How Do Reductions in Potential Benefit Duration Affect Medium-Run Earnings and Employment?

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January 2013

Abstract

We study how a reduction of the potential duration of unemployment benefit receipt (PBD) affects medium-run earnings and employment of job seekers. The analysis is based on a Swiss reform that reduced PBD from 24 months to 18 months for job seekers younger than 55 years in 2003. Adopting a difference-in-difference framework, we find that this reduction in PBD increases employment and earnings of job seekers aged 50 to 54 years not only in the first 24 months after entering unemployment but also up to 50 months after entering. The positive medium-run effects are concentrated among job seekers who were previously employed in R&D intense industries and whose previous occupation consisted mainly of cognitive tasks. These findings suggest that unemployment insurance has an important role in containing depreciation of human capital or long-term unemployment stigma among older job seekers.

JEL Classification: C41, J64, J65

Keywords: potential benefit duration, unemployment duration, earnings, employment, policy change

*We would like to thank Thierry Mayer and seminar audiences in Lausanne and Zurich for comments. Romain Mayer provided excellent research assistance. This paper was supported by the NCCR LIVES. Addresses: Kathrin Degen, University of Lausanne, Department of Economics, Internef, CH-1015 Lausanne, kathrin.degen@unil.ch. Rafael Lalive, Department of Economics, University of Lausanne, CH-1015 Lausanne-Dorigny, Rafael.Lalive@unil.ch. Rafael Lalive is also associated with CESifo, IFAU, IfW and University of Zurich (IEW).

1 Introduction

The global crisis that erupted in 2008 put around 25 million worker out of a job (ILO, 2012). Unemployment insurance (UI) is the key first safety net to workers and probably the most important program to feather the effects of crises. This is perhaps why all OECD member countries currently have a system of unemployment insurance. Yet the details of their systems vary a lot.[1] Recent research shows that details of an UI system matter and how they matter for optimal policy. Yet this literature has typically assumed that PBD does not affect the types of jobs that people get after leaving unemployment.

This paper studies whether PBD matters for earnings and employment up to 50 months after entering unemployment. Understanding whether PBD matters beyond unemployment is important. First, a policy assessment of changes to PBD that focuses only on its impacts on the government budget is too narrow. The fiscal benefit of reducing PBD comes at a large and potentially long-term cost if reductions to PBD deteriorate post unemployment job quality. Conversely, shaving off a few weeks of PBD might carry a double dividend if reduced PBD improves labor market chances. A pure policy assessment therefore requires more information on the post unemployment effects of PBD. Second, available optimal UI formulas for optimal UI currently ignore its effects on post unemployment jobs (Chetty, 2008; Schmieder et al., 2012a). These formulas need to be adapted if PBD affects job quality.

Theory does not offer guidance as to how longer benefit duration affects post-unemployment outcomes. On the one hand, according to standard search theory, shorter PBD forces job seekers to be less selective and prevents them from waiting for better job offers (Mortensen, 1977; van den Berg, 1990). This is likely to decrease post-unemployment wages. Also, job match quality might be reduced and subsequent jobs would then end earlier. On the other hand, in a context where human capital depreciates quickly, shortened unemployment duration preserves human capital and by doing so improve the quality of jobs offered to workers (Shimer and Werning, 2006). Alternatively, firms may use unemployment duration as a screening device (Gibbons and Katz, 1992). Shortening unemployment duration again improves labor market chances of job seekers (Oberholzer-Gee, 2008).

We study a reform to Swiss unemployment insurance that cut PBD from 24 months

[1] For instance, the net replacement rate for a family earning the average production worker wage with two children ranges from 55 percent in New Zealand to 92 percent in Luxembourg in the initial phase of unemployment. The picture is different for the long-term unemployed (4 to 5 years into the unemployment spell). A two children family earning the average production worker wage sees 41 percent of that wage replaced in Greece but up to 72 percent in Denmark. This shows that the degree to which benefits are maintained in the course of the spell also varies tremendously across OECD members.

to 18 months for job seekers who were younger than 55. This reform, enacted in July 2003, can be used to measure the role of shorter PBD for older workers in a differences-in-differences design. As expected, we find that the reform significantly reduced monthly unemployment benefit payments by 132 CHF (109 EUR or 141 USD) in the period 18 to 24 months after entering unemployment, or about 5 percent of average benefits in the second month after unemployment start. Job seekers compensate this reduction in benefits by leaving unemployment for jobs thus increasing employment by 3.1 percentage points (pctp) and labor earnings by 191 CHF. Interestingly, we find that the positive effects of the benefit reduction *persists* beyond the period that is insured by UI. Specifically, employment remains 2.3 pctp higher and earnings stay at 187 CHF higher than expected from the evolution in the control group. A range of sensitivity analyses suggest these effects are not spurious. Sub-sample analyses indicate that the post-UI effects are especially important for job seekers coming from R&D-intense industries and for individuals whose previous occupation required cognitive skills. These analyses suggest that the beneficial effects of reduced depreciation of human capital or improvements in non-employment stigma outweigh the negative effects of reduced reservation wages.

This paper is related to at least three strands of literature. The first strand discusses reduced form evidence on the effects of PBD on unemployment duration. Several US studies estimate the effects on the exit rate from unemployment of variations in PBD that take place during recessions.[2] Early studies, including Moffitt and Nicholson (1982), Moffitt (1985), and Grossman (1989) find significantly negative incentive effects. Meyer (1990) and Katz and Meyer (1990) show that the exit rate from unemployment rises sharply just before benefits are exhausted. Such spikes are absent for non-recipients. More recent work by Addison and Portugal (2004) confirms these findings.[3] A common objection against these studies is policy endogeneity. Benefits are typically extended in anticipation of a worse labour market for the eligible workers. Card and Levine (2000) exploit variation in benefit duration that occurred independently of labour market condition and show that policy bias is substantial. Lalive and Zweimüller (2004a,b) show similar evidence for the Austrian labour market. Evidence on the effect of PBD in European studies is mixed. Hunt (1995) finds substantial dis-

[2]Fredriksson and Holmlund (2006) give a recent overview of empirical research related to incentives in unemployment insurance. See Green and Riddell (1997, 1993), and Ham and Rea (1987) for studies that focus on Canada.

[3]Note that there is no theoretical explanation for the existence of end-of-benefit spikes. It could be that the spikes have to do with strategic timing of the job starting date, i.e. workers have already found a job but they postpone starting to work until their benefits are close to expiration. Card and Levine (2000) point at the possibility that there is an implicit contract between the unemployed worker and his previous employer to be rehired just before benefit expire.

incentive effects of extended benefit entitlement periods for Germany. Carling et al. (1996) find a big increase in the outflow from unemployment to labour market programs whereas the increase in the exit rate to employment is substantially smaller. Winter-Ebmer (1998) uses Austrian data and finds significant benefit duration effects for males but not for females. Roed and Zhang (2003) find for Norwegian unemployed that the exit rate out of unemployment increases sharply in the months just prior to benefit exhaustion where the effect is larger for females than for males. Puhani (2000) finds that reductions in PBD in Poland did not have a significant effect on the duration of unemployment whereas Adamchik (1999) finds a strong increase in re-employment probabilities around benefit expiration. van Ours and Vodopivec (2006) studying PBD reductions in Slovenia find both strong effects on the exit rate out of unemployment and substantial spikes around benefit exhaustion. Schmieder et al. (2012a) discuss the effects of extended PBD for benefit duration and non-employment duration over 20 years for Germany.

The second strand of the literature discusses whether changes to PBD affect post unemployment job quality. Ehrenberg and Oaxaca (1976) were the first to look at the effect of unemployment insurance on post unemployment outcomes and find positive effects of unemployment benefits on post unemployment wages for different age groups and gender. Addison and Blackburn (2000) provide evidence for a weakly positive effect of unemployment benefits on post unemployment wages. Centeno and Novo (2006) use a quantile regression approach to analyze the relationship between the unemployment insurance system and the quality of subsequent wages and tenure over the whole support of the wage and tenure distributions. They find a positive impact of unemployment benefits on each quantile of the wage and tenure distribution. van Ours and Vodopivec (2008) analyse how a change in Slovenia's unemployment insurance law affected the quality of post-unemployment jobs. Using a difference-in-difference approach, they find that a reduction in the potential benefit duration has only small effects on wages, on the duration of subsequent employment and on the probability of securing a permanent rather than a temporary job. In a companion paper, van Ours and Vodopivec (2006) found that a reduction in potential benefit duration increased the exit rate from unemployment. The authors conclude that a shorter potential benefit duration decreased unemployment duration without deteriorating the quality of post-unemployment outcomes. They argue that their findings strongly suggest the presence of strategic opportunistic behavior. Caliendo et al. (2012) use a regression-discontinuity approach to identify the causal effect of an extended benefit duration on unemployment duration and on post unemployment outcomes using German data. They find a spike in the re-employment hazard as it is common

in similar empirical studies. Further they show that the unemployed who obtain a new job close to benefit exhaustion are more likely to exit subsequent employment and receive lower wages than than their counterparts with extended benefit duration. Centeno and Novo (2009) use sharp discontinuities in the eligibility of unemployment benefits in Portugal to identify the existence of a liquidity effect of the unemployment insurance system. In particular, they detect a positive impact in the match quality for the most liquidity constrained individuals, i.e. the individuals at the bottom of the wage distribution. Deroyon and Le Barbanchon (2011) use French administrative data to investigate the effect of potential benefit duration on unemployment exits and job quality using a regression-discontinuity design. They find a significant and large effect of benefit duration on unemployment exits to work. They also find evidence for a positive effect of benefit duration on starting wages. However, they do not find any evidence that prolonged benefit duration leads to longer lasting post unemployment employment spells. Finally, Schmieder et al. (2012b) analyze the long-term effects of extensions in UI durations taking into account not only the initial, but also all recurrent nonemployment spells. They find significant long-run effects of an extension in UI duration on the duration of nonemployment up to three years after the start of the initial spell.

The third literature discusses policy design. Starting from the original insight of Baily (1978), Chetty (2008) use the evidence by reduced form studies to discuss whether the level of unemployment benefits is set so as to maximize welfare.[4] Schmieder et al. (2012a) discuss optimal potential benefit duration over the business cycle. Haan and Prowse (2010) discuss the employment, fiscal and welfare effects of unemployment insurance using a structural life-cycle model allowing for endogenous accumulation of experience. They conclude that from a welfare point of view, reductions of benefit entitlement should be favored over reforms of replacement rate reductions.

Our paper complements existing studies on the post unemployment effects of PBD. We focus on employment and earnings, outcomes that can be observed for *all* job seekers. In contrast, by focusing on wages and sub-sequent tenure, the existing literature analyzes outcomes that are only observed for job seekers who find employment. We also adopt a longer time window and focus on the global outcomes employment and earnings. Doing so allows picking up not only short-term immediate effects but also effects that build up over time. For instance, if shortened PBD improves job seekers' leadership skills, one might see this effect only in a longer-run context. Furthermore, sub-group analyses by industry and occupation of previous job shed light on the role of reduced human capital and skill depreciation as a potential explanation

[4]Also, see Chetty (2009) for a general description of the sufficient statistics approach.

for positive medium run effects. Finally, we conceptually contribute to the literature by discussing results in a novel and encompassing framework.

The remainder of this paper is structured as follows. Section 2 discusses the institutional background. Section 3 provides information on the data sources and a set of key descriptive statistics. Section 4 discusses the econometric framework and our main identification strategies. Section 5 presents the main results, and section 6 provides a summary and implications of our findings.

2 Institutional Background

This section discusses the relevant background on unemployment insurance, earnings, and employment in Switzerland. Job seekers are entitled to unemployment benefits if they meet two requirements. First, they must have paid unemployment insurance taxes for at least six months in the two years prior to registering at the public employment service (PES). The contribution period is extended to 12 months for those individuals who have been registered at least once in the three previous years. Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job - they must be "employable". If a job seeker is found not to be employable there is the possibility to collect social assistance. Social assistance is means tested and replaces roughly 76 % of unemployment benefits for a single job seeker with no other sources of earnings (OECD, 1999).

