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

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Abstract

Caseworkers are the main human resources 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 a meeting with their caseworker stay unemployed 5% longer. Results show large heterogeneity in the personal impact of caseworkers: the effect of a foregone meeting is zero for caseworkers in the lower half of the productivity distribution, while it amounts to more than twice the average effect for caseworkers in the upper half.

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1 Introduction

Many 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, such as unemployment insurance (UI). Nevertheless, the use of UI caseworker resources varies widely across OECD countries. Many countries, such as France, the United Kingdom, Germany, Switzerland, and the Scandinavian countries, provide regular personal caseworker meetings (OECD, 2015). In turn, caseworkers are less systematically used to support job seekers in the United States. To decide on the use of caseworkers, it is important to understand whether, and how much, the personal interaction with a caseworker matters for the outcomes of unemployed individuals. This question entails two components: first, does the presence of a caseworker foster labor market re-integration? Second, how heterogeneous is the personal impact of individuals in the caseworker profession?

This paper addresses the two questions. I first estimate how the face-to-face interaction with a UI caseworker affects, on average, the time spent in unemployment. In a second step, I study how the effect differs by the caseworker's rank in her office's 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.[1] Importantly, absences are not analyzed as the intervention of interest, but serve 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 use absences as an instrument 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 month fixed effects, excluding time-constant productivity differences between caseworkers and aggregate time shocks from the identifying variation.[2] Conditional on the fixed effects, neither job seeker characteristics nor workload predict the incidence of caseworker absences. Placebo tests further validate the approach by demonstrating that future absences do not affect current outcomes.

Results show that individuals remain unemployed longer when they cannot interact with their caseworker. Losing one meeting (40% relative to the mean over six months) due to an absence

[1] Related research designs include Jäger and Heining (2019), 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.

[2] 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.

increases the unemployment duration by 12 days (5% relative to the mean). As about half of meetings foregone due to an absence are replaced by meetings with another caseworker, the estimate is a lower bound to the effect of losing a meeting without replacement possibilities.[3]

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 outcome changes.

In the second part of the analysis, I interact the incidence of caseworker absences with the absent caseworker's rank in her office's productivity distribution. Provided that replacements are in expectation performed by the average caseworker in the office,[4] losing a meeting with a caseworker in the upper half of the distribution is expected to cause a quality reduction. The reverse applies when the absent caseworker ranks in the lower half: here, job seekers can gain because the replacement is in expectation more productive. I estimate a caseworker's relative productivity at work as her fixed effect on the unemployment duration of job seekers who are not affected by an absence. By ranking caseworkers within offices, 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 caseworkers. Strikingly, absences of caseworkers with lower-than-median productivity cause a zero net effect: the loss in meeting quantity appears fully offset by a productivity gain due to the replacement by a better caseworker. In turn, losing a meeting with a caseworker in the upper half of the distribution increases the unemployment duration by 13%, which is more than twice the average effect. I further find evidence that the meeting with a productive caseworker reduces the recurrence to unemployment. This suggests that caseworkers who are productive in reducing the duration of unemployment also improve the quality of 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 increase, on average, in the number of caseworker meetings. However, the effects hardly vary by caseworker productivity.[5] The success of a caseworker thus appears to be mostly driven by unobserved personal qualities and counseling styles, which are difficult to replace.[6] 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, holding all input fators constant (e.g., Rockoff, 2004; Rivkin et al., 2005; Rothstein, 2010; Chetty et al., 2014b,a). Further,

[3]At the same time, it is important to note that the loss of a meeting may come along with feelings of lack of support or disappointment. The estimates may thus contain psychological effects, which would not be generated in the same way by policies changing the general availability of caseworker services.

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

[5]This result does not contradict the idea that the assignments made by caseworker affect the outcomes of unemployed individuals. Arni et al. (2015) show that the policy regimes chosen by PES offices caseworkers (carrots versus sticks) influence job finding and post-unemployment outcomes.

[6]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 assign 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.

individual managers have been found to be central determinants of firm policies (Bertrand and Schoar, 2002) and worker productivity (Lazear et al., 2015). Jäger and Heining (2019) shows that firms face difficulties in finding replacements after exogenous worker separations, in particular when human capital is largely firm-specific.

By identifying a strong role of individual caseworkers, I complement previous experimental evidence on the effects of counseling in UI systems.[7] In most of the experiments, personal meetings at the public employment service (PES) were combined with additional interventions. For instance, Dolton and O'Neill (1996, 2002) estimate the effects of the British Restart program, which combined stricter eligibility rules with an interview at the PES. Graversen and Van Ours (2008) and Rosholm (2008) evaluate a Danish activation program, which included both a two week job search program and a more regular contact with the employment service. Hägglund (2011) estimates the anticipation effect of being invited to a meeting at the PES. Van Landeghem et al. (2017) study the effect of a collective information session followed by a one-to-one interview, and Maibom et al. (2017) evaluate several Danish experiments, including different combinations of early meetings. Non-experimental evidence comes from Crépon et al. (2005) and Van den Berg et al. (2012), who use duration analysis to study how job search outcomes change after the incidence of a counseling session or caseworker meeting, respectively.[8] All of these studies suggest that an intensified contact with the employment office can increase unemployment exit. However, it remains open how much individuals in the caseworker profession shape the effects. Most related, Behncke et al. (2010a) find, based on propensity score matching, that "tough" caseworkers are more successful and Huber et al. (2017) show that this result cannot be explained by ALMP assignments.[9] My findings reveal more generally that individual caseworkers have a large personal impact on the success of labor market re-integration. This suggests that investments into caseworker quality can strongly increase the effectiveness of services provided to the unemployed.

On a more general level, the paper relates to the existing literature on labor market policies targeted at unemployed individuals. The recent review and meta-analysis conducted by Card et al. (2018) shows that training and private sector employment programs tend to have positive effects only in the medium and longer runs, while the success of job search assistance programs also manifests in the short run. The findings in my paper confirm this notion, as they show that caseworker meetings already affect unemployment exit probabilities in the first four to six months after entry. They further provide a novel angle to the literature by revealing large quality differences in the personal interactions through which job search assistance is mediated. This result implies that not only the provision of assistance itself, but also the selection of workers providing the assistance, is a sensible policy choice.

Finally, the paper illustrates 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.[10] The results in this

[7] Caseworker meetings can also serve as a monitoring device for job search effort. For instance, recent evidence by Cockx et al. (2018) shows that the effectiveness of job search monitoring crucially depends on the frequency of assessments and the precision of monitoring.

[8] A comprehensive overview on evaluations of job search assistance is provided by Card et al. (2010, 2018). Rosholm (2014) provides a non-technical summary of studies that evaluate counseling programs offered to job seekers.

[9] 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.

[10] Pichler and Ziebarth (2017) show that presenteeism can exert negative health externalities on colleagues if employees come to work when suffering from a contagious disease.

paper confirm the notion that worker absences may induce large costs. They further identify negative spillover effects on present workers and emphasize the low replaceability of productive workers.

The remainder of the paper is structured as follows: section 2 lays out the institutional context and data sources. It further explains 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 local average treatment effect of caseworker meetings, instrumented by absences. Section 5 decomposes the effect by 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 twelve months during the two previous years. To be eligible for the full potential duration of unemployment benefits, the contribution period extends up to 18 months. The full potential duration is usually 1.5 years for eligible prime age individuals, but varies by the contribution period, age and family situation. The replacement ratio ranges between 70% and 80% of previous earnings, depending on family size and 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 around four caseworker meetings over the first six months in unemployment. The average meeting lasts 40 minutes. During the meetings, caseworkers provide information and guidance in the job search process. They can also assign training programs and refer vacancies.

By default, meetings take place with the assigned caseworker. If the assigned caseworker is 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.[11] 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

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

meetings on the job seeker-caseworker level, including 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 show up 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 1.83% (N=7,248) of observations because the caseworker assigned to the individual never appears in the meeting database, and additional 2.16% (N=8,388) because the caseworker has less than 30 cases.[12] These have a high likelihood of being mis-classified assignments.

The sample used in the empirical analysis contains 379,358 job seekers, assigned to 2,250 caseworkers. 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 newest assignment made up to one week after the job seeker's entry into unemployment (2.3% get updated during the first week), thereby correcting for erroneous initial assignments. Results are robust to not updating assignments or to using updates made up to week 2 instead (c.f. section 4).

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. Table 1 shows summary statistics on how often job seekers interact with a caseworker over this period. In total, job seekers experience about 1.3 meetings during the first two months of unemployment. The meeting intensity is fairly stable over the first six months of unemployment, as job seekers have on average 1.1 meetings when being unemployed during months 5 to 6. Between 70 and 80% of meetings take place with the initially assigned caseworker, while the remaining meetings are realized by a replacing caseworker, who jumps in when the assigned caseworker is not available.

Table 1: Summary Statistics on Meetings over the Unemployment Spell

		Period after Entry into Unemployment		
Number of Meetings		Months 1-2	Months 3-4	Months 5-6
In Total	Mean	1.290	1.206	1.129
	SD	0.929	1.110	1.074
With Assigned Caseworker	Mean	1.000	0.899	0.793
	SD	0.938	1.082	1.034
With Replacing Caseworker	Mean	0.290	0.307	0.336
	SD	0.682	0.758	0.767
	N	379,358	317,272	233,856

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 the job seeker was unemployed during t. Job seekers are excluded from a given column if they exited unemployment before the start of the period.

