How Does a Reduction in Potential Benefit Duration Affect Medium-Run Earnings and Employment?

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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 reform that reduced PBD from 24 months to 18 months for job seekers younger than 55 years in Switzerland in 2003. Adopting a difference-in-difference framework, we find that this reduction in PBD increases 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. Effects on employment are also positive but weaker than earnings effects. The positive medium-run effects are concentrated among job seekers who were previously employed in R&D intensive industries and whose previous occupation consisted mainly of manual tasks. Unemployment insurance can affect medium-run labor market outcomes via its effects on skill depreciation or unemployment stigma among older job seekers.

JEL Classification: C41, J64, J65

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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. All OECD member countries currently have a system of unemployment insurance. Yet the details of the unemployment insurance system vary tremendously across the OECD.[1]

This paper studies whether PBD affects earnings and employment of job seekers in the period of four years after entering unemployment. Understanding whether PBD matters for medium-run earnings and employment is important for at least two reasons. First, a policy assessment of changes to PBD that focuses only on its impacts on the government budget is too narrow if PBD also affects job quality. The fiscal benefit of reducing PBD comes at a potentially large cost if reductions to PBD deteriorate post unemployment job quality. Conversely, reducing 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, existing discussions of the optimality of unemployment insurance ignore its potential effects on post unemployment jobs (Chetty, 2008; Schmieder et al., 2012a). These formulas need to be adapted if PBD affects job quality.

On a theoretical level, it is not clear how longer benefit duration affects post-unemployment outcomes. Standard job search theory predicts that 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. In contrast, shortening PBD might even improve wages and earnings in a context where skill depreciation is important. Reductions in PBD improve labor market chances by shortening unemployment duration (Shimer and Werning, 2006). Alternatively, firms may use unemployment duration as a screening device (Gibbons and Katz, 1992). Evidence indicates that prolonged unemployment duration is detrimental to the hiring chances of job seekers (Oberholzer-Gee, 2008; Kroft et al., 2013).

This paper analyzes a reform to Swiss unemployment insurance that reduced PBD

[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 in 2011. 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 both the benefit level and the degree to which benefits are maintained in the course of the spell varies tremendously across OECD members.

from 24 months to about 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 receipt by 6.5 percentage points in the period 18 to 24 months after entering unemployment. Job seekers compensate this reduction in benefits by leaving unemployment for jobs thus increasing employment by 3.3 percentage points (pctp) and labor earnings by 3.7 percent. Interestingly, we find that the positive effects of the benefit reduction *persists* beyond the period that is insured by UI. Specifically, employment remains 1.5 pctp higher and earnings stay 3.3 percent higher compared to the situation without the reduction in PBD. Sub-sample analyses indicate that the post-UI effects are especially important for job seekers coming from R&D intensive industries and for individuals whose previous occupation required manual 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. In contrast, Card et al. (2007) show that the spike at benefit exhaustion has been over-stated in analyses that focus on registered unemployment duration. Evidence on the effect of PBD in European studies also finds strong effects.[3] A common objection against these studies is policy endogeneity. Benefits are typically extended

[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]Hunt (1995) finds substantial disincentive 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.

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.

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. Caliendo et al. (2013) 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 that the unemployed who obtain a new job close to benefit exhaustion are more likely to leave 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 individuals at the bottom of the wage distribution. Le Barbanchon (2012) finds a significant and large effect of benefit duration on unemployment exits to work but no effects on wages or employment. 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 strand of the literature discusses policy design. Starting from the original insight of Baily (1978), Chetty (2008) uses reduced form evidence 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 replacement rate reductions.

This paper complements existing studies on the job quality effects of PBD in at least three respects. First, we focus on employment and earnings, outcomes that can be observed for *all* job seekers. In contrast, by focusing on wages and sub-sequent job tenure, the existing literature analyzes outcomes that are only observed for job seekers who find employment. Interpreting effects on job finders is challenging due to selection into employment. Second, we adopt a longer time window that allows estimating not only short-term immediate effects but also effects that build up over time. For instance, if shortening PBD reduced the depreciation of job seekers' leadership skills, labor market outcomes will improve only in the medium-run when job seekers had time to demonstrate those better leadership skills. Finally, we perform 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 for positive medium run effects.

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

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

requirements of a regular job - they must be "employable". Job seekers who are ineligible for unemployment insurance can claim 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 benefit payments during a two year framework period. Those 520 benefit days are equivalent to two years of potential benefit duration since a calendar year has 260 work days. The replacement ratio is 80 % for workers earning less than 3'536 CHF.[5] prior to unemployment and not caring for children. The replacement rate decreases gradually to 70 % for job seekers who earned between 3'536 CHF and CHF 4'030 and it stays at 70 % thereafter. Benefits insure monthly earnings up to a top cap.[6] 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. For instance, a job seeker who leaves unemployment after 3 months remains eligible for the remaining months of unemployment benefits during the two year framework period.

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 contributed 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 3'797 CHF and then gradually reduced over the earnings bracket 3'797 to 4'340 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

[5] 1 CHF = 0.83 EUR.

[6] The cap is currently at 10'500 CHF per month and stood at 8'900 CHF before the reform.

[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.

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 and it increased considerably after the bursting of the "dot.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. The changing macroeconomic environment will not introduce a bias into our estimates if aggregate demand for work varies similarly for the treatment and control groups in our analysis. We assess this key condition further below.[8]

3 Data and Descriptive Statistics

This section discusses the data and provides first descriptive information about treatment and control groups.

3.1 Data

The study is based on two data sources. The first concerns 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.[9] The UIR also contains information on the date when the job seeker starts her or his new job – 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 de-registration date, 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.

The second data source contains information on unemployment benefit payments, employment and earnings from the Social Security Data (SSD). This data covers the universe of all individuals who have contributed to the mandatory first pillar retire-

[8]Note that our analysis identifies a lower bound on the positive effects as younger worker's unemployment is more sensitive to the cycle than older workers' unemployment (Clark and Summers, 1981).

[9]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 lose a job. This is typically a quarter before they actually lose their job.

ment pension system between the period between 1982 and 2010. 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 and some information on transfer income (e.g. unemployment benefits are included but not social assistance). Moreover, for a subsample of around 35 % of the universe of spells we also observe 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.

We impose a number of additional sampling restrictions on the merged database. 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. Second, the sample contains only individuals who contributed to the unemployment insurance for at least 18 of the last 24 months before getting unemployed. This ensures that all job seekers aged 55 or older kept eligibility to two years of benefits. Third, the reform was applied to in-progress spells. This implies that some individuals in the before-treatment regime could actually have experienced a reduction in PBD while unemployed. In order to reduce this potential source of bias, we exclude job seekers who enter unemployment up to 12 months before the reform in July 2003. Fourth, we only consider individuals who are full-time unemployed in the first month of unemployment.[10] The final sample contains 62'563 spells.

3.2 Treatment and Control Groups

Table 1 provides information on how treatment and 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. Excluding job seekers who were employed for less than 18 months in the last 24 months prior to the start of the unemployment spell ensures that only job seekers in the treatment group are affected by the cut in PBD. Still, a potential issue could be that the months employed within a two year window prior to unemployment start do not necessarily perfectly coincide with the two year framework period that determines eligibility for benefits. However, over 85 % of our sample claimed unemployment benefits within 3 months after unemployment start, so that eligibility issues should not play a major role.[11]

[10] Workers who lose one of two part-time jobs are eligible for UI on the job they lost. These job seekers are part-time unemployed. We focus on the full-time unemployed to achieve a homogeneous sample.

[11] One might think that the regression discontinuity (RD) design could also be implemented (Lee and Lemieux, 2010). Yet note that benefit eligibility does change discontinuously in age. A job seeker who enters unemployment at age 54 years and 11 months will initially be entitled to 18.5 months of

Table 1: Treatment assignment

Age	Prior UI contributions	Benefit entitlement before	Benefit entitlement after	Group
< 55	$\geq$ 18 months	520	400	Treatment
≥ 55	$\geq$ 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.[12] 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. Also, we define a binary variable for benefit receipt that takes the value 1 if unemployment benefits were positive in a month, and zero otherwise. We observe 22'170 spells of job seekers whose unemployment spell started before the reform was implemented on July 1st, 2003 – 9'529 in the treatment group, and 12'641 in the control group (table 2). We observe 40'393 unemployment spells starting after July 1st, 2003 – 17'307 spells belong to the treatment group and 23'086 belong to the control group.

benefits but rapidly up-grade to 24 months of benefits once he or she has celebrated her or his 55^{th} birthday. Alternatively, one could think of using the number of contribution months as a running variable. This is challenging for two reasons. Our records indicate that prior contribution months as measured in the SSD are an imperfect predictor of eligibility. We suspect measurement error in prior contribution months. Second, prior contribution months are also unlikely to satisfy the requirement that the running variable can not be manipulated. For these reasons we have adopted a difference-in-difference framework.

[12]Individuals can appear multiple times in our sample: For 8 % of the individuals in the sample, we observe two or more spells.

Table 2: Selected descriptive statistics

	Before reform		After reform		
Treatment status	$D_i = 0$	$D_i = 1$	$D_i = 0$	$D_i = 1$	DiD
	(1)	(2)	(3)	(4)	(5)
Dependent variables (prior to unemployment)					
Employment	0.92	0.91	0.94	0.93	0.00
Earnings (CHF per month)	5003.82	5075.77	5204.16	5252.01	-24.09
Unemployment benefits (CHF per month)	240.78	250.26	167.47	174.73	-2.21
Control variables					
R&D intensity	0.53	0.51	0.49	0.46	0.00
Cognitive	0.50	0.49	0.48	0.48	0.02**
Prior work exp.	0.75	0.73	0.82	0.78	-0.02**
Leader position	0.74	0.72	0.74	0.73	0.01
Female	0.42	0.43	0.45	0.46	-0.01
Swiss	0.72	0.70	0.75	0.71	-0.03***
Marital status					
Single	0.11	0.12	0.11	0.13	0.01
Married	0.64	0.62	0.63	0.63	0.02**
Widow	0.04	0.03	0.04	0.02	0.00
Divorced	0.22	0.23	0.23	0.21	-0.02***
Years of schooling					
$\leq$ 7 years	0.04	0.04	0.03	0.04	0.00
8-9 years	0.11	0.11	0.15	0.15	0.00
10-11 years	0.04	0.04	0.05	0.05	0.00
12-13 years	0.26	0.24	0.38	0.36	0.00
$\geq$ 14 years	0.06	0.06	0.08	0.10	0.01***
Other	0.50	0.51	0.31	0.30	-0.01
No. of observations	475'900	631'589	865'350	1'154'300	
No. of spells	9'529	12'641	17'307	23'086	

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. Column (5) shows the differences in differences. 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. *** P<0.01 ** P<0.05 * P<0.1.