Prior to July 1, 2003, job seekers were eligible for 520 daily payments of benefits during a two year framework period. Those 520 benefit days are equivalent to two years of potential benefit duration since the calendar year is composed of 260 working days in the Swiss UI benefit system. The replacement ratio is 80 % for workers earning less than 3,536 CHF.[5] prior to unemployment and are not caring for children. The replacement rate decreases gradually to 70 % for job seekers who earned between 3,536 CHF and CHF 4030 and it stays at 70 % thereafter. Benefits insure monthly earnings up to a top cap of 8,900 CHF. Job seekers have to pay all earnings and social insurance taxes except the unemployment insurance tax rate (which stands at about 2 %). This means that the gross replacement rate is similar to the net replacement rate. Job seekers keep these entitlements during a framework period of two years. This means that a job seeker who experiences a spell of 3 months of unemployment and earns 80 % of his insured income remains eligible for the same unemployment

[5] 1 CHF = 0.83 EUR.

benefits for an additional 21 months regardless of how often he leaves and re-enters unemployment. After the framework period of 2 years has ended, the job seeker needs to re-qualify for benefits.[6]

The July 2003 reform changed a range of aspects of the benefit system. First, the reform now requires everyone to have contributed for at least 12 out of the 24 months prior to registering for unemployment benefits. Second, the reform reduced PBD for individuals below the age of 55 years to 400 daily benefit payments, or to 18.5 months.[7] Job seekers aged 55 years or older who had contributed for at least 18 months prior to entering unemployment remained unaffected by the reform. Yet job seekers aged 55 years or older who had only contributes between 12 and 17 months to UI also experienced a cut in PBD. Third, the reform increased benefit levels somewhat for low to medium earners to reflect inflation adjustment. In order to achieve this objective, the replacement rate was kept at 80 % for job seekers with insured earnings of up to 3797 CHF and then gradually reduced over the earnings bracket 3797 to 4340 CHF.

From an identification point of view, the following issues are crucial. First, there were no concurrent changes to other social insurance programs in the period around the 2003 reform. This ensures that our estimates pick up the specific consequences of the reform rather than changes to other social programs. Second, benefit rules depend on current age of individuals rather than on age at registry. Also, reforms to the UI system apply to all job seekers, not just to those who register after the reform. We will discuss below how we take this into account in our estimation framework. Third, the reform was signed into force around a time when the Swiss labor market situation was deteriorating. The unemployment rate reached a low of slightly over 1.5 % in the first quarter of 2001, increased considerably after the bursting of the ".com" bubble to a high of 4 % in the last quarter of 2003. Unemployment decreased first slightly then more rapidly to reach a trough of 2.5 % in the second quarter of 2008. Deteriorating aggregate demand for work is likely to introduce a downward bias into our estimates (in absolute value). Our estimates should be read as lower bounds on the true effects of benefit duration reductions.

[6] Also, a repeatedly unemployed job seeker who qualifies for a new framework period with more advantageous benefits moves to this new framework period regardless of his status with respect to unemployment insurance.

[7] A year counts 260 benefit days. A job seeker who is eligible for 400 benefit payments can therefore claim benefits for 18.46 (=400/260 * 12) months.

3 Data and Descriptive Statistics

This section discusses the data and provides first descriptive evidence on the effects of PBD on medium run earnings and employment.

3.1 Data

The study is based on administrative records of the unemployment insurance register (UIR) database covering information on all individuals registering with the public employment service (PES) between 1999 and 2007. This can be job seekers who are eligible for unemployment benefits, but also individuals who ask the public employment service for assistance.

The UIR contains the exact date when a job seeker can start a new job – the unemployment start-date.[8] The UIR also contains information on when the job starts that a job seeker has found – the job start date. We measure the duration of unemployment as the number of days elapsed between the unemployment start-date the job start-date if those two pieces of information are available. We use the date when the file of a job seeker was closed as a proxy for the unemployment end-date for individuals who do not start a new job. The database also contains socio-demographic characteristics such as gender, age, education, and marital status.

We use information on unemployment benefit payments, employment and earnings from the Swiss social security data (SSD). This data covers a 25 % sample of the universe of all individuals who have contributed to the mandatory first pillar retirement pension system between the period between 1982 and 2008. The social security database can be merged to the unemployment insurance register data through a unique person identifier. The data provides monthly information about earnings from employment, income from non-labor such as unemployment benefits, and also disability and old-age retirement pensions. We extract a history of 50 months before, and 50 months after the beginning of each unemployment spell from SSD for each unemployment spell.

From the merged database containing unemployment register, and social security data, we make a number of additional sampling restrictions. First, we only consider individuals aged between 50 and 59 years at the start of the spell of unemployment, in order to avoid confounding effects because of early retirement considerations[9] Fur-

[8]The data also contains date of registration and de-registration. The registration date does not correspond to the start date of the unemployment spell because job seekers need to register with the PES the moment they know they will loose a job. This is typically a quarter before they actually loose their job.

[9]In addition to that, as a sensitivity test to our main estimates, we also look at a restricted sample that excludes the oldest age cohort of the control group, but do not find evidence that would impair our main findings.

ther, the sample contains only individuals who contributed to the unemployment insurance for at least 12 of the last 24 months before getting unemployed. Lastly, we only consider individuals who are fulltime unemployed at least in the first unemployment month. The final sample contains 24'653 spells.

3.2 Treatment and Control Groups

Table 1 provides information on how treatment and the control groups are defined. Individuals aged below 55 at the start of their unemployment spell are assigned to the treatment group, and individuals aged 55 or older are assigned to the control group. Treatment assignment is solely based on age, because the information about prior UI contributions is not available for the whole sample. Nevertheless, over 85% of our sample claimed unemployment benefits within 3 months after unemployment start, so that eligibility issues should not play a major role. Note however that the control group partly contains treated individuals, so the effects we find should be interpreted as a lower bound to the true effects.

Table 1: Treatment assignment

Age	Prior UI contributions	Benefit entitlement before	Benefit entitlement after	Group
< 55	12 months	520	400	Treatment
≥ 55	12 months	520	400	Control
≥ 55	18 months	520	520	Control

Notes: Table 1 shows the treatment assignment, which is based on the age at unemployment start.

For each individual unemployment spell we observe a history of monthly unemployment benefits, earnings from employment around unemployment start of up to 50 months before, and up to 50 month after unemployment start.[10] We construct a binary indicator on employment that takes the value 1 if the job seeker has generated positive earnings from employment, and zero otherwise. We observe 11'941 spells of job seekers whose unemployment spell started before the reform was implemented on July 1st, 2003 – 5205 in the treatment group, and 6736 in the control group (table 2). We observe 12'712 unemployment spells starting after July 1st, 2003 – 5549 spells belong to the treatment group and 7163 belong to the control group. Table 2 presents selected summary statistics for the treatment ($D_i = 1$) and control (D_i) group for spells that start before and after the reform.

[10]Individuals can appear multiple times in our sample: For 13 % of the individuals in the sample, we observe two or more spells.

Table 2: Selected descriptive statistics (means)

	Before reform $A_c = 0$		After reform $A_c = 1$	
Treatment status	$D_i = 0$	$D_i = 1$	$D_i = 0$	$D_i = 1$
Dependent variables (prior to unemployment)				
Unemployment benefits	261.85	238.41	243.16	247.07
Employment	0.87	0.87	0.88	0.87
Earnings	4556.15	4621.17	4593.2	4639.01
Control variables				
R&D intensity	0.51	0.5	0.47	0.45
Cognitive	0.5	0.46	0.46	0.47
Prior work exp.	0.66	0.66	0.71	0.67
Female	0.42	0.43	0.43	0.45
Swiss	0.72	0.68	0.73	0.68
Leader position	0.73	0.69	0.72	0.7
Marital status				
Single	0.09	0.11	0.1	0.12
Married	0.67	0.65	0.63	0.63
Widow	0.04	0.03	0.04	0.03
Divorced	0.2	0.21	0.22	0.22
Years of schooling				
$\leq$ 7 years	0.04	0.05	0.04	0.04
8-9 years	0.04	0.05	0.04	0.04
10-11 years	0.05	0.04	0.05	0.05
12-13 years	0.28	0.27	0.39	0.37
$\geq$ 14 years	0.06	0.06	0.08	0.09
Other	0.44	0.45	0.28	0.28
No of observations	520,300	673,379	502,031	645,795
No of spells	5205	6736	5549	7163

Notes: Table 2 shows means of selected variables for the treatment and control group for individuals who registered before or after July 1, 2003 respectively. R&D intensity is a dummy that equals to one if the R&D-intensity of the industry of the previous employer is above median. Cognitive is a dummy that equals one if the previous occupation of a job seeker is mainly cognitive. Prior work exp. shows the proportion of individuals who were continuously employed during at least 24 months prior to their unemployment spell.

Job seekers in the control group claimed on average 262 CHF per month of unemployment benefits in the period 50 months before entering unemployment. Treatment group job seekers earn benefits that are 25 CHF unemployment lower that control group job seekers before the reform, and virtually the same for unemployment starts after the reform. Employment probabilities are basically identical for treatment and control group, for both, unemployment starts before and after the reform. Earnings are on average slightly higher in the treatment group before and after the reform. This difference might be explained by the fact that the treatment group is on average younger than the control group and is less likely to early retire.

R&D intensity is a dummy that equals one for job seekers whose previous employer is active in a R&D-intense industry (splitted by median).[11] As expected the dummy

[11] R&D intensity of an industry is the average expenditures for R&D for the neighboring countries of Switzerland (Germany, Austria, France and Italy) over the years 2005 to 2008 at the two digit NACE level. We merge this information to each job seeker based on industry prior to loosing job. R&D in-

varies around 0.5 for treated and untreated before the reform. However, after the reform, the proportion of job seekers from R&D-intense industries decreases slightly to around 0.47 and 0.45 respectively. Cognitive is the proportion of job seekers whose previous occupation consisted mainly of cognitive tasks.[12] Before the reform, the proportion is 0.5 for untreated and 0.46 for treated individuals respectively. After the reform, the proportion of mainly cognitive skilled job seekers in the control group decreases to 0.46, and the one of the treatment group increases slightly to 0.47. Prior work experience is the proportion of job seekers who with a continuous work experience of at least 24 months prior to their unemployment spell. The proportion of job seekers with a long work history is around two thirds for spells that started before the reform. After the reform, this proportion slightly increases to 0.71 for individuals in the control group, and stay virtually unchanged for the treatment group. The share of female job seekers varies between 42 % and 45 %. The proportion of Swiss citizens is fairly stable for unemployment spells starting before and after the reform, and amounts to 68 % in the treatment group and around 72 % in the control group. Around 73 % of the individuals in the control group, and roughly 70 % of the individuals in the treatment group worked in a leader or expert position. There are no large differences between the four groups relative to their marital status: Around two thirds of the individuals are married, one fifth is divorced, roughly 10 % are singles and 4 % are divorced. The largest differences between unemployment starts before and after the reform are found for years of schooling: The share of individuals with less than 7 years of schooling, between 8 to 9 years of schooling, between 10 and 11 years, and those with more than 14 years of schooling remains fairly stable over time and across treatment and control groups. The share of individuals with 12 to 13 years of schooling, however, increases largely from 28 % before to almost 40 % after the reform. At the same time the share of individuals for whom the attained education level is unknown decreases from 45 % to 28 % over time. Thus, changes in data quality account for this substantial shift in measured education levels. This shift af-

tensive industries are those that have expenditures that exceed the median expenditure, the remaining industries representing the low R&D industries. High R&D industries are for example manufacture of chemicals and pharmaceuticals, manufacture of computer, electronic and optical products, manufacture of machinery, equipment and motor vehicles, or industries in professional, scientific and technical activities.

[12]For the classification of occupations into cognitive and manual task content, we follow an approximation suggested in Acemoglu and Autor (2011). The autors propose a simple classification of occupations into four broad task dimensions: (1) abstract, non-routine cognitive tasks, (2) routine cognitive tasks, (3) routine manual tasks, and (4) non-routine manual tasks. We further condense the first and second category into a "cognitive tasks" group, and the third and fourth into a "manual tasks" group. The most important occupations requiring cognitive skills are engineers, clericals and occupations in administrative support, sales, and education. The most important occupations requiring manual skills are occupations in construction, in production and manufacture of raw materials, and in services and housekeeping.

fected treated and untreated individuals in the same way, and will not invalidate our identification strategy.

