Four Empirical Essays on the Economics of Job Search

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General Introduction

Modern unemployment insurance (UI) systems are designed to meet two objectives. On the one hand, they provide financial transfers to compensate income losses. On the other hand, they include formalized support and incentives to fasten the transition into re-employment. This second objective is commonly motivated by the costs imposed by prolonged unemployment. For society as a whole, the most direct cost is caused by the payment of unemployment benefits, which account for 0.9% of GDP in the average OECD country (OECD, 2017). Further, high unemployment leads to foregone tax incomes, reductions in aggregate output and losses in human capital (Layard, Nickell and Jackman, 1991). On the individual side, unemployment has been found to cause substantial harm to work careers. Kroft, Lange and Notowidigdo (2013) and Eriksson and Rooth (2014) show that prolonged unemployment is perceived as a negative signal on the labor market, which lowers the probability to be invited to a job interview. Schmieder, Von Wachter and Bender (2016) find that the time spent in unemployment has a causal negative effect on re-employment wages, implying that the losses in individual income streams are potentially long-lasting. Moreover, a number of studies identify negative non-pecuniary effects of unemployment, for instance on physical and mental health (e.g. Clark and Oswald, 2016; Sullivan and von Wachter, 2016).

To prevent these costs, welfare states nowadays aim at designing UI schemes which minimize the length of unemployment spells. A variety of institutions and incentives, which are embedded in UI schemes across OECD countries, reflect this attempt (OECD, 2013). For instance, job seekers entering UI are often provided with personal support through a caseworker. They also face the requirement to regularly submit a minimum number of job applications, which is typically enforced through the use of benefit cuts in the case of non-compliance. Moreover, job seekers may systematically receive information on their re-employment prospects.[1]

As a consequence, UI design has become a complex task. Policy makers need to define not only the amount and duration of benefit payments, but also several additional choice parameters. These include the intensity and quality of personal support through caseworkers, the level of job search requirements, the strictness of enforcement, and the information provided to unemployed individuals. Causal estimates on how these parameters affect re-employment outcomes are thus central inputs to the design of modern UI systems: how much do individual caseworkers influence the transition out of unemployment? Does the requirement of an additional job application translate into increased job finding? Do individuals behave differently when facing a strict versus mild enforcement system? And how does information on re-employment prospects influence the job search decision?

Obtaining evidence on these questions is non-trivial, due to a lack of data availability and exogenous variation in the parameters of interest. To overcome this challenge, this dissertation proposes four novel research designs. Chapters one to three elaborate quasi-experimental identification strategies, which are applied to large-scale administrative data from Switzerland. They, respectively, measure how personal interactions with caseworkers (chapter one), the level of job search requirements (chapter two) and the strictness of enforcement (chapter three) affect re-employment outcomes. Chapter four proposes a structural estimation approach, based on linked survey and administrative data from Germany. It identifies how over-optimism on future wage offers affects the decision to search for work, and how the provision of information changes this decision. In the following, I briefly motivate the parameters studied in this dissertation and outline how the different chapters contribute to their understanding.

Chapter 1: The Role of UI Caseworkers Chapter one focuses on the effect of face-to-face interactions between unemployed individuals and their caseworker. In most OECD countries, the interaction with a caseworker has become unavoidable to UI benefit recipients: the registration at the employment service entails the assignment to a caseworker, who supports and monitors the job search process through regular

[1] This list of instruments used in UI only covers the focus this dissertation. It is by no means complete, as modern UI systems offer various additional instruments, (e.g. training programs or employment subsidies) to support the transition into re-employment. Card, Kluve and Weber (2010, 2015) provide comprehensive overviews on commonly used active labor market programs.

meetings. Caseworkers are thus the central human resources used to re-integrate UI benefit recipients into the labor market.

From an individual perspective, caseworkers can have a large impact on the job search process, for instance by providing decisive advise or information. The interaction with a caseworker may, however, also be worthless if it does not contain elements which help the individual in finding a job. From a policy perspective, the human resources of caseworkers are a margin of investment. Whether human resource investments can pay off depends on how caseworkers influence the outcomes of unemployed individuals. Increasing the number of caseworkers may be beneficial if the frequent interaction with a caseworker fosters the exit from unemployment. Moreover, investments into the quality of caseworkers are expected to pay off if individuals in the caseworker profession strongly shape the returns to an interaction. In this case, policy makers may want to attract high quality caseworkers and invest into the qualifications of existing caseworkers. Understanding if and how much individual caseworkers matter for the unemployment exit of job seekers is thus a necessary condition to design human resource policies in UI systems.

Obtaining causal empirical evidence on the role of caseworkers is difficult. The main challenge is that caseworkers choose how intensely to interact with a given job seeker. Most likely, this choice does not occur randomly. As a solution, I propose to explore unplanned caseworker absences as a source of quasi-random variation in how frequently an individual interacts with her caseworker. Importantly, absences generate exogenous variation along two dimensions: on the one hand, they reduce the average number of caseworker meetings. On the other hand, they may lead to 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.

Using register data on the universe of Swiss job seekers entering UI between 2010 and 2012, I proceed in two steps. I first exploit absences to estimate the return to an additional meeting with the average caseworker. Results show that one foregone caseworker meeting increases the duration of unemployment by 10 days, i.e., by 5% relative to the mean. I further find that caseworker absences have negative spillover effects on the performance of present colleagues. In a second part, I identify the importance of quality differences. To this end, I interact the incidence of absence days with the absent caseworker's productivity at work. Productivity is measured as the caseworker's fixed effect on unemployment exit when present at work (conditional on office-time effects). I find that the average effect of caseworker absences on the duration of unemployment masks important heterogeneity: it amounts to 9% for caseworkers in the upper productivity tercile and to 5% for caseworkers in the medium tercile. If the caseworker ranks in the lowest tercile, the absence has no effect. This heterogeneity cannot be explained by a differential usage of active labor market programs, suggesting that counseling skills and personality traits of caseworkers are central inputs in the job matching process. Taken together, the results point to large margins for reducing the length of unemployment spells by investing into the human resources of caseworkers, along both the quantity and the quality dimension.

Chapter 2: The Effect of Job Search Requirements Chapter two (joint work with Patrick Arni) analyzes the effect of effort incentives generated by job search requirements. Most modern UI systems condition the receipt of benefits on the provision of a minimum number of job applications per month (see, e.g. OECD, 2013; Venn, 2012). Individuals therefore see their quantity of search effort constrained by a fixed requirement. For policy makers, individual reactions to the amount of required effort are important inputs for deciding on the size of the requirement. Ex-ante, it is unclear whether job finding increases in response to incentives which exclusively target the quantity of effort. Due to a lack of available data on individual-level requirements and effort provision, there is up to date no empirical evidence on this question. More generally, there is few evidence on the link between search effort and job finding, although effort is commonly assumed to be the central driver of the job search process (c.f. the standard model by Mortensen, 1986).

We address this gap by identifying how the number of required job applications affects effort provision and labor market outcomes. The study relies on unique administrative data from the Swiss UI, reporting both required and actually provided monthly job applications on an individual level. For causal identification, we exploit caseworker stringency as an instrument for the individual requirement.[2] Within offices, some caseworkers tend to set higher requirements than others. As the match between caseworkers and job seekers occurs in a conditionally random way, this gen-

[2]This approach is inspired by an increasing number of studies using judge or caseworker leniency as an instrument for individual treatments (e.g. Aizer and Doyle, 2015; Kling, 2006; French and Song, 2013; Dahl, Kostol and Mogstad, 2014; Autor et al., 2017; Bhuller et al., 2017; Maestas, Mullen and Strand, 2013).

erates exogenous variation in the amount of required effort.[3] We provide a detailed assessment illustrating the instrument's relevance and validity in terms of exclusion and monotonicity.

Results show that the duration of formal unemployment and the duration to re-employment both decrease by 3% if the requirement increases by 1 monthly application. We further find that that the elasticity of effort to the requirement is strong, but imperfect: one required application translates into 0.67 provided applications. As a consequence, the effect of a provided application reduces the length of unemployment spells by 4%. We identify substantial heterogeneity in the effect of the requirement level: it is strongest for individuals with relatively low voluntary effort (proxied by effort provided before the first caseworker meeting). This is in line with theoretical intuition, as the requirement is more likely to impose a relevant increase in effort for these individuals. Further, we find that job seekers with relatively low skill levels react most to the requirement level. This suggests that requirements are less effective when the quality of applications matters, which is likely the case for individuals with high skill levels.

Finally, results show that the requirement level determines the cost and incidence of compliance. The probability of non-compliance increases by 12% in response to an additional required application. We, however, find only minor side effect on the quality of accepted jobs: job stability reduces by 0.3% per required monthly application and wages are unaffected.

Chapter 3: The Effect of Enforcement Strictness Job search requirements are typically enforced through benefit sanctions, which reduce UI benefit payments if the requirement is not met (see, e.g. OECD, 2013; Venn, 2012). When setting up the enforcement system, the central choice parameter is the strictness of enforcement. Policy makers can directly determine enforcement strictness by deciding on the probability that a sanction is imposed in the case of a non-compliance. While a strict system can have the advantage of reducing moral hazard, it may also affect the behavior and outcomes of non-compliant individuals who are more likely to be sanctioned in such a system.

Chapter three (joint work with Patrick Arni) analyzes how enforcement strictness affects the labor market outcomes of non-compliant individuals. A prior literature,

[3]Compared to chapter one of this thesis, the objective is not to assess the overall value of a caseworker, but to use variation in the behavior with respect to one specific treatment.

relying on timing-of-events models, has found that the imposition of benefit sanctions reduces the duration of unemployment, but potentially harms the quality of re-employment outcomes.[4] However, there exists no estimate on the effects of policy changes in the probability of imposing benefit sanctions. Our study addresses this gap based on quasi-experimental variation.

To identify the effects of a strict versus mild enforcement system, we exploit a reform which occurred in the Swiss UI in April 2011. For a particular type of non-compliance, the reform increased enforcement strictness in a sharp and unanticipated way. In the pre-reform system, a second chance was given to individuals who had not submitted proof of their application activity by a given deadline. There was thus a low sanction probability for these individuals (around 0.3). The reform omitted the second chance, implying that the sanction probability more than doubled (up to 0.65). Using a (dynamic) difference-in-difference design and administrative data from the Swiss UI, we assess how non-compliant individuals were affected by this policy change. Results show that the exit from unemployment increases by 12% in response to the reform. However, the effect is pre-dominantly driven by exits into non-compensated job search, as affected individuals increasingly leave the UI system without having found a job. This suggests that the strict enforcement response decreases the utility of being in the UI system, such that unemployment without benefits becomes relatively more attractive. The overall duration to re-employment is unaffected by the reform, suggesting that formal job search with benefits is fully substituted by informal job search without benefits. Although we only find minor negative effects on the quality of re-employment jobs, the findings imply that a strict enforcement system can substantially harm the income stream of non-compliant individuals. This finding stands in contrast to the first two chapters, where both instruments fastened unemployment exits through job finding. A likely explanation is that enforcement is very negatively perceived by affected job seekers. Strict enforcement regimes therefore bear the risk of crowding out certain individuals from the system.

Chapter 4: The Role of Subjective Wage Expectations Chapter four (joint work with Sascha Drahs and Luke Haywood) shifts the focus to the role of un-

[4]These studies include, in particular, Van den Berg, Van der Klaauw and Van Ours (2004); Abbring, van den Berg and van Ours (2005); Lalive, van Ours and Zweimueller (2005); Rosholm and Svarer (2008); Arni, Lalive and Van Ours (2013); Van den Berg and Vikstroem (2014).

certainty about future wage prospects. Unemployed individuals make their job search decision under the consideration of future payoffs and therefore need to build subjective expectations on these payoffs. For policy makers, it is important to understand how subjective expectations influence decisions and outcomes, since expectations can be influenced through relatively simple information treatments.[5]

In chapter four, we analyze the expectations held by unemployed individuals on their wage prospects. We investigate to which extent subjective expectations are in line with rational expectations, and assess how potential deviations from rationality affect the job search decision. Based on matched survey and administrative data from Germany (IZA/IAB Linked Evaluation Dataset), we observe that unemployed individuals over-estimate their future wage prospects by 10% on average. In particular, individuals do not anticipate that prolonged unemployment comes along with reduced wage offers. To assess the consequences of wage optimism, we set up and estimate a dynamic job search model in which subjective expectations potentially differ from reality. We use our model estimates to simulate a counter-factual scenario in which there is perfect information about average future wage offers. This exercise reveals highly dynamic effects of wage optimism: at the beginning of the spell, optimism lowers job search effort, because individuals ignore that they risk receiving worse offers when staying unemployed for too long. At later stages, when wage offers have already worsened, optimism prevents individuals from becoming discouraged: perfectly informed individuals search less because they know that they can only get worse wage offers. The findings suggest ambiguous implications of information provision.

[5] A concrete example of an information treatment is an experimental study by Altmann et al. (2017), who send out information brochures to newly unemployed individuals. They find that re-employment increases for individuals with a high risk of long-term unemployment.

CHAPTER 1

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

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

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1.1 Introduction

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

In this chapter, I ask for the economic value of individual caseworkers employed in UI systems. I first estimate how the presence of a caseworker affects, on average, the job seeker's exit from unemployment.[2] In a second step, I study how the effect differs by the caseworker's rank in the productivity distribution. From a policy perspective, these analyses reveal to which extent welfare states can improve the effectiveness of social services by investing into their human resources.

My research design relies on the incidence of unplanned caseworker absences.[3] Importantly, absences are not analyzed as the intervention of interest, but as a source of exogenous variation in the quantity and quality of caseworker interactions experienced by unemployed individuals: on the one hand, absences reduce the average number of caseworker meetings. On the other hand, they may induce the replacement by a different caseworker in the office. In this case, the unemployed individual can experience a loss or a gain in caseworker quality. I first exploit absences to estimate the return to

[1]Further, an increasing literature uses the stringency of caseworkers or judges as an instrument for individual treatments (e.g. Kling, 2006; French and Song, 2013; Dahl, Kostol and Mogstad, 2014; Autor et al., 2017; Bhuller et al., 2017; Maestas, Mullen and Strand, 2013, and chapter two of this dissertation).

[2]In this chapter, I observe the unemployment exit through the de-registration from unemployment insurance. I do not observe subsequent employment spells. As the data report the reason of de-registration, I can still draw conclusions on the job seeker's employment status at the time of de-registration. In the other chapters, the UI data could be linked to social security data on employment spells, allowing for an explicit distinction between the duration of unemployment and the duration of non-employment.

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

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.[4] Conditional on the fixed effects, neither job seeker characteristics nor workload predict the incidence of caseworker absences. Placebo tests further corroborate the approach by demonstrating that future absences do not affect current outcomes.

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

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

In the second part of the analysis, I interact the incidence of caseworker absences with the absent caseworker's productivity at work. Provided that replacements are

[4]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.

in expectation performed by the average caseworker in the office, absences of caseworkers with average productivity should not cause any quality effect.[5] If the absent caseworker is, however, more productive than the average, the meeting with a replacement will in expectation cause a quality loss. The reverse applies when the absent caseworker is less productive than the average. I estimate a caseworker's relative productivity at work as her fixed effect on the six-months exit probability of job seekers who are not affected by a long absence. By comparing caseworkers within office $\times$ quarter cells, I hold the working environment constant. The assignment of job seekers to caseworkers is based on availability or on observable job seeker characteristics included in the regression, ensuring that caseworker fixed effects can indeed be interpreted as a measure of productivity.

Results confirm that there are large quality differences between caseworker meetings. Strikingly, absences of caseworkers in the lowest tercile cause a zero net effect: the loss in meeting quantity is offset by a productivity gain due to the replacement by a better caseworker. In turn, absences of caseworkers in the upper tercile, who are in expectation replaced by a worse caseworker, induce twice the average effect. This strong effect is mostly driven by exits to stable jobs, suggesting that productive caseworkers achieve good job matches.

To understand mechanisms, I explore whether the differential success of caseworkers can be explained by the active labor market programs (ALMPs) they prescribe. Results show that ALMP assignments decrease, on average, in response to caseworker absences. However, the effects hardly vary by caseworker productivity. In addition, I find no relation between an individual's job search requirement and the caseworker's absence. While these findings do not imply that ALMP assignments or requirements do not affect unemployment exit. They, however, point to an important, independent influence of the caseworker's personal qualities and counseling styles, which are difficult to replace.[6] This intuition is in line with findings from other economic contexts.

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

[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 apply programs in a bureaucratic way. Lechner and Smith (2007) use propensity score matching and find that the payoffs of treatment assignments made by a statistical program exceed those made by caseworkers.

A large literature documents that teacher quality is a central determinant of student performance (e.g., Rockoff, 2004; Rivkin, Hanushek and Kain, 2005; Rothstein, 2010; Chetty, Friedman and Rockoff, 2014*b*,*a*). Further, individual managers have been found to be central determinants of firm policies (Bertrand and Schoar, 2002) and worker productivity (Lazear, Shaw and Stanton, 2015). Jäger (2016) shows that firms face difficulties in finding replacements after exogenous worker separations, in particular when human capital is largely firm-specific. In a final analysis, I show that the value of caseworker presence also increases in experience, suggesting that caseworkers accumulate task-specific human capital which decreases their replaceability.

By identifying a strong role of individual caseworkers, I complement previous experimental evidence on the effects of counseling in UI systems. 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 public employment service (PES). Graversen and Van Ours (2008) and Rosholm (2008) evaluate a Danish activation program, which included both a two week job search program and an intensified contact with the employment service. Hägglund (2011) estimates the anticipation effect of being invited to a meeting at the PES. Maibom, Rosholm and Svarer (2017) evaluate several Danish experiments, including different combinations of early meetings, and Van Landeghem, Cörvers and de Griep (2017) estimate the effect of a collective information session followed by a one-on-one interview.[7] All of the evoked studies find that counseling can increase unemployment exit. However, they remain agnostic on the specific role of individual caseworkers. Most related, Behncke, Frölich and Lechner (2010*a*) find that "tough" caseworkers are more successful and Huber, Lechner and Mellace (2017) show that this result cannot be explained by ALMP assignments.[8] My findings show more generally that investments into caseworker quality can strongly increase the effectiveness of services provided to the unemployed.

In addition, this chapter adds to the scarce evidence on the micro-foundations of the job matching function and on the role of public institutions. One part of the literature investigates how workers direct their search to a job match (e.g. Blau and Robins, 1990; Holzer, Katz and Krueger, 1991; Marinescu and Wolthoff, 2016). A few studies

[7] Card, Kluve and Weber (2010, 2015) provide a comprehensive overview on interventions targeted at unemployment benefit recipients.

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

have pointed out that the efficiency of the PES determines how well unemployed workers are matched to a new job. Using structural estimation techniques, Fougère, Pradel and Roger (2009) and Launov and Wälde (2016) show that a more productive PES increases outflows from unemployment.[9] My results add an additional layer to these findings, by showing that caseworker productivity is an important input in the job matching process.

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

The remainder of the chapter is structured as follows: section 1.2 lays out the institutional context and the data sources. It further shows how absences are measured and assigned to the job seeker's unemployment spell. In section 1.3, I discuss the conceptual link between caseworker absences and the exit from unemployment. Section 1.4 presents the empirical analysis on the average effects of caseworker absences. Section 1.5 decomposes the effect according to the absent caseworker's productivity, and section 1.6 concludes.

1.2 Institutions and Data

1.2.1 Caseworkers in the Swiss UI

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

To claim benefits, individuals register at the local Public Employment Service (PES)

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

office. As in most OECD countries, the registration is followed by the assignment to a caseworker. According to a survey realized by Behncke, Frölich and Lechner (2010*a*), the most common assignment criteria are caseload, occupation or industry (all mentioned by about 50% of surveyed caseworkers and PES officials) and randomness (mentioned by 24%).

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

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

1.2.2 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.[10] The data, which are described in detail by Gast, Lechner and Steiger (2004), include extensive information on the entry into and exit from formal unemployment,[11] socio-demographics, 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 chapter, I can link the data to all scheduled meetings on the job seeker-caseworker level, with the exact date and time of each meeting. It is further reported whether a scheduled meeting was realized, canceled, re-scheduled or whether the job seeker did not appear at the meeting.

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

[11] As I could not link the data to social security records, I do not observe subsequent employment spells (as in chapters 2 and 3). Since the UI data report the reason of de-registration from unemployment, I can, however, draw conclusions on the job seeker's employment status at the time of de-registration.

I restrict the analysis to full-time unemployed individuals aged 20-55, who are eligible for UI and not eligible for disability benefits. I drop 1.11% (N=4,351) of observations because the caseworker assigned to the individual never appears in the meeting database, and additional 2.15% (N=8,520) because the caseworker has less than 30 cases.[12] These have a high likelihood of being mis-classified assignments.

The sample used in the empirical analysis contains 382,123 job seekers assigned to 2,269 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 assignment made up to one week after the job seeker's entry into unemployment: if the caseworker assignment gets updated during the first week of unemployment, I use the updated assignment. This allows correcting for erroneously made initial assignments.[13]

Table 1.1: Summary Statistics on Meetings over the UE Spell

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

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

Table 1.1 shows summary statistics on how often job seekers interact with a caseworker during different three-months periods of the unemployment spell. In total, job seekers experience about 2.1 meetings during the first three months of unemployment. The meeting intensity decreases over the unemployment spell, down to 1.6 meetings in months 9-12. In particular, meetings with the initially assigned caseworker drop

[12]Results are robust to modifying this cutoff (c.f. section 1.4).

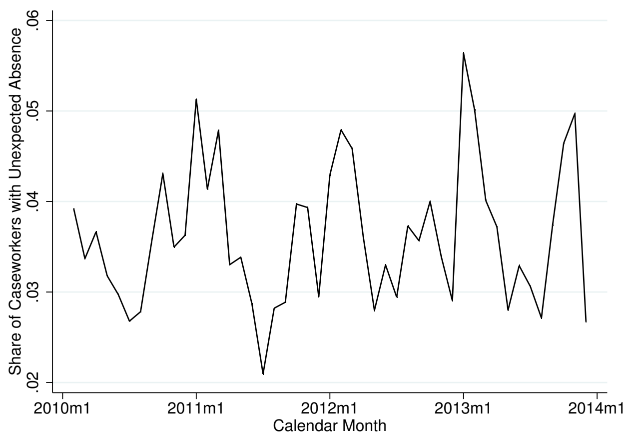
[13]Results are robust to not updating assignments or to using updates made up to week 2 instead (c.f. section 1.4).

from 1.5 in months 1-3 to 1.0 in months 9-12. In turn, the average number of meetings with a different caseworker increases from 0.6 to 0.8. From anecdotal evidence, it is common practice that caseworkers switch cases after around six months. As only initial caseworker assignments can be considered exogenous to the dynamics of the unemployment spell, I focus on the role of caseworkers during the first six months of unemployment.

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

Therefore, I define an unplanned absence to start on the day during which none of a caseworker's scheduled meetings take place. Unrealized meetings take the status "scheduled", "canceled" or "re-scheduled". The two other possible categories are "realized" or "job seeker did not appear". I apply a conservative approach to ensure that unrealized meetings indeed reflect caseworker absences: I require that during at least two subsequent day entries, at least two meetings were scheduled and not realized.[14] It is highly unlikely that such a sequence of consecutively unrealized meetings by one caseworker is caused by chance. The absence duration is computed as the number of workdays between the first day of the absence and the first day at which the caseworker is reported to conduct a meeting again.

Figure 1.1: Monthly Share of Caseworkers with Unplanned Absence



The figure shows the share of caseworkers who have at least one day of unplanned absence per calendar month. The measurement of unplanned absences based on unrealized caseworker meetings is described in section 1.2.2.

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

Figure 1.1 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 1.2 shows caseworker-level summary statistics on the number of workdays and the number of absent days. The average caseworker unexpectedly misses 2.3% of her workdays. Taken over a year with 230 workdays,this means that, on average, 5.3 workdays are missed due to an unplanned absence. This number appears of reasonable size, as the average Swiss public sector employee missed 63 hours ($\approx$ 7.5 days) per year in 2015 (Swiss Federal Statistical Office, 2016), which also contain anticipated absences (e.g., planned surgeries).

Table 1.2: Summary Statistics on Unexpected Absences: Caseworker Level

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

Summary statistics are at the level of the caseworker. The sample covers job seeker inflows between 2010 and 2012. Workdays are the number of days during which caseworkers schedules meetings with job seekers in the sample. The measurement of unexpected absences based on unrealized caseworker meetings is described in section 1.2.2.

The analysis focuses on absences occurring up to month six after unemployment entry. Later absences will be used to run placebo tests. Table 1.3 reports summary statistics on how job seekers are affected by caseworker absences. The average job seeker's caseworker has 0.45 workdays of unplanned absence during a given three months interval after entry.[15] About 0.9% of job seekers are affected by ten or more workdays of caseworker absence over a given three-months period. Importantly, the largest share of the variation in the exposure to absences comes from within-caseworker variation. This ensures that there exists enough variation to estimate models with caseworker fixed effects.

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

Table 1.3: Summary Statistics on Unexpected Absences: Job Seeker Level

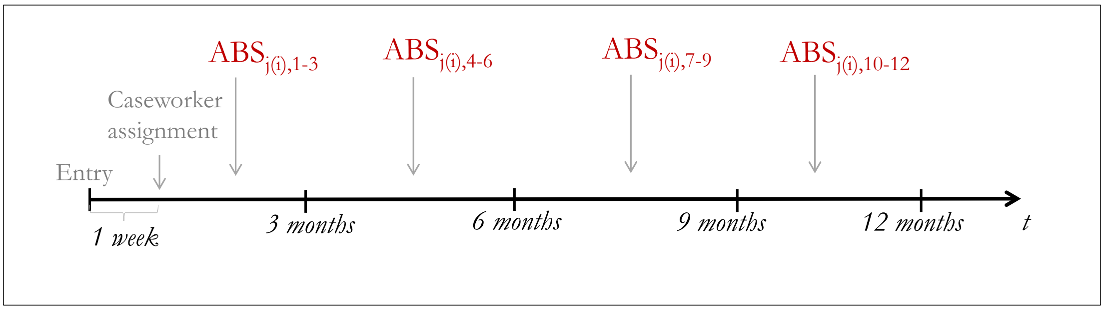
	Time Period after Entry into UE			
	Months 1-3	Months 4-6	Months 7-9	Months 10-12
Workdays with CW absence				
Mean	0.409	0.474	0.463	0.491
SD	1.870	2.035	2.044	2.197
SD between CW	0.969	1.168	1.077	1.119
SD within CW	1.673	1.804	1.821	1.953
CW absence $\geq$ 10 workdays				
Mean	0.009	0.009	0.010	0.010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

Summary statistics are at the level of the job seeker. The sample inflow period is 2010-2012. The measurement of unexpected absences is based on unrealized caseworker meetings, as described in section 1.2.2. The number of caseworker absence days in a given period is reported independently of the job seeker's exit from unemployment at the time of the absence. In months 1-3, the first week after entry is excluded to allow for updates in the assignment of job seekers to caseworkers.

To assign absences to the individual unemployment spell, I count the number of workdays during which the job seeker's caseworker was absent in a given three-months period t after entry.[16] Figure 1.2 illustrates the assignment over the course of the unemployment spell. For instance, the variable $ABS_{j(i),1-3}$ contains the number of days during which caseworker j was absent during months 1-3 after job seeker i's entry (excluding week 1, during which caseworker assignments are allowed to be updated). $ABS_{j(i),t}$ is an intended treatment, as it does not condition on the job seeker's survival in unemployment at the time of the absence. The empirical analysis will scale the effects of the intended treatment to the average treatment effect on job seekers who are still unemployed at the first day of caseworker absence.

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

Figure 1.2: Timeline



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

1.2.3 Caseworker Absences and Unemployment Exit: Raw Data

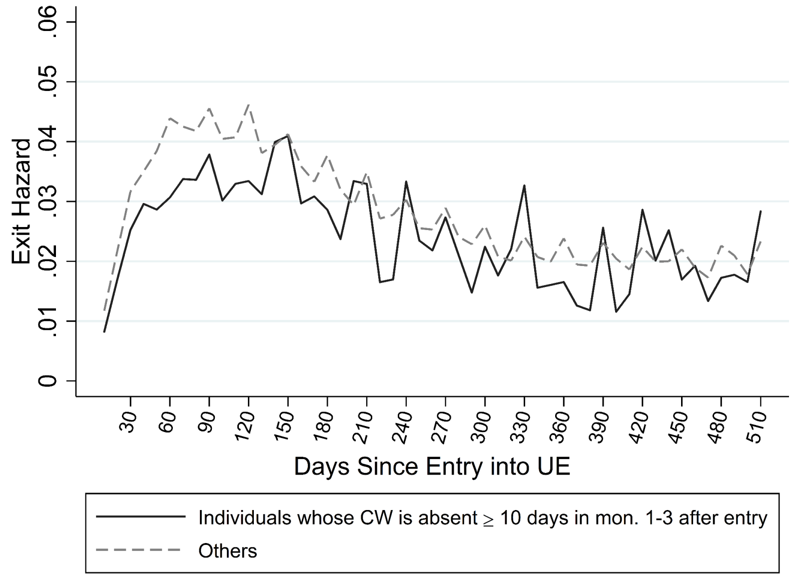
As a purely descriptive exercise, figure 1.3 plots the unemployment exit hazard and survival rate over the first 520 days of unemployment.[17] The solid line includes job seekers whose caseworker is absent during at least ten workdays over the first three months after entry into unemployment (0.9%). To avoid dynamic selection, this status is assigned regardless of whether the job seeker is still unemployed at the start of the absence (intention-to-treat). The dashed line includes all other job seekers in the sample.

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

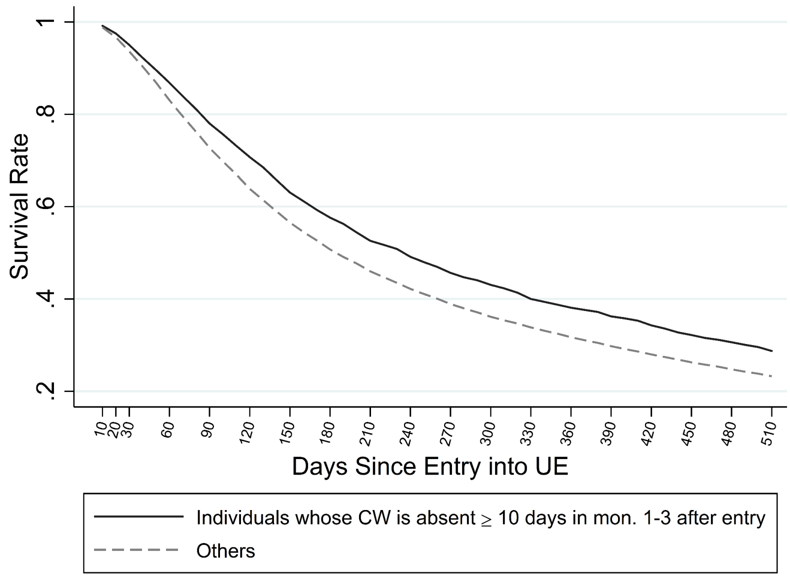
[17] 520 days is the maximum potential benefit duration in the sample.

Figure 1.3: Caseworker Absences and UE Exit: Raw Data

(a) Unemployment Exit Hazard



(b) Unemployment Survival Rate



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

1.3 Conceptual Framework

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

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

$$c_{j(i)} = (1+q_j)m = \begin{cases} (1+q_j)\, m^0 & \text{if } A_{j(i)} = 0 \\ (1+\underbrace{\bar{q}_{-j}}_{\approx 0})\, m^1;\; m^1 < m^0 & \text{if } A_{j(i)} = 1 \end{cases} \tag{1.1}$$

If caseworker j is present at work after being assigned to i ($A_{j(i)} = 0$), i has a fixed number of meetings with j.[18] The meeting quantity affects the job search process through two components: the caseworker-constant component m^0 contains the average content of a meeting, i.e., standard advice and information, but also the potential disutility associated with the obligation of going to a meeting.The second component is the product between m^0 and an additive, caseworker-specific productivity term, $q_j \sim \mathcal{N}(0, \sigma_q^2)$.[19] 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. As not all meetings are replaced due to transaction costs and capacity constraints, m^1 is lower than m^0. In expectation, replaced meetings have the productivity of the average caseworker present in the office,

[18] 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.

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

$\bar{q}_{-j}$, which is close to zero due to the distribution of q_j.[20] Caseworker absences thus cause two effects: (i) a reduction in the average number of meetings, $m^1 - m^0$, and (ii) a loss of the caseworker-specific term $m^0 q_j$.

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

1.4 Average Effects of Caseworker Absences

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

1.4.1 Empirical Model

Estimation Equation (Intention-to-Treat) As discussed in section 1.2, the main treatment variable of interest, $ABS_{j(i),t}$, contains the number of workdays during which caseworker j is absent over the three-months period t after job seeker i's entry into unemployment. Outcomes y_i include the linear probability to exit unemployment within a given period, the linear duration of unemployment in days and the number of realized caseworker meetings. I estimate the following equation using OLS:[21]

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

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

$$y_i = \alpha + \rho_j + \delta_{1-3} A_{j(i),1-3} + \delta_{4-6} A_{j(i),4-6} + \lambda_\tau + X_i' \beta + \varepsilon_i \tag{1.2}$$

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

The coefficients of interest δ_t measure the effect of caseworker j's absence occurring in time period $t \in (1-3, 4-6)$ after i's entry into unemployment. The focus is on absences in months 1-3 and 4-6 after entry, because job seekers are most intensely followed by their caseworker during these initial periods of the unemployment spell. Later absences will mainly serve as placebo tests. The treatment variable $A_{j(i),t}$ either includes the linear number of days during which caseworker j was absent in period t ($ABS_{j(i),t}$) or a binary variable which equals one if $ABS_{j(i),t}$ contains ten or more days ($\mathbb{1}(ABS_{j(i),t} \geq 10)$).[22] As $ABS_{j(i),t}$ does not take into account whether individual i is still unemployed when the caseworker becomes absent, δ_t defines an intention-to-treat effect (ITT).

ρ_j contains caseworker fixed effects. As discussed in section 1.3, ρ_j measures j's additive productivity during workplace presence. It further controls for all time-constant caseworker characteristics and addresses the threat that the caseworker's productivity while at work coincides with the likelihood of an absence. The empirical model thus compares individuals assigned to the same caseworker during workplace presence versus absence. λ_τ contains fixed effects for the job seeker's calendar month of entry into unemployment. It controls for aggregate time shocks (e.g., health-related) which correlate both with the caseworker's probability of being absent and with the job seeker's labor market conditions. X_i features job seeker characteristics, whose summary statistics are reported in appendix table 1.A.1.

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

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

dynamic selection, the ATT cannot be estimated directly. I therefore instrument the actual treatment (=the caseworker becomes absent in period t of the unemployment spell, and the job seeker is still unemployed at the first day of the absence) by the intended treatment (=the caseworker becomes absent in period t after the job seeker's entry). Equivalently, this Wald estimator divides the ITT effects by the share of individuals who are unemployed at the first day of the absence.[23] In the results section, I show both ITT and ATT results. In addition, I present effects of absences on the number of realized meetings, to interpret the effects on unemployment exit in terms of foregone caseworker meetings.

1.4.2 Identification

Equation 1.2 exploits variation in absences within caseworkers over time. Its identification relies on the assumption that the exact caseworker-specific timing of an absence is as good as random from the job seeker's perspective – conditional on aggregate time effects.[24] In the following, I whether this is an internally valid strategy to study the role of caseworkers.

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

Table 1.4 tests whether a job seeker's exposure to caseworker absences is influenced by her pre-determined characteristics. In panel A, the outcome is the number of days during which the job seeker's caseworker is absent during months 1-3 and 4-6 after entry ($ABS_{j(i),t}$). In panel B, it is the linear probability that $ABS_{j(i),t}$ contains 10 or more workdays. In columns 1 and 4, no caseworkers fixed effects are included in the regression. The reported coefficients suggest that there is some selection of job seekers to frequently absent caseworkers. The selection may be due to spatial correlations between job seeker characteristics and PES-specific absence rates, or due to the endogenous assignment of frequently absent caseworkers to certain job seekers within

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

[24] 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.

PES offices. To test the relevance of these two mechanisms, columns 2 and 5 add PES fixed effects. Absences and observed job seeker characteristics no longer correlate. Within offices, frequently absent caseworkers are thus not systematically assigned to certain types of job seekers. After replacing PES fixed effects by caseworker fixed effects in columns 3 and 6, it remains that absences are unrelated to pre-determined characteristics. This supports the assumption that the caseworker-time specific variation in absences occurs independently of job seeker characteristics.[25]

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

Table 1.4: Pre-Determined Job Seeker Characteristics and Caseworker Absences

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

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

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

Table 1.5: Workload and Caseworker Absences

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

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

In table 1.5, columns 1 and 3 report results from regressions without caseworker fixed effects. They show that absenteeism is unrelated to the overall PES-level inflow, but negatively correlated with the caseworker's share of this inflow. The correlation is most likely mechanical: caseworkers who are overall less present at work are assigned on average a lower share of the inflow. Indeed, the correlation disappears when caseworker fixed effects are included (columns 2 and 4), supporting the assumption that the caseworker- specific timing of an absence is not induced by workload.

Unobserved Caseworker-Time Trends: Placebo Test A remaining threat is that unobserved time-varying factors influence both caseworker absences and job seeker outcomes. To test for the existence of unobserved caseworker-time trends, future absences can serve as placebo variables in regressions on current outcomes. For instance, caseworker absences occurring in months 4-6, 7-9 and 10-12 after unemployment entry are placebo absences when regressing on an outcome which realizes within the first three months of unemployment.[26] Placebo tests will be shown in the next section, jointly with the main results.

1.4.3 Results

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

Unemployment Exit To start, I analyze whether caseworker absence days affects the exit from unemployment in a linear way. To this end, figure 1.5 plots effects of the number of absence days occurring in months 1-3 of unemployment. Instead of the linear number of days $ABS_{j(i),t}$, the treatment variable used to estimate equation 1.2 contains five-days categories (e.g., 1-5 absence days, 6-10 absence days etc.). Outcomes are the probability to exit unemployment within six months (panel a) and the duration of unemployment in days (panel b).[27] For both outcomes, it appears clearly that absences influence the exit from unemployment if they accumulate to at least 10 days.[28] It is also visible that a linear specification of absence days would not match the observed effect pattern. In the following analysis, I thus use a binary

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

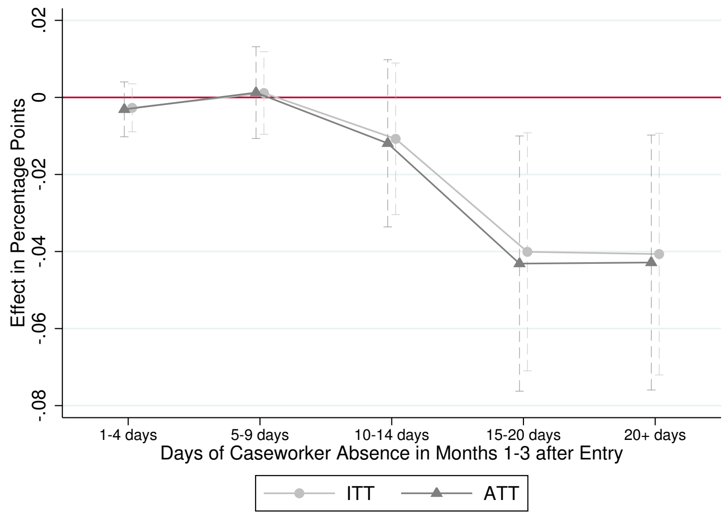
[27] I cap unemployment spells at 520 days because this is the maximum potential UI benefit duration in the estimation sample. 12.3% of observations are affected by the cap, as job seekers are not automatically de-registered after benefit exhaustion.

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

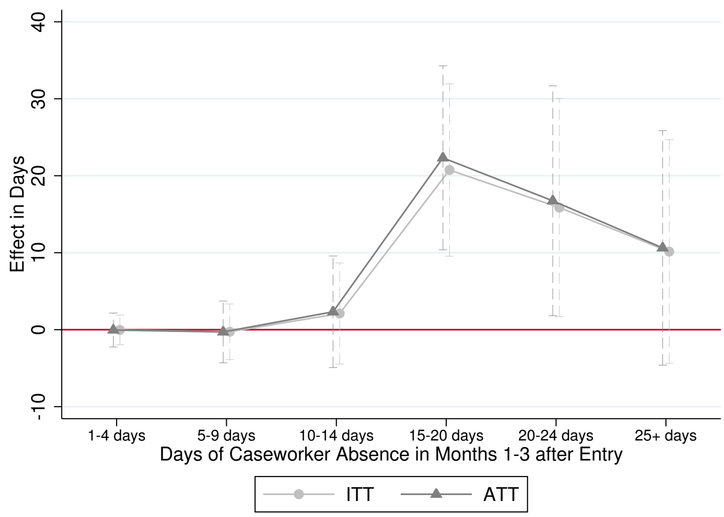
treatment variable, which equals one if the job seeker's caseworker is absent during at least ten workdays over the three month window t after entry. Appendix table 1.A.2 additionally presents coefficients of the linear variable $ABS_{j(i),t}$.