Measurement of Caseworker Absences I exploit the detailed information on scheduled meetings to identify absences where a caseworker planned to come to work, but was retained

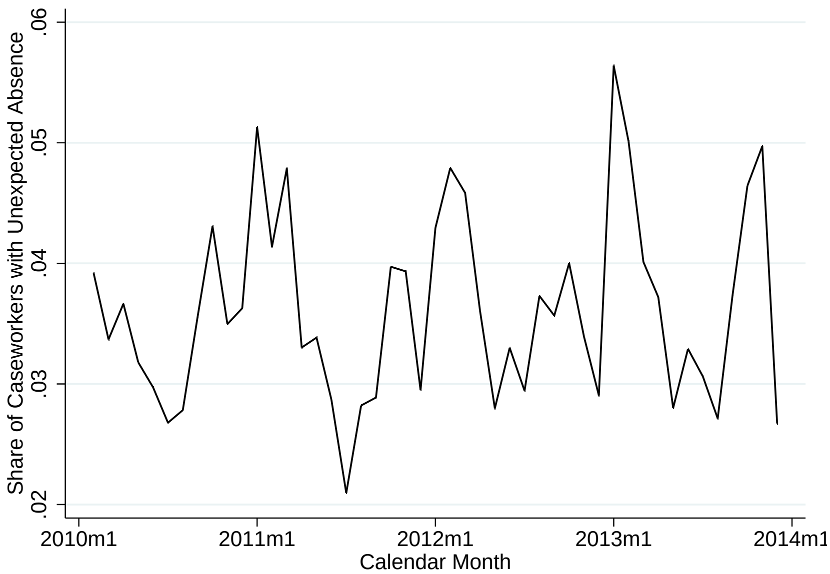
[12] Results are robust to modifying this cutoff (c.f. section 4).

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 at which none of a caseworker's scheduled meetings take place. Unrealized meetings take the status "scheduled", "canceled" or "re-scheduled". The two other possible statuses are "realized" or "job seeker did not appear". To ensure that unrealized meetings indeed reflect caseworker absences, I require that during at least two subsequent day entries, *all and at least two* meetings were scheduled and not realized.[13] The absence is defined to end on the day at which the caseworkers realizes meetings again.[14]

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 3.3% of her workdays. Taken over a year with 230 workdays, this means that, on average, 7.6 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). Among caseworkers who are absent at least once, the average duration of an absence is 8.7 days.

Figure 1: Monthly Share of Caseworkers Starting an Unplanned Absence



The figure shows the share of caseworkers who start an unplanned absence per calendar month. The measurement of unplanned absences based on unrealized caseworker meetings is described in section 2.2.

On the job seeker level, absences will serve as an instrument for the number of meetings with the assigned caseworker, realized during the first six months of unemployment. The main instrumental variable equals one if the job seeker's assigned caseworker has an unplanned absence spell which takes place during the first six months after unemployment entry and lasts more than one week (i.e. >5 workdays). Additional analyses will differentiate by the exact timing and by the length of the caseworker's absence spell. Table 3 reports summary statistics on how job seekers are

[13] The median caseworker-day cell has two meetings.

[14] I ignore the rare cases where a caseworker remains absent more than 90 days.

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

	Mean	SD	Min	Max	N
Total workdays during sample period	670.124	293.044	1	1075	2,250
Days absent during sample period	24.458	22.283	0	143	2,250
Ratio of absent over total workdays	0.033	0.032	0	0.277	2,250
Average duration of an absence in workdays	8.730	6.810	2	58	1,777

Summary statistics are at the level of the caseworker. Workdays are the number of days during which a caseworker schedules meetings with job seekers in the sample. The measurement of unplanned absences based on unrealized caseworker meetings is described in section 2.2. In the last row, the sample only includes caseworkers with at least one absence. Unplanned absences are defined as lasting at least 2 days, explaining why the minimum length of an absence is 2 days (last row).

affected by caseworker absences. The average probability that the assigned caseworker becomes absent for more than one week in the first six month after entry is 2.9%. This probability does not reflect whether an individual actually becomes affected by the absences, as individuals can exit unemployment before the absence occurs. It solely measures which share of individuals was potentially exposed to an absence (intention-to-treat).

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

	Period after Entry into UE			
	Months 1-6 (1)	Months 1-2 (2)	Months 3-4 (3)	Months 5-6 (4)
CW Absence of > 1 Week				
Mean	0.029	0.009	0.011	0.011
SD	0.167	0.094	0.105	0.103
N	379,358	379,358	379,358	379,358

Summary statistics are at the level of the job seeker. The measurement of unplanned absences is based on unrealized caseworker meetings, as described in section 2.2.

2.3 Caseworker Absences and Unemployment Exit: Raw Data

As a purely descriptive exercise, figure 2 plots the unemployment exit hazard and survival rate over the first 520 days of unemployment (520 days is the maximum potential benefit duration in the sample). The solid line includes job seekers who experience a caseworker absence over the first six months after entry into unemployment (2.9%). To avoid dynamic selection, this status is assigned regardless of whether the job seeker herself experienced a meeting cancellation, and regardless of whether she is still unemployed at the start of the absence (intention-to-treat). The dashed line includes all other job seekers in the sample.

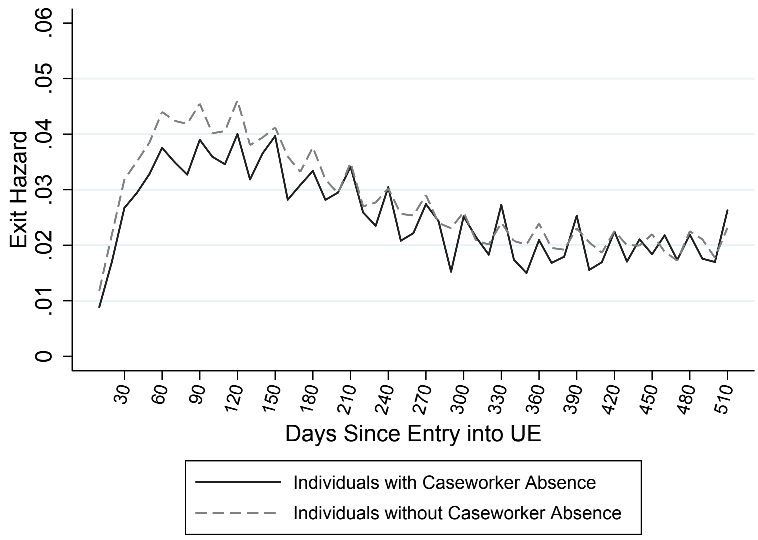
The graph reveals that the initial spike of the exit hazard is visibly less pronounced for job seekers with a caseworker absence (panel a). As shown by the survival rate in panel b, this goes along with a rather persistent increase in the medium-run probability of staying in unemployment. Motivated by this descriptive evidence, the following section presents a stylized conceptual framework to discuss how caseworker absences map to the job seeker's exit from unemployment.

3 Conceptual Framework

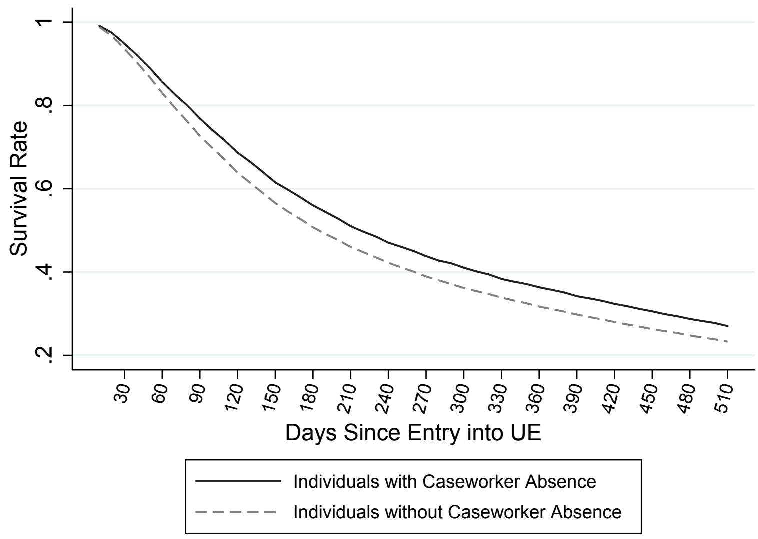
This section provides a simple conceptual discussion on the role of caseworkers in the job search process. In particular, I describe how the variation caused by caseworker absences can inform

Figure 2: 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 10 day intervals. The solid line refers to job seekers whose caseworker has an absence of more than a week during the first six months after the job seeker's entry into unemployment (2.9%). 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=379358.

about quantity versus quality effects of caseworkers.

Setup I suppose that the job finding rate 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 personal meetings:

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

If caseworker j is present at work after being assigned to job seeker i ($A_{j(i)} = 0$), i is allocated a fixed number of meetings with j.[15] The meeting quantity affects the job search process through two components: the caseworker-constant component m^0 describes 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, $q_j \sim \mathcal{N}(0, \sigma_q^2)$.[16] 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, 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. Because 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.[17] 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 lower half of the productivity distribution experience a quality gain due to the replacement. The opposite is true when the absent caseworker ranks in the upper half. I do not exploit heterogeneity in the replacement's productivity, because whether and by whom a meeting is replaced can be endogenous to the job seeker's situation.

4 Estimating the Effect of Caseworker Meetings

In the following, I first present the baseline estimation framework, in which the number of caseworker meetings is instrumented by the incidence of an absence. I then discuss identifying assumptions and present estimation results. These also contain an analysis of spillover effects arising from absences.

[15] 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.