4 Econometric Framework

This section presents an econometric analysis of the effects of PBD on employment and earnings, and discusses the underlying identification assumptions. The specific design of the reform creates a natural control group for which the benefit entitlement remained unchanged, and a treatment group for which the PBD was reduced from 24 months (520 days) to 18 months (400 days). In order to discuss estimation and idenfication assumption, let $Y(1)$ be the treated outcome, and $Y(0)$ the non-treated outcome. $D \in \{0,1\}$ is a treatment indicator that is 1 if an individuals receives treatment, i.e. is below 55 years old at unemployment start, and 0 else. Let Y_0 denote the outcome prior to the reform, and Y_1 the outcome after the reform. The observed outcome after the reform can then be written as $Y_1 = DY_1(1) + (1-D)Y_1(0)$. The difference-in-difference estimator is then given by

$$DiD = [E(Y_1 \mid D=1) - E(Y_1 \mid D=0)] - [E(Y_0 \mid D=1) - E(Y_0 \mid D=0)]$$

The difference-in-difference estimator identifies the average treatment effect on the treated by comparing differences in outcomes between the outcomes of the treated and the untreated before and after the reform. The main assumption that has to hold for the difference-in-difference estimator to identify the average treatment effect on the treated in repreated cross sections are parallel time trends for the treatment and control group in absence of the treatment, i.e. $E(Y_1(0)-Y_0(0) \mid D=1) = E(Y_1(0)-Y_0(0) \mid D=0)$.[13]

We test this assumption in section 5 by analyzing the time trends of the outcomes for the treatment and control group and find that time trends for unemployment benefits, employment and earnings are equal in periods that were not affected by the 2003 reform. We take this as an indicator that the main identification assumption of equal time trends is not violated. In this case, the difference-in-difference estimator can be rewritten as

$$DiD = E(Y_1(1) - Y_1(0) \mid D=1)$$

and identifies the average treatement effect on the treated.

[13] See also Lee and Kang (2006) for a detailed discussion of the identification assumptions in repeated cross sections.

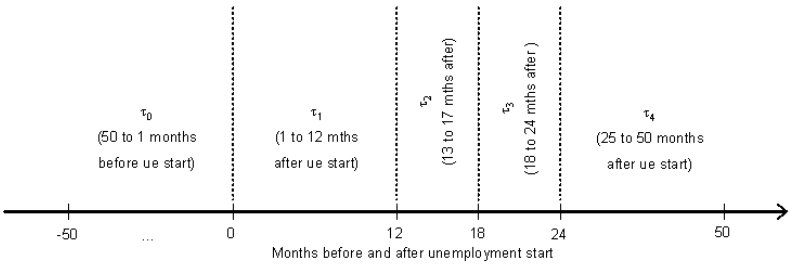
5 Results

This section discusses the estimation results. Subsection 5.1 presents graphical evidence, subsection 5.2 presents the main estimation, subsection 5.3 discusses some sensitivity estimations, and subsection 5.4 analyses the issue of heterogeneity in treatment effects.

5.1 Descriptive evidence

Figure 1 shows the structure of the data. We can distinguish five periods: τ_0 is the period before unemployment start, i.e. 50 to 1 months before registering. τ_1 marks the period 1 to 12 months after unemployment start. In this period, treatment and control group are both entitled to benefits. τ_2 identifies the period 13 to 17 months after unemployment start, where treated - like the untreated - still get unemployment benefits. In this period anticipation effects start to play a role, because unemployment benefits of the treated will run out soon. τ_3 is the period 18 to 24 months after unemployment start, and is directly affected by the reform. During this period, untreated individuals still get benefits, whereas treated individuals are no longer entitled. This period captures the direct or mechanic effect of the reduced PBD. Finally, τ_4 captures the period 25 to 50 months after unemployment start and allows to identify medium run effects of the PBD.

Figure 1: Data structure



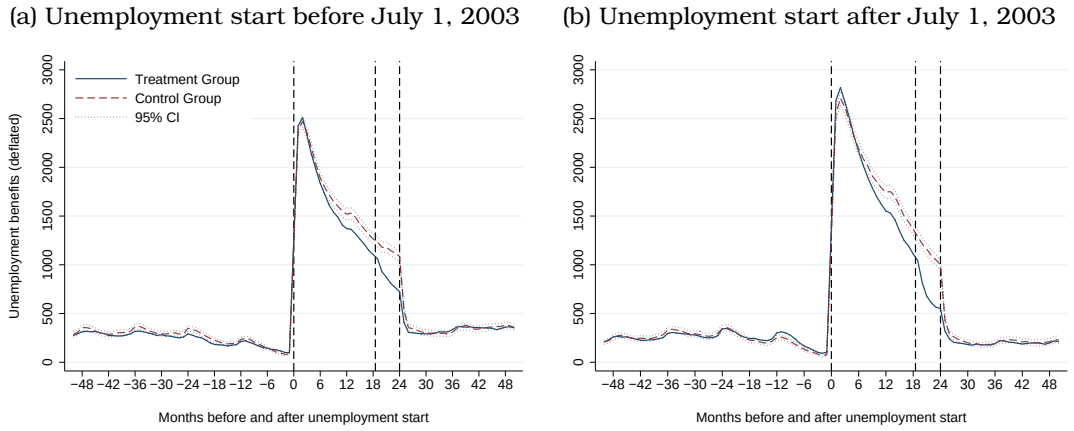
Notes: Figure 1 shows the data structure with its division into τ_0 to τ_4.

There are three issues with this data structure: First, we cannot observe the full history of 50 months after the beginning of unemployment for spells starting after November 2004 since our observation period ends in December 2008. This lack of observation window should, however, not impair our identification strategy, because both, control and treatment groups, are affected by this gradual sample reduction in the same way. Second, due to the treatment assignment which is based on age at unemployment start, individuals in the treatment group gradually "grow" into the

control group over time. For example, an individual who is 54 at the start of his unemployment spell will grow into the control group at most 12 months after the start of unemployment. We therefore potentially underestimate the true effects and, again, effects should be regarded as lower bounds. Third, the 2003 reform affected both benefit duration and benefit level. However, this fact is unlikely to affect our results because the change to benefit level affected a narrow income bracket earning between 3,500 CHF and 4,300 CHF, and it targeted job seekers without dependents, a minor fraction of our sample.

Figure 2 shows the average unemployment benefits for the treated (50 to 54 years old) and untreated (55 to 59 years old) 50 months around their unemployment start date. The vertical line at time 0 identifies the start of unemployment. The vertical line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical line at 24 months marks the old exhaustion date before the reform, and the benefit exhaustion date for the control group after the reform respectively. Figure 2a depicts average unemployment benefits for individuals who registered before the policy change in July, 2003 and figure 2b illustrates the same situation for individuals who registered after the reform in July, 2003.

Figure 2: Unemployment benefits before and after the reform



Notes: Figure 2a shows aggregate unemployment benefits 50 months before and 50 months after unemployment start for individuals who entered unemployment before July 1, 2003. The benefit history is shown for both, the treatment group (< 55 years old at unemployment start) and the control group (≥ 55 years old at unemployment start). Figure 2b shows the benefit history for unemployment spells that started after July 1, 2003. The dotted lines around the benefit history of the control group indicate the 95 % confidence interval.

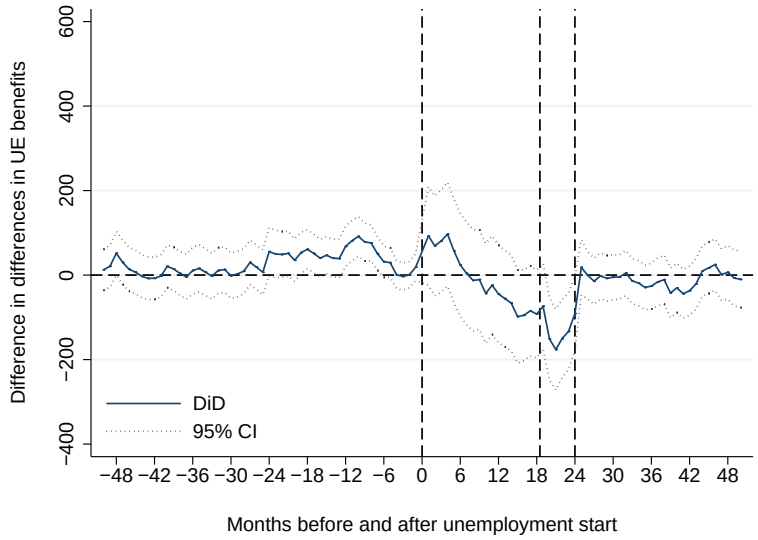
Unemployment benefits do not differ between the treated and the untreated before the start of the unemployment spell. Average unemployment benefits range between 72 and 368 CHF per month due to seasonal patterns. Pre-unemployment benefits are not exactly zero, because there can be spells of unemployment before the one we analyze. After registering at the PES, job seekers are entitled to unemployment bene-

fits.[14] This is observed in the data by a sharp increase in average benefits to around 2500 CHF in the first month after unemployment start. Unemployment benefits drop as time elapses because job seekers gradually re-enter employment or exit the labor force through alternative pathways. The benefits of treated and untreated start to diverge after the peak around unemployment start: Average benefits of the treated are lower than those of the untreated. 12 months after the start of a spell there is a kink for both groups. The kink is due to the benefit exhaustion for job seekers who are exempted from the contribution requirements. They can claim a maximum of 260 days of benefit payments, which is equivalent to 12 months. For the treated group, there is another a kink after 18.5 months (equivalent to 400 days) after the beginning of unemployment: This marks the benefit exhaustion date for the treated group after the reform. Note that the kink is also observed *before* (figure 2a) the UI policy change because job seekers in the treated group gradually get affected by the reform even for spells that started before the 2003 reform. But the kink is more pronounced in the data covering job seekers who enter *after* the reform consistent with a larger treatment intensity among this group. After 24 months (equivalent to 520 days), benefits also end for the control group. Average unemployment benefits sharply drop, and fall back to almost its pre-unemployment level thereafter.

Figure 3 highlights the above observations. It shows the difference in differences between the treated and the control group before and after the policy change. In the pre-unemployment period τ_0 (50 to 1 month before unemployment start), the difference in differences is close to zero and not significantly different from zero (except for the period between 12 and 6 months before unemployment start). Around 6 months after the beginning of a spell, the difference in differences starts to get negative, reaching its minimum in the treatment period τ_3 (18 to 24 months after). Between 18 and 24 months after unemployment start one can thus observe a negative and significant treatment effect for unemployment benefits. This is the direct and purely mechanic effect of cutting PBD by 6 months for the below 55 years old job seekers. Beyond 24 months, the difference in differences almost immediately reverts to zero.

[14]Note that the unemployment start date is defined as the potential entry date for the next job. According to our sample definition, individuals thus fulfill the eligibility for daily benefit payments, conditional on being "employable". Indeed, 85 % of the sample claims unemployment benefits within 3 months after unemployment start.

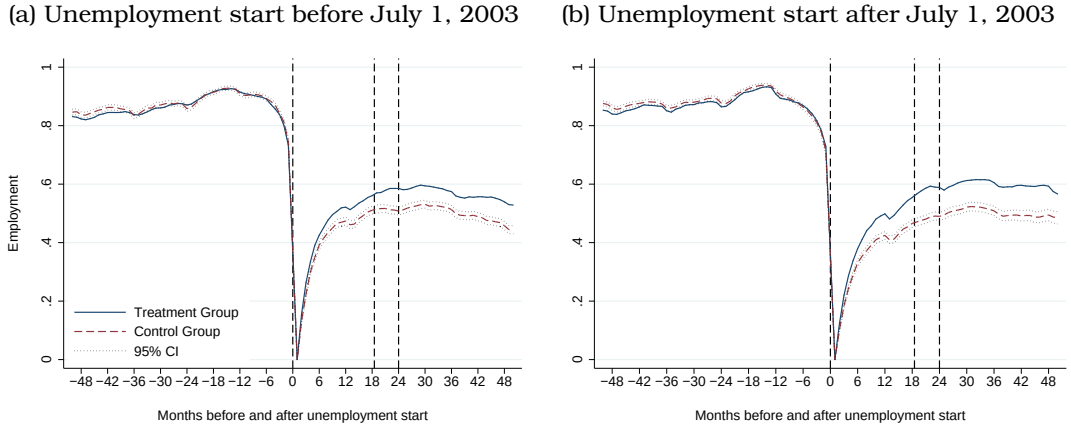
Figure 3: Difference in differences in unemployment benefits



Notes: Figure 3 shows the difference in differences for unemployment benefits for the 50 months before and 50 months after unemployment start. The dotted lines around the difference in differences indicate the 95 % confidence interval.