Figure 1.5: Effect of Caseworker Absences in Months 1-3 on the Exit from UE

(a) P(Exit within 6 Months)



(b) Unemployment Duration in Days

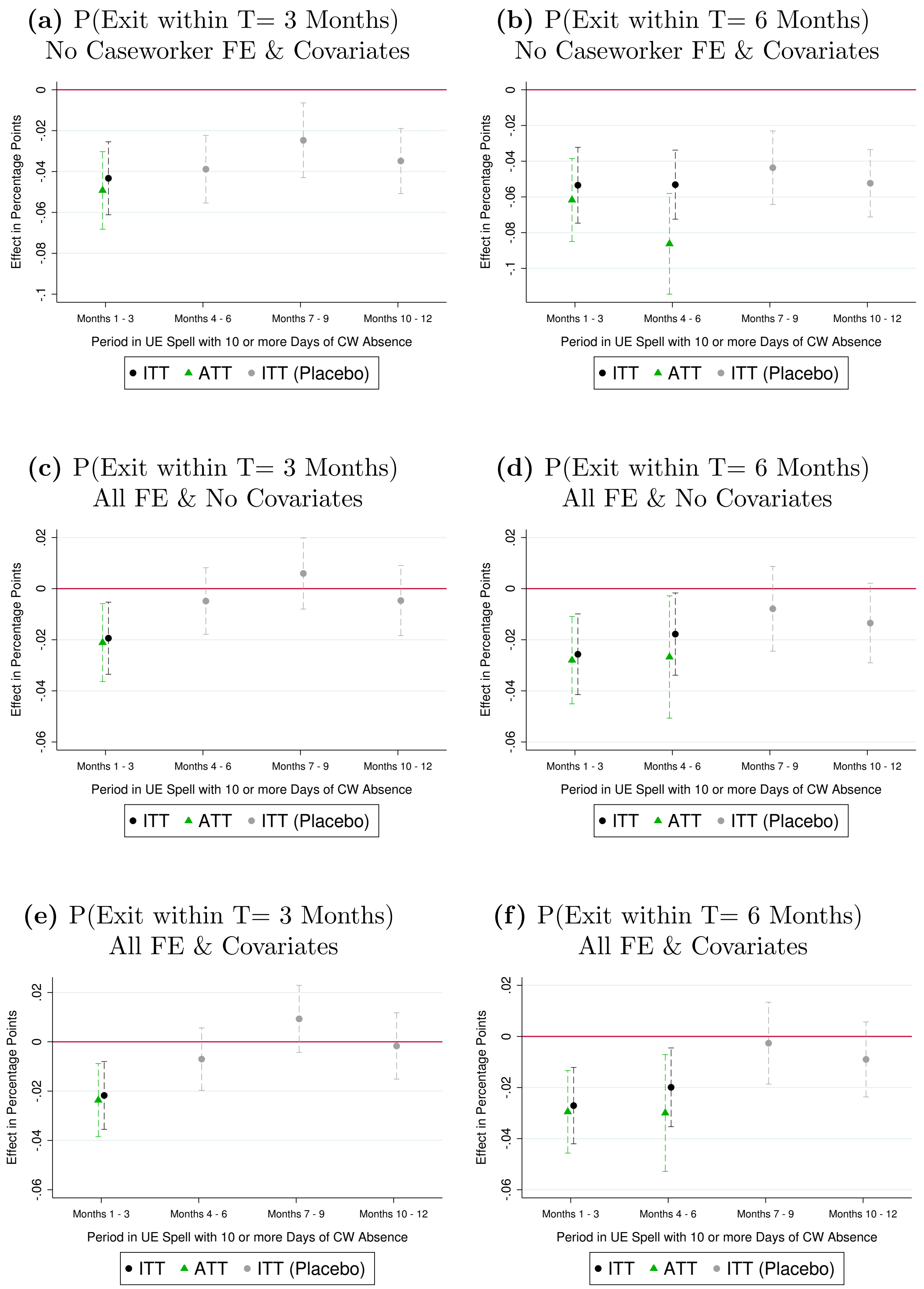


The figure plots the estimated effects of caseworker absences occurring within the first three months after entry. The x-axis denotes the cumulative number of days (in five day categories) during which the caseworker was absent in months 1-3 after the job seeker's entry into unemployment. The reference group contains job seekers with no caseworker absence during this period. The y-axis denotes the size of the estimated coefficient. In panel (a), the outcome is the linear probability of exiting unemployment within six months. In panel (b), the outcome is the linear duration of unemployment in days (capped at 520 days for 12.3% of the sample). Regressions include fixed effects for the job seeker's calendar month of entry into unemployment, caseworker fixed effects and covariates (c.f. equation 1.2). ATT estimates scale ITT estimates by the share of job seekers still unemployed at the first day of absence (0.92). Dashed lines represent 90% confidence intervals. Further estimation details can be found in section 1.4. N=382123.

Figure 1.7 reports the short and medium run effect of being exposed to a caseworker absence which accumulates to ten or more days over a three-months period. In particular, the figure shows how the main effects and placebo estimates (effects of future absences on current outcomes) react to the introduction of caseworker fixed effects and individual covariates. Outcomes are the probability to exit from unemployment within $T = 3$ and $T = 6$ months. The x-axis denotes the three-months periods in which the absences occurred; the y-axis shows the size of the estimated coefficient. As described in section 1.4.2, ATT effects scale ITT effects by the share of job seekers still unemployed at the first day of a caseworker absence. This share is 0.92 for t=1-3 and 0.67 for t=4-6.

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

Figure 1.7: Effects of Absences on the Probability to Exit UE w/in T Months



The figure illustrates the estimated effects of absences, based on equation 1.2. The regressions also estimate placebo effects of absences occurring beyond the outcome window. The treatment variable equals one if the job seeker's caseworker was absent during 10 or more days in the three-months period t after unemployment entry. In panels a and b, fixed effects and covariates are excluded. Panels c and d add fixed effects and panels e and f add covariates. Effects shown in e and f are also reported in columns 1 and 2 of table 1.6. The x-axis denotes the three-months period in which the caseworker absence occurred. The y-axis denotes the size of the estimated coefficient. ATT estimates scale ITT estimates by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). Dashed lines represent 90% confidence intervals. N=382123.

Table 1.6 reports the corresponding effects of the preferred specification with fixed effects and covariates. Panel A presents the ITT effects and Panel B the scaled ATT effects. Panel B of column 1 shows that an absence of ten or more days in months 1-3 decreases the probability to exit within three months by 2.4 percentage points, corresponding to a decrease of 8% relative to the mean. The probability to exit within six months decreases by 2.8 percentage points in response to both absences in months 1-3 and absences in months 4-6 (5% relative to the mean, panel B of column 2). Column 3 reports no significant effect of early absences on the probability to exit within 12 months, suggesting that job seekers with early caseworker absences catch up later in the spell. As shown in column 4, absences in months 1-3 increase the duration of unemployment by 10 days (5% relative to the mean). Absences of months 4-6 show no significant effect on the duration of unemployment, suggesting that the support by a caseworker matters mostly at early stages of the unemployment spell. This intuition is in line with evidence showing that early policy interventions in UI can have large effects. For instance, Black et al. (2003) find that early announcements of re-employment services exert a substantial threat effect on newly unemployed individuals. Similarly, Bolhaar, Ketel and Van der Klaauw (2016) show that steering the initial search effort of Dutch welfare recipients increases early exits from unemployment.

Appendix table 1.A.3 documents that the main estimates are invariant to several modifications of sampling choices and variable specifications.

Table 1.6: The Effect of Absences on UE Exit within T Months

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

$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1 - 3$, 0.67 for $t = 4 - 6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

As an additional analysis, I test in table 1.7 whether the effects differ by observed job seeker characteristics. The table directly presents ATT estimates of absences during months 1-3 of unemployment. It shows a low degree of heterogeneity in the effects. Very early reactions (effects on exits within three months, columns 1 to 4) are significantly more pronounced among women and individuals with a previous income below the median. This is in line with the literature on active labor market programs, which typically finds stronger reactions for women and individuals of low income potential (Card, Kluve and Weber, 2010, 2015). However, columns 5 to 8 show that the heterogeneity fades out over the six months outcome window. It appears that in the medium run, all types of job seekers benefit from the interaction with a caseworker.

Table 1.7: The Effect of Absences on UE Exit within T Months, Heterogeneity by Job Seeker Characteristics

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

$ABS_{j(i),1-3} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. T denotes the outcome period in months. Regressions report instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. ITT effects are available upon request. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Low Education" equals one if the job seeker completed no more than the obligatory level of schooling. "Low Previous Earnings" equals one if the job seeker's pre-unemployment earnings were lower than the median. ${}^{*}p < 0.10, {}^{**}p < 0.05, {}^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

Meeting Quantity To interpret presented effects in terms of foregone caseworker interactions, table 1.8 reports effects on the number of meetings realized over the first $T = 3/T = 6$ months of unemployment.[29] The number of meetings is normalized by the time spent in unemployment over period T.[30] Figure 1.9 illustrates the effects and additionally presents placebo estimates. To relate to the presented effects on unemployment exit, I focus directly on absences which sum to 10 days or more over three months. Linear effects are shown in appendix table 1.A.4.

Columns 1 and 2 of table 1.8 report estimated effects on the number of meetings realized between the job seeker and her *assigned* caseworker. As expected, this outcome decreases in response to caseworker absences. Absences of at least ten days during months 1-3 of unemployment induce on average a decrease of 0.62 meetings over the first three months and of 1.12 meetings over the first six months (ATTs). For both outcome windows, this corresponds to a decrease of about 40% relative to the mean.[31] Absences in months 4-6 induce a drop of 0.56 meetings over the first six months (25 % relative to the mean). Given that the previously presented effects on the duration of unemployment were driven by absences in months 1-3, I conclude that job seekers who loose one caseworker meeting (or 40% of the average meeting quantity) over the first six months of unemployment stay unemployed 10 days longer.[32] I interpret this estimate as a local effect, as it is likely that caseworker meetings have decreasing marginal returns.

It is a priori unclear whether job seekers affected by absences have *overall* less caseworker meetings. If there was perfect replacement, the total number of meetings should hardly react to absences. Therefore, columns 3 to 6 decompose the effect into

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

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

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

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

an increased number of replaced meetings and a reduced total number of meetings. Results show that about half of the reduction in meetings caused by absences in months 1-3 is compensated by replacements (columns 3 and 4).[33] The other half translates into a reduction in the total number of meetings (columns 5 and 6). Column 4 shows that only one third of meetings foregone due to absences in months 4-6 are replaced. This suggests that meetings later in the spell are less prioritized by the PES and therefore less likely to be replaced. Altogether, the existence of replacement suggests that the estimated cost of loosing a meeting with the assigned caseworker is a lower bound to its costs in contexts without replacement mechanisms.

Figure 1.9 reports that placebo estimates on the effects of future absences on current meetings are equal or close to zero, suggesting the absence of any major confounding trend.[34]

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

[34] Placebo effects on the number replaced meetings over 6 months are marginally significant and negative (Figure 1.10d). These effects are however economically very small and work into the opposite direction as the main effects, which contain an increase in the number of replaced meetings.

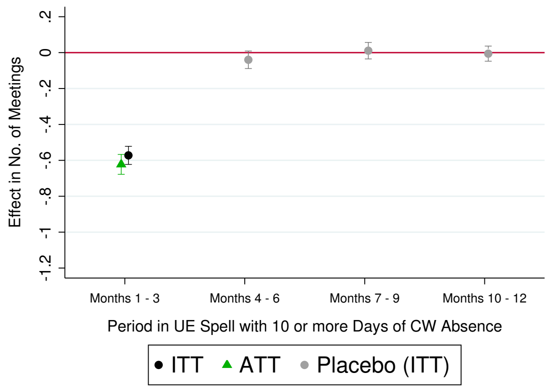
Table 1.8: The Effect of Absences on the No. of Meetings Realized over T Months

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

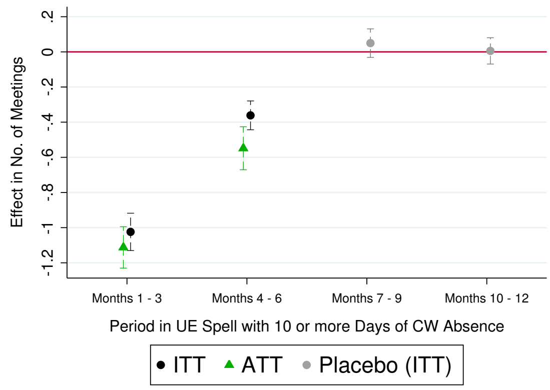
$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 1.2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

Figure 1.9: Effects of Caseworker Absences on the No. of Meetings Realized Within T Months

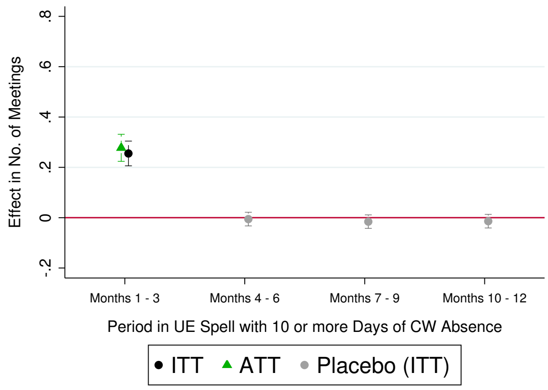
(a) Meetings with Assigned CW, T=3 (Column 1)



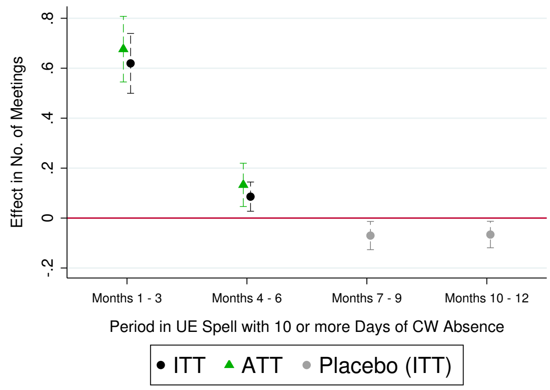
(b) Meetings with Assigned CW, T=6 (Column 2)



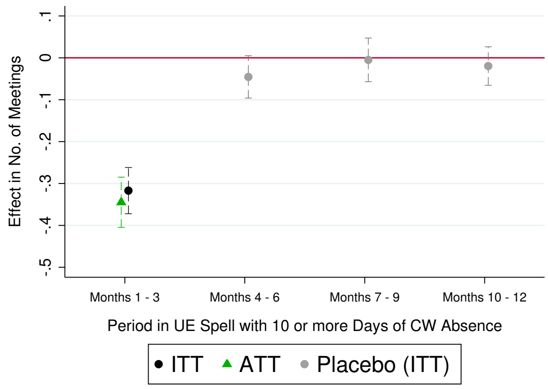
(c) Meetings with Replacement, T=3 (Column 5)



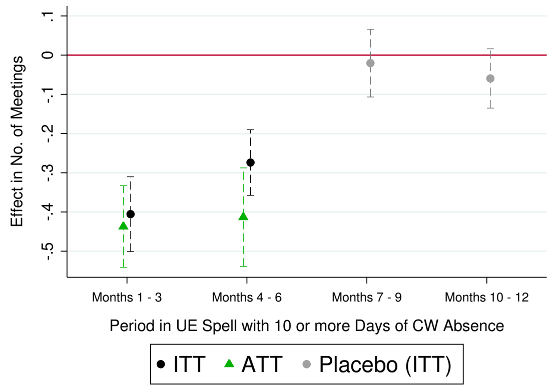
(d) Meetings with Replacement, T=6 (Column 6)



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



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



The figure illustrates the estimates reported in table 1.8. It further reports estimated placebo effects of absences occurring beyond the outcome window. The treatment variable equals one if the job seeker's caseworker was absent during 10 or more days in the three-months period t after unemployment entry. The x-axis denotes the three-months period in which the caseworker absence occurred. The y-axis denotes the size of the estimated coefficient. ATT estimates scale ITT estimates by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). Dashed lines represent 90% confidence intervals. Further estimation details are included in the notes of table 1.8. N=382123.

Spillover Effects on the Performance of Present Caseworkers A caseworker's absence may also affect the performance of present colleagues in the office. The direction of spillover effects from caseworker absences is ex ante ambiguous: on the one hand, job seekers with an absent caseworker search less and decrease the competition for available vacancies in the local labor market (Crépon et al., 2013; Lalive, Landais and Zweimueller, 2015).[35] This is expected to cause positive spillover effects on the exit probability of job seekers with present caseworkers. On the other hand, absent caseworkers temporarily increase the workload of their colleagues, who have to jump in as replacements. Therefore, all job seekers potentially receive less attention from their caseworker. From this second mechanism, I expect negative spillover effects.

Table 1.9 presents spillover effects of caseworker absences. For each job seeker i, I measure as $\overline{ABS}_{oq-j,1-3}$ the average of $ABS_{j(i),1-3}$ among individuals who enter the same PES office o in the same calendar quarter q as i, and who are not assigned to the same caseworker as i (leave-out mean). $\overline{ABS}_{oq-j,1-3}$ is introduced into equation 1.2. As the equation contains caseworker and calendar month fixed effects, the identifying assumption is that the exact time-office specific variation in the absence rate is quasi-random from the individual job seeker's perspective.

To start, columns 1 to 3 assess whether there are spillover effects of caseworker absences on the number of meetings realized over the first six months of unemployment. Such effects occur if present caseworkers have to replace their absent colleagues and can therefore meet their own cases less frequently.[36] Indeed, column 1 shows that when the average exposure to caseworker absences increases by 1 day, meetings with present colleagues decrease by 0.06 days. This effect is as large as the linear effect of one absence day of the own caseworker. In turn, the reduction in meetings arising from spillover effects is -as expected- not replaced by a different caseworker (column 2). The effect therefore translates directly into a reduction in the total number of meetings (column 3). Columns 4 further shows that spillover effects increase the duration of unemployment. The ratio between the direct effect of $ABS_{j(i),1-3}$ and the spillover effect equals their ratio when the outcomes is the total number of meetings

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

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

Table 1.9: Spillover Effects of Absences

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

$ABS_{j(i),1-3}$ contains the number of days job seeker's caseworker is absent during period t after the job seeker's entry into unemployment. $\overline{ABS}_{oq-j,1-3}$ contains the office-quarter specific average of $ABS_{j(i),1-3}$, based on job seekers who are not assigned to the same caseworker as i (leave-out mean). The mean of $\overline{ABS}_{oq-j,1-3}$ is 0.42. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Regressions estimate equation 1.2, adding the spillover variable. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). In column 6, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

(column 3).

Table 1.9 further suggests that the spillovers do not bias the estimated effect of $ABS_{j(i),1-3}$, which does not differ from the estimates reported in table 1.A.2. However, the empirical setting does not allow for conclusions on job search externalities that may arise when changing the number of job seeker-caseworker interactions on a larger scale.

1.5 Effects by Caseworker Productivity

Having established the average effect of caseworker absences, I now analyze its heterogeneity with respect to caseworker productivity. As discussed in section 1.3, absences can cause both a reduction in the quantity of caseworker meetings and a change in the quality of realized meetings. Provided that all job seekers receive on average the same type of replacement, the quality effect depends on the absent caseworker's productivity at work. Therefore, heterogeneity in the effects by the absent caseworker's productivity can reveal the relative importance caseworker quality for the effectiveness of counseling. In the following, I first set up the empirical model and then present results for different outcomes.

1.5.1 Empirical Model

Additive Caseworker Productivity I estimate additive caseworker productivity by means of fixed effects, in the spirit of the commonly used method to measure teacher or manager value added (e.g., Chetty, Friedman and Rockoff, 2014*b*,*a*; Bertrand and Schoar, 2002; Lazear, Shaw and Stanton, 2015). I consider medium-run unemployment exit as the relevant output,[37] and hold other input factors, such as working conditions, the local employment office's resources or the job seeker's characteristics, constant. To this end, I set up the following expression for s_i, job seeker i's linear probability to exit unemployment within six months:

$$s_i = \alpha_c + X_i' \beta_c + \kappa_{o \times q} + \theta_j + \varepsilon_i \tag{1.3}$$

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

The estimation is stratified at the level of canton c. Therefore, α_c includes a canton-specific constant term, and β_c measures canton-specific returns to individual covariates X_i.[38] $\kappa_{o\times q}$ contains interacted PES office × calendar quarter fixed effects.[39] This ensures that caseworkers face the same workplace conditions, office policies and local labor market conditions. θ_j measures the parameter of interest, caseworker j's additive effect on the probability to exit within six months. As I am interested in the interaction between productivity at work and the effect of absences, I want to avoid that job seekers treated by an absence contribute to the estimated productivity θ_j (c.f. the intuition of a leave-out mean). Therefore, the regression is run without job seekers who are affected by at least ten days of caseworker absence in months 1-3 or 4-6 after entry (i.e., for whom $ABS_{j(i),1-3} \geq 10$ or $ABS_{j(i),4-6} \geq 10$).

The criteria for assigning job seekers to caseworkers are mostly regulated by the cantonal authorities. Running regressions at the cantonal level therefore has the advantage of controlling more flexibly for potential influences of observed job seeker characteristics in the assignment process. As discussed in section 2.1, the assignment is often based on caseworker availability. However, observed criteria included in X_i, such as the job seeker's occupation or education level, may also influence the assignment. Appendix figure 1.A.1 shows the densities of estimated caseworker effects $\hat{\theta}_j$ from regressions with and without covariates X_i. As the shape and variance of the two distributions hardly differ, job seeker characteristics appear to have a minor influence on the productivity measure.Also recall from section 1.4.2 (Table 1.4) that within PES offices, frequently absent caseworkers are not systematically assigned to certain types of job seekers. This further supports the intuition that there are no sophisticated assignment rules which map job seeker characteristics to caseworkers.

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

$$y_i = \alpha + \rho_j + \sum_{k=1}^{3} \gamma_k (A_{j(i),1-3} \times Tk_j^{\hat{\theta}}) + \pi_k + \lambda_\tau + X_i'\beta + \varepsilon_i \tag{1.4}$$

[38] X_i includes the same covariates as in equation 1.2. Summary statistics are reported in Table 1.A.1.

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

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

The heterogeneity analysis focuses on absence days occurring in months 1-3 of the spell ($A_{j(i),1-3}$). Given that more productive caseworkers cause faster exits, the composition of job seekers who are unemployed at the time of the absence risks to vary with $\hat{\theta}_j$. For instance, productive caseworker may in the medium run remain with less employable job seekers. In the first three months after entry, 92% of job seekers are still unemployed at the first day of an absence.[40] Therefore, compositional changes are a minor issue for absences occurring in months 1-3.

As in the previous analysis, $A_{j(i),1-3}$ either contains the linear number of absence days, $ABS_{j(i),1-3}$, or a dummy variable which equals one if $ABS_{j(i),1-3}$ contains ten or more days. $A_{j(i),1-3}$ is interacted with $j's$ tercile $Tk_j^{\hat{\theta}}$ in the office-specific distribution of estimated caseworker productivity $\hat{\theta}_j$.[41]

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

1.5.2 Results

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

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

[40] 93% for caseworkers in T1 and T2, 90% for caseworkers in T3.

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

same pattern in terms of heterogeneity.

Table 1.10 how the effect of absences on the exit from unemployment interacts with the absent caseworker's productivity tercile. It documents a largely heterogeneous economic value of caseworker presence. Columns 1 and 2 show effects on the probability to exit within six months, estimated without and with covariates. Caseworkers in the second tercile are on average as productive as the replacement – provided that replacements are in expectation performed by the average caseworker in the office. Their absence should thus mostly induce a reduction in the quantity of meetings, due to imperfect replacement. Results show that absences of caseworkers ranking in the second tercile decrease the probability to exit within six months by 2.6 percentage points (column 2). This estimate is close to the average effect reported in the previous section, and corresponds to a change of 5% relative to the group-specific mean. Absences of caseworkers in the lowest productivity tercile show no effect on the exit from unemployment. For job seekers assigned to one of these caseworkers, the absence-induced loss in meeting quantity appears to be offset by a gain in productivity due to the replacement by a better caseworker. On the contrary, absences of caseworkers in the third tercile induce a negative quality effect, as the replacement is less productive. This expresses in a significantly more negative effect on unemployment exit. For instance, the probability to exit within six months decreases by 6.4 percentage points if the absent caseworker ranks in the third tercile (9% relative to the group-specific mean). Results further show that early absences of highly productive caseworkers have a persistent effect: twelve months after entry, the probability to exit is still lowered by 3.2 percentage points (column 3). In terms of the overall unemployment duration, absences induce an increase by 19 days for caseworkers ranking in the third tercile and by 12.5 days for caseworkers ranking in the second tercile (column 4).[42] Absences of caseworkers in the lowest tercile have no effect on the duration. Appendix table 1.A.6 further decomposes the heterogeneity by quintiles. It shows that the positive productivity effect of absences is mostly driven by caseworkers in the lowest quintile. The negative productivity effect is equally driven by caseworkers in the upper two quintiles.

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

Table 1.10: The Effect of Absences on UE Exit within T Months, by Tercile of Caseworker Productivity

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

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

Exit Destinations From a policy perspective, it is of interest to decompose the presented effects into different exit destinations: do productive caseworkers foster the job seeker's own search effort, or do they rely on vacancy referrals? Do they generate sustainable job matches? To answer these questions, table 1.11 presents heterogeneous effects of caseworker absences on the probability to exit towards different destinations within six months.

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

Effects on job stability are reported in columns 3 and 4, where the outcome is split into stable and unstable job finding. A job match is coded as stable if the job seeker stays out of formal unemployment for at least 12 months after exit.[43] Absences of caseworkers in the third tercile show the most negative effect on the propensity to find a stable job match. In turn, exits to unstable jobs react equally to absences of caseworkers in the second and third terciles. This implies that not only the absolute number, but also the share of stable job matches is higher among the most productive caseworkers. This is remarkable, as treatments commonly used in UI - in particular job search monitoring and sanctions- have been shown to lower post-unemployment job stability (e.g., Petrongolo, 2009; Arni, Lalive and Van Ours, 2013). Finally, column 5 shows that exits to non-employment do not react to caseworker absences.

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

Table 1.11: The Effect of Absences on UE Exit within 6 Months to Different Destinations, by Tercile of Caseworker Productivity

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

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). The exit destination is obtained from a variable specifying the job seeker's reason of de-registering from unemployment. "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

Quantity of Meetings and Productivity of Replacement The presented heterogeneities can only be interpreted as pure quality effects if absences of caseworkers in the three productivity terciles equally reduce the meeting quantity.[44] Further, the productivity of the replacement needs to be constant with respect to the absent caseworker's productivity.

Columns 1 to 2 of table 1.12 shows that in all three productivity terciles, absences lead to a loss of roughly one meeting with the assigned caseworker (ATTs in panel B).[45] It is therefore granted to conclude that the effect of one caseworker meeting on the exit from unemployment strongly depends on caseworker quality.

Columns 3 to 4 show that about half of caseworker meetings are replaced in all productivity terciles. Further, columns 5 to 6 report results from regressions in which the difference between a job seeker's replacement productivity and the average PES productivity is regressed on dummies for the absent caseworker's productivity tercile, conditional on PES fixed effects.[46] This regression only includes job seekers who are affected by an absence and who receive at least one replaced meeting during the outcome period. Therefore, the second tercile is the omitted baseline category. Results show clearly that there is no difference in replacement productivity between the three terciles. This holds for both outcome periods (three and six months after entry).

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

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

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

Table 1.12: The Effect of Absences on the No. of Meetings Realized over T Months, by Tercile of Caseworker Productivity

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

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 1.4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month fixed effects and job seeker covariates (summary statistics reported in table 1.A.1). In columns 1 to 4, regressions include fixed effects for the job seeker's calendar month of entry into unemployment as well as caseworker fixed effects. Columns 5 to 6 only contain job seekers with $ABS_{j(i),1-3} \geq 10$ who received at least one replaced meeting during the outcome period. The outcome is the difference between the replacement productivity and the average caseworker productivity in the office. In these two columns, regressions include fixed effects for the job seeker's calendar month of entry into unemployment as well as PES office fixed effects. "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

The Role of Treatment Assignments The strong heterogeneity in the effect of caseworker absences raises questions about the underlying channels. Interactions between caseworkers and job seekers have mostly two components: first, they contain the unobserved counseling process, in which caseworkers motivate job seekers, generate pressure to actively search for work, provide information and give guidance. Second, caseworker meetings can result in observed outcomes, such as the assignment of active labor market programs, the referral of vacancies and the imposition of benefit sanctions due to job search monitoring. In the following, I analyze how the number of treatments assigned over the first $T = 3/T = 6$ months of unemployment reacts to absences of caseworkers in the three productivity terciles.[47] This can shed light on the importance of observed treatments versus unobserved counseling techniques in the caseworker production function. As in the previous analyses, estimations are based on equation 1.4 and the binary treatment variable equals one if the caseworker is absent during ten or more workdays in months 1-3 after unemployment entry.

Columns 1 and 2 of table 1.13 present estimated effects on the number of assigned training programs. These programs mostly include job search trainings or skill classes, such as computer or language courses. While there is a jointly significant negative effect of absences on this outcome, the effect does clearly not differ systematically by caseworker productivity. Columns 3 to 6 show that there is a joint negative effect of absences on the referral of vacancies and on the incidence of benefit sanctions. While it appears from eyeballing that the effects are stronger for absences of caseworkers in the third tercile (columns 3 and 5), the difference between the interaction terms is insignificant or at the margin to significance.[48] Finally, appendix table 1.A.7 shows for the subsample of individuals with an observed job search requirement that the requirement level is unrelated to caseworker absences for all terciles of the productivity distribution.

These findings do obviously not exclude that the treatments chosen by a caseworker can have substantial effects on the exit from unemployment.[49] They, however imply that there must be a significant contribution of unobserved factors to the success of caseworkers. The most likely interpretation is that unobserved counseling quali-

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

[48] Further note that only 3.9% of all job seekers find a job through a vacancy referral within the first six months of unemployment (c.f. table 1.11, column 2.

[49] Indeed, chapter 2 of this dissertation shows that the job search requirement assigned by caseworkers can have large effects.

ties have a large influence, which renders the replacement of productive caseworkers difficult. This result is in line with Huber, Lechner and Mellace (2017), who use mediation analysis to show that the success of tough caseworkers cannot be explained by program assignments. My findings further suggest that "being tough", as proxied by the use of sanctions, is not the central determinant of caseworker performance. From a policy perspective, this implies that the hiring of high quality caseworkers or investments into the counseling style of existing caseworkers may have large payoffs.

Table 1.13: The Effect of Absences on Treatments Assigned over T Months, by Tercile of Caseworker Productivity

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

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. In columns 1 and 2, the outcome is the number of trainings (e.g., job application training, computer class, language course) assigned over T months. In columns 3 and 4, the outcome is the number of vacancies referred over T months. In columns 5 and 6, the outcome is the number of benefit sanctions (e.g., due to insufficient search effort) imposed over T months. All outcomes are normalized by the time spent in unemployment over T. To this end, the number of assignments realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 1.4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the three interaction terms. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

The Role of Caseworker Tenure In a final analysis, I assess the role of task-specific human capital for the performance of caseworkers. Provided that tenure measures the specificity of human capital (c.f. Becker, 1962), the negative effect of absences is expected to increase with tenure if caseworker human capital is task-specific. The UI registers do not provide information on caseworker characteristics. I observe, however, the full population of caseworker-job seeker matches since January 2008 and can therefore construct proxies of tenure and experience.[50]

Table 1.14 reports how the effects of early caseworker absences on unemployment exit interact with tenure.[51] For each caseworker $\times$ calendar month cell, I measure tenure as the number of prior months since 2008 during which the caseworker was assigned to job seekers (columns 1 and 3). As an alternative measure, I count for each caseworker $\times$ calendar month cell the number of prior cases assigned since 2008 (columns 2 and 4). I then perform median splits within each month cell. The same caseworker can thus have different levels of relative tenure and experience in different calendar months.

Results show that absences of more tenured and experienced caseworkers cause stronger reductions in unemployment exit (statistically significant difference in columns 1 and 4). It thus appears that caseworkers hold largely task-specific human capital, implying that more tenured caseworkers are harder to replace in the case of an absence. In line with this intuition, Lazear and Shaw (2008) show that worker productivity strongly increases with tenure. Further, Jäger (2016) finds that if is difficult for firms to replace a long-tenured worker after an exogenously caused job separation.[52]

[50] I do not use the years 2008-2009 for the main analysis because the meeting data is incomplete prior to 2010.
[51] The table directly reports ATT effects. ITT effects are available upon request.
[52] In addition, a large literature documents that job seniority increases wages and makes job separations more costly for workers (e.g., Topel, 1991).

Table 1.14: The Effect of Absences on UE Exit within T Months, by Caseworker Tenure

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

$ABS_{j(i),1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. For each caseworker $\times$ calendar month cell, I measure tenure as the number of prior months during which the caseworker was matched to at least one job seeker since 2008 (columns 1 and 3). As an alternative measure, I count for each caseworker $\times$ calendar month cell the cumulative number of prior cases assigned since 2008 (columns 2 and 4). I then perform median splits within each month cell. The same caseworker can thus have different levels of relative tenure and experience in different calendar months. T denotes the outcome period in months. Regressions report instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. ITT effects are available upon request. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the two interaction terms. In columns 3 and 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

1.6 Conclusion

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

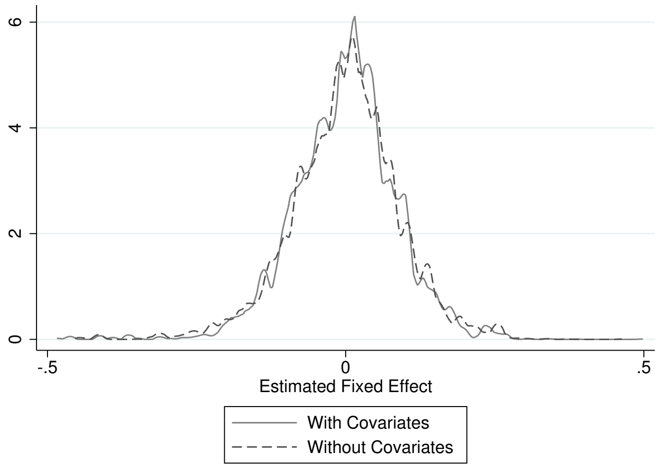
As an additional core result, the economic value of caseworkers turns out to be largely heterogeneous. Absences of caseworkers in the lowest productivity tercile show no effect. In turn, the average return of a caseworker meeting would double if all caseworkers had on average the productivity of caseworkers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

The results suggest that investments into the human resources of welfare systems can have high economic payoffs. On the *quantity* side, caseload reductions can increase the time spent on each unemployed individual. The spillover analysis showed that individuals stay unemployed longer if their caseworker has to replace absent colleagues, confirming the economic relevance of caseloads. Investments into caseworker *quality* could target the counseling skills of existing caseworkers (e.g., through training) or the selection of individuals attracted by the caseworker profession (e.g., through higher salaries[53]) Further, reducing the number of job separations among caseworkers may help increasing the amount of task-specific human capital. Lazear and Oyer (2013) review existing evidence on the determinants of productivity in firms, as offered by research in personnel economics. Future research is needed to understand which interventions and personnel policies work to increase caseworker performance in welfare systems.

[53]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, Finan and Rossi (2013) show that changes in posted salaries change the composition of applicants for public service jobs. Ashraf, Bandiera and Scott (2016) find that agents attracted to the public service by career concerns have more skills and ambitions than those attracted by purely altruistic motives.

Appendix 1.A Additional Figures and Tables

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



N=382123. The distribution is weighted by the number of job seekers per caseworker. Predictions are based on regression of caseworker fixed effects and interacted PES-calendar quarter effects on the job seeker's probability of exiting unemployment within six months (equation 1.3). The dashed line reports the density of estimated caseworker fixed effects predicted from a regression without job seeker covariates. The solid line reports the density of fixed effects predicted from a regression with covariates. Regressions is preformed at the cantonal level on job seekers who are unaffected by a caseworker absence of 10 or more days in the first or second three-month period of unemployment.

Table 1.A.1: Summary Statistics on Job Seeker Covariates

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

Table 1.A.2: The Linear Effect of Absences on UE Exit within T Months

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

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

Table 1.A.3: The Effect of Absences on UE Exit within 6 Months: Robustness of ITT Effects to Sample Modifications

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

$ABS_{j(i),t} \geq 10$ equals one if job seeker i's caseworker j is absent during at least ten workdays in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). Further estimation details can be found in section 1.4. In column 2, caseworkers who have less than 60 cases over the sample period are excluded (in the baseline specification, I exclude caseworkers with less than 30 cases). In column 3, all caseworkers are included. In column 4, job seekers affected by absences of 30 or more days during months 1-3 or 4-6 are excluded. In column 6, initially made caseworker assignments are not updated (in the baseline specification, I use updated assignments if the update occurs up to week 1 after the job seeker's entry). In column 6, updated assignments are used if the update occurs up to week 2 after the job seeker's entry. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269).

Table 1.A.4: The Linear Effect of Absences on the No. of Meetings Realized over T Months

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

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. T denotes the outcome period in months. The number of meetings is normalized by the duration of unemployment. To this end, the number of meetings realized during period T is multiplied by the share of days a job seeker was unemployed during T. Panel A reports ITT estimates based on equation 1.2. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.92 for $t = 1-3$, 0.67 for $t = 4-6$). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.4.

Table 1.A.5: The Linear Effect of Absences on UE Exit within T Months, by Caseworker Productivity

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

$ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.4. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

Table 1.A.6: The Effect of Absences on UE Exit within T Months, by Quintile of Caseworker Productivity

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

Regressions estimate equation 1.4 using OLS, $ABS_{j,1-3} \geq 10$ equals one if the job seeker's caseworker is absent during at least ten workdays in months 1-3 after the job seeker's entry into unemployment. Qk equals one if the caseworker ranks in the k^{th} quintile of the productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.4, replacing terciles by quintiles. Panel B reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence. The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the three interaction terms. In column 4, the unemployment duration is capped if it lasts longer than 520 days (12.3% of the sample). $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=2269). Further estimation details can be found in section 1.5.

Table 1.A.7: The Effect of Absences on Modal Search Requirement Assigned over T Months, by Tercile of Caseworker Productivity

	Modal Requirement in Months 1-3	Modal Requirement M in Months 1-6
Panel B: ATTs		
$ABS_{j(i),1-3} \geq 10$ interacted w/:		
$T1(\gamma_1)$	0.061	-0.038
	(0.146)	(0.144)
$T2(\gamma_2)$	-0.003	0.042
	(0.166)	(0.151)
$T3(\gamma_3)$	0.149	0.138
	(0.186)	(0.177)
Tests (ATTs):		
$\gamma_1 = \gamma_2$ (p-value)	0.772	0.704
$\gamma_2 = \gamma_3$ (p-value)	0.539	0.679
$\gamma_1 = \gamma_3$ (p-value)	0.710	0.442
Joint Sign. (p-value)	0.843	0.861
Outcome Mean	8.633	8.593
Covariates	Yes	Yes
N	56714	58333

Search requirements define the number of job applications to be submitted on a monthly basis. The modal requirement over T denotes the requirement which the job seeker faces most often in the first T months of unemployment. The number of observation differs from the baseline sample because requirements are only observed for a subsample. $ABS_{j(i),t}$ contains the workdays of caseworker absence in period t after the job seeker's entry into unemployment. Tk equals one if the caseworker ranks in the k^{th} tercile of the office-specific productivity distribution. T denotes the outcome period in months. Panel A reports ITT estimates based on equation 1.4. The table reports instrumented ATT estimates, where the ITTs are scaled by the share of job seekers who are still unemployed at the first day of absence (0.93 for caseworkers in T1 and T2, 0.90 for caseworkers in T3). The unit of observation is the job seeker. All regressions include calendar month and caseworker fixed effects, as well as job seeker covariates (summary statistics reported in table 1.A.1). "Joint Sign." = test for joint significance of the three interaction terms. ${}^{*}p < 0.10, {}^{**}p < 0.05, {}^{***}p < 0.01$. Standard errors are clustered at the caseworker level (here N=472). Further estimation details can be found in section 1.5.

CHAPTER 2

Job Search Requirements, Effort Provision and Labor Market Outcomes*

How effective are effort targets? This chapter provides novel evidence on the effects of job search requirements on effort provision and labor market outcomes. Based on large-scale register data, we estimate the returns to required job search effort, instrumenting individual requirements with caseworker stringency. Identification is ensured by the conditional random assignment of job seekers to caseworkers. We find that the duration of un- and non-employment both decrease by 3% if the requirement increases by one monthly application. As a consequence of imperfect compliance with the requirement, an additionally provided monthly application decreases the length of spells by 4%. In line with theory, we find that the effect of required effort decreases in the individual's voluntary effort. Finally, the requirement level causes small negative effects on job stability, reducing the duration of re-employment spells by 0.3% per required application. We find a zero effect on re-employment wages.

*This chapter is based on joint work with Patrick Arni. The corresponding paper has been re-submitted to the *Journal of Public Economics*.

2.1 Introduction

Targets on effort provision are used to improve productivity and counteract moral hazard in many contexts of the labor market. Commonly known examples include performance targets in firms,[1] as well as the enforcement of minimum job search effort in unemployment insurance (UI) and welfare systems. For the successful design of these targets, it is key to understand how they translate into effort provision and into the final economic outcome. Most of the existing empirical evidence comes, however, from small-scale laboratory or field experiments with limited external validity (e.g., Abeler et al., 2011; Fehr and Goette, 2007; Hennig-Schmidt, Sadrieh and Rockenbach, 2010). Evidence based on large-scale representative data is scarce, as standard data sources rarely report the individual target, the provided effort and the economic outcome simultaneously.

To address this gap, we exploit novel register data to estimate the effect of changes in effort requirements. We consider the context of job search, where unemployment insurance (UI) regimes systematically use search requirements to regulate the provision of effort. Requirements, which usually define a minimum number of monthly job applications enforced through the threat of benefit sanctions, are increasingly used both in Europe and in the United States (see, e.g. Venn, 2012). Nevertheless, it is unknown how job finding reacts to the number of required applications. We provide first evidence by analyzing how marginal changes in the search requirement affect the provision of effort, the duration of unemployment and re-employment outcomes.

The study is based on unique register data from the Swiss UI system, reporting required and provided monthly job applications on an individual level. Our empirical design exploits the conditional random assignment of caseworkers to job seekers, which is based either on caseload or on observable job seeker characteristics. Within a public employment service (PES) office, some caseworkers tend to set higher requirement levels than others, inducing conditionally exogenous variation in the job seeker's expected requirement. We exploit this feature by using caseworker stringency as an instrument for individual requirement levels, conditional on PES $\times$ year fixed effects.[2] Formally, the instrument is the caseworker's average requirement level, excluding the individual's own requirement (leave-out mean). The approach is inspired

[1] See, e.g., Prendergast (1999) for an overview on financial incentives in firms.
[2] PES $\times$ year fixed effects account for office- and time-specific policies regarding requirement levels, which may for instance be endogenous to local labor market conditions.

by an increasing number of studies exploiting judge or caseworker leniency as an instrument for individual treatments (e.g. Aizer and Doyle, 2015; Kling, 2006; French and Song, 2013; Dahl, Kostol and Mogstad, 2014; Autor et al., 2017; Bhuller et al., 2017; Maestas, Mullen and Strand, 2013). Compared to chapter one of this thesis, the objective is not to assess the overall value of a caseworker, but to use variation in the behavior with respect to one specific treatment.[3]

Caseworker stringency shows a strong first stage relation with individual requirements: when the caseworker requests one application more from her other cases, the individual's predicted requirement size rises by 0.67 applications. In turn, caseworker stringency is hardly related to observed job seeker characteristics and unrelated to other actions taken by the caseworker (assigned training programs, referred vacancies, enforced sanctions). We further find large support of the monotonicity assumption, as caseworker stringency shows a strong first stage for a large set of different subgroups, even when being constructed based on out-of-subgroup individuals.[4]

As a main result, the study shows that the duration of un- and non-employment both decrease by 3% when the requirement increases by one monthly job application (effect of 7 and 11 days, respectively).[5] This estimate is robust to a large number of specification tests and additional controls for actions taken by the caseworker. We also use the identifying variation to estimate the returns to actually provided search effort. As the elasticity of search effort to the requirement is imperfect (0.67), an additionally provided monthly application decreases the length of spells by 4%.

Furthermore, results reveal that changes in the requirement mostly affect lower-skilled job seekers. For skilled job seekers, targeting the quantity of job applications appears to be less effective. Moreover, we find larger effects among individuals who exhibit low levels of voluntary effort, which we proxy by the number of applications provided prior to the first caseworker meeting. The requirement level usually imposes a strong incentive for those individuals. In line with theory, the results suggest that

[3]Note that I found in chapter one no evidence that the quantity or quality effects of caseworker absence operate through the channel of job search requirements (c.f. section 1.5.2 and table 1.A.7). It is thus reasonable to assume that the variation used in the two chapters are independent of each other.