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

[17] Empirically, the replacement productivity is indeed zero on average and unrelated to the absent caseworker's productivity.

4.1 Estimation Framework

Empirical Model The empirical strategy estimates the causal effect of caseworker meetings on job seeker outcomes y_i, using the incidence of unplanned caseworker absences as an instrument. y_i includes as main outcomes the duration of unemployment in days and the probability to exit unemployment within a given time period.[18]

The baseline two stage framework, estimated using 2SLS, writes as follows:

$$Meet_{i(j)} = \rho_j + \theta Absence_{j(i)} + \lambda_t + X_i'\beta + u_i \tag{1}$$

$$y_i = \pi_j + \delta Meet_{i(j)} + \eta_t + X_i'\gamma + v_i \tag{2}$$

$Meet_{i(j)}$ denotes the number of meetings which individual i has with her assigned caseworker j over the first six months of unemployment.[19] In the first stage equation 1, $Meet_{i(j)}$ is instrumented by $Absence_{j(i)}$, which equals one if caseworker j has an absence of one week or more during the first six months after job seeker i's unemployment entry. Additional analyses split up the six months into smaller time windows. The parameter of interest is the coefficient δ in the second stage equation 2. It estimates the local average treatment effect (LATE) of one additional meeting with the assigned caseworker j, induced by j's presence, on the outcome y_i. I also estimate reduced form effects of $Absence_{j(i)}$ on y_i.

The vectors ρ_j and π_j contain caseworker fixed effects, measuring j's additive effect during workplace presence. Thereby, they control for all time-constant caseworker characteristics and address the threat that the caseworker's productivity at work may coincide with the likelihood of an absence. The empirical model thus compares individuals assigned to the same caseworker during workplace presence versus absence. λ_t and η_t include fixed effects for the job seeker's calendar month of entry into unemployment. They control for aggregate time shocks (e.g., health-related), which can 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 B.1. Point estimates do not react significantly to the introduction of X_i.

Interpretation of the LATE The parameter of interest, δ, corresponds to the local average treatment effect (LATE) of having an additional caseworker meeting because the caseworker is not absent from work ($Absence_{j(i)} = 0$). In this setup, compliers are job seekers whose number of meetings reduces from m_1 to $m_0 < m_1$ due to their caseworker's absence. In turn, non-compliers have the same meeting frequency with and without the absence ($m_1 = m_0$). One example of non-compliers are job seekers who had a meeting just before the caseworker turned absent – and do therefore not see a change in their meeting frequency. Another example are job seekers who already exited unemployment before the absence.[20] The estimated LATE does not inform about these job seekers' reaction to a meeting.

[18] Reduced form results are robust to specifying the exit from unemployment as a proportional hazard (available upon request).

[19] To control for unemployment dynamics, $Meet_{i(j)}$ is normalized by the time spent in unemployment over the first six months after entry. I.e., I multiply the number of meetings realized during the first six months after entry by the share of days the job seeker remained unemployed during this period. Results hardly differ when not applying the normalization (available upon request).

[20] For instance, a job seeker's meeting frequency is by definition unaffected if she exits unemployment within 2 months and her caseworker becomes absent in month 3 after her entry into unemployment.

Given that the number of meetings is a multi-valued treatment, there can be different types of complier reactions in the data. Compliers can comply at different base level m_1 and with different intensities $m_1 - m_0$ (c.f. Frölich, 2007). For example, some compliers move from 1 to 0 meetings due to the absence, others from 4 to 3 etc. – and some compliers may lose more than one meeting. Angrist and Imbens (1995) show that in the case of a multi-valued treatment, the LATE is a weighted average of unit causal effects, where the weighting function is determined by the distribution of complier reactions (c.f. Angrist and Imbens (1995) or Frölich (2007) for a formal definition). To illustrate which types of complier reactions drive the LATE estimated in this section, appendix figure A.1 plots the average difference in the probability of having x meetings between individuals with $Absence_{j(i)} = 1$ and individuals with $Absence_{j(i)} = 0$.[21] Individuals with an absence are in sum about 10 percentage points more likely to have 2 or less meetings over the first 6 months of unemployment. In turn, they are in sum about 10 percentage points less likely to have between 3 and 6 meetings. The probability of having more than 6 meetings is hardly affected by the instrument, as job seekers rarely have more than a meeting per month.

As an additional remark, it is important to note that the estimate of δ from equation 2 corresponds to a weighted average of caseworker-specific LATEs – since it relies on within-caseworker variation in absences. Clearly, these caseworker-specific LATEs do not need to be homogeneous: a job seeker's reaction to losing a meeting with caseworker A may not be the same as her reaction to losing a meeting with caseworker B. Section 5 will indeed provide evidence that there is strong heterogeneity in caseworker-specific LATEs, depending on how productive a caseworker is in helping individuals to exit unemployment.

4.2 Identification

The identification strategy exploits variation in absences within caseworkers over time. It 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 assess this key assumption.

Composition of Job Seekers The identification assumption requires that the timing of a caseworker's absences does not respond to the characteristics of her assigned job seekers.

Table 4 tests whether a job seeker's exposure to caseworker absences is influenced by her predetermined characteristics. The outcome is the binary variable $Absence_{j(i)}$, as specified in the estimation framework (i.e., the incidence of a caseworker absence lasting more than a week and occurring during the first six months of unemployment). In column 1, no caseworkers fixed effects are included in the regression. The estimates suggest some degree of selection into frequently absent caseworkers, as a few coefficients show a significant sign, and the coefficients of the model are jointly significant. This 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 job seekers within PES offices. To test the relevance of these two mechanisms, column 2 adds PES fixed effects. Absences and observed job seeker characteristics no longer

[21]The difference is computed at the caseworker level and then averaged over all caseworkers with at least one absence in the sample. The graph is inspired by Acemoglu and Angrist (2000), who use a similar figure to illustrate compliance with different compulsory schooling laws.

[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.

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 column 3, it remains that absences are unrelated to pre-determined characteristics. This provides clear evidence that the caseworker-time specific variation in absences occurs independently of job seeker characteristics.

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

	P(Caseworker Absence)		
	(1)	(2)	(3)
Female	-0.021	-0.009	-0.007
	(0.013)	(0.011)	(0.007)
Experience < 3 years	0.013	0.014	0.012
	(0.013)	(0.009)	(0.008)
HH size >2	-0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)
Aged > 40	0.008	0.002	0.004
	(0.008)	(0.007)	(0.006)
Low education	-0.015	0.009	-0.005
	(0.012)	(0.009)	(0.006)
Log previous earnings	-0.009	-0.002	-0.005
	(0.014)	(0.010)	(0.007)
UE in last 12 months	-0.027***	0.001	0.002
	(0.009)	(0.008)	(0.006)
PBD>260	0.019**	0.008	0.006
	(0.008)	(0.007)	(0.006)
Replacement rate > 75%	0.009	0.010	0.008
	(0.008)	(0.007)	(0.006)
p-value (joint significance)	0.016	0.617	0.724
Outcome Mean	0.029	0.029	0.029
Month FE	Yes	Yes	Yes
PES FE	No	Yes	No
Caseworker FE	No	No	Yes
N	379358	379358	379358

The outcome variable equals one if the job seeker's caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. In all columns, regressions include fixed effects for the calendar month of entry into unemployment. In columns 2, regressions also include PES office fixed effects. In column 3, regressions also include caseworker fixed effects. PBD= potential benefit duration in days. All explanatory variables, except log previous earnings, are specified as binary variables. Coefficients are multiplied by 10 to improve readability. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250).

Caseworker Workload Changes in workload, which tend to reflect changes in the local unemployment rate, 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 t, I count a caseworker's stock of cases, as well as the number of new cases assigned to the caseworker. I then assess whether the stock and the inflow of cases in months $t-1$, $t-2$ and $t-3$ affects the incidence of an absence (lasting > 1 week) in t.

In table 5, column 1 reports results from regressions including only month fixed effects. The estimates suggest a small negative correlation between the measures of workload and absenteeism, which is jointly significant at the 10% level. When introducing PES office fixed effects in column 2, this correlation disappears. This remains the case when using caseworker fixed effects (column 3). The results thus report no evidence that the caseworker- specific timing of an absence is induced by workload.

Table 5: Identification Test: Workload and Caseworker Absences

	P(Absence in Month t)		
	(1)	(2)	(3)
Total Cases in t-1	0.010	-0.004	-0.003
	(0.011)	(0.011)	(0.012)
Total Cases in t-2	0.004	0.002	-0.000
	(0.017)	(0.017)	(0.017)
Total Cases in t-3	-0.010	0.002	0.004
	(0.009)	(0.009)	(0.010)
New Cases in t-1	-0.011	0.008	0.011
	(0.012)	(0.013)	(0.014)
New Cases in t-2	-0.012	0.006	0.010
	(0.013)	(0.013)	(0.014)
New Cases in t-3	-0.016**	-0.011	-0.009
	(0.008)	(0.008)	(0.008)
p-value (joint significance)	0.081	0.521	0.509
Outcome Mean	0.006	0.006	0.006
Month FE	Yes	Yes	Yes
PES FE	No	Yes	No
Caseworker FE	No	No	Yes
N	57996	57996	57984

Coefficients are multiplied by 100. The unit of observation is the caseworker-month cell. The outcome variable equals one if the caseworker had an absence spell of more than one week in calendar month t. The total number of cases is defined as the stock of job seekers held by the caseworker in a given calendar month. The number of new cases are defined as the number of job seekers assigned to the caseworker in a given calendar month. In all columns, regressions include calendar month fixed effects. In column 2, regressions additionally include PES fixed effects. In column 3, regressions additionally include caseworker fixed effects. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250).

