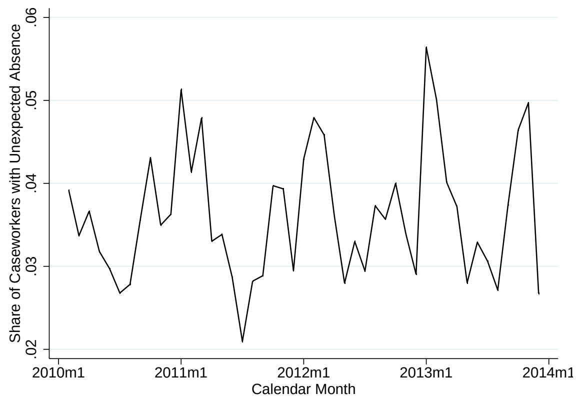
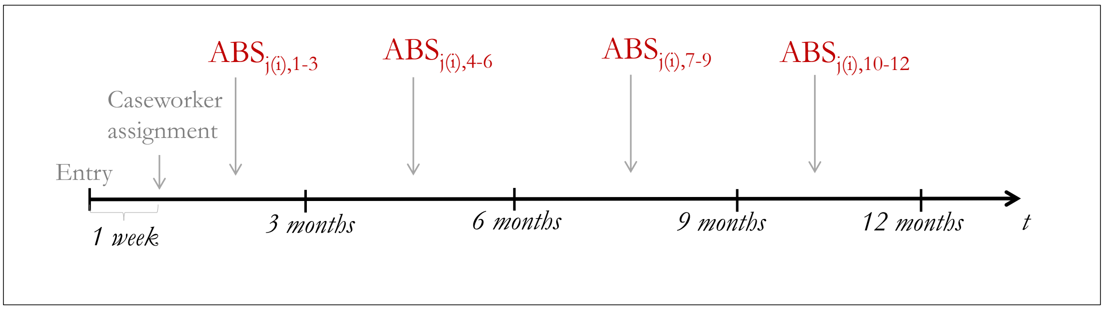
7 Figures

Figure 1: Monthly Share of Caseworkers with unplanned Absence



The figure shows the share of caseworkers who have at least one day of unplanned absence per calendar month. The measurement of unplanned absences based on unrealized caseworker meetings is described in section 2.2.

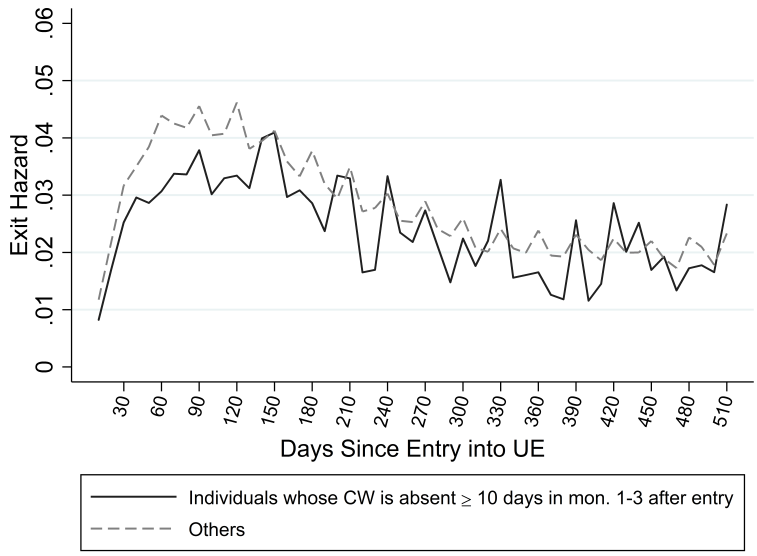
Figure 2: Timeline



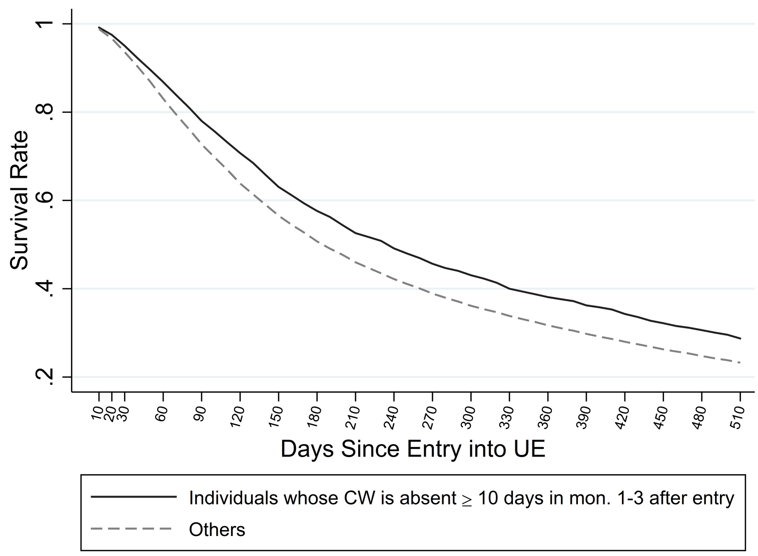
The figure illustrates the assignment of caseworker absences to the job seeker's unemployment spell. $ABS_{j(i),t}$ includes the cumulative number of days during which caseworker j is absent over a three-months period t of job seeker i's unemployment spell.