[4]Testing monotonicity based on "reverse-sample instruments" is inspired by Bhuller et al. (2017).

[5]The unemployment duration is defined as the number of days between entry into and de-registration from unemployment (based on UI data). The nonemployment duration is defined as the number of days between entry into unemployment and re-employment (based on social security records).

the requirement effect is concave in voluntary effort.

In a final step, we assess the effect of requirement levels on additional outcome dimensions. First, the willingness to become non-compliant appears to increase with the requirement: the probability to be sanctioned through a benefit cut rises by 12%. Second, an additional required monthly application causes a modest reduction in job stability: the length of subsequent re-employment spells decreases, on average, by 0.3%. In turn, we find a zero effect on the re-employment wage.

Our findings provide first estimates of the returns to individual search effort induced by requirements. This has relevant implications for the understanding of job search behavior and the design of modern UI regimes. The extensive use of job search requirements across OECD countries is usually motivated by the assertion that UI benefits can induce an under-provision of search effort. This claim is based on a large literature showing that an increased generosity of UI benefits prolongs unemployment spells (e.g., Katz and Meyer, 1990; Card and Levine, 2000; Chetty, 2008; Lalive, 2008; Schmieder, Wachter and Bender, 2012). Further, the theoretical literature on optimal UI suggests that the enforcement of minimum effort provision can be welfare improving, as compared to a situation without monitoring (Pavoni and Violante, 2007). There is, however, no previous evidence on whether required search effort changes translate into relevant outcomes, as individual effort is not reported in standard UI registers.[6] To our knowledge, we are the first to estimate how job search outcomes react to the number of required job applications.

More generally, this chapter makes a novel contribution by estimating the returns to job applications, which constitute the most direct form of search effort. Both structural and reduced form analyses commonly assume search effort to be the main source of variation in the duration of unemployment spells, without actually observing effort provision. Recent exceptions include reduced form studies by Marinescu (2017), Fradkin (2017) and Lichter (2017), who estimate the effects of UI benefit generosity on effort provision using online search data and survey data,

[6] A number of studies show how the introduction or strengthening of a job search monitoring regime changes job finding outcomes (Arni, Lalive and Van Ours, 2013; Van den Berg and Van der Klaauw, 2006; McVicar, 2008; Petrongolo, 2009; Manning, 2009; Bloemen, Hochguertel and Lammers, 2013). Another set of studies exploits variation in job search monitoring resulting from field experiments in different U.S. states (Johnson and Klepinger, 1994; Meyer, 1995; Klepinger, Johnson and Joesch, 2002; Ashenfelter, Ashmore and Deschenes, 2005). However, none of these studies can analyze required effort changes that vary at the individual level.

respectively. We contribute with direct evidence from register data on the elasticity of labor market outcomes to induced job applications.

The chapter is structured as follows. We begin by discussing theoretical predictions on the effects of changes in requirements (section 2.2). Section 2.3 presents the data sources. In section 2.4, we discuss the empirical design. Section 2.5 presents the results and section 2.6 concludes.

2.2 Theoretical Discussion

In the following, we provide basic theoretical intuition on the effects of the search requirement level on the duration of job search. We further discuss factors which are expected to influence the elasticity of individual outcomes with respect to requirement changes. Throughout the discussion, the definition of search effort as the number of job applications is limited to its quantitative dimension. This is in line with the design of search requirements across OECD countries, which define the number of applications to be submitted.[7]

We consider a simple, single-period job search model with requirements and benefit sanctions, similar to Abbring, van den Berg and van Ours (2005) and Lalive, van Ours and Zweimueller (2005). The value function and its implications are described in Appendix A.1. In absence of a requirement policy, the unemployed individual chooses a voluntary level of search effort e^0, which trades off marginal costs and benefits of effort. The requirement policy introduces a minimum effort level r to be provided by the job seeker. Through the threat of a benefit cut in case of non-compliance, the policy introduces an implicit cost of providing less applications than r. This cost equals the amount of the benefit cut times the enforcement probability.

Effect of Requirement Level on the Duration of Non-Employment

We are interested in the intensive margin effect of an increase in the requirement level, $\partial r > 0$. Provided that the voluntary effort e^0 is smaller than r, $\partial r > 0$ causes the job seeker to raise effort e if its net cost (cost of the effort increase minus the benefit

[7] In most countries, monitoring of compliance with the requirement also includes guaranteeing a minimum quality standard, as caseworkers can for instance check the content of application letters. This is also the case in Switzerland (c.f. section 3).

from possible additional job offer arrival) exceeds the cost of risking a sanction. Under the assumption that effort e increases the job offer rate λ ($\lambda'(e) > 0$), we expect the duration of non-employment D to decrease ($\frac{\partial D}{\partial r} < 0, \forall e^0 < r$).[8]

The responsiveness of D to r can be influenced by different factors. In the following, we describe three factors and provide testable implications for an empirical analysis of their relevance.

1. Imperfect Compliance: First, the possibility of non-compliance ($e < r$) limits the amount of search effort which policy makers can induce with requirements. Non-compliance arises when individuals expect the cost of non-compliance to be lower than the net cost[9] of an additional application. In these cases, individuals prefer the risk of a benefit cut to an increase in effort. A testable implication of non-compliance is that an increase in the requirement induces a less-than-proportional increase in effort ($\frac{\partial e}{\partial r} < 1$). As a consequence, the elasticity of non-employment in response to a required application should, in absolute terms, be smaller than its elasticity with respect to a provided application ($|\frac{\partial D}{\partial r}| < |\frac{\partial D}{\partial e}|$).

2. Level of Voluntary Effort: Second, the change in effort provision imposed by the requirement depends on whether, and by how much, the requirement exceeds the voluntary effort e^0. The smaller the e^0 is relative to the requirement, the larger the incentive for the individual to increase effort provision.

As a testable implication for the empirical analysis, we should observe heterogeneous effects when interacting the effect of the requirement level with a proxy of voluntary effort. Our data source reports the number of applications which individuals provide in the month before their first caseworker meeting, i.e., before learning about their individual requirement. We use this information as a proxy for e^0, to test whether the effects of requirement increases differ between individuals with high versus low levels of e^0.

Individuals with a relatively high e^0 have a higher probability of facing a requirement that does not exceed their voluntary effort. We thus expect less effects for them than for individuals with relatively low e^0, whose requirement is likely to exceed their voluntary effort. For the same reason, we also expect that individuals with low e^0

[8] In the empirical analysis, we study both the un- and the non-employment durations as an outcome.

[9] I.e., the net of the additional effort costs minus the benefit of a potential additional job offer arrival.

have higher costs of complying with the requirement.

3. Responsiveness of Job Finding Rate to Application Quantity: Third, the responsiveness of the job offer rate to the number of applications may vary according to the individual's labor market characteristics. For instance, the application quantity may have little effects when the application quality matters substantially. As a consequence, we expect lower effects of the requirement level on high-skilled individuals, who have to signal their skills through a high-quality application. The testable implication is that the effect of the requirement level is heterogeneous across skill groups.

Effect of Requirement Level on Re-Employment Quality The effect of the requirement level on re-employment quality is of ambiguous sign. On the one hand, a higher requirement can induce increased sampling of low quality job offers. Furthermore, the reservation wage may reduce due to the disutility associated with being required additional effort. On the other hand, the boost in search effort could also extent the scope of search and thereby lead to the sampling of more high quality job offers. Further, the reduction in non-employment duration induced by additional applications can reduce the depreciation of wage offers (see, e.g., the discussion by Schmieder, Von Wachter and Bender, 2016). We will empirically assess these ambiguous predictions by estimating the effect of the requirement level on re-employment job stability and wages.

2.3 Data & Background

2.3.1 Data

Data Source and Sampling We base the empirical analysis on individual-level data from the Swiss UI registers, merged to social security records.[10] Our sample covers all individuals who entered UI between 2010 and 2014 in the cantons of Bern, Fribourg, Solothurn and Tessin.[11] In these cantons, job search monitoring is system-

[10]Sources: Swiss State Secretariat for Economic Affairs SECO (for UI register); Central Compensation Office CCO (for social securtiy records).

[11]Prior to 2010, search requirements were not systematically registered in the data.

atically reported in the central database, to which we have access.[12] The four cantons cover around 22% of UI benefit recipients and three different geographic and language regions in Switzerland. The database reports for each calendar month individual-level information on required and provided job applications. In addition, the data include exhaustive information on other treatments assigned over the unemployment spell, socio-demographics, benefit payments as well as employment and unemployment histories. Summary statistics on these variables are reported in appendix tables 2.B.1 and 2.B.2.

We define the duration of unemployment as the duration between the registration and de-registration from UI (UI records) and the duration of non-employment as the duration between entry into UI and the first month with earnings from employment (social security records).[13] The duration of unemployment is capped at 520 days (17% of the sample), which is the maximum benefit duration.[14] The duration of non-employment is capped after 900 days (10% of the sample) or at the maximum observation period of the social security data, which is December 2015 (6% of the sample).[15]

We limit the estimation sample to individuals who are aged between 20 and 55, full-time unemployed and not eligible for disability insurance. We further require that the individual was not registered as unemployed during at least one month prior to the current registration. To reduce noise in the instrument, we drop observations whose caseworker has less than 30 cases over the sample period (2.9%, the median caseworker has 191 cases). Dropping caseworkers with few cases is in line with, e.g., French and Song (2013), Maestas, Mullen and Strand (2013) and Dahl, Kostol and Mogstad (2014). Results are unaffected by modifying the cutoff (c.f. sensitivity analysis in section 2.5.4).

Our analysis is at the intensive margin, as we are interested in the effect of marginal changes in the number of required applications, within the population of eligible individuals. Therefore, we exclude individuals who did not receive a requirement

[12]Federal Swiss law prescribes the enforcement of job search requirements. Therefore, it is ensured that all cantons, including those outside the estimation sample, participate in the requirement policy. Anecdotal evidence suggests that these cantons have their own system of requirement registration rather than employing the central data base.

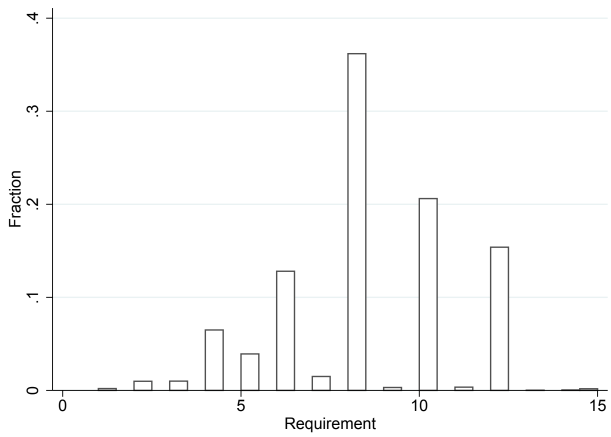
[13]We require an employment spell to last at least 2 months to define a non-employment exit. We do not count subsidized employment occurring during the formal unemployment spell as employment, unless it directly translates into an un-subsidized employment spell.

[14]The UI data are observed until January 2017.

[15]Results are insensitive to the exact choice of the cap (available upon request).

during the first six months after their entry into unemployment. By law, every benefit recipient has to receive a requirement. Nevertheless, individuals may not become eligible if they do not stay unemployed until their first meeting with a caseworker, or if their individual situation (e.g. parental leave or participation at an active labor market program) exempts them from the requirement policy. In total, 16% of individuals are excluded because they do not become eligible for a requirement. As individuals cannot anticipate their exact requirement level ex ante, out-of-sample selection with respect to the treatment is highly unlikely. Indeed, appendix table 2.B.3 shows that the probability of not having a requirement, i.e., being out-of-sample, is completely unrelated to the assigned caseworker's stringency.

Figure 2.1: Distribution of Requirement Levels



Requirements refer to monthly job applications. Data sources and sampling choices are described in section 2.3.1. N=96,833.

Measurement of Individual Requirements and Effort The requirement level, r_i, denotes the monthly number of applications to be submitted by individual i. We measure r_i as the default (modal) requirement assigned to an individual during the first six calendar months following the month of entry into unemployment.[16] There is a large stability of requirement levels over the spell. 77% of individuals only have one requirement level during the first six months of unemployment. 92% experience at most one change, and 98% at most two changes. In the sensitivity analysis, we show that results remain unaffected when excluding individuals whose requirement level changes, or when defining r_i as the modal requirement over the first three months

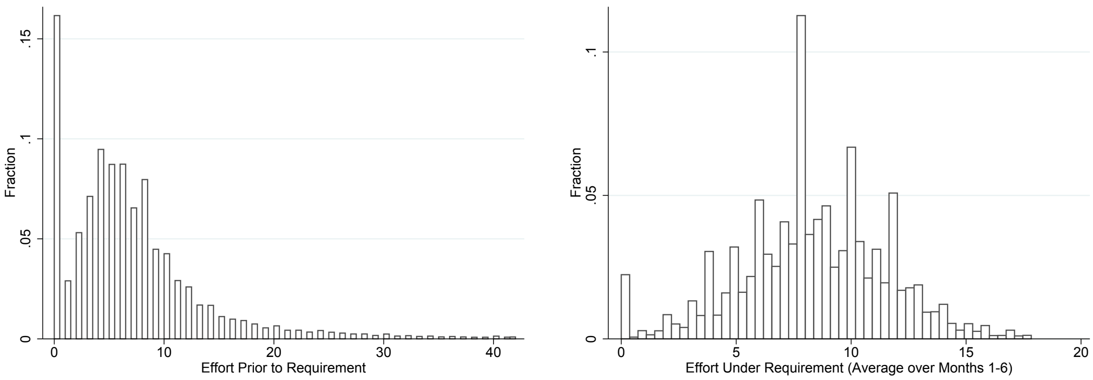
[16] 5% of individuals have more than one mode. In these cases, the highest mode is used. Results are robust to using the lowest mode instead (available upon request).

of unemployment (c.f. section 2.5.4). Figure 2.1 plots the sample distribution of requirement levels r_i. Requirements range roughly between 5 and 12 monthly job applications, with a mean of 8.3 and a standard deviation of 2.4.

The provided effort, e_i, is measured as as the average number of monthly job applications over the first six months following the month of entry into unemployment.[17] In the heterogeneity analysis, we will also exploit the number of applications provided in the month prior to the first caseworker meeting as a proxy for voluntary effort.[18] This information is available for 82,297 individuals (85% of the full sample). Figure 2.2 plots the distribution of the two effort variables. As shown in panel a, voluntary effort is broadly spread, and around 15% of individuals provide zero applications voluntarily. The requirement compresses the distribution of effort and induces peaks at the typical requirement levels 6, 8, 10 and 12 (panel b).

Figure 2.2: Distribution of Effort Levels

(a) Voluntary Effort (Proxy) **(b)** Effort Under the Requirement



Effort levels refer to monthly job applications. Panel a plots the number of applications provided before announcement of the requirement at the first caseworker meeting. Panel b plots the average number of applications provided under the requirement, up to month 6 after unemployment entry. Data sources and sampling choices are described in section 2.3.1. N=96,833 in panel a and N=82,297 in panel b.

2.3.2 Institutional Background

The Swiss Unemployment Insurance (UI) System In Switzerland, individuals are entitled to UI benefits if they contributed for at least six months during the two years prior to unemployment. To be eligible for the full benefit period, the

[17] When assessing the evolution of effects on effort in appendix table A.4, we will use the average application number over two-months-intervals as an alternative measure.

[18] Caseworkers are asked to register this information in the database.

contribution period extends up to 18 months for job seekers up to 55. The potential duration of unemployment benefits is usually 1.5 years for fully eligible prime age individuals. It varies, however, by the job seeker's contribution period, age and family situation. The replacement ratio ranges between 70% and 80% of gross previous earnings, depending on the individual family situation and the level of past earnings.

Caseworkers 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 survey results reported by Behncke, Frölich and Lechner (2010*a*), the most common assignment criteria in the Swiss UI are caseload, occupation or industry sector (all mentioned by about 50% of surveyed caseworkers and PES officials) and randomness (mentioned by 24%). Caseworkers are in charge of assigning individual requirement levels to the job seekers, in the form of a monthly number of job applications. They also monitor and counsel benefit recipients in their search for work, and refer them to labor market programs.

Job Search Requirements The first caseworker meeting usually takes place around two to three weeks after registration. At this meeting, the caseworker defines the requirement, i.e., the minimum number of monthly job applications which the job seeker must submit to avoid benefit cuts. From then onward, job seekers document their application activity in a monthly "protocol of search effort". A copy of the official form is included in appendix 2.C. Individuals fill in the date of application, the name and address of the potential employer, the mode of application (written, personal, via phone) and the status of the application. If the application was rejected, job seekers have to fill in the reason of rejection.[19]

Over the unemployment spell, caseworkers ask for proofs of submitted applications at their regular ($\approx$ monthly) meetings with the job seeker.[20] Once non-compliance with the search requirement is detected, the job seeker is notified. In the estimation sample, 27% of individuals are notified at least once for not complying with the requirement during the first six months of unemployment. With a probability of 50%, the notification results in a benefit sanction. The median size of a sanction is the monetary equivalent of 6 days of UI benefits (i.e., on average around 900 CHF).[21]

[19] In the data, we do not observe the mode of application and reason of rejection.

[20] Moreover, caseworkers occasionally check the truthfulness of reported applications by calling the prospective employer.

[21] 1 CHF = 0.86 EUR = 1.00 USD.

Individuals remain registered as unemployed after receiving a sanction.

2.4 Empirical Design

Our empirical design exploits the conditional random assignment of caseworkers to job seekers. Within a PES office, some caseworkers tend to set higher requirement levels than others, inducing conditionally exogenous variation in the job seeker's expected requirement.[22] We exploit this feature by using caseworker stringency (leave-out mean of requirements, excluding individual i's observation) as an instrument for individual requirement levels. The approach is inspired by an increasing number of studies exploiting judge or caseworker leniency as an instrument for individual treatments (e.g. Aizer and Doyle, 2015; Kling, 2006; French and Song, 2013; Dahl, Kostol and Mogstad, 2014; Autor et al., 2017; Bhuller et al., 2017).

In the following, we present the empirical model and assess the instrument.

2.4.1 Estimation Model

We exploit variation in caseworker stringency within PES offices as an instrument for the required and provided number of monthly job applications. We estimate the following baseline model using 2SLS:

$$s_i = \alpha + \gamma\ \overline{r}_{c,-i} + \pi_{o,t} + x_i'\beta + v_i \tag{2.1}$$

$$y_i = \alpha + \delta\ s_i + \eta_{o,t} + x_i'\theta + u_i \tag{2.2}$$

$$s_i \in (r_i, e_i)$$

The endogenous variable s_i denotes either the search requirement r_i or the provided effort level e_i, both measured in terms of monthly job applications. s_i is instrumented in the first stage equation 2.1 with the average requirement assigned by i's caseworker

[22] The exogenous assignment criteria are described in section 2.3.2. In total, the sample includes 506 caseworkers. A PES office has on average 15 caseworkers over the sample period.

c to job seekers other than i, $\overline{r}_{c,-i}$ (leave-out mean). $\overline{r}_{c,-i}$ is thus a measure of caseworker c's stringency in the requirement-setting process. The parameter of interest is the coefficient δ in the second stage equation 2.2. It estimates the marginal effect of one additional required/provided job application, induced by caseworker c's stringency, on the outcome y_i. We also estimate reduced form effects of $\overline{r}_{c,-i}$ on y_i.

The terms $\pi_{o,t}$ and $\eta_{o,t}$ include interacted PES office $\times$ year fixed effects. They ensure that we only exploit variation in caseworker stringency within offices and time periods. Thereby, office-time specific policies regarding requirement levels, which could for instance be influenced by local labor market conditions, are accounted for. In the sensitivity analysis (section 2.5.4), we show that results are unaffected when we include PES office $\times$ calendar quarter fixed effects instead. x_i includes job seeker i's socio-demographic characteristics and labor market history (summary statistics reported in appendix table 2.B.1). Results show that estimates of δ are invariant to the introduction of x_i. In a sensitivity check, we include further controls for other policy choices made by the caseworker (using corresponding leave-out means) and for caseworker experience into the model. Results remain unaffected.

y_i features the linear durations of un- and non-employment as the main outcomes. Further outcomes include measures of the individual non-compliance propensity, as well as re-employment stability and earnings.

2.4.2 Assessment of the Instrument

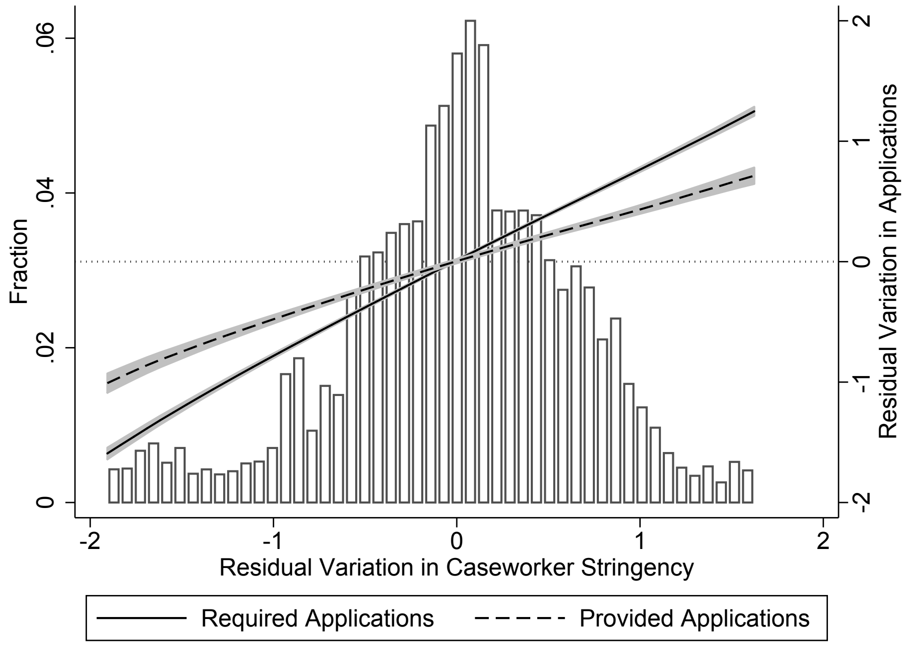
In the following, we first provide evidence on the strength of the first stage relationship and then test two conditions regarding instrument validity. The assessment is closely related to Bhuller et al. (2017), who test whether judge stringency is an appropriate instrument for incarceration.

Relevance of the First Stage

To illustrate the identifying variation, figure 2.1 plots a histogram of the residual variation in caseworker stringency $\overline{r}_{c,-i}$, conditional on interacted PES office $\times$ year fixed effects. There is a considerable amount of residual variation in the average requirement level which caseworkers tend to assign (standard deviation of 0.8 applications).

In addition, the figure illustrates how the residual variation in $\overline{r}_{c,-i}$ relates to the residual variation in required applications, r_i, and provided applications, e_i. The two lines plot local linear regressions of the residual variation in r_i and e_i, respectively, on the one in $\overline{r}_{c,-i}$.[23] The solid line illustrates how individual requirements respond to caseworker stringency along the distribution of caseworker stringency. The line documents a first stage relationship slightly below one. The dashed line plots the response of effort provision. It clearly ranks below the solid line, reflecting that requirements induced by caseworker stringency do not translate one-to-one into increases in effort. The figure thus provides first empirical evidence on the relevance of non-compliance as discussed in section 2.2, by showing that $\dfrac{\partial e}{\partial r} < 1$.

Figure 2.1: Caseworker Stringency, Requirement Levels and Effort Provision



Residuals stem from regressions of the respective variable (caseworker stringency/required applications/provided applications) on interacted PES $\times$ calendar year fixed effects. Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The graph is inspired by Dahl, Kostol and Mogstad (2014) and Bhuller et al. (2017). The histogram shows the density of residuals in caseworker stringency along the left y-axis (top and bottom 2% excluded). The solid line plots a local linear regression of residuals in required monthly applications on residuals in caseworker stringency. The solid line plots a local linear regression of residuals in provided monthly applications on residuals in caseworker stringency. The gray areas show 90% confidence intervals (standard errors clustered at the caseworker level).

Table 2.1 reports linear first stage estimates based on regression equation 2.1. Columns 1 and 2 show first stage effects of caseworker stringency on the individ-

[23]The illustration of the first stage relationship through local linear regressions is inspired by Dahl, Kostol and Mogstad (2014) and Bhuller et al. (2017).

ual requirement, with and without covariates. When caseworker stringency raises by one monthly application, the individual's requirement raises by 0.7 applications (column 2). The F-statistic of 79 documents the strength of the instrument.

Columns 3 and 4 report effects of caseworker stringency on the average number of monthly job applications provided over the first six months of unemployment. In line with the graphical evidence, results confirm that the reaction of provided effort to caseworker stringency is substantially below its effect on required applications: when caseworkers assign on average one monthly application more to their other job seekers, the individual's number of provided applications only increases by 0.47 applications (column 4). This implies that one required application results, on average, in 0.67 (=0.47/0.7) provided applications. Compliance is thus clearly imperfect. In appendix table 2.B.4, we find that the elasticity of effort to caseworker stringency is fairly stable over the first six months of unemployment, as it hardly decreases from 0.48 in months 1-2 to 0.45 in months 5-6 (panel A). The same holds true when we regress effort on the instrumented requirement (panel B). The elasticity is 0.69 up to month 4 and decreases slightly down to 0.64 in months 5-6.

Table 2.1: First Stage Regressions

	Requirement		Effort	
	(1)	(2)	(3)	(4)
Caseworker Stringency	0.766***	0.700***	0.487***	0.465***
	(0.022)	(0.023)	(0.341)	(0.335)
F-Stat (Test for Underidentification)	79.45	79.01	61.65	61.18
Covariates	No	Yes	No	Yes
Outcome Mean	8.287	8.288	8.517	8.518
N	96833	96833	96833	96833

Requirement and effort refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Instrument Validity

Conditional Independence Caseworker stringency ($\overline{r}_{c,-i}$) is valid as an instrument for individual requirements if it is conditionally independent of the job seeker's characteristics. Table 2.2 assesses to which extent requirement levels and caseworker stringency can be explained by individual characteristics observed in the data.

In column 1, we begin by regressing requirement levels on individual covariates. Results show a large degree of correlation between requirement levels and most of the variables. For example, individuals whose pre-unemployment earnings were higher than the median have to write 0.4 monthly applications less on average, and non-permanent residents have to write 0.3 applications more. The introduction of PES $\times$ year effects in column 2 hardly changes the pattern.

In columns 3 and 4, the outcome is our measure of caseworker stringency, $\overline{r}_{c,-i}$. Column 3 excludes PES office $\times$ year fixed effects from the regression. In this specification, results report a high degree of correlation between $\overline{r}_{c,-i}$ and most individual characteristics. Column 4 shows that this largely results from between-office variation, as the number and size of significant coefficients strongly reduces after the introduction of PES office $\times$ year fixed effects. Within offices, individuals aged older than the median have, on average, caseworkers who require 0.031 monthly applications less. Further, individuals previously employed in the white collar sector have caseworkers who require on average 0.063 monthly applications less, and individuals with low levels of education have caseworkers who require on average 0.041 applications more. This reflects a certain specialization of caseworkers on sectors and age profiles, as the assignment of job seekers to caseworkers may -in addition to caseload- be based on occupation or industry (c.f. section 2.3.2). Nevertheless, the size of the correlation is economically small, provided that the standard deviation of $\overline{r}_{c,-i}$ amounts to 0.8 after conditioning on PES office $\times$ year fixed effects. None of the other covariates relates significantly to $\overline{r}_{c,-i}$. In particular, there remains no relation to the individual's previous earnings and labor market attachment, which likely reflect a large degree of unobserved productivity differences. Indeed, it is unlikely that unobserved characteristics influence the assignment, because the officer deciding on the assignment only disposes of information which is also contained in the administrative data. This strongly suggests that we can address the small amount of non-randomness due to observable-based assignment by controlling for a flexible vector of age dummies and detailed occupation dummies.[24]

[24]Summary statistics on job seeker covariates included in the baseline regressions are reported in appendix table 2.B.1.

Table 2.2: Relation of Job Seeker Characteristics to Requirements and Caseworker Stringency

	Requirement		Caseworker Stringency (Leave-Out Mean)	
	(1)	(2)	(3)	(4)
Female	-0.126***	-0.075***	-0.060**	-0.000
	(0.037)	(0.022)	(0.030)	(0.015)
Aged > Median	-0.312***	-0.450***	0.125***	-0.031***
	(0.034)	(0.024)	(0.026)	(0.008)
Married	-0.058**	-0.099***	0.025	-0.008
	(0.024)	(0.017)	(0.018)	(0.007)
HH size >2	-0.141***	-0.048***	-0.107***	-0.005
	(0.031)	(0.016)	(0.025)	(0.008)
Non-Permanent Resident	0.288***	0.088***	0.172***	0.003
	(0.030)	(0.018)	(0.028)	(0.010)
Low Education	0.065	0.185***	-0.051	0.041***
	(0.040)	(0.025)	(0.034)	(0.014)
Blue Collar	-0.388***	-0.096***	-0.282***	0.032
	(0.073)	(0.032)	(0.070)	(0.023)
White Collar	-0.326***	-0.312***	-0.121*	-0.063***
	(0.071)	(0.031)	(0.066)	(0.020)
No of Prev. UE Spells	-0.054	-0.041**	0.027	0.007
	(0.044)	(0.021)	(0.032)	(0.007)
Previous Earnings > Median	-0.384***	-0.174***	-0.156***	0.005
	(0.034)	(0.021)	(0.028)	(0.010)
Share Employed in 5 Yrs Pre UE	-0.511***	-0.114***	-0.341***	0.000
	(0.057)	(0.034)	(0.047)	(0.019)
PBD <400 Days	-0.003	-0.008	0.014	0.008
	(0.029)	(0.018)	(0.024)	(0.008)
Office × Year FE	No	Yes	No	Yes
Outcome Mean	8.287	8.287	8.287	8.287
N	96833	96833	96833	96833

Requirement levels refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). UE=unemployment, PBD=potential benefit duration. Previous unemployment and employment are specified as linear variables. The sector of activity takes the three values "blue collar", "white collar" and "low-skilled service sector" (baseline). All other covariates are specified as binary variables. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Exclusion Restriction An additional challenge when using caseworker stringency as an instrument for requirements is the role of other, potentially related, policy choices. These could possibly affect the exclusion restriction. For instance, caseworkers who assign on average higher requirements may also enforce the compliance with rules more strictly. Similar issues are discussed in the empirical literature using judge stringency (in particular by Bhuller et al., 2017; Mueller-Smith, 2017), where judges not only decide on incarceration, but also on, e.g., fines, community service, probation, and guilt.

In table 2.3, we test whether the instrument $\overline{r}_{c,-i}$ correlates with other decisions typically taken by UI caseworkers. To this aim, we regress $\overline{r}_{c,-i}$ on the probability that job seeker i experiences other treatment assignments during the first six months after entry into unemployment. We distinguish between the assignment to training programs, the referral of vacancies, and the incidence of sanctions due to the non-compliance with a rule. To measure the type of vacancies a caseworker tends to refer, we also consider the share of vacancies that are in the job seeker's prior occupation.[25] Sanctions are distinguished according to whether they relate to the requirement (the job seeker provided too little or no applications) or not (e.g., the job seeker was not present at a caseworker meeting or at a training program). Summary statistics on the variables are reported in appendix table 2.B.2. In columns 1 and 2, the dependent variable is the individual's requirement level. No matter whether regressions include office $\times$ year fixed effects (column 2) or not (column 1), there is a large degree of correlation between the individual requirement level and all the other assignments.

In column 3, the dependent variable is the caseworker stringency measure $\overline{r}_{c,-i}$, but interacted PES office $\times$ year fixed effects are excluded from the regression. Results show a high degree of correlation, which is likely due to office-specific policy regimes. In offices where requirements are high, individuals are on average referred to more vacancies and assigned to more training programs. However, after we introduce office $\times$ year effects in column 4, caseworker stringency only relates to sanctions which refer to a non-compliance with the requirement. This relation is most likely driven by job seeker behavior, reflecting that job seekers with higher requirements face higher costs of compliance. Indeed, sanctions which are unrelated to requirements show no significant relation to $\overline{r}_{c,-i}$. This is also the case for the probability of a vacancy referral and the type of referred vacancies, as well as for the probability of a training

[25] We classify jobs as being in the same occupation if the three first digits of the occupation codes coincide.

assignment. It thus appears that caseworkers who are more stringent requirement setters do *not* behave in a systematically different way when it comes to other observed policy choices. This evidence is further supported in section 2.5.4, where we include the leave-out means of other policy choices made by the caseworker as control variables and find that results are unaffected.

Table 2.3: Relation of Other Policy Assignments to Requirements and Caseworker Stringency

	Requirement		Caseworker Stringency (Leave-Out Mean)	
	(1)	(2)	(3)	(4)
P(Sanction Related to Requirements)	0.179***	0.188***	0.046	0.069***
	(0.042)	(0.025)	(0.040)	(0.021)
P(Sanction Unrelated to Requirements)	0.166***	0.219***	-0.057	0.021
	(0.043)	(0.027)	(0.036)	(0.014)
P(Training Program)	0.421***	0.236***	0.172***	0.010
	(0.062)	(0.022)	(0.064)	(0.015)
P(Vacancy Referral)	0.849***	0.347***	0.307***	0.012
	(0.043)	(0.024)	(0.039)	(0.015)
Share Vacancies of Same Occupation	0.986***	0.404***	0.380***	0.027
	(0.064)	(0.032)	(0.048)	(0.018)
Office × Year FE	No	Yes	No	Yes
Outcome Mean	8.287	8.287	8.287	8.287
N	96833	96833	96833	96833

Requirement levels refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). Sanctions are classified as "related to requirements" if their stated reason is the under-provision of job applications by the job seekers, and as "unrelated" otherwise. Vacancy referrals are classified as "same occupation" if the first three digits of the vacancy's occupation code are the same as those of the job seeker's previous job. All regressions control for individual covariates (x_i). $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

As an additional check of the exclusion restriction, table 2.4 tests for a relationship between the caseworker's average requirement and her on-the-job experience.[26] To this end, we regress the average requirement assigned by a caseworker on different measures of experience and cases handled. In column 1, the explanatory variable is the number of cases handled by the caseworker during the ten years prior to the

[26]We do not observe additional caseworker characteristics in the data.

sample period.[27] In column 2, we consider the pre-sample number of years worked at the PES, in column 3 the in-sample number of years, and in column 4 the total observed number of years. None of the columns reports a significant relationship between experience and the average requirement set by the caseworker.

Table 2.4: Relation of Caseworker Experience to Average Assigned Requirement

	Average Requirement Set by the Caseworker			
	(1)	(2)	(3)	(4)
Pre-Sample No of Cases/100	0.004			
	(0.008)			
Pre-Sample No of Yrs Worked at PES		0.003		
		(0.011)		
Sample No of Yrs Worked at PES			-0.026	
			(0.037)	
Total No of Yrs Worked at PES				0.001
				(0.009)
Outcome Mean	8.129	8.129	8.129	8.129
N	506	506	506	506

The pre-sample period is 2010-2009. As the unit of observation is the caseworker, the number of observations reduces to 506. In column 1, the explanatory variable is the number of cases (divided by 100) which the caseworker treated during the pre-sample period 2000-2009 (mean=5.8). In column 2, it is the number of years which the caseworker worked at the PES during the pre-sample period 2000-2009 (mean=4.6). In column 3, it is the number of years which the caseworker worked at the PES during the sample period 2010-2014 (mean=3.8). In column 4, it is the total number of years worked at the PES from 2000 to 2014 (mean=8.5). All regressions include PES office fixed effects, but no additional covariates. ${}^{*}p < 0.10, {}^{**} p < 0.05, {}^{***} p < 0.01$. Robust standard errors.

Monotonicity Finally, we test for the monotonicity of our instrument. In his study on the effects of incarceration, Mueller-Smith (2017) argues that monotonicity can be violated in settings when judge stringency is used as an instrument, because judges can be more stringent in some and more lenient in other cases. In our setting, monotonicity would be violated if some caseworkers systematically set higher requirements for some types of job seekers and lower ones for others.

As pointed out by Bhuller et al. (2017), the monotonicity assumption has two testable implications in the context of judge/caseworker stringency instruments. On the one hand, the first stage relationship should be non-negative for any subsample. Bhuller et al. (2017) propose to test this implication by constructing the instrument

[27] We do not consider the number of cases handled during the sample period, as it is a potential outcome of the average unemployment duration of a caseworker's cases, which, in turn, can depend on the caseworker's requirement setting strategy.

based on the entire sample, and then using it in first stage estimations on subsamples. We implement this approach in column 1 of table 2.5. We first reproduce the linear first stage relationship for the entire estimation sample, which is 0.7. The sample is then split according to the job seeker's sector of activity (blue collar, low-skilled service sector and white collar), but the instrument remains the same. For all three subsamples, the first stage coefficient remains around 0.7. We perform the same exercise for a large number of additional job seeker covariates; the results are reported in column 1 of appendix table 2.B.5. The first stage is positive and strong for all subsamples.

Table 2.5: Testing Monotonicity

	Requirement (First Stage)	
	Baseline Instrument (1)	Reverse Sample Instrument (2)
0. Full Sample		
CW Stringency	0.700***	
	(0.023)	
Outcome Mean	8.287	
N	96833	
1. Subsample: Blue Collar Sector		
Estimate	0.662***	0.587***
	(0.027)	(0.031)
Outcome Mean	8.181	8.181
N	34057	34057
2. Subsample: Low-Qualif. Service Sector		
Estimate	0.721***	0.683***
	(0.035)	(0.039)
Outcome Mean	8.653	8.653
N	24935	24935
3. Subsample: White Collar Sector		
Estimate	0.697***	0.670***
	(0.027)	(0.032)
Outcome Mean	8.142	8.142
N	37841	37841

The test for instrument monotonicity is inspired by Bhuller et al. (2017). The reverse sample instrument is based on individuals excluded from the given subsample. For example, the reverse sample instrument for the white collar subsample is computed based on individuals in the blue collar or low-skilled service sector.The requirement refers to monthly job applications. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Results for subsamples based on other individual characteristics are provided in Appendix table 2.B.5.

As a second implication of monotonicity, the first stage relationship needs to be non-negative in a given subsample when the instrument is constructed based on individuals outside the subsample (c.f. Bhuller et al., 2017). This is tested in column 2 of tables 2.5 and 2.B.5. We construct a "reverse-sample instrument", which corresponds to the caseworker mean of requirements for individuals excluded from the subsample. This instrument is then used in a first stage regression run on individuals in the subsample. Results show that the coefficient size slightly decreases relative to the baseline instrument. However, it is consistently positive and ranges between 0.55 and 0.7. We thus find no evidence that the monotonicity assumption is violated in our setting.

2.5 Results

How does the requirement level affect labor market outcomes? We first report the requirement's average effect on the duration of un- and non-employment. We also estimate the effect of actually provided effort. In a second step, we analyze heterogeneity to understand the factors which influence the effect of search requirements. We then study how the requirement affects individual non-compliance by estimating its impact on the incidence of benefit sanctions. Fourth, we assess effects on the quality of accepted jobs. In a final step, we provide several sensitivity analyses.

2.5.1 Effects on the Duration of Un- and Non-Employment

Average Effects

OLS Estimates Before discussing the causal effect of changes in search requirements, we report OLS results as a baseline. We expect OLS estimates to be biased because caseworkers take individual labor market characteristics into account when setting requirements. Table 2.1 reports results from OLS estimations regressing the outcome on the individual requirement level, while controlling for office $\times$ year effects and individual covariates.

Results report a negative correlation between the size of the requirement and unemployment exit. Increasing the requirement by one application is associated with a 3 days longer unemployment spell (column 1) and a 6 days longer non-employment spell (column 2).[28] This may point to a selection mechanism according to which caseworkers assign higher requirements to less employable job seekers. Similarly, an individual who provides one more application per month stays, on average, about 6 days longer in un- and non-employment (columns 3 and 4).

Table 2.1: Effect on the Duration of Un-/Non-Employment: OLS Estimates

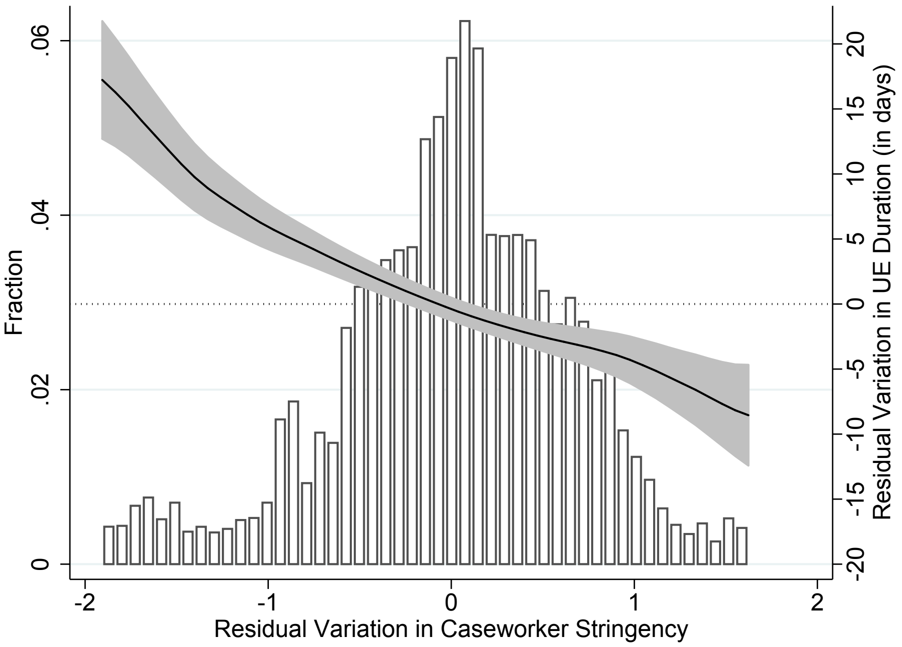
	UE Duration (1)	NE Duration (2)	UE Duration (3)	NE Duration (4)
Requirement	3.203***	5.533***		
	(0.462)	(0.790)		
Effort			6.557***	5.801***
			(0.333)	(0.412)
Outcome Mean	257.672	302.698	257.672	302.698
N	96833	96833	96833	96833

Requirement and effort refer to monthly job applications. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

IV Estimates We now turn to the causal analysis, using caseworker stringency (leave-out mean of requirements, $\overline{r}_{c,-i}$) as an instrument for individual requirement levels. We first provide graphical evidence on the reduced-form relationship between caseworker stringency and the duration of unemployment in figure 2.1. Inspired by Dahl, Kostol and Mogstad (2014), the graph represents an analogue of figure 2.1. It plots the residual variation in the unemployment duration (conditional on PES office × year fixed effects) against the residual variation in caseworker stringency. Based on local linear regressions, the figure reveals that unemployment spells shorten when caseworkers tend to set higher requirements. This is the case along the whole support of stringency.