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 later than six months after i's unemployment entry are "placebo absences" when the outcome is i's probability to exit unemployment within six months.[23] Placebo tests will be shown in the next section, jointly with the results.

4.3 Results

In the following, I present the estimated effects of personal caseworker meetings, instrumented by the incidence of a caseworker absence, on the duration of unemployment. The results include analyses of effect dynamics and of heterogeneity by job seeker characteristics. In addition, I provide evidence that absent caseworkers exert negative spillover effects on their present colleagues.

[23]The idea to use future workplace presence/absence as placebos for current outcomes is adapted from Mas and Moretti (2009) and Herrmann and Rockoff (2012).

Baseline Estimates In table 6, column 1 shows the first stage effect of a caseworker absence on the number of personal meetings which a job seeker has with her assigned caseworker. Both meetings and absences refer to the job seeker's first six months of unemployment.[24] The estimate shows that on average half a meeting is lost when the caseworker has an absence (17% relative to the mean). This holds true both with and without covariates (panels A and B, respectively). Column 2 reports the reduced form effect of an absence on the duration of unemployment. It shows that individuals remain unemployed 5.5 days longer on average if their caseworker is absent during the first six months of their unemployment spell (panel B). In column 3, this coefficient is scaled by the first stage. One meeting with the assigned caseworker is estimated to reduce unemployment by 11.6 days, i.e., by 5% relative to the mean (panel B). I interpret this estimate as a local effect, as caseworker meetings are likely to have decreasing marginal returns. In addition, it is important to recall that the effect of a meeting is estimated from meeting losses induced by caseworker absences. Possibly, such losses come along with feelings of disappointment or lack of support among affected job seekers. Therefore, the estimates may contain psychological effects which would not be generated in the same way by policies changing the general availability of caseworker services.

Table B.2 in the appendix documents that the main 2SLS estimate is invariant to several modifications of sampling choices and variable specifications. Appendix figure A.2 shows both first stage and reduced form effects differentiating by the length of the absence in workdays. Absences lasting one workweek (5 workdays) or less are apparently of insufficient treatment intensity. They show a small first stage effect and no reduced form effect on the duration of unemployment. Effects increase in the length of absence, but not beyond two workweeks.

Effects on the duration of unemployment can only be interpreted as a result of actual job finding if the share of individuals exiting from unemployment to a job (instead of exiting to non-employment) is unaffected. Column 5 provides evidence by reporting the effect of an additional caseworker meeting on the probability of job finding upon unemployment exit.[25] As the unemployed duration is censored at 520 days, the regression only includes individuals exiting up to this point (88%). Results show that the probability to exit from unemployment to a job, instead of exiting to non-employment, is unaffected by the absence. This strongly suggests that the effect of a meeting on the duration until re-employment is proportional to its effect on the duration of unemployment.

Effect Dynamics and Placebo Estimates Figures 3 and 4 investigate effect dynamics and present Placebo estimates, i.e., effects of future absences on current outcomes. Absences are now counted over two-months periods: is the caseworker absent for more than a week in months 1-2, 3-4, etc. after the job seeker's entry into unemployment?[26]

Figure 3 plots dynamic effects of absences on the average number of meetings realized over the first two (panel a), four (panel a) and six (panel c) months of unemployment. In all three panels, Placebo effects are not different from zero. The graphs further show that the effect of absences on the number of meetings decreases over the spell, which is a mechanical result of unemployment

[24] To control for unemployment dynamics, the number of meetings is normalized by the time spent in unemployment over the first six months after entry. I.e., I multiply the number of meetings realized during the first six months after entry by the share of days the job seeker remained unemployed during this period. Results hardly differ when not applying the normalization (available upon request).

[25] This information is registered by the PES office.

[26] I do not use caseworker absences occurring before the job seeker's unemployment entry to run placebo tests because these may potentially affect the meeting frequency after entry. For instance, the caseworker may have to catch up with foregone meetings and therefore schedule less meetings with her newly assigned job seekers.

Table 6: Effects the Duration of Unemployment

	Meetings w/ Assigned CW	Duration of UE in Days		P(Exit to Job)
	First Stage	Reduced Form	2SLS	2SLS
	(1)	(2)	(3)	(4)
Panel A: Without Covariates				
Absence	-0.472***	5.079**		
	(0.039)	(2.058)		
Meetings w/ Assigned CW			-10.755**	0.004
			(4.384)	(0.012)
Panel B: With Covariates				
Absence	-0.473***	5.484***		
	(0.039)	(1.957)		
Meetings w/ Assigned CW			-11.604***	0.006
			(4.167)	(0.012)
F-Stat (Weak ID)			144.657	124.487
Outcome Mean	2.861	218.017	218.017	0.764
N	379358	379358	379358	332453

"Absence" equals one if the job seeker's assigned caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. "Meetings w/ Assigned CW" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the time spent in unemployment during this period. All regressions include calendar month and caseworker fixed effects. In panel B, regressions also include job seeker covariates (summary statistics reported in table B.1). The unemployment duration is capped at 520 days (12.3% of the sample). Column (4) only includes job seekers who exit unemployment with 520 days. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250). Further estimation details can be found in section 4.

exit. For instance, less individuals are still unemployed in months 5-6 than in months 1-2, implying that less meetings can be lost.

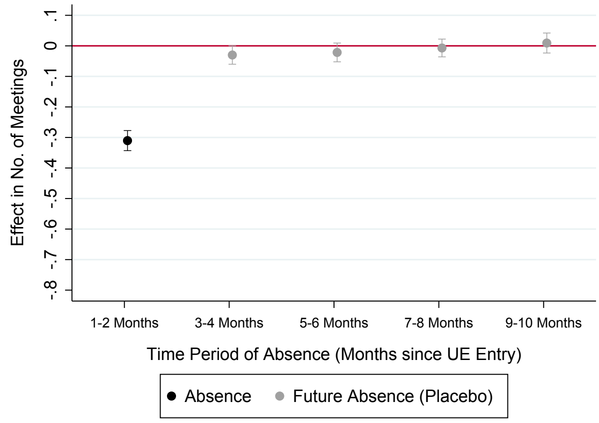
Following the same logic, figure 4 reports reduced form effects of absences on the probability to exit unemployment within different time periods. In panel a, the outcome period is two months. There is no effect of absences, most likely because the time interval is not large enough for caseworker interventions to translate into actual job finding. Panel b focuses on the probability to exit within four months. This outcome is equally affected by absences occurring in months 1-2 and months 3-4. Placebo absences, i.e. absences occurring beyond month four after the job seeker's entry, show no effect. Similarly, the probability to exit within six months is affected by absences occurring up to month six, but not by later absences (panel c).

Taken together, figures 4 and 3 provide suggestive evidence that the importance of caseworker meetings increases over the first six months of unemployment: while the first stage effect of absences on meetings decreases, their reduced-form effect on unemployment exit is stable.

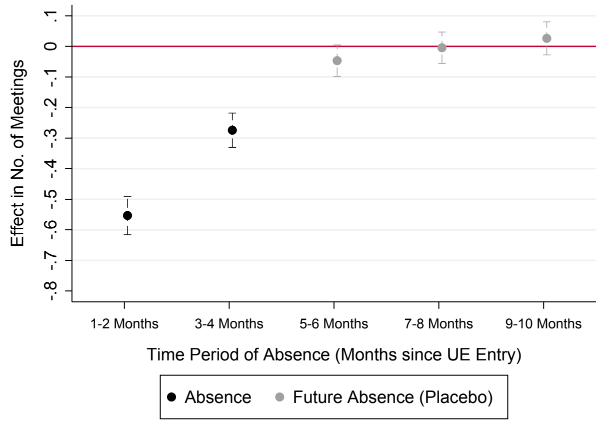
Replacement Mechanisms It is a priori unclear whether job seekers who lose a meeting with their assigned caseworker have *overall* less caseworker meetings. If there was perfect replacement, the total number of meetings should hardly react to absences. Table 7 assesses the degree of replacement within PES offices. Column 1 shows that the incidence of an absence leads, on average, to 0.23 additional meetings with a replacing caseworker. As an absence reduces the number of meetings with the assigned caseworker by 0.47 (c.f. column 1 of table 6), this reveals a replacement rate of 50%. Indeed, column 2 shows that an additional meeting with the assigned caseworker, induced by caseworker presence, reduces the number of replaced meetings by 0.49. Analogously, columns 3 and 4 show that in about 50%, absences directly translate into a loss of overall meetings.