Table 2 presents selected summary statistics for the treatment ($D_i = 1$) and control ($D_i = 0$) group for spells that start before (columns 1 and 2) and after (columns 3 and 4) the reform. In the period 50 months before entering unemployment, about 92 percent of all job seekers who start a spell before the reform were employed. The employment probability does not differ across the treated and control groups. Employment prior to the unemployment spell was about 1 percentage point higher for job seekers who start a new spell in the period after the reform. Results are similar for earnings. Job seekers in the control group earn about 5'000 CHF per month (about 4'150 EUR) before the reform, and monthly earnings are about the same for treated job seekers and for both groups after the reform.

Table 2 also presents information on two key pieces of information that we will use to learn more about skill obsolence and depreciation. The first information is *R&D intensity* of the previous employer. We infer R&D intensity of an industry as 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 losing job. R&D intensive industries are those that have expenditures that exceed the median expenditure, the remaining industries representing the low R&D industries.[13] The share of job seekers from high R&D industries slightly exceeds 50 % for treated and untreated before the reform. However, after the reform, the proportion of job seekers from R&D intensive industries decreases slightly to around 49 % and 46 % respectively.

The second information is related to the task content of the occupation of job seeker. *Cognitive* refers to job seekers whose previous occupation consisted mainly of cognitive tasks. For the classification of occupations into cognitive and manual task content, we adopt an approximation suggested in Acemoglu and Autor (2011). The authors 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.[14] Before the reform, the proportion is 50 % for untreated and 49 % for treated individuals respectively. After the reform, the proportion of mainly cognitive skilled job seekers in the control and treatment groups decreases to 48 %.

Prior work experience is the proportion of job seekers 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 three quarters for spells that started before the reform. After the reform, this proportion slightly increases to 82 % for individuals in the control group, and to 78 % for the treatment group. Around 74 % of the individuals in the control group, and roughly 72 % of the individuals in the treatment group worked in a leader or expert position.

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 70 % in the treatment group and around 72 % in the control group. 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 around 4 % are widowed. The largest differences between unemployment starts before and after the reform are found for years of schooling: The

[13] 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.

[14] 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.

share of individuals with less than 7 years of schooling, and between 10 and 11 years of schooling remains fairly stable over time and across treatment and control groups, and increases slightly over time for job seekers with between 8 to 9 years of schooling and with more than 14 years of schooling. The share of individuals with 12 to 13 years of schooling, however, increases largely from around 24-26 % before to around 36-38 % after the reform. At the same time the share of individuals for whom the attained education level is unknown decreases from around 50 % to 30 % over time. Changes in data quality account for this substantial shift in measured education levels. This shift affected treated and untreated individuals in the same way, and will not invalidate our identification strategy. Moreover, except for job seekers with 14 years or more of schooling, education levels do not differ statistically significantly between control and treatment groups before and after the reform.

Column (5) of Table 2 presents difference-in-difference estimates on the control variables. The null hypothesis that the composition of the treated group did not change can not be rejected for most of the variables. Yet a few characteristics show significant differences between the treatment and the control group before and after the reform. We reject the null hypothesis of no change in the composition of the two groups for the share of job seekers in cognitive occupations, prior work experience, Swiss nationality, marital status, and education. Yet note that the resulting changes in sample composition are small. We find below that accounting for these changes in sample composition does not affect results.

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 identification 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 difference-in-difference estimator can be rewritten as

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

which is the average treatment effect on the treated.

The main assumption that has to hold for the difference-in-difference estimator to identify the average treatment effect on the treated in repeated cross sections are parallel time trends for the treatment and control group in absence of the treatment, i.e. $E(Y_1(0) - Y_0(0) \mid D = 1) = E(Y_1(0) - Y_0(0) \mid D = 0)$.[15] This assumption could be violated for at least three reasons. First, repeated cross sections could differ in terms of sample composition. Second, labor market outcomes might evolve differently across treatment and control groups because their outcomes differ with respect to sensitivity to the cycle. Third, the reform might also have changed the incentives to become unemployed thereby changing the composition of the unemployment inflow.

We now test each of these reasons for failure of the identifying assumption. We have already presented a test for a change in sample composition (see Table 2, last column). We do find that the test rejects the null of no change in sample composition for a range of background characteristics. But note that the changes in sample composition are fairly small in an economic sense. We further address changing sample composition by discussing the sensitivity of our results to adding observed characteristics.

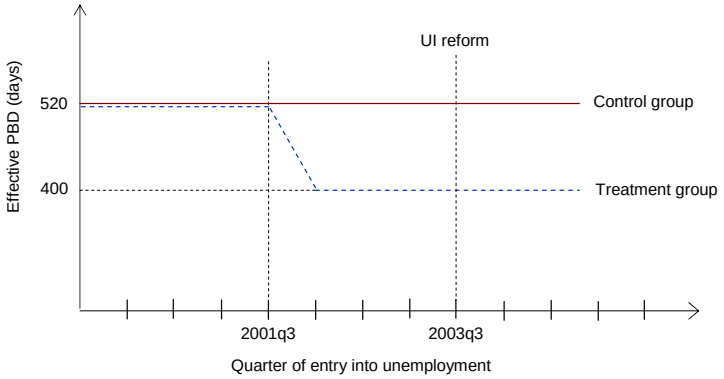
Second, we assess whether time trends evolve in a parallel fashion across treated and control groups. We focus on unemployment benefit receipt in 22 to 24 months after job seekers, i.e. benefit receipt in the last quarter of a job seeker's framework period of two years. The idea of this check is simple. The last quarter of a job seeker's framework period should be mechanically affected by the reform in July 2003. Plotting benefit receipt by quarter of entry into unemployment for groups that were not affected by the reform will provide a visual test of parallel trends. We also visually inspect time trends after the reform was implemented to see whether the effect of the reform is constant and time trends continue to evolve in a parallel fashion after the reform has been implemented.

Note that the reform was applied to in-progress spells. This means that treated job seekers start to be affected by the cut in PBD even if their spell started before July

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

1, 2003. Figure 1 shows that the treatment group starts to be affected by the cut in PBD for spells that start after July 1, 2001 because the reform gradually removes the final months of benefit eligibility. For instance, a job seeker starting unemployment on January 1, 2002 will be fully affected by the reform since her or his last 6 months of benefit eligibility will be cut by the reform in July 1, 2003. In other words, the effective PBD for the treatment group reduces gradually from 520 to 400 days for entries into unemployment between July 2001 to January 2002. Finally, for spells that started after January 2002, the treated job seekers get a maximum number of 400 days, whereas untreated job seekers still get 520 days of unemployment benefits.

Figure 1: Timing of Reform



Notes: Figure 1 shows the stylized pattern of effective PBD over the quarter of entry into unemployment for the treatment and the control group respectively.

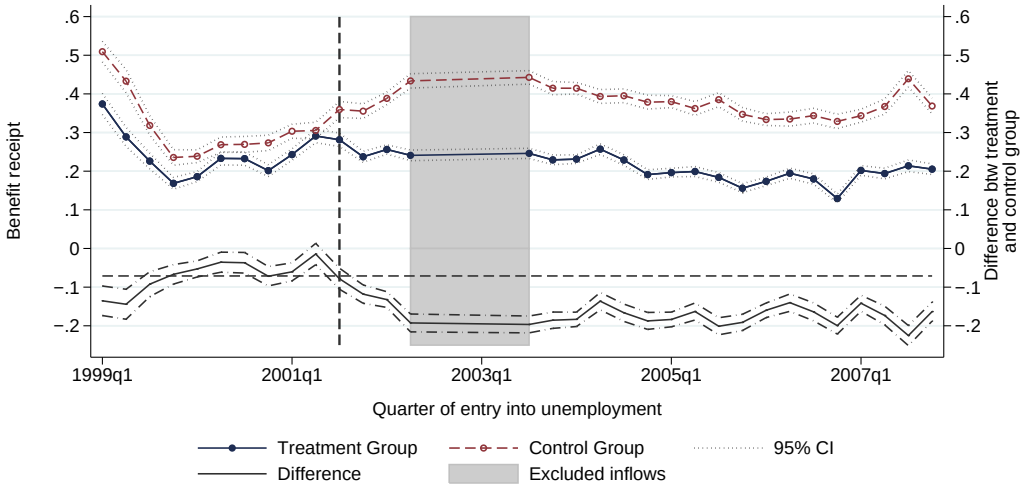
Figure 2a shows unemployment benefit receipt 22 to 24 months after unemployment start of treated and control groups for every quarter between 1999 and 2007. The left hand axis measures the share of job seekers who claim benefits. The right hand axis measures the difference between treatment and control groups. The dashed vertical line in the third quarter of 2001 depicts the first possible date for which effects of the reform are potentially observable. The dashed horizontal line indicates the difference between the treated and control group before the reform.

Figure 2a shows several interesting facts. First, the control group tends to have about 10 percentage points higher benefit receipt than the treated group before the reform because the control group is older than the treated group. Second, benefit receipt varies quite strongly over the period 1999 to 2007 – very much in line with the business cycle. Third, time trends are roughly parallel in the period before the reform, especially so for job seekers entering unemployment between 2000 to the second quarter of 2001. Fourth, the reform led to a substantial reduction in unemployment benefit receipt. This effect can be seen for job seekers entering unemployment in the third quarter of 2001 and later. Finally, the difference in benefit receipt remains

approximately constant for all job seekers entering unemployment after the reform. This evidence is therefore consistent with parallel trends in benefit receipt also after the reform.