[28]The duration of unemployment is capped at 520 days, while the duration of non-employment is capped after 900 days or at the maximum observation period of the social security data (December 2015).

Figure 2.1: Caseworker Stringency and UE Duration



Residuals stem from regressions of the respective variable (caseworker stringency/unemployment duration) on interacted PES × calendar year fixed effects. Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The graph is inspired by Dahl, Kostol and Mogstad (2014). The histogram shows the density of residuals in caseworker stringency along the left y-axis (top and bottom 2% excluded). The solid line plots a local linear regression of residuals in the duration of unemployment on residuals in caseworker stringency. The gray areas show 90% confidence intervals (standard errors clustered at the caseworker level).

Table 2.2 presents the linear regression results. In panel A, only interacted PES office × year fixed effects are included in the regressions. We add individual covariates (x_i) in panel B.[29] Column 1 shows the reduced-form effect of $\overline{r}_{c,-i}$ on the duration of unemployment. It reports that the duration decreases by 5 days when $\overline{r}_{c,-i}$ increases by one monthly application (panel B). As shown by column 2, the non-employment duration (i.e., the duration until re-entering employment) decreases by 7 days. The coefficient signs thus switch compared to the OLS estimates, confirming the intuition that requirement levels are endogenous to the job seeker's employability.

To estimate the return to an additional required application, columns 3 and 4 scale up the coefficients by the first stage (c.f. section 2.4.2 and table 2.1 for the first stage estimates). The IV estimates show that individuals reduce unemployment by 7 days and non-employment by 10 days when they are required to send one more

[29]Summary statistics on covariates are reported in appendix table 2.B.1.

application per month. Both estimates correspond to an effect of 3% relative to the mean. Appendix table 2.B.6 additionally reports effects on the probability to find a job (i.e., to exit non-employment) after 3, 6, 12 and 18 months. The effect in terms of percentage points is roughly stable over the outcome periods, implying that the percentage effect relative to the outcome mean decreases over the spell. The effect thus operates mostly at early stages.

As a further analysis, appendix table 2.B.7 reports how the probability to enter a PES-posted job is affected by the requirement. Coefficients for individuals exiting after different periods of non-employment all show an increase by around 2 percentage points (20% relative to the mean). This suggests that increased requirement levels also foster the use of PES-posted vacancies as application channel. One explanation is that applications to PES-posted vacancies are easier to monitor by the caseworker and presumably impose low search costs to the job seeker. However, it has to be noted that PES-posted vacancies only cover a small proportion of job findings (around 9% of exits from non-employment).[30]

Finally, we use the identifying variation to estimate the returns to provided search effort. In columns 5 and 6, we report IV estimates on the elasticity of un- and non-employment durations to the number of provided monthly job applications (first stage reported in section 2.4.2). As a consequence of imperfect effort compliance, the effects of an induced application exceed those of a required one: the duration of unemployment decreases by 11 days (4% relative to the mean), and the duration of non-employment by 15 days (5% relative to the mean).

Overall, the coefficients do not change significantly from panel A to B, suggesting a minor influence of job seeker covariates. We will document the robustness to additional control variables and sample changes in section 2.5.4.

[30] An open question is whether applications to PES-posted vacancies explicitly come at the expense of job finding through other channels. For instance, Van den Berg and Van der Klaauw (2006) show that individuals tend to substitute informal by formal job search when they are monitored by the PES. We leave this question for future research which disposes of data on search channels.

Table 2.2: Effect on the Duration of Un-/Non-Employment: IV Estimates

	Reduced Form		2SLS: Requirement		2SLS: Effort	
	UE Duration (1)	NE Duration (2)	UE Duration (3)	NE Duration (4)	UE Duration (5)	NE Duration (6)
Panel A: Without Controls						
CW Stringency	-5.675***	-7.839***				
	(1.321)	(2.158)				
Requirement			-7.412***	-10.237***		
			(1.778)	(2.893)		
Effort					-11.654***	-16.096***
					(2.935)	(4.724)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: W/ Controls for Individual Covariates						
CW Stringency	-5.044***	-6.950***				
	(1.226)	(1.919)				
Requirement			-7.205***	-9.927***		
			(1.792)	(2.806)		
Effort					-10.842***	-14.938***
					(2.861)	(4.403)
Outcome Mean	257.672	302.698	257.672	302.698	257.672	302.698
N	96833	96833	96833	96833	96833	96833

Requirement and effort levels refer to monthly job applications. They are instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects. In panel B, regressions additionally include individual covariates. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

Heterogeneous Effects

We now assess the underlying effect heterogeneity, to analyze the factors which might influence the responsiveness of unemployment durations to required job applications.[31] First, we study how the effect differs by the individual effort chosen before the first caseworker meeting, to proxy heterogeneity by voluntary effort. In a second step, we explore heterogeneity by the job seeker's individual labor market characteristics.

Voluntary Effort In table 2.3, we use the number of applications provided before the first caseworker meeting as a source of heterogeneity.[32] Since individuals do not know their caseworker or their exact requirement ex-ante, we interpret their pre-

[31] We report here heterogeneous effects on the duration of unemployment. The heterogeneity is proportional when the outcome is the duration of non-employment (available upon request).

[32] This information is only available for 82,297 individuals (85%). Caseworkers are asked to register this information in the database, since individuals are required to actively search for work as soon as they learn about the termination of their job.

meeting application effort as a proxy for voluntary effort.[33] Recognizing that the proxy is possibly noisy and can be mis-reported by the job seeker, we use it to test whether the effect of requirement changes depend on the individual's initial voluntary effort.[34] The theoretical intuition is that requirement increases are less costly for individuals whose voluntary effort is already at a high level. Therefore, the effect of an increased requirement is expected to be larger for individuals who have low levels of voluntary effort.

Table 2.3: Effect Heterogeneity: Job Seeker's Voluntary Effort

	Duration of Unemployment	
	Reduced Form (1)	2SLS (2)
CW Stringency × Low e_0	-7.477***	
	(1.475)	
CW Stringency × High e_0	-3.618**	
	(1.464)	
Requirement × Low e_0		-10.211***
		(1.990)
Requirement × High e_0		-5.473***
		(2.096)
p-value for H_0: coeff equality	0.027	0.035
Outcome Mean	249.660	249.660
N	82297	82297

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14536 individuals (15%). All regressions include interacted PES office × year fixed effects and individual covariates. PES office × year fixed effects are fully interacted with the dummy classifying individuals as "low e_0". $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

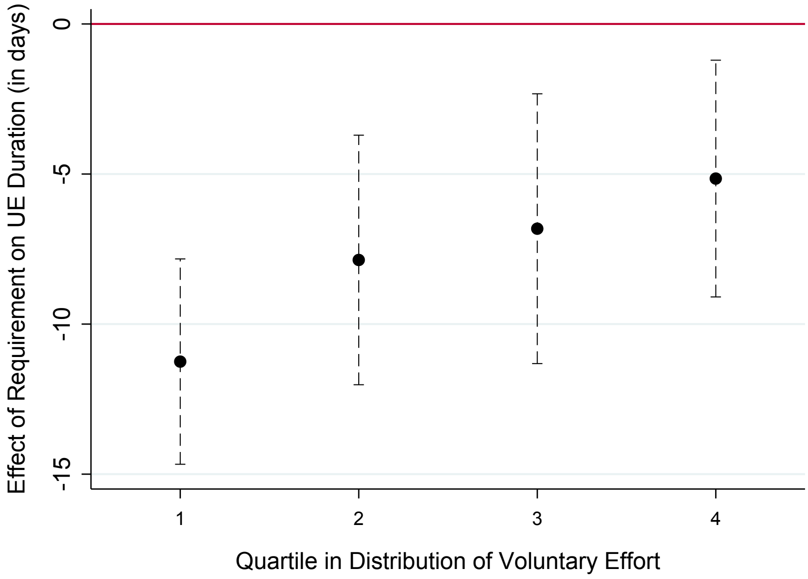
The empirical results support this intuition. Individuals whose original effort level ranks below the median (5 monthly applications) show significantly stronger reactions to required effort increases. As reported in column 1, a one-unit increase in caseworker stringency induces a decrease of 7.5 days of unemployment for this group.

[33] Appendix table 2.B.8 shows that there is no relation between the proxy of voluntary effort and caseworker stringency

[34] Our focus is on effort at the beginning of the unemployment spell, and we take no stance on whether our measure of e_0 proxies voluntary search in later periods of the spell.

One additional required application reduces unemployment by 10 days (column 2). Both effects are half as large for individuals with above-median initial effort levels (effect difference significant at the 5% level). Figure 2.2 additionally shows that the coefficient size decreases with the individual's quartile in the distribution of e_0.[35] It also reveals a concave pattern in the effect heterogenity, as individuals in the lowest quartile show by far the strongest reaction. This may reflect (i) convex effort costs and (ii) the fact that the share of individuals who do not have an incentive to react to the instrument increases with e_0.

Figure 2.2: Effects of Requirements on UE Duration, by Level of Voluntary Effort



The figure plots coefficients from a 2SLS regression, in which the instrumented requirement is interacted with the individual's quartile in the distribution of voluntary effort. Voluntary effort is measured as the number of applications provided by the individual in the month preceding her first caseworker meeting. Information on prior application activity is not available for 14536 individuals (15%). PES office × calendar year fixed effects are fully interacted with the quartile of voluntary effort. The average voluntary effort is 0.5 applications in the first quartile, 3.9 applications in the second quartile, 6.9 applications in the third quartile and 16.6 applications in the fourth quartile. The dashed lines show 90% confidence intervals (standard errors clustered at the caseworker level).

Individual Labor Market Characteristics As a second heterogeneity analysis, we explore whether the effect of required job applications differs by skill levels and socio-demographics. We thereby test whether the elasticity of job finding to the number of applications depends on the individual's labor market characteristics. From

[35]The average e_0 is 0.5 applications in the first quartile, 3.9 applications in the second quartile, 6.9 applications in the third quartile and 16.6 applications in the fourth quartile.

a policy perspective, this question is relevant for the design of targeted requirement levels.

Columns 1 to 3 of table 2.4 reveal that the effect of an additional required application is stronger for individuals with relatively low pre-unemployment income and educational attainment, as well as for individuals who were previously employed in the low-qualified service sector (mostly cleaning and restaurants). For all of these groups, the duration of unemployment decreases significantly more than for individuals with relatively high income and skills. A plausible explanation is that job seekers with a higher degree of education and specialization are bounded in their quantitative search effort by the availability of suitable offers. Moreover, the quality of applications might be of higher importance for highly educated job seekers. Therefore, search requirements that solely target the quantity of applications can be expected to have less effects on them.

Column 4 further shows that the effect of one required application is slightly higher for female job seekers. This estimate likely reflects that women are more often employed in the restaurant and cleaning sector. Somewhat surprisingly, column 5 shows that individuals aged higher than the median (35) react more. One possible interpretation is that older individuals could have more financial commitments and therefore a higher incentive to comply and to provide additional effort.

Table 2.4: Effect Heterogeneity: Individual Labor Market Characteristics

	Duration of Unemployment (2SLS)				
	(1)	(2)	(3)	(4)	(5)
Requirement × (Prev. Income > Median)	-6.212***				
	(1.918)				
Requirement × (Prev. Income ≤ Median)	-8.124***				
	(1.759)				
Requirement × (Low Education =1)		-13.104***			
		(2.024)			
Requirement × (Low Education =0)		-5.357***			
		(1.847)			
Requirement × (Low-Skill Service Sector =1)			-11.924***		
			(1.951)		
Requirement × (Low-Skill Service Sector =0)			-5.460***		
			(1.858)		
Requirement × Female				-8.152***	
				(1.805)	
Requirement × Male				-6.571***	
				(1.860)	
Requirement × (Age > Median)					-9.048***
					(1.891)
Requirement × (Age ≤ Median)					-5.487***
					(1.777)
p-value for H_0: coeff equality	0.015	0.000	0.000	0.047	0.000
Outcome Mean	257.672	257.672	257.672	257.672	257.672
N	96833	96833	96833	96833	96833

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

2.5.2 Effects on Non-Compliance

Beyond affecting the duration of unemployment, the requirement level may be relevant for further dimensions of individual behavior. In particular, the individual compliance decision may be affected by effort targets. From a theoretical perspective, higher requirements increase the cost of compliance. As a consequence, increases in the requirement level may increase non-compliance and thus trigger a higher incidence of benefit sanctions. In table 2.5, we test for this mechanism. Column 1 reports that one additional required application per month increases the probability of receiving a sanction during the first six months of unemployment by 2 percentage points (12% relative to the mean). Higher requirements thus indeed cause non-negligible increases in non-compliance behavior.[36]

Column 2 reveals that the effect is significantly stronger for individuals with relatively low voluntary effort e_0 (3 percentage points, versus 1 for individuals with high e_0, difference significant at the 1% level). This is in line with the theoretical intuition that lower voluntary effort reflects higher marginal effort costs. As a consequence, compliance with the requirement is more costly. Column 3 and 4 report analogous effects on the average number of sanctions imposed over the first six months of unemployment.

Taken together, the evidence clearly suggests that higher requirement levels make compliance harder to achieve. As a consequence, policy makers need to be aware that higher requirement levels induce a higher incidence of non-compliance. Further, the result calls for a discussion on the channels through which the effect of requirements on unemployment exit operates. Given that previous research has shown that benefit sanctions increase job finding (e.g., Abbring, van den Berg and van Ours, 2005; Lalive, van Ours and Zweimueller, 2005; Van den Berg and Van der Klaauw, 2006; Van der Klaauw and Van Ours, 2013; Van den Berg, Van der Klaauw and Van Ours, 2004; Arni, Lalive and Van Ours, 2013), an increased incidence of sanctions could be a relevant channel through which the effects of requirements operate.

We perform a back-of-the envelope calculation to get a tentative sense of the importance of benefit sanctions as an effect channel. Our results show that the number of sanctions raises by 0.02 in response to one additional required application. In their study based on Swiss data, Arni, Lalive and Van Ours (2013) find that the announcement and enforcement of a sanction reduce the duration of unemployment by 27 days.[37] The effect of an additional required application operating through benefit sanctions would thus be of around $27 \times 0.02 = 0.54$ days. This only corresponds to around 8% of the main effect (7 days, c.f. panel C of table 2.2). It therefore appears that only a minor part of the effect on unemployment duration operates through additional sanctioning. The sensitivity analysis of section 2.5.4 confirms this intuition by showing that results are unaffected when we introduce sanctions (through a caseworker-level leave-out mean, excluding individual i) as an endogenous control variable in the regression.

[36]Recall from section 2.4.2 that we found no association between the requirement level and the probability of being sanctioned for a reason that does not relate to the requirement. It is thus unlikely that the caseworker's enforcement strictness drives the effects reported here.

[37]Lalive, van Ours and Zweimueller (2005) find an effect of 20 days.

Table 2.5: Effect on the Incidence of Benefit Sanctions

	Prob. of Sanction (2SLS)		No. of Sanctions (2SLS)	
	(1)	(2)	(3)	(4)
Requirement	0.017***		0.020***	
	(0.005)		(0.006)	
Requirement × Low e_0		0.031***		0.037***
		(0.007)		(0.008)
Requirement × High e_0		0.009***		0.011***
		(0.004)		(0.004)
p-value for H_0: coeff equality		0.000		0.000
Outcome Mean	0.123	0.118	0.133	0.129
N	96833	82297	96833	82297

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The probability of receiving at least one benefit sanction (columns 1-2) and the number of sanctions (columns 3-4) are computed over the first six months of unemployment. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14536 individuals (15%). All regressions include interacted PES office × year fixed effects and individual covariates. In columns 2 and 4, PES office × year fixed effects are interacted with the dummy classifying individuals as "low e_0". $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

2.5.3 Effects on Job Quality

We next explore whether additional required applications affect the quality of accepted job offers. As noted in section 2.2, the expected direction of a potential effect is ambiguous: on the one hand, individuals may expand their search to worse wage offers to comply with the requirement. On the other hand, the additional applications may increase the chance of a good offer and counteract the depreciation of job offers over the non-employment spell.

In table 2.6, we assess how the requirement level affects the duration of re-employment spells.[38] Column 1 reports that the duration of the re-employment spell decreases by 0.6 days in response to an additional required monthly application. This effect can be considered as being small, as it only corresponds to a 0.3% change relative to the mean. Column 2 reports that the effect is only insignificantly stronger

[38] We consider the first re-employment spell: it starts at exit from initial non-employment and ends at a subsequent return to non-employment, measured on the basis of the social security data. It is available for individuals with an observed exit from non-employment (i.e., an exit before the maximum observation period of Dec 2015). We cap re-employment spells at 300 days because of our limited observation window. In all analyses of job quality, we control for the duration of non-employment through monthly dummies.

for individuals with relatively low voluntary effort.[39] In columns 3 and 4, we consider the more homogeneous sample of individuals with a non-employment duration of less than 360 days. For this group, statistical precision is larger, while the effects remain of similar size.

Table 2.6: Effect on Job Stability

	Duration of Re-Employment Spell (2SLS)			
Sample	*All with NE Exit*		*With NE Duration < 1 Yr*	
	(1)	(2)	(3)	(4)
Requirement	-0.649*		-0.929**	
	(0.388)		(0.414)	
Requirement × Low e_0		-0.852		-1.160**
		(0.595)		(0.587)
Requirement × High e_0		-0.359		-0.726
		(0.503)		(0.554)
p-value for H_0: coeff equality		0.508		0.566
Outcome Mean	269.388	268.362	274.889	273.761
N	82839	70486	65287	56137

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The duration of re-employment is measured as the difference between the first exit from non-employment and the first return to non-employment in the social security data. We cap it at 300 days because of our limited time window. The duration of re-employment is available if the individual exited from non-employment within the observation window. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size in columns 2 and 4 differs because information on prior application activity is not available for all individuals. All regressions include interacted PES office × year fixed effects and individual covariates. In columns 2 and 4, the fixed effects are interacted with the dummy classifying individuals as "low e_0". $^*p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

Next, we analyze how the requirement level affects re-employment wages. Table 2.7 reports effects on the average monthly log earnings obtained during the first three months after exit from non-employment. We find a zero effect in all specifications from column 1 to 4. There may be two explanations for this zero effect. It may be that the two theoretical channels discussed above counteract each other, or that marginal requirement changes are too small to affect job quality. It is interesting to compare this result to studies evaluating the introduction of a job search monitoring regime (i.e., the extensive margin). These studies tend to find negative effects on the quality of accepted jobs (e.g. McVicar, 2008; Petrongolo, 2009; Manning, 2009). In turn, our results show that small intensive margin increases in the requirement level

[39] We also find no significant heterogeneity for the characteristics considered in table 2.4

do not induce substantial additional harm to employment quality.

Table 2.7: Effect on Re-Employment Wages

	Log Monthly Re-Employment Wage (2SLS)			
Sample	*All with NE Exit*		*With NE Duration < 1 Yr*	
	(1)	(2)	(3)	(4)
Requirement	0.002		-0.002	
	(0.006)		(0.005)	
Requirement × Low e_0		-0.000		-0.003
		(0.007)		(0.008)
Requirement × High e_0		-0.000		-0.002
		(0.007)		(0.006)
p-value for H_0: coeff equality		0.983		0.892
Outcome Mean	8.117	8.107	8.185	8.171
N	82839	70486	63778	55326

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The outcome is the average monthly log wage obtained during the first three months after exit from non-employment. It is available if the individual exited from non-employment within the observation window. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size in columns 2 and 4 differs because information on prior application activity is not available for all individuals. All regressions include interacted PES office × year fixed effects and individual covariates. In columns 2 and 4, the fixed effects are interacted with the dummy classifying individuals as "low e_0". $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

2.5.4 Sensitivity Analysis

Influence of Other Policy Choices As discussed in section 2.4.2, one possible threat to the validity of the instrument is the influence of other, possibly related, policy choices made by the caseworker, or of other caseworker characteristics. While we did not find a relationship between caseworker stringency and individual-level policy assignments as well as caseworker experience in tables 2.3 and 2.4, we provide here an additional check. Similar to Bhuller et al. (2017), we include the leave-out means of other policy choices made by the caseworker (sanctions unrelated to the requirement, training programs and vacancy referrals) as control variables. Results are reported in table 2.8. The first row reproduces, for convenience, the baseline 2SLS estimate of the effect of an additional required application on the duration of unemployment.[40] In row 2, we control for different measures of caseworker experience and number of

[40] A sensitivity analysis with respect to the non-employment duration leads to the same conclusions (available upon request).

cases (all variables included in table 2.4). This hardly changes the coefficient. In row 3, we include as controls the leave-out means of other policy choices made by the caseworker, except for sanctions related to requirements, which are an outcome of the requirement level (c.f. section 2.5.2). The coefficient only changes by a small, insignificant amount (-0.8 days).

In row 4, we introduce the probability to be sanctioned due to the non-compliance with a requirement as an (endogenous) control variable. We again specify this variable as a leave-out mean on the caseworker level, excluding individual i's observation. While the result of this "bad control" exercise has to be interpreted with caution, we observe that the coefficient hardly reacts. This supports the idea that sanctions are not the main mechanism underlying the effects of requirement changes (c.f. discussion in section 5.3).

Table 2.8: Sensitivity Analysis for the Influence of Other Policy Choices

	Effect of Requirement on UE Duration (2SLS)	N
1. Baseline	-7.205***	96833
	(1.792)	
2. With caseworker experience	-7.279***	96833
	(1.783)	
3. With leave-out mean of other assignments	-6.409***	96833
	(1.823)	
4. With leave-out mean of endogenous sanctions	-8.077***	96833
	(1.902)	

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office $\times$ year fixed effects and individual covariates. In row 2, the additional control variables are measures of caseworker experience (cf. independent variables in table 2.4). In row 3, the additional control variables are the caseworker's leave-out mean probabilities of assigning a training program, of referring a vacancy and of imposing an unrelated sanction during the first six months of unemployment. In row 4, the additional control variable includes the caseworker's leave-out mean probability of a requirement-related benefit sanction during the first six months of unemployment. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level. Further estimation details can be found in section 2.4.

Additional Checks In a final step, we check the main result's sensitivity along additional dimensions. In table 2.9, the first row again recalls the baseline 2SLS estimate on the effect of an additional required application on the duration of unemployment. Row 2 shows that the coefficient does not change when controlling for PES office $\times$ quarter instead of year effects. Further, the effect significance remains unaffected when we cluster at the office level (N=35), instead of the caseworker level (row 3). In the fourth row, we include all caseworkers in the sample, also those with less than 30 cases. Row 5 excludes caseworkers with less than 50 cases. Both modifications leave the coefficient statistically unaffected. In row 6, we exclude individuals whose requirement levels change over the first six months of unemployment. Row 7 re-defines the individual requirement as the modal requirement assigned over the first three (instead of six) months of unemployment. Both changes do not affect the estimates significantly. Finally, we use in row 8 an alternative specification of caseworker stringency as the instrument. Instead of the caseworker's leave-out mean, we define the instrument as the share of cases in which the caseworker assigns a requirement which exceeds the PES office mean (excluding individual i). The coefficient is quasi-identical to the baseline.

Table 2.9: Additional Sensitivity Analyses

	Effect of Requirement on UE Duration (2SLS)	N
1. Baseline	-7.205*** (1.792)	96833
2. Control for PES office × calender quarter FE	-7.466*** (1.768)	96833
3. Cluster S.E.s at PES level	-7.205*** (2.015)	96833
4. Include caseworkers with < 30 cases	-8.615*** (1.806)	98655
5. Exclude caseworkers with < 50 cases	-6.967*** (1.829)	94983
6. Exclude individuals with requirement changes	-7.612*** (1.932)	74546
7. Use modal requirement over first 3 months of UE	-6.905*** (1.794)	93399
8. Use alternative measure of caseworker stringency	-6.950*** (1.962)	96833

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. In row 8, caseworker stringency is measured as the share of cases in which the caseworker assigns a requirement which exceeds the PES office mean (excluding individual i). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level. Further estimation details can be found in section 2.4.

2.6 Conclusion

We provide first empirical estimates of the individual returns to job search effort imposed by requirements. Exploiting conditionally random variation in caseworker stringency, we find that, on average, one additional required application reduces the duration of un- and non-employment spells by 3% (7 and 11 days, respectively). The corresponding effect of an additional provided application amounts to 4%. These results show that search requirements induce additional applications which are indeed relevant for the success of job search. However, policy makers need to be aware that required search effort does not translate one-to-one in provided search effort. We quantify the elasticity of effort provision to the requirement to be 0.67. In line with theory, it further turns out that higher requirements also induce higher rates of non-compliance and benefit sanctions.

When considering the longer run, we find modest reactions of post-nonemployment job quality to requirements. An additionally required job application causes re-employment spells to shorten by 0.3%; the effects on wages are zero. Strengthening the requirement regime thus seems only to marginally reduce job quality. One has to be aware, however, that this study quantifies the intensive margin effect of an additional job application. The introduction of a requirement policy, compared to the counterfactual of no such regime, could well induce stronger impacts on job quality outcomes (c.f. McVicar, 2008; Petrongolo, 2009; Manning, 2009, on the effects of introducing job search monitoring).

As a further important result, we find substantial heterogeneity in how individuals react to requirements. Effects of effort targets are strongest among lower-skilled job seekers. Furthermore, individuals who start off their unemployment spell with a low level of voluntary effort show stronger reactions to requirements. This suggests that there is substantial between-individual variation in effort cost and the returns to effort. Knowing these heterogeneous patterns can help policy makers to improve the design of requirement policies.

Overall, our estimates contribute to the scarce empirical evidence on how individual outcomes react to explicit targets and effort incentives provided by social insurance policies. In traditional theoretical analyses of the optimal UI problem, benefit levels are the social planner's only instrument to trade off moral hazard and insurance concerns, subject to budget constraints (Hopenhayn and Nicolini, 1997). Pavoni and

Violante (2007) show that introducing job search monitoring as an additional instrument into UI can be welfare improving. The intuition is that policy makers can afford to set higher benefit levels when monitoring counteracts moral hazard. Our findings show how marginal changes in search requirements affect the outcomes of job seekers in a real-world context. This provides a base for future research that empirically assesses the welfare consequences of policy mixes that enforce effort targets.

Appendix 2.A Theory Appendix

The job seeker's value function in presence of a search requirement r writes:[41]

$$\rho R = \max_e \left[b - c(e) + \lambda(e) \int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w) - I(e < r)\, p_0\, c \right] \tag{2.3}$$

p_0 denotes the probability of being sanctioned in case of non-compliance[42] and c the amount of a benefit cut imposed in the case of a sanction. b is the unemployment benefit, e the search effort measured as the realized number of applications and w the wage of the final job match. ϕ denotes the reservation wage, which equals ρR after optimization.

When *no requirement policy is in place*, the job seeker chooses the optimal effort level e^0, solving the decision problem (2.3) without the last term. e^0 results from a trade-off between the marginal cost of effort $c'(e)$ and its marginal benefit, which involves an increase in the job offer arrival rate $\lambda'(e)$ and the associated differential in value between employment and unemployment $\int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w)$.

In a *system with requirements*, the job seeker chooses her level of effort e by optimizing equation 2.3. In this expression, the requirement enters through the term $I(e < r)\ p_0\ c$: in case the job seeker provides a search effort that is lower than the requirement ($I(e < r) = 1$), there is an exogenous probability p_0 that the job seeker receives a benefit sanction which reduces the value of unemployment. There is thus an implicit cost of providing less applications than required. If the individually optimal effort level e^0 is smaller than r and if the net cost of providing the differential effort r - e^0 (i.e., cost of increased $c(e)$ minus benefit from increased $\lambda(e)$) is smaller than the cost associated to the risk of a sanction, the individual chooses to provide the required amount of effort r.

[41] Abbring, van den Berg and van Ours (2005) and Lalive, van Ours and Zweimueller (2005) set up similar value functions when discussing the effects of benefits sanctions.

[42] In principle, p_0 could be decomposed into two components: the probability that the non-compliance is detected and the probability that the detected non-compliance leads to the imposition of a benefit sanction. This decomposition would, however, not change the insights of our theoretical discussion.

Appendix 2.B Additional Tables

Table 2.B.1: Summary Statistics on Individual Covariates

Variable		Mean	Std. Dev.	Min	Max
Female		0.389	0.487	0	1
Married		0.392	0.488	0	1
Non-Swiss national		0.443	0.497	0	1
Non-permanent resident		0.181	0.385	0	1
Previous UE in yrs		0.598	0.808	0	4
Share employed in 5 yrs prior UE		0.752	0.257	0	1
Potential benefit duration < 400		0.283	0.450	0	1
Agegroup (omitted baseline: 20-24)					
	25-29	0.177	0.382	0	1
	30-34	0.160	0.367	0	1
	35-39	0.384	0.486	0	1
	40-44	0.127	0.333	0	1
	45-49	0.127	0.333	0	1
	50-55	0.118	0.322	0	1
Additional household members (omitted baseline: 0)					
	1	0.124	0.330	0	1
	2 to 3	0.120	0.325	0	1
	4 and more	0.008	0.090	0	1
Position in last job (omitted baseline: professional or self-empl.):					
	Manager	0.033	0.177	0	1
	Support	0.429	0.495	0	1
Level of education (omitted baseline: apprenticeship):					
	Minimum education	0.227	0.419	0	1
	Short further education	0.058	0.233	0	1
	High School	0.056	0.229	0	1
	Professional diploma	0.061	0.240	0	1
	Applied university	0.041	0.199	0	1
	University	0.066	0.248	0	1
	Missing	0.023	0.150	0	1
Domain of occupation in last job (omitted baseline: admin and office):					
	Food and raw materials	0.040	0.196	0	1
	Production (blue collar)	0.132	0.338	0	1
	Engineering	0.031	0.173	0	1
	Informatics	0.020	0.139	0	1
	Construction	0.130	0.336	0	1
	Sales	0.116	0.321	0	1
	Tourism, transport, communication	0.039	0.193	0	1
	Restaurant	0.174	0.379	0	1
	Cleaning and personal service	0.036	0.186	0	1
	Management and HR	0.036	0.185	0	1
	Journalism and arts	0.013	0.112	0	1
	Social work	0.012	0.110	0	1
	Education	0.011	0.102	0	1
	Science	0.011	0.104	0	1
	Health	0.035	0.184	0	1
	Others (skilled)	0.050	0.219	0	1
Sector of activity (omitted baseline: unskilled service sector)					
	Blue Collar	0.352	0.478	0	1
	White Collar	0.391	0.488	0	1
Average earnings in 2 yrs prior UE, Decile (omitted baseline: 1st)					
	2nd Decile	0.100	0.300	0	1
	3rd Decile	0.100	0.300	0	1
	4th Decile	0.100	0.300	0	1
	5th Decile	0.100	0.300	0	1
	6th Decile	0.100	0.300	0	1
	7th Decile	0.100	0.300	0	1
	8th Decile	0.100	0.300	0	1
	9th Decile	0.100	0.299	0	1
	10th Decile	0.100	0.300	0	1
N			96833		

Table 2.B.2: Summary Statistics on Treatments Assigned over the First Six Months of UE

Variable	Mean	Std. Dev.	Min	Max
P (Sanction Related to Requirements)	0.123	0.328	0	1
P (Sanction Unrelated to Requirements)	0.050	0.217	0	1
P (Training Program)	0.318	0.466	0	1
P (Vacancy Referral)	0.205	0.404	0	1
Share of Vacancies in Prior Occupation	0.167	0.348	0	1
N		96833		

Sanctions are classified as related to requirements if their stated reason is the under-provision of job applications by the job seekers. Vacancy referrals are classified as being in the job seeker's prior occupation if the first three digits of the occupation code coincide with those of the job seeker's previous job.

Table 2.B.3: Test for Out of Sample Selection

	P(Out of Sample)	
	(1)	(2)
CW Stringency	-0.004	-0.003
	(0.011)	(0.010)
Covariates	No	Yes
Outcome Mean	0.161	0.161
N	115446	115446

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Table 2.B.4: Effort Provision over the First 6 Months of UE

	Provided Applications per Month, Average over:		
	Months 1-2 of UE	Months 3-4 of UE	Months 5-6 of UE
	(1)	(2)	(3)
Panel A: Reduced Form Estimates			
CW Stringency	0.479***	0.484***	0.445***
	(0.032)	(0.035)	(0.038)
	(1)	(2)	(3)
Panel B: IV Estimates			
Requirement	0.685***	0.692***	0.644***
	(0.041)	(0.048)	(0.050)
Outcome Mean	8.926	8.552	8.336
N	87465	74968	56838

Requirements refer to monthly job applications. In panel B, the individual requirement is instrumented by caseworker stringency, which is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. The sample size decreases from column 1 to 3 because individuals exit unemployment. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

Table 2.B.5: Testing Monotonicity: Additional Job Seeker Characteristics

	Requirement (First Stage)	
	Baseline Instrument	Reverse Sample Instrument
	(1)	(2)
A. Gender		
1. Subsample: Male		
Estimate	0.689***	0.589***
	(0.023)	(0.029)
Outcome Mean	8.266	8.266
N	59195	59195
2. Subsample: Female		
Estimate	0.725***	0.671***
	(0.031)	(0.036)
Outcome Mean	8.320	8.320
N	37638	37638
B. Age		
1. Subsample: Age $\leq$ *Median*		
Estimate	0.695***	0.612***
	(0.027)	(0.031)
Outcome Mean	8.521	8.521
N	51001	51001
2. Subsample: Age $>$ *Median*		
Estimate	0.704***	0.645***
	(0.027)	(0.031)
Outcome Mean	8.026	8.026
N	45832	45832
C. Marriage Status		
1. Subsample: Unmarried		
Estimate	0.705***	0.638***
	(0.024)	(0.028)
Outcome Mean	8.344	8.344
N	58888	58888
2. Subsample: Married		
Estimate	0.690***	0.664***
	(0.027)	(0.029)
Outcome Mean	8.198	8.198
N	37945	37945
D. Household Size		
1. Subsample: Household Size ≤ 2		
Estimate	0.705***	0.666***
	(0.026)	(0.029)
Outcome Mean	8.418	8.418
N	51671	51671
2. Subsample: Household Size > 2		
Estimate	0.691***	0.659***
	(0.024)	(0.027)
Outcome Mean	8.137	8.137
N	45162	45162

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	First Stage: Requirement	
	Baseline Instrument (1)	Reverse Sample Instrument (2)
E. Residence Status		
1. Subsample: Swiss or Permanent Resident		
Estimate	0.696***	0.563***
	(0.023)	(0.030)
Outcome Mean	8.206	8.210
N	79296	79206
2. Subsample: Non-Permanent Resident		
Estimate	0.719***	0.708***
	(0.033)	(0.035)
Outcome Mean	8.652	8.652
N	17537	17537
F. Education		
1. Subsample: Apprenticeship or Higher		
Estimate	0.710***	0.558***
	(0.023)	(0.039)
Outcome Mean	8.234	8.237
N	69307	69225
2. Subsample: Unlearned		
Estimate	0.654***	0.609***
	(0.036)	(0.039)
Outcome Mean	8.420	8.420
N	27526	27526
G. Prior Unemployment		
1. Subsample: Without Prior Unemployment		
Estimate	0.710***	0.558***
	(0.023)	(0.039)
Outcome Mean	8.234	8.237
N	69307	69225
2. Subsample: With Prior Unemployment		
Estimate	0.654***	0.609***
	(0.036)	(0.039)
Outcome Mean	8.420	8.420
N	27526	27526
H. Labor Market Attachment (Share Employed in 5 Previous Yrs)		
1. Subsample: $\leq$ Median		
Estimate	0.709***	0.657***
	(0.027)	(0.030)
Outcome Mean	8.596	8.596
N	47549	47549
2. Subsample: $>$ Median		
Estimate	0.690***	0.663***
	(0.023)	(0.027)
Outcome Mean	7.989	7.989
N	49284	49284
I. Income in Previous Job		
1. Subsample: $\leq$ Median		
Estimate	0.724***	0.663***
	(0.027)	(0.032)
Outcome Mean	8.619	8.619
N	48281	48281
2. Subsample: $>$ Median		
Estimate	0.670***	0.622***
	(0.023)	(0.026)
Outcome Mean	7.957	7.957
N	48552	48552

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	First Stage: Requirement	
	Baseline Instrument (1)	Reverse Sample Instrument (2)
J. Potential Benefit Duration		
1. Subsample: $\geq$ *400 Days*		
Estimate	0.684***	0.599***
	(0.024)	(0.029)
Outcome Mean	8.172	8.172
N	69463	69463
2. Subsample: $<$ *400 Days*		
Estimate	0.735***	0.719***
	(0.028)	(0.030)
Outcome Mean	8.578	8.578
N	27370	27370

The test for instrument monotonicity is inspired by Bhuller et al. (2017). The reverse sample instrument is based on individuals excluded from the given subsample. For example, the reverse sample instrument for the subsample of females is computed based on male individuals. Requirements refer to monthly job applications. All regressions include interacted PES office × year fixed effects and individual characteristics. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Table 2.B.6: Effects on Job Finding Probability

	Prob. of Job Finding w/in T Months (2SLS)			
	T=3 (1)	T=6 (2)	T=12 (3)	T=18 (4)
Requirement	0.008**	0.013**	0.015***	0.013***
	(0.004)	(0.005)	(0.005)	(0.004)
Outcome Mean	0.260	0.487	0.674	0.792
N	96833	96833	96833	96833

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. The outcome is coded as one if an individual's social security records report positive employment earnings in month T after entry into unemployment. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506). Further estimation details can be found in section 2.4.

Table 2.B.7: Effects on the Probability to Leave from Unemployment to a Job Posted at the PES

	P(Exit to Job Posted at the PES)		
Sample	*NE Duration < 180*	*NE Duration < 360*	*All*
	(1)	(2)	(3)
Requirement	0.016***	0.020***	0.018***
	(0.006)	(0.006)	(0.005)
Outcome Mean	0.084	0.096	0.085
N	47168	65287	96833

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). Exit to a job posted at the PES is inferred from the reason of de-registration stated in the UI data. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Table 2.B.8: Caseworker Stringency and Voluntary Effort

	Caseworker Stringency	
	(1)	(2)
Voluntary Effort (e_0)	-0.001	-0.000
	(0.002)	(0.002)
Covariates	No	Yes
Outcome Mean	8.303	8.303
N	82297	82297

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. Voluntary effort (e_0) is measured as the number of applications provided by the individual in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14536 individuals (15%). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N=506).

Appendix 2.C Additional Background

Figure 2.C.1: Official Protocol of Job Search Effort (in German)

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Arbeitslosenversicherung

Eingangsdatum / Datum des Poststempels

Nachweis der persönlichen Arbeitsbemühungen

Name und Vorname	AHV-Nr.	Monat und Jahr

Datum der Bewerbung	Firma, Adresse Kontaktperson, Telefon-Nr.	Stellenbezeichnung	Zuweisung RAV	Pensum		Bewerbung			Ergebnis der Bewerbung				
Tag Monat				Vollzeit	Teilzeit (%)	Schriftlich / elektronisch	Persönlich	Telefonisch	noch offen	Vorstellungs- gespräch	Anstellung	Absage	Absagegrund

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(b) Page 2

Datum der Bewerbung	Firma, Adresse Kontaktperson, Telefon-Nr.	Stellenbezeichnung	Zuweisung RAV	Pensum		Bewerbung			Ergebnis der Bewerbung				
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Datum: ______________________ Unterschrift der versicherten Person:

Beilagen: ______________________ ______________________

Hinweis

Die versicherte Person muss alles Zumutbare unternehmen, um Arbeitslosigkeit zu vermeiden oder zu verkürzen. Insbesondere ist es ihre Sache, Arbeit zu suchen, wenn nötig auch ausserhalb ihres bisherigen Berufes (Art. 17 AVIG).

Die Pflicht, sich persönlich um Arbeit zu bemühen, gilt bereits vor Eintritt der Arbeitslosigkeit (z.B. während der Kündigungsfrist oder dem befristeten Arbeitsverhältnis).

Die versicherte Person muss der zuständigen Amtsstelle für jede Kontrollperiode (Kalendermonat) bis spätestens am 5. Tag des Folgemonats schriftliche Angaben über ihre Bemühungen um Arbeit einreichen (Art. 26 AVIV). Dazu dient dieses Formular. Schriftliche Unterlagen wie Kopien von Bewerbungsschreiben oder Absagebriefen sind beizulegen.

Nach dem 5. Tag des Folgemonats eingereichte Arbeitsbemühungen können nicht mehr berücksichtigt werden, ausser es liegt ein entschuldbarer Grund vor.

Versicherte Personen, die sich nicht genügend um zumutbare Arbeit bemühen oder eine solche ablehnen, werden je nach dem Verschulden bis zu einer Dauer von höchstens 60 Tagen in der Anspruchsberechtigung eingestellt (Art. 30 AVIG).

Mit unwahren oder unvollständigen Angaben macht sich die versicherte Person strafbar (Art. 105 ff. AVIG).

CHAPTER 3

Strengthening Enforcement in Unemployment Insurance: A Natural Experiment*

Enforcing compliance through a system of financial sanctions is a popular instrument in many areas of public policy. For policy design, it is key to understand how enforcement strictness affects the outcomes of non-compliant individuals. We provide first quasi-experimental evidence on this question, focusing on the case of unemployment insurance (UI). We exploit a sharp and unanticipated increase in the probability of being sanctioned after the failure to submit proof of the monthly job application activity. Based on a (dynamic) difference-in-differences design, we find that strengthening the enforcement probability by 10 percentage points increases unemployment exit within six months by 3%. A large proportion of this effect is, however, driven by transitions to job search without benefit receipt. While the UI saves on benefit payments, individuals thus experience losses in their income streams.

*This chapter is based on joint work with Patrick Arni. A prior version was published as IZA Discussion Paper No. 10353.

3.1 Introduction

Enforcing the compliance with rules is a challenge in many areas of public policy. In times of public budget austerity, financial penalties for non-compliance have become increasingly popular. Prominent examples include tax, environmental and welfare policies. Among the latter, enforcement is also used as an instrument to increase individual exertion of effort: many modern unemployment insurance (UI) and welfare systems condition the receipt of benefits on the compliance with job search requirements (c.f. the analysis in chapter 2). To enforce requirements, benefit recipients can be sanctioned by a benefit cut.

When designing the enforcement process, policy makers have to choose their degree of leniency towards individuals who did not comply with a rule. As outlined by Gray (2003) and Venn (2012), there exists large heterogeneity in leniency between OECD countries, with a tendency that UI regimes have become stricter over the past years. It is thus of high policy relevance to understand the elasticity of an individual's labor market outcomes to the experienced enforcement strictness: how do labor market outcomes change when individuals receive a strict instead of a mild response to their non-compliance?

This chapter provides to our knowledge the fist empirical evidence on this question. We exploit an unanticipated and sharp increase in the strictness of enforcement towards individuals who were detected not complying. The policy change affected individuals who had failed to deliver a list of their monthly job applications. It is particularly suitable for identification because it did not explicitly aim at strengthening enforcement. Instead, its intention was to reduce the administrative burden faced by the local authorities.[1] Nevertheless, the way non-compliant job seekers were treated changed substantially: before the reform, they would receive a rather "mild" notification, defining a second deadline for submitting the list of applications. The reform abolished this practice and turned to a "no excuse" policy. Detected job seekers were now informed that a benefit cut would be imposed in case they had no special reason or circumstance that excused the non-compliance.