Figure 3: Caseworker Absences and Unemployment Exit: Raw Data

(a) Unemployment Exit Hazard



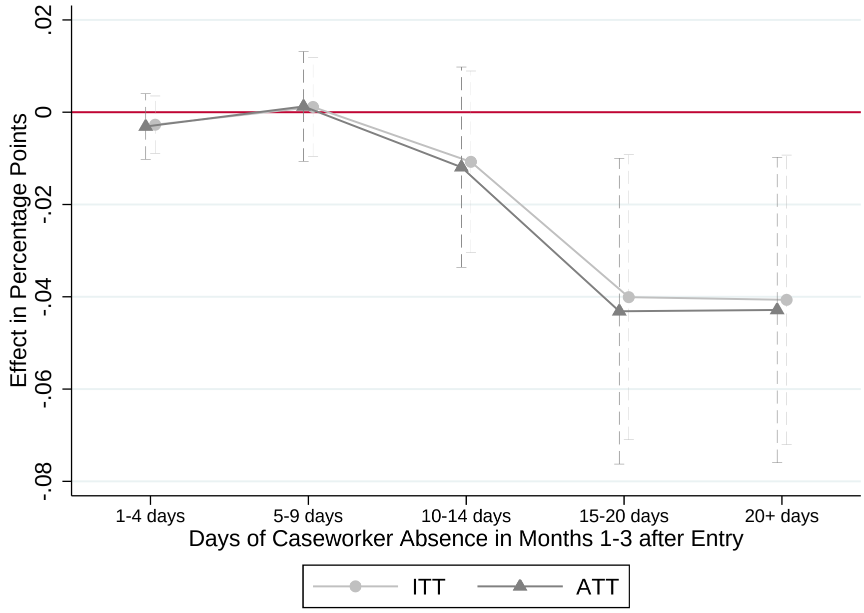
(b) Unemployment Survival Rate



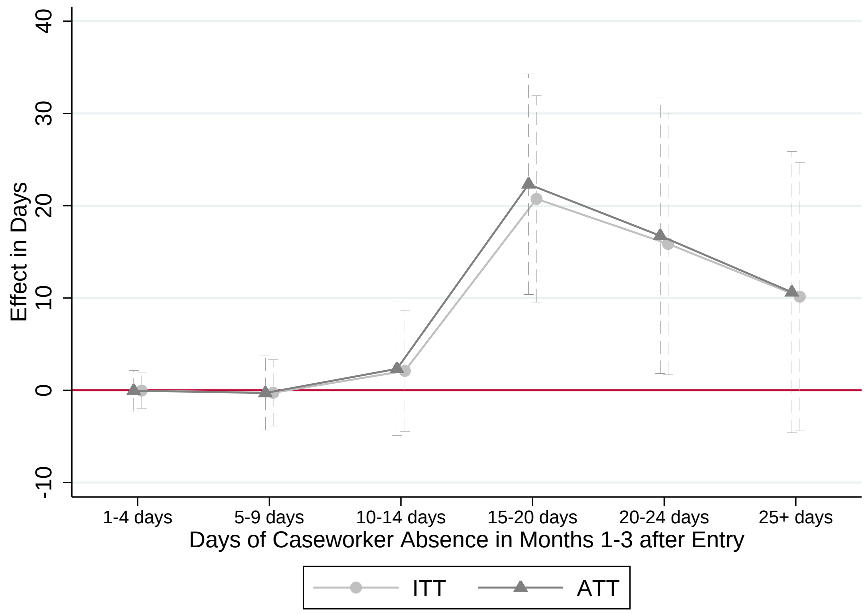
The unemployment exit hazard and the unemployment survival rate are computed over over 10 day intervals. The solid line refers to job seekers whose caseworker is absent during at least 10 workdays over the first three months after entry into unemployment (0.9%). This status is independent of whether the job seeker is still unemployed at the time of the absence (intention-to-treat). The dashed line refers to all other job seekers. N=382123.

Figure 4: Effect of Caseworker Absences in Months 1-3 on the Exit from Unemployment

(a) P(Exit within 6 Months)

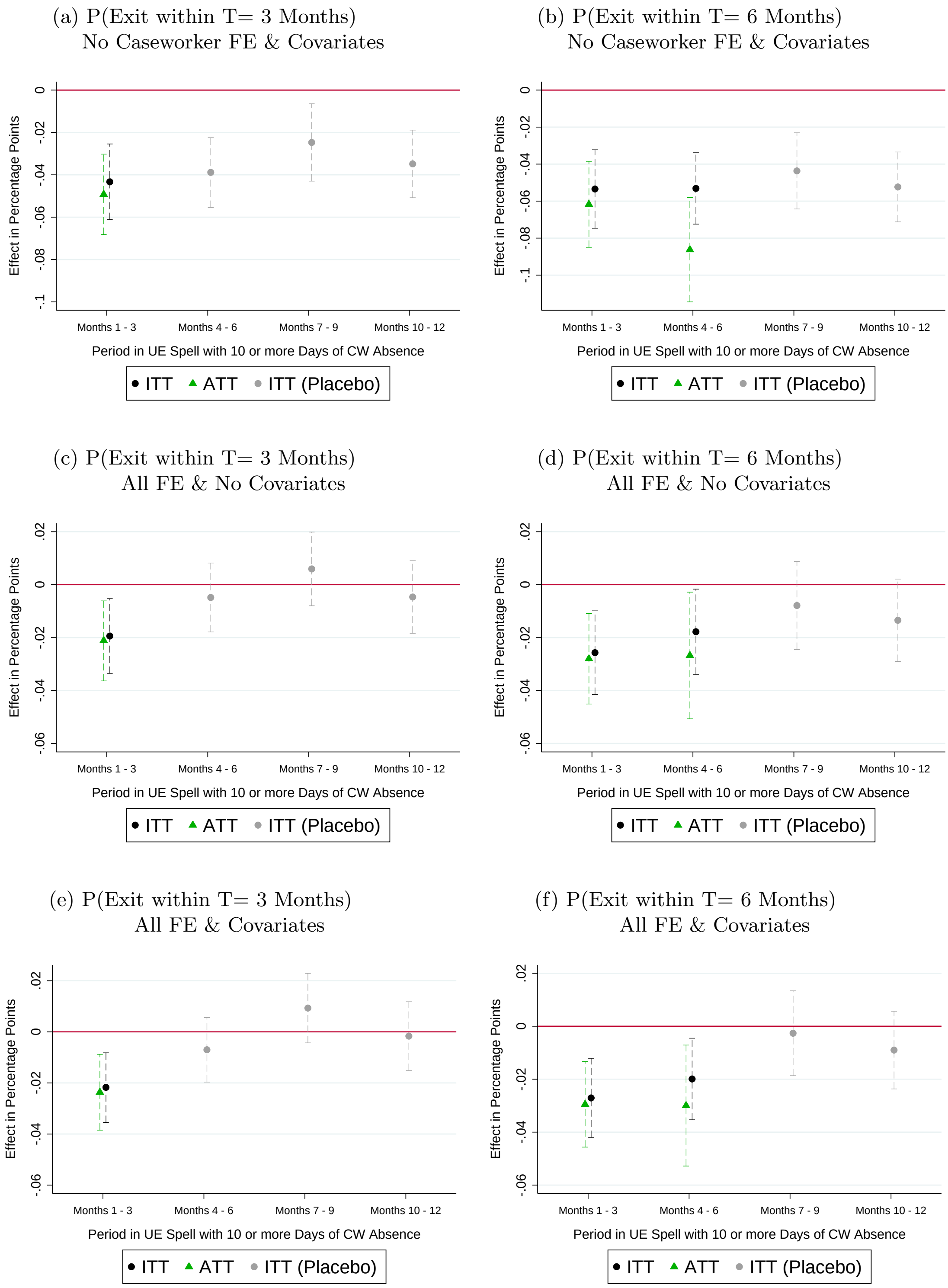


(b) Unemployment Duration in Days



The figure plots the estimated effects of caseworker absences occurring within the first three months after entry. The x-axis denotes the cumulative number of days (in five day categories) during which the caseworker was absent in months 1-3 after the job seeker's entry into unemployment. The reference group contains job seekers with no caseworker absence during this period. The y-axis denotes the size of the estimated coefficient. In panel (a), the outcome is the linear probability of exiting unemployment within six months. In panel (b), the outcome is the linear duration of unemployment in days (capped at 520 days for 12.3% of the sample). Regressions include fixed effects for the job seeker's calendar month of entry into unemployment, caseworker fixed effects and covariates (c.f. equation 2). ATT estimates scale ITT estimates by the share of job seekers still unemployed at the first day of absence (0.92). Dashed lines represent 90% confidence intervals. Further estimation details can be found in section 4. N=382123.