Figure 2: Time trends 22 to 24 months after unemployment start

(a) Benefit receipt

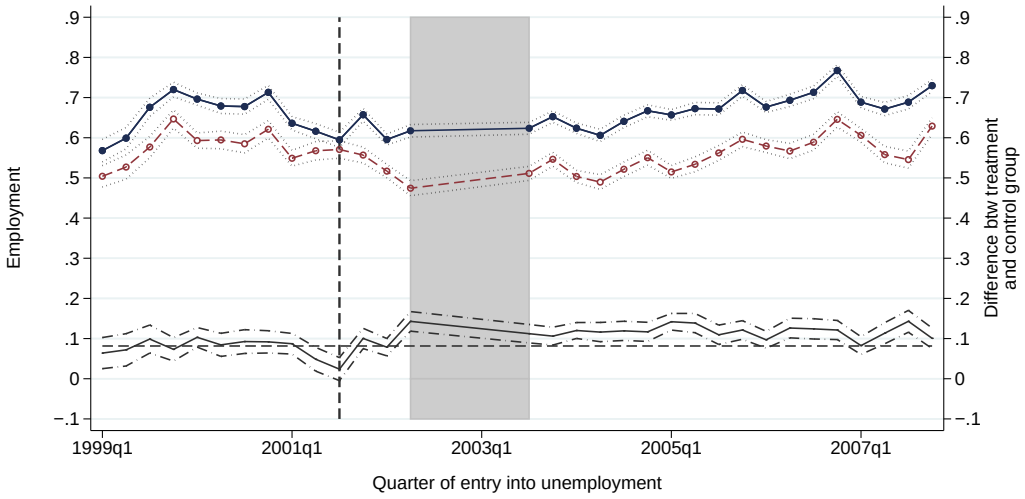


Notes: On the left hand axis figure 2 shows the time trends for the treatment and the control group for benefit receipt. The dotted lines around the time trends of the control group indicate the 95 % confidence interval. On the right hand axis, the solid line at the bottom of each subfigure shows the difference between treatment and control group together with the 95 % confidence interval. The dashed vertical line at the 3rd quarter of 2001 depicts the first possible date for which treatment effects are possibly detectable. The dashed horizontal line shows the difference in benefit receipt between the treated and the control groups. Shaded area indicates that no data is available for that time period (inflow between July 2002 and June 2003 was omitted from the analysis).

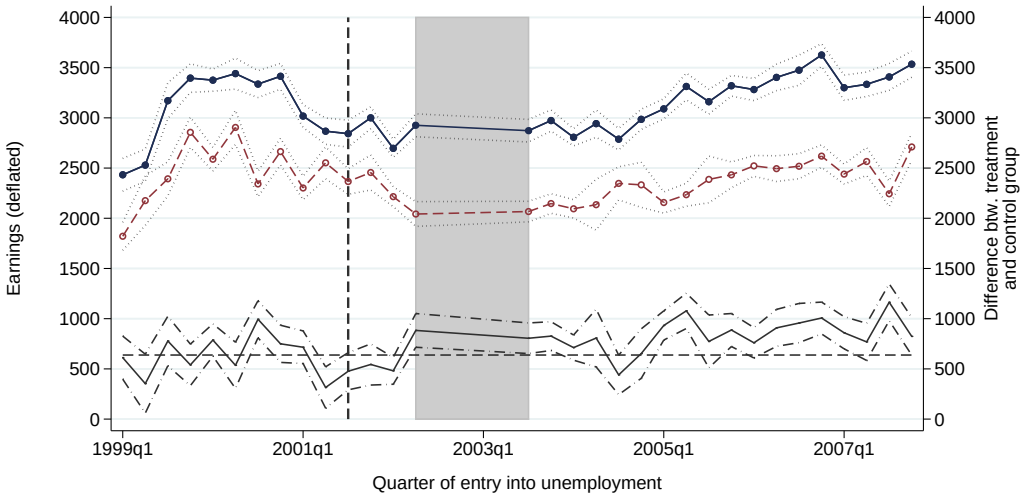
Are trends in employment and earnings also parallel? Figures 3a and 3b report a similar analysis for employment and earnings. Results indicate that trends are parallel for both outcomes for spells that start before the third quarter of 2001. This evidence suggests trends in outcomes are similar. Moreover, 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 3: Time trends 22 to 24 months after unemployment start (ctd.)

(a) Employment



(b) Earnings



Notes: On the left hand axis figure 3 shows the time trends for the treatment and the control group for employment (figure 3a) and earnings (figure 3b) 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 dashed vertical line at the 3rd quarter of 2001 depicts the first possible date for which treatment effects are possibly detectable. The dashed horizontal line shows the difference in benefit receipt between the treated and the control groups. Shaded area indicates that no data is available for that time period (inflow between July 2002 and June 2003 was omitted from the analysis).

The third test we implement checks for endogenous entry into unemployment, i.e. if the treated enter unemployment less frequently because they expect a lower benefit duration, this assumption would be violated. Figure 4 shows the inflows into unemployment for the treatment and control groups. The left hand axis measures the number of unemployment registrations per quarter. The right hand axis measures the inflow ratio between treatment and control group. If there was endogenous entry

into unemployment, we would expect a drop in the number of registrations in the treatment group after the reform relative to the control group. Graphical evidence however indicates that the inflow ratio does not drop after the reform, but is relatively stable over time.

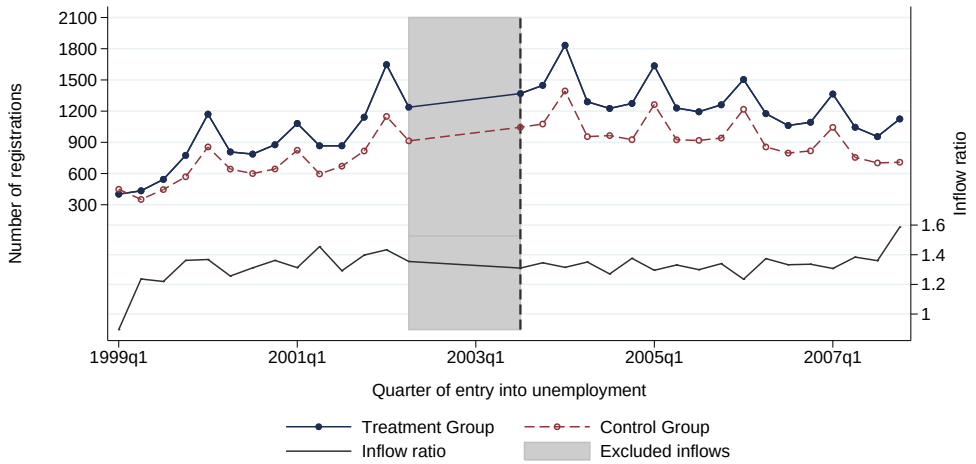
Table 3: Difference in differences estimates for unemployment inflows

	Log(# of registrations)
$D_i A_c$	0.033
	(0.034)
D_i	0.260***
	(0.032)
Time Fixed Effects	Yes
Observations	64
R-squared	0.986

Notes: Table 3 shows the difference in differences estimates for the logarithm of the number of registrations. The regression includes quarterly time dummies. Robust standard errors in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Table 3 presents a formal test of stability of the inflow. It presents a regression of the treatment dummy D_i, the interaction term $D_i A_c$ and a set of quarterly time dummies on the logarithm of the number of registrations per quarter. The reform does not significantly affect the inflow into unemployment in the treated group. This confirms that the reform did not affect the likelihood of entering unemployment.

Figure 4: Unemployment inflows (number of registrations)



Notes: On the left hand axis figure (4) shows the time trends of the unemployment inflows of the treatment and the control group. On the right hand axis, the solid line at the bottom of each subfigure shows the inflow ratio between treatment and control group. The vertical line in the 3rd quarter of 2003 depicts the date of the reform.

We conclude that the key assumption of parallel trends is likely to be satisfied in the current context.

5 Results

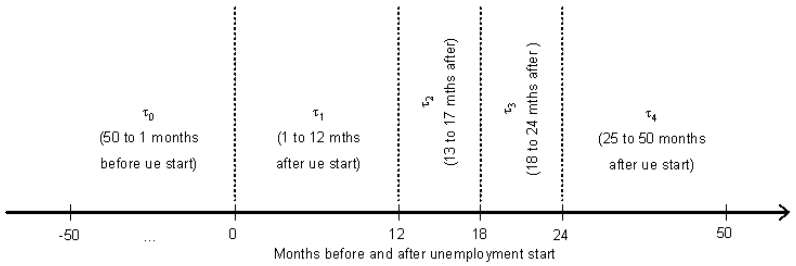
This section discusses the estimation results. Subsection 5.1 presents graphical evidence, subsection 5.2 presents the main estimation, subsection 5.3 discusses some sensitivity estimations, and subsection 5.4 analyses the issue of heterogeneity in treatment effects. Subsection 5.5 relates our results to the existing literature on job-match quality.

5.1 Descriptive evidence

Figure 5 shows the structure of the data. We distinguish five periods: τ_0 is the period before unemployment start, i.e. 50 to 1 months before unemployment start. 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 effect of the reduced PBD. The effect on benefit receipt will be negative and largely mechanic since the reform removes unemployment benefit payments during that period.[16] The effects on employment and earnings will show endogenous responses to the removal of benefits during period τ_3. Finally, τ_4 captures the period 25 to 50 months after unemployment start and allows to identify medium-run effects of the PBD. Period τ_4 is our primary focus since all job seekers have exhausted their framework period after two years. This period allows detecting effects of PBD reductions on medium-run earnings and employment.

[16] Some job seekers will keep eligibility to benefits even during period τ_3. These are the job seekers who re-enter after a short employment spell or job seekers who re-establish eligibility to unemployment benefits.

