Can Unemployment Benefit Cuts Improve Employment and Earnings?

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Abstract

We study how a reduction of potential benefit duration (PBD) affects employment and earnings of job seekers before and after unemployment benefits exhaust. Reducing PBD induces job seekers to become less selective and accept jobs earlier, which can worsen or improve labor market outcomes. We study a 2003 reform that reduces PBD from 24 months to 18 months for job seekers younger than 55 years in Switzerland. Using older job seekers as a control group, we find that reducing PBD increases employment and earnings even after unemployment benefits have run out. Employment and earnings increase particularly strongly for job seekers who previously worked in industries with high R&D expenditures, industries where job seekers' skills can depreciate rapidly.

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1 Introduction

A key parameter of unemployment insurance is the potential duration of unemployment benefits (PBD). We assess how PBD affect earnings and employment of job seekers, an effect that could go both ways. Consider a job search setting where unemployment benefits exhaust but everything else remains the same as the spell lengthens. Job seekers choose a high reservation wage initially but it decreases rapidly as they approach benefit exhaustion (van den Berg, 1990). Shortening PBD forces job seekers to lower their reservation wage throughout the entire spell (Mortensen, 1977; van den Berg, 1990). If job search duration was unaffected, lower reservation wages would imply worse job matches. But job seekers leave unemployment earlier, during a time when their reservation wage is still high. Shortening PBD can therefore decrease or increase job match quality.[1]

Suppose that employment opportunities worsen over time, e.g. in the form of a reduced flow of job offers. In this setting, reservation wages decline more rapidly as spells lengthen (van den Berg, 1990). Shortening PBD, again, reduces reservation wages and job seekers accept job offers earlier in the spell. Accepting jobs earlier improves the quality of job matches more dramatically than in the setting without depreciation since the reservation wage path is steeper. Reductions in PBD can improve labor market chances by shortening unemployment duration, especially for workers facing rapid depreciation of employment opportunities.

This pattern arises even if job seekers are forward looking and aware of the negative impact of accepting jobs later in their spell. In a job search setting, a job seeker's reservation wage is tied to the flow value of unemployment. In a non-stationary setting, where benefits and possibly also employment opportunities decrease, the flow value of unemployment declines as the spell lengthens.

Chances of finding employment fall as job search spells lengthen. Kroft et al. (2013) send a large number of job applications, varying randomly the time since layoff. The likelihood that a job applicant is called for a job interview, the call-back rate, decreases strongly during the first eight months of an unemployment spells. This suggests that firms use elapsed unemployment as a screening device (Gibbons and Katz, 1992). Alternatively, the skills of job seekers may depreciate as spells lengthen. Workplace skills may atrophy, particularly in fast-changing industries, e.g. experience in programming in a certain computer language. Job search skills or motivation may also deteriorate during the course of a spell.[2]

[1] Nekoei and Weber (2017) formalize this intuition, highlighting the two opposing forces on job quality.
[2] Shimer and Werning (2006) discuss implications of skill depreciation for labor market equilibrium.

Understanding whether PBD matters for medium-run earnings and employment is important. An assessment of the role of 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 needs to be weighed against a potentially large cost if reductions to PBD deteriorate post unemployment job quality. Conversely, reducing PBD could be an even more attractive policy choice if reduced PBD improves labor market outcomes.

We analyze a reform to Swiss unemployment insurance that reduced PBD from 24 months to about 18 months for job seekers younger than 55 years. This reform, enacted in July 2003, can be used to measure the effects of shortening PBD for older workers in a difference-in-differences setting. We find that the reform significantly reduced monthly unemployment benefit receipt by 11.6 percentage points (pp) in the period 18 to 24 months after entering unemployment. Triggered by this loss in unemployment benefits, job seekers leave unemployment for jobs faster increasing employment by 5.9 pp and labor earnings by 4.3 percent. Interestingly, we find that the positive effects of the benefit reduction *persists* beyond the period that is insured by UI. Specifically, job seekers on reduced benefits are 4.2 pp more likely to remain employed and earnings stay 3.9 percent higher compared to the situation without the reduction in PBD.