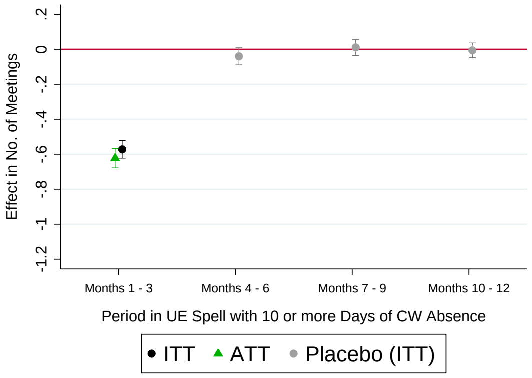
Figure 5: Effects of Absences on the Probability to Exit Unemployment within T Months



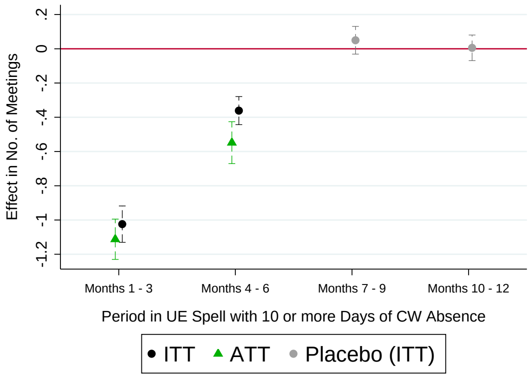
The figure illustrates the estimated effects of absences, based on equation 2. The regressions also estimate placebo effects of absences occurring beyond the outcome window. The treatment variable equals one if the job seeker's caseworker was absent during 10 or more days in the three-months period t after unemployment entry. In panels a and b, fixed effects and covariates are excluded. Panels c and d add fixed effects and panels e and f add covariates. Effects shown in e and f are also reported in columns 1 and 2 of table 6. The x-axis denotes the three-months period in which the caseworker absence occurred. The y-axis denotes the size of the estimated coefficient. ATT estimates scale ITT estimates by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). Dashed lines represent 90% confidence intervals. N=382123.

Figure 6: Effects of Caseworker Absences on the Number of Meetings Realized Within T Months (Illustration of Results from Table 8 and Placebo Tests)

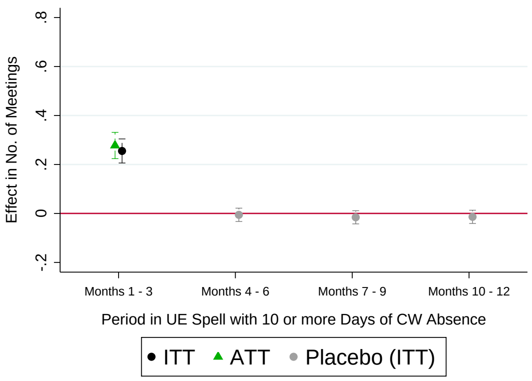
(a) Meetings with Assigned CW, T=3
(Column 1)



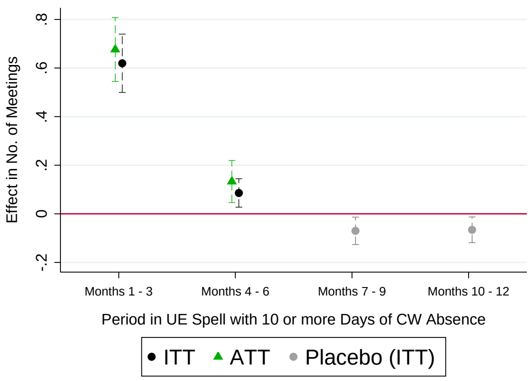
(b) Meetings with Assigned CW, T=6
(Column 2)



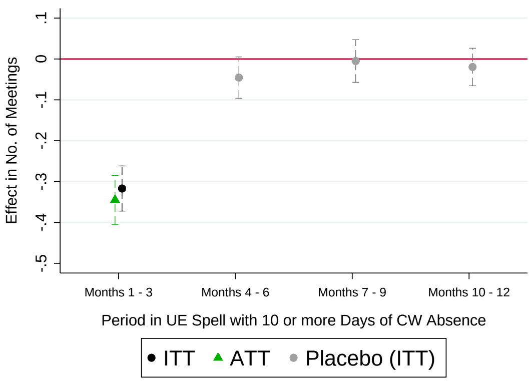
(c) Meetings with Replacement, T=3
(Column 5)



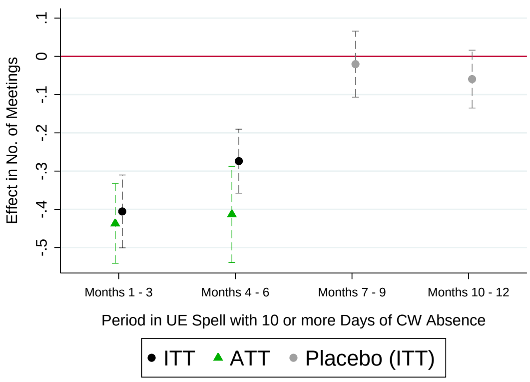
(d) Meetings with Replacement, T=6
(Column 6)



(e) Total No. of Meetings, T=3
(Column 3)



(f) Total No. of Meetings, T=6
(Column 4)



The figure illustrates the estimates reported in table 8. It further reports estimated placebo effects of absences occurring beyond the outcome window. The treatment variable equals one if the job seeker's caseworker was absent during 10 or more days in the three-months period t after unemployment entry. The x-axis denotes the three-months period in which the caseworker absence occurred. The y-axis denotes the size of the estimated coefficient. ATT estimates scale ITT estimates by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). Dashed lines represent 90% confidence intervals. Further estimation details are included in the notes of table 8. N=382123.

8 Tables

Table 1: Summary Statistics on Meetings over the Unemployment Spell

		Period in Unemployment Spell			
Number of Meetings		Months 1-3	Months 4-6	Months 7-9	Months 10-12
In Total	Mean	2.119	1.981	1.912	1.556
	SD	1.280	1.065	1.060	1.135
With Assigned Caseworker	Mean	1.472	1.307	1.113	1.009
	SD	1.307	1.198	1.110	1.196
With Replacing Caseworker	Mean	0.647	0.676	0.736	0.779
	SD	1.160	1.111	1.072	1.028
	N	382123	274698	173942	121175

Summary statistics are at the level of the job seeker. The sample covers job seeker inflows between 2010 and 2012. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period t is multiplied by the share of days a job seeker was unemployed during t. Job seekers are excluded from a given column if they exited unemployment before the start of period t.