Figure 4 replicates the above graphical analysis for employment shares. Pre-unemployment (50 to 1 months before unemployment) employment shares lie between 0.94, and 0.71. For both, the treated and the untreated, the employment share already starts to fall in the last 12 to 6 months before getting unemployed. In the first month of unemployment, the employment share drops to zero. The unemployed start to find new jobs, and the average employment share rises again to around 60 %. The employment patterns of the treated and control groups start to diverge only after the start of the unemployment spell: Average employment of the treated individuals increases more than the average employment of the untreated individuals before (figure 4a) and after (figure 4b) the reform. This might be due to the fact that the control group is older on average and faces more problems to find a new job. Interestingly, however, the difference in average employment between treated and control group is larger for unemployment spells that started *after* the change in PBD in July, 2003.

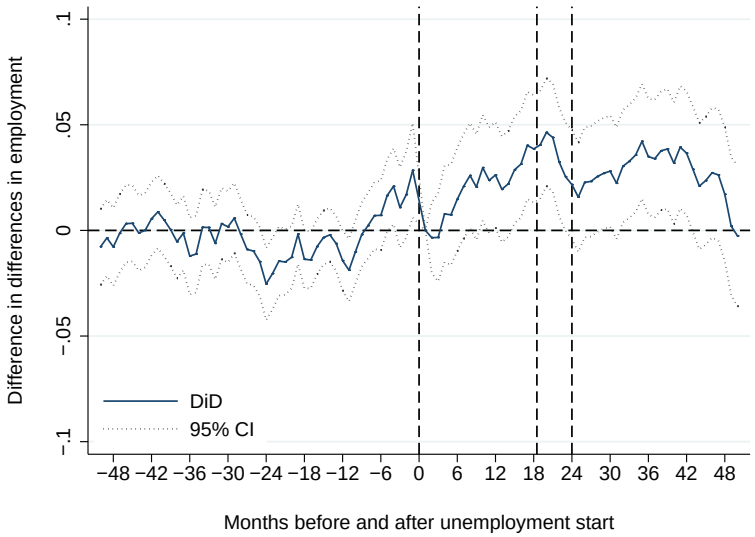
Figure 4: Employment before and after the reform



Notes: Figure 4a shows aggregate employment 50 months before and 50 months after unemployment start for individuals who entered unemployment before July 1, 2003. The employment history is shown for both, the treatment group ($<$ 55 years old at unemployment start) and the control group ($\geq$ 55 years old at unemployment start). Figure 4b shows the employment history for unemployment spells that started after July 1, 2003. The dotted lines around the employment history of the control group indicate the 95 % confidence interval.

Figure 5 confirms this observation. In the period before unemployment start, no treatment effect is detectable and the difference in differences is not statistically different from zero. The employment effect rises up to almost 5 % 20 months after entering unemployment and is statistically different from zero in the anticipation period τ_2 and in the direct treatment period τ_3. Also in the medium run period τ_4, the positive difference in differences persists and stays well above zero.

Figure 5: Difference in differences in employment



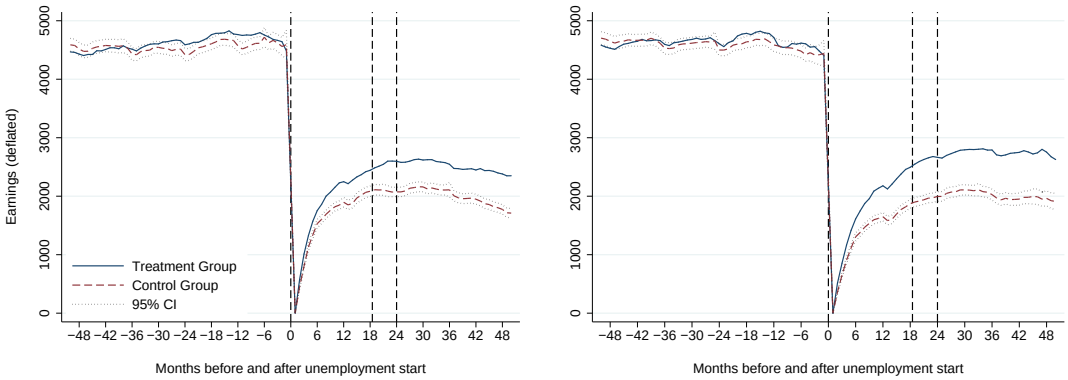
Notes: Figure 5 shows the difference in differences for employment for the 50 months before and 50 months after entering unemployment. The dotted lines around the difference in differences indicate the 95 % confidence interval.

A similar, but more volatile pattern is also observed for earnings. Figure 6 shows that pre-unemployment earnings lie around 4500 CHF, and drop to zero at unemploy-

ment start. Like the employment share, earnings rise again, but do no longer reach the pre-unemployment level, and stay at a level of around 2000 CHF for the control group, and around 2500 CHF for the treatment group after entering unemployment. Again, although earnings are higher for the treatment group irrespective of whether the start date of a spell was *before* (figure 6a) or *after* (figure 6b) the reform, earnings increase more for the treated than for the untreated in the *after* reform period.

Figure 6: Earnings before and after the reform

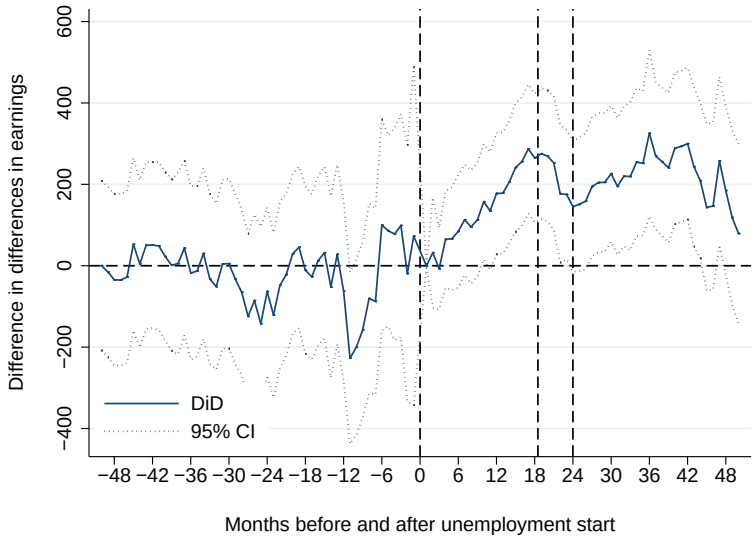
(a) Unemployment start before July 1, 2003 (b) Unemployment start after July 1, 2003



Notes: Figure 6a shows aggregate earnings after unemployment start for individuals who entered unemployment before July 1, 2003. The earnings history is shown for both, the treatment group (< 55 years old at unemployment start) and the control group (≥ 55 years old at unemployment start). Figure 6b shows the earnings history for unemployment spells that started after July 1, 2003. The dotted lines around the earnings history of the control group indicate the 95 % confidence interval.

A look at the difference in differences graph for earnings completes the picture: We do not observe a significant difference in earnings in the pre-unemployment period τ_0. The earnings difference starts to rise significantly after the beginning of a spell to around 290 CHF in the beginning of the treatment period τ_3 (18 to 24 months after unemployment start), and remains significantly different from zero also in the medium run period τ_4 (25 to 50 months after unemployment start).

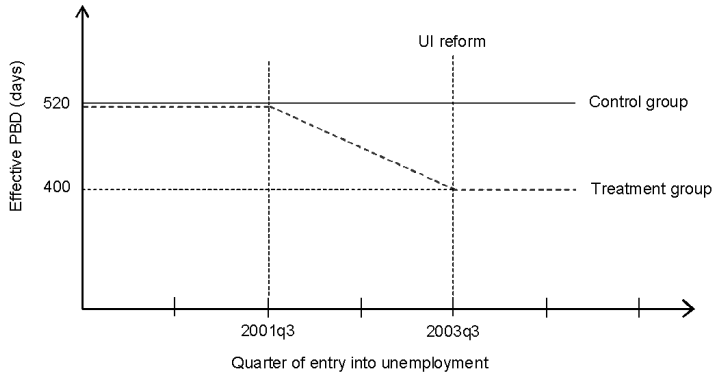
Figure 7: Difference in differences in earnings



Notes: Figure 7 shows the difference in differences for earnings for the 50 months before and 50 months after unemployment start. The dotted lines around the difference in differences indicate the 95 % confidence interval.

We have seen so far that the reform differentially affected job seekers in a difference-in-difference setting. The key concern with this analysis is the identifying assumption that the time trends in outcomes must be parallel in the period after the reform was implemented. This assumption can not be directly tested. We still analyze time trends in outcomes to assess the plausibility of this assumption. Our test proceeds as follows. The reform was applied to in-progress spells. Treated job seekers start to be affected by the cut in PBD even if their spell started before July 1, 2003. Figure 8 shows that the treatment group effectively starts to be affected by the cut in PBD for spells that start after July 1, 2001. Before July 1, 2001, both treated and untreated job-seekers are entitled to 520 days of daily benefits. After July 1, 2001, the treatment group starts to be affected by the benefit reduction with an increasing intensity. That is they lose an increasing number of daily benefit payments at the end of their benefit entitlement period. In other words, the effective PBD for the treatment group reduces gradually from 520 to 400 days for entries into unemployment between July 1, 2001 to July 1, 2003. Finally, for spells that started after July 1, 2003, the treated job seekers are entitled to 400 days, whereas untreated job seekers still get the full 520 days of unemployment benefits.

Figure 8: Timing of Reform



Notes: Figure 8 shows the stylized pattern of effective PBD over the quarter of entry into unemployment for the treatment and the control group respectively.

We assess whether the assumption of parallel trends is plausible by analyzing unemployment benefits that job seekers earn in the period 22 to 24 months after entering unemployment. Unemployment benefits should differ between treated and control groups only by a constant amount in the period prior to the third quarter of 2001 if pre-reform trends are parallel. However, starting from July 2001, job seekers in the treated group gradually loose eligibility for benefits in the period 22 to 24 months after their spell starts whereas job seekers in the control group continue to be eligible for benefits. This should create diverging trends in unemployment benefits due to the reform in 2003.

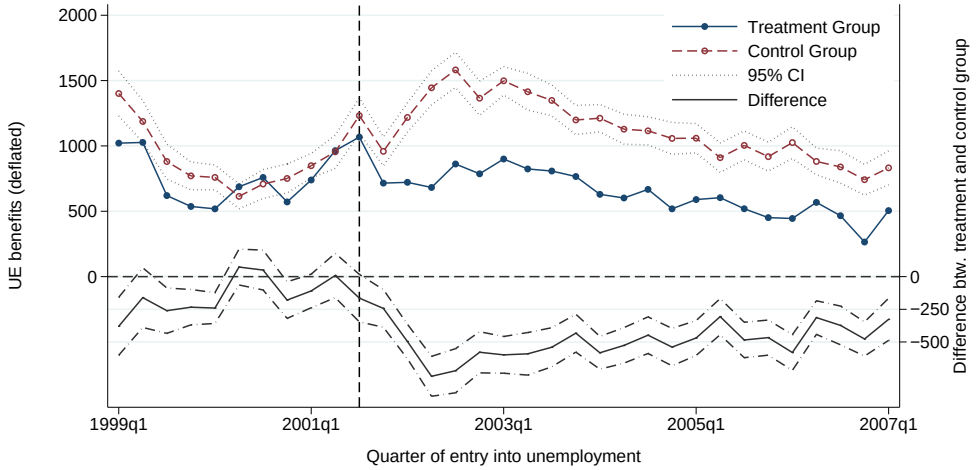
Figure 9a shows unemployment benefits received by treated and control groups in for every quarter between 1999 and 2007. The left hand axis measures the deflated levels of unemployment benefits. The right hand axis measures the difference between treatment and control groups. The vertical line in the third quarter of 2001 depicts the first possible date for which effects of the reform are potentially observable. Indeed, by and large we can observe parallel time trends of spells starting before the third quarter of 2001 for unemployment benefits (figure 9a). For unemployment spells starting after July 1, 2001, we observe an increasing difference between treatment and control group. Figure 9a shows that trends are roughly parallel in the period 1999 to 2001.2 and that the reform led to a reduction in unemployment benefit receipt.