We also probe the sources of the positive employment effects. We use R&D intensity of the prior industry as a proxy for the extent to which labor market opportunities decline over the course of an unemployment spell. We find that it is mostly job seekers who held a job in an R&D intensive industry prior to entering unemployment who experience improvements in their earnings after unemployment benefits have exhausted. We interpret the positive effects of a reduction in PBD as the result of two opposing forces. Reduced benefit duration forces job seekers to be less selective thereby compromising job quality. But reduced PBD also forces job seekers to accept jobs earlier. Accepting jobs early helps job seekers in industries with strong depreciation of labor market opportunities, presumably those in R&D intensive industries.

Our work is related to two strands in the literature. The first and main strand of the literature discusses whether changes to PBD affect post unemployment job quality.[3] A string of recent studies based on regression discontinuity designs find little or

[3]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.

no effects of PBD. Card et al. (2007a) and Lalive (2007) find little evidence on wages and/or job stability in an Austrian context. van Ours and Vodopivec (2008) 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. Le Barbanchon (2012) finds no effects on wages or employment. Two studies find positive effects of PBD on low wage earners or job seekers at risk of exhausting their benefits. Centeno and Novo (2009) detect a positive impact in the match quality for individuals at the bottom of the wage distribution. Caliendo et al. (2013) 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. Two studies on Germany find negative effects of PBD extensions. 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. Schmieder et al. (2013) study the effects of PBD changes on re-employment wages in Germany finding sharp negative effects of PBD extensions for older workers, as we do. Two studies on the Austrian context find positive effects of benefit extensions. Degen (2014) and Nekoei and Weber (2017) study the effects of PBD for job quality in Austria, exploiting a sharp increase in PBD from 30 to 39 weeks for workers aged 40 years or older. Both papers find a positive effect of prolonged PBD on wages on the order of 0.5 percentage points. Nekoei and Weber (2017) rationalize this finding in a directed job search framework and discuss the implications of this finding for policy.

The second and earlier 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.[4] Evidence on the effect of PBD in European studies also finds strong effects.[5] A common

[4]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. 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. (2007b) show that the spike at benefit exhaustion has been over-stated in analyses that focus on registered unemployment duration.

[5]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 labor 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

objection against these studies is policy endogeneity. Benefits are typically extended in anticipation of a worse labor market for the eligible workers. Card and Levine (2000) exploit variation in benefit duration that occurred independently of labor market condition and show that policy bias is substantial.[6] Another issue with these findings is general equilibrium effects. Lalive et al. (2015) document that extending benefits for some job seekers has beneficial impacts on other job seekers who are not eligible for the benefit extension.

These two strands of the literature point to two key results. First, changes in the PBD have strong effects on the time that job seekers spend looking for new jobs. Second, there is no consistent pattern in the effects of PBD on job quality. The existing literature has been looking for job quality effects by exploring mostly outcomes that are observed for re-employed job seekers: wages, contract type, commuting distance, or tenure. The challenge with this approach is dealing with sample selection. Extending PBD delays employment entry so at any given point in time the group of job seekers who re-entered differs potentially strongly across the group that had long PBD and short PBD, even if these groups are initially perfectly comparable. This makes detecting causal effects of extending PBD on job quality outcomes very challenging.

Our approach is to adopt a somewhat different empirical framework. We follow each job seeker and ask whether he or she is employed, and how much he or she is earning, setting earnings to zero for job seekers who are not employed. This framework offers the key advantage that employment and earnings can be observed for all job seekers so selection problems are not an issue.[7] We believe that this somewhat different approach can complement the existing literature. Employment and earnings are two key outcomes that matter in the policy making process because they are defined for all individuals. But our approach is reduced-form, providing limited information on the underlying causal mechanisms.

The remainder of this paper is structured as follows. Section 2 discusses the institutional background. Section 3 provides information on the data sources, discusses our empirical setup, and presents descriptive statistics. Section 4 presents descriptive evidence and assesses the validity of the identifying assumptions. Section 5 presents

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.

[6] Lalive and Zweimüller (2004a,b) show similar evidence for the Austrian labor market.

[7] Right censored unemployment spells do not pose a problem, as both employment and earnings are zero for job seekers with very long unemployment spells. Caliendo et al. (2013) account for selectivity by estimating a bivariate hazard model jointly with wages, and allowing for unobserved heterogeneity.

the main results, and section 6 provides a summary and implications of our findings.