Table 2: Summary Statistics on Unexpected Caseworker Absences: Caseworker Level

	Mean	SD	N
Total workdays during sample period	874.450	394.048	2269
Workdays absent during sample period	20.856	19.960	2269
Ratio of absent over total workdays	0.023	0.024	2269

Summary statistics are at the level of the caseworker. The sample covers job seeker inflows between 2010 and 2012. Workdays are the number of days during which caseworkers schedules meetings with job seekers in the sample. The measurement of unexpected absences based on unrealized caseworker meetings is described in section 2.2.

Table 3: Summary Statistics on Unexpected Caseworker (CW) Absences: Job Seeker Level

	Time Period after Entry into UE			
	Months 1-3	Months 4-6	Months 7-9	Months 10-12
Workdays with CW absence				
Mean	0.409	0.474	0.463	0.491
SD	1.870	2.035	2.044	2.197
SD between CW	0.969	1.168	1.077	1.119
SD within CW	1.673	1.804	1.821	1.953
CW absence $\geq$ 10 workdays				
Mean	0.009	0.009	0.010	0.010
SD	0.093	0.097	0.097	0.102
SD between CW	0.041	0.045	0.042	0.044
SD within CW	0.086	0.090	0.091	0.095
N	382123	382123	382123	382123

Summary statistics are at the level of the job seeker. The sample inflow period is 2010-2012. The measurement of unexpected absences is based on unrealized caseworker meetings, as described in section 2.2. The number of caseworker absence days in a given period is reported independently of the job seeker's exit from unemployment at the time of the absence. In months 1-3, the first week after entry is excluded to allow for updates in the assignment of job seekers to caseworkers.

Table 4: Identification Test: Pre-Determined Job Seeker Characteristics and Caseworker Absences

	Absences in Months 1-3 After Entry			Absences in Months 4-6 After Entry		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. Var.: $ABS_{j(i),t}$						
Female	-0.017	-0.003	-0.004	-0.012	0.004	-0.003
	(0.013)	(0.011)	(0.007)	(0.014)	(0.012)	(0.008)
Married	-0.024***	-0.010	-0.010	-0.027***	-0.010	-0.009
	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)
HH size >2	-0.009	-0.005	0.002	0.009	0.013	0.017
	(0.014)	(0.014)	(0.013)	(0.015)	(0.014)	(0.013)
Aged > 40	0.008	-0.000	0.003	0.003	-0.006	-0.007
	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)
Low education	-0.020	0.003	-0.012*	-0.023	0.009	-0.005
	(0.013)	(0.009)	(0.007)	(0.015)	(0.011)	(0.008)
Log previous earnings	-0.011	-0.008	-0.006	-0.014	-0.008	-0.005
	(0.015)	(0.012)	(0.007)	(0.016)	(0.011)	(0.008)
UE in last 12 months	-0.036***	0.002	0.009	-0.040***	0.004	0.006
	(0.011)	(0.009)	(0.007)	(0.012)	(0.009)	(0.007)
PBD>260	0.020**	0.005	0.003	0.018*	0.001	-0.000
	(0.008)	(0.006)	(0.006)	(0.010)	(0.008)	(0.007)
Replacement rate > 75%	0.006	0.009	0.007	-0.003	0.001	-0.001
	(0.009)	(0.007)	(0.007)	(0.010)	(0.008)	(0.007)
Outcome Mean	0.409	0.409	0.409	0.474	0.474	0.474
Panel B						
Dep. Var.: $ABS_{j(i),t} \geq 10$						
(Coeffs Multiplied by 100)						
Female	-0.041	-0.002	-0.016	-0.011	0.038	-0.006
	(0.057)	(0.053)	(0.034)	(0.055)	(0.051)	(0.039)
Married	-0.054	-0.024	-0.034	-0.043	-0.013	-0.018
	(0.040)	(0.038)	(0.035)	(0.038)	(0.037)	(0.036)
HH size >2	0.000	0.000	0.001	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Aged > 40	-0.020	-0.052	-0.042	0.003	-0.027	-0.033
	(0.041)	(0.039)	(0.033)	(0.039)	(0.035)	(0.031)
Low education	-0.038	0.022	-0.019	-0.092*	-0.007	-0.036
	(0.056)	(0.043)	(0.036)	(0.055)	(0.047)	(0.038)
Log previous earnings	-0.066	-0.024	-0.023	-0.039	0.017	0.024
	(0.062)	(0.052)	(0.037)	(0.056)	(0.045)	(0.038)
UE in last 12 months	-0.090*	-0.017	0.009	-0.062	0.023	0.037
	(0.047)	(0.039)	(0.033)	(0.041)	(0.040)	(0.036)
PBD>260	0.071**	0.035	0.025	0.104***	0.059*	0.048
	(0.036)	(0.032)	(0.030)	(0.038)	(0.036)	(0.033)
Replacement rate > 75%	0.022	0.039	0.035	-0.051	-0.034	-0.034
	(0.036)	(0.034)	(0.033)	(0.044)	(0.039)	(0.034)
Outcome Mean	0.009	0.009	0.009	0.009	0.009	0.009
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
PES FE	No	Yes	No	No	Yes	No
Caseworker FE	No	No	Yes	No	No	Yes
N	382123	382123	382123	382123	382123	382123

In panel A, the outcome is the number of workdays during which the job seeker's caseworker is absent during months 1-3/4-6 after entry. In panel B, the outcome is the job seeker's linear probability that her caseworker is absent for at least ten workdays during months 1-3/4-6 after entry. Except for log previous earnings, all independent variables are specified as dummy variables. PBD=potential benefit duration. The unit of observation is the job seeker. In all columns, regressions include fixed effects for the calendar month of entry into unemployment. In columns 2 and 5, regressions also include PES office fixed effects. In columns 3 and 6, regressions also include caseworker fixed effects. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269).

Table 5: Identification Test: Workload and Caseworker Absences

	Absences in Months τ+1-τ+3		Absences in Months τ+4-τ+6	
	(1)	(2)	(3)	(4)
Panel A				
Dep. Var.: $ABS_{j(i),t}$				
New Cases in τ (/10)	0.029	-0.026	0.066	-0.042
	(0.047)	(0.042)	(0.054)	(0.057)
Share of PES Inflow in τ	-26.707***	2.200	-27.211***	7.342
	(4.786)	(4.766)	(5.215)	(5.800)
Outcome Mean	1.503	1.503	1.517	1.517
Panel B				
Dep. Var.: $ABS_{j,t} \geq 10$				
New Cases in τ (/10)	0.001	-0.002	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.003)
Share of PES Inflow in τ	-1.362***	0.146	-1.473***	0.071
	(0.208)	(0.253)	(0.221)	(0.253)
Outcome Mean	0.071	0.071	0.071	0.071
Month FE	Yes	Yes	Yes	Yes
Caseworker FE	No	Yes	No	Yes
N	58029	58029	51679	51679

In panel A, the outcome is the number of workdays during which the job seeker's caseworker is absent during months $\tau + 1$ to $\tau + 3/\tau + 4$ to $\tau + 6$ after entry. In panel B, the outcome is the job seeker's linear probability that her caseworker is absent for at least ten workdays during months $\tau + 1$ to $\tau + 3/\tau + 4$ to $\tau + 6$ after entry. The number of new cases are the number of job seekers that enter in calendar month τ and are assigned to the caseworker. The share of the PES inflow is the ratio of the caseworker's new cases over the total number of new cases at the PES in τ. The unit of observation is the caseworker-month cell. In all columns, regressions include fixed effects for calendar month τ. In columns 2 and 4, regressions additionally include caseworker fixed effects. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269).