Figure 3: Dynamic Effects of Absences on the Number of Meetings with the Assigned Caseworker

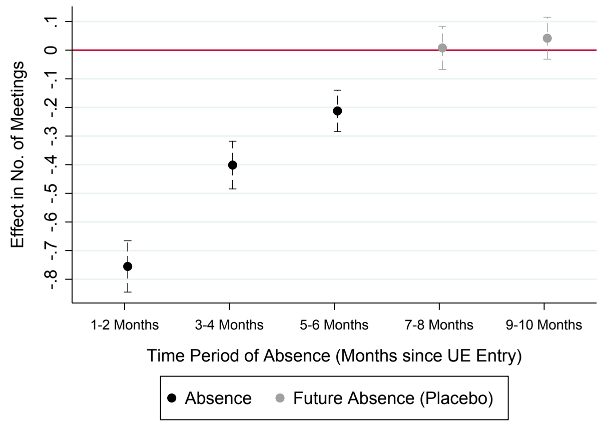
(a) Number of Meetings over the First 2 Months



(b) Number of Meetings over the First 4 Months



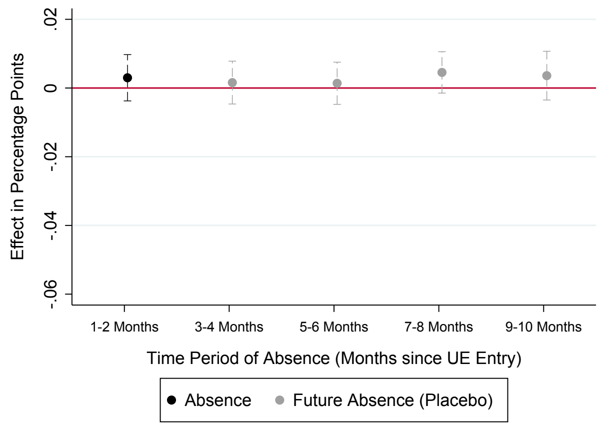
(c) Number of Meetings over the First 6 Months



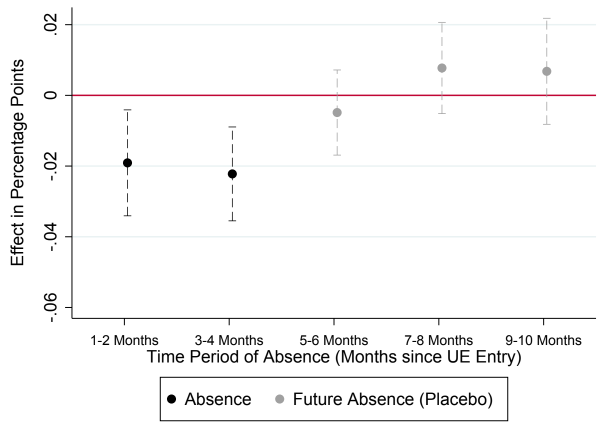
The x-axis denotes the period of the unemployment spell in which a caseworker absence of at least one week occurred. The y-axis denotes the effect of the absence on the job seeker's number of meetings with her assigned caseworker. The number of meetings is normalized by the share of days the job seeker remained unemployed during the outcome period. Regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). Dashed lines represent 90% confidence intervals. N=379358.

Figure 4: Dynamic Effects of Absences on the Probability to Exit Unemployment

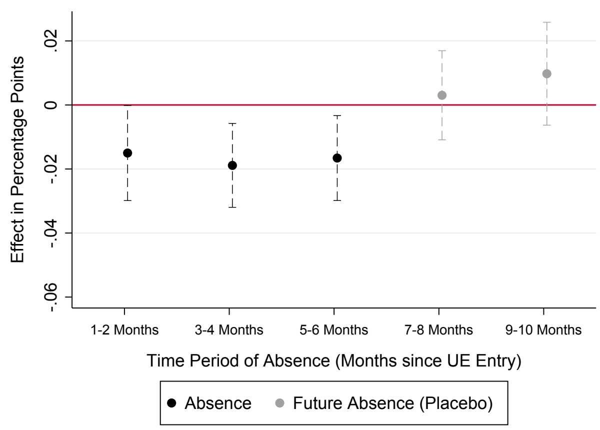
(a) P(Exit within 2 Months)



(b) P(Exit within 4 Months)



(c) P(Exit within 6 Months)



The x-axis denotes the period of the unemployment spell in which a caseworker absence of at least one week occurred. The y-axis denotes the effect of the absence on the job seeker's probability to exit unemployment. Regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). Dashed lines represent 90% confidence intervals. N=379358.

Altogether, the existence of partial replacement suggests that the estimated cost of losing a meeting with the assigned caseworker is a lower bound to its cost in contexts without replacement mechanisms.

Table 7: Effects on the Number of Replaced Meetings

	Meetings w/ Replacing CW		Meetings in Total	
	Reduced Form (1)	2SLS (2)	Reduced Form (3)	2SLS (4)
Absence	0.232***		-0.240***	
	(0.029)		(0.034)	
Meetings w/ Assigned CW		-0.491***		0.508***
		(0.052)		(0.052)
F-Stat (Weak ID)		144.657		144.657
Outcome Mean	0.914	0.914	3.774	3.774
N	379358	379358	379358	379358

"Absence" equals one if the job seeker's assigned caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. "Meetings w/ Assigned CW" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the time spent in unemployment during this period. The outcome variables (meetings) also refer to the first six months of unemployment and are normalized by the time spent in unemployment during this period. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250). Further estimation details can be found in section 4.

Heterogeneity by Job Seeker Characteristics As an additional analysis, table 8 tests whether the effects of instrumented caseworker meetings differ by observed job seeker characteristics. Columns 1 to 4 report that effects do not differ significantly by gender or age. According to columns 5 and 6, individuals with lower pre-unemployment earnings tend to respond more, as their duration reduces by 17 days in response to a meeting. This is in line with the literature on active labor market programs, which typically finds stronger reactions for individuals of low income potential (Card et al., 2010, 2018). Overall, the table however points out that very different types of job seekers benefit from the interaction with a caseworker. Restricting caseworker resources to certain subgroups would thus not be a sensible policy choice.

Table 8: Effects on the Duration of Unemployment, Heterogeneity by Job Seeker Characteristics

	2SLS Estimates					
	Gender		Age		Previous Earnings	
	Female (1)	Male (2)	≤40 (3)	>40 (4)	≥ Median (5)	< Median (6)
Meetings w/ Assigned CW	-11.135**	-13.896**	-10.942**	-13.402*	-7.089	-16.627***
	(4.927)	(6.831)	(4.588)	(7.432)	(6.131)	(5.426)
Outcome Mean	212.229	226.759	203.680	251.137	217.913	218.122
N	228240	151111	264749	114607	190755	188603

"Meetings w/ Assigned CW" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the time spent in unemployment during this period. Meetings are instrumented by the incidence of a caseworker absence. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). The unemployment duration is capped at 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=2250). Further estimation details can be found in section 4.

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 (c.f. Crépon et al., 2013; Lalive et al., 2015; Gautier et al., 2018). This is expected to cause positive spillover effects on the unemployment exit 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, we would expect negative spillovers.

Table 9 presents spillover effects of caseworker absences. For each job seeker i, I measure the share of individuals who are affected by an absence, who enter the same PES office in the same calendar quarter as i, and who are not assigned to the same caseworker as i (leave-out mean). This measure is introduced in the reduced-form specification linking job seeker outcomes to caseworker absences. As the specification contains caseworker and calendar month fixed effects, identification relies on the assumption that the exact time-office specific variation in the absence rate is quasi-random from the individual job seeker's perspective.

To start, column 1 shows that spillover effects increase the duration of unemployment. An increase in the absence rate by 10% translates into a 1.3 days longer spell. To explain this effect, column 2 shows that individuals can meet their caseworker less often due to spillover effects. This effect is relatively small: when the office-quarter absence rate increases by 10%, individuals have on average 0.05 meetings less with their assigned caseworker. However, these lost meetings are not replaced (column 3), but translate directly into a reduction in the total number of meetings (column 4). The likely reason is that the office-level supply of caseworkers is limited in times of high absence rates. The lack of replacement can then explain why a reduced meeting frequency with the assigned caseworker due to spillovers comes along with a relatively high effect on the duration of unemployment.

Table 9 further suggests that spillovers do not bias the estimated direct effect of absences, which does not differ from the estimates reported in table 6. However, the empirical setting does not allow for conclusions on job search externalities that may arise when changing the number of caseworker meetings on a larger scale.

Table 9: Spillover Effects of Absences

	UE Duration	Number of Meetings		
		w/ Assigned CW	w/ Replacing CW	In Total
	(1)	(2)	(3)	(4)
Absence	5.351***	-0.467***	0.232***	-0.234***
	(1.957)	(0.039)	(0.029)	(0.034)
Office-Quarter Absence Rate	13.013**	-0.544***	-0.027	-0.571***
	(6.614)	(0.141)	(0.094)	(0.132)
Outcome Mean	218.017	2.861	0.914	3.774
N	379358	379358	379358	379358

"Absence" equals one if the job seeker's assigned caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. The office-quarter absence rate excludes the caseworker assigned to job seeker i (leave-out mean). The mean absence rate is 0.03. The number of meetings refers to the first six months after the job seeker's unemployment entry and is normalized by the share of days the job seeker remained unemployed during this period. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). In column 1, the unemployment duration is capped at 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=2250). Further estimation details can be found in section 4.

5 The Role of Caseworker Productivity

The following section analyzes how the effect of personal caseworker interactions differs by the individual caseworker's 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 expected quality effect depends on the absent caseworker's productivity at work. In the following, I first present details on the estimation of caseworker productivity and then report how productivity interacts with the effect of absences and meetings.

5.1 Estimation Details

Estimation of 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., Chetty et al., 2014b,a; Bertrand and Schoar, 2002; Lazear et al., 2015). I consider the duration of unemployment as the relevant output, and hold other input factors, such as local labor market conditions and the job seeker's characteristics, constant. To this end, I estimate the following expression for d_i, job seeker i's duration of unemployment:

$$d_i = \mu_j + X_i'\beta + \kappa_{o\times t} + \varepsilon_i \tag{3}$$

The vector μ_j measures the parameter of interest, caseworker j's additive effect on the duration of unemployment. 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 μ_j. Therefore, the regression is run without job seekers who are affected by an absence. The vector $\kappa_{o\times t}$ contains interacted PES office $\times$ calendar month fixed effects. It thereby controls for office-specific time shocks potentially affecting caseworker performance. X_i includes the same covariates as in equations 1 and 2. Summary statistics are reported in table B.1.