Due to its unintended nature and sudden implementation, the reform generated a sharp quasi-experimental increase in the probability of being sanctioned conditional upon detection (from around 0.3 to 0.65). As a natural control group, we use job seek-

[1]Source: own inquiries at the federal UI authorities.

ers detected with a different type of non-compliance.[2] While this group was equally affected by aggregate conditions and has similar characteristics as the treatment group, it experienced no change in enforcement rules. We set up a difference-in-differences framework to evaluate the effect of a strict versus mild intensity of enforcement, given a detected non-compliance. Our estimations rely on detailed register data from the Swiss UI, linked to social security data.

When considering changes in enforcement policy, it is natural to conjecture that the immediate policy effect on non-compliant job seekers will soon be complemented by the adaptation of individual compliance behavior. As this could induce compositional changes, we start the empirical analysis by testing whether the reform changed the characteristics of non-compliant job seekers. To this end, we estimate a dynamic difference-in-differences specification on pre-unemployment wages. Results show that the wage profiles of non-compliant job seekers in the treatment and control group do not diverge in the first four months after the reform. In the medium run, the reform however seems to induce negative selection effects, as individuals in the treatment group have relatively lower pre-unemployment wages. It appears that after a few months, individuals with higher wage profiles refrain from becoming non-compliant in response to the increased enforcement strictness.[3] To estimate causal effects of the policy change on the behavior of non-compliant job seekers, we therefore restrict the main difference-in-differences estimation to a small window around the reform date, including four pre- and four post-reform months.

The results reveal substantial effects of the policy change on the exit from unemployment. For instance, the probability that job seekers exit within 6 months increases by 6.9 percentage points (12% relative to the mean). The overall duration of registered unemployment thereby decreases by 12%. However, a substantial part of the effect is driven by exits to unpaid job search. In response to stricter enforcement policy, individuals systematically prefer searching for work without benefits and become temporary non-participants. It thus appears that enforcement strictness boosts the disutility of registered unemployment. We find that this translates into a close to zero net effect on the overall duration of non-employment. This finding stands

[2]The primary types of non-compliance in the control group concern the submission of an insufficient amount of job applications or the failure to show up at a caseworker meeting. Over the unemployment spell, a job seeker can become non-compliant for several reasons. We use the first non-compliance to define the treatment status.

[3]This is in line with anecdotal evidence suggesting that the change was not officially announced and that individuals only gradually learned about it.

in contrast to the first two chapters, where both caseworkers and the requirement level influenced unemployment exit through job finding. This clearly suggests that enforcement is experienced by affected individuals in an exceptionally negative way.

Effects on the post-unemployment job quality are estimated with a large extent of statistical noise. While point estimates on wages are negative, they do not reach statistical significance. We, however, find significant negative effects for job seekers with good re-employment prospects. These individuals experience wage losses of around 7.8% in their first year of employment.

The findings in this chapter contribute to two main strands of the literature. First, we add new evidence to the empirical study of transitions into temporary nonparticipation. The fact that individuals frequently move between unemployment and nonparticipation has been pointed out by several previous studies (e.g. Flinn and Heckman, 1983; Elsby, Michaels and Solon, 2009; Kroft et al., 2016). Rothstein (2011) and Farber and Valletta (2015) show that UI benefit extensions reduce exits from unemployment to nonparticipation. Our findings offer new insights by showing that an increased use of unpleasant policies in UI can induce individuals to become temporary nonparticipants even before benefit exhaustion. This finding is in line with the intuition provided by Frijters and der Klaauw (2006), who set up a job search model in which transitions into nonparticipation occur when the reservation wage drops below the utility of being nonparticipant.

Second, our study is related to the empirical literature on the effects of benefit sanctions in UI and welfare regimes. The existing evidence is dominated by non-experimental studies (relying on the timing-of-events approach). These studies thus use a different source of variation and focus on a different parameter: they estimate ex-post treatment effects of an imposed benefit sanction and/or the warning that a sanction might be imposed in the future (e.g. Van den Berg, Van der Klaauw and Van Ours, 2004; Abbring, van den Berg and van Ours, 2005; Lalive, van Ours and Zweimueller, 2005; Rosholm and Svarer, 2008; Arni, Lalive and Van Ours, 2013; Van den Berg and Vikstroem, 2014). In turn, we quasi-experimentally identify the effects of a *policy change* in the enforcement probability.[4] Furthermore, we contribute

[4]Besides the literature on benefit sanctions, a branch of quasi-experimental and experimental studies assesses, among other components, monitoring practices in UI (e.g. Black et al., 2003; Ashenfelter, Ashmore and Deschenes, 2005; Van den Berg and Van der Klaauw, 2006; McVicar, 2008; Petrongolo, 2009; Cockx and Dejemeppe, 2012). However, these studies evaluate a whole “package” of measures, like e.g. monitoring and job search assistance.

by considering a comprehensive set of outcomes, including the exit from un- and non-employment as well as post-unemployment outcomes. This allows assessing in detail how an enforcement shock affects the job seeker's choice of different pathways back into employment. In particular, we identify increases in the duration of unpaid job search (temporary nonparticipation) as a non-classical route taken by job seekers who exit from unemployment before benefit exhaustion.

The remainder of the chapter is structured as follows: in section 3.2, we lay out the institutional framework of the Swiss UI and the natural experiment. Section 3.3 provides theoretical intuition on how the policy change is expected to affect behavior in the short versus medium run. In section 3.4, we describe the data sources and sampling criteria. Section 3.5 presents the empirical analysis. In Section 3.6, we discuss results and quantify the main trade off induced by the policy change. Section 3.7 concludes.

3.2 Institutional Setting and the Natural Experiment

This section outlines the institutional setting and the natural experiment which we exploit to identify the effects of a strengthened enforcement policy in UI.

Rules and Requirements in the Swiss UI Claiming UI benefits in Switzerland[5] entails a number of obligations. These include the provision of sufficient search effort, the regular attendance at caseworker meetings and participation in active labor market programs. The local Public Employment Service (PES) office is obliged by law to monitor the job seeker's compliance with these requirements and rules.

In this study, we analyze a reform in the enforcement of job search obligations. During their first contact with the caseworker, job seekers are informed about the monthly number of applications they have to provide. Job seekers list their applications in a "protocol of search effort", which they have to submit up to the 5th day of the following month. PES offices have to monitor whether the protocol is sent in by

[5]For fully eligible prime age individuals, potential benefit duration is 400 working days. For young or only partially eligible workers, benefit duration is reduced by 140 or 200 days. For older workers (aged 55+) it is topped up by 120 days. The replacement rate is 80% or 70% of the previous salary, depending on the family status and on the amount of the previous salary.

the deadline and whether the realized number of applications fulfills the requirement. Moreover, caseworkers are required to check the listed applications,[6] which prevents job seekers from reporting fake applications in the protocol.

Natural Experiment: Policy Change in the Enforcement Process The enforcement process is launched if a job seeker is detected not to comply with one of the UI rules. The process can lead to the imposition of a benefit sanction. Sanctions cut benefit levels to zero for a limited number of days (usually between 5 and 10).

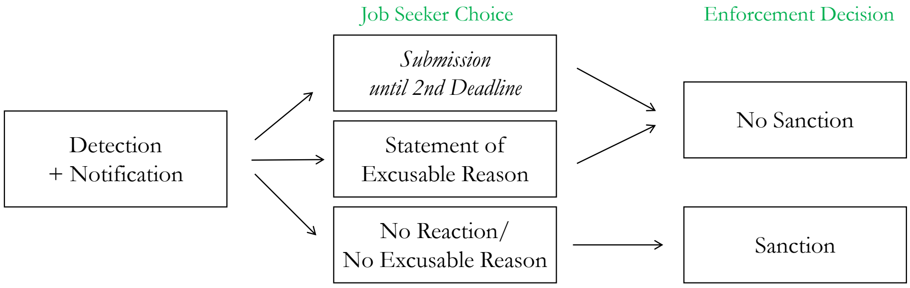
We exploit a policy change in the enforcement process, which links the detection of non-compliance to the imposition of a sanction. The policy change abolished the accordance of a second chance to job seekers who did not submit their protocol of search effort by the deadline. In the pre-reform regime, these job seekers received a notification which defined a second deadline. They could submit the missing protocol up to this second deadline and thereby avoid a benefit sanction. Alternatively, they could state the reasons for not submitting the protocol to reduce the risk of being sanctioned. The PES authorities would then decide whether the stated reasons were "excusable" or whether the job seeker would be sanctioned. The pre-reform enforcement process is illustrated in figure 3.2a.

In April 2011, the federal ministry abolished the practice of setting second deadlines. The motivation behind this policy change was purely administrative: the cantonal authorities had complained about the organizational burden of the enforcement process.[7] The reform became effective for protocols reporting on job applications submitted in April 2011 or later. This implies that from May 2011 onward,[8] non-compliance notifications did no longer set a second deadline (c.f. figure 3.2b).

[6]Caseworkers usually ask job seekers to bring submitted applications to their regular meetings; they may also call up potential employers to verify the application effort.
[7]Source: inquiries at the state secretary for economic affairs (SECO).
[8]May 5th was the deadline for protocols referring to April.

Figure 3.1: Job Seekers' Choices in the Enforcement Process, Pre and Post Reform

(a) Pre Reform



(b) Post Reform

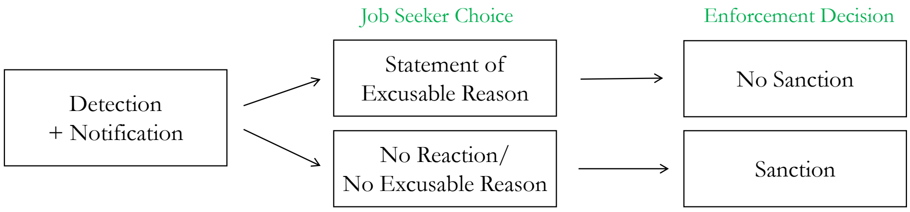
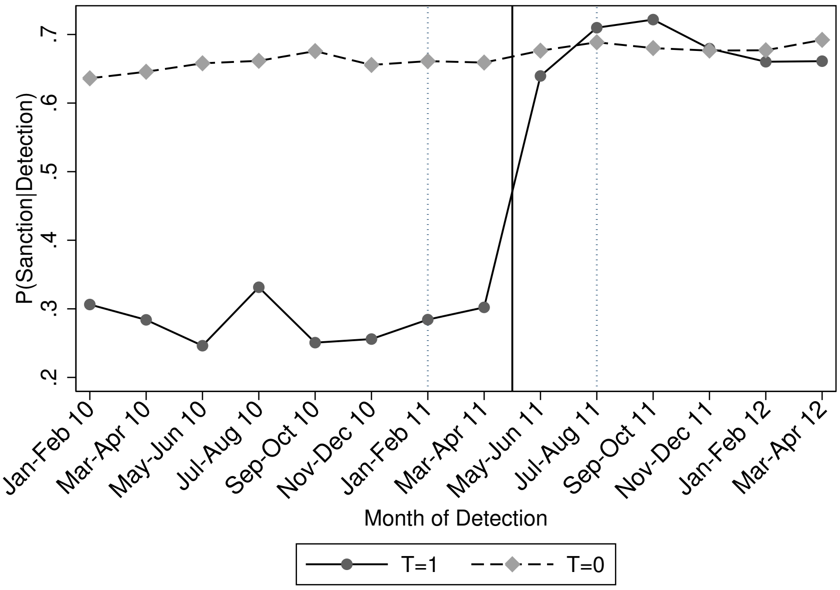


Figure 3.3 shows that the abolition of second chances had a large effect on the enforcement strictness faced by job seekers who had not submitted their protocol by the first deadline (treatment group, T=1). The dashed vertical lines denote the short-run sample window. Within this time window, the probability of receiving a benefit sanction conditional upon receiving a notification jumped sharply by more than 100%, from around 0.3 to 0.65.[9] At the same time, the probability of sanction for all other types of non-compliance notifications (control group, T=0) remained stable. For these other types, a second chance policy had not existed prior to the reform date and the enforcement process already followed the procedures described figure 3.2b.[10]

[9]Recall that after the reform job seekers can still avoid being sanctioned by stating an "excusable reason" (e.g. sickness or an accident) for not having submitted the protocol. This is why the probability does not increase to 1.

[10]This procedure is also described in Lalive, van Ours and Zweimueller (2005) and Arni, Lalive and Van Ours (2013), who estimate the effects of non-compliance notifications and sanctions using a timing-of-events framework.

Figure 3.3: Probability of Sanction, Conditional on Detection



The dotted vertical lines delimit the short-run sample window. The solid vertical line indicates the reform date. The treatment group (T=1) contains job seekers who receive a non-compliance notification in the treatment group (for not having submitted the job search protocol). The control group (T=0) contains job seekers who are notified about a non-compliance in the control group (other reasons of non-compliance). The underlying data sources and sampling choices are described in section 4.2.

3.3 Theoretical Discussion

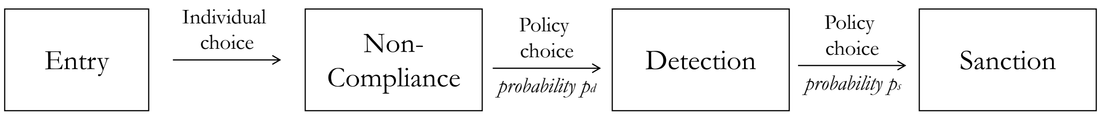
In the following, we discuss briefly how the increased enforcement probability is expected to affect the behavior of job seekers in the short versus medium run.

Figure 3.1 illustrates the different states created by the UI enforcement process. After entry into unemployment, individuals choose whether to comply with the rules. When being non-compliant, the probability $p_d \in (0, 1)$ determines whether the non-compliance is detected. The policy maker can vary this probability, for instance through the choice of monitoring technologies. In the context of our analysis, p_d is stable. Conditional on detection, the probability of being sanctioned after detection, $p_s \in (0, 1)$, determines the likelihood of receiving a benefit cut. The policy maker varies this parameter through her leniency towards non-compliant job seekers, e.g., in the form of second chances.

Given that the sanction implies a cut in UI benefits, the present value of detected individuals decreases in the probability of sanction p_s, implying a decrease in reservation wages and an increase in search effort. Appendix 3.A shows formal expressions for the present value of detected and sanctioned individuals, as represented by a standard job search framework.

In this chapter, we estimate the effects of a policy-driven increase in the sanction probability ($\Delta p_s > 0$) on non-compliant individuals. We distinguish the short run, where individuals learn about Δp_s only upon detection, from the medium-run, where job seekers are potentially aware of it when deciding about compliance.

Figure 3.1: States in UI Enforcement System



1. Short Run: Job Seekers Learn about the Policy Change Upon Detection In the short run, we assume that the policy-driven $\Delta p_s > 0$ is unrelated to the individual's perceived probability prior to non-compliance detection. In sections 4.2 and 5.2, we provide empirical evidence that this assumption holds.

We thus compare two groups of individuals who were –prior to non-compliance detection– holding the same expectations about their sanction probability. After detection, individuals in the post-reform group learn that they face a high sanction probability, while individuals in the pre-reform group learn that they have a second chance. The policy change thus causes two effects in the short run: first, individuals whose non-compliance is detected after the reform receive a much stronger signal about the strictness of the UI regime and their *prospective chances* of being sanctioned. Second, the share of individuals who will *actually experience* a benefit cut is larger in this group.

As both effects induce a decrease in the present value of unemployment, reservation wages decrease and search effort increases. Therefore, the the unemployment duration is expected to decrease. The net effect on post-unemployment earnings is ambiguous: on the one hand, job seekers are expected to accept lower wage offers due to reduced reservation wages. On the other hand, faster unemployment exit results in less depreciation of wage offers (see, e.g., the discussions by Schmieder, Von Wachter and Bender, 2016; Nekoei and Weber, 2017).[11] In addition, job seekers can be induced

[11] Both Schmieder, Von Wachter and Bender (2016) and Nekoei and Weber (2017) estimate how the potential duration of UI benefit payments affects post-unemployment wages.

to transit from unemployment to job search without UI benefit receipt if the policy decreases the reservation value below the utility of nonparticipation ((c.f. Frijters and der Klaauw, 2006).[12]

2. Medium Run: Job Seekers Are Aware of the Policy Change Prior to Detection In section 5.2, we provide evidence that the reform changed the selection of non-compliant individuals in the medium run. This points towards a third, anticipatory effect of an increased sanction probability: a high future sanction probability increases the cost associated to non-compliance. This may affect the number and type of job seekers becoming non-compliant.

In the presence of anticipation, it is impossible to distinguish composition effects from actual behavioral effects of the policy change on non-compliant job seekers. We therefore interpret mid-run effects as a mixture of selection and behavioral effects.

As a consequence, the short run impact directly identifies the effect of an unanticipated Δp_s on the behavior of non-compliant job seekers. The mid run effects are less informative of behavioral changes among non-compliant job seekers. They can, however, provide some evidence on the types of job seekers who ex-ante adapt their compliance behavior when being aware of the policy reform.

3.4 Data and Descriptive Statistics

This section first describes the data sources and sampling rules. In a second step, it provides descriptive evidence on how the different parameters of the enforcement process evolved around the reform date.

3.4.1 Data Sources and Sampling

Data Sources We use Swiss UI administrative data on the full population of job seekers entering formal unemployment. The data include extensive information on entry into and exit from unemployment on a daily basis, as well as individual socio-demographic characteristics and employment history. Most importantly, they report the date and reason of each non-compliance detection. We further observe if and when the job seeker submitted a statement on the reasons for the non-compliance,

[12]Frijters and der Klaauw (2006) estimate a structural job search model allowing job seekers to exit the labor force.

as well as the final decision on sanction imposition. To track mid-run employment outcomes, we match the UI data to social security records, which report information on employment status and earnings on a monthly basis. The data are available until the end of 2014.[13] Moreover, the social security data are used to control for individual wages during the 24 months prior to unemployment.

Sampling The official enforcement procedure for imposing benefit sanctions entails three steps: (i) the detection and registration of the non-compliance, which includes a written notification to the job seeker, (ii) the job seeker's statement and (iii) the enforcement decision. In practice, not all cantons appear to respect this procedure, which leads to systematically missing dates of job seeker statements and systematically coinciding dates of notification and final sanction decisions. In these cases, we do not know whether and when job seekers were notified about the non-compliance detection. As this information is crucial for the analysis, we need to exclude cantons who do not report full information on the enforcement processes. By excluding cantons where more than a quarter of enforcement cases do not report a job seeker statement,[14] we end up including 14 out of 26 cantons in our data set, which corresponds to 65% of registered enforcement cases.[15] Further, we apply standard sampling restrictions by focusing on job seekers who are eligible for UI benefits and aged between 20 and 55 years. We further exclude part-time unemployed job seekers, as well as job seekers eligible for disability insurance.

We analyze the behavior of job seekers who receive at least one non-compliance notification during their unemployment spell.[16] To achieve a sample of job seekers with a relatively homogeneous elapsed unemployment duration at the time of notification, we include only job seekers who received their first notification during the first 120 days after entry. This covers 80% of all first notifications.[17] For the short run

[13] For 98.4%, we observe post-unemployment job and earnings paths up to at least 18 months after unemployment exit. The other 1.6% are censored before.

[14] This is a plausibility cutoff; our results are not affected if we shift it to the left or right. Documentation is available upon request.

[15] Note that we are able to cover substantially more cantons than previous studies on the Swiss UI benefit sanction system using data from the late nineties and early two thousands by Lalive, van Ours and Zweimueller (2005) and Arni, Lalive and Van Ours (2013), who cover respectively 3 and 7 cantons.

[16] We exclude notifications that concern the refusal of acceptable job offers (3% of notifications), because they generate sanctions which are on average four times higher than those of the other enforcement types. They are thus likely to concern special cases and not suitable as part of the control group.

[17] Sensitivity analyses show results are robust to modifications of the 120-days-cutoff.

diff-in-diff analysis, the sample contains unemployment spells with a first notification is registered between the four pre- and four post-reform months, i.e., between January and August 2011. In a dynamic design, we use additional pre- and post-reform months to test for common pre-trends and to document medium-run effects. The sample then spans from January 2010 to April 2012.

3.4.2 Descriptive Evidence on the Enforcement Process

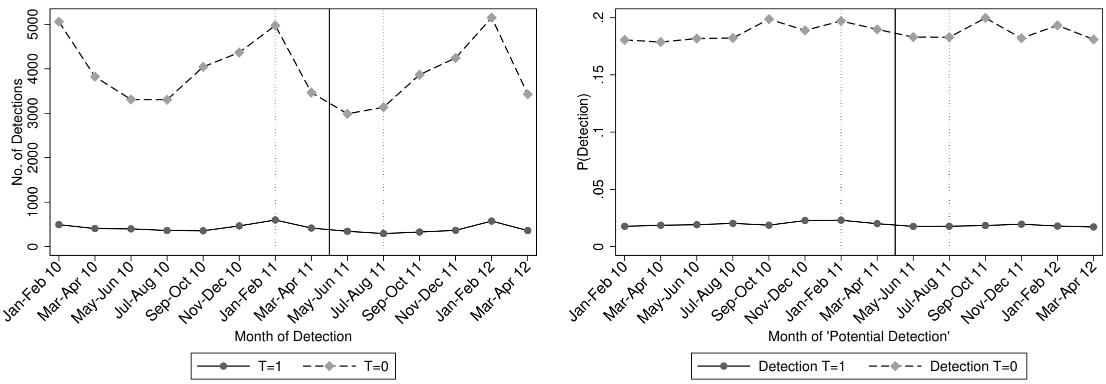
Non-Compliance Detection The policy change raises the costs associated to a non-compliance. If anticipated by the job seeker, it is therefore likely to reduce the number of non-compliant individuals. In the following, we show descriptively how the propensity of non-compliance detection evolved around the reform date.

Figure 3.2a shows a time series of the number of detected non-compliances. As it is clearly driven by cyclical components in the stock of unemployed individuals, figure 3.2b additionally reports the probability of non-compliance detection.[18] Both figures suggest that the propensity of non-compliance evolves similarly in the treatment and control group around the reform date. It appears that the policy change did not induce a strong reaction in terms of non-compliance avoidance. However, it remains necessary to test for effects on the selection of job seekers into a detected non-compliance. Such a test will be provided in section 5.2.

[18]Job seekers who never committed a detected non-compliance do not have any “actual” date of detection. For them, we calculate a month of “potential detection”: it is the month of the date of registration +30 days (as the median lag between registration and the first detection is 30 days).

Figure 3.1: Registered Non-Compliance Detections

(a) Number of Detections **(b)** Probability of Detection



The dotted vertical lines delimit the short-run sample window. The solid vertical line indicates the reform date. The treatment group (T=1) contains job seekers who receive a non-compliance notification in the treatment group (for not having submitted a job search protocol by the deadline). The control group (T=0) contains are job seekers who are notified about a non-compliance in the control group (other reasons of non-compliance). The underlying data sources and sampling choices are described in section 4.2. In panel (b), job seekers who never committed a detected non-compliance do not have any "actual" date of detection. For them, we calculate a month of "potential detection": it is the month of the date of registration +30 days (as the median lag between registration and the first detection is 30 days).

There are several practice-related reasons why job seekers did not anticipate the policy change in the short run: first, the reform aimed at reducing the bureaucratic burden of the enforcement regime and was therefore of a purely administrative nature. It was not considered as a true policy change and therefore not announced as such. Second, the final enforcement decision is not taken by the caseworkers themselves, but by a higher authority in the PES or canton. As a consequence, the caseworkers were not responsible for executing the policy change, which makes it less likely that they actively advised job seekers to change their compliance behavior around the reform date. Third, the change occurred within a larger reform package whose principal element was to reduce the potential duration of benefit payments for job seekers aged below 25. Compared to these reforms, the practice change in the enforcement rules was of minor nature. For instance, it did not appear in the presentation that was used to communicate the political reform package to caseworkers.[19]

Note that the political reform package does not confound with the policy change in enforcement strictness: the reform's most important element was a reduction in the potential benefit duration of job seekers aged below 25. In turn, the change in

[19]The only official channel in which caseworkers were informed about this change of enforcement practice was within the delivery of the updated collection of practice ordinances ("Kreisschreiben"); this collection features several hundred pages.

enforcement strictness affected job seeker depending on their type of non-compliance and independent of their age. Therefore, the treatment and control groups of the two natural experiments are independent of each other. We show that results are robust to the exclusion of individuals aged below 25 (c.f. section 6.4).

Features of the Enforcement Process Table 3.1 shows the distribution of non-compliance reasons in the short-run estimation sample before and after the policy change.[20] The treatment group constitutes about 10% of the sample. Within the control group, the most common type of notification refers to insufficient search effort before the first meeting with the caseworker. Job seekers are obliged to actively search for a job as soon as they learn about their unemployment. Non-compliances with this obligation mechanically dominate the distribution of first notifications, as they are registered at the first caseworker meeting, i.e., about three weeks after registration. Other common types of non-compliance are insufficient search effort and the delay or absence at a scheduled meetings with the caseworker.

Table 3.1: Non-Compliance Notifications Before and After the Policy Change (Short Run Window: Four Pre- and Four Post-Months)

Reason of Non-Compliance Notification	N_{pre}	% of sample pre	N_{post}	% of sample post
Search protocol not submitted by deadline (T=1)	1015	10.73%	637	9.42%
Other Reasons (T=0):	8443	89.27%	6123	90.58%
- Insufficient search effort before registration	5609	59.30%	4256	62.96%
- Protocol submitted, but insufficient effort	1352	14.29%	719	10.64%
- Delay or absence at caseworker meeting	1164	12.31%	868	12.84%
- Other	170	1.80%	160	2.37%
Total	**9458**		**6760**	

N=16218. "Other" contains the non-participation at an active labor market program or the failure to comply with orders made by the PES. "Pre/post" refers to non-compliance notifications registered before/after the reform date.

Table 3.2 shows the main features of the enforcement process in the four pre- and four post-reform months. It reports simple difference-in-differences (in bolt) for the average sanction probability, the average number of days to notification, the average

[20]The assignment is based on the first non-compliance notification event. 39% of non-compliant job seekers receive more than one notification during the unemployment spell. It is however likely that the first experience of an enforcement process is the most important one.

number of days from notification to sanction in case of enforcement and the average days of benefit cuts imposed in the case of a sanction.

Clearly, the only substantial change concerns the probability of non-compliers to be sanctioned. While this probability stayed constant in the control group, it increased from .285 to .673 in the treatment group. There is a small positive difference-in-differences in the number of days between entry and the first notification. The econometric framework will take this into account by controlling for the duration to notification. The amount of the imposed sanction slightly decreases after the change, by .7 days of UI benefits. The duration from notification to sanction in the case of enforcement remained stable.

Table 3.2: Features of the Enforcement Process (Short Run Window: Four Pre- and Four Post-Months)

		Before	After	Difference
P(Sanction)	T=1	0.292	0.672	0.380
	T=0	0.660	0.683	0.022
	Difference	-0.369	-0.011	**0.358**
Days to Notification	T=1	63.492	65.656	2.165
	T=0	35.061	32.316	-2.745
	Difference	28.431	33.340	**4.909**
Days Notification to Sanction	T=1	18.644	20.317	2.498
	T=0	19.567	21.142	0.751
	Difference	-0.923	-0.825	**0.098**
Amount of Sanction (days)	T=1	6.880	6.157	-0.723
	T=0	7.141	7.094	-0.047
	Difference	-0.260	-0.936	**-0.676**

N=16218. The bolt numbers are the difference-in-differences in the respective parameter. The amount of benefit sanction and the number of days between notification and sanction are computed based on the unmerged unemployment insurance register data, as they are available with less precision in the merged data.

3.5 Empirical Analysis

This section first presents the econometric framework and then tests for reform effects on the selection of non-compliant job seekers.

3.5.1 Econometric Framework

In the following, we describe the econometric framework used to estimate how non-compliers react to the increased enforcement strictness. We first set up a basic difference-in-differences (D-i-D) specification, which we use to estimate the short-run reform effect (four pre- and post-reform months). We then specify a dynamic D-i-D, which we apply when including additional time periods into the estimation sample.

Basic Difference-in Differences The difference-in-differences (D-i-D) specification compares the short-run pre-post difference in outcomes of treated job seekers to the one of job seekers in the control group. In this framework, the outcome y of job seeker i is specified as follows:

$$y_i = \delta \; (post_t \times T_i) + \gamma \; T_i + \eta_{t,c} + \pi \; \tau_i + x_i'\beta + u_i \tag{3.1}$$

The D-i-D term $post_t \times T_i$ takes the value one if job seeker i's first non-compliance notification was affected by the enforcement policy change. This is the case if the non-compliance refers to the failure of submitting a search protocol by the deadline ($T_i = 1$) and if it was registered after April 2011 ($post_t = 1$).[21] The coefficient of interest δ thus measures the effect of the policy change.

T_i and $\eta_{t,s}$ contain the D-i-D second order terms. T_i controls for time-constant differences between the treatment and the control group. The control group consists of job seekers who became non-compliant for another reason than the treatment group (c.f. section 4.2). $\eta_{t,c}$ is a set of interacted fixed effects between the 14 cantons (state) and the calendar month of notification. It controls for group-constant time effects and allows these two vary at the cantonal level. The motivation for interacting month and canton fixed effects is that seasonalities vary largely across regions. Further, the cantonal authorities implement the enforcement process. The dummy $post_t$ is collinear with $\eta_{t,c}$ and therefore omitted. τ_i contains the duration in days between job seeker i's entry into unemployment and her date of notification. It addresses that individuals in the treatment group had a slightly longer duration to notification after the reform (c.f. section 4.2). Results are robust to specifying τ_i in a non-linear way

[21] The reform started to become effective for protocols that referred to the job seeker's activities in the month of April. All protocols registered as not submitted after April were thus affected.

(c.f. section 6.4).

x_i includes an extensive set of individual covariates. Summary statistics on covariates are reported in appendix table 3.B.1.

Dynamic Difference-in Differences We now set up a dynamic specification, extending the sample to notifications issued between January 2010 and April 2012. We thereby assess whether outcomes in the treatment and control group evolved similarly prior to the reform (common trend assumption). In addition, we show effects beyond the short-run post-reform horizon.

The dynamic D-i-D takes the following form:

$$y_i = \sum_{\kappa=-3}^{3} \delta_\kappa \times T_i \times \mathbb{1}(period_\kappa) + \gamma \; T_i + \eta_{t,c} + \pi \; \tau_i + x_i'\beta + u_i \tag{3.2}$$

In this specification, the treatment group T_i is interacted with a set of dummies for the different four months periods κ. κ is normalized to zero in the pre-reform period, January to April 2011. δ_κ thus measures whether the difference in outcomes between treatment and control group is different in period κ than in the period January to April 2011. The baseline effect of κ is collinear with the month $\times$ canton fixed effects, $\eta_{t,c}$, and therefore omitted. All other terms are as in the D-i-D specification.

Further Estimation Details When estimating effects on the duration to un- and non-employment exit, we specify the proportional hazard θ^e as (D-i-D framework):

$$ln \; \theta^e = ln \; \lambda(t_e) + \delta \; (post_t \times T_i) + \gamma \; T_i + \eta_{t,c} + \pi \; \tau_i + x_i'\beta \tag{3.3}$$

Duration dependence takes a non-parametric form, expressed through the step function:

$$\lambda(t_e) = exp(\sum_k (\lambda(t_{e,k}) I_k(t))$$

where k(= 1, . . . , K) is a subscript for the time intervals and $I_k(t)$ are time-varying dummy variables for subsequent intervals. $\lambda(t_{e,k})$ contain thus the piece-wise constant levels of the baseline hazard. When we right-censor the duration of unemployment after 6 months, we distinguish the following time intervals: 1-2 months, 2-3 months, 3-4 months, 4-5 months, 5-6 months and 6-12 months. As we estimate a constant term, we normalize $\lambda(t_{e,1})$ to be 0. The other terms of the equation are as in the linear estimation framework. The proportional hazard of the dynamic specification takes the equivalent form.

3.5.2 Did the Reform Change the Composition of Non-Compliers?

The econometric framework aims at identifying the effects of a surprisingly strict response to a non-compliance. This requires that job seekers did not anticipate the policy change prior to non-compliance. In the case the reform induces anticipatory behavior, this potentially affects the decision to become non-compliant (c.f. the discussion in section 3). In the descriptive analysis of section 4, we showed that the reform did not come along with any major change in the overall probability of non-compliance. Nevertheless, anticipation effects can change the selection of individuals choosing to become non-compliant.

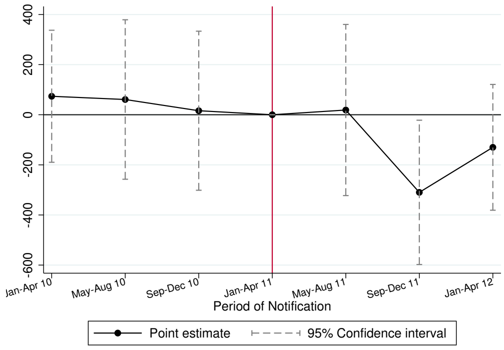
In the following, we test whether the composition of non-compliant individuals changes in the short and medium run after the reform. To this end, we run the dynamic D-i-D specification (equation 3.2), using pre-unemployment wages reported in the social security data as outcomes.[22] Covariates x_i are excluded from the regression. Figure 3.1 presents the results. In panels (a) and (b), the outcomes are average monthly wages obtained during months -1 to -12 and months -13 to -24 prior to unemployment entry, respectively.[23] In panels (c) and (d), the outcomes are average log wages over the same periods. The pre-reform period of January to April 2011 is the baseline period. In the three preceding periods, the wage profiles of non-compliant individuals do not evolve differently in the treatment and control group. Similarly, there are no significant difference-in-differences in the first post-reform period May to August 2011, which we use in the short-run analysis. In the periods thereafter the differences in pre-unemployment wages however diverge: Compared to the pre-reform period, job seekers in the treatment group earned significantly less than job seekers in the control group.

[22]We use pre-unemployment wages because they are the most comprehensive proxy of the job seeker's productivity observed in the data.

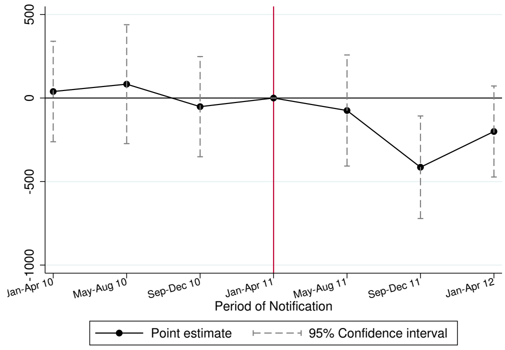
[23]If pre-unemployment wages are missing in the social security data (5.2% of observations), we replace them by the variable reporting insured monthly earnings in the UI registers.

Figure 3.1: Test for Compositional Changes: Dynamic D-i-D on Pre-UE Wages

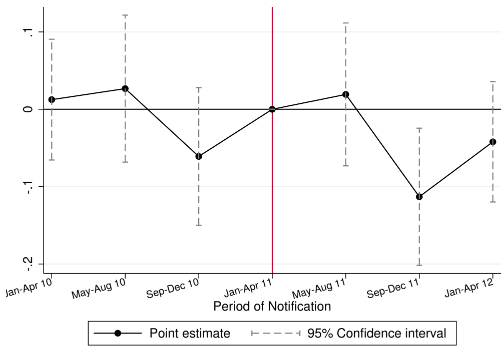
(a) Pre-UE Monthly Wage, Average over Months -1 to -12



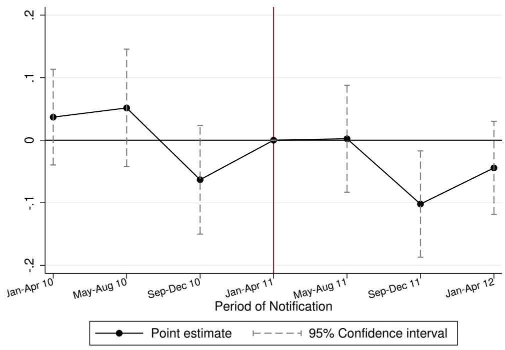
(b) Pre-UE Monthly Wage, Average over -13 to -24



(c) Pre-UE Monthly Log Wage, Average over Months -1 to -12



(d) Pre-UE Monthly Log Wage, Average over Months -13 to -24



Graphs report results of the dynamic D-i-D design specified by equation 3.2. The pre-unemployment wage over period t is computed as the average monthly wage reported in the social security data over period t. Reported coefficients correspond to the vector $\hat{\delta}_\kappa$ of equation 3.2. The baseline period are the four pre-reform months January to April 2011. The solid vertical line indicates the reform date. Regressions are estimated using OLS. They include all fixed effects and exclude covariates.

This picture suggests that shortly after the reform, there was no anticipation which caused a change in the selection of non-compliant job seekers. In the mid run, the policy change induced a negative selection effect: individuals with higher wages profiles became relatively less non-compliant. One possible interpretation is that job seekers with higher wage profiles are more able to anticipate enforcement strictness and to adapt their behavior accordingly.

In the following, we use the short-run sample to estimate the causal effect of a surprising increase in enforcement on job search behavior.[24] In the dynamic D-i-D specifications, we also show medium-run effects, which we interpret as the joint result of compositional changes and the behavioral reform effect.

[24] Appendix table 3.B.2 shows further evidence that the composition of job seekers remained stable during this window, by running the basic D-i-D framework on additional covariates.

3.6 Results

3.6.1 Exit from Unemployment

Short-Run D-i-D Table 3.1 reports how the quasi-experimental change in enforcement strictness affected the exit from unemployment and non-employment. Estimates are based on equation 3.1. In column 1, results show that the probability to exit unemployment within 6 months increases by 6.9 percentage points (12% relative to the mean). The coefficient remains unchanged when additionally controlling for individual covariates (column 2).[25] This confirms that there are no changes in the composition of non-compliant job seekers which influence the results.

Columns 3 to 4 decompose exits within 6 months into exits to employment versus unpaid job search. An exit to employment is coded as one if the job seeker's social security records report positive employment earnings during at least one of the two months following unemployment exit. If individuals exit unemployment without employment earnings, but return to employment within the observation window,[26] they are coded as entering unpaid job search. As these individuals eventually re-enter employment, we assume that they continue searching without benefit receipt and thus become only temporary non-participants. Results show that the effect on exit from unemployment does not translate one-to-one into an effect on job finding. As we lose statistical power when splitting the outcome, the coefficient on exits to employment is only at the margin to significance (column 3). It suggests that individuals are 4 percentage points more likely to find a job. Strikingly, column 4 shows that individuals are significant 3 percentage points more likely to exit to unpaid job search, which corresponds to an increase of 50% relative to the mean. This suggests that for some indviduals, the strengthened enforcement regime decreased the reservation value below the utility of job search without UI benefits (c.f. Frijters and der Klaauw, 2006).[27] Columns 5 to 6 show that this reaction results in diverging effects on the overall duration of unemployment versus non-employment.[28] The unemployment exit

[25] Summary statistics on covariates are reported in table 3.B.1.

[26] For 98.4%, we observe post-unemployment job and earnings paths up to at least 18 months after unemployment exit. The other 1.6% are censored before.

[27] Frijters and der Klaauw (2006) set up a job search model and show that transitions into nonparticipation occur when the reservation value drops below the utility of being non-participant.

[28] Unemployment and non-employment spells are censored at 520 days, as this is the maximum potential UI benefit duration in the estimation sample.

hazard increases by 12% (=exp(0.111)-1), which corresponds to an average reduction in the unemployment duration of 16 days. However, we find no significant effect on the overall non-employment duration. The shorter time spent in UI does thus not translate into more time in employment, but rather into more time in unpaid job search. Column 7 confirms this picture by showing that the probability of searching without benefits for more than six months after exit from unemployment increases by 3.2 percentage points (30% relative to the mean).

The fact that individuals frequently move between unemployment and nonparticipation has been pointed out by several previous studies (e.g. Flinn and Heckman, 1983; Elsby, Michaels and Solon, 2009; Kroft et al., 2016). Rothstein (2011) and Farber and Valletta (2015) show that UI benefit extensions reduce exits from unemployment to nonparticipation. Our results show that unpleasant policy choices in UI can induce individuals to become temporary nonparticipants even before benefit exhaustion.

The effects on un- and non-employment duration will be further quantified in a simulation exercise presented in section 6.5.

Dynamic D-i-D We now present results from the dynamic D-i-D specification for the main outcomes, to assess the common pre-trend assumption and to show how outcomes evolve in the medium run. To this end, we extend the sample by including job seekers who received a notification between January 2010 and April 2012. Figure 3.1 shows the results.

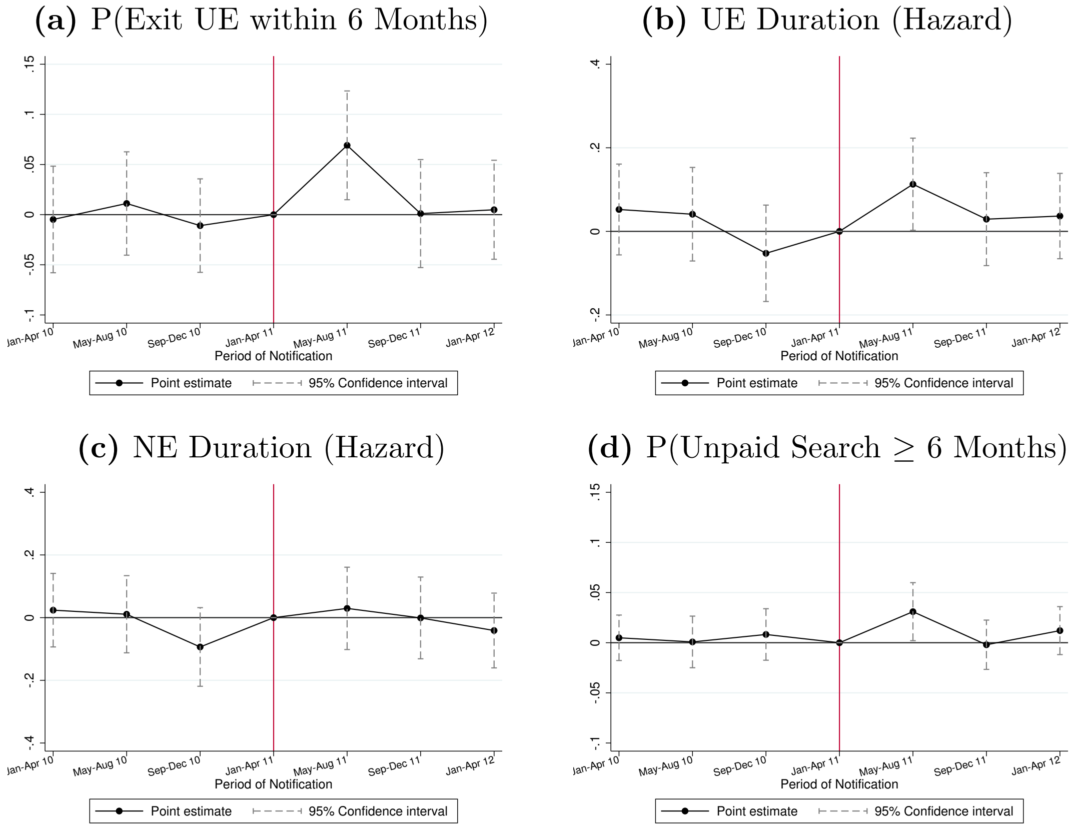
In section 5.2 (Figure 3.1), we provided evidence that the policy change induced a change in the composition of non-compliant job seekers in the medium run. This also reflects in the results. The short run increase in the exit from unemployment does not persist in the medium run. It appears that the negative selection effect counteracts the causal effect of an increased enforcement strictness. As a consequence, we observe a zero net effect. The figures further document the absence of any significant divergence in pre-reform trends of the treatment and control group.