Table 6: The Effect of Absences on Unemployment Exit within T Months

	P(Exit), T=3	P(Exit), T=6	P(Exit), T=12	UE Duration in Days
	(1)	(2)	(3)	(4)
Panel A: ITTs				
$ABS_{j(i),1-3} \geq 10$	-0.022***	-0.026***	-0.013	9.234***
	(0.008)	(0.009)	(0.008)	(3.261)
$ABS_{j(i),4-6} \geq 10$		-0.018**	-0.000	3.229
		(0.009)	(0.007)	(2.802)
Panel B: ATTs				
$ABS_{j(i),1-3} \geq 10$	-0.024***	-0.028***	-0.014	10.049***
	(0.009)	(0.010)	(0.009)	(3.526)
$ABS_{j(i),4-6} \geq 10$		-0.028**	-0.000	4.858
		(0.014)	(0.011)	(4.181)
Outcome Mean	0.281	0.541	0.768	217.820
N	382123	382123	382123	382123

$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1 - 3$, 0.67 for $t = 4 - 6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table 7: The Effect of Absences on Unemployment Exit within T Months, Heterogeneity by Job Seeker Characteristics

	P(Exit), T=3				P(Exit), T=6			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATTs								
$ABS_{j(i),1-3} \geq 10$	-0.011	-0.030***	-0.020*	-0.011	-0.029**	-0.026**	-0.023**	-0.028*
	(0.012)	(0.011)	(0.010)	(0.012)	(0.012)	(0.011)	(0.011)	(0.015)
× Female	-0.030*				0.009			
	(0.016)				(0.018)			
$\times Age > 40$		0.021				0.004		
		(0.016)				(0.019)		
× Low Education			-0.014				-0.007	
			(0.019)				(0.020)	
× Low Prev. Earnings				-0.026*				0.006
				(0.015)				(0.021)
Outcome Mean	0.281	0.281	0.281	0.281	0.541	0.541	0.541	0.541
N	382123	382123	382123	382123	382123	382123	382123	382123

$ABS_{j(i),1-3} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. T denotes the outcome period in months. Regressions report instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. ITT effects are available upon request. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Low Education" equals one if the job seeker completed no more than the obligatory level of schooling. "Low Previous Earnings" equals one if the job seeker's pre-unemployment earnings were lower than the median. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table 8: The Effect of Absences on the Number of Meetings Realized over T Months

	No. of Meetings					
	w/ Assigned Caseworker		w/ Replacing Caseworker		In Total	
	T=3	T=6	T=3	T=6	T=3	T=6
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITTs						
$ABS_{j(i),1-3} \geq 10$	-0.567***	-1.031***	0.259***	0.637***	-0.308***	-0.396***
	(0.029)	(0.064)	(0.030)	(0.073)	(0.033)	(0.058)
$ABS_{j(i),4-6} \geq 10$		-0.369***		0.104***		-0.264***
		(0.048)		(0.034)		(0.048)
Panel B: ATTs						
$ABS_{j(i),1-3} \geq 10$	-0.617***	-1.121***	0.282***	0.695***	-0.335***	-0.427***
	(0.032)	(0.071)	(0.033)	(0.080)	(0.036)	(0.064)
$ABS_{j(i),4-6} \geq 10$		-0.561***		0.160***		-0.399***
		(0.072)		(0.050)		(0.072)
Outcome Mean	1.472	2.840	0.647	1.302	2.119	4.140
N	382123	382123	382123	382123	382123	382123

$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table 9: Spillover Effects of Absences

	Number of Meetings (T=6)			P(Exit, T=6)	UE Duration
	w/ Assigned CW	w/ Replacing CW	In Total		
	(1)	(2)	(3)	(4)	(5)
$ABS_{j(i),1-3}$ (ITT)	-0.059***	0.038***	-0.022***	-0.001**	0.415**
	(0.004)	(0.004)	(0.003)	(0.000)	(0.166)
$\overline{ABS}_{oq-j,1-3}$	-0.057***	-0.015	-0.072***	-0.004*	1.541**
	(0.012)	(0.013)	(0.014)	(0.002)	(0.661)
Outcome Mean	2.840	1.302	4.140	0.541	217.820
N	382123	382123	382123	382123	382123

$ABS_{j(i),1-3}$ contains the number of days job seeker's caseworker is absent during period t after the job seeker's entry into unemployment. $\overline{ABS}_{oq-j,1-3}$ contains the office-quarter specific average of $ABS_{j(i),1-3}$, based on job seekers who are not assigned to the same caseworker as i (leave-out mean). The mean of $\overline{ABS}_{oq-j,1-3}$ is 0.42. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Regressions estimate equation 2, adding the spillover variable. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). In column 6, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table 10: The Effect of Absences on Unemployment Exit within T Months, by Tercile of Caseworker Productivity

	P(Exit)			UE Duration
	T=6	T=6	T=12	
	(1)	(2)	(3)	(4)
Panel A: ITTs				
$ABS_{j(i),1-3} \geq 10$ interacted w/:				
$T1(\gamma_1)$	0.010	0.010	0.003	-2.144
	(0.018)	(0.017)	(0.017)	(6.126)
$T2(\gamma_2)$	-0.022	-0.024*	-0.014	11.534**
	(0.015)	(0.014)	(0.013)	(5.063)
$T3(\gamma_3)$	-0.053***	-0.058***	-0.029**	17.258***
	(0.015)	(0.015)	(0.013)	(5.432)
Panel B: ATTs				
$ABS_{j(i),1-3} \geq 10$ interacted w/:				
$T1(\gamma_1)$	0.011	0.011	0.003	-2.295
	(0.019)	(0.018)	(0.018)	(6.589)
$T2(\gamma_2)$	-0.023	-0.026*	-0.015	12.482**
	(0.016)	(0.015)	(0.014)	(5.450)
$T3(\gamma_3)$	-0.059***	-0.064***	-0.032**	19.090***
	(0.017)	(0.016)	(0.014)	(5.923)
Tests (ATTs):				
$\gamma_1 = \gamma_2$ (p-value)	0.170	0.126	0.433	0.085
$\gamma_2 = \gamma_3$ (p-value)	0.128	0.089	0.394	0.411
$\gamma_1 = \gamma_3$ (p-value)	0.006	0.002	0.124	0.015
Joint Sign. (p-value)	0.002	0.000	0.092	0.001
Outcome Mean	0.541	0.541	0.768	217.820
Covariates	No	Yes	Yes	Yes
N	382123	382123	382123	382123