Figures 9b and 9c report a similar analysis for employment and earnings. Results indicate that trends are parallel for both outcomes for spells that start before the third quarter of 2001. This evidence suggests trends in outcomes are similar. Moreover, both figures indicate that employment and earnings patters start to differ from the third quarter of 2001 onwards. These graphs suggest that the assumption of parallel trends is plausible and that the reform effects build up over time as would be expected

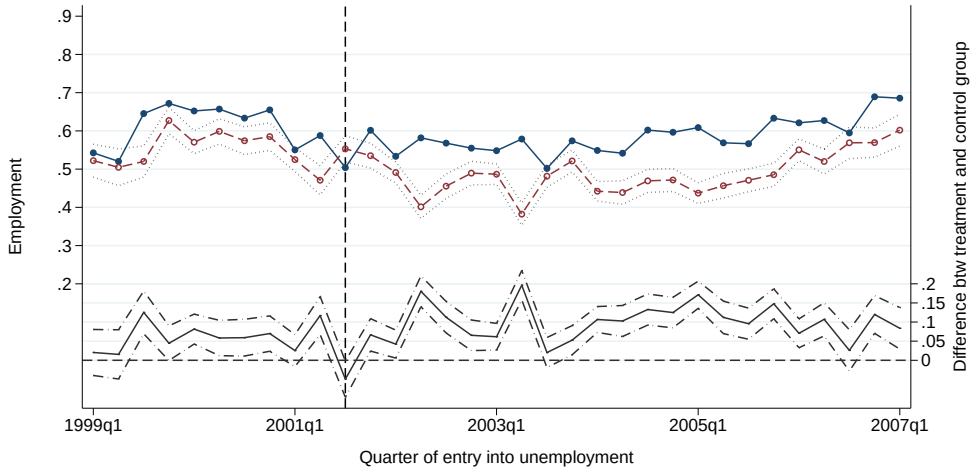
also for employment and earnings.

Figure 9: Time trends in unemployment benefits received 22 to 24 months after start of spell

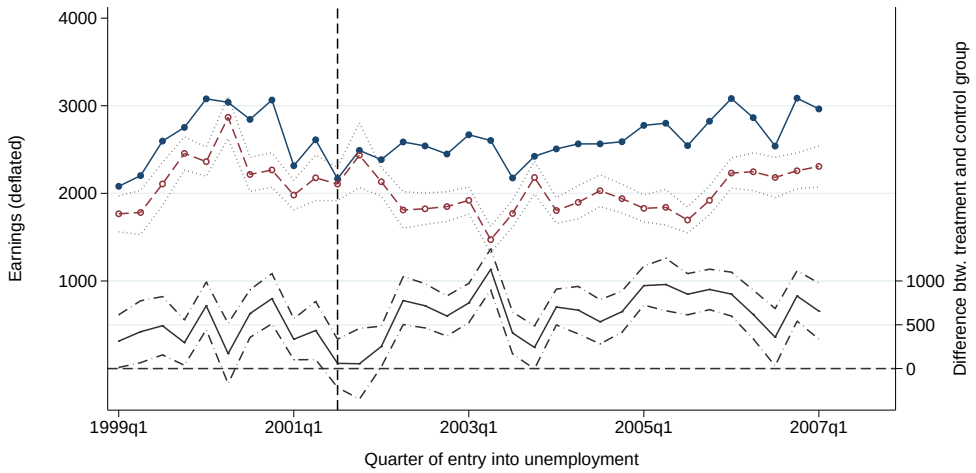
(a) UE benefits



(b) Employment



(c) Earnings



Notes: On the left hand axis figure 9 shows the time trends for the treatment and the control group for unemployment benefits in the 8th quarter (22 to 24 months) after unemployment start. The dotted lines around the time trends of the control group indicate the 95% confidence interval. On the right hand axis, the solid line at the bottom of each subfigure shows the difference between treatment and control group together with the 95% confidence interval. The vertical line at the 3rd quarter of 2001 depicts the first possible date for which treatment effects are possibly detectable.

5.2 Main estimates

The difference-in-difference estimator is estimated by the following econometric specification

$$\begin{aligned} Y_{itc} =& \alpha_1 + \alpha_2\tau_2 + \ldots + \alpha_4\tau_4 + \beta_1\tau_1 D_i + \ldots + \beta_4\tau_4 D_i + \gamma_1\tau_1 A_c + \ldots + \gamma_4\tau_4 A_c + \\ &+ \delta_1\tau_1 D_i A_c + \ldots + \delta_4\tau_4 D_i A_c + X_i'\eta + \varepsilon_{itc} \end{aligned} \tag{1}$$

where Y_{itc} is the outcome variable, that is unemployment benefits, employment, or earnings respectively. i is an indicator for the individual, t indicates the month after unemployment start, and c denotes calendar time. D_i is the treatment dummy which is equal to 1 if an individual belongs to the treatment group and 0 otherwise. A_c is a dummy for unemployment starts after July 1, 2003. τ_1 to τ_4 are indicators for the different periods after unemployment start, i.e. $\tau_1 = \mathbb{1}(1 \leq t \leq 12 \text{ months})$, $\tau_2 = \mathbb{1}(13 \leq t \leq 17 \text{ months})$, $\tau_3 = \mathbb{1}(18 \leq t \leq 24 \text{ months})$, and $\tau_4 = \mathbb{1}(25 \leq t \leq 50 \text{ months})$ respectively. δ_1 to δ_4 are the coefficients for the interaction effects $\tau_1 D_i A_c$ to $\tau_4 D_i A_c$, and identify the average treatment effect on the treated. X_i is a vector of control characteristics, such as gender, nationality, marital status (4 categories), professional status (leader/expert function versus non-leader function), and years of schooling (5 categories). As further controls we include a dummy for individuals with a high continuous work experience prior to their unemployment spell, i.e. at least 24 months of continuous employment before their unemployment start, a dummy for individuals whose previous employer is active in a R&D-intensive industry, and a dummy for individuals whose task content of previous occupation was mainly cognitive, and all interactions. Finally, we also include the sums of pre-unemployment earnings and benefits, as well as the total number of months spent in employment prior to unemployment start to address the significant diff-in-diff in unemployment benefits prior to the spell we analyze (see figure 3). In order to adjust for potential correlation across spells and across time, standard errors of this and all following tables are clustered by person.

Table 3 presents the baseline regression. In columns (1), (3), and (5), we estimate the treatment effects using equation (1) without controls. Columns (2), (4), and (6) show that the estimates of δ_1 to δ_4 remain stable and precisely estimated after the inclusion of covariates and their interactions. The estimates for unemployment benefits in column (2) indicate that already between 13 and 17 months after unemployment start benefits are 91 CHF lower in the treatment group. This marginally significant treatment effect is interpreted as an anticipation effect of the treated group. In the

period between 18 and 24 months after unemployment start, benefits are on average around 132 CHF lower for the treated, or around 5 % of average benefits two months after unemployment start. δ_3 quantifies the mechanic effect of reducing benefits for the below 55 years old, but not for the above 55 years old job seekers. In the medium run, there is no longer any significant difference between treated and untreated in terms of unemployment benefits.

The estimates for employment in column (4) show that we observe an anticipation effect of 2.4 percentage points for the treatment group (13 to 17 months after unemployment start). Already before the actual reform period, the treated re-enter employment more than the untreated. The direct effect of the reform, δ_3 amounts to 3.1 percentage points. In the period between 25 and 50 months after unemployment start the employment share is 2.6 percentage points higher for the treated. The relative magnitude of the treatment effects ranges between 2.9 % in the anticipation period to 3.7 % in the reform period of average employment three months before unemployment start.

For earnings in column (6), we also observe a strong and highly significant anticipation effect of 200 CHF. The direct effect for earnings amounts to 191 CHF, and the medium run effect stays at the about same level at 187 CHF. In relative terms, treatment effects range between 4.4 % in the anticipation period and 4 % in the medium run of average earnings three months before getting unemployed. The significant medium run coefficients δ_4 for employment and earnings show that reducing PBD does not have a purely mechanic effect, but that the positive employment and earnings effects persist in the medium run. The direct treatment effects (18 to 24 months after unemployment start) are all statistically highly significant with a t-value of -3.04 for unemployment benefits, 2.58 for employment, and 2.65 for earnings. The medium run effects (25 to 50 months after unemployment start) for employment are significant on a 10 % level with a t-value of 1.92. For earnings, the effects are significant at the 5 % level with a t-value of 2.55.

These main findings suggest that the beneficial effects of a reduced human capital and skill depreciation or improvements in the non-employment stigma seem to outweigh the negative effects of reduced reservation wages and could be act as a driving force for the positive medium run effects on employment and earnings.

Table 3: Difference in differences estimates for unemployment benefits, employment and earnings

	UE benefits		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
$\tau_1 D_i A_c$ (1-12 mths after)	24.131	11.867	0.014	0.010	85.911*	52.816
	(52.891)	(48.991)	(0.009)	(0.009)	(51.579)	(50.468)
$\tau_2 D_i A_c$ (13-17 mths after)	-79.550	-91.685*	0.028**	0.024**	233.018***	200.458***
	(53.889)	(51.708)	(0.012)	(0.012)	(75.676)	(69.084)
$\tau_3 D_i A_c$ (18-24 mths after)	-121.833***	-132.321***	0.035***	0.031***	222.350***	191.107***
	(45.161)	(43.541)	(0.012)	(0.012)	(79.745)	(72.020)
$\tau_4 D_i A_c$ (25-50 mths after)	-12.048	-19.050	0.026**	0.023*	208.153**	187.026**
	(20.747)	(22.661)	(0.012)	(0.012)	(83.290)	(73.276)
Avg. of dep. var.	2636	2636	0.827	0.827	4568	4568
R-squared	0.151	0.196	0.037	0.084	0.025	0.166
Observations	1,109,276	1,109,276	1,109,276	1,109,276	1,109,276	1,109,276
Clusters	21,323	21,323	21,323	21,323	21,323	21,323
Controls	No	Yes	No	Yes	No	Yes

Notes: Table 3 shows the baseline difference in differences estimates for unemployment benefits (columns 1 and 2), employment (columns 3 and 4) and earnings (columns 5 and 6). Regressions with controls include also the interactions of all controls. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

5.3 Sensitivity analyses

A widely discussed potential concern when looking at a sample of older job seekers is that the effects of reducing PBD could be biased because of early retirement considerations and/or disability retirement as an alternative way to exit the labor force after unemployment.[15]

Table 4 discusses how the cut in PBD affected disability retirement pensions. A cut in PBD could affect disability pensions in mainly two ways: First, reducing PBD could aggravate potential health consequences of job-loss leading to a larger need of disability pensions, and second, cutting PBD could induce a substitution of unemployment benefits with disability pensions. Table 4 shows the effects of reducing PBD on disability retirement pensions. Although positive in sign, estimates are economically small and not statistically significant for none of the periods. This suggests that the cut in PBD does not seem to induce treated individuals to claim significantly more disability pensions and that substitution effects do not seem to bias our main findings.

[15]Inderbitzin et al. (2012) study a regional extended benefit program in Austria and find substantial early retirement through disability insurance triggered by the unemployment benefit reform.