Table 3.1: Effects on the Exit from Un- and Non-Employment

	P(Exit within 6 Months)				Duration (Hazard)		P(Unpaid Search
	All		To Job	To Unpaid Search	UE	NE	≥ 6 Months)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D-i-D	0.069**	0.069**	0.040	0.031**	0.111**	0.025	0.032**
	(0.028)	(0.028)	(0.030)	(0.014)	(0.056)	(0.066)	(0.015)
T=1	0.024	0.005	0.020	-0.013	0.021	0.051	-0.008
	(0.018)	(0.018)	(0.022)	(0.009)	(0.039)	(0.045)	(0.008)
Covariates	No	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.595	0.595	0.516	0.065			0.090
Exits					14657	13132	
N	16218	16218	16218	16218	16218	16218	16218

In columns 1 to 4 and 4, regressions estimate equation 3.1 using OLS. In columns 5 and 6, regressions estimate equation 3.3 using Maximum Likelihood. Regressions are based on the short-run estimation sample, containing notifications registered four month before to four month after the reform. Summary statistics on covariates are reported in table 3.B.1. The coefficient reported in the first line is the estimated difference-in-differences parameter, $\hat{\delta}$. The coefficient reported in the second line is the estimated baseline effect of being in the treatment group, $\hat{\gamma}$. In column 3, the outcome is coded as one if a job seeker exits within six months and has positive employment earnings during at least one of the first two months following unemployment exit. In column 4, the outcome is coded as one if a job seeker exits unemployment without employment earnings, but returns to employment within the observation period. In columns 5 and 6, outcomes are the unemployment (UE) and non-employment (NE) exit hazard, respectively. Duration dependence is specified as a piece-wise constant (c.f. equation 3.3). UE and NE spells are censored at 520 days, as this is the maximum potential UI benefit duration in the estimation sample. In column 7, the outcome is the probability that the job seeker continues job search without benefits for more than six months after exit from UE. Further estimation details can be found in section 5. Standard errors (in brackets) are clustered at the canton-month level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

Figure 3.1: Effects on the Exit from Un- and Non-Employment, Dynamic D-i-D



Graphs report results of the dynamic D-i-D design specified by equation 3.2. Information on the outcomes can be found in the notes of table 3.1. The reported coefficients correspond to the vector $\hat{\delta}_{\kappa}$. The baseline period are the four pre-reform months January to April 2011. The solid vertical line indicates the reform date. Regressions are estimated using OLS (panels a and d) or Maximum Likelihood (panels b and c). They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history. Summary statistics on covariates are reported in table 3.B.1. Standard errors are clustered at the canton-month level.

3.6.2 Job Quality

In the following, we analyze whether the increase in enforcement strictness affected the quality of post-unemployment jobs. Job search theory makes ambiguous predictions on potential wage effects (c.f. section 3): on the one hand, an increased sanction probability lowers the reservation value of non-compliant job seekers and can thereby raise the willingness to accept lower wages. On the other hand, it can alleviate the depreciation of wage offers by reducing the duration of unemployment (see, e.g., the discussions by Schmieder, Von Wachter and Bender, 2016; Nekoei and Weber, 2017).

Short-Run D-i-D In columns 1 to 3 of table 3.2, we report effects on the average log monthly wage received during the 12 months following unemployment.[29] Column 1 presents results from regressions without covariates. In column 2, we add covariates and in column 3, we additionally control for the duration spent in unemployment through a vector of dummies for each 10-days category. In all three columns, point estimates are negative, but statically not different from zero. The same holds true when we consider the difference between the pre- and the post unemployment average monthly log wage in columns 4 and 5.[30] The negative point estimates increase in size, but remain insignificant. Altogether, the wage effects are estimated with a large degree of statistical imprecision, which doesn't allow for the identification of a significant wage effect. The point estimates are, however, consistently below zero (and a positive wage effect is statistically largely improbable). This provides tentative evidence that enforcement strictness has negative impacts on the post-unemployment wage situation.

Finally, columns 6 to 7 report that there is no effect on the linear duration until recurrence into unemployment.[31]

[29] To compute this outcome, the total amount of earnings from employment during the first year after unemployment is divided by the number of months in employment during that period. We exclude the first month after unemployment from the calculations, as the reporting of the end of the unemployment spell may differ between the UI and the social security data. Results are robust to including the first month after exit (available upon request). Job seekers reporting no positive wages during the first twelve months of unemployment are excluded from the regressions (N=2057).

[30] The average pre-unemployment log wage is computed over the first 12 months before entry into unemployment, excluding the last month prior to entry. If this variable is missing (5.2% of observations), it is replaced by the variable reporting insured monthly earnings in the UI registers. In regressions on the difference in log wages, controls for the pre-unemployment wage are excluded.

[31] The duration to recurrence is computed as the number of days between an individual's exit from unemployment and her next entry into unemployment. It is capped at 360 days.

Table 3.2: Effects on Post-UE Job Quality

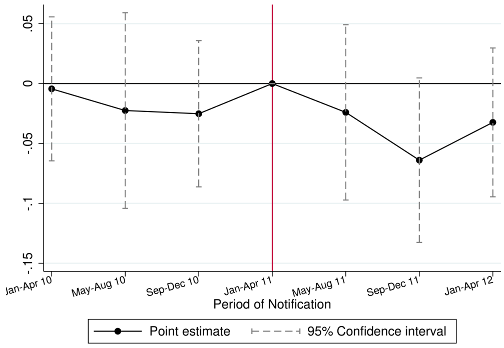
	Log Monthly Wage			Diff. in Log Wages		Duration to Recurrence	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D-i-D	-0.037	-0.019	-0.028	-0.064	-0.073	-0.906	-0.453
	(0.045)	(0.038)	(0.038)	(0.049)	(0.050)	(4.584)	(4.542)
T=1	0.037	0.028	0.023	0.049	0.045	0.201	0.711
	(0.028)	(0.022)	(0.021)	(0.031)	(0.030)	(3.089)	(2.956)
Outcome Mean	8.129	8.129	8.129	-0.037	-0.037	303.688	303.688
Covariates	No	No	Yes	No	Yes	Yes	Yes
UE Duration	No	Yes	Yes	No	Yes	No	Yes
N	14161	14161	14161	14161	14161	16218	16218

In columns 1 to 3, the log wage is computed as the log of the average monthly wage obtained during the 12 months after exit from unemployment. Individuals who are never employed during this period are excluded. In columns 4 to 5, the pre-unemployment log wage is computed as the log of the average monthly wage obtained during the 12 months prior to unemployment. In columns 7 to 8, the outcome is the linear duration until recurrence to unemployment insurance, which is capped at 360 days. Regressions estimate equation 3.1 using OLS. They are based on the short-run estimation sample, containing notifications registered four month before to four month after the reform. Summary statistics on covariates are reported in table 3.B.1. In columns 4 and 5, the pre-unemployment wage is excluded from the vector of covariates. Controls for the duration of unemployment in columns 3, 5 and 7 are specified as dummies including 10-day categories. Further estimation details can be found in section 5. Standard errors (in brackets) are clustered at the canton-month level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

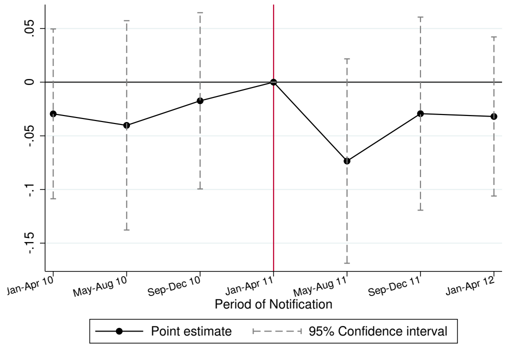
Dynamic D-i-D The results from the dynamic specification (figure 3.3) confirm that there is a large degree of statistical imprecision in the estimation of the wage effects (panels a and b). Panel a reflects the negative mid-run selection effect reported in section 5.2, as it reports a negative coefficient on wages in the medium run after the reform. In panel b, the selection effect is alleviated, as the outcome is the difference between post- and pre-unemployment wages. There is no significant effect on the duration to recurrence in any of the post-reform periods (panel c). All panels confirm the absence of diverging outcome trends between the treatment and the reform group before the reform.

Figure 3.3: Post-UE Job Quality, Dynamic D-i-D

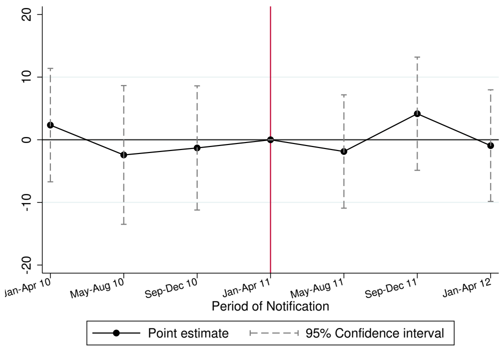
(a) Log Wage



(b) Difference in Log Wages



(c) Duration to Recurrence



Graphs report results of the dynamic D-i-D design specified by equation 3.2. Information on the outcomes can be found in the notes of table 3.1. The reported coefficients correspond to the vector $\hat{\delta}_{\kappa}$. The baseline period four months period January to April 2011. The solid vertical line indicates the reform date. Regressions are estimated using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a pre-unemployment wages. Summary statistics on covariates are reported in table 3.B.1. Further, regressions control for the duration of unemployment in the form of dummies including 10-day categories (as in columns 3, 5 and 7 of table 3.2). Standard errors are clustered at the canton-month level.

3.6.3 Subgroup Analysis

In the following, we analyze the effects of an increased enforcement strictness by subgroups. In a first step, we test how the effects differ between cantons with high versus low pre-reform enforcement strictness. In a second step, we assess how different types of job seekers responded to the change.

Canton-Level Treatment Intensity

Prior to the policy change, the cantons had different levels of initial enforcement strictness. As a consequence, the "bite" of the policy change differs across cantons. We classify the sample into low- and high- intensity cantons, depending on whether the average sanction probability was higher or lower than 0.4 over the four months prior to the reform.[32]

Table 3.3: Subgroup Analysis: Canton-Level Pre-Reform Enforcement Probability (p_{pre})

		$p_{pre} \geq .4$ (low treatment intensity) (1)	$p_{pre} < .4$ (high treatment intensity) (2)
P(Exit w/in 6 Months)	D-i-D	0.017	0.087***
		(0.057)	(0.033)
	Mean	0.594	0.596
	N	7205	9013
Log Wage	D-i-D	0.010	-0.069
(Avg. Over 12 Months)		(0.082)	(0.049)
	Mean	8.144	8.117
	N	6324	7837
Duration to Recurrence	D-i-D	2.089	-2.858
		(8.489)	(5.574)
	Mean	307.657	300.516
	N	7205	9013

The canton-level pre-reform enforcement probability is computed as the average enforcement probability among individuals in the treatment group, during the four months prior to the policy change. Cantons where the sanction probability was $\geq$ 0.4 before the policy change are classified into the low treatment intensity (average increase in enforcement probability of 0.17). Cantons where the sanction probability was $<$ 0.4 before the policy change are classified into the high treatment intensity (average increase in enforcement probability of 0.34). All regressions estimate equation 3.1 using OLS. They are based on the short-run estimation sample, containing notifications registered four month before to four month after the reform. Summary statistics on covariates are reported in table 3.B.1. Regressions on monthly log wages and on the duration to recurrence additionally control for the duration of unemployment (using dummies with 10 day intervals). Further details on the outcome variables are provided in the notes of tables 3.1 and 3.2. Standard errors are clustered at the canton-month level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 3.3 reports heterogeneous effects on the probability to exit unemployment within six months, on log wages and on the duration to recurrence. There is no significant effect in cantons with a high level of pre-reform strictness (column 1). Effects on unemployment exit appear to be driven by cantons where the pre-reform strictness was relatively low (column 2). In these cantons, the probability to exit within six months increases by 8.2 percentage points (15% relative to the mean). Although point estimates on wages are stronger for cantons with a higher treatment

[32] For each canton, we compute the pre-reform sanction probability as the share of individuals in the treatment group who received a sanction after being detected in January to April 2011. In cantons where the probability was already higher or equal than 0.4 before the reform, the average increase in sanction probability is of 17 percentage points. In cantons with a pre-reform probability of less than 0.4, it is of 34 percentage points.

intensity, they remain statistically insignificant. For both groups, there are no effects on the recurrence to unemployment.[33]

Job Seeker Characteristics

Table 3.4 shows D-i-D coefficients on the main outcomes by gender, pre-unemployment wages and the (out-of-sample) predicted probability to exit unemployment within six months.

From columns 1 and 2, it appears that female job seekers show stronger reactions in their exit from unemployment. Sample sizes are, however, too small to conclude on statistically significant differences. Columns 3 to 4 further suggest that effects on exit are stronger for individuals with lower pre-unemployment earnings (difference at the margin to significance). Columns 1 to 4 report no effects on the job quality in any of the four groups.

In columns 5 and 6, we split the sample by the median (out-of-sample) predicted probability to exit unemployment within six months.[34] The effects on unemployment exit do not differ between the two groups. However, negative point estimates on post-unemployment wages (conditional on unemployment duration) are stronger and statistically significant at the 10% level for individuals with a higher ex-ante exit probability. The estimate suggests that these job seekers experience a loss of 7.8% in their average monthly wage obtained during the 12 months after unemployment. It appears that job seekers with a higher propensity to exit unemployment fast are more prone to reduce their reservation wage after learning about a high level of enforcement strictness in UI.

[33]The causal interpretation of the e heterogeneity results relies on the assumption that the controls for covariates appropriately take into account compositional differences between the job seeker populations by canton. Due to the very rich set of individual covariates, including pre-unemployment earnings, we believe that this assumption holds.

[34]To construct this measure, we first regress the probability to exit within six months on the job seeker covariates reported in table 3.B.1, using the sample of job seekers receiving a notification between January and August 2010. We then predict the outcome for job seekers in the main sample (January to August 2011), using the coefficients from this regression.

Table 3.4: Subgroup Analysis: Job Seeker Characteristics

		Gender		Previous Earnings		$\widehat{P}(Exit)$	
		Male	Female	< 4000	≥ 4000	> Median	≤ Median
		(1)	(2)	(3)	(4)	(5)	(6)
P(Exit w/in 6 Months)	D-i-D	0.055	0.102**	0.105***	0.030	0.068*	0.059
		(0.034)	(0.046)	(0.031)	(0.047)	(0.038)	(0.039)
	Mean	0.621	0.551	0.588	0.602	0.716	0.468
	N	10203	6015	8566	7652	8296	7922
Log Wage	D-i-D	0.003	-0.080	-0.016	-0.026	-0.078*	-0.004
(Avg. Over 12 Months)		(0.049)	(0.072)	(0.058)	(0.043)	(0.044)	(0.071)
	Mean	8.213	7.984	7.884	8.393	8.187	8.061
	N	8946	5215	7359	6802	7618	6543
Duration to Recurrence	D-i-D	3.333	-12.531	5.935	-10.850	1.162	1.022
		(5.814)	(9.307)	(7.185)	(7.474)	(6.781)	(7.620)
	Mean	299.782	310.314	298.881	309.070	299.775	307.786
	N	10203	6015	8566	7652	8296	7922

All regressions estimate equation 3.1 using OLS. They are based on the short-run estimation sample, containing notifications registered four month before to four month after the reform. Summary statistics on covariates are reported in table 3.B.1. Regressions on monthly log wages and on the duration to recurrence additionally control for the duration of unemployment (using dummies with 10 day intervals). Further details on the outcome variables are provided in the notes of tables 3.1 and 3.2. To measure a job seeker's predicted exit probability in columns 5 to 6, we first regress the probability to exit within six months on the covariates reported in table 3.B.1, using the sample of job seekers receiving a notification between January and August 2010. We then predict the outcome for job seekers in the main sample (January to August 2011), using the coefficients from this regression. Standard errors are clustered at the canton-month level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

3.6.4 Robustness Analysis

Before turning to the simulation exercise that quantifies the presented results, we test the robustness of the estimates to alternative specifications and sampling choices. The outcome of reference is the probability to exit unemployment within 6 months.[35]

In Table 3.5, column 1 recalls the baseline estimate. Column 2 replaces the linear control variable for the days until non-compliance detection by a set of dummies for the number of full weeks until detection. Column 3 extends the sample by including job seekers who experience their first detection up to 150 days after the start of their unemployment spell (instead of 120 in the baseline). The motivation to exclude job seekers whose notification occurred later than 120 days after entry into unemployment was to achieve a homogeneous sample of elapsed duration at the time of notification. Column 4 extends the sampling window to detections between December 2010 and September, 2011 (instead of January to August 2011 in the baseline). Column 5 excludes detections that referring to the non-compliance with job search requirements

[35] The robustness results hold for the other outcomes, which are omitted for space reasons. Documentation is available upon request.

from the control group, as these relate to the same general topic as notifications of the treatment group. Finally, we drop job seekers aged below 25 in column 6, as these job seekers experienced a change in their potential benefit duration in April 2011. None of the tests leads to significant changes in the estimated coefficients.

Table 3.5: Effects on the Probability to Exit UE Within 6 Months: Robustness Analysis

	Baseline	Weeks to Detection Dummies	Notifications <150 Days	Larger Time Window	Alternative Control Group	Aged > 25
	(1)	(2)	(3)	(4)	(5)	(6)
D-i-D	0.069**	0.068**	0.059**	0.060**	0.069**	0.073**
	(0.028)	(0.028)	(0.029)	(0.026)	(0.028)	(0.032)
T=1	0.005	0.002	0.019	0.006	0.007	0.005
	(0.018)	(0.018)	(0.018)	(0.015)	(0.019)	(0.019)
Covariates	Yes	Yes	YES	YES	YES	YES
Outcome Mean	0.595	0.595	0.581	0.590	0.594	0.556
N	16218	16218	16982	20571	15701	11435

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the canton-month level. Regressions estimate equation 3.1 using OLS. Summary statistics on covariates are reported in table 3.B.1. Column 1 recalls the baseline estimates. Column 2 adds dummies for the number of full weeks between entry into UE and the notification into the regression. Column 3 extends the sample to job seekers who received their first notification up to 150 days after the start of their unemployment spell (instead of 120). Column 4 extends the sampling window to notifications sent out between December 2010 and September 2011. Column 5 excludes notifications that refer to the compliance with job search requirements from the control group. Column 6 limits the sample to job seekers who are older than 25 years.

3.7 Conclusion

This dissertation chapter presents first quasi-experimental evidence on how enforcement strictness in UI affects labor market outcomes. The question is of high policy relevance because enforcement has become a commonly used instrument to reduce moral hazard. Through the design of the enforcement process, policy makers can target directly their degree of strictness towards non-compliant job seekers.

This study shows that an unanticipated increase in enforcement strictness by 10 percentage points increases the probability to exit unemployment within six months by almost 2 percentage points among non-compliant job seekers (3% relative to the mean). Remarkably, a substantial part of this effect is driven by exits to unpaid job search. As a consequence, we find diverging effects on the overall duration of unemployment versus non-employment. While the unemployment duration reduces by 3% per 10 p.p. of enforcement probability, we find no significant effect on the non-employment duration. This suggests that strengthened enforcement systematically raises the disutility of job search with benefit receipt. As a consequence, a larger share of job seekers prefers unpaid job search outside the UI system. This causes additional transitions into temporary nonparticipation prior to actual benefit exhaustion. This finding is in contrast to the first two chapters, where both instruments fastened unemployment exits through job finding. A likely explanation is that enforcement is very negatively perceived by concerned job seekers. For instance, individuals may feel treated in an unfair way, or in a way which threatens their self-esteem. Wage effects of the policy change show a negative sign but turn out to be insignificant, due to a large degree of statistical imprecision.

As an additional result, we find that the policy change modified the composition of non-compliant job seekers in the medium run. It appears that individuals with higher wage profiles refrain from becoming non-compliant. This demonstrates that, through anticipation effects, enforcement strictness can affect the non-compliance behavior of certain individuals.

The presented results improve the empirical basis for policy design in UI systems. This chapter delivers first evidence on the elasticity of job seeker behavior to changes in enforcement strictness. Enforcing job search obligations is a more targeted instrument for reducing moral hazard than changes in the overall benefit generosity, which affect compliers and non-compliers to the same extent. A natural next step for research on optimal UI design would be to compare the welfare implications of changing enforcement strictness versus adapting overall benefit generosity. The enforcement elasticities estimated in this study provide key ingredients for such a comparison.

Appendix 3.A Theory Appendix

Value Functions of Individuals with a Detected Non-Compliance
Closely related to prior work by Abbring, van den Berg and van Ours (2005) and Lalive, van Ours and Zweimueller (2005), we write the present discounted value of individuals with a detected non-compliance writes as:

$$\rho R_d = \max_{s_d} \left[b - c(s_d) + \lambda(s_d) \int_{\phi_d}^{\infty} (\frac{w}{\rho} - R_d) dF(w) + p_s(R_s - R_d) \right]. \qquad (3.4)$$

p_s is the probability of sanction conditional on detection. It is communicated to job seekers in state d through a written notification by the UI authority, informing about the enforcement process. If the sanction gets enforced, benefits are cut and the expected value of unemployment $R_s < R_d$ reduces to:

$$\rho R_s = \max_{s_s} \left[b - sanction - c(s_s) + \lambda(s_s) \int_{\phi_s}^{\infty} (\frac{w}{\rho} - R_s) dF(w) \right], \qquad (3.5)$$

where $sanction$ denotes the amount by which benefits are reduced in case of enforcement. [36]

In both equations, b denotes the unemployment benefit, s the search effort chosen by the job seeker, w the wage of the final job match and ϕ the reservation wage, which equals the present discounted value ρR in equilibrium. The job seeker chooses the search effort s by maximizing ρR. The choice of effort s thus depends on the marginal effort cost $c'(s)$ and the marginal benefit of effort, which is composed of an increase in the job arrival rate, $\lambda'(s)$, and the associated value differential between employment and unemployment, $\int_{\phi}^{\infty}(\frac{w}{\rho} - R)dF(w)$. Therefore, an increase in the sanction probability p_s and the reduction in present discounted value associated with the imposition of a sanction is expected to increase search effort and reduce reservation wages.

[36] Equation 3.5 implies the symplifying assumption that sanctions last forever. In reality, this is not the case. However, as the reform did not affect the amount and length of sanctions, the assumption does not affect our qualitative predictions.

Appendix 3.B Additional Tables

Table 3.B.1: Summary Statistics on Covariates

Variable	Mean	Std. Dev.	Min	Max	Obs
Female	0.371	0.483	0	1	16218
Age	32.889	9.819	20	55	16218
Age Squared	1178.078	701.907	400	3025	16218
Log total UE duration in past 3y	2.490	2.646	0	6.980	16218
Log Dur of longest UE spell in past 3 y	2.395	2.541	0	6.980	16218
Mother tongue ≠ regional language	0.441	0.497	0	1	16218
Experience (omitted baseline:>3 years):					
None	0.016	0.124	0	1	16218
< 1 Year	0.075	0.264	0	1	16218
1-3 Years	0.166	0.372	0	1	16218
Missing	0.452	0.498	0	1	16218
Civil status (omitted baseline: single):					
Married	0.336	0.472	0	1	16218
Widowed	0.094	0.291	0	1	16218
Level of Education (omitted baseline: apprenticeship):					
Minimum education	0.234493	0.423694	0	1	16218
Short further education	0.0624	0.241888	0	1	16218
High School	0.040079	0.19615	0	1	16218
University	0.099889	0.299861	0	1	16218
Missing	0.07504	0.263464	0	1	16218
Potential benefit duration (omitted baseline: 260-400 days):					
≤90 days	0.046	0.210	0	1	16218
>90, ≤ 200 days	0.161	0.367	0	1	16218
>200, ≤ 260 days	0.230	0.421	0	1	16218
=520 days	0.014	0.118	0	1	16218
Replacement rate (omitted baseline: 75%):					
70%	0.296082	0.456529	0	1	16218
71-74%	0.055288	0.228542	0	1	16218
75-79%	0.048082	0.213941	0	1	16218
Domain of occupation in last job (omitted baseline: admin and office):					
Food and agriculture	0.030	0.171	0	1	16218
Preparation of raw material	0.011	0.106	0	1	16218
Production (blue collar)	0.119	0.324	0	1	16218
Electro & watches	0.005	0.068	0	1	16218
Marketing and print	0.016	0.124	0	1	16218
Chemistry	0.004	0.065	0	1	16218
Engineering	0.017	0.128	0	1	16218
Informatics	0.024	0.152	0	1	16218
Construction	0.144	0.351	0	1	16218
Sales	0.111	0.314	0	1	16218
Tourism, transport, communication	0.045	0.207	0	1	16218
Banking, trust and insurance	0.014	0.118	0	1	16218
Restaurant	0.157	0.363	0	1	16218
Cleaning and personal service	0.042	0.201	0	1	16218
Management and HR	0.034	0.182	0	1	16218
Security and law	0.010	0.102	0	1	16218
Journalism and arts	0.014	0.118	0	1	16218
Social work	0.013	0.113	0	1	16218
Education	0.011	0.106	0	1	16218
Science	0.008	0.090	0	1	16218
Health	0.036	0.187	0	1	16218
Others (skilled)	0.067	0.249	0	1	16218
Missing	0.001	0.029	0	1	16218
Country of Nationality (omitted baseline: Switzerland):					
France or Italian	0.068	0.251	0	1	16218
Portugal, Spain or Greece	0.090	0.286	0	1	16218
Baltic States or Turkey	0.123	0.329	0	1	16218
nonEU Eastern Europe	0.008	0.088	0	1	16218
EU, U.S., Canada	0.091	0.288	0	1	16218
African countries	0.024	0.152	0	1	16218
Middle and South America	0.018	0.131	0	1	16218
Asian countries	0.027	0.162	0	1	16218
No of other household members (omitted baseline: none):					
1	0.168	0.374	0	1	16218
2	0.118	0.323	0	1	16218
3	0.038	0.192	0	1	16218
4+	0.012	0.110	0	1	16218
Log Previous Wage (-12 to -1)	8.155	0.712	0	12.158	16218
Log Previous Wage (-24 to -13)	8.089	0.754	0	11.411	16218

Table 3.B.2: Test for Compositional Changes (Short-Run): Additional Job Seeker Covariates

	Female	Age	Log Duration of Previous UE	Low Education	Married	Mother Tongue ≠ Regional Language	Non Swiss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D-i-D	0.012	-0.937	0.174	-0.015	0.016	0.001	0.031
	(0.030)	(0.588)	(0.146)	(0.024)	(0.024)	(0.025)	(0.025)
T=1	-0.038**	0.240	0.609***	-0.007	-0.007	-0.034**	-0.045**
	(0.016)	(0.442)	(0.090)	(0.016)	(0.015)	(0.015)	(0.018)
Outcome Mean	0.371	32.889	2.490	0.234	0.336	0.441	0.448
N	16218	16218	16218	16218	16218	16218	16218

Regressions estimate equation 3.1 using OLS, escluding covariates. They are based on the short-run estimation sample, containing notifications registered four month before to four month after the reform. The coefficient reported in the first line is the estimated difference-in-differences parameter, $\hat{\delta}$. The coefficient reported in the second line is the estimated baseline effect of being in the treatment group, $\hat{\gamma}$. Outcomes are a female dummy (column 1), the job seeker's age (column 2), the log total number of days in unemployment during the three previous years (column 3), a dummy which equals one if the job seeker only holds the obligatory level of schooling (column 4), a married dummy (column 5), a dummy which equals one if the job seeker's mother tongue is not the regional language (column 6), and a dummy for not holding the Swiss nationality (column 7). Standard errors (in brackets) are clustered at the canton-month level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

CHAPTER 4

Job Search with Subjective Wage Expectations*

This paper analyzes how subjective expectations about wage opportunities influence the job search decision. We exploit data on subjective wage expectations, which reveal that individuals over-estimate their future net re-employment wage by 10% on average. In particular, individuals do not anticipate that wage offers decline in value with elapsed time out of employment. We estimate a structural job search model in which subjective expectations about future wage offers are not constrained to be consistent with reality. Simulations show that wage optimism has highly dynamic effects on the path of search effort: right after entry into unemployment, optimism decreases job finding by around 15%. This effect weakens over the spell and eventually switches sign after about 8 months of unemployment. From then onward, optimism prevents unemployed individuals from becoming discouraged and thus increases search.

*This chapter is based on joint work with Sascha Drahs and Luke Haywood.

4.1 Introduction

Most welfare states support unemployed workers in their search for a job. This has been justified by the costs which unemployment imposes on both public finances and individuals' work careers. In particular, previous research has shown that prolonged unemployment decreases job finding prospects (Kroft, Lange and Notowidigdo, 2013; Eriksson and Rooth, 2014), as well as the quality of wage offers (Schmieder, Von Wachter and Bender, 2016). These studies provide strong evidence that the returns to job search decrease with elapsed unemployment. Less knowledge exists on how individuals perceive these returns, and how they react to them. Such knowledge is, however, important to effectively counsel and inform unemployed individuals. Given the wide use of counseling in modern welfare states, it is crucial to take into account beliefs held by unemployed individuals, and to assess how these beliefs affect job search behavior.

In this chapter, we focus on the expectations held by newly unemployed job seekers about their wage prospects. We show descriptively that job seekers tend to strongly over-estimate their future wage outcomes. We then use a structural dynamic job search model to analyze how this "wage optimism" affects the decision to search for work at different stages of the unemployment spell.

The data on subjective wage expectations stem from the "Linked IZA/IAB Evaluation Dataset", which contains both survey data and administrative records on labor market histories and outcomes. In line with previous evidence (e.g., by Schmieder, Von Wachter and Bender, 2016), we observe that re-employment wages decrease over the unemployment spell, relative to prior wages. In turn, subjective expectations are heavily anchored in past wages and do not take account of future reductions in the quality of wage offers. The average gap between initial expectations and actually realized re-employment wages amounts to 10% in net terms. Even after one year out of employment, most individuals do not update their expectations.[1] These patterns reveal that unemployed individuals do not recognize that they are searching in a highly dynamic environment.

To assess the consequences of wage optimism for job search, we introduce subjective

[1] Over-optimism by job-seekers regarding future wages is in line with the findings of Krueger and Mueller (2016) and Koenig, Manning and Petrongolo (2016), who find that workers persistently misjudge their prospects and set their reservation wage according to their previous wage.

wage expectations into a simple non-stationary job search model similar to Card, Chetty and Weber (2007) and Frijters and der Klaauw (2006). We model individuals as holding potentially incorrect beliefs about both the average value of wage offers and, importantly, about the evolution of wage offers over the unemployment spell. We identify these two key parameters using the data on subjective expectations about future wages. Additionally, we model three sources of dynamics in the job search environment: first, the quality of the average wage offer can evolve with the time out of employment. Second, search costs are allowed to change over time, capturing the phenomenon that job seekers may have increasing difficulty in generating job offers.[2] Third, UI benefits reduce to welfare benefits at the exhaustion of the job seeker's potential benefit duration. The model controls for observable heterogeneity in search costs and wage offers. Similar to Card, Chetty and Weber (2007) and DellaVigna et al. (2016), we focus on search effort instead of reservation wage dynamics and assume that every wage offer is accepted.[3] As the discount rate is not credibly identified in our setting, we provide a range of estimates for different choices of the discount rate.

We use the parameter estimates to simulate a scenario in which individuals are perfectly informed about the average evolution of wage offers. The simulation results show that wage optimism affects the trajectory of job search in a highly dynamic way. At first, the knowledge about falling wage prospects creates an incentive for unemployed workers to search more. Therefore, information increases job finding by around 15% during very early unemployment. This effect weakens over the spell and eventually switches sign after about 8 months of unemployment. From then onward, the information about worsened wage offers discourages search and thereby decreases job finding. Long-term unemployed individuals thus search less when being perfectly informed about the wage offers they face. As a result, wage optimism has ambiguous implications, as it discourages search at early stages of the spell, while encouraging it at later stages. This qualitative pattern persists for different choices of the discount rates, while the estimated size of the initial negative effect of optimism is stronger for lower discount rates. We further find that information has less of an encouraging effect for highly educated individuals, who appear already aware of the wage depreciation.

[2] See, e.g., Kroft, Lange and Notowidigdo (2013) and Eriksson and Rooth (2014) for evidence that the probability of a callback decreases over the unemployment spell.

[3] In line with this assumption, Schmieder, Von Wachter and Bender (2016) find that reservation wages of German job seekers are not binding, suggesting that they are not a meaningful driver of search dynamics.

Our results contribute to the understanding of job search behavior in a dynamic setting and under potentially non-rational expectations. A growing literature analyzes job search under alternative behavioral assumptions than those made by standard models. For instance, Della Vigna and Paserman (2005) and Paserman (2008) study job search with non-exponential discounting. DellaVigna et al. (2016) introduce reference-dependent preferences into a job search model and make the case for a benefit schedule which decreases in several steps. Caliendo, Cobb-Clark and Uhlendorff (2015) analyze job search strategies when the subjective job offer arrival rate depends on the locus of control. They find that a more internal locus of control is associated with higher job search effort. Spinnewijn (2015) shows descriptively that job seekers in the U.S. are overly optimistic regarding their wage prospects. He presents theoretical evidence that optimal unemployment insurance design is affected by the presence of biased beliefs. Altmann et al. (2017) show that the provision of information to unemployed individuals via a brochure increases job finding among individuals with low-re-employment prospects. We add to this literature by providing a first structural analysis of dynamic job search behavior under subjective wage expectations: based on our simple and estimable model, we trace search choices over the unemployment spell and contrast choices made by over-optimistic agents to choices made by perfectly rational agents.

The remainder of the chapter is structured as follows: Section 4.2 presents a search model with subjective expectations about the wage offer distribution. Section 4.3 describes the matched administrative and survey data. Section 4.4 provides reduced form evidence on wage expectations and outcomes. We describe the structural estimation in section 4.5 and results in section 4.6. section 4.7 concludes.

4.2 Model

We set up a discrete-time non-stationary job search model similar to Card, Chetty and Weber (2007), where job seekers choose the level of search intensity in each period of time. Since we confirm previous findings that wage offers decline in value over the unemployment spell, the non-stationarity of the model is central to analyzing subjective wage expectations in the context of job search. We first present a rational-expectations version of the model and then introduce the possibility of diverging subjective expectations.

In each period of time t, a worker is either employed at a job paying wage w, or unemployed with unemployment benefits b_t. At the beginning of each period, job seekers determine their level of search intensity s_t, and thereby their per-period job finding hazard such that $s_t \in [0,1]$. Upon finding a job, workers remain employed for the entire future. As in Card, Chetty and Weber (2007) and DellaVigna et al. (2016), we focus on search effort instead of reservation wage dynamics and assume that every wage offer is accepted.[4] We discuss alternative assumptions on reservation wages below.

Value Functions The resulting value functions are given by:

$$V(w) = \frac{1+r}{r} u(w), \tag{4.1}$$

$$U_t = u(b_t) + \frac{J_{t+1}}{1+r}, \text{ and} \tag{4.2}$$

$$J_t = \max_{s\in[0,1]} s\mathrm{E}_{F_T} V(w) + (1-s)U_t - c_t(s), \tag{4.3}$$

where u denotes the utility of consumption, r is the time discount rate, c_t is the strictly convex effort cost function in period t, and F_t is the wage distribution of job offer arrivals in period t. In this problem, the job seeker's optimal search policy is the path $(s_t)_{t=1}^{\infty}$. It is determined by the first-order conditions

$$c_t'(s_t) = \mathrm{E}_{F_t} V(w) - U_t, \tag{4.4}$$

for $t = 1, \ldots$. Assuming that the job search environment becomes stationary after T time periods, we then get the stationary solution

$$U_T = \frac{1+r}{s_T + r} u(b_T) + \frac{1}{s_T + r}[s_T \mathrm{E}_{F_t} V(w) - c_T(s_T)], \tag{4.5}$$

$$c_T'(s_T) = \mathrm{E}_{F_t} V(w) - U_T, \tag{4.6}$$

such that a complete solution of the model is obtained by backward induction.

Subjective Wage Expectations The model of job search allows for non-stationarity in the benefit level b_t, the cost of effort c_t and in the wage offer dis-

[4] Schmieder, Von Wachter and Bender (2016) find that reservation wages of German job seekers are not binding, suggesting that they are not a meaningful driver of search dynamics.

tribution F_t. As shown by the first-order conditions (4.4), the wage offer distribution enters the optimal search decision both directly through the value of finding a job today, $\mathrm{E}_{F_t} V(w)$, and indirectly through the value of finding a job in the future. Higher future wages make current unemployment more attractive and lead to a lower optimal search effort. Therefore, the perceived evolution of the wage offer distribution matters for search behavior. In what follows, we distinguish the objective (true) from the subjective wage offer distributions, the latter being denoted by F_t^{sub} $(t = 1, \ldots)$.

Definition of Wage Optimism Denote by $(F_t)_{t=1}^{\infty}$ the *true* sequence of cumulative wage offer distributions for a job seeker, and allow the job seeker to face the job search problem with *subjective* wage expectations $(F_t^{sub})_{t=1}^{\infty}$. We call a job seeker *wage optimistic* if $F_t^{sub} \succ F_t$ for all $t \geq 1$ in the sense of first order stochastic dominance. Conversely, a job seeker is *wage pessimistic* if $F_t^{sub} \prec F_t$ for all $t \geq 1$ holds.

Consequences of Wage Optimism To see the implications of wage optimism, consider the following expanded expression of the value of unemployment:

$$U_t = \sum_{\eta=0}^{\infty} \frac{1}{(1+r)^{\eta}} S_{t+1,t+\eta+1} u(b_{t+\eta}) + \sum_{\eta=1}^{\infty} \frac{1}{(1+r)^{\eta}} S_{t+1,t+\eta} \left(s_{t+\eta} \mathrm{E}_{F_{t+\eta}^{sub}} V(w) - c_t(s_{t+\eta}) \right), \tag{4.7}$$

where $S_{t,t+\eta} = (1 - s_t) \ldots (1 - s_{t+\eta-1})$. The first term on the right hand side is the expected utility stream from unemployment benefits, conditional on the job seeker's search behavior. The second term is the expected value of finding a job in the future less any future effort costs. From this equation, it is clear that the chosen search intensity involves a trade-off between the current and any future value of taking up employment.

Consider a marginal change in the perceived wage offer distribution induced by a wage parameter ϕ_W. The effect on the current expected value of work is $\partial \mathrm{E}_{F_t^{sub}} V(w) / \partial \phi_W$, and the effect on the value of unemployment is

$$\frac{\partial U_t}{\partial \phi_w} = \sum_{\eta=1}^{\infty} \frac{1}{(1+r)^{\eta}} S_{t+1,t+\eta} s_{t+\eta} \frac{\partial \mathrm{E}_{F_{t+\eta}^{sub}} V(w)}{\partial \phi_W}. \tag{4.8}$$

This result can be used to derive qualitative predictions. First consider the case of wage optimism in a stationary job search model. In this case, the marginal effect of the increase in the subjectively expected wage on current and future values of employment is constant, i.e., $\partial \mathrm{E}_{F_t^{sub}} V(w) = \partial \mathrm{E}_{F_{t+\eta}^{sub}} V(w)$ for all $\eta = 1, \ldots$. Therefore, the effect of wage optimism on today's search effort is unambiguously positive.

A more interesting setting arises when allowing for non-stationarity. If the marginal effect on future utility is large relative to the effect on current utility, wage optimism makes current unemployment more attractive, as future wage losses due to prolonged unemployment are ignored. Therefore, job seekers initially search less than they would under rational expectations. Toward the stationary period, wage optimism causes search effort to be higher: optimistic individuals perceive their current payoffs from search to be higher than rational individuals who know about the deterioration of wage offers.

We thus expect that wage optimism causes individuals to search less early on. By contrast, individuals search more at later stages in the unemployment spell, when wage depreciation has already come into effect.

Learning and Reservation Wages The presented model assumes that job seekers accept every wage offer, thereby omitting the reservation wage choice. This central assumption is motivated by a recent empirical literature suggesting that reservation wages are not the main driver of search dynamics and the job finding hazard (e.g. Card, Chetty and Weber, 2007; Krueger and Mueller, 2016; Schmieder, Von Wachter and Bender, 2016). Given the popularity of job search models with reservation wages, a discussion of our modeling approach is, however, necessary.

When considering the initial periods of the unemployment spell, a model with reservation wages will lead to similar predictions as our model. The main intuition is that individuals with optimistic wage expectations will over-set their reservation wages, and potentially adjust them too little over time. If reservation wages are binding, wage optimism thus leads to an increased rejection of wage offers and therefore reduces job finding. At the initial periods of the unemployment spell, this is in line with our model, which also predicts a reduced job finding hazard in response to optimism. When it comes to later periods, our model implies that optimism can also encourage search by the long-term unemployed, and thereby increase job finding at later stages of the spell. In a model with reservation wages, this effect would be (partly) counteracted by the increased rejection of wage offers due to optimism. We thus conclude that the two models imply similar behavioral reactions at initial periods of the unemployment spell, but potentially diverging reactions during later periods.

Unfortunately, we do not observe rejected offers in our data and can therefore not test for a reservation wage policy directly. Instead, we will test whether individuals learn about wage offers, which would hint towards the relevance of reservation wages (see section 4.4).

Finally, it is worth noting that acceptance of any wage offer is not the only possible interpretation of the search effort model. The model may also, as in Card, Chetty and Weber (2007), be viewed as one with a deterministic individual wage which is observed only with measurement error.

4.3 Data

Data Sources and Sampling The study relies on administrative data on unemployed individuals from the German IAB Employment Biographies, matched to survey data from the IZA Evaluation Dataset. The IZA Evaluation Dataset is a survey of randomly chosen individuals who entered the German unemployment insurance (UI) between June 2007 and May 2008.[5] The interview is realized around 5 to 12 weeks after entry into unemployment.