2 Background

This section discusses unemployment insurance in Switzerland.[8] Job seekers are entitled to unemployment benefits if they meet two requirements. First, they must have paid unemployment insurance taxes for at least six months in the two years prior to registering at the public employment service (PES). The contribution period is extended to 12 months for those individuals who have been registered at least once in the three previous years. Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job – they must be "employable". During the unemployment spell, job seekers have to fulfill certain job search requirements and participate in active labor market programs in order to remain eligible for benefits.[9] 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 Sfr.[10] prior to unemployment and not caring for children. The replacement rate decreases gradually to 70 % for job seekers who earned between 3,536 Sfr and 4,030 Sfr and it stays at 70 % thereafter. Benefits insure monthly earnings up to a top cap.[11] Job seekers have to pay all earnings and social insurance taxes.[12] 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,

[8] This section borrows from a similar section in Arni et al. (2013).

[9] See Gerfin and Lechner (2002) and Lalive et al. (2008) for detailed background information on and an evaluation of the active labor market programs.

[10] 1 Euro is 1.16 Sfr, December 2017.

[11] The cap is currently at 10,500 Sfr per month and stood at 8,900 Sfr before the reform.

[12] Except the unemployment insurance tax rate (which stands at about 2 %) and contributions to the occupational pension plan.

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.[13] 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 Sfr and then gradually reduced over the earnings bracket 3,797 to 4,340 Sfr.

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. Also, the reform was first discussed in the parliament in February 2001 and approved by popular vote in November 2002, not long before the policy change was implemented. Changes in unemployment insurance requirements may affect job seeker behavior already in advance (Hullegie and van Ours, 2014). We explain below how we deal with anticipation effects by considering only job seeker who entered unemployment long before the reform or after the reform started.

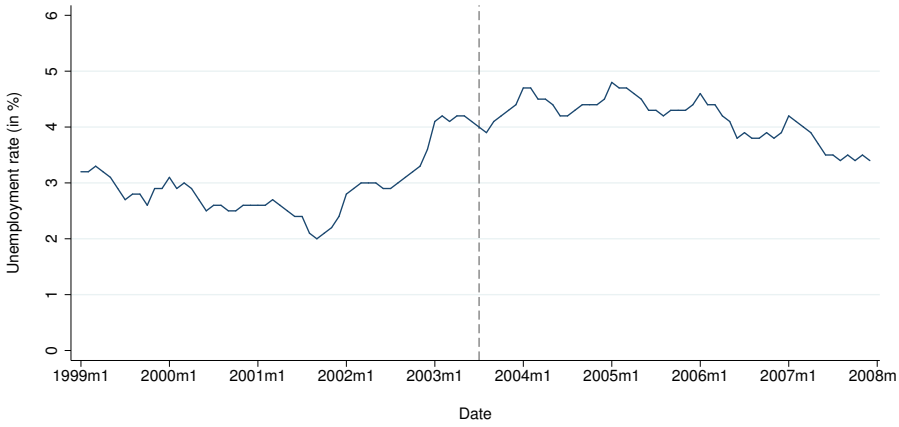
Second, the reform was signed into force around a time when the Swiss labor market situation was deteriorating. Figure 1 illustrates the evolution of the unemployment rate over the study period. The unemployment rate reached a low of roughly 2 % in the third quarter of 2001 and it increased considerably after the bursting of the "dot.com" bubble to a high of over 4.5 % in the first quarter of 2005. Unemployment decreased first slightly then more rapidly to reach around 3.5 % in the first 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 requirement by comparing labor market outcomes of the treated and control groups, as defined in the next section, over time.

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 Sfr and 4,300 Sfr, and it targeted job

[13] 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.

seekers without dependents, a minor fraction of our sample.[14]

Figure 1: Unemployment rate



Notes: This figure shows monthly averages of the unemployment rate over the time period under study (1999 to 2007)
Source: Swiss Federal Statistical Office.

3 Data and descriptive statistics

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

3.1 Data

We use data from 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.[15] 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 Administration (SSA). This data covers the universe of all individuals who have contributed to the mandatory first pillar retirement pension system between the period between 1982 and 2010. The social security database can be merged to the unemployment insurance register data

[14] See Eugster (2015) for an evaluation of this policy change.
[15] 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.

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).[16] Moreover, for a sub-sample 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 SSA.