As discussed in section 2.1, the assignment of caseworkers to job seekers is often based on caseworker availability or randomness. 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.3 shows the densities of estimated caseworker effects $\hat{\mu}_j$ 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.

Estimating the Effect of Absences and Meetings by Productivity The goal is to estimate how the reduced-form effect of a caseworker absence and the effect of an instrumented caseworker meeting interact with productivity at work. To this end, I interact the incidence of an absence ($Absence_{j(i)}$) and the number of caseworker meetings ($Meet_{i(j)}$) with a simple PES-level median split of the estimated productivity measure. As a result of the PES-level median split, caseworkers are ranked in comparison to their office-specific colleagues, who face the same workplace conditions.[27]

To ensure that the absences are exogenous for both "high" ($\hat{\mu}_j >$ office median) and "low" ($\hat{\mu}_j \leq$ office median) productivity caseworkers, appendix tables B.3 and B.4 replicate the identification tests from section 4.2 separately for the two groups. They show that for both productivity groups, neither job seeker characteristics nor workload are related to the incidence of an absence, conditional on caseworker and month effects.

5.2 Results

In the following, I first report how the effect of caseworker meetings on the duration of unemployment differs by productivity. Secondly, I examine effects on the stability of accepted jobs. In a final step, I explore whether differences in the replacement of meetings or the usage of active labor market programs can explain the heterogeneity.

Effects on Unemployment Duration Table 10 reports effects on the duration of unemployment by productivity. Column 1 shows that the number of meetings equally responds to absences of high and low productivity caseworkers (first stage). However, column 2 reveals striking differences in the reduced form effect of an absence on the duration of unemployment. The effect is not different from zero when the caseworker ranks in the lower half of the productivity distribution. In turn, unemployment prolongs by 13.7 days if the absent caseworker ranks in the upper half. As shown by column 3, this estimate implies that a meeting induced by the presence of a high-productivity caseworker reduces unemployment by 30 days (13% relative to the group-specific mean). Column 4 reveals that the probability to actually enter a job when exiting unemployment is for both groups unaffected by an absence. The success of productive caseworkers is thus not driven by exits out of the labor force.

Appendix figure A.4 further decomposes the heterogeneity by quintiles. It shows that individuals whose caseworker ranks at the bottom quintile benefit from an absence, while individuals with a caseworker at the top quintile incur strong losses. This further illustrates the high degree of heterogeneity in the performance of caseworkers.

Effects on Job Stability From a policy perspective, it is of interest to understand whether productive caseworkers are able to generate sustainable job matches. To address this question, table 11 presents heterogeneous effects of caseworker absences on the probability of recurring to unemployment within 12 months. Effects are reported both for the full sample and for the more homogeneous sample of job seekers who exit unemployment within one year. Regressions further control for the duration of unemployment in days.

Columns 1 and 2 report the homogeneous reduced form effect of a caseworker absence. Estimates report a clear zero effect, both for the full sample and for job seekers with an unemployment duration of one year or less. On average, the presence of a caseworker does not affect whether individuals enter a stable or an unstable job.

Columns 3 and 4 split the reduced form effect by caseworker productivity. While point estimates fluctuate around the margin of 10% significance, the effect of an absence significantly differs by productivity. As reported in column 4, the absence of a highly productive caseworker increases recurrence by 1.2 percentage points (4% relative to the group-specific mean) if individuals exited

[27] 282 out of 2250 caseworkers work in more than one office over the sample period. In these cases, I use the rank in the office in which the caseworker had the majority of cases.

Table 10: Effects on the Duration of Unemployment, by Caseworker Productivity

	Meetings w/ Assigned CW	Duration of UE in Days		P(Exit to Job)
	First Stage (1)	Reduced Form (2)	2SLS (3)	2SLS (4)
Absence × Low Productivity	-0.491*** (0.054)	-1.397 (2.702)		
Absence × High Productivity	-0.451*** (0.057)	13.716*** (2.681)		
Meetings × Low Productivity			2.242 (5.416)	0.010 (0.015)
Meetings × High Productivity			-29.830*** (6.451)	0.001 (0.018)
p-value for H_0: coeff equality	0.608	0.000	0.000	0.715
Outcome Mean	2.861	218.017	218.017	0.764
N	379358	379358	379358	332453

"Absence" equals one if the job seeker's assigned caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. "Meetings" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the time spent in unemployment during this period. "High Productivity" equals one if the caseworker's estimated work productivity is higher than the PES office median. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). The unemployment duration is capped at 520 days (12.3% of the sample). Column (4) only includes job seekers who exit unemployment with 520 days. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250). Further estimation details can be found in section 5.

unemployment within a year. In turn, the absence of a low productivity caseworker tends to decrease recurrence (effect difference significant at the 5% level). IV estimates (columns 5 and 6) confirm that the meeting with a high productivity caseworker tends to reduce recurrence. As reported in column 6, the effect is of 2.8 percentage points (9%) for individuals remaining unemployed one year or less. Again, this estimate significantly differs from the one of low productivity caseworkers, whose meetings tend to increase recurrence.

Taken together, it appears that heterogeneity in caseworker performance has long-lasting effects on individual labor market outcomes. Although the productivity measure only reports how successful caseworkers are in shortening unemployment spells, it is also predictive of how caseworker meetings affect job stability.

Potential Channels The strong heterogeneity in the effect of caseworker absences raises questions about the underlying channels: do the extent and the quality of meeting replacement correlate with the absent caseworker's productivity? Do productive caseworkers prescribe different types of treatments over the unemployment spell? To answer these questions, table 12 reports IV estimates on how the number of replaced meetings and the likelihood of different treatment assignments are affected by an additional meeting with a high versus low productivity caseworker. All outcomes refer to the first six months of unemployment.

Column 1 shows that there is no heterogeneity in the degree of replacement. One realized meeting with the assigned caseworker (instrumented by the incidence of an absence) reduces the number of replaced meetings by about 0.5 for both productivity types. To test whether the quality of replacement responds to the absent caseworker's productivity, appendix figure A.5 plots the replacement productivity against the absent caseworker's productivity (conditional on PES office effects). There is now evidence that the two correlate in a significant way.

In columns 2 to 4, outcomes are the probabilities of being assigned to different treatments, which are typically at the (partial) discretion of the caseworker. The aim is to shed light on the importance of observed treatments versus unobserved counseling techniques in the caseworker

Table 11: Effects on Job Stability, by Caseworker Productivity

	P(Recurrence within 12 Months)					
	Reduced Form				2SLS	
Sample	All (1)	UE ≤1 Year (2)	All (3)	UE ≤1 Year (4)	All (5)	UE ≤1 Year (6)
Absence	-0.000 (0.004)	0.001 (0.005)				
Absence × Low Productivity			-0.009 (0.006)	-0.013 (0.008)		
Absence × High Productivity			0.007 (0.006)	0.012* (0.007)		
Meetings × Low Productivity					0.019 (0.013)	0.030 (0.019)
Meetings × High Productivity					-0.014 (0.012)	-0.028* (0.016)
p-value for H_0: coeff equality			0.063	0.017	0.064	0.020
Outcome Mean	0.257	0.301	0.257	0.301	0.257	0.301
N	379358	293070	379358	293070	379358	293070

"Absence" equals one if the job seeker's assigned caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. "Meetings" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the time spent in unemployment during this period. "High Productivity" equals one if the caseworker's estimated work productivity is higher than the PES office median. Columns with "UE $leq1$ Year" include job seekers whose unemployment spell lasted one year or less. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). They further control for the duration of the unemployment spell in days. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2250). Further estimation details can be found in section 5.

production function. Column 2 reports that the probability of being assigned to a training program slightly increases in the number of caseworker meetings. However, this effect is not statistically significant and, importantly, does not vary by caseworker productivity. The probability of being referred to a PES-posted vacancy (column 3) increases significantly in the number of meetings. However, the size of the increase does, again, not differ significantly by productivity. As shown in column 4, the same holds true for the probability of receiving a benefit sanction due to the non-compliance with a rule (e.g. with the job search requirement).

Altogether, I cannot find any evidence that the usage of active 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 find based on a mediation analysis 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 meta-analyses by Card et al., 2010, 2018), finding mixed results. This paper suggests that the counseling style of caseworkers, which is unobserved in traditional data sources, may be a central driver of early exits from unemployment. From a policy perspective, this implies that the personal component of job search assistance is of key importance. The hiring of high quality caseworkers or investments into the counseling qualities of existing caseworkers may therefore have large payoffs.

Table 12: Potential Channels: Replacement and Treatment Assignments

	2SLS Estimates			
	Replaced Meetings (1)	P(Training) (2)	P(Vacancy Referral) (3)	P(Sanction) (4)
Meetings × Low Productivity	-0.479***	0.008	0.048***	0.016
	(0.062)	(0.015)	(0.018)	(0.014)
Meetings × High Productivity	-0.506***	0.008	0.035*	0.032*
	(0.086)	(0.020)	(0.020)	(0.017)
p-value for H_0: coeff equality	0.792	0.979	0.608	0.471
Outcome Mean	0.914	0.355	0.256	0.229
N	379358	379358	379358	379358

"Meetings" includes the number of meetings which the job seeker had with her assigned caseworker during the first six months after the job seeker's unemployment entry, normalized by the share of days the job seeker remained unemployed during this period. Meetings are instrumented by the incidence of a caseworker absence "High Productivity" equals one if the caseworker's estimated work productivity is higher than the PES office median. All outcomes refer to the first six months of unemployment. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2250). Further estimation details can be found in section 5.