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

Table 11: The Effect of Absences on Unemployment Exit within 6 Months to Different Destinations, by Tercile of Caseworker Productivity

	P(Exit, T=6) to:				
	Job (Own Effort) (1)	Job (Referral) (2)	Job (Stable) (3)	Job (Unstable) (4)	Non-Employment (5)
Panel A: ITTs					
$ABS_{j(i),1-3} \geq 10$ interacted w/:					
$T1(\gamma_1)$	0.012	0.001	0.007	0.005	-0.002
	(0.016)	(0.009)	(0.014)	(0.009)	(0.008)
$T2(\gamma_2)$	-0.008	-0.008	-0.001	-0.016*	-0.007
	(0.013)	(0.007)	(0.013)	(0.008)	(0.008)
$T3(\gamma_3)$	-0.042***	-0.006	-0.036***	-0.012	-0.010
	(0.016)	(0.005)	(0.012)	(0.010)	(0.011)
Panel B: ATTs					
$ABS_{j(i),1-3} \geq 10$ interacted w/:					
$T1(\gamma_1)$	0.013	0.001	0.008	0.005	-0.002
	(0.017)	(0.010)	(0.015)	(0.010)	(0.009)
$T2(\gamma_2)$	-0.008	-0.009	-0.001	-0.017*	-0.008
	(0.015)	(0.008)	(0.014)	(0.009)	(0.009)
$T3(\gamma_3)$	-0.047***	-0.006	-0.040***	-0.013	-0.011
	(0.017)	(0.005)	(0.013)	(0.011)	(0.012)
Tests (ATTs):					
$\gamma_1 = \gamma_2$ (p-value)	0.338	0.442	0.656	0.099	0.652
$\gamma_2 = \gamma_3$ (p-value)	0.089	0.817	0.036	0.785	0.811
$\gamma_1 = \gamma_3$ (p-value)	0.013	0.504	0.014	0.225	0.529
Joint Sign. (p-value)	0.042	0.447	0.018	0.144	0.615
Outcome Mean	0.414	0.039	0.303	0.149	0.074
N	382123	382123	382123	382123	382123

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). The exit destination is obtained from a variable specifying the job seeker's reason of de-registering from unemployment. "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

Table 12: The Effect of Absences on the Number of Meetings Realized over T Months, by Tercile of Caseworker Productivity

	No. of Meetings				Productivity of	
	w/ Assigned Caseworker		w/ Replacing Caseworker		Replacing Caseworker	
	T=3 (1)	T=6 (2)	T=3 (3)	T=6 (4)	T=3 (5)	T=6 (6)
Panel A: ITTs						
$ABS_{j(i),1-3} \geq 10$ interacted w/:						
$T1(\gamma_1)$	-0.519***	-0.702***	0.269***	0.524***	-0.002	-0.003
	(0.052)	(0.099)	(0.045)	(0.094)	(0.007)	(0.006)
$T2(\gamma_2)$	-0.493***	-0.712***	0.272***	0.579***		
	(0.047)	(0.090)	(0.052)	(0.106)		
$T3(\gamma_3)$	-0.566***	-0.775***	0.235***	0.580***	0.003	-0.004
	(0.042)	(0.082)	(0.046)	(0.103)	(0.007)	(0.005)
Panel B: ATTs						
$ABS_{j(i),1-3} \geq 10$ interacted w/:						
$T1(\gamma_1)$	-0.585***	-0.942***	0.287***	0.614***	-0.002	-0.003
	(0.059)	(0.119)	(0.052)	(0.125)	(0.008)	(0.008)
$T2(\gamma_2)$	-0.592***	-1.058***	0.305***	0.737***		
	(0.056)	(0.121)	(0.065)	(0.150)		
$T3(\gamma_3)$	-0.680***	-1.212***	0.252***	0.680***	0.003	-0.004
	(0.049)	(0.107)	(0.053)	(0.131)	(0.007)	(0.005)
Tests (ATTs):						
$\gamma_1 = \gamma_2$ (p-value)	0.923	0.495	0.827	0.530		
$\gamma_2 = \gamma_3$ (p-value)	0.240	0.341	0.523	0.775		
$\gamma_1 = \gamma_3$ (p-value)	0.220	0.095	0.643	0.720	0.611	0.961
Joint Sign. (p-value)	0.000	0.000	0.000	0.000	0.861	0.759
Outcome Mean	1.472	2.840	0.647	1.302	-0.001	-0.001
PES FE	No	No	No	No	Yes	Yes
Caseworker FE	Yes	Yes	Yes	Yes	No	No
N	382123	382123	382123	382123	955	1275

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month fixed effects and job seeker covariates (summary statistics reported in table A.1). In columns 1 to 4, regressions include fixed effects for the job seeker's calendar month of entry into unemployment as well as caseworker fixed effects. Columns 5 to 6 only contain job seekers with $ABS_{j(i),1-3} \geq 10$ who received at least one replaced meeting during the outcome period. The outcome is the difference between the replacement productivity and the average caseworker productivity in the office. In these two columns, regressions include fixed effects for the job seeker's calendar month of entry into unemployment as well as PES office fixed effects. "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10, ^{**} p < 0.05, ^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

Table 13: The Effect of Absences on Treatments Assigned over T Months, by Tercile of Caseworker Productivity

	No. of Trainings		No. of Vacancy Referrals		No. of Benefit Sanctions	
	T=3	T=6	T=3	T=6	T=3	T=6
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITTs						
$ABS_{j(i),1-3} \geq 10$ interacted w/:						
$T1(\gamma_1)$	-0.034*	-0.048*	-0.045	-0.057	-0.026**	-0.018
	(0.018)	(0.026)	(0.077)	(0.099)	(0.013)	(0.021)
$T2(\gamma_2)$	-0.002	-0.014	-0.114***	-0.130**	-0.011	-0.003
	(0.019)	(0.030)	(0.043)	(0.061)	(0.016)	(0.022)
$T3(\gamma_3)$	-0.032*	-0.014	-0.197***	-0.203**	-0.051***	-0.038
	(0.018)	(0.026)	(0.060)	(0.099)	(0.018)	(0.030)
Panel B: ATTs						
$ABS_{j(i),1-3} \geq 10$ interacted w/:						
$T1(\gamma_1)$	-0.037*	-0.066**	-0.069	-0.133	-0.033*	-0.046
	(0.020)	(0.031)	(0.098)	(0.164)	(0.017)	(0.036)
$T2(\gamma_2)$	-0.008	-0.023	-0.173***	-0.286***	-0.018	-0.036
	(0.021)	(0.038)	(0.055)	(0.104)	(0.022)	(0.040)
$T3(\gamma_3)$	-0.042**	-0.049	-0.213**	-0.329*	-0.070***	-0.092*
	(0.020)	(0.032)	(0.091)	(0.191)	(0.023)	(0.047)
Tests (ATTs):						
$\gamma_1 = \gamma_2$ (p-value)	0.325	0.383	0.354	0.432	0.581	0.848
$\gamma_2 = \gamma_3$ (p-value)	0.245	0.608	0.711	0.843	0.096	0.360
$\gamma_1 = \gamma_3$ (p-value)	0.864	0.700	0.278	0.434	0.190	0.445
Joint Sign. (p-value)	0.043	0.066	0.001	0.011	0.003	0.093
Outcome Mean	0.278	0.616	0.537	1.067	0.229	0.472
N	382123	382123	382123	382123	382123	382123