Figure 5: Data structure



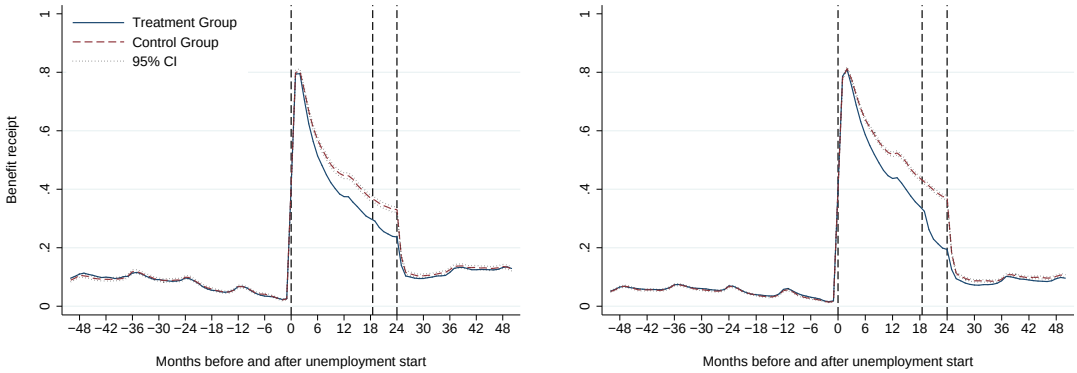
Notes: Figure 5 shows the data structure with its division into τ_0 to τ_4.

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 2006 since our observation period ends in December 2010 (13 % of all spells). This lack of observation window should, however, not impair our identification strategy, because both treated and untreated 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 years old 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 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 6 shows average benefit receipt - i.e. the proportion of treated (50 to 54 years old) and untreated (55 to 59 years old) receiving unemployment benefits - up to 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 6a depicts benefit receipt for individuals who registered before the policy change in July, 2003 and figure 6b shows the same for individuals who registered after the reform in July, 2003.

Figure 6: Unemployment benefit receipt before and after the reform

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



Notes: Figure 6a shows unemployment benefit receipt 50 months before and 50 months after unemployment start for individuals who entered unemployment before July 1, 2003. Benefit receipt 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 proportion of job seekers with unemployment benefits for spells that started after July 1, 2003. The dotted lines around the benefit receipt of the control group indicate the 95 % confidence interval.

Benefit receipt does not differ between the treated and the untreated before the start of the unemployment spell. Unemployment benefit receipt prior to unemployment start amounts to around 6 % on average. Pre-unemployment benefit receipt is 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 benefits.[17] This is observed in the data by a sharp increase in average benefit receipt to around 80 % in the first month after unemployment start. The share of job seekers claiming unemployment benefits drops as time passes because job seekers gradually re-enter employment or exit the labor force through alternative pathways.

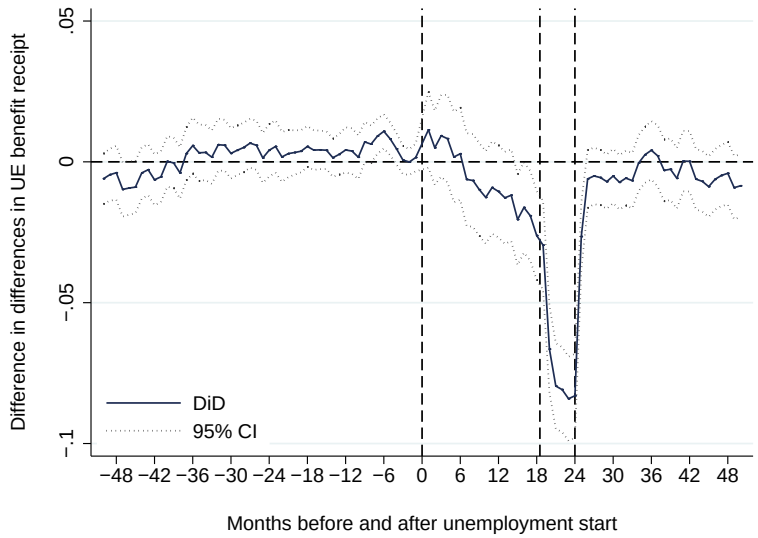
Benefit receipt of treated and untreated starts to diverge after the peak around unemployment start: Job seekers in the treatment group claim on average fewer unemployment benefits than job seekers in the control group. 12 months after the start of a spell there is a kink for both groups. The kink is 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. A small kink is also observed for job seekers whose spells started *before* the UI policy change (Figure 6a). This is because the reform was applied to in-progress spells: Some job seekers in the before-treatment regime

[17] 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.

are affected by the reform even if their spells started before the 2003 reform. The kink is however much 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. Benefit receipt sharply drops, and falls back to almost its pre-unemployment level thereafter.

Figure 7 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), benefit receipt has evolved in the same way for treated and control groups, the diff-in-diff estimates are close to zero and not significantly different from zero (except for the period between 7 and 5 months before unemployment start). Around 6 months after the beginning of a spell, the difference in differences starts to turn negative, reaching its minimum in the treatment period τ_3 (18 to 24 months after) where benefit receipt of treated job seekers is on average around 8 percentage points lower compared to the untreated individuals. 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, benefit receipt is no longer affected by the reform, the diff-in-diff turns not significantly different from zero.

Figure 7: Difference in differences in unemployment benefit receipt

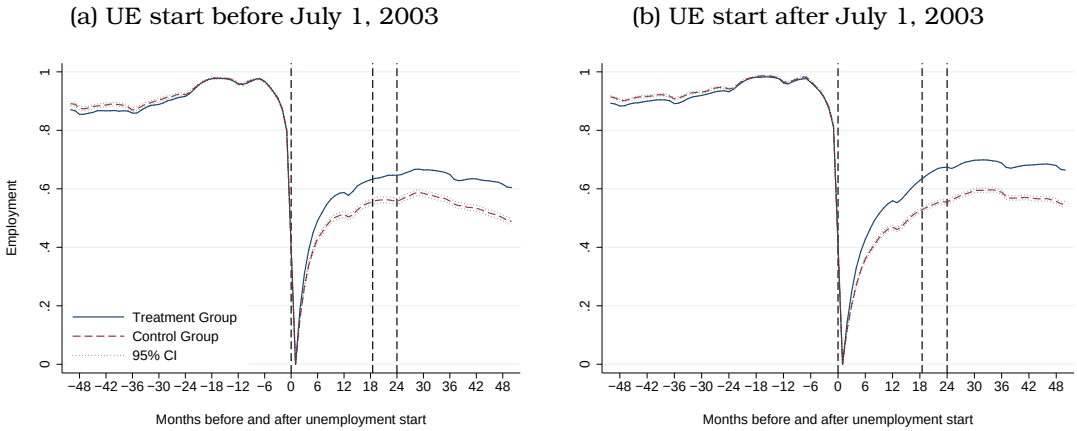


Notes: Figure 7 shows the difference in differences for unemployment benefit receipt for the 50 months before and 50 months after unemployment start. The dotted lines around the difference in differences indicate the 95 % confidence interval.

Figure 8 replicates the above graphical analysis for the employment ratio. Prior to the unemployment spell (50 to 1 months before unemployment) anywhere between 80 % and 98 % of all job seekers are employed. For both the treated and the untreated,

employment already starts to fall in the last 12 to 6 months before getting unemployed. In the first month of unemployment, the employment ratio drops to zero. The unemployed start to find new jobs, and the average employment share rises again to around 60 % in the control group and to around 65 - 70 % in the treatment group. 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 8a) and after (figure 8b) 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.

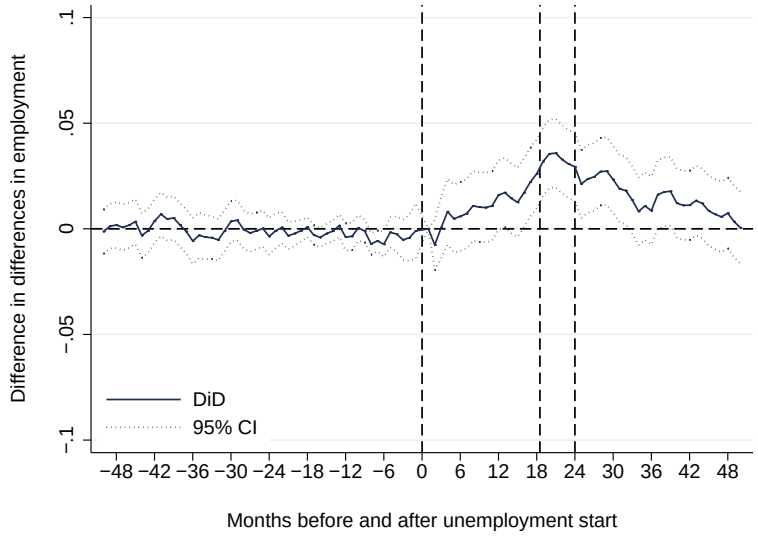
Figure 8: Employment before and after the reform



Notes: Figure 8a 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 (≥ 55 years old at unemployment start). Figure 8b 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 9 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 around 3.5 percentage points 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. In the medium run period τ_4, the positive employment effects gradually taper off.

Figure 9: Difference in differences in employment

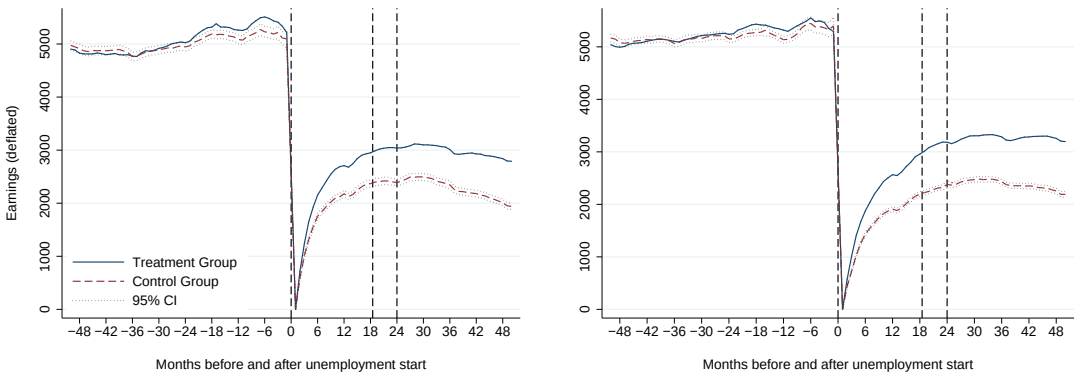


Notes: Figure 9 shows the difference in differences for employment for the 50 months before and 50 months after entering unemployment. The dotted lines around the difference in differences indicate the 95 % confidence interval.