5.3 Policy Implications: Back-of-the-Envelope Quantification

In the UI literature, it is common to quantify the benefits from reduced unemployment spells in terms of saved UI payments and additional tax revenues (e.g., Nekeoi and Weber, 2017).[28] The results reported in table 6 imply that one meeting with the assigned caseworker, realized over the first six months of unemployment, saves about 12 days of unemployment. Assuming that the duration to re-employment reduces by about the same order of magnitude,[29] the welfare state saves 12 days of benefit payments and gains 12 days of tax income.

In Switzerland, the average replacement rate in UI is 75%, and the average income tax rate varies locally between 15% and 22%. In a municipality with a lower bound tax rate, the average fiscal gain from a caseworker meeting would thus be the equivalent of 90% × 12= 11 average daily wages. For caseworkers whose productivity exceeds the office median, the gain would be 90% × 30= 27 daily wages. (c.f. the estimate from table 10, column 3). Provided that the average daily wage in the sample is 165 CHF (1 CHF ≈ 1 USD), the average financial gain from providing a caseworker meeting is thus estimated to be around 1800 CHF, and the average gain from meeting a highly productive caseworker is estimated to be around 4500 CHF. This quantification obviously needs to be interpreted as a simplifying approximation. In particular, it does not claim to inform about the payoffs from large-scale increases in the number of caseworker meetings, whose effects would likely be limited by decreasing marginal returns and/or job search externalities. As a further cautionary note, one needs to keep in mind that the effect of a meeting is estimated from meeting losses induced by caseworker absences. These may come along with psychological effects, such as feelings of disappointment, which would not be generated in the same way by policies which change the general availability of caseworker services.

[28] Nekeoi and Weber (2017) also incorporate wage effects in their calculation. This is not possible here because I do not observe re-employment wages.

[29] This assumption is supported by the result that caseworker meetings do not lead to an increased exit to non-employment, c.f. column 5 of table 6.

6 Conclusion

This paper exploits exogenous variation in unplanned work absences to estimate how the face-to-face interaction with a caseworker affects the unemployment duration of job seekers. Results reveal a substantial economic value of caseworkers: reducing the amount of early caseworker interactions by 40% ($\approx$ one meeting) increases the average duration of unemployment by about 5% (12 days). According to a simple back-of-the-envelope calculation assuming a tax rate of 15% and an unemployment benefit replacement rate of 75%, the fiscal benefit of one meeting with the average caseworker is estimated to be the equivalent of 11 average daily wages.

Results further show that the economic value of caseworkers is largely heterogeneous. Absences of caseworkers in the lower half of the productivity distribution show no effect. In contrast, the average meeting's estimated return would multiply by a factor of about 2.5 if all caseworkers were as productive as caseworkers in the upper half. This heterogeneity cannot be explained by differential prescriptions of labor market programs, suggesting that personal counseling styles are central inputs to a successful job search assistance.

These two main 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 on the one hand target the counseling qualities of existing caseworkers, e.g., through trainings or incentives. Lazear and Oyer (2013) review existing evidence on the determinants of productivity in firms, as offered by research in personnel economics. On the other hand, the selection of individuals attracted to the caseworker profession appears central for the successful provision of services to the unemployed. 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. (2018) find that agents attracted to the public service by career concerns have more skills and ambitions than those attracted by purely altruistic motives. Future research is needed to understand which interventions and personnel policies work to increase caseworker performance in welfare systems.

As a final policy implication, the findings in this paper show that workplace absenteeism can induce substantial costs. This holds in particular when the absent worker is difficult to replace due to high work productivity. Commonly used investments into the reduction of work absences include preventive health interventions or the improvement of working conditions. This paper presents evidence that such investments may translate into important economic payoffs.

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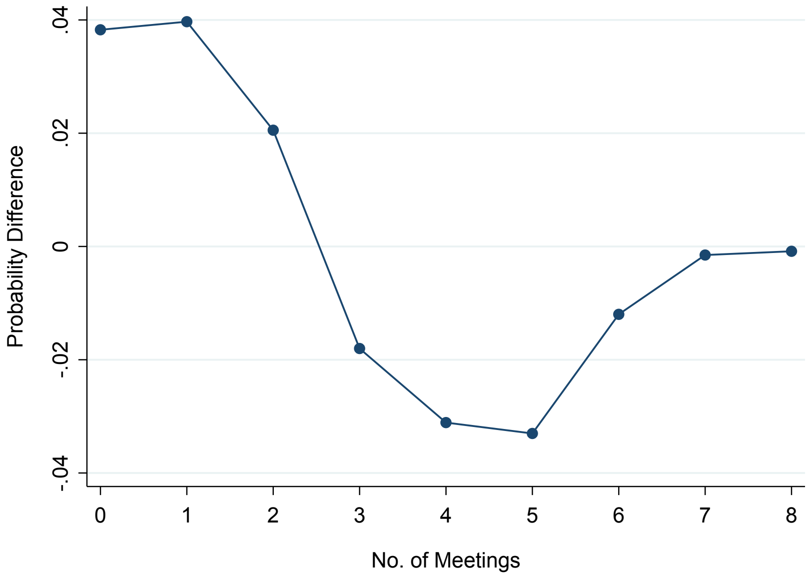
Appendix

A Additional Figures

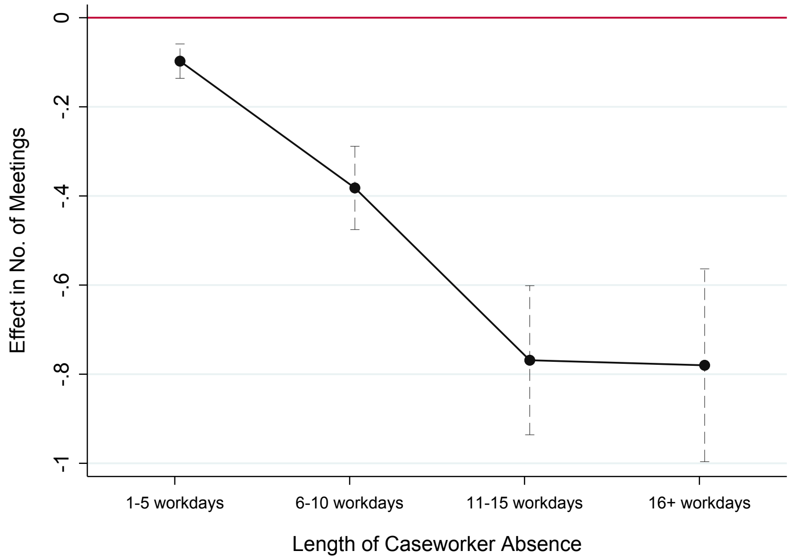
Figure A.1: Illustration of Complier Reactions



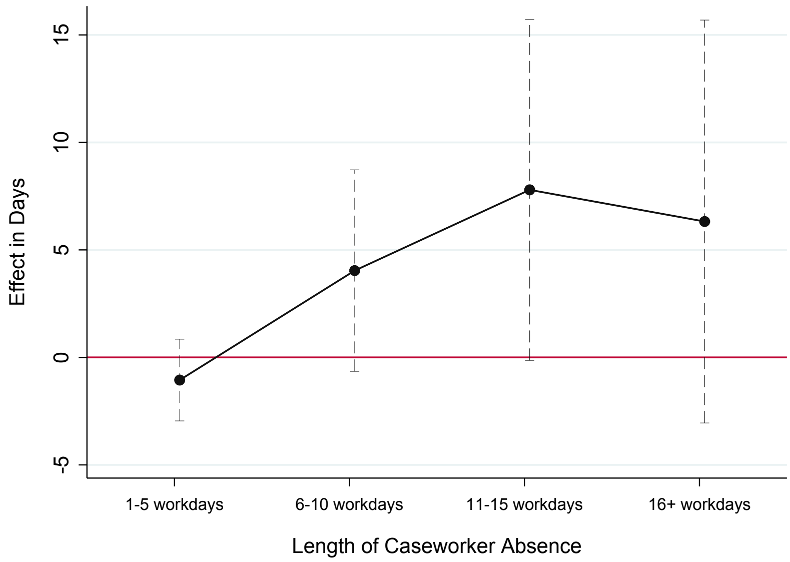
The graph plots the average difference in the probability of having x meetings between individuals with $Absence_{j(i)} = 1$ and individuals with $Absence_{j(i)} = 0$. The difference is computed at the caseworker level and then averaged over all caseworkers with at least one absence in the sample.