Table 4: Difference in differences estimates for disability retirement

	Disability Pensions
$\tau_1 D_i A_c$ (1-12 mths after)	6.461
	(10.403)
$\tau_2 D_i A_c$ (13-17 mths after)	16.546
	(12.762)
$\tau_3 D_i A_c$ (18-24 mths after)	19.322
	(13.570)
$\tau_4 D_i A_c$ (25-50 mths after)	8.784
	(15.298)
Observations	1,109,276
R-squared	0.056
Clusters	21323

Notes: Table (4) shows the difference in differences estimates for disability and old age retirement pensions. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Old-age retirement pensions are never observed for the treated group, and we start to observe them for the control group 26 months after unemployment start at the earliest for females, and 36 months after unemployment start for males respectively. This is because individuals are eligible for early retirement at the age of 62 for females and 63 for males respectively. Still, the age of the job seekers could be a potential issue, because the oldest job seekers in the treatment group "grow" into the control group shortly after their unemployment start. This could mitigate our estimates for the periods τ_2 to τ_4. To adress this concern, we estimate a model that excludes the oldest age cohorts of the treatment and the control group. That is, we exclude the 54 years old individuals in the treatment group, and the 59 years old individuals in the control group. Table A2 in the appendix reports the estimates for this restricted sample. Excluding the oldest age cohorts in each group does not change the estimates drastically: Compared to our main estimates, the treatment effects are somewhat smaller for employment and earnings, and slightly stronger for unemployment benefits. Statistical significance decreases slightly, because in the restricted sample around one fifth of all observations is lost. The overall picture however is unchanged.

To further test the robustness of our main estimates, we analyse how sensitive the estimates are to the definition of the start date of unemployment. Table 5 contains estimates that use the formal registration and deregistration date at the PES as start date of a spell. Job seekers have to register at the PES as soon as they know that they will lose a job, which is typically a quarter before they actually lose their job. Columns (1) to (3) of table 5 replicate the main estimates for ease of comparison, and columns (4) to (6) show the estimates based on the formal registration at the PES as start date of a spell. For unemployment benefits, the anticipation effect (13 to

17 months after unemployment start) increases slightly in magnitude to -100 CHF, and the direct treatment effect (18 to 24 months after unemployment start) decreases somewhat to -118 CHF. The shift from the direct to the anticipation period might be explained by the fact that the formal registration date is earlier than the potential entry date of the next job, which also shifts the treatment effects back somewhat on average. The employment and earnings effects tend to increase slightly relative to the main estimates, but do not affect the general picture.

Table 5: Difference in differences estimates using administrative registration and deregistration dates

	Baseline			Administrative reg. dates		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
1-12 mths after	11.867	0.010	52.816	-3.697	0.010	60.220
	(48.991)	(0.009)	(50.468)	(49.359)	(0.009)	(50.526)
13-17 mths after	-91.685*	0.024**	200.458***	-100.413*	0.023*	199.359***
	(51.708)	(0.012)	(69.084)	(52.011)	(0.012)	(69.171)
18-24 mths after	-132.321***	0.031***	191.107***	-117.677***	0.035***	206.934***
	(43.541)	(0.012)	(72.020)	(43.717)	(0.012)	(72.100)
25-50 mths after	-19.050	0.023*	187.026**	-16.463	0.026**	196.719***
	(22.661)	(0.012)	(73.276)	(22.699)	(0.012)	(73.542)
Avg. of dep. var.	2636	0.827	4568	2636	0.827	4568
R-squared	0.196	0.084	0.166	0.195	0.084	0.1661
Observations	1,109,276	1,109,276	1,109,276	1,108,532	1,108,532	1,108,532
Clusters	21,323	21,323	21,323	21,310	21,310	21,310

Notes: Table (5) shows the difference in differences estimates for the baseline specification in columns 1 to 3, and for a specification that uses the administrative registration and deregistration dates in columns 4 to 6. A job seeker spell in the baseline specification is defined as the duration between the first possible date at which individuals are available for a new job and the start date of the employment if available, and the administrative deregistration date elsewhere. The specification using administrative registration and deregistration dates defines a job seeker spell as the duration between the registration and deregistration at one of the regional placement offices. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

5.4 Heterogeneity of treatment effects

This section focuses on the issue of heterogeneity in treatment and control groups. For different subgroups of job seekers, we analyze whether they are differentially affected by the cut in PBD.

In a first sample split we aim at sheding light on the role of human capital and skill depreciation as a possible driving force of the positive medium-run effects. We split the sample into two groups which likely differ in terms of the importance of human capital depreciation. To this end, we split the sample according to the R&D-intensity of the industry in which the previous employer of the job seeker is active in. Human capital depreciation is assumed to play a more important role for individuals working in fast-evolving, highly R&D-intense industries, because a job-loss disconnects the unemployed faster from the job requirements in those industries. An extended

period of unemployment is therefore expected to be more harmful for job seekers in highly R&D-intense industries. In other words, if human capital and skill depreciation played an important role, the beneficial effects of reducing PBD would be stronger in the subsample of highly R&D-intense industry job seekers.

Table 6 presents estimates for the sample split by R&D-intensity of previous industry. Columns (1) to (3) of these two tables reproduces the baseline estimates for the sake of comparison. Columns (4) to (6) report estimates for job seekers coming from above median R&D-intense industries, and columns (7) to (9) for job seekers from industries with below median R&D intensity respectively. Effects are much stronger for job seekers who left R&D intensive industries than for job seekers who left industries with little expenditure on R&D. We observe strong and significant anticipation effects in the period from 13 to 17 months after unemployment start. The direct or mechanic effects of the reform (18 to 24 months after unemployment start) lead to a 5.7 percentage points difference in employment and to a earnings difference of around 441 CHF. The effects persist also in the medium run (25 to 50 months after unemployment start): Cutting PBD by 6 months leads to a 5.2 percentage points difference in terms of employment and to a difference in earnings of 406 CHF in the medium run. On the other side, treatment effects in low R&D-intense industries are completely absent in all periods. Point estimates are close to zero for employment, and even negative for earnings, but none of them are statistically significant.

Looking at the effects in absolute terms however can be misleading: Average unemployment benefits, earnings and employment shares differ a lot between highly R&D-intense and low R&D-intense industries. Average earnings three months before unemployment start amount to 5278 CHF in highly R&D-intense industries and only 3916 CHF in low R&D-intense industries respectively. Also the employment share is somewhat higher in the R&D-intense industries, with a share of 84.8 % compared to a share of 80.7 % in the less R&D-intense industries. Also the average unemployment benefits two months after unemployment start are over 400 CHF higher in the R&D-intense industries. But even taking into consideration these differences, the relative treatment effects are larger in the R&D-intense industries: The medium run relative employment effect for high R&D-intense industries is 10 times the effect for the low R&D-intense industries (6.1 % versus 0.61 % of the average employment share). Similarly for earnings, the relative effects are much larger in the R&D-intense industries: The medium run earnings effect amounts to 7.7 % of average earnings for the highly R&D-intense industries compared to 0.4 % in the less R&D-intense industries.

A second subgroup analysis investigates further on the importance of human capital and skill depreciation. In this analysis, we take the task content of previous

occupation as a proxy for the skills of the job seeker. As above, we split the sample into two subgroups: One of them contains job seekers with primarily cognitive skills, and the other subgroup contains job seekers with mainly manual skills. Similar to the above subgroup analysis, if we assume that human capital and skills depreciate faster in mainly cognitive occupations, we would expect that a reduced PBD affects job seekers with mainly cognitive skills more than job seekers with primarily manual skills. Table 7 shows the estimates for this sample split by task content of previous occupation. Column (1) to (3) repeat the baseline estimates, columns (4) to (6) contains the subgroups of job seekers with mainly cognitive skills and columns (7) to (9) the subgroups of job seekers with mainly manual skills. Already 13 to 17 months after unemployment start, treated job seekers with mainly cognitive skills (columns 4 to 6) start to anticipate that their unemployment benefit run out after 18.5 months. These anticipation effects amount to 4.8 percentage points for employment and 250 CHF for earnings. In the direct reform period (18 to 24 months after unemployment start) treated job seekers claim on average 164 CHF less unemployment benefits, and earn 228 CHF more from employment than their untreated counterparts during the direct reform period. The employment effect increases to 5.4 percentage points. In the medium run (25 to 50 months after unemployment start), treated job seekers with mainly cognitive skills earn around 254 CHF more than the untreated and have on average an employment share that exeeds the one of the untreated by 3.3 percentage points. In contrast to this, the treated job seekers with primarily manual skills (columns 7 to 9) receive 121 CHF less unemployment benefits, and earn around 156 CHF more than their older counterparts. However, in terms of employment no significant effect is detectable. In the medium run there is no significant effects, neither for the employment share nor for earnings, although positive in sign.

As above, there are large differences between the two subgroups regarding their average unemployment benefits, earnings and employment shares. Average earnings three months before entering unemployment is 5475 CHF for the ”cognitive tasks” subgroup and 3761 CHF for the ”manual tasks” subgroup. Also the employment share is 2.8 percentage points lower in the latter subgroup. Finally, unemployment benefits in the second month after unemployment start differ by over 700 CHF. Looking at the relative importance of the effect might thus be more insightful. The earnings effect in the direct reform period (18 to 24 months after unemployment start) amounts to 6.4 % of the average employment share for job seekers with mainly cognitive skills and to 1.3% for job seekers with mainly manual skills. For earnings, the relative importance of the direct effects across subgroups is almost identical, with 4.2 % and 4.1 % of average earnings respectively. Yet the medium-run effect is not significantly

different from zero for workers from low-skill occupations whereas it is significantly positive for workers form high-skill occupations.[16]

5.5 Relation to Existing Literature

The existing literature mainly focuses on outcomes that capture job-match quality for job seekers who find jobs after their unemployment spell. In order to relate our results to existing studies, we analyse the effects of the PBD reduction on different measures of job-match quality. Table 8 presents the difference-in-difference estimates for the duration of unemployment, and earnings in the 2nd month after re-employment. Table 9 discusses the effects for the job loss probability within 3, 6, 12, and 24 months.

Panel 8a of table 8 reports the effects of reducing the PBD on the unemployment duration. Unemployment duration is defined as the number of months spent in unemployment until the next job. If there is no next job observed in the data, unemployment duration is censored by the last available date. On average job seekers spent around 16.5 months in unemployment.[17] Reducing the PBD lowers the time spent in unemployment for the treatment group by 0.518 months for the whole sample. Overall, the effect is not statistically significant. However if we look at the subgroup of job seeker in R&D-intense industries, the effect is more than three times bigger and statistically significant: Treated job seekers in those industries are on average 1.5 months less unemployed than their untreated counterparts. On the other side, if we look at the subgroup of job seekers from low R&D-intense industries, the point estimate is positive, but not statistically significant. When looking at the subgroup analysis that splits job seekers into cognitive and manual skilled groups respectively, we find that treated job seekers with mainly cognitive skills are on average 1.2 months less unemployed than their counterparts in the control group. For job seekers with primarily manual skills the effect is slightly positive and not significant. Again, it becomes evident that job seekers in different industries and occupations are heterogeneously affected by a reduction in PBD.

Panel 8b presents the difference-in-difference estimates for post-unemployment earnings measured in the second month after re-employment. In the overall sample,

[16]Further subsample analyses were performed by gender and previous earnings, and are reported in the appendix. Table A3 presents subgroup estimations splitted by gender and table A4 by previous earnings respectively. The results suggest that the baseline effects are mainly driven by male and high-earning job seekers. However, the concentration of high-earning male job seekers among the highly R&D-intense industries is large, so that the results in tables A4 and A3 could reflect the same effects as in table 6. For task content of previous occupation, we observe a high concentration of above median earners among the cognitive skill group, but at the same time a low concentration of male job seekers. It is thus not clear *a priori* how the estimates in tables A4 and A3 relate with those in table 7.

[17]The average duration spent in unemployment for individuals who actually found a job within the observed time period is almost cut by half, with around 8.4 months.