A similar, but more volatile pattern is also observed for earnings. Figure 10 shows that pre-unemployment earnings lie around 5'000 CHF (about 4'150 EUR), and drop to zero at unemployment start. Like the employment share, earnings rise again, but do no longer reach the pre-unemployment level, and stay at a level of between 2'500 CHF for the control group, and around 3'000 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 10a) or *after* (figure 10b) the reform, earnings increase more for the treated than for the untreated in the *after* reform period.

Figure 10: Earnings before and after the reform

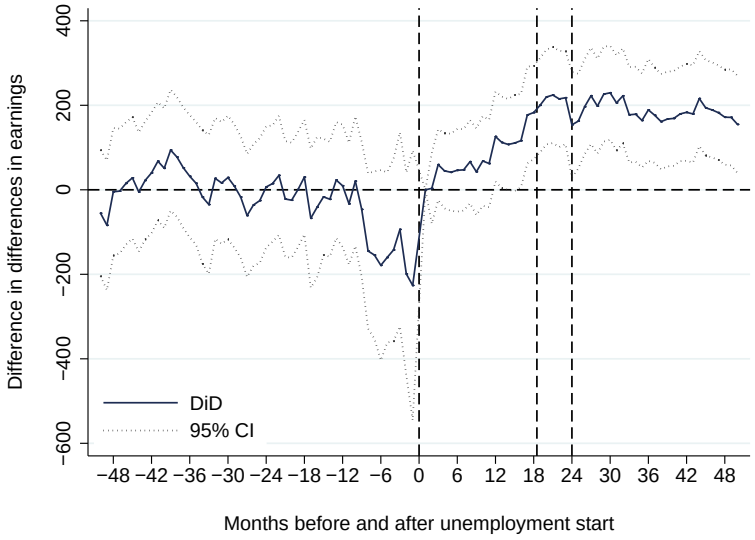
(a) UE start before July 1, 2003 (b) UE start after July 1, 2003



Notes: Figure 10a 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 10b 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.

The difference in differences graph for earnings completes the picture. Prior to entering unemployment, earnings are perfectly balanced across the treatment and control groups (period τ_0). The earnings difference starts to rise significantly after the beginning of a spell to around 200 CHF in the beginning of the treatment period τ_3 (18 to 24 months after unemployment start), and it remains relatively stable and significantly different from zero also in the medium run period τ_4 (25 to 50 months after unemployment start). In contrast to the result for employment, shortened PBD therefore increases earnings permanently.

Figure 11: Difference in differences in earnings



Notes: Figure 11 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.

5.2 Main estimates

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

$$\begin{aligned} Y_{itc} =& \alpha_1 + \alpha_2\tau_{2t} + \ldots + \alpha_4\tau_{4t} + \beta_1\tau_{1t}D_i + \ldots + \beta_4\tau_{4t}D_i + \gamma_1\tau_{1t}A_c + \ldots + \gamma_4\tau_{4t}A_c + \\ &+ \delta_1\tau_{1t}D_iA_c + \ldots + \delta_4\tau_{4t}D_iA_c + X_i'\eta + \varepsilon_{itc} \end{aligned} \tag{1}$$

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_{1t} to τ_{4t} are indicators for the different periods after unemployment start, i.e. $\tau_{1t} = \mathbb{1}(1 \leq t < 13$ months),

$\tau_{2t} = \mathbb{1}(13 \leq t < 18 \text{ months})$, $\tau_{3t} = \mathbb{1}(18 \leq t < 24 \text{ months})$, and $\tau_{4t} = \mathbb{1}(24 \leq t \leq 50 \text{ months})$ respectively. δ_1 to δ_4 are the coefficients for the interaction effects $\tau_{1t} D_i A_c$ to $\tau_{4t} D_i A_c$, 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 benefit receipt during months 7 to 5 prior to the spell we analyze (see figure 7). 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 4 presents the baseline results. 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 with the treatment dummy D_i, the time dummy A_c, and the product of the two. This specification therefore allows for full flexibility of the effect of extended PBD on medium-run earnings and employment. The fact that estimates are stable suggests that the composition effects we found for some covariates are not large enough to introduce a significant amount of bias.

The estimates for unemployment benefit receipt in column (2) indicate that already between 13 and 17 months after unemployment start the treated claim less unemployment benefits than the control group. The treatment effect on benefit receipt amounts to 1.7 percentage points. This treatment effect is interpreted as an anticipation effect. In the period between 18 and 24 months after unemployment start, benefit receipt is on average around 6.5 percentage points lower for the treated. 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 benefit receipt.

The estimates for employment in column (4) show that we observe an anticipation effect of 1.7 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

3.3 percentage points. This effect is not large enough to compensate for the reduction in benefit receipt. Yet employment is also 1.5 percentage points higher for the treated in the medium-run. This positive effect compensates somewhat for lost benefit months among treated job seekers. We will explore below whether the compensation is sufficient to undo the removal of benefits.

Earnings are normalized by average earnings 3 months prior to unemployment start. In column (6), we observe a significant anticipation effect of around 2.2 percentage points. The direct effect for earnings amounts to 3.7 percentage points, and the medium run effect stays at about the same magnitude with 3.3 percentage points. 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 earnings effect and to some smaller extent the employment effect persist in the medium run.

These baseline 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. Baseline results could, however, still be spurious. We now turn to discussing the sensitivity of these baseline findings.

Table 4: Difference in differences estimates for unemployment benefits, employment and earnings

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
$\tau_{1t} D_i A_c$ (1-12 mths after)	-0.001	-0.002	0.006	0.007	0.009	0.008
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
$\tau_{2t} D_i A_c$ (13-17 mths after)	-0.016**	-0.017**	0.017**	0.017**	0.023**	0.022**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.009)
$\tau_{3t} D_i A_c$ (18-24 mths after)	-0.064***	-0.065***	0.032***	0.033***	0.037***	0.037***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.009)
$\tau_{4t} D_i A_c$ (25-50 mths after)	-0.005	-0.006	0.014*	0.015**	0.034***	0.033***
	(0.004)	(0.004)	(0.007)	(0.007)	(0.010)	(0.009)
Controls	No	Yes	No	Yes	No	Yes
Avg. of dep. var.	0.81	0.81	0.91	0.91	5'387.37	5'387.37
R-squared	0.22	0.22	0.05	0.09	0.03	0.18
Obs.	3'073'557	3'073'557	3'073'557	3'073'557	3'073'557	3'073'557
Clusters	57'429	57'429	57'429	57'429	57'429	57'429

Notes: Table 4 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. Earnings are relative to average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

5.3 Sensitivity analyses

We first implement a placebo analysis to check whether trends are indeed parallel before the reform was implemented. To this end, we simulated a UI reform in July

2000 and used only inflows before July 2001. If the treatment effects in the reform periods get significant although there was no treatment in that period, this could be an indication for unequal time trends for the treated and the untreated. Table 5 reports difference in difference estimates of the treatment effects on this placebo reform. The estimated placebo treatment effects are not significant with the exception of a marginally significant employment effect 1 to 12 months after unemployment start (t-statistic of 1.68). In the actual reform period between 18 and 24 months all estimates are however non-significant. We therefore argue that the assumption of equal time trends is not violated in July 2000.[18]

Table 5: Difference in differences estimates for a placebo reform in July 2000

	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)
1-12 mths after	-0.004	0.021*	0.023
	(0.012)	(0.013)	(0.015)
13-17 mths after	0.014	0.003	0.014
	(0.015)	(0.016)	(0.020)
18-24 mths after	0.009	-0.003	0.005
	(0.014)	(0.016)	(0.020)
25-50 mths after	0.000	-0.017	-0.022
	(0.009)	(0.015)	(0.019)
Avg. of dep. var.	0.78	0.91	5'136.36
R-squared	0.17	0.08	0.18
Obs.	604'550	604'550	604'550
Clusters	11'681	11'681	11'681

Notes: Table 5 shows the baseline difference in differences estimates for unemployment benefit receipt (column 1), employment (column 2) and earnings (column 3) for a placebo reform in July 2000. Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

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.[19]

Table 6 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 amplify the adverse health effects of job-loss[20] and thereby increase disability

[18]Note that the power of the Placebo analysis to detect departures from a null effect is smaller than in the main analysis (since standard errors are two times larger). If we adopt the standard errors from the main analysis and test for significance of the effects in the Placebo analysis more Placebo estimates are significant. Nonetheless, the magnitude of the effects are smaller than in the main analysis.

[19]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.

[20]Kuhn et al. (2009) find important health effects of job loss, particularly for men.

pensions, and second, reducing PBD could induce a substitution of unemployment benefits with disability pensions. Table 6 shows the effects of reducing PBD on disability retirement pensions.[21] Point estimates are negative and only marginally significant. Estimates suggest that the use of disability pensions decreases between 20 and 30 % compared to average disability pension benefits before the unemployment spell. In contrast to the concerns mentioned above, these results suggest that the positive employment effects of reducing PBD also lower the need for disability pension claims.

Table 6: Difference in differences estimates for disability retirement

	Disability Pensions
1-12 mths after	-0.206*
	(0.120)
13-17 mths after	-0.185
	(0.147)
18-24 mths after	-0.163
	(0.155)
25-50 mths after	-0.293*
	(0.166)
Avg. of dep. var.	93.59
R-squared	0.06
Obs.	1'153'356
Clusters	21'463

Notes: Table 6 shows the difference in differences estimates for disability pensions normalized by the average disability pensions 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Do reductions in PBD affect old-age pensions? 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 women are eligible for early retirement at the age of 62 years and and men are eligible for old-age pensions at the age of 63 years. This suggests that reductions in PBD do not affect the claiming of old-age pensions.