We restrict the analysis to individuals aged between 20 and 55 years, who worked full-time prior to unemployment,[6] stayed unemployed at least one full month and are searching for work.[7]

To measure the relationship between pre-unemployment wages, wage expectations and re-employment wages in a consistent way, we need to apply additional sample restrictions. These may appear restrictive, but they ensure that our picture of optimistic wage expectations is not driven by confounding factors. We first exclude individuals whose self-reported net wage is above the gross wage reported in the administrative data (17.2%). In these cases, self-reported and administratively reported wages do

[5] Arni et al. (2014) provide a detailed description of the sampling method and content of the survey. The merged IZA/IAB data is described by Eberle, Mahlstedt and Schmucker (2017).

[6] We exclude part-time workers to avoid that changes in working hours confound the wage effects we are interested in.

[7] Individuals who state that they are not searching for work are not asked for their wage expectation and are therefore not relevant for our analysis.

obviously not refer to the same previous employment spell.[8] This is problematic in our context, as we study both wage expectations and re-employment wages in relation to the pre-unemployment wage. To focus on regular employment, we exclude individuals whose net pre-unemployment monthly wage was less than 631 euros gross, i.e., below the level of social welfare benefits (including rent payments) for a single household (7.5% of the remaining sample). In addition, we exclude individuals whose pre-unemployment wage is top-coded by the IAB at monthly 4470 euros gross (3.1% of the remaining sample). For these individuals, it is impossible to infer the wage depreciation profile. We further drop individuals who did not state a wage expectation although they are not yet re-employed at the time of the survey (7.5% of the remaining sample). Individuals who already found a job at the time of the interview and therefore do not have a stated expectation remain in the sample.[9] After these restrictions, the estimation sample contains 4,729 job seekers.

Measurement of Unemployment Duration and Wages The administrative data allow for a precise measurement of the realized unemployment duration, the pre-unemployment and the re-employment wage, the unemployment history and unemployment benefit payments. We observe entry into unemployment and can follow individuals until they are re-employed, independent of whether or not they receive unemployment benefits. Information on the employment status is reported on a monthly basis. An individual is defined as re-employed when entering a job that is subject to social insurance contributions (=monthly wage > 400 euros).

Measurement of Subjective Wage Expectations The survey data are used, in particular, to measure subjective wage expectations held by job seekers at the time of the survey. The corresponding question is framed as: "Now I am interested in your wage expectations concerning your next job. What do you expect to earn in net euros per month?" The wage expectation is naturally available only for job seekers who are still searching for work at the time of the interview. 726 individuals (15.4%) already found a job at the time of the interview and do therefore not state a wage expectation.

[8]Additionally, the administrative data can under-state the actual pre-unemployment gross wage in case of sickness or maternity leave, during which the income from employment is zero.

[9]In the estimation, these individuals contribute to the likelihood of job finding, but not to the likelihood of wage expectations.

For individuals who participate in the second survey wave and who are still unemployed after one year, the data report a second wage expectation (N=629, 13.3%). Recognizing that selection and attrition play a role for the availability of a second expectation, we use this data point to gain evidence on the role of updating in both the descriptive analysis and the structural estimation.

The survey-reported wage expectations are stated in net terms. As individuals also state their net pre-unemployment wage, we observe how much individuals expect to earn, in net terms, relative to their pre-unemployment wage. In the administrative data, wages are reported in gross terms. There, we observe how the gross re-employment wage relates to the gross pre-unemployment wage.[10] To relate gross wages to net expectations, both in the descriptive analysis and the structural estimation, we convert re-employment wages into net terms according to the procedure described in Appendix 4.A. The procedure relies on both the theoretical tax schedule for 2008 and on the fact that we observe pre-unemployment wages in both gross and net terms.

Summary Statistics Table 4.1 contains basic summary statistics on the variables used in the baseline estimation. The average pre-unemployment gross wage is at 1922 euros gross per month. The average re-employment gross wage is at 1812 euros, i.e. 110 euros below. In turn, the average net expectation is 120 euros above the average net pre-unemployment wage.

[10]For individuals who enter re-employment within 12 months and participate at the second survey wave, we also observe the re-employment wage in net terms. Due to severe attrition and the limited time horizon, we do not rely on this measure.

Table 4.1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Data Source
UE Duration	4729	8.772	7.319	Admin
Censored at T=20	4729	0.230	0.421	Admin
Gross Pre-UE Wage	4729	1922.074	743.745	Admin
Net Pre-UE Wage	4729	1261.188	419.720	Survey
Gross Re-Employment Wage	3642	1811.737	734.186	Admin
Net Re-Employment Wage	3642	1214.120	444.508	Admin, own calculation
Net Expected Wage, Wave 1	4003	1380.786	461.143	Survey
Net Expected Wage, Wave 2	629	1363.968	455.802	Survey
Education: Medium	4729	0.487	0.500	Survey
Education: High	4729	0.176	0.381	Survey
Female	4729	0.387	0.487	Survey
Prior UE in Years	4729	1.008	1.289	Admin
Work Experience in Years	4729	6.055	2.777	Admin
PBD in Months	4729	11.319	2.268	Own calculation
UI Benefits (ALG I)	4729	835.048	281.755	Admin
Welfare Benefits (ALG II)	4729	631.000	0.000	Own calculation

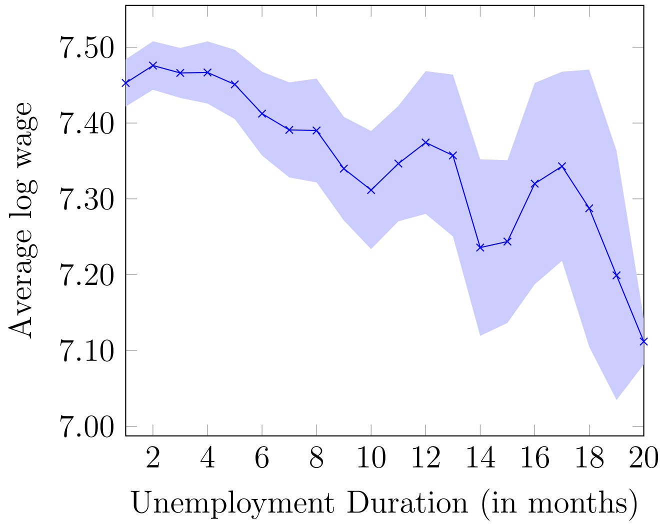
Source: IZA Evaluation Dataset (survey) & IAB Employment Biographies (admin). An exit from unemployment is defined as the transition to a job which is subject to social security contributions and lasts more thn one month. Right-censoring applies if individuals are unemployed for more than 20 months. Monthly wages are reported in gross terms. “Education: Medium” takes the value one if the individual has finished the German Realschule or Fachoberschule. “Education: High” takes the value one if the individual holds the German Abitur. Prior unemployment and work experience both refer to the 10 years prior to entry into the current unemployment spell. PBD is a function of the number of months worked during the 5 years prior to unemployment, and of age. UI benefits are a function of the pre-unemployment wage and of the number of children. Welfare benefits are means-tested and contain a payment for rent expenses and a payment for other living expenses. In practice, welfare benefits vary with household size. For simplification, we use here the average payment for a single individual.

4.4 Descriptive Evidence

In the following, we provide descriptive evidence on the realized and perceived evolution of wage offers after entry into unemployment. We first show that wages decrease in the time out of employment. We then document that job seekers do not expect the fall in wage offers at the beginning of their spell, and that they hardly update their wage expectations later on.

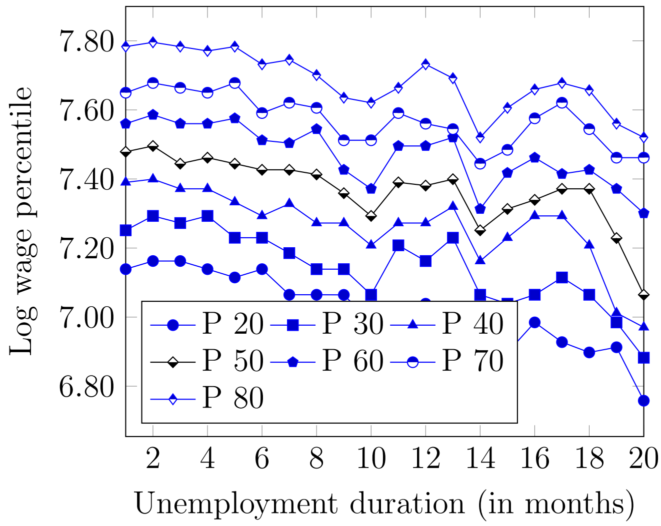
Wage Depreciation Over the Unemployment Spell Figure 4.1 shows how log monthly re-employment wages evolve with the realized duration of unemployment. Clearly, the average wage decreases over the spell. As illustrated by Figure 4.2, this pattern holds for all deciles of the wage distribution.

Figure 4.1: Re-Employment Net Log Wage (Gross)



Source: IAB Employment Biographies. The shaded area shows 95% confidence bands. The graph includes individuals who enter re-employment within 20 months (N=3642).

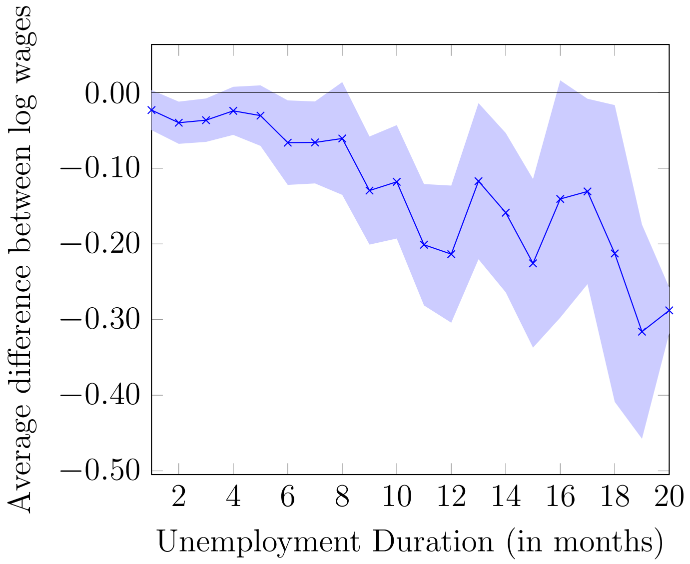
Figure 4.2: Deciles of Re-Employment Log Wage (Gross)



Source: IAB Employment Biographies. The graph includes individuals who enter re-employment within 20 months (N=3642).

Since wage levels may be correlated with the time spent in unemployment for various reasons, a more relevant measure of wage depreciation may result from a comparison of re-employment wages with pre-unemployment wages. Figure 4.3 shows the difference between the log re-employment and the log pre-unemployment wage. The absolute difference increases strongly over the spell. While individuals who remain unemployed up to 4 months lose around 2 to 3% relative to their pre-unemployment wage, longer spells are associated with significant higher losses. In particular, individuals who are unemployed for longer than one year lose already 10-20% on average. In a linear specification, the monthly depreciation factor is -1.2%, i.e., slightly above the 0.8% estimated quasi-experimentally by Schmieder, Von Wachter and Bender (2016). Appendix figures 4.B.1 to 4.B.3 show the same pattern for converted net wages. Tax progression slightly alleviates the wage depreciation, which is here 1% per month according to a linear specification.

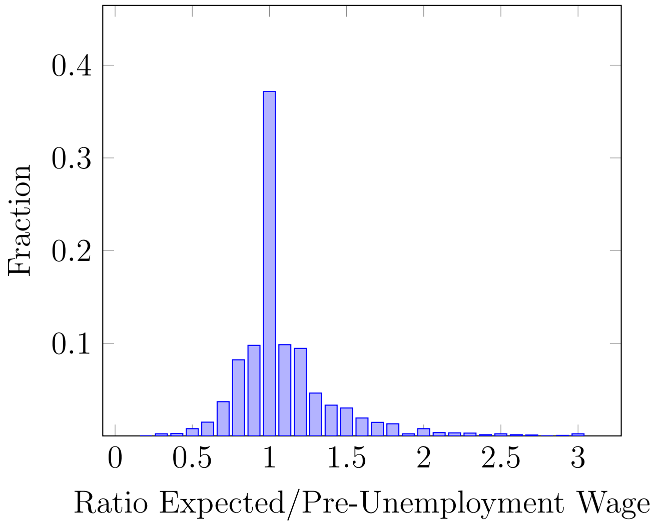
Figure 4.3: Re-Employment Minus Pre-Unemployment Log Wage (Gross)



Source: IAB Employment Biographies. The shaded area shows 95% confidence bands. The graph includes individuals who enter re-employment within 20 months (N=3642).

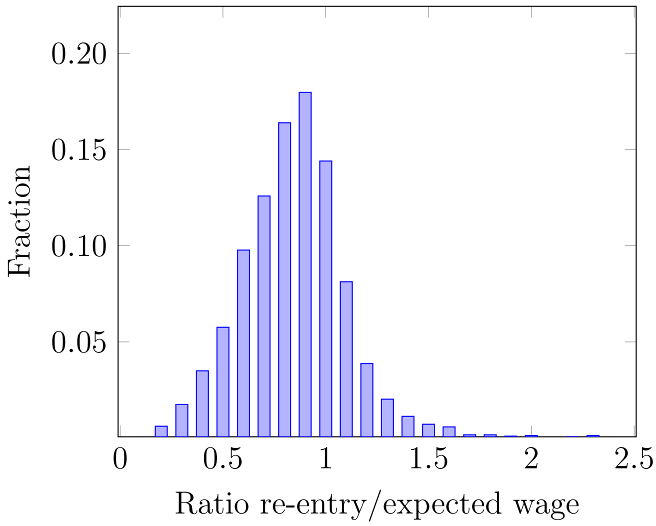
Subjective Wage Expectations How do individuals perceive their wage prospects when entering unemployment? Figure 4.4 shows the sample distribution expected over pre-unemployment net wages. Clearly, most job seekers do not expect a wage loss. Almost 40% expect to earn a wage which is very close to their last wage, and more individuals expect to gain than to lose relative to their pre-unemployment wage. As illustrated by figure 4.5, this pattern results for many individuals in a gap between the expected and the realized net re-employment wage. Both the average and the median job seeker obtain only 90% of their expected wage.

Figure 4.4: Initial Wage Expectation over Pre-Unemployment Wage (Net)



Source: IAB Employment Biographies (pre-unemployment wage) and IZA Evaluation Dataset (wage expectation). The graph includes individuals who have not entered re-employment at the interview date (N=4003). Individuals with a ratio larger than 3 are excluded from the graph (<1%)

Figure 4.5: Re-Employment Wage over Wage Expectation (Net)

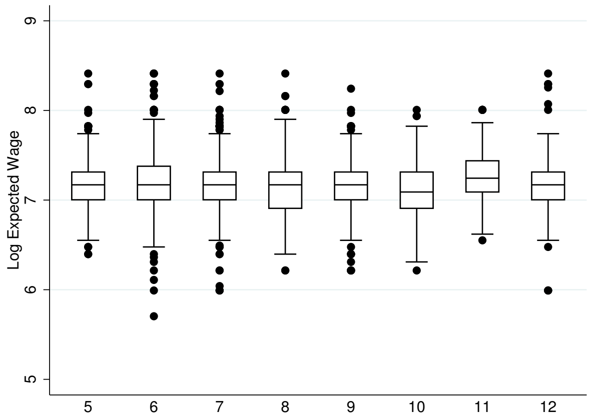


Source: IAB Employment Biographies (re-employment wage) and IZA Evaluation Dataset (wage expectation). The graph includes individuals who have not entered re-employment at the interview date and who enter re-employment within 20 months (N=2916).

In table 4.B.1 of appendix 4.B, we regress the ratio of the re-employment wages over the expected wage on individual job seeker characteristics, to get a sense of the degree of heterogeneity in wage optimism. Column 1 shows that the ratio of the re-employment wages over the expected wage decreases in the pre-unemployment wage. This suggests that individuals with high pre-unemployment wages receive a smaller share of their expected wage. Also female individuals receive on average a smaller share. Prior work experience and education is associated with a more realistic expectation, i.e. a higher ratio. The same pattern holds in column 2, where the dependent variable is an indicator for whether the ratio of the re-employment wages over the expected wage is lower than the sample median. Since education has a strong correlation with wage optimism, we allow for heterogeneous effects by level of education in our structural estimation.

A natural question arises: do individuals correct for their initial optimism during their period of job search, i.e., is there evidence of learning? The data allow us to test for this in two ways. First, job seekers are interviewed at slightly different points in their unemployment spell, between week 5 and 12 after entry into unemployment. This provides a small degree of random variation in the time at which job seekers are asked for their initial expectation. Figure 4.6 plots the ratio of expected over last wages by the week of interview. It clearly shows that the distribution of expectations does not change over this time window: job seekers with 5 weeks of elapsed unemployment do not hold different expectations than job seekers whose elapsed unemployment is 12 weeks.

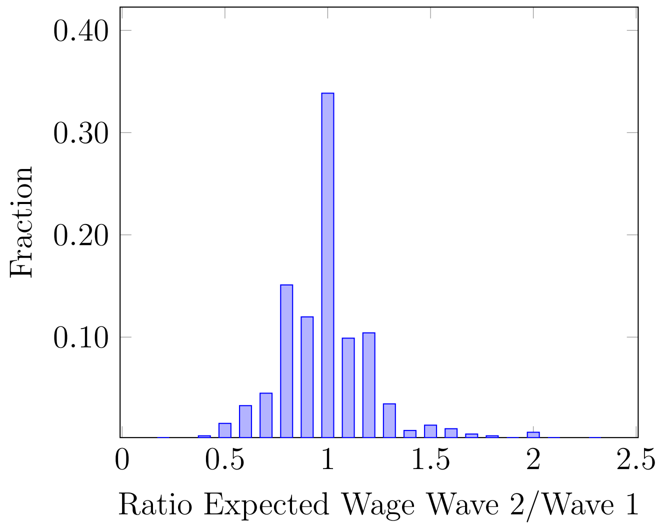
Figure 4.6: Subjective Wage Expectations (Net), by Week of Interview



Source: IZA Evaluation Dataset. The graph shows box plots on initial net wage expectations (in log) over the job seeker's week of interview. The upper line of the box shows $75^t h$ percentiles, the line inside the box shows medians and the lower line shows $25^t h$ percentiles. Dots show outside values. The graph includes individuals who have not entered re-employment at the interview date (N=4003).

As an additional source of variation in the timing of stated expectations, we use a second survey wave, realized 12 months after entry into unemployment. For the second wave, all survey participants were re-contacted for an interview. For the subset of job seekers participating in the second wave and being still unemployed at that point in time, we thus observe an additional wage expectation (N=629). Figure 4.7 plots the ratio of wage expectations reported in wave 2 against the initial expectation of these individuals. It shows that more than 30% of job seekers maintain about their initial expectation after one year in unemployment. The average and median job seeker perform zero updating. Although individuals still unemployed after a year are a selective group and despite attrition, the pattern suggests that there is on average no relevant updating of wage expectations over the spell. This observation is in line with Krueger and Mueller (2016), who find that the reservation wages of U.S. workers hardly adapt over the unemployment spell. Koenig, Manning and Petrongolo (2016) confirm the notion of reference dependence in reservation wages for UK and West German job seekers.

Figure 4.7: Net Wage Expectation, Wave 1 over Net Wage Expectation, Wave 2



Source: IZA Evaluation Dataset. Wave 1 takes place between 5 and 11 weeks after entry into unemployment. Wave 2 re-interviews all available job seekers after 12 months of unemployment. The graph includes individuals with a stated wage expectation in waves 1 and 2 (N=629).

4.5 Structural Estimation

In this section, we discuss the likelihood function, econometric specification and identification of the model specified in section 4.2. Our goal is to obtain parameter estimates allowing to simulate how individuals respond to information on their (future) wage opportunities.

4.5.1 Likelihood Function

We estimate the non-stationary job search model with subjective beliefs about wage offers using maximum likelihood. The likelihood function describes the joint density of observed wages w_i, job search durations d_i, and wage expectations w_i^{exp}. The parameter vector ϕ contains the wage offer distributions F_t, the benefit levels b_t, the utility functions $u(w)$ and $u(b)$, and the search cost function e. We allow subjective wage expectations to be subject to normal measurement error, with $\varepsilon^{exp} := \log(\widetilde{w}^{exp}(\phi)) - \log(w^{exp})$, where w^{exp} is the reported subjective wage expectation and $\widetilde{w}^{exp}(\phi)$ the underlying subjective wage expectation. Denote the density of the measurement error $h_{exp|\phi}$. The likelihood contribution for an uncensored observation i ending in month d_i is:

$$\mathcal{L}_i^{uncens}(\phi) = \prod_{\eta=1}^{d_i-1} (1 - s_\eta(\phi)) h_{exp|\phi}(w_\eta^{exp}|\widetilde{w}_{d_i}^{exp}(\phi))^{d_\eta^{exp}} s_{d_i}(\phi) f_{d_i}^{obj}(w_i|\phi), \tag{4.9}$$

where d_η^{exp} indicates if a wage expectation is observed in period η. Similarly, for observations censored at time $t = T_C$, we have:

$$\mathcal{L}_i^{cens}(\phi) = \prod_{\eta=1}^{T_C} (1 - s_\eta(\phi)) h_{exp|\phi}(w_\eta^{exp}|\widetilde{w}_\eta^{exp}(\phi))^{d_\eta^{exp}}. \tag{4.10}$$

4.5.2 Econometric Specification

Utility is linear in consumption, which is equal to income, implying the absence of savings as in Frijters and der Klaauw (2006). Individuals employed at wage w derive logarithmic utility from their net wage, $\tau(w)$: $u(w) = log(\tau(w))$. The conversion of gross wages into net terms follows the procedure described in appendix 4.A. When unemployed with benefits b_t, individuals have utility $u(b) = log(b_t)$. When unemployed, agents receive wage offers from a log-normal distribution, $w_t \sim \mathcal{N}(\mu_t^w, \sigma)$ where the level of wage offers μ_0 is allowed to depend on individual characteristics X. We suppress individual subscripts to ease notation. Over time, the mean of the wage offer distribution depreciates with rate θ^{obj},

$$\mu_t^w = \mu_0 - \theta^{obj} t, \tag{4.11}$$

$$\mu_0 = \beta^\mu X. \tag{4.12}$$

As in Paserman (2008) and DellaVigna et al. (2016), we assume the search cost function to be of power form:

$$c_t(s) = e_t \frac{s^{1+\gamma}}{1+\gamma}, \tag{4.13}$$

$$e_t = \exp(\beta^e X - \theta^e t). \tag{4.14}$$

The search cost component e_t is allowed to vary geometrically with time, as measured by the cost-depreciation factor θ^e. For instance, search can become more costly over the unemployment spell as easily available offers become exhausted or because motivation decreases. But there may also be other elements of duration dependence entering in θ^e. For the purpose of our study, we can remain agnostic on the exact contents of θ^e.

As noted by DellaVigna et al. (2016), the search cost parameter γ is the inverse elasticity of the search intensity to the net value of unemployment, $(\mathrm{E}_{F_t^{sub}} V(w) - U_t)/e_t$. Both for wage offers and for search costs, the vector of observable characteristics X includes the last wage received before entrance into unemployment (in addition to education, as well as prior work and unemployment experience). This allows us to control for differences in ability or productivity, which may cause selection into prolonged unemployment. We discuss the role of unobserved heterogeneity below, in section 4.5.3.

Subjective Wage Expectations

The survey data report subjective wage expectations w_t^{exp} at different points early in the unemployment spell (between week 5 and 12). Some individuals (N=629) also report a second subjective wage expectation after about 12 months of unemployment. Formally, we interpret the underlying subjective wage expectation as a weighted average of future subjective wage offers:

$$\widetilde{w}_t^{exp}(\phi) = \sum_{\eta=0}^{\infty} \mathrm{Prob}(d = t+\eta|\phi)\mathrm{E}_{F_{t+\eta}^{sub}} w \tag{4.15}$$

$$= \sum_{\eta=0}^{\infty} S_{t,t+\eta}(\phi) s_{t+\eta}(\phi) \mathrm{E}_{F_{t+\eta}^{sub}} w, \tag{4.16}$$

where d is the duration of unemployment and $S_{t,t+\eta}(\phi) = \text{Prob}(d \geq t+\eta | d \geq= t, \phi)$ is the survival probability from t to $t+\eta$, given the model parameters ϕ. F_t^{sub} denotes the subjective wage offer distribution at time t characterized by $\mathcal{N}(\mu_t^{w,sub}, \sigma_w)$, where

$$\mu_t^{w,sub} = \mu_0 - \theta^{sub} t + \alpha^{sub}. \tag{4.17}$$

In this specification, an individual's subjective wage distribution may differ from the true wage offer distribution in two regards. First, there may be a different perception of the *rate* at which mean wage offers depreciate during the unemployment spell, i.e. $\theta^{sub} \neq \theta^{obj}$. Second, there may be a misperception of the overall *level* of wage offers, such that $\alpha^{sub} \neq 0$. Thus, wage optimism may be characterized by $\theta^{sub} < \theta^{obj}$, or $\alpha^{sub} > 0$, or both.

We account for measurement error in the subjective wage expectation by defining $\varepsilon_{exp,t} \sim \mathcal{N}(0, \sigma_{exp})$ as

$$\varepsilon_{i,t}^{exp} = w_t^{exp} - \widetilde{w}_t^{exp}(\phi). \tag{4.18}$$

4.5.3 Identification

The central parameters in our job search model are the rate of wage depreciation θ^{obj} and its subjective counterpart θ^{sub}, as well as the level parameter of wage optimism α^{sub}. Since the model abstracts from reservation wage choices,[11] we identify the full path of wage offer distributions F_t^{obj}, hence θ^{obj} and σ_w, from accepted wages at different job search durations t. This naturally also holds for all combinations of the vector X, such that β^{μ} is identified as well. The parameters of the subjective counterparts of the wage distribution, denoted by θ^{sub} and α^{sub}, are identified by the (repeated) observations of the subjective wage expectations, for different duration outcomes.

In the model, the job finding effort depends on the net value of employment, $V_t - U_t$, and the costs of search, $c_t(s_t)$. We restrict the intercept of the search cost scale parameter to be zero, and identify the remaining elements of β_e from differences in job finding hazard across subgroups defined by X. Thus, the inverse elasticity of search intensity γ is identified from the scale of the hazard rate and its reaction to time-varying unemployment benefits. It follows that for groups of individuals with the benefit path and observable characteristics, duration dependence in the job finding

[11] Cf. section 4.2 for a discussion of this modeling choice.

hazard identifies dynamics in the search cost function θ^e.

We include past wages, education, work and unemployment history into the vector of covariates X to control for heterogeneity between individuals in the average wage offer μ_w and in the cost of effort parameter e. In the context of job search, controls for the labor market history have been shown to be a powerful control for (usually unobserved) heterogeneity between unemployed individuals (see, e.g., Caliendo, Mahlstedt and Mitnik (2017) who analyze the same data as in this paper). A transparent separate identification of unobserved heterogeneity is not given in our model with the data at hand. Nevertheless, we cannot fully exclude the possibility of remaining unobserved heterogeneity. Such heterogeneity would mostly lead to an over-estimation of θ^e, which is identified from duration dependence conditional on X.

We set the discount rate to 20% p.a. ($r = 0.0153$), following the estimates obtained by Frijters and der Klaauw (2006) for German individuals. In our setting, it is difficult to disentangle the parameter values of γ and r, since both respond to variation in individual benefit and wage and benefit paths over the spell. As in Frijters and der Klaauw (2006), we assume that job finding is an absorbing state, justifying the relatively high baseline discount rate of 20 % per year. To assess the sensitivity of our policy effects on the choice of the discount factor, we provide results based on estimations with r=0.01 and r=0.02, as lower and upper bounds, respectively, in section 4.6.3.

4.6 Estimation Results

We first report the parameter estimates and the model fit for the job search model with and without subjective wage expectations. We then use the estimates of the subjective expectations model to simulate a scenario in which individuals are perfectly informed about their wage prospects. On this basis, we discuss the effect of wage optimism on the duration to re-employment, re-employment wages, and benefit payments.

4.6.1 Parameter Estimates

Average Effects

We first estimate the model as outlined in section 4.2, taking into account subjective expectations about wage offers. We then compare the estimates from this model to those from a non-stationary job search model with rational expectations about wage offers. In this alternative specification, subjective expectations are not used as model inputs, since individuals are assumed to know their current and future wage offers.

Table 4.1 shows baseline parameter estimates resulting from the two models, using a discount factor of $r = 0.153$ (implying an annual discounting of 20%). As described by equation (4.17), the average subjective wage expected for period t, $\mu_t^{w,sub}$, is composed of the average initial wage level, μ_0, the level difference between expected and actual wage offers, α^{sub}, and the subjective wage depreciation factor, θ^{sub}. Results show that, as expected, μ_0 increases with the pre-unemployment wage and with education. Conditional on these variables, work experience and unemployment experience have a minor influence. The level parameter of wage optimism, α^{sub}, indicates that job seekers expect 6% higher wages on average. The estimated subjective wage offer depreciation rate, θ^{sub}, is not different from zero, implying that individuals do not anticipate their wage offers to fall. The actual wage depreciation factor, θ^{obj}, is estimated to be 1.2% per month of unemployment, as in the descriptive analysis. Therefore, individuals are wage optimistic both with respect to the average wage offer level, and with respect to its depreciation.

Assuming rational expectations implies that individuals realistically perceive actual wage offers μ_t^w, both their initial wage level, μ_0, and the factor of depreciation over time, θ^{obj}. From the job seeker's perspective, this implies that the subjectively perceived net value of employment, $(\mathrm{E}_{F_t^{sub}} V(w) - U_t)/e_t$, is lower both in its level and in its evolution over the unemployment spell.

The results highlight the importance of the assumptions made about individual wage perceptions. Assuming rational expectations decreases the perceived net value of employment over unemployment, which leads to a higher estimate of γ (4.0) than in the model allowing for subjective wage expectations (2.21). The elasticity of search ($\frac{1}{\gamma}$) is thus lower under rational expectations. In the subjective expectations model, individuals are not aware that their value of re-employment changes over time. From an individual perspective, this implies less changes in the monetary incentives for job search. To explain the same behavioral reaction, the sensitivity to monetary incentives thus needs to be larger under subjective expectations. In this sense, the difference in the estimated gammas reveals that the rational expectations model under-estimates how the job search decision reacts to dynamic changes in monetary incentives.

Table 4.1: Parameter Estimates

	(1)	(2)	(3)	(4)
	Subjective Expectations		*Rational Expectations*	
Parameter	Estimate	S.E.	Estimate	S.E.
Wage Offers				
μ_0				
Constant	2.802	0.053	3.134	0.018
Log Pre-UE Wage	0.625	0.008	0.571	0.002
Education: Medium	0.006	0.007	-0.011	0.010
Education: High	0.140	0.008	0.130	0.014
Work Experience in Yrs	-0.012	0.006	0.030	0.011
Previous UE in Yrs	-0.007	0.003	-0.005	0.004
θ^{obj}	0.012	0.001	0.012	0.001
θ^{sub}	-0.001	0.001		
α^{sub}	0.056	0.011		
Inverse Elasticity to Net Value of Employment				
γ	2.209	0.397	3.989	1.044
Search Costs e				
Log Pre-UE Wage	0.918	0.104	1.410	0.278
Education: Medium	-0.029	0.112	-0.001	0.164
Education: High	0.295	0.150	0.299	0.225
Work Experience in Yrs	-0.184	0.109	-0.300	0.178
Previous UE in Yrs	0.097	0.043	0.176	0.073
θ^e	0.253	0.040	0.385	0.101
σ_w	0.330	0.003	0.330	0.003
σ_ϵ	0.248	0.001		
Log L	-2.786		-2.760	
N	4,729		4,729	

The subjective expectations model estimates the likelihood specified in equations 4.9 and 4.10. The discount rate is set to r=0.0153. The rational expectations model estimates the same likelihood, excluding the contribution of subjective expectations. As the rational expectations model does not have to fit the subjective expectations data, it has a larger likelihood value.

Heterogeneity by Education

Table 4.2 reports parameter estimates in which subjective and objective wage parameters are interacted with individuals' level of education. There is as good as no heterogeneity in the actual wage depreciation parameter θ^{obj}. We also observe no heterogeneity in wage optimism between the lower two education groups. In turn, the estimates report that individuals with high levels of education (high school degree "Abitur" or more) anticipate the depreciation of wages at least partly ($\theta^{sub} \times HighEducation = 0.007$). At the same time, they also are substantially more baseline optimistic ($\alpha^{sub} \times HighEducation = 0.08$). While both interaction terms are only at the margin to statistical significance, they suggest that highly educated individuals over-estimate their general wage opportunities. However, these individuals appear also more aware that prolonged unemployment can cause wage reductions. In the simulations section 4.6.3, we assess how this pattern changes the predicted effects of an information intervention.

Table 4.2: Parameter Estimates, Heterogeneity by Education

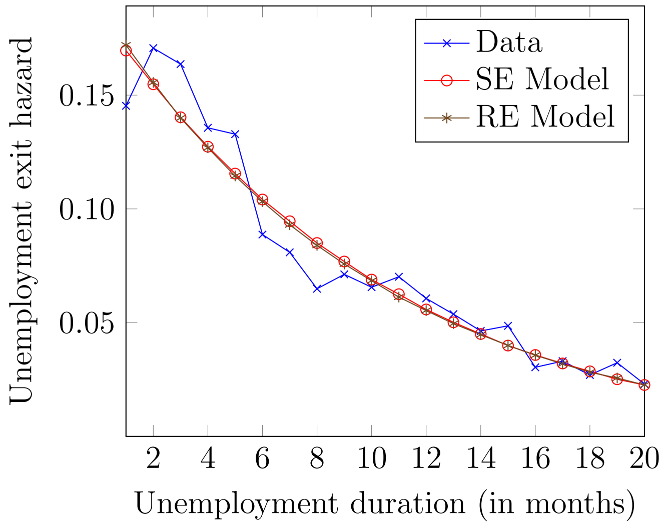
	(1)	(2)	(3)	(4)
	Subjective Expectations		Rational Expectations	
	Estimate	S.E.	Estimate	S.E.
Wage Offers				
μ_0				
Constant	2.805	0.060	3.064	0.018
Log Pre-UE Wage	0.623	0.008	0.583	0.002
Education: Medium	0.015	0.019	-0.013	0.012
Education: High	0.141	0.022	0.139	0.019
Work Experience in Yrs	-0.012	0.006	0.018	0.009
Previous UE in Yrs	-0.007	0.003	-0.006	0.005
θ^{obj} (Baseline)	0.012	0.002	0.011	0.001
θ^{obj} $\times$ (Education: Medium)	0.002	0.003	-0.002	0.002
θ^{obj} $\times$ (Education: High)	0.001	0.003	0.002	0.002
θ^{sub} (Baseline)	-0.002	0.002		
θ^{sub} $\times$ (Education: Medium)	0.001	0.002		
θ^{sub} $\times$ (Education: High)	0.007	0.004		
α^{sub} (Baseline)	0.058	0.027		
α^{sub} $\times$ (Education: Medium)	0.000	0.031		
α^{sub} $\times$ (Education: High)	0.082	0.049		
Inverse Elasticity to Net Value of Employment				
γ	2.848	0.663	5.086	1.653
Search Costs e				
Log Pre-UE Wage	1.061	0.166	1.677	0.435
Education: Medium	0.038	0.163	-0.028	0.208
Education: High	0.585	0.222	0.428	0.288
Work Experience in Yrs	-0.183	0.131	-0.319	0.222
Previous UE in Yrs	0.128	0.055	0.217	0.098
θ_{e0}	0.312	0.066	0.489	0.158
σ_w	0.330	0.003	0.330	0.003
σ_ϵ	0.249	0.001		
Log L	-2.785		-2.763	
N	4,729		4,729	

The subjective expectations model estimates the likelihood specified in equations 4.9 and 4.10. The discount rate is set to r=0.0153. The rational expectations model estimates the same likelihood, excluding the contribution of subjective expectations. As the rational expectations model does not have to fit the subjective expectations data, it has a larger likelihood value. Individuals with low education (lowest education track/ "Hauptschule") are in the baseline category. Individuals with medium education have completed the second education track ("Realschule" or "Fachoberschule"). Individuals with high education have completed the highest track and obtained the German "Abitur".

4.6.2 Fit

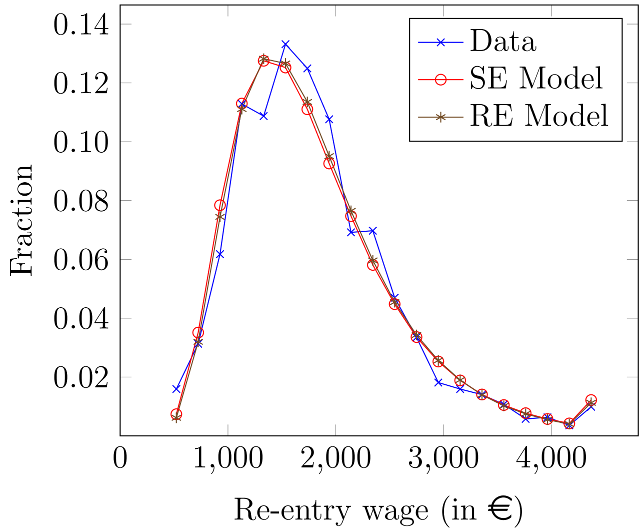
Figures 4.1 to 4.3 illustrate how well the two models fit the data. As is seen in figure 4.1, job finding exhibits strong negative duration dependence in the data. the job finding hazard starts out from about 15 to 17% and declines to as little as 3% after 20 months. This pattern is captured equally well by the standard model and our model with subjective wage expectations. Figures 4.2 to 4.3 show histograms for the fit of gross re-entry wages and the net wage expectations, respectively. Importantly, the model fit for re-entry wages is equally well for both models. However, 4.3 clearly shows -in line with descriptive evidence that the rational expectations cannot be reconciled with actually observed subjective wage expectations: for wage expectations to be rational, the fraction of individuals expecting to earn between 1000 and 1500 €net would have to be roughly 10 p.p. higher, with less people expecting to earn between 1500 and 2500 euros net.

Figure 4.1: Fit of Job Finding Hazard



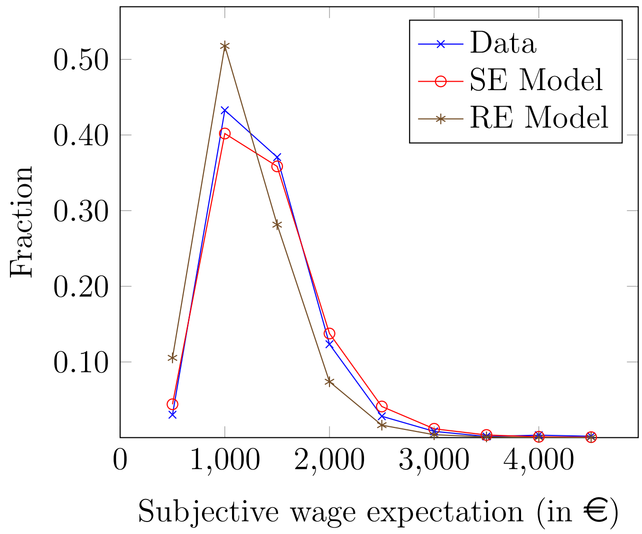
SE=subjective expectations, RE=rational expectations.

Figure 4.2: Fit of Re-Employment Wages



SE=subjective expectations, RE=rational expectations.

Figure 4.3: Fit of Subjective Wage Expectations



SE=subjective expectations, RE=rational expectations.

4.6.3 Simulation: The Effect of Wage Optimism on Job Finding

In the following, we use the parameter estimates to simulate a scenario in which individuals are perfectly informed about future wage offers, to quantify the impact of wage optimism on job finding. We first present average effects and then test for effect heterogeneity by education. In a final step, we assess how sensitive the results are with respect to the choice of the discount factor. All simulations are based on 1,000 independent random draws for each of the 4,729 individuals, using the parameter estimates reported in table 4.1.

Average Effect of Wage Optimism

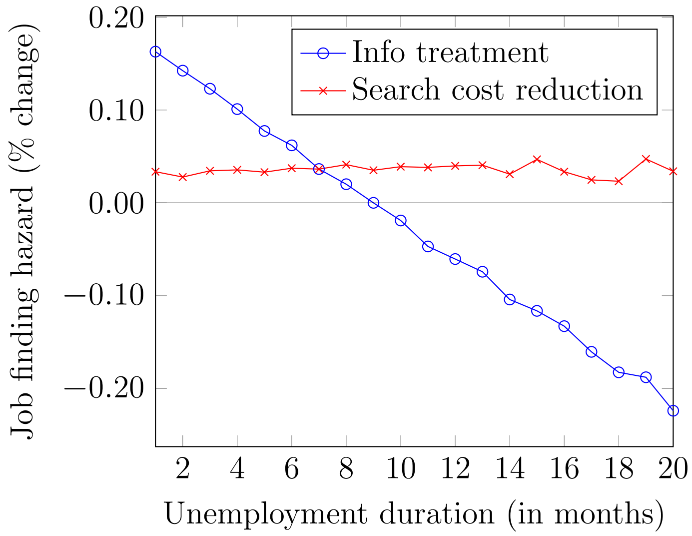
To understand how wage optimism affects job finding, we simulate a counter-factual scenario in which job seekers are fully aware of the path of wage offers over their spell. To this end, we estimate the subjective expecations model while imposing $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$. In terms of policy, we predict the effects of an intervention in which job seekers are provided with perfect information about their wage profiles. I.e., we simulate a policy that leads to a full adjustment of individual wage expectations to actual wage offer distributions. This is, of course, an ideal that is not reached in practice. However, well-designed counseling, or an "information treatment", will likely lead to a partial adjustment of subjective wage expectations. Therefore, we use our simulation results to understand the dynamics of reactions to information, and to obtain an upper bound of the effect of information treatments could have on job finding.

We contrast the effects of an information treatment with those of a 10% reduction in search costs e_t in each month t in the unemployment spell. From a policy perspective, search costs can for instance be reduced by offering support with application writing or by referring suitable vacancies.

Figure 4.4 plots predicted percentage changes in the job finding hazard over the spell. The search cost reduction increases job finding at all points in time. Given that search costs increase over the spell (due to a positive estimate of θ^e), the benefits of a 10% cost reduction also follow a slightly increasing pattern. Overall, the effect ranges around 3-5%.

By contrast, the information treatment shows highly dynamic effects over the course of the unemployment spell. As wage losses can be avoided by exiting in an earlier stage, perfectly informed individuals are around 15% more likely to find a job during the first month of unemployment. This effect sharply decreases over the spell and reaches a point estimate of zero in month nine. From then onward, perfect information reduces incentives to search and the job finding probability is 20% lower in month 20. This simulated pattern is in line with the qualitative predictions discussed in section 4.2: at the beginning of the unemployment spell, the prospect of falling wage offers creates incentives for individuals to search more today. Under wage optimism, this incentive is absent, which explains that the counter-factual with perfect information predicts more individuals finding a job very early on. For individuals who remain unemployed, rational expectations reduce motivation at later stages of the spell: individuals now realize they have lower returns from searching since the quality of their wage offers has depreciated.