Table 6: Difference in differences estimates by R&D intensity of previous industry

	Baseline			High R&D intensity			Low R&D intensity		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	11.867	0.010	52.816	47.630	0.018	142.373*	-22.583	0.002	-21.740
	(48.991)	(0.009)	(50.468)	(74.966)	(0.013)	(78.016)	(62.917)	(0.013)	(64.675)
13-17 mths after	-91.685*	0.024**	200.458***	-135.994*	0.056***	454.783***	-60.142	-0.006	-20.458
	(51.708)	(0.012)	(69.084)	(81.068)	(0.017)	(109.855)	(64.935)	(0.016)	(84.892)
18-24 mths after	-132.321***	0.031***	191.107***	-200.448***	0.057***	441.163***	-83.484	0.007	-32.202
	(43.541)	(0.012)	(72.020)	(68.346)	(0.017)	(113.253)	(54.453)	(0.017)	(88.712)
25-50 mths after	-19.050	0.023*	187.026**	-38.854	0.052***	406.323***	-1.830	-0.005	-17.164
	(22.661)	(0.012)	(73.276)	(33.509)	(0.017)	(113.832)	(30.034)	(0.016)	(92.179)
Avg. of dep. var.	2636	0.827	4568	2881	0.848	5278	2412	0.807	3916
R-squared	0.196	0.084	0.166	0.215	0.097	0.174	0.177	0.079	0.160
Observations	1,109,276	1,109,276	1,109,276	538,840	538,840	538,840	570,436	570,436	570,436
Clusters	21,323	21,323	21,323	10,729	10,729	10,729	11,177	11,177	11,177

Notes: Table (6) shows the difference in differences estimates for subsamples splitted by innovative pace of industries (as measured by R&D intensity of industries) for unemployment benefits, employment, and earnings respectively. Columns 1 to 3 replicate the baseline estimates, columns 4 to 6 include industries with an R&D intensity above the median, and columns 7 to 9 include industries with an R&D intensity below the median. R&D intensity of industries is measured as an average of R&D in percentage of GDP for the neighboring countries Germany, Austria, France and Italy over the years 2005 to 2008. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Table 7: Difference in differences estimates by task content of previous occupation

	Baseline			Cognitive Tasks			Manual Tasks		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	11.867	0.010	52.816	-55.464	0.019	115.030	23.623	0.009	51.893
	(48.991)	(0.009)	(50.468)	(78.062)	(0.013)	(81.017)	(59.286)	(0.012)	(59.797)
13-17 mths after	-91.685*	0.024**	200.458***	-129.013	0.048***	250.500**	-82.250	0.004	164.267**
	(51.708)	(0.012)	(69.084)	(83.918)	(0.017)	(115.418)	(62.529)	(0.016)	(79.751)
18-24 mths after	-132.321***	0.031***	191.107***	-164.801**	0.054***	228.470*	-121.377**	0.011	156.385*
	(43.541)	(0.012)	(72.020)	(70.937)	(0.017)	(120.090)	(52.248)	(0.017)	(82.693)
25-50 mths after	-19.050	0.023*	187.026**	22.654	0.033*	254.773**	-38.928	0.011	114.611
	(22.661)	(0.012)	(73.276)	(36.995)	(0.017)	(123.471)	(26.171)	(0.016)	(82.400)
Avg. of dep. var.	2636	0.827	4568	3023	0.842	5475	2292	0.814	3761
R-squared	0.196	0.084	0.166	0.211	0.093	0.168	0.179	0.080	0.148
Observations	1,109,276	1,109,276	1,109,276	522,984	522,984	522,984	586,142	586,142	586,142
Clusters	21,323	21,323	21,323	10,497	10,497	10,497	11,520	11,520	11,520

Notes: Table (7) shows the difference in differences estimates for subsamples splitted by the task content of the previous occupation for unemployment benefits, employment, and earnings respectively. Columns 1 to 3 replicate the baseline estimates, columns 4 to 6 include occupations with primarily cognitive task content, and columns 7 to 9 include occupations with mainly manual task content. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

average post-unemployment earnings are 3597 CHF (column 1). Not surprisingly, earnings are lower for job seekers in low R&D-intense industries (3391 CHF in column 3) and for mainly manual skilled job seekers (3233 CHF in column 5), and higher for job seekers in R&D-intense industries (3818 CHF in column 2) and with mainly cognitive skills (3968 CHF in column 4). With the exception of high R&D- intense industries, post-unemployment earnings are lower for treated job seekers. However none of the estimates is statistically significant. This finding suggests, that although the reform has a negligible immediate effect on post-unemployment earnings for those who did get a job, the positive earnings effects documented in the baseline specification seem to build over time.

Table 8: Difference in differences estimates for unemployment duration, subsequent earnings, and subsequent employment duration

(a) Dependent variable is the number of months spent in unemployment

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-0.518	-1.593**	0.466	-1.207*	0.111
	(0.467)	(0.670)	(0.651)	(0.647)	(0.664)
Avg. of dep. var.	16.49	17.27	15.76	15.74	17.16
R-squared	0.082	0.088	0.081	0.059	0.106
Observations	1,109,276	538,840	570,436	522,984	586,292
Clusters	21,323	10,729	11,177	10,497	11,523

(b) Dependent variable is earnings (2nd month after re-employment)

	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-40.773	70.103	-120.716	-57.541	-18.100
	(77.030)	(126.190)	(93.236)	(125.376)	(89.740)
Avg. of dep. var.	3597	3818	3391	3968	3233
R-squared	0.288	0.306	0.261	0.291	0.260
Observations	804,932	388,511	416,421	398,056	406,876
Clusters	15,436	7748	8084	7954	7973

Notes: Table 8 shows difference in differences estimates for unemployment duration and a number of job-match quality measures together with their means. Table 8a shows the estimates for number of months spent in unemployment. Table 8b illustrates the estimates for earnings in the second month of re-employment. Standard errors clustered by individual. *** P<0.01 ** P<0.05 * P<0.1.

Table 9 focuses on the job loss probabilities 3, 6, 12 and 24 months after re-employment respectively. Estimations include only observations which we observe for at least 24 months after re-employment, that is, we exclude around 3 % of re-employed job seekers with right-censored re-employment durations. The probability of losing the job after re-employment varies between roughly 20 % for a job loss within

3 months (table 9a), and 60 % for a job loss within 24 months after reemployment (table 9d). Overall, reducing the PBD lowers the job loss probabilities somewhat, but not statistically significantly so. The point estimates for job seekers in high R&D-intense industries and with mainly manual skills are slightly positive, but small and insignificant. A marginally significant effect is found only for job seekers in R&D-intense industries for the probability of losing the job within 12 months after re-employment: Treated job seekers have a 3.6 percentage point lower probability of losing their job compared to the untreated job seekers.

All in all, tables 8 and 9 confirm the general findings of the existing literature, which finds only small or no effects of UI policy changes on job-match quality. The findings however also support the view that the beneficial effects of reduced human capital depreciation and improvements in non-employment stigma overweigh the negative effects of reduced reservation wages, leading to positive overall effects on earnings and employment in the medium run.

Table 9: Difference in differences estimates for the probability of losing the employment within 3, 6, 12 or 24 months

(a) Dependent variable is the probability of losing the job within 3 months

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-0.003	0.001	-0.009	-0.017	0.009
	(0.012)	(0.018)	(0.017)	(0.017)	(0.018)
Avg. of dep. var.	0.193	0.191	0.196	0.178	0.208
R-squared	0.018	0.026	0.021	0.025	0.017
Observations	841,621	407,159	434,462	412,255	429,366
Clusters	15,858	7977	8271	8098	8238

(b) Dependent variable is the probability of losing the job within 6 months

	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-0.008	-0.014	-0.005	-0.018	0.002
	(0.014)	(0.020)	(0.020)	(0.019)	(0.020)
Avg. of dep. var.	0.299	0.288	0.309	0.271	0.325
R-squared	0.029	0.040	0.029	0.034	0.026
Observations	841,621	407,159	434,462	412,255	429,366
Clusters	15,858	7977	8271	8098	8238

(c) Dependent variable is the probability of losing the job within 12 months

	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-0.015	-0.036*	0.003	-0.017	-0.014
	(0.015)	(0.022)	(0.021)	(0.021)	(0.021)
Avg. of dep. var.	0.459	0.425	0.491	0.409	0.507
R-squared	0.061	0.052	0.073	0.054	0.061
Observations	841,621	407,159	434,462	412,255	429,366
Clusters	15,858	7977	8271	8098	8238

(d) Dependent variable is the probability of losing the job within 24 months

	(1)	(2)	(3)	(4)	(5)
$D_i A_c$	-0.008	-0.033	0.012	0.000	-0.018
	(0.015)	(0.022)	(0.020)	(0.022)	(0.020)
Avg. of dep. var.	0.594	0.563	0.623	0.547	0.639
R-squared	0.059	0.051	0.073	0.053	0.056
Observations	841,621	407,159	434,462	412,255	429,366
Clusters	15,858	7977	8271	8098	8238

Notes: Table 9 shows difference in differences estimates for the probability of losing the subsequent employment within 3, 6, 12, or 24 months respectively. Right-censored employment durations within 24 months are excluded. Standard errors clustered by individual. *** P<0.01 ** P<0.05 * P<0.1.

Finally, we take a closer look at post-unemployment earnings. The earnings effect in the baseline specification can either be driven by an increased employment share, or by both, an increased employment share and increased individual earnings. To shed some light on the channels for the positive earnings effect documented in our

baseline specification, we re-estimate the earnings effects conditional on employment. Table 10 reports the results. Overall, the earnings effect for employed individuals is considerably smaller and, if anything, only marginally significant in the periods anticipation period from 13 to 17 months after unemployment start.

For job seekers in low R&D-intense industries the earnings effect basically disappears, whereas for job seekers in high R&D-intense industries we find significant effects in the anticipation and reform period, which are however, relatively small (around 4.5 % of average earnings three months before unemployment start) and, if at all, only weakly significant. Furthermore, we also find a marginally significant effect in the period from 1 to 12 months after unemployment start. Estimates for a sample split by task content of previous occupation are positive, but in general not statistically significant. These findings are not contradictive to the findings of panel 8b of table 8, but once again highlights that negative effects of the reform due to reduced reservation wages are more than compensated by the positive aspects of reduced human capital and skill depreciation or reduced stigma effects which build up over time. At the same time the estimates in table 10 suggest that the main driver of the positive baseline earnings effects is increased employment rather than higher individual incomes. These results, however, need to be interpreted with caution. They may not represent causal effects if selection into employment is affected by the duration of PBD.

Table 10: Difference in differences estimates for employed individuals

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
$\tau_1 D_i A_c$ (1-12 mths after)	95.446	234.763*	-9.385	109.545	93.210
	(77.730)	(129.872)	(93.899)	(132.282)	(89.587)
$\tau_2 D_i A_c$ (13-17 mths after)	148.337*	292.470**	19.065	106.579	194.960*
	(88.870)	(144.247)	(104.890)	(146.229)	(100.695)
$\tau_3 D_i A_c$ (18-24 mths after)	77.855	238.456*	-57.942	39.635	113.139
	(86.323)	(141.618)	(99.421)	(143.032)	(94.558)
$\tau_4 D_i A_c$ (25-50 mths after)	131.846	231.237	42.072	231.997	38.402
	(90.184)	(140.666)	(112.674)	(142.644)	(100.677)
Avg. of dep. var.	4568	5278	3916	5475	3760
R-squared	0.316	0.320	0.302	0.313	0.290
Observations	537,700	259,714	277,986	270,683	267,017
Clusters	16,646	8370	8745	8526	8691

Notes: Table (10) illustrates the difference in differences estimates for the effect of reduced unemployment benefit duration on earnings for individuals conditional on employment. The effects are shown for the baseline specification in column 1, for the sample splits by R&D-intensity in columns 2 and 3, and for the sample splits by task content of previous occupation in columns 4 and 5. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

6 Conclusions

We discuss the effects of shortening potential benefit duration (PBD) for job seekers aged 50 to 54 years. Shortening PBD pushes job seekers into jobs during the period when benefit payments are cut. But these jobs may be of lower quality than the jobs that job seekers would have found with longer PBD. Conversely, inciting job seekers to leave unemployment more quickly may contain human capital depreciation or negative stigma of long-term unemployment.