Nonetheless, age could be an issue because job seekers in the treatment group "grow" into the control group. This will end up reducing our estimates of the treatment effects for the periods τ_2 to τ_4. To address 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 7 reports the estimates for this restricted sample. Excluding the oldest age cohorts in each group does not affect the estimates drastically: Compared to our main estimates, the treatment effects are virtually unchanged

[21] Disability pension data is only available for a random sub-sample of around 35 % of job seekers.

for employment and earnings, and slightly stronger for unemployment benefit receipt. Statistical significance decreases somewhat, because in the restricted sample around one fifth of all observations is lost. The overall picture however is unchanged.[22]

Table 7: Difference in differences estimates by age

	Baseline			50-53 vs. 55-58 years old		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
1-12 mths after	-0.002	0.007	0.008	-0.002	0.007	0.008
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
13-17 mths after	-0.017**	0.017**	0.022**	-0.018**	0.017**	0.023**
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)
18-24 mths after	-0.065***	0.033***	0.037***	-0.073***	0.030***	0.037***
	(0.007)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)
25-50 mths after	-0.006	0.015**	0.033***	-0.008*	0.012	0.034***
	(0.004)	(0.007)	(0.009)	(0.004)	(0.008)	(0.010)
Avg. of dep. var.	0.81	0.91	5'387.37	0.81	0.91	5'419.94
R-squared	0.22	0.09	0.18	0.22	0.09	0.18
Obs.	3'073'557	3'073'557	3'073'557	2'532'295	2'532'295	2'532'295
Clusters	57'429	57'429	57'429	47'919	47'919	47'919

Notes: Table (7) shows the difference in differences estimates for sub-samples 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. Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

5.4 Treatment Effect Heterogenetiy

This section analyzes whether the effects of a reduction in potential benefit duration differ between sub-groups of job seekers with different previous industry and occupation.

A first sample split discusses the role of human capital depreciation due to skill obsolence as a possible driving force of the positive medium-run effects. We split the sample in two groups which likely differ in terms of the speed at which industry specific skills become obsolete: job seekers from industries with high R&D intensity vs job seekers from industries with low R&D intensity (see section 3 for a discussion of how we define R&D intensity of the industry). Skill depreciation is assumed to play a more important role for individuals working in fast-evolving, highly R&D intensive industries, because a job-loss disconnects the unemployed faster from rapid technological change in those industries. An shortened period of unemployment is therefore

[22]We have also explored whether our results are sensitive to how we define the start date of the unemployment spell. Overall, results are similar to the baseline result when we use the date a job seeker registers at the employment center as the start date of her or his unemployment spell. Registration dates are, however, not ideal as unemployment start dates because job seekers need to register at the job center as soon as they are informed that their employment spell ends. This leads to a situation where the effects on outcomes in different phases of the spell get blurred since the timing is not quite correct. Results are available upon request.

expected to be more beneficial for job seekers in highly R&D intensive industries.

Table 8 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 intensive industries, and columns (7) to (9) for job seekers from industries with below median R&D intensity respectively. For both sub-samples we observe a negative effect on benefit receipt in the reform period from 18 to 24 months after unemployment start. The effects on earnings and employment, however, differ considerably between the two groups. The 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. In the former group we observe strong and significant anticipation effects in the period from 13 to 17 months after unemployment start. The direct effects of the reform (18 to 24 months after unemployment start) lead to a 4.6 percentage points increase in employment and to a earnings effect of around 6.8 percentage points. The effects persist also in the medium run (25 to 50 months after unemployment start): Reducing PBD by 6 months leads to a 2.2 percentage points increase in terms of employment and it boosts earnings by 5.2 percentage points in the medium run. In contrast, treatment effects for job seekers leaving industries with low R&D intensity are mostly absent except for an employment effect in the period from 18 to 24 months after unemployment start directly induced by the cut in benefits over that period. Other point estimates are close to zero for employment, and even negative for earnings, but none of them are statistically significant.[23]

A second subgroup analysis discusses the importance of job-specific human capital depreciation due to lack of use of skills (atrophy). In this analysis, we assess whether the task content of the previous occupation matters for the medium-run effect of benefit reductions on earnings and employment. 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 (see section 3 for the definition of these two groups). We expect that skill depreciation differs between the two groups. However, whether skill depreciation would be stronger in mainly cognitive or mainly manual occupations is not clear. One argument holds that cognitive skills depreciate faster because extended periods of unemployment generate adverse effects on mental health. This would indicate that occupations with cognitive

[23]Job seekers in industries with high R&D expenditure have higher mean earnings than job seekers in industries with low R&D expenditure. Yet the difference in the effects of PBD is not simply related to the difference in earnings. When we split the sample by previous earnings, we find positive medium-run effects for both sub-samples (see Table A3 in the appendix).

Table 8: Difference in differences estimates by R&D intensity of previous industry

	Baseline			High R&D intensity			Low R&D intensity		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	-0.002	0.007	0.008	-0.011	0.020**	0.024***	0.006	-0.004	-0.011
	(0.006)	(0.006)	(0.007)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)
13-17 mths after	-0.017**	0.017**	0.022**	-0.030***	0.036***	0.055***	-0.006	0.002	-0.017
	(0.008)	(0.008)	(0.009)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.013)
18-24 mths after	-0.065***	0.033***	0.037***	-0.077***	0.046***	0.068***	-0.056***	0.021**	-0.001
	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.014)
25-50 mths after	-0.006	0.015**	0.033***	-0.007	0.022**	0.052***	-0.004	0.008	0.011
	(0.004)	(0.007)	(0.009)	(0.006)	(0.010)	(0.011)	(0.006)	(0.010)	(0.013)
Avg. of dep. var.	0.81	0.91	5'387.37	0.82	0.92	6'168.21	0.79	0.90	4'642.51
R-squared	0.22	0.09	0.18	0.25	0.10	0.19	0.20	0.08	0.17
Obs.	3'073'557	3'073'557	3'073'557	1'503'425	1'503'425	1'503'425	1'570'132	1'570'132	1'570'132
Clusters	57'429	57'429	57'429	28'927	28'927	28'927	29'473	29'473	29'473

Notes: Table (8) shows the difference in differences estimates for subsamples splitted by innovative pace of industries (as measured by R&D intensity of industries) for unemployment benefit receipt, 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. Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

tasks suffer more from job loss than occupations with manual tasks. Conversely, one might also argue that occupations with cognitive skills might be insulated from atrophy because they are used to maintain those skills better than occupations with manual skills.

Table 9 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. The reform led to a decrease in benefit receipt of around 6.5 percentage points for both groups in the reform period from 18 to 24 months after unemployment start (and a small anticipation effect for job seekers with manual skills). Employment effects are also quite similar between the two groups in the first 24 months (columns (5) and (8)). Reductions in PBD tend to increase employment in the reform period (and also a bit earlier for job seekers with manual skills.) Employment effects over the medium-run period differ strongly between the two groups. Employment is significantly higher for job seekers with manual skills 25 to 50 months after the start of the spell whereas employment is not affected among job seekers with cognitive skills. Earnings patterns also differ strikingly between the two sub-groups (columns (6) and (9)). Job seekers from occupations with largely manual skill content enjoy significantly higher earnings already from the start. The effect is small (1.6 percent) immediately after the spell starts but it builds up to a sizeable 5.1 percent differential in the medium-run period. There is also a positive earnings effects for job seekers in manual occupations but this effect is concentrated in the reform period and comparably small (2.8 percent). Taken at face value, these results suggests that skill depreciation affects occupations with manual skill content more strongly than occupations with cognitive skill content. This result is consistent with direct evidence on skill atrophy. Li (2013), for instance, finds strong human capital depreciation for some manual occupations such as sales and production workers, or, conversely, human capital appreciation for cognitive occupations like education professionals. Görlich and de Grip (2009) find that skill depreciation rates are higher for low-skilled workers than for high-skilled workers.[24]

Finally, we take a closer look at post-unemployment earnings. Reduced PBD can increase earnings either by increasing employment, or by increasing earnings of em-

[24]Table A2 presents subgroup estimations split by gender. We find that the effects are very similar among male and female job seekers. We have also investigated results by the extent of routine or non-routine tasks involved. These estimates (not shown) indicate that occupations with a routine tasks have higher medium run earnings and employment with reduced PBD. No such effect is present for occupations involving non-routine tasks.

Table 9: Difference in differences estimates by task content of previous occupation

	Baseline			Cognitive Tasks			Manual Tasks		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	-0.002	0.007	0.008	0.003	0.008	0.005	-0.008	0.009	0.016*
	(0.006)	(0.006)	(0.007)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)
13-17 mths after	-0.017**	0.017**	0.022**	-0.010	0.020*	0.015	-0.024**	0.016	0.033***
	(0.008)	(0.008)	(0.009)	(0.011)	(0.011)	(0.013)	(0.011)	(0.011)	(0.012)
18-24 mths after	-0.065***	0.033***	0.037***	-0.067***	0.035***	0.028**	-0.064***	0.030***	0.048***
	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)	(0.013)	(0.010)	(0.011)	(0.012)
25-50 mths after	-0.006	0.015**	0.033***	-0.008	0.007	0.019	-0.003	0.021**	0.051***
	(0.004)	(0.007)	(0.009)	(0.006)	(0.010)	(0.012)	(0.006)	(0.010)	(0.012)
Avg. of dep. var.	0.81	0.91	5'387.37	0.82	0.91	6'270.97	0.79	0.91	4'551.34
R-squared	0.22	0.09	0.18	0.24	0.10	0.18	0.20	0.09	0.18
Obs.	3'073'557	3'073'557	3'073'557	1'494'298	1'494'298	1'494'298	1'579'209	1'579'209	1'579'209
Clusters	57'429	57'429	57'429	28'631	28'631	28'631	29'955	29'955	29'955

Notes: Table (9) shows the difference in differences estimates for subsamples splitted by the task content of the previous occupation for unemployment benefit receipt, 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. Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

ployed individuals, or both. Table 10 presents estimates of earnings effects conditional on employment. These results need to be interpreted with caution because earnings of employed individuals are only observed for job seekers who have found employment. The causal effects of PBD may therefore be masked by selection into employment effects.[25]

Results present a clear picture. Reductions in potential benefit duration increase earnings of employed individuals from the first month of unemployment onwards (Table 10 column 1). Earnings of treated job seekers are about 2 percent higher than they would have been without the extension of potential benefits. Earnings are particularly positively affected for job seekers leaving industries that spend a lot on R&D (column 2) – earnings gains are between 4 to 5 percent of post-unemployment earnings. In contrast, job seekers who leave industries with low R&D expenditure tend to have lower earnings when employed, particularly so in the period 18 to 24 months after the unemployment spell started.[26] Interestingly, results by task-content of the occupation display positive point estimates, both for occupations with mainly cognitive task content as well as occupations with mainly manual task content. Point estimates are on the order of 2 percent of post-unemployment earnings, and significant in 4 out of 8 cases. In sum, results in table 10 suggest that reductions in PBD have a positive effect on medium run earnings because of both, somewhat higher employment and higher earnings while employed.