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. In columns 1 and 2, the outcome is the number of trainings (e.g., job application training, computer class, language course) assigned over T months. In columns 3 and 4, the outcome is the number of vacancies referred over T months. In columns 5 and 6, the outcome is the number of benefit sanctions (e.g., due to insufficient search effort) imposed over T months. All outcomes are normalized by the time spent in unemployment over T. To this end, the number of assignments realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

Table 14: The Effect of Absences on Unemployment Exit within T Months, by Caseworker Tenure

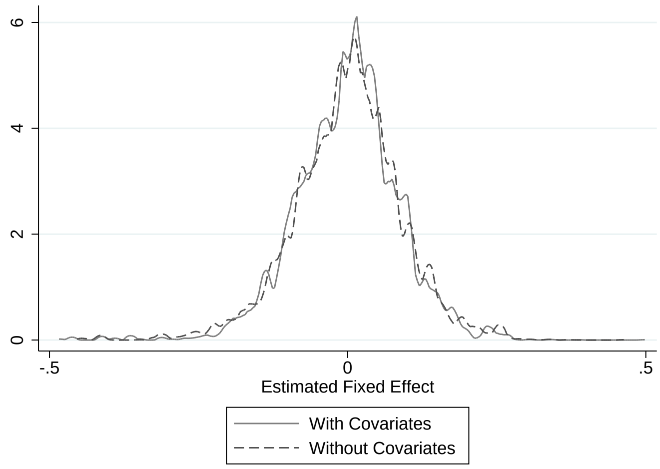
	P(Exit), T=6		UE Duration	
	(1)	(2)	(3)	(4)
ATTs				
$ABS_{j(i),1-3} \geq 10$ interacted w/:				
Tenure $\leq$ Median	-0.000		5.448	
	(0.016)		(5.708)	
Tenure $>$ Median	-0.046***		12.885***	
	(0.012)		(4.279)	
Experience (Prior Cases) $\leq$ Median		-0.007		1.656
		(0.018)		(5.665)
Experience (Prior Cases) $>$ Median		-0.034***		13.185***
		(0.011)		(4.251)
Tests:				
$\gamma_1 = \gamma_2$ (p-value)	0.023	0.202	0.297	0.100
Joint Sign. (p-value)	0.001	0.011	0.007	0.008
Outcome Mean	0.541	0.541	217.820	217.820
N	382123	382123	382123	382123

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. For each caseworker $\times$ calendar month cell, I measure tenure as the number of prior months during which the caseworker was matched to at least one job seeker since 2008 (columns 1 and 3). As an alternative measure, I count for each caseworker $\times$ calendar month cell the cumulative number of prior cases assigned since 2008 (columns 2 and 4). I then perform median splits within each month cell. The same caseworker can thus have different levels of relative tenure and experience in different calendar months. T denotes the outcome period in months. Regressions report instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. ITT effects are available upon request. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Joint Sign." = test for joint significance of the two interaction terms. In columns 3 and 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

A Appendix

A.1 Additional Figures

Figure A.1: Kernel Density of Caseworker FE on P(Exit w/in 6 Months)



N=382123. The distribution is weighted by the number of job seekers per caseworker. Predictions are based on regression of caseworker fixed effects and interacted PES-calendar quarter effects on the job seeker's probability of exiting unemployment within six months (equation 3). The dashed line reports the density of estimated caseworker fixed effects predicted from a regression without job seeker covariates. The solid line reports the density of fixed effects predicted from a regression with covariates. Regressions is preformed at the cantonal level on job seekers who are unaffected by a caseworker absence of 10 or more days in the first or second three-month period of unemployment.

A.2 Additional Tables

Table A.1: Summary Statistics on Job Seeker Covariates

Variable	Mean	Std. Dev.	Min	Max
Female	0.398	0.489	0	1
Age	34.513	9.950	20	55
Age Squared	1290.117	725.860	400	3025
UE in previous 6 mts	0.160	0.367	0	1
UE in previous 12 mts	0.270	0.444	0	1
Additional household members (omitted baseline: 0)				
1	0.191	0.393	0	1
2 to 3	0.185	0.389	0	1
4 and more	0.014	0.117	0	1
Position in last job (omitted baseline: professional or self-empl.):				
Manager	0.049	0.215	0	1
Support	0.312	0.463	0	1
Experience (omitted baseline:>3 years):				
None	0.034	0.181	0	1
< 1 Year	0.085	0.278	0	1
1-3 Years	0.211	0.408	0	1
Missing	0.239	0.427	0	1
Civil status (omitted baseline: single):				
Married	0.386	0.487	0	1
Divorced	0.097	0.296		
Level of Education (omitted baseline: apprenticeship):				
Minimum education	0.231	0.421	0	1
Short further education	0.058	0.234	0	1
High School	0.043	0.203	0	1
Professional diploma	0.031	0.173	0	1
Applied university	0.053	0.224	0	1
University	0.081	0.274	0	1
Missing	0.080	0.272	0	1
Potential benefit duration (omitted baseline: 260-400 days):				
$\leq$90 days	0.043	0.203	0	1
>90, $\leq$ 260 days	0.339	0.473	0	1
>400 days	0.025	0.157	0	1
Replacement rate (omitted baseline: > 80%):				
<75%	0.373	0.484	0	1
75-80%	0.040	0.196		
missing	0.035	0.183	0	1
Domain of occupation in last job (omitted baseline: admin and office):				
Food and raw Materials	0.042	0.200	0	1
Production (blue collar)	0.109	0.312	0	1
Engineering	0.032	0.175	0	1
Informatics	0.024	0.154	0	1
Construction	0.131	0.337	0	1
Sales	0.103	0.304	0	1
Tourism, transport, communication	0.039	0.195	0	1
Restaurant	0.151	0.358	0	1
Cleaning and personal service	0.036	0.186	0	1
Management and HR	0.048	0.213	0	1
Journalism and arts	0.017	0.128	0	1
Social work	0.013	0.114	0	1
Education	0.012	0.110	0	1
Science	0.012	0.109	0	1
Health	0.033	0.178	0	1
Others (skilled)	0.061	0.239	0	1
Previous Earnings in Swiss Francs (omitted baseline:> 3500, $\leq$ 4000)				
$\leq$ 1500	0.046	0.209	0	1
> 1500, $\leq$ 2000	0.027	0.162	0	1
> 2000, $\leq$ 2500	0.037	0.188	0	1
> 2500, $\leq$ 3000	0.053	0.224	0	1
> 3000, $\leq$ 3500	0.089	0.285	0	1
> 4000, $\leq$ 4500	0.120	0.325	0	1
> 4500, $\leq$ 5000	0.124	0.329	0	1
> 5000, $\leq$ 5500	0.105	0.307	0	1
> 5500, $\leq$ 6000	0.076	0.265	0	1
> 6000	0.208	0.406	0	1
N		382123		