Figure A.2: Effect of Caseworker Absences, by Length of Absence

(a) Number of Meetings with the Assigned Caseworker

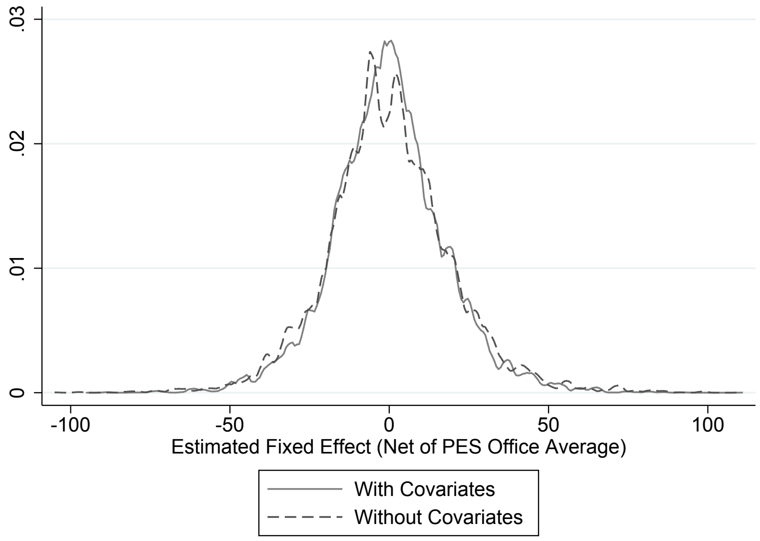


(b) Unemployment Duration in Days



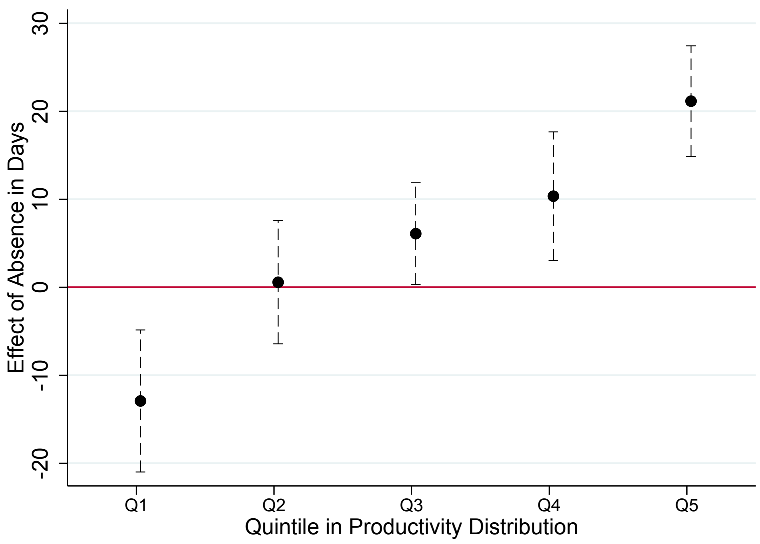
The figure plots estimated effects of caseworker absences with different lengths, occurring within the first six months after the job seeker's entry. In case a job seeker experiences more than one caseworker absence during this period (0.3% of the sample), the longer absence is used. The x-axis denotes the number of days (in five workday categories) of the caseworker's absence. The reference group contains job seekers with no caseworker absence. The y-axis denotes the size of the estimated coefficient. In panel (a), the outcome is the number of meetings realized with the assigned caseworker during the first six months of unemployment, normalized by the share of days a job seeker remained unemployed during this period. In panel (b), the outcome is the 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, as well as job seeker covariates (summary statistics reported in table B.1). Dashed lines represent 90% confidence intervals. N=379358.

Figure A.3: Kernel Density of Caseworker FE on the Duration of Unemployment



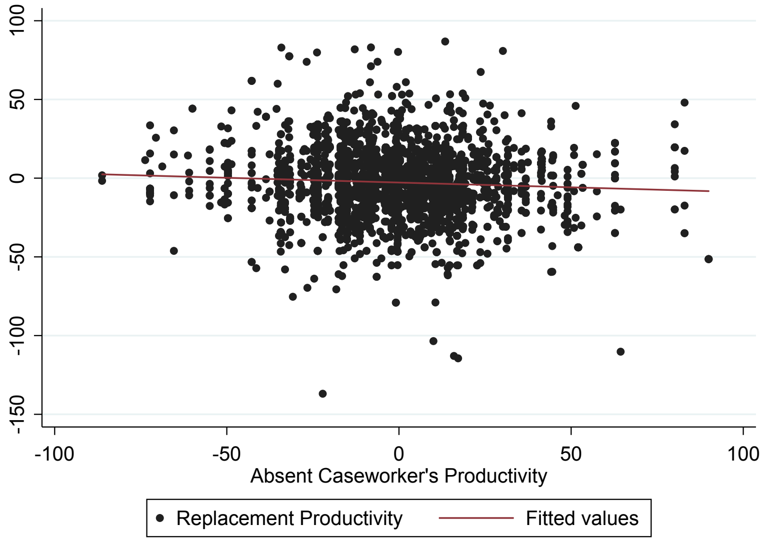
N=379358. The graph plots caseworker fixed effects estimated by equation 3, subtracting the PES office average duration of unemployment (to compare caseworkers within offices). The distribution is weighted by the number of job seekers per caseworker. The dashed line reports estimates from a regression without job seeker covariates. The solid line reports estimates from a regression with covariates.

Figure A.4: Effect of Caseworker Absence on the Duration of Unemployment, by Productivity Quintile



N=379358. Qk equals one if the caseworker ranks in the k^{th} quintile of the productivity distribution. The quintiles are interacted with the incidence of a caseworker absence of more than a week during the job seeker's first six months of unemployment. Regressions include fixed effects for the job seeker's calendar month of entry into unemployment, caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). Dashed lines represent 90% confidence intervals.

Figure A.5: Productivity: Absent Caseworker vs. Replacing Caseworker



N=2727 (individuals with a caseworker absence and at least one replaced meeting). The plotted measures of caseworker productivity are caseworker fixed effects estimated by equation 3, subtracting the PES office average duration of unemployment (to compare caseworkers within offices).

B Additional Tables

Table B.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			379358		

Table B.2: Sensitivity Analyses

	Effect of Meeting on UE Duration (2SLS)	N
1. Baseline	-11.604***	379358
	(4.167)	
2. Exclude caseworkers with < 60 Cases	-11.791***	362590
	(4.316)	
3. Include caseworkers with < 30 cases	-11.536***	387601
	(4.177)	
4. Exclude absences of ≥ 30 days	-11.238***	379267
	(4.276)	
5. No update of caseworker assignment	-10.840***	378380
	(4.206)	
6. 2 week update of caseworker assignment	-11.026***	382185
	(4.279)	

Estimates report the effect of the number of meetings which a job seeker had with her assigned caseworker during the first six months after unemployment entry. Meetings are instrumented by the incidence of a caseworker absence. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table B.1). The unemployment duration is capped at 520 days (12.3% of the sample). In row 2, caseworkers who have less than 60 cases over the sample period are excluded (in the baseline specification, caseworkers with less than 30 cases are excluded). In row 3, all caseworkers are included. In row 4, job seekers affected by absences of 30 or more days are excluded. In row 5, initially made caseworker assignments are not updated (in the baseline specification, assignments are updated if the update occurs up to week 1 after the job seeker's entry). In row 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=2250).

Table B.3: Identification Test: Pre-Determined Job Seeker Characteristics and Caseworker Absences, by Caseworker Productivity

	P(Caseworker Absence)	
Sample	High Productivity CW	Low Productivity CW
	(1)	(2)
Female	-0.003	-0.010
	(0.009)	(0.010)
Experience < 3 years	0.007	0.015
	(0.012)	(0.012)
HH size >2	0.000	0.001
	(0.001)	(0.001)
Aged > 40	0.005	0.002
	(0.007)	(0.008)
Low education	-0.003	-0.006
	(0.009)	(0.008)
Log previous earnings	0.002	-0.010
	(0.010)	(0.010)
UE in last 12 months	0.006	-0.001
	(0.008)	(0.008)
PBD>260	0.001	0.010
	(0.008)	(0.008)
Replacement rate > 75%	0.011	0.004
	(0.008)	(0.008)
p-value (joint significance)	0.869	0.850
Outcome Mean	0.029	0.028
Month FE	Yes	Yes
Caseworker FE	Yes	Yes
N	183303	198820

"High Productivity CW" (column 1) includes individuals whose caseworker has an estimated productivity that is higher than the PES office median. "Low Productivity CW" (column 2) includes individuals whose caseworker has an estimated productivity that is equal or lower than the PES office median. The outcome variable equals one if the job seeker's caseworker had an absence spell of more than one week during the first six months after the job seeker's unemployment entry. The unit of observation is the job seeker. In all columns, regressions include fixed effects for the calendar month of entry into unemployment, as well as caseworker fixed effects. PBD=potential benefit duration in days. All explanatory variables, except log previous earnings, are specified as binary variables. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2250).

Table B.4: Identification Test: Workload and Caseworker Absences, by Caseworker Productivity

	P(Caseworker Absence)	
Sample	High Productivity CW	Low Productivity CW
	(1)	(2)
Total Cases in t-1	0.009	-0.020
	(0.015)	(0.021)
Total Cases in t-2	-0.024	0.030
	(0.021)	(0.029)
Total Cases in t-3	0.015	-0.007
	(0.012)	(0.018)
New Cases in t-1	-0.003	0.029
	(0.017)	(0.024)
New Cases in t-2	0.023	-0.004
	(0.016)	(0.025)
New Cases in t-3	-0.026**	0.012
	(0.011)	(0.012)
p-value (joint significance)	0.334	0.295
Outcome Mean	0.005	0.006
Month FE	Yes	Yes
Caseworker FE	Yes	Yes
N	30535	27508

"High Productivity CW" (column 1) includes caseworkers with an estimated productivity that is higher than the PES office median. "Low Productivity CW" (column 2) includes includes caseworkers with an estimated productivity that is equal or lower than the PES office median. Coefficients are multiplied by 100. The unit of observation is the caseworker-month cell. The outcome variable equals one if the caseworker had an absence spell of more than one week in calendar month t. The total number of cases is defined as the stock of job seekers held by the caseworker in a given calendar month. The number of new cases are defined as the number of job seekers assigned to the caseworker in a given calendar month. In all columns, regressions include calendar month and caseworker fixed effects. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2250).