[25]Note, however, our results give a lower bound on the earnings effects if job seekers select into employment based on ability or earnings potential. Reductions in PBD lead job seekers to accept jobs earlier, so more job seekers are observed with reduced PBD than with extended PBD.

[26]Note that this negative effect has been documented also by Caliendo et al. (2013).

Table 10: Difference in differences estimates for employed individuals

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
1-12 mths after	0.019**	0.042***	-0.006	0.014	0.028**
	(0.009)	(0.013)	(0.012)	(0.013)	(0.011)
13-17 mths after	0.023**	0.056***	-0.016	0.025*	0.022*
	(0.010)	(0.014)	(0.014)	(0.014)	(0.013)
18-24 mths after	0.014	0.048***	-0.025*	0.018	0.011
	(0.010)	(0.013)	(0.013)	(0.014)	(0.012)
25-50 mths after	0.022**	0.045***	-0.007	0.025**	0.018
	(0.009)	(0.012)	(0.013)	(0.012)	(0.012)
Avg. of dep. var.	5'916.48	6'705.41	5'148.71	6'861.50	5'015.72
R-squared	0.32	0.32	0.31	0.32	0.31
Obs.	1'702'374	822'642	879'732	861'405	840'957
Clusters	48'188	24'139	24'918	24'682	24'549

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. Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

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. We now discuss what happens if we analyse the effects of the PBD reduction on these direct measures of job-match quality.

Panel A of table 11 reports the effects of reducing the PBD on 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 right censored by the last observed date. On average job seekers spent around 14.6 months in unemployment.[27] Reducing the PBD by 6 months lowers the time spent in unemployment for the treatment group by about 1 month for the entire sample. The effect of benefit reductions is considerably larger in the sub-group of job seekers from R&D intensive industries: treated job seekers leave unemployment about 1.7 months earlier than they would have without a cut in benefits. Interestingly, there is a negative yet insignificant effect on the duration of unemployment of job seekers in industries with low R&D intensity. The effect is less strong on job seekers who leave R&D industries because they do not exhibit an anticipation effect in the period 13 to 17 months after the start of the unemployment spell (see Table 8).

[27]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.

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 0.88 months less unemployed than their counterparts in the control group. Job seekers whose occupation predominantly require manual skills leave unemployment about 1.16 months earlier than they would without the benefit reduction. Effects are similar across the two groups of job seekers.

Panel B presents the difference-in-difference estimates for post-unemployment earnings measured in the second month after re-employment. This analysis is based on all spells where job seekers left unemployment and stayed in their job for at least two months.[28] Results indicate no significant effect in the overall sample. Interestingly, point estimates are positive for the sub-groups with large skill depreciation. The effects are significant for job seekers in occupations with high manual task content, and positive but insignificant for job seekers leaving R&D intensive industries. Point estimates are negative for job seekers with low skill depreciation. The effect is significant for job seekers leaving industries with low R&D intensity and insignificant for job seekers in occupations with cognitive task content. These results reinforce the interpretation that reductions in PBD may improve the lot of job seekers who face rapid skill depreciation or skill obsolence. In contrast, reducing PBD tends to hurt job seekers who do not face human capital depreciation.

[28]We focus on earnings in the second month after re-employment because the first month after re-employment is the month when job seekers leave unemployment. If a job seeker starts her job in the middle of this month, earnings do not reflect full-time monthly earnings. (We do not observe number of days worked on the job so we can not adjust for this.)

Table 11: Effects on unemployment duration, subsequent earnings, and job loss

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
	A. Unemployment duration (months)				
$D_i A_c$	-1.022***	-1.737***	-0.374	-0.882**	-1.160***
	(0.287)	(0.402)	(0.410)	(0.391)	(0.418)
Avg. of dep. var.	14.61	15.35	13.90	14.22	14.98
R-squared	0.08	0.09	0.08	0.07	0.10
Obs.	3'073'557	1'503'425	1'570'132	1'494'298	1'579'259
Clusters	57'429	28'927	29'473	28'631	29'956
	B. Monthly earnings (CHF)				
$D_i A_c$	-21.121	115.620	-138.611**	-131.590	104.986*
	(52.225)	(81.413)	(65.243)	(84.173)	(60.302)
Avg. of dep. var.	3'777.91	4'041.25	3'529.98	4'071.17	3'484.18
R-squared	0.28	0.30	0.24	0.29	0.26
Obs.	2'399'823	1'163'748	1'236'075	1'200'875	1'198'948
Clusters	44'763	22'395	23'123	23'008	22'672
	C. Job loss within 12 months				
$D_i A_c$	0.003	0.010	-0.007	-0.004	0.010
	(0.009)	(0.013)	(0.013)	(0.013)	(0.013)
Avg. of dep. var.	0.41	0.38	0.44	0.37	0.45
R-squared	0.05	0.04	0.06	0.04	0.06
Obs.	2'555'279	1'238'717	1'316'562	1'272'023	1'283'256
Clusters	47'507	23'777	24'562	24'326	24'200
	D. Job loss within 24 months				
$D_i A_c$	-0.004	-0.001	-0.009	0.000	-0.006
	(0.009)	(0.013)	(0.013)	(0.013)	(0.013)
Avg. of dep. var.	0.53	0.51	0.56	0.50	0.57
R-squared	0.05	0.04	0.06	0.04	0.06
Obs.	2'555'279	1'238'717	1'316'562	1'272'023	1'283'256
Clusters	47'507	23'777	24'562	24'326	24'200

Notes: Table 11 shows difference in differences estimates for unemployment duration and a number of job-match quality measures together with their means. Panel A shows the estimates for number of months spent in unemployment. Panel B illustrates the estimates for earnings in the second month of re-employment. Panels C and D focus on job loss within 12 and 24 months. Standard errors clustered by individual. *** P<0.01 ** P<0.05 * P<0.1.

Table 11 also looks at the job loss probabilities 12 (Panel C) and 24 months (Panel D) after re-employment respectively. Estimations include only observations which we observe for at least 24 months after re-employment.[29] The probability of losing the job after re-employment varies between roughly 37 % for a job loss within 12 months

[29]A total of 52'795 of all job seekers leave unemployment for a job. Out of those, 47'507 job seekers start their new jobs at least two years before the end of the observation period. We exclude about 10 % of re-employed job seekers whose employment durations are observed for less than two years.

and 57 % for a job loss within 24 months after reemployment. Overall, reducing the PBD does not affect the duration of employment spells.

All in all, results in table 11 confirm the general pattern of 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.

5.6 Effects on Income

We have documented that a reduction in PBD reduces benefit receipt but increases employment and earnings of job seekers. We now assess the effects on income. Income is the sum of labor earnings and unemployment benefits, i.e. income from social assistance or other transfer programs is not counted. Effects on total income provide information a how disposable income – a key component of individual welfare – is affected by reductions of PBD.[30]

Results in table 12 indicate that reductions in PBD do not lower total income, not even in the period when benefits are cut (18 to 24 months after the start of the unemployment spell). This is surprising considering that benefit receipt goes down considerably in the reform period. Yet loss of benefits is more than compensated by increased earnings. The effect of PBD once the framework period has ended is even positive with income increasing by 2.9 percent. The average effect of reducing PBD on income remains positive and amounts to 1.7 percent of income. Thus, reducing PBD tends to increase income *on average*.

[30]Note, however, that assessing individual welfare would imply accounting for a number of additional aspects (leisure, discounting, general equilibrium effects). Assessing these aspects is beyond the scope of the current analysis.

Table 12: Difference in differences estimates for total income

	All	R&D intensity		Task content	
		High	*Low*	*Cognitive*	*Manual*
	(1)	(2)	(3)	(4)	(5)
1-12 mths after	0.013*	0.025***	-0.003	0.014	0.011
	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)
13-17 mths after	0.012	0.037***	-0.019	0.012	0.010
	(0.008)	(0.011)	(0.013)	(0.011)	(0.012)
18-24 mths after	-0.007	0.018*	-0.039***	-0.006	-0.010
	(0.008)	(0.011)	(0.013)	(0.012)	(0.011)
25-50 mths after	0.029***	0.047***	0.010	0.015	0.048***
	(0.009)	(0.011)	(0.013)	(0.012)	(0.012)
Total	0.017**	0.036***	-0.004	0.010	0.026***
	(0.007)	(0.009)	(0.010)	(0.009)	(0.009)
Avg. of dep. var.	5'453.29	6'238.86	4'703.92	6'338.44	4'615.68
R-squared	0.24	0.25	0.23	0.24	0.24
Obs.	3'073'557	1'503'425	1'570'132	1'494'298	1'579'209
Clusters	57'429	28'927	29'473	28'631	29'955

Notes: Table 12 illustrates the difference in differences estimates for the effect of reduced unemployment benefit duration on total income. 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. Income is the sum of labor earnings and unemployment benefits relative to its mean three months before the unemployment spell starts. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

How does this look in sub-samples? Results by R&D intensity of the previous industry indicate that reductions in PBD lower income during the period when benefits are withheld only for job seekers in low R&D industries (income drops by 3.9 percent). The effect is positive and even significant for job seekers in high R&D industries. Income of job seekers leaving industries with high R&D expenditure increases considerably, by 4.7 percent, in the medium-run period. No corresponding effect can be detected for job seekers leaving low R&D industries. As a result, job seekers from high R&D industries on net enjoy a 3.6 percent higher income in the system with reduced PBD on average. There is no effect of reduced PBD on average income for job seekers from low R&D industries.