Figure 4.4: Simulation: Information Treatment and Search Cost Reduction



The counter-factual simulation is based on 1000 independent random draws for each of the 4,729 individuals in the sample. The information treatment imposes perfect information about wage offers by setting $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$. The simulated search cost reduction reduces e_t by 10% at each month of the spell.

In table 4.3, we report how the change to perfect information affects the average duration to re-employment, the amount of benefit payment and wages. Given that a large share of individuals exits unemployment at early stages, the average duration to re-employment reduces substantially in response to information, by about 1.2 months ($\approx$ 12%). As a consequence, the welfare state saves on average about 839 euros ($\approx$ 11%) of benefit payments per person. As the average individual avoids around one month of wage depreciation, the average re-employment wage increases by around 1%. The 10% search cost reduction reduces unemployment and benefit payments by about 2% and increases wages by about 0.2%.

Table 4.3: Simulated Average Effect of Perfect Information

	Information Treatment		Search Cost Reduction	
	Simulated Effect	% Change	Simulated Effect	% Change
Duration to Re-Empl. in Mts	-1.2	-11.8%	-0.2	-2.2%
UI Benefit Payment in Euros	-839.3	-10.9%	-151.7	-2.0%
Monthly Gross Wage in Euros	17.5	1.0%	2.9	0.2%

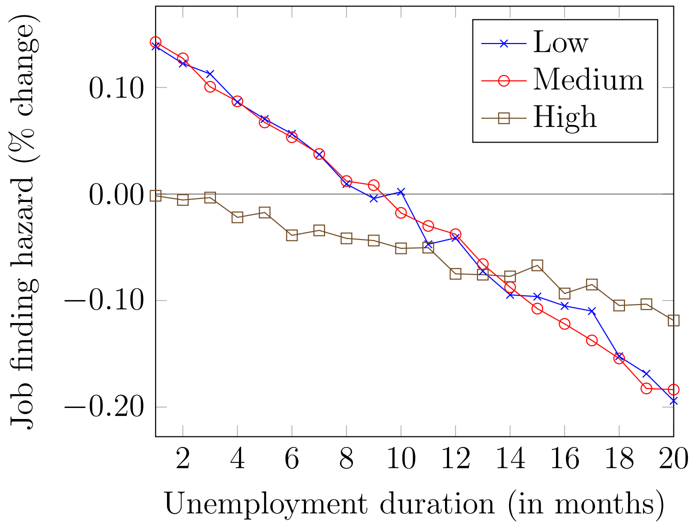
The counter-factual simulation is based on 1000 random independent draws for each of the 4,729 individuals in the sample. The information treatment imposes perfect information about wage offers by setting $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$. The search cost reduction reduces e_t by 10% at each month of the spell.

Taken together, the simulation illustrates an economically significant potential to increase early exits from unemployment by raising the awareness about falling wage offers. This benefit, however, comes at the price of discouragement among long-term unemployed workers, which renders overall welfare implications ambiguous.

Heterogeneity by Education

The parameter estimates of section 4.6.1 revealed that highly educated individuals show a different pattern of optimism: they are more optimistic on general wage opportunities, but more realistic when it comes to the depreciation of wage offers over the spell. Figure 4.5 plots the effect of perfect information for the three different education levels. While the two lower education groups do not differ in their reactions, highly educated individuals react substantially less to information. They do not change behavior at the beginning of the spell, and decrease search down to 10% at later stages. This results can be explained by the different beliefs held by these individuals: when their high optimism on wage levels is counteracted, the perceived returns from search lower, and job finding decreases overall. At the same time, there is less response to information on the path of wage offers, because educated individuals are partly aware of wage depreciation already. Therefore, we observe a less dynamic pattern for this group, and the initial positive effect of information is absent.

Figure 4.5: Simulation: Information Treatment Effect by Education



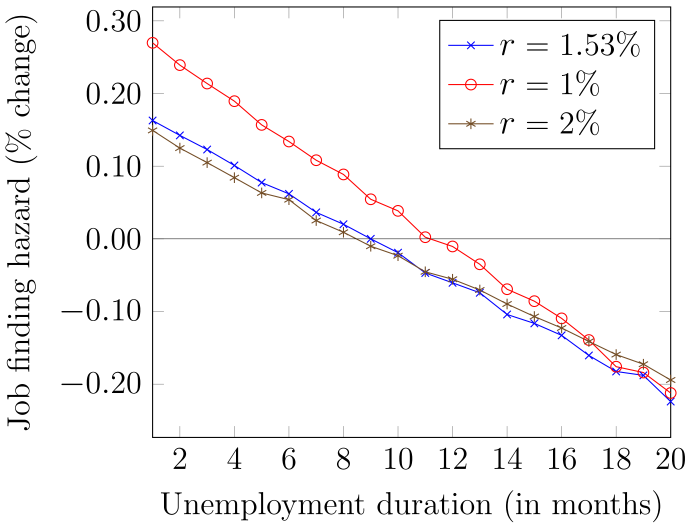
The counter-factual simulation is based on 1000 independent random draws for each of the 4,729 individuals in the sample. The information treatment imposes perfect information about wage offers by setting $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$. Individuals with low education completed the lowest education track ("Hauptschule"). Individuals with medium education have completed the second education track ("Realschule" or "Fachoberschule"). Individuals with high education have completed the highest track and obtained the German "Abitur".

Sensitivity to the Chosen Discount Factor

As mentioned in section 4.5.3, the estimated returns to information may be influenced by the chosen discount factor r. Setting r too low may lead to an over-estimated effect of information on initial search: more patient job seekers will find a decrease in future wage offers more salient, such that initial job seeking shows stronger reactions to future wage offer reductions. Setting the discount rate too high has the opposite effect of under-estimating the treatment effect. In turn, we do not expect that the estimated effect on search at later stages of the spell is affected, as the effect is purely driven by changes in current payoffs when stationarity is close.

To provide evidence on the sensitivity of predicted effects to the discount rate, we estimate the effect of optimism for a low value of $r = 1\%$, and for a high value of $r = 2\%$. The corresponding parameter estimates are reported in tables 4.C.1 and 4.C.2 of appendix 4.C. Figure 4.6 shows that the qualitative pattern of the policy effect looks very similar across discount factors. However, the initial effect sizes show some degree of sensitivity: for the lower bound of $r = 1\%$, the initial increase in the job finding hazard due to information starts off at 28% and becomes zero only in month 11. This reflects that patient individuals perceive future wage losses more severely at the beginning of the unemployment spell. In turn, the effects simulated with the upper bound of 2% hardly differ from our baseline with $r \approx 1.5\%$. The effect of the information treatment for longer-term unemployed (12+ months) does not depend on the chosen discount rate. This is expected, as the decision to search is less influenced by future payoffs when the stationarity approaches.

Figure 4.6: Simulation: Information Treatment Effect for Different Choices of r



The counter-factual simulation is based on 1000 independent random draws for each of the 4,729 individuals in the sample. The information treatment imposes perfect information about wage offers by setting $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$.

Given that job finding predominantly occurs at the beginning of the spell, the size of average effects reacts to the choice of r. As reported by table 4.4, the duration to re-employment is predicted to decrease by 17% for $r = 1\%$, versus 12% in the baseline case of $r = 1.53\%$ and 11% for $r = 2\%$.

We conclude that the exact initial effect sizes are sensitive to the choice of the discount factor. However, we can confirm the robustness of the following findings. First, the effect of an information policy is economically significant and potentially large. Second, the effect on the job findinghazard is initially positive and switches signs later in the spell. It is unambiguously positive in the first seven months, and unambiguously negative for spells lasting longer than a year.

Table 4.4: Simulated Average Effects of Perfect Information, for Different Discount Factors

	r=1%		r=1.5% (Baseline)		r=2%	
	Effect	% Change	Effect	% Change	Effect	% Change
Duration to Re-Empl. in Mts	-1.8	-17.3%	-1.2	-11.8%	-1.1	-10.6%
UI Benefit Payment in Euros	-1226.1	-16.0%	-839.3	-10.9%	-756.4	-9.8%
Monthly Gross Wage in Euros	23.3	1.3%	17.5	1.0%	16.6	0.9%

The counter-factual simulation is based on 1000 random independent draws for each of the 4,729 individuals in the sample. The information treatment imposes perfect information about wage offers by setting $\alpha^{sub} = 0$ and $\theta^{sub} = \theta^{obj}$. The search cost reduction reduces e_t by 10% at each month of the spell.

4.7 Conclusion

We combine subjective data on stated expectations from job seekers with realized search outcomes. We show that, on average, job seekers significantly over-estimate their future wage outcomes, by 10% on average.

We build a structural job search model that accounts for the divergence of subjective beliefs from the rational-expectations benchmark. Based on simulations of a counter-factual with perfect information, we find that wage optimism increases the duration to re-employment by around 1.2 months (12%). However, this average effect masks important effect heterogeneity: more information leads individuals to increase their search efforts over the first few months of unemployment. During this time, the information about future reductions in job offer quality raises the incentives to search for a job. For individuals who remain unemployed for longer and are already affected by worsened wage offers, information lowers the incentive to search. This implies a cautionary note for efforts aimed at improving the quality of information that unemployed individuals hold about the job search environment: care needs to be taken not to discourage job search by the long-term unemployed.

As a final implication of our study, it is possible that falsely assuming rational expectations translates into incorrect policy prescriptions. We document that expectations about wage offer distributions do not reflect the wage declines seen in actual re-entry wages. Based on this robust finding, we suggest an easy-to-implement remedy for potential mis-specification in dynamic job-search models: combine the use of actually observed declines in wage offers with the assumption of a non-dynamic subjective wage offer distribution. This procedure can serve as a useful robustness check for policy simulations whenever data on subjective wage expectations is not available.

Appendix 4.A Gross-Net Conversion

To convert gross re-employment into net terms, we exploit two main elements: (i) the theoretical tax schedule for 2008 and (ii) the fact that we observe pre-unemployment wages both in gross (administrative data) and net (survey data) terms.

We describe the theoretical relationship between gross pre-unemployment wages pre_i and net pre-unemployment wages $\tau(pre)_i$ as follows:

$$\widehat{\tau}(pre)_i = \hat{\beta}^* pre_i^{1-\mu}, \tag{4.19}$$

where $1 - \mu$ describes the curvature of the tax function, i.e., its progressivity. We proceed in two steps. We first obtain μ from the theoretical schedule governing the income taxation of a single individual in 2008. The estimated μ is 0.16. We then relate net to gross pre-unemployment wages with equation 4.19 to estimate $\widehat{\tau(pre)}_i$. The function fits the data remarkably well, with an R^2 of 0.97. Figure 4.A.2a presents the relationship between pre_i and $\widehat{\tau(pre)}_i$, and figure 4.A.2b the relationship between the predicted $\widehat{\tau(pre)}_i$ and the actual $\tau(pre)_i$ observed in the survey data. We interpret the deviation in % of $\tau(pre)_i$ from $\widehat{\tau(pre)}_i$ as a result of taxation rules applying to the individual's situation (e.g., marriage and family status etc.): $Dev_i = \dfrac{\tau(pre)_i - \widehat{\tau(pre)}_i}{\widehat{\tau(pre)}_i}$.

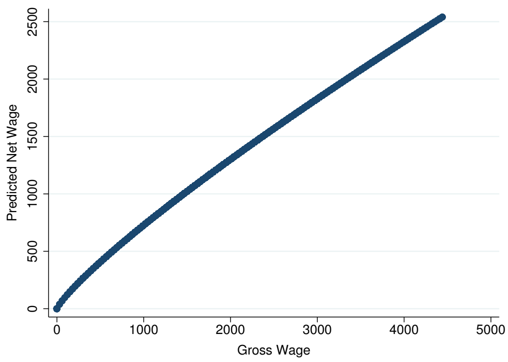
We assume that the individual-specific rules still apply after the unemployment spell, and measure the net re-employment wage as:

$$\tau(post)_i = \widehat{\tau(post)}_i + Dev_i \times \widehat{\tau(post)}_i, \tag{4.20}$$

where $\widehat{\tau(post)}_i = \hat{\beta}^* post_i^{1-mu}$ is the theoretical net gross re-employment wage, with $\hat{\beta}$ estimated from equation 4.19.

Figure 4.A.1: Pre-Unemployment Wages: Gross, Predicted Net and Net

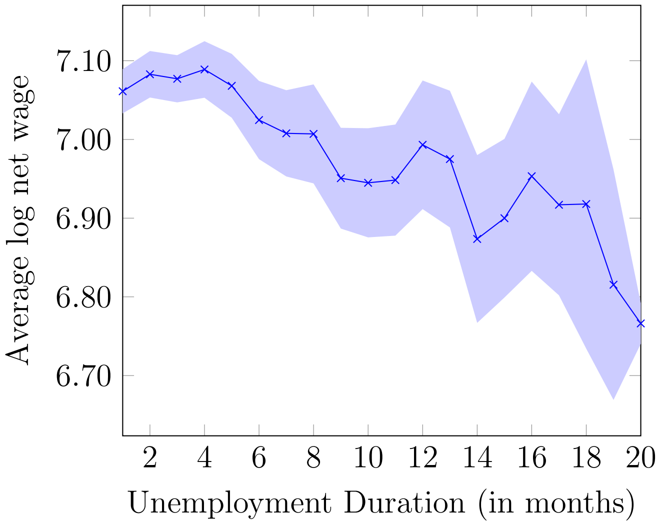
(a) Gross and Predicted Net Pre-Unemployment Wages



(b) Predicted Net and Net Pre-Unemployment Wages

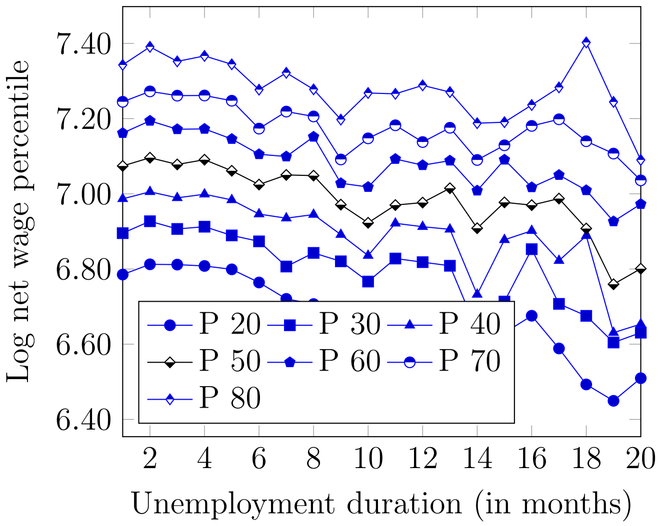
Appendix 4.B Additional Descriptive Evidence

Figure 4.B.1: Re-Employment Log Wage (Net)



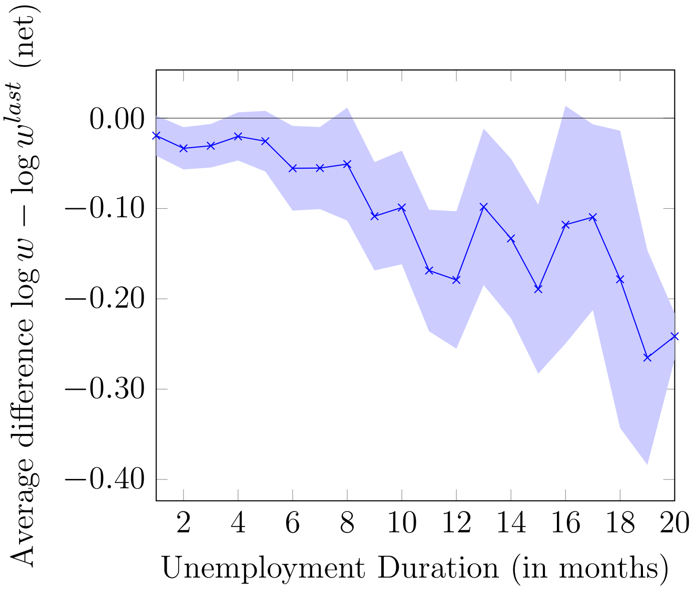
Source: IAB Employment Biographies. The shaded area shows 95% confidence bands. The graph includes individuals who enter re-employment within 20 months (N=3642).

Figure 4.B.2: Deciles of Re-Employment Log Wage (Net)



Source: IAB Employment Biographies. The graph includes individuals who enter re-employment within 20 months (N=3642).

Figure 4.B.3: Re-Employment Minus Pre-Unemployment Log Wage (Net)



Source: IAB Employment Biographies. The shaded area shows 95% confidence bands. The graph includes individuals who enter re-employment within 20 months (N=3642).

Table 4.B.1: Wage Optimism and Individual Characteristics

	Ratio Re-Entry/Expected Wage (1)	$\mathbb{1}$(Ratio < Median) (2)
Log Pre-Unemployment Wage	-0.064***	0.022
	(0.016)	(0.028)
Female	-0.024**	0.033*
	(0.011)	(0.020)
Education: Medium	0.013	-0.027
	(0.011)	(0.021)
Education: High	0.035**	-0.069**
	(0.016)	(0.029)
Prior Work Experience in Years	0.008***	-0.011***
	(0.002)	(0.004)
Prior Unemployment in Years	-0.003	-0.001
	(0.004)	(0.008)
Outcome Mean	0.896	0.486
N	2916	2916

The sample includes individuals with an observed wage expectation, re-entering employment within the observation period (20 months). "Education: Medium" takes the value one if the individual has finished the German Realschule or Fachoberschule. "Education: High" takes the value one if the individual holds the German Abitur. Individuals in the baseline category hold a lower level of education. Robust standard errors. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Appendix 4.C Additional Estimates

Table 4.C.1: Parameter Estimates: Lower Bound Discount Factor (r=0.01)

	(1)	(2)	(3)	(4)
	Subjective Expectations		*Rational Expectations*	
Parameter	Estimate	S.E.	Estimate	S.E.
Wage Offers				
μ_0				
Constant	2.830	0.056	3.152	0.018
Log Pre-UE Wage	0.621	0.008	0.567	0.002
Education: Medium	0.004	0.007	-0.006	0.011
Education: High	0.140	0.008	0.138	0.014
Work Experience in Yrs	-0.013	0.006	0.036	0.011
Previous UE in Yrs	-0.008	0.003	-0.006	0.005
θ^{obj}	0.012	0.001	0.012	0.001
θ^{sub}	-0.002	0.001		
α^{sub}	0.050	0.012		
Inverse Elasticity to Net Value of Employment				
γ	2.187	0.120	4.864	1.620
Search Costs e				
Log Pre-UE Wage	0.932	0.028	1.670	0.428
Education: Medium	-0.040	0.118	-0.027	0.196
Education: High	0.297	0.158	0.308	0.270
Work Experience in Yrs	-0.188	0.105	-0.319	0.218
Previous UE in Yrs	0.098	0.043	0.206	0.094
θ^e	0.254	0.014	0.463	0.155
σ_w	0.330	0.003	0.330	0.003
σ_ϵ	0.248	0.001		
Log L	-2.783		-2.760	
N	4,729		4,729	

The subjective expectations model estimates the likelihood specified in equations 4.9 and 4.10. The rational expectations model estimates the same likelihood, excluding the contribution of subjective expectations. As the rational expectations model does not have to fit the subjective expectations data, it has a larger likelihood value.

Table 4.C.2: Parameter Estimates: Upper Bound Discount Factor (r=0.02)

	(1)	(2)	(3)	(4)
	Subjective Expectations		*Rational Expectations*	
Parameter	Estimate	S.E.	Estimate	S.E.
Wage Offers				
μ_0				
Constant	2.841	0.029	2.852	0.027
Log Pre-UE Wage	0.620	0.004	0.612	0.003
Education: Medium	0.001	0.007	0.018	0.007
Education: High	0.138	0.008	0.123	0.014
Work Experience in Yrs	-0.013	0.006	0.016	0.007
Previous UE in Yrs	-0.008	0.003	-0.006	0.005
θ^{obj}	0.012	0.001	0.011	0.001
θ^{sub}	-0.003	0.001		
α^{sub}	0.037	0.012		
Inverse Elasticity to Net Value of Employment				
γ	2.650	0.237	4.738	0.461
Search Costs e				
Log Pre-UE Wage	1.004	0.083	1.565	0.150
Education: Medium	-0.058	0.124	0.031	0.191
Education: High	0.303	0.164	0.302	0.256
Work Experience in Yrs	-0.218	0.126	-0.307	0.195
Previous UE in Yrs	0.108	0.042	0.198	0.065
θ^{e}	0.300	0.026	0.460	0.047
σ_w	0.330	0.003	0.331	0.003
σ_ϵ	0.248	0.001		
Log L	-2.786		-2.767	
N	4,729		4,729	

The subjective expectations model estimates the likelihood specified in equations 4.9 and 4.10. The rational expectations model estimates the same likelihood, excluding the contribution of subjective expectations. As the rational expectations model does not have to fit the subjective expectations data, it has a larger likelihood value.

Summary and Overall Conclusion

Modern unemployment insurance (UI) systems include a variety of instruments to reduce the length of unemployment spells. This dissertation offers novel empirical evidence on how these instruments work in changing the job search behavior and labor market outcomes of unemployed individuals. Based on large-scale register data, the dissertation develops four research designs to identify causal effects of different institutions and incentives commonly embedded in UI: in the first chapter, I analyze how the number and quality of interactions with a caseworker affects the length of unemployment spells. The second chapter is focused on the effects of job applications imposed by search requirements. Chapter three evaluates the strictness with which requirements are enforced. Finally, chapter four studies how information about wage prospects affects job search at different stages of the unemployment spell. Chapters one to three rely on quasi-experimental designs, while chapter four uses a structural estimation approach. In the following, I briefly summarize each chapter and discuss implications for UI design as well as other policy areas.

Chapter one estimates how caseworkers affect the unemployment exit of job seekers, exploiting quasi-random variation in unplanned work absences. Importantly, absences are not analyzed as the intervention of interest, but as a source of exogenous variation in the quantity and quality of caseworker interactions experienced by unemployed individuals. Identification is ensured by the use of within-caseworker and within-month variation in absences. Based on large-scale register data from Switzerland, the findings identify 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 5% (10 days). Swiss UI benefit recipients receive on average around 3300 CHF benefits per month. According to a basic back-of-the-envelope calculation, the direct value of 40 minutes working time spent by a caseworker (average duration of a meeting) is thus estimated to be around 1100 CHF ($\approx$ 1100 USD).

The results further show that the economic value of caseworkers is highly heterogeneous. Absences of caseworkers in the lowest productivity tercile show no effect. In turn, the average return of a caseworker meeting would double if all caseworkers had on average the productivity of caseworkers in the upper tercile. This heterogeneity cannot be explained by differential usage of trainings, requirements or sanctions, suggesting that unobserved counseling skills are the main driver. Additionally, the negative effects of absences are driven by caseworkers with high tenure and experience, revealing low replaceability of caseworkers with large task-specific human capital.

The results of chapter one suggest that UI design can benefit substantially from human resource investments. On the *quantity* side, caseload reductions can increase the time spent on each unemployed individual. Investments into caseworker *quality* could target the counseling skills of existing caseworkers (e.g., through training) or the selection of individuals attracted by the caseworker profession (e.g., through higher salaries). Further, reducing the number of job separations among caseworkers may help increasing the amount of task-specific human capital.

Chapter two provides first empirical estimates of the returns to job search effort that is imposed by requirements. Across OECD countries, job search requirements typically set a minimum number of applications which individuals have to provide to avoid benefit cuts. Chapter two asks how an additional required monthly application affects re-employment prospects. The study relies on detailed Swiss register data on the monthly number of required and provided job applications. Individual requirement levels are instrumented with caseworker stringency: some caseworkers tend to set higher requirements than others. Given that the assignment of caseworkers is random conditional on basic observables, this generates exogenous variation in the individual number of required applications. The analysis reveals that one additional required monthly application reduces the duration of un- and non-employment spells by 3% (7 and 11 days, respectively). The corresponding effect of an additional provided application amounts to 4%. These results show that search requirements induce additional applications which are indeed relevant for the success of job search. They can thus be a successful tool when designing UI systems where moral hazard is not counteracted by lowering overall benefit levels, but by conditioning benefit receipt on the provision of minimum effort. However, policy makers need to be aware that required search effort does not translate perfectly into provided search effort. We quantify the elasticity of effort provision to the requirement to be 0.67. In line with theory, it further turns out that higher requirements also induce higher rates of non-compliance and benefit sanctions.

As an additional result, we find substantial heterogeneity in how individuals react to requirements. Effects of effort targets are strongest among lower-skilled job seekers. Furthermore, individuals who start off their unemployment spell with a low level of voluntary effort show stronger reactions to requirements. This suggests that there is substantial between-individual variation in effort cost and the returns to effort, which calls for a targeted design of requirement levels.

When considering the longer run, we find modest reactions of job quality to requirements. An additionally required job application causes re-employment spells to shorten by 0.3%; the effects on wages are zero. Strengthening the requirement regime thus seems only to marginally reduce job quality. It is, however, important to stress that the estimates of chapter two quantify the intensive margin effect of an additional job application. The introduction of a requirement policy, compared to the counterfactual of no such regime, could well induce stronger impacts on job quality outcomes.

Job search requirements are typically enforced through the imposition of benefit sanctions to non-compliant individuals. Chapter three of this dissertation analyzes how the strictness of enforcement, i.e., the probability that a sanction gets enforced, affects the outcomes of non-compliant individuals. Identification is based on a policy reform in the Swiss UI. The reform more than doubled the probability that a sanction gets imposed for individuals who did not provide a proof of their application activity by their deadline. Using individuals who were non-compliant for other reasons as a control group, we set up and estimate a difference-in-difference framework. We find that the increased enforcement strictness leads individuals to exit unemployment 12% faster, without however having found a job. It appears that a strict response to non-compliance lowers utility such that individuals systematically prefer searching without the compensation through unemployment benefits. This finding stands in contrast to the first two chapters, where both caseworker interacts and required job applications fastened unemployment exits through job finding. A likely explanation is that enforcement is very negatively perceived by concerned job seekers. For instance, individuals may feel treated in an unfair way, or in a way which threatens their self-esteem. Further research on the behavioral mechanisms behind exits from UI without job finding is needed to fully understand this phenomenon.

Compared to the first three chapters, chapter four studies a more subtle influence on job search behavior: changes in subjective beliefs about future wage prospects. Linked survey-administrative data on German job seekers reveal a striking pattern: individuals heavily over-estimate their re-employment wage, as the average job seeker only receives 90% of her initially expected wage. It appears that this phenomenon results from anchoring in past wages, since individuals tend to expect the wage gained prior to unemployment. Individuals do thus not anticipate that their unemployment incidence may lead to wage losses. To assess the consequences of wage optimism, chapter four sets up and estimates a dynamic job search model in which subjective expectations are not constrained to be consistent with reality. In particular, the subjective wage distribution can differ from its objective counterpart. The parameter estimates confirm the descriptive evidence: while re-employment wages depreciate over the unemployment spell, individuals do not anticipate this phenomenon. We use the model estimates to simulate a counter-factual scenario in which individuals are perfectly informed about average future wages. Results show that information has highly dynamic effects: at the beginning of the spell, it increases job search effort, as individuals understand that they risk receiving worse offers when staying unemployed for too long. At later stages, when wage offers have already worsened, information has a discouraging effect: perfectly informed individuals search less because they know that they can only get worse wage offers. This result suggests ambiguous implications of information provision in UI systems, which benefit easily employable individuals, while discouraging the long-term unemployed.

While this dissertation focuses on the parameters of UI schemes, its findings have potentially relevant implications for other areas of public policy. Chapter one identified a large impact of public human resources, which is likely to hold in other areas of publicly provided social services, such as disability insurance, (early) child care or social assistance. In particular, the chapter shows that the individual-specific quality of public employees has a large economic value. This result points towards high expected payoffs from human resource policies which improve the selection of workers into the public sector. Chapter two finds strong evidence that individual effort targets can be effective, while having largely heterogeneous effects according to the individual's voluntary effort and individual characteristics. These findings may, for instance, be of relevance to design effort incentives in firms in a targeted way. As pointed out by chapter three, the strictness with which an incentive system is enforced may have undesired side effects on individual behavior. In particular, individuals may select themselves outside the incentive system. This result may apply to different contexts where effort incentives are enforced through the threat of negative payoffs: such a scheme bears the threat of crowding out certain individuals. Finally, chapter four provides first evidence on the potential dynamics of information treatments: informing individuals on the (future) consequences of their current behavior can have different effects at different points in time. This pattern may apply to various areas of economic behavior, for instance in health or education, where the popularity of "nudging" interventions with information provision is increasing.

German Summary

In modernen Wohlfahrtsstaaten dient die Arbeitslosenversicherung zwei primären Zielen: Zum einen sollen Einkommensverluste abgefedert werden, die durch die Arbeitslosigkeit entstehen. Zum anderen soll die Re-integration in den Arbeitsmarkt mithilfe verschiedener Instrumente unterstützt werden. Die vorliegende Dissertation beschäftigt sich mit der Wirkung dieser Instrumente. Sie entwickelt vier Forschungsdesigns, um den kausalen Effekt verschiedener Anreize und Institutionen zu messen, die zahlreiche OECD-Länder in ihre Arbeitslosenversicherung integriert haben: Kapitel eins analysiert, wie die Anzahl und Qualität persönlicher Gespräche mit einem Fallberater die Dauer der Arbeitslosigkeit beeinflusst. Kapitel zwei misst den Effekt von Bewerbungen, die durch Suchvorgaben seitens der Arbeitslosenversicherung erwirkt werden. In Kapitel drei wird die Stringenz evaluiert, mit dem die Einhaltung der Vorgaben durchgesetzt werden. Schließlich untersucht Kapitel vier, wie Informationen über zukünftige Lohnperspektiven zu unterschiedlichen Zeitpunkten in der Arbeitslosigkeit wirken. Kapitel eins bis drei beruhen auf quasi-experimentellen Forschungsdesigns, wohingegen Kapitel vier einen strukturellen Schätzansatz verfolgt. Im Folgenden werden die vier Kapitel zusammengefasst und ihre Implikationen für die Gestaltung von Arbeitslosenversicherungssystemen sowie für weitere Politikfelder diskutiert.

Kapitel eins analysiert, wie individuelle Fallberater (oder "Fallmanager") den Abgang aus der Arbeitslosigkeit beeinflussen. Die empirische Untersuchung nutzt exogene Variation in ungeplanten Ausfällen von Fallberatern. Die Idee ist, dass ein Fallberater, der nicht am Arbeitsplatz erscheint, dem einzelnen Arbeitssuchenden nicht für eine Beratung zur Verfügung steht. Infolgedessen interagiert der Arbeitssuchende weniger mit dem ihm zugewiesenen Fallberater als vom System vorgesehen.

Auf der Basis von Registerdaten aus der Schweizer Arbeitslosenversicherung zeigt sich, dass Fallberater einen hohen Einfluss auf den Verlauf der Arbeitslosigkeit haben. Wenn sich die Anzahl der Gespräche mit einem Berater um 40% ($\approx$ ein Treffen) verringert, erhöht sich die Dauer der Arbeitslosigkeit um 5% (10 Tage). Das durchschnittliche Arbeitslosengeld in der Schweiz beläuft sich auf ca. 3300 CHF pro Monat. Nach einer groben Rechnung beläuft sich der direkte ökonomische Wert von 40 Minuten Arbeitszeit (=durchschnittliche Dauer eines Treffens) eines Fallberaters somit auf ca. 1100 CHF. Weiterhin zeigen die Resultate, dass der ökonomische Wert von Fallberatern stark heterogen ist. Ausfälle von Fallberatern, die im untersten Terzil der Produktivitätsverteilung rangieren, zeigen keinen Effekt. Hingegen ist der Wert eines Treffens mit einem Berater im obersten Terzil der Verteilung doppelt so hoch wie der durchschnittliche Wert. Diese Heterogenität lässt sich nicht durch eine unterschiedliche Nutzung von Instrumenten, wie z.B. Weiterbildungskursen oder Vorgaben zur Arbeitssuche, erklären. Es scheint also, dass unbeobachtbare Faktoren, wie z.B. der Gesprächsstil, einen hohen Einfluss auf die Wirksamkeit der Beratung haben. Die Studie zeigt außerdem, dass Fallberater mit relativ hoher Arbeitserfahrung einen besonders hohen Einfluss haben. Dies weist darauf hin, dass Fallberater mit hohem spezifischen Humankapital schwer zu ersetzen sind.

Die Ergebnisse des ersten Kapitels zeigen auf, dass Systeme der Arbeitslosenvesicherung substanziell von Investitionen in Humanresourcen profitieren können. Zum einen können hier geringere Fallzahlen dazu beitragen, dass jeder Arbeitssuchende ausreichend Gesprächszeit mit seinem Fallberater erhält. Zum anderen kann versucht werden, die Qualität der Gespräche zu erhöhen, indem die Qualifikation der Berater verbessert wird. Weiterhin muss es ein Ziel sein, geeignete Individuen durch attraktive Arbeitsbedingungen für den Beruf zu gewinnen.

Kapitel zwei beschäftigt sich mit der Wirkung von Bewerbungsnachweisen, die von Arbeitslosengeld-Empfängern gefordert werden. In der Mehrheit der OECD-Länder ist es Standard geworden, den Bezug von Arbeitslosengeld auf die Einhaltung verschiedener Vorgaben zu bedingen. Hierzu gehört auch die regelmäßige Erbringung einer Mindestanzahl Bewerbungen. Kapitel zwei analysiert, wie die Anzahl geforderter Bewerbungen auf die tatsächlich erbrachte Bewerbungsaktivität, sowie auf den Eintritt in die Wiederbeschäftigung wirkt. Die Untersuchung beruht auf detaillierten Registerdaten aus der Schweizer Arbeitslosenversicherung, die für jedes Individuum sowohl die geforderte, als auch die tatsächlich erbrachte monatliche Bewerbungsanzahl enthalten. Zur kausalen Identifikation wird Variation in der Stringenz von Fallberatern verwendet: Einige Fallberater setzen systematisch höhere Bewerbungsvorgaben als andere. Da die Zuweisung zu Fallberatern –konditional auf beobachtbare

Charakteristika– so gut wie zufällig erfolgt, entsteht hierdurch exogene Variation in der individuellen Anzahl geforderter Bewerbungen. Die Analyse zeigt, dass eine zusätzliche geforderte monatliche Bewerbung die Dauer der Arbeitslosigkeit um 3% senkt. Der Effekt einer tatsächlich erbrachten monatlichen Bewerbung beläuft sich auf 4%. Diese Ergebnisse veranschaulichen, dass Bewerbungsanforderungen einen relevanten Einfluss auf die Arbeitssuche haben. Gleichzeitig zeigt die Analyse jedoch auch, dass eine erhöhte Anzahl geforderter Bewerbungen zu erhöhten Nichteinhaltung der Vorgabe, und somit zu einer erhöhten Sanktionierungsrate, führt. Dieser Nebeneffekt sollte in die Entscheidung über Bewerbungsanforderungen mit einbezogen werden.

Weiterhin zeigt sich, dass der Effekt der geforderten Bewerbungen stark heterogen ist. Er ist insbesondere bei niedrig qualifizierten Individuen konzentriert. Bei hoch qualifizierten Berufen, in denen die Bewerbungsqualität vermutlich von höherer Bedeutung ist, zeigen sich geringe Effekte. Außerdem reagieren solche Individuen stärker, die auf freiwilliger Basis relativ wenige Bewerbungen erbracht haben, als sie in die Arbeitslosigkeit eingetreten sind. Dies weist darauf hin, dass die individuellen Kosten zusätzlicher Bewerbungen, wie auch der individuelle Ertrag von Bewerbungen stark variieren. Es scheint daher sinnvoll, Bewerbungsanforderungen an die Situation des einzelnen Arbeitssuchenden anzupassen.

Im Hinblick auf die längere Frist finden sich moderate negative Effekte der Bewerbungsanforderung auf die Qualität der Wiederbeschäftigung. Eine zusätzliche geforderte monatliche Bewerbung reduziert die Dauer des Wiederbeschäftigungsverhältnisses um 0.3%; der Effekt auf den Wiedereinstiegslohn ist null. Hier muss jedoch angemerkt werden, dass sich das Kapitel ausschließlich mit marginalen Veränderungen in der Bewerbungsvorgabe auseinandersetzt. Die Einführung eines Systems von Bewerbungsvorgaben scheint laut früherer Studien stärkere negative Effekte auf die Qualität der Wiederbeschäftigung zu haben.

Suchvorgaben werden typischerweise durch die Androhung einer Sanktion in Form von Reduktionen des Arbeitslosengeldes durchgesetzt. Kapitel drei untersucht, wie die Stringenz bei der Durchsetzung –also die Wahrscheinlichkeit, dass im Falle einer nicht eingehaltenen Vorgabe eine Sanktion erfolgt– auf betroffene Individuen wirkt. Die empirische Analyse beruht auf einem Politikwechsel in der Schweizer Arbeitslosenversicherung. Die Reform führte zu einem radikalen Anstieg (um mehr als 100%) der Sanktionierungswahrscheinlichkeit für Individuen, die ihre Bewerbungsnachweise nicht innerhalb ihrer Frist erbracht hatten. Vor der Reform wäre diesen Individuen eine Nachfrist gesetzt, und somit eine zweite Chance zur Erfüllung der Vorgabe eingeräumt worden. Die Reform schaffte diese Praxis ab. Der Effekt dieser Praxisänderung wird durch einen "Differenz von Differenzen"-Ansatz gemessen, bei dem Individuen, die eine andere Vorgabe nicht erfüllt haben, als Kontrollgruppe fungieren.

Die Resultate zeigen, dass von der Reform betroffene Individuen um 10% schneller aus der Arbeitslosigkeit abgehen. Jedoch sind diese Abgänge nicht durch Wiederbeschäftigungserfolge getrieben, da die betroffenen Individuen das System verlassen, ohne einen Job gefunden zu haben. Vielmehr suchen sie nun ohne Arbeitslosengeldbezüge weiter. Die erhöhte Stringenz scheint also den individuellen Nutzen des Empfangs von Arbeitslosengeld derartig zu senken, dass Individuen systematisch eine unkompensierte Arbeitssuche bevorzugen. Dieses Ergebnis steht in Kontrast zu den ersten beiden Kapiteln, wo sowohl Fallberater als auch Änderungen in Bewerbungsvorgaben die Dauer der Arbeitslosigkeit durch Wiederbeschäftigung beeinflussen. Eine wahrscheinliche Interpretation ist, dass Sanktionierungsmaßnahmen als sehr negativ wahrgenommen werden. Beispielsweise ist wahrscheinlich, dass Betroffene sie als unfair oder als das Selbstwertgefühl einschränkend empfinden. Um Abgänge aus der Arbeitslosenversicherung ohne Wiederbeschäftigung tiefergehend zu erklären, wird weitere Forschung über das Phänomen benötigt.

Schließlich beschäftigt sich Kapitel vier mit den subjektiven Erwartungen, die Arbeitssuchenden über ihre zukünftigen Lohnangebote halten. Auf Basis von Umfragedaten, die Registerdaten zugespielt wurden (IZA/IAB Evaluationsdatensatz), zeigt sich ein auffallendes Phänomen: Arbeitssuchende Individuen überschätzen den Lohn, den sie nach der Arbeitslosigkeit erlangen werden, um durchschnittlich 10%. Offenbar resultiert diese Überschätzung aus einer Verankerung der Lohnerwartungen in vorherige Löhnen, da ein Großteil der Individuen erwartet, exakt den gleichen Lohn wie vor der Arbeitslosigkeit wiederzuerhalten. Es wird also nicht antizipiert, dass jeder Monat der Arbeitslosigkeit im Durchschnitt zu Lohnverlusten führt. Um die Folgen dieses "Lohnoptimismus" zu quantifizieren, wird in Kapitel vier ein dynamisches Modell der Arbeitssuche aufgesetzt und strukturell geschätzt. Das Modell beinhaltet subjektive Lohnerwartungen, welche nicht der tatsächlich beobachteten Lohnverteilung entsprechen müssen.

Die Schätzergebnisse bestätigen die deskriptive Evidenz, dass Individuen zukünftige Senkungen in Lohnangeboten nicht antizipieren. Die Schätzergebnisse werden dann verwendet, um ein kontrafaktisches Szenario zu simulieren, in dem Individuen perfekte Information über ihre durchschnittlichen zukünftigen Lohnangebote erhalten. Die Simulation zeigt, dass die Effekte einer solchen Intervention über den Verlauf der Arbeitslosigkeit stark variieren: Zu Beginn der Arbeitslosigkeit erhöht die Information die Suchanstrengung, da Individuen zukünftige Lohnverluste durch einen schnellen Abgang aus der Arbeitslosigkeit verhindern wollen. Zu späteren Zeitpunkten hat die Information jedoch einen demotivierenden Effekt. Da die Lohnangebote nach einer gewissen Zeit in der Arbeitslosigkeit bereits an Wert verloren haben, sinkt für Individuen mit perfekter Information der erwartete Nutzen der aktuellen Jobsuche. Dieses Resultat zeigt auf, dass Informationspolitiken keine eindeutigen Konsequenzen haben. Auf der einen Seite können Individuen mit guten Wiederbeschäftigungschancen profitieren. Auf der anderen Seite können Individuen in der Langzeitarbeitslosigkeit demotiviert werden.

Auch wenn sich diese Dissertation auf Instrumente der Arbeitslosenversicherung fokussiert, beinhalten ihre Ergebnisse Implikationen für andere Politikfelder. Kapitel eins identifiziert eine bedeutende Rolle von Humanresourcen im öffentlichen Dienst. Diese sollte auf andere Sozialleistungen, bei denen der persönliche Kontakt von Bedeutung ist, anwendbar sein. Insbesondere zeigt das Kapitel, dass die individuelle Qualität von Mitarbeitern im öffentlichen Dienst einen hohen ökonomischen Wert beinhalten kann. In Kapitel zwei zeigt sich, dass Anreize die Anstrengung bei der Arbeitssuche effektiv erhöhen können, wobei dieser Effekt nach individuellen Charakteristika sowie der eigenen Motivation variiert. Diese Ergebnisse können beispielsweise für die Gestaltung von Anreizsystemen in Firmen von Relevanz sein. Kapitel drei weist darauf hin, dass die Stringenz, mit der ein Anreizsystem durchgesetzt wird, unerwünschte negative Konsequenten auf individuelles Verhalten haben kann. Insbesondere zeigt das Kapitel, dass Individuen sich komplett aus dem Anreizsystem herausselektieren können, wenn sie von Sanktionierungsmaßnahmen betroffen werden. Dieses Resultat kann auf verschiedene Kontexte angewendet werden, in denen Anreize durch die Drohung mit Sanktionen durchgesetzt werden. Schließlich illustriert Kapitel vier die möglichen Dynamiken von Informationspolitiken: Wenn Individuen über die (zukünftigen) Konsequenzen ihres aktuellen Verhaltens informiert werden, kann dies je nach Zeitpunkt unterschiedlich wirken. Dieses Phänomen kann für zahlreiche Bereiche ökonomischen Handelns relevant sein, so beispielsweise im Gesundheits- oder Bildungsbereich, wo die Bedeutung von Information für Entscheidungen groß ist.

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