Table A.2: The Linear Effect of Absences on Unemployment Exit within T Months

	P(Exit), T=3 (1)	P(Exit), T=6 (2)	P(Exit), T=12 (3)	UE Duration (4)
Panel A: ITTs				
$ABS_{j(i),1-3}$	-0.001	-0.001**	-0.001**	0.426**
	(0.000)	(0.000)	(0.000)	(0.167)
$ABS_{j(i),4-6}$		-0.001	0.000	0.080
		(0.000)	(0.000)	(0.133)
Panel B: ATTs				
$ABS_{j(i),1-3}$	-0.001	-0.001**	-0.001**	0.462**
	(0.000)	(0.001)	(0.000)	(0.180)
$ABS_{j(i),4-6}$		-0.001	0.000	0.121
		(0.001)	(0.001)	(0.198)
Outcome Mean	0.281	0.541	0.768	217.820
N	382123	382123	382123	382123

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table A.3: The Effect of Absences on Unemployment Exit within 6 Months: Robustness of ITT Effects to Sample Modifications

	P(Exit), T=6					
	Baseline (1)	Excluding CW with < 60 Cases (2)	Including All CW (3)	Excluding Abs. ≥ 30 Days (4)	No Update of CW Assignment (5)	2 Week Update of CW Assignment (6)
$ABS_{j(i),1-3} \geq 10$	-0.026***	-0.030***	-0.024***	-0.026***	-0.025***	-0.024***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
$ABS_{j(i),4-6} \geq 10$	-0.018**	-0.016*	-0.019**	-0.017*	-0.016*	-0.016*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Outcome Mean	0.541	0.542	0.543	0.541	0.541	0.542
N	382123	364917	394816	381898	381560	384118

$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). Further estimation details can be found in section 4. In column 2, caseworkers who have less than 60 cases over the sample period are excluded (in the baseline specification, I exclude caseworkers with less than 30 cases). In column 3, all caseworkers are included. In column 4, job seekers affected by absences of 30 or more days during months 1-3 or 4-6 are excluded. In column 6, initially made caseworker assignments are not updated (in the baseline specification, I use updated assignments if the update occurs up to week 1 after the job seeker's entry). In column 6, updated assignments are used if the update occurs up to week 2 after the job seeker's entry. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269).

Table A.4: The Linear Effect of Absences on the Number of Meetings Realized over T Months

	With Assigned Caseworker		With Replacement		In Total	
	T=3	T=6	T=3	T=6	T=3	T=6
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITTs						
$ABS_{j(i),1-3}$	-0.035***	-0.061***	0.016***	0.038***	-0.019***	-0.023***
	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.003)
$ABS_{j(i),4-6}$		-0.020***		0.003*		-0.017***
		(0.003)		(0.002)		(0.003)
Panel B: ATTs						
$ABS_{j(i),1-3}$	-0.038***	-0.066***	0.017***	0.041***	-0.020***	-0.025***
	(0.002)	(0.005)	(0.002)	(0.004)	(0.002)	(0.003)
$ABS_{j(i),4-6}$		-0.030***		0.005**		-0.025***
		(0.004)		(0.003)		(0.004)
Outcome Mean	1.472	2.840	0.647	1.302	2.119	4.140
N	382123	382123	382123	382123	382123	382123

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 4.

Table A.5: The Linear Effect of Absences on Unemployment Exit within T Months, by Caseworker Productivity

	P(Exit)			UE Duration
	T=6	T=6	T=12	T=24
	(1)	(2)	(3)	(4)
Panel A: ITTs				
$ABS_{j(i),1-3}$ interacted w/:				
$T1(\gamma_1)$	0.000	0.000	-0.000	-0.135
	(0.001)	(0.001)	(0.001)	(0.331)
$T2(\gamma_2)$	-0.001	-0.001*	-0.001	0.541*
	(0.001)	(0.001)	(0.001)	(0.278)
$T3(\gamma_3)$	-0.002***	-0.003***	-0.002**	0.913***
	(0.001)	(0.001)	(0.001)	(0.277)
Panel B: ATTs				
$ABS_{j(i),1-3} \geq 10$ interacted w/:				
$T1(\gamma_1)$	0.000	0.000	-0.000	-0.145
	(0.001)	(0.001)	(0.001)	(0.356)
$T2(\gamma_2)$	-0.001	-0.001*	-0.001	0.589*
	(0.001)	(0.001)	(0.001)	(0.301)
$T3(\gamma_3)$	-0.003***	-0.003***	-0.002**	0.996***
	(0.001)	(0.001)	(0.001)	(0.301)
$\gamma_1 = \gamma_2$ (p-value)	0.287	0.194	0.758	0.117
$\gamma_2 = \gamma_3$ (p-value)	0.241	0.236	0.292	0.339
$\gamma_1 = \gamma_3$ (p-value)	0.037	0.021	0.177	0.013
Joint Sign. (p-value)	0.023	0.005	0.053	0.002
Outcome Mean	0.541	0.541	0.768	217.820
Covariates	No	Yes	Yes	Yes
N	382123	382123	382123	382123

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.

Table A.6: The Effect of Absences on Unemployment Exit within T Months, by Quintile of Caseworker Productivity

	P(Exit)			UE Duration
	T=6	T=6	T=12	T=24
	(1)	(2)	(3)	(4)
ATTs				
$ABS_{j(i),1-3} \geq 10$ interacted w/:				
$Q1(\gamma_1)$	0.033	0.034	0.036	-16.151*
	(0.026)	(0.025)	(0.023)	(8.296)
$Q2(\gamma_2)$	-0.018	-0.021	-0.028	11.387
	(0.021)	(0.020)	(0.021)	(7.346)
$Q3(\gamma_3)$	-0.022	-0.019	-0.004	9.648
	(0.022)	(0.020)	(0.015)	(6.966)
$Q4(\gamma_4)$	-0.052**	-0.054**	-0.032*	20.832***
	(0.022)	(0.021)	(0.019)	(7.432)
$Q5(\gamma_5)$	-0.054**	-0.065***	-0.039**	20.098**
	(0.022)	(0.021)	(0.020)	(8.006)
Joint Sign. (p-value)	0.009	0.001	0.050	0.000
Outcome Mean	0.541	0.541	0.768	217.820
Covariates	No	Yes	Yes	Yes
N	382123	382123	382123	382123

Regressions estimate equation 4 using OLS, $ABS_{j,1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Qk equals one if the caseworker ranks in the k^{th} quintile of the productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 4, replacing terciles by quintiles. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 5.