Results by task content of the previous occupation also indicate important differences. Interestingly, none of the two occupation groups suffers a significant reduction in income in the reform period when benefits are removed. Job seekers with manual occupations benefit from a significant increase in income in the medium run (income increases by 4.8 percent); there is no corresponding effect for job seekers with cognitive tasks. On net, job seekers with manual occupations have a 2.6 percentage points higher net income in a system with lower PBD. Job seekers whose occupations entail mainly cognitive tasks do not fare worse in a system with reduced PBD.

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 help them find jobs before their human capital depreciates or before they acquire the stigma of long-term unemployment.

We find strong evidence for the job push effect. Interestingly, we also find 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 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 who left R&D intensive industries and basically absent for job seekers in low R&D intensive industries. We find similar discrepancies for job seekers whose occupation necessitate manual skills and no medium-run benefits for job seekers with occupations rich in cognitive skills. Moreover, when we assess the effects on total income, we find that reduced PBD raises total income of job seekers who enjoy medium-run benefits and has no effect on income of job seekers where such medium-run effects are absent.

The evidence we find is consistent with unemployment insurance having an important role in human capital depreciation, especially for sub-groups that face rapid skill depreciation. Reductions in PBD can improve earnings and employment of job seekers in these sub-groups whereas extensions of PBD could probably also lead to reductions in labor market outcomes. Should benefit duration be reduced across the board? We believe this conclusion is premature for a number of reasons. First, we have seen that the effects of reducing PBD differ by task content and previous industry. Second, reducing PBD carries a cost in terms of reduced protection against economic shocks. This cost should be weighed against the potential benefits we have isolated. Third, reducing PBD to zero will, arguably, have more detrimental effects than removing 6 months out of 24 months. Fourth, human capital depreciation and long-term unemployment stigma might be more important for old job seekers than for younger ones. These issues should be explored in further research.

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A Appendix

A.1 Tables

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

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
$\tau_1 D_i A_c$ (1-12 mths after)	-0.001	-0.002	0.006	0.007	0.009	0.008
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
$\tau_2 D_i A_c$ (13-17 mths after)	-0.016**	-0.017**	0.017**	0.017**	0.023**	0.022**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.009)
$\tau_3 D_i A_c$ (18-24 mths after)	-0.064***	-0.065***	0.032***	0.033***	0.037***	0.037***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.009)
$\tau_4 D_i A_c$ (25-50 mths after)	-0.005	-0.006	0.014*	0.015**	0.034***	0.033***
	(0.004)	(0.004)	(0.007)	(0.007)	(0.010)	(0.009)
τ_2	-0.175***	-0.175***	0.149***	0.149***	0.131***	0.131***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
τ_3	-0.244***	-0.244***	0.183***	0.183***	0.160***	0.160***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
τ_4	-0.466***	-0.466***	0.170***	0.170***	0.136***	0.136***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
$\tau_1 D_i$	-0.048***	-0.049***	0.057***	0.061***	0.070***	0.070***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$\tau_2 D_i$	-0.075***	-0.075***	0.078***	0.083***	0.108***	0.108***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)
$\tau_3 D_i$	-0.084***	-0.085***	0.081***	0.085***	0.115***	0.115***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)
$\tau_4 D_i$	-0.007**	-0.008**	0.095***	0.099***	0.131***	0.131***
	(0.003)	(0.003)	(0.006)	(0.006)	(0.008)	(0.007)
$\tau_1 A_c$	0.055***	0.053***	-0.054***	-0.060***	-0.046***	-0.046***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$\tau_2 A_c$	0.074***	0.072***	-0.038***	-0.044***	-0.042***	-0.041***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
$\tau_3 A_c$	0.054***	0.052***	-0.015**	-0.022***	-0.022***	-0.021***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)
$\tau_4 A_c$	-0.024***	-0.026***	0.029***	0.023***	0.020***	0.021***
	(0.003)	(0.003)	(0.006)	(0.006)	(0.007)	(0.006)
Sum of pre-reg. benefits		0.000***		0.000***		0.000***
		(0.000)		(0.000)		(0.000)
Sum of pre-reg. earnings		-0.000***		0.000***		0.000***
		(0.000)		(0.000)		(0.000)
Mths employed before reg.		-0.000		0.007***		0.002***
		(0.000)		(0.000)		(0.000)
$\geq$ 24 mths of work exp.		0.016***		-0.094***		-0.079***
		(0.002)		(0.004)		(0.005)
R&D intensive industry		0.018***		-0.030***		-0.011***
		(0.002)		(0.003)		(0.004)
Cognitive task		0.022***		-0.001		0.008**
		(0.002)		(0.003)		(0.004)
Female		0.001		0.009***		-0.066***
		(0.002)		(0.003)		(0.006)
Swiss		-0.022***		0.095***		0.066***
		(0.002)		(0.004)		(0.005)
Leader position		0.002		0.046***		0.081***
		(0.003)		(0.004)		(0.005)
Marital status (reference group are singles)						
Married		-0.025***		0.027***		0.033***
		(0.003)		(0.005)		(0.005)
Widowed		-0.020***		0.006		-0.017
		(0.007)		(0.011)		(0.012)
Divorced		-0.015***		0.027***		0.031***
		(0.003)		(0.005)		(0.006)

Table A1 – continued

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Education (reference group is "8-9 years of schooling")						
$\leq$ 7 years		0.010		-0.067***		-0.034**
		(0.009)		(0.014)		(0.013)
10-11 years		-0.002		0.013		0.015*
		(0.005)		(0.008)		(0.008)
12-13 years		-0.018***		0.058***		0.067***
		(0.003)		(0.005)		(0.006)
$\geq$ 14 years		-0.017***		0.051***		0.165***
		(0.006)		(0.009)		(0.013)
Other		-0.044***		0.050***		0.086***
		(0.003)		(0.005)		(0.006)
Avg. of dep. var.	0.81	0.81	0.91	0.91	5'387.37	5'387.37
R-squared	0.22	0.22	0.05	0.09	0.03	0.18
Obs.	3'073'557	3'073'557	3'073'557	3'073'557	3'073'557	3'073'557
Clusters	57'429	57'429	57'429	57'429	57'429	57'429

Notes: Table A1 shows the baseline difference in differences estimates for unemployment benefit receipt (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. Earnings are normalized by the average earnings 3 months prior to 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 gender

	Baseline			Female			Male		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	-0.002	0.007	0.008	-0.007	0.008	0.001	0.002	0.006	0.013
	(0.006)	(0.006)	(0.007)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)	(0.008)
13-17 mths after	-0.017**	0.017**	0.022**	-0.029**	0.023**	0.023	-0.007	0.013	0.023**
	(0.008)	(0.008)	(0.009)	(0.012)	(0.012)	(0.014)	(0.010)	(0.010)	(0.011)
18-24 mths after	-0.065***	0.033***	0.037***	-0.082***	0.035***	0.035**	-0.051***	0.030***	0.038***
	(0.007)	(0.008)	(0.009)	(0.011)	(0.011)	(0.014)	(0.009)	(0.010)	(0.012)
25-50 mths after	-0.006	0.015**	0.033***	-0.010*	0.010	0.035**	-0.002	0.019**	0.034***
	(0.004)	(0.007)	(0.009)	(0.006)	(0.011)	(0.014)	(0.005)	(0.009)	(0.011)
Avg. of dep. var.	0.81	0.91	5'387.37	0.81	0.91	3'881.60	0.80	0.91	6'593.96
R-squared	0.22	0.09	0.18	0.24	0.10	0.18	0.20	0.09	0.16
Obs.	3'073'557	3'073'557	3'073'557	1'366'336	1'366'336	1'366'336	1'707'221	1'707'221	1'707'221
Clusters	57'429	57'429	57'429	25'796	25'796	25'796	31'633	31'633	31'633

Notes: Table (A2) shows the difference in differences estimates for subsamples splitted by gender for unemployment benefit receipt, 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. Earnings are normalized by the average earnings 3 months prior to 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 earnings

	Baseline			Low (≤ 4395 CHF per month)			High (> 4395 CHF per month)		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-12 mths after	-0.002	0.007	0.008	0.000	0.003	-0.007	-0.004	0.010	0.010
	(0.006)	(0.006)	(0.007)	(0.009)	(0.009)	(0.012)	(0.008)	(0.008)	(0.008)
13-17 mths after	-0.017**	0.017**	0.022**	-0.010	0.029**	0.017	-0.021**	0.007	0.019*
	(0.008)	(0.008)	(0.009)	(0.011)	(0.011)	(0.016)	(0.010)	(0.010)	(0.011)
18-24 mths after	-0.065***	0.033***	0.037***	-0.054***	0.035***	0.030*	-0.073***	0.029***	0.034***
	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)	(0.017)	(0.010)	(0.010)	(0.011)
25-50 mths after	-0.006	0.015**	0.033***	-0.004	0.019*	0.024	-0.007	0.010	0.032***
	(0.004)	(0.007)	(0.009)	(0.006)	(0.011)	(0.016)	(0.005)	(0.009)	(0.010)
Avg. of dep. var.	0.81	0.91	5'387.37	0.78	0.88	2'750.17	0.83	0.93	7'520.64
R-squared	0.22	0.09	0.18	0.21	0.08	0.10	0.24	0.10	0.14
Obs.	3'073'557	3'073'557	3'073'557	1'373'671	1'373'671	1'373'671	1'699'886	1'699'886	1'699'886
Clusters	57'429	57'429	57'429	25'723	25'723	25'723	32'398	32'398	32'398

Notes: Table (A3) shows the difference in differences estimates for subsamples splitted by earnings for unemployment benefit receipt, employment, and earnings respectively. Columns 1 to 3 replicate the baseline estimates, columns 4 to 6 include low earnings (≤ 4395 CHF per month), and columns 7 to 9 include high earnings (> 4395 CHF per month). Earnings are normalized by the average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.