We find strong evidence for the job push effect. This evidence is consistent with numerous studies that document that reducing PBD will decrease unemployment duration. Interestingly, we also document that the initial push into jobs carries longer lasting benefits. Job seekers who find employment more quickly because of a reduction in PBD tend to remain employed more likely and earn more not only during the period when benefits are removed but up to 2 years later on. The medium-run benefits are especially strong for job seekers in high R&D-intense industries and basically absent for job seekers in low R&D-intense industries.

The evidence we find is consistent with a strong role for unemployment insurance to contain human capital depreciation. Cuts in PBD duration that make sense if medium-run benefits for earnings and employment are ignored make even more sense if these effects are taken into account. Yet our evidence does not imply that benefit duration should be cut across the board. Reducing PBD carries a cost in terms of reduced protection against economic shocks. Moreover, a reduction of PBD from a baseline of 6 months might have differential effects than the same reduction from a baseline of 24 months. Human capital depreciation and long-term unemployment stigma is likely to be more important for old job seekers than for younger ones. We consider these issues as interesting avenues for future research.

A Appendix

A.1 Tables

Table A1: Difference in differences estimates for unemployment benefits, employment and earnings

	UE benefits		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
$\tau_1 D_i A_c$ (1-12 mths after)	24.131	11.867	0.014	0.010	85.911*	52.816
	(52.891)	(48.991)	(0.009)	(0.009)	(51.579)	(50.468)
$\tau_2 D_i A_c$ (13-17 mths after)	-79.550	-91.685*	0.028**	0.024**	233.018***	200.458***
	(53.889)	(51.708)	(0.012)	(0.012)	(75.676)	(69.084)
$\tau_3 D_i A_c$ (18-24 mths after)	-121.833***	-132.321***	0.035***	0.031***	222.350***	191.107***

Table A1 – continued

	UE benefits		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	(45.161)	(43.541)	(0.012)	(0.012)	(79.745)	(72.020)
$\tau_4 D_i A_c$ (25-50 mths after)	-12.048	-19.050	0.026**	0.023*	208.153**	187.026**
	(20.747)	(22.661)	(0.012)	(0.012)	(83.290)	(73.276)
τ_2	-507.659***	-507.659***	0.139***	0.139***	633.860***	633.860***
	(19.912)	(19.913)	(0.005)	(0.005)	(28.154)	(28.155)
τ_3	-758.736***	-758.736***	0.171***	0.171***	769.407***	769.407***
	(22.574)	(22.575)	(0.005)	(0.005)	(31.838)	(31.840)
τ_4	-1,584.710***	-1,584.710***	0.156***	0.156***	668.374***	668.374***
	(27.033)	(27.034)	(0.006)	(0.006)	(34.028)	(34.029)
$\tau_1 D_i$	-66.953**	-62.805**	0.036***	0.046***	243.461***	277.516***
	(34.042)	(30.696)	(0.007)	(0.007)	(36.784)	(36.321)
$\tau_2 D_i$	-161.999***	-157.851***	0.053***	0.062***	364.574***	398.629***
	(36.565)	(34.744)	(0.009)	(0.008)	(55.311)	(51.135)
$\tau_3 D_i$	-276.173***	-272.025***	0.064***	0.073***	448.163***	482.218***
	(32.280)	(30.857)	(0.009)	(0.009)	(57.293)	(52.446)
$\tau_4 D_i$	-9.416	-5.268	0.069***	0.078***	517.484***	551.538***
	(16.047)	(16.799)	(0.008)	(0.008)	(54.901)	(49.423)
$\tau_1 A_c$	255.873***	259.535***	-0.049***	-0.046***	-189.943***	-141.093***
	(39.836)	(37.145)	(0.007)	(0.007)	(36.673)	(37.288)
$\tau_2 A_c$	176.822***	179.584***	-0.051***	-0.048***	-263.206***	-218.024***
	(41.397)	(39.947)	(0.009)	(0.009)	(55.031)	(51.618)
$\tau_3 A_c$	-2.165	-1.472	-0.033***	-0.031***	-155.179***	-119.098**
	(36.531)	(35.429)	(0.009)	(0.009)	(58.176)	(54.248)
$\tau_4 A_c$	-121.312***	-132.636***	0.005	0.006	31.886	37.894
	(15.719)	(17.136)	(0.009)	(0.009)	(59.862)	(54.128)
Sum of pre-reg. benefits		0.004***		0.000***		0.004***
		(0.000)		(0.000)		(0.001)
Sum of pre-reg. earnings		0.001***		0.000***		0.005***
		(0.000)		(0.000)		(0.000)
Mths employed before reg.		-0.953		0.008***		13.020***
		(1.006)		(0.000)		(2.916)
$\geq$ 24 mths of work exp.		51.466***		-0.100***		-392.406***
		(16.180)		(0.006)		(38.486)
R&D intense industry		67.499***		-0.028***		-107.770***
		(12.640)		(0.005)		(28.961)
Cognitive task		152.407***		0.003		88.559***
		(14.283)		(0.005)		(33.997)
Female		-171.428***		0.021***		-239.150***
		(16.486)		(0.006)		(47.369)
Swiss		-51.428***		0.090***		269.295***
		(17.836)		(0.006)		(39.806)
Leader position		125.678***		0.046***		355.738***
		(17.079)		(0.007)		(39.388)
Marital status (reference group are singles)						
Married		-89.771***		0.001		26.989
		(21.211)		(0.008)		(47.736)
Widowed		-170.732***		-0.076***		-314.385***
		(43.211)		(0.017)		(96.045)
Divorced		-54.125**		0.001		46.489
		(24.632)		(0.009)		(56.070)
Education (reference group is "8-9 years of schooling")						
$\leq$ 7 years		19.106		-0.055**		-190.116*
		(56.973)		(0.023)		(103.237)
10-11 years		92.745**		0.025*		118.485*
		(36.434)		(0.014)		(70.794)
12-13 years		63.092***		0.051***		299.408***
		(22.367)		(0.009)		(46.649)
$\geq$ 14 years		208.251***		0.048***		625.645***
		(42.973)		(0.016)		(100.002)
Other		-78.404***		0.062***		454.765***
		(21.120)		(0.009)		(45.816)
Constant	1,930.618***	1,524.901***	0.342***	-0.098***	1,322.741***	-908.454***

Table A1 – continued

	UE benefits		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	(25.765)	(52.082)	(0.005)	(0.019)	(26.942)	(108.373)
Avg. of dep. var.	2636	2636	0.827	0.827	4568	4568
R-squared	0.151	0.196	0.037	0.084	0.025	0.166
Observations	1,109,276	1,109,276	1,109,276	1,109,276	1,109,276	1,109,276
Clusters	21,323	21,323	21,323	21,323	21,323	21,323

Notes: Table A1 shows the baseline difference in differences estimates for unemployment benefits (columns 1 and 2), employment (columns 3 and 4) and earnings (columns 5 and 6). Regressions with controls include also the interactions of all controls. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Table A2: Difference in differences estimates by age

	Baseline			50-53 vs. 55-58 years old		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
1-12 mths after	11.867	0.010	52.816	17.090	0.006	24.211
	(48.991)	(0.009)	(50.468)	(54.572)	(0.010)	(56.668)
13-17 mths after	-91.685*	0.024**	200.458***	-87.082	0.019	175.157**
	(51.708)	(0.012)	(69.084)	(56.509)	(0.013)	(77.560)
18-24 mths after	-132.321***	0.031***	191.107***	-136.862***	0.025*	171.645**
	(43.541)	(0.012)	(72.020)	(47.346)	(0.013)	(80.717)
25-50 mths after	-19.050	0.023*	187.026**	-40.172	0.022*	176.130**
	(22.661)	(0.012)	(73.276)	(24.866)	(0.013)	(82.444)
Avg. of dep. var.	2636	0.827	4568	2652	0.827	4585
R-squared	0.196	0.084	0.166	0.193	0.086	0.172
Observations	1,109,276	1,109,276	1,109,276	911,152	911,152	911,152
Clusters	21,323	21,323	21,323	17,902	17,902	17,902

Notes: Table (A2) shows the difference in differences estimates for subsamples splitted by age. Columns 1 to 3 replicate the baseline estimates, and columns 4 to 6 include only 50 to 53, and 55 to 58 years old individuals respectively. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Table A3: Difference in differences estimates by gender

	Baseline			Female			Male		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	11.867	0.010	52.816	-0.773	0.010	17.502	19.147	0.009	85.923
	(48.991)	(0.009)	(50.468)	(58.215)	(0.013)	(55.165)	(73.341)	(0.012)	(78.098)
13-17 mths after	-91.685*	0.024**	200.458***	-126.383**	0.009	74.694	-65.998	0.035**	299.588***
	(51.708)	(0.012)	(69.084)	(62.063)	(0.018)	(77.241)	(77.548)	(0.016)	(106.162)
18-24 mths after	-132.321***	0.031***	191.107***	-146.490***	0.019	58.403	-121.576*	0.041**	295.033***
	(43.541)	(0.012)	(72.020)	(51.753)	(0.018)	(80.612)	(65.422)	(0.016)	(110.486)
25-50 mths after	-19.050	0.023*	187.026**	-34.300	0.005	25.710	-4.243	0.037**	320.325***
	(22.661)	(0.012)	(73.276)	(28.007)	(0.018)	(86.180)	(33.245)	(0.016)	(110.776)
Avg. of dep. var.	2636	0.827	4568	2071	0.827	3356	3069	0.827	5494
R-squared	0.196	0.084	0.166	0.206	0.094	0.156	0.192	0.081	0.154
Observations	1,109,276	1,109,276	1,109,276	479,620	479,620	479,620	629,656	629,656	629,656
Clusters	21,323	21,323	21,323	9395	9395	9395	11,928	11,928	11,928

Notes: Table (A3) shows the difference in differences estimates for subsamples splitted by gender for unemployment benefits, employment, and earnings respectively. Columns 1 to 3 replicate the baseline estimates, columns 4 to 6 include only females, and columns 7 to 9 include only males. Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Table A4: Difference in differences estimates by earnings

	Baseline			Low ($\leq$ 3901.615 CHF per month)			High ($>$ 3901.6 CHF per month)		
	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings	UE Benefits	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	11.867	0.010	52.816	-58.991	0.008	48.202	98.863	0.012	53.910
	(48.991)	(0.009)	(50.468)	(46.654)	(0.012)	(45.101)	(81.818)	(0.013)	(87.790)
13-17 mths after	-91.685*	0.024**	200.458***	-21.624	0.007	62.159	-148.263*	0.042**	340.684***
	(51.708)	(0.012)	(69.084)	(48.935)	(0.016)	(62.227)	(89.659)	(0.017)	(122.616)
18-24 mths after	-132.321***	0.031***	191.107***	-14.336	0.014	12.512	-234.009***	0.050***	370.059***
	(43.541)	(0.012)	(72.020)	(41.907)	(0.017)	(66.230)	(75.468)	(0.017)	(126.754)
25-50 mths after	-19.050	0.023*	187.026**	-14.982	0.024	38.185	-31.313	0.025	328.908***
	(22.661)	(0.012)	(73.276)	(23.904)	(0.017)	(65.578)	(35.963)	(0.016)	(127.203)
Avg. of dep. var.	2636	0.827	4568	1726	0.773	2411	3554	0.881	6743
R-squared	0.196	0.084	0.166	0.165	0.071	0.078	0.217	0.094	0.131
Observations	1,109,276	1,109,276	1,109,276	553,081	553,081	553,081	556,195	556,195	556,195
Clusters	21,323	21,323	21,323	10,450	10,450	10,450	10,873	10,873	10,873

Notes: Table (A4) shows the difference in differences estimates for subsamples splitted by earnings for unemployment benefits, employment, and earnings respectively. Columns 1 to 3 replicate the baseline estimates, columns 4 to 6 include low earnings ($\leq$ 3901.6 CHF per month), and columns 7 to 9 include high earnings ($>$ 3901.6 CHF per month). Averages of dependent variables show average employment and earnings 3 months before unemployment start, and average unemployment benefits 2 months after unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

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