Our baseline analysis is based on the following set of unemployment spells. *First*, we only consider full-time[17] job losers aged between 50 and 59 years at the start of the spell of unemployment who register with the public employment service. *Second*, the sample contains only individuals who contributed to the unemployment insurance for at least 18 of the last 24 months before becoming unemployed. This ensures that all job seekers aged 55 or older kept eligibility to two years of benefits. *Third*, the reform applied also to spells in progress in July 2003. We exclude spells that started between February 1 2001 (after the reform was first publicly discussed) and October 1 2003 (since job seekers register up to a quarter later than actually losing their job) to deal with likely anticipation effects of the reform. *Fourth,* we focus on the first unemployment spell of each job seeker. Job seekers may experience additional unemployment spells. Measuring benefit eligibility for the first spell is straightforward, but very challenging for later spells.

3.2 Treatment and Control Groups

Empirical strategy. This section presents the empirical strategy we employ for the 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).[18] In order to discuss estimation and identification assumption, let $Y(1)$

[16]There is no perfect agreement between the UIR and the SSA data, as the former covers job seekers registered at the public employment service for job search purposes, while the latter covers earnings and transfer income payments.

[17]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.

[18]One might think that the regression discontinuity (RD) design could also be implemented (Lee and Lemieux, 2010). Yet note that benefit eligibility does not 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 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.

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 in a current month, 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 differences (DiD) 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 DID estimator identifies the average treatment effect on the treated (ATT) by comparing differences in outcomes between the outcomes of the treated and the untreated before and after the reform. The DiD estimator can be rewritten as

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

The main assumption that has to hold for the DiD estimator to identify the ATT in repeated cross sections are parallel time trends for the treatment and control group in absence of the treatment, i.e. $E(Y_1(0) - Y_0(0) \mid D=1) = E(Y_1(0) - Y_0(0) \mid D=0)$.[19] 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.

Treatment assignment. The reform we described earlier reduced PBD by 6 months. We will analyze the effects of that reform in a standard DiD setting. Table 1 shows how we define treatment and control groups. Individuals aged below 55 in a current month are assigned to the treatment group. As soon as these individuals turn 55 years old, their status is switched from the treatment to the control group, because benefit eligibility is upgraded to 24 months after the 55^{th} birthday of a job seeker. 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.[20] Our analysis covers several years during

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

[20] 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

which the economic cycle has been changing. A key identification concern is that older job seekers could be more cyclically sensitive than younger workers. Below, we will present sensitivity analyses to probe this important issue.

Table 1: Treatment assignment

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

Notes: The table shows the treatment assignment, which is based on current age in a given month.

For each unemployment spell, we observe a monthly history of unemployment benefits, employment, and earnings that ranges from 50 months before to 50 month after entering unemployment.[21] 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 count 10,194 unemployment spells starting before the reform was discussed on February 1st 2001 – 5,736 in the treatment group, and 4,458 in the control group (table 2). We count 33,049 unemployment spells starting after October 1st 2003 – 19,888 spells belong to the treatment group and 13,161 belong to the control group. Spells after the reform are more numerous, because we excluded spells starting between February 2001 and October 2003 in order to deal with anticipation effects.

3.3 Descriptive statistics.

Recall the first identifying assumption that the composition of the treatment and control groups needs to be stable. Table 2 presents selected summary statistics to assess the validity of this assumption. The table shows the means of selected variables three months prior to unemployment start 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. Three months before entering unemployment, about 91 to 92 percent of all job seekers who start a spell before and after the reform were employed. Job seekers in the treatment group earn about 5,350 Sfr per month before the reform, and job

months after unemployment start, so that eligibility issues should not play a major role.

[21] Note that 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). We discuss whether right censoring affects our key results in a sensitivity analysis.

seekers in the control group earn around 5,080 Sfr per month prior to the reform. For spells that started after the reform, monthly earnings vary between 5,540 and 5,360 Sfr per month for the treatment and the control group respectively.

Table 2 also presents a information on our proxy for skill obsolescence and depreciation in a job seeker's industry: *R&D intensity* in the industry of the previous employer. As Switzerland lacks good data on R&D, 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. We classify a job seeker as being from a R&D intensive industry if this industry spent more than the median on R&D.[22] The share of job seekers from industries that spend a lot on R&D is between 53 and 55 % 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 46 % and 50 % respectively.

Table 2 also presents information on a number of characteristics which will be included in the estimations as control covariates. Cognitive refers to job seekers whose previous occupation consisted mainly of cognitive tasks.[23] Before the reform, the proportion is 50 % for the treated group, and 51 % for the control group. After the reform, the proportion of mainly cognitive skilled job seekers in treatment and control groups decreases to 48 and 47 % respectively. 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 both groups for spells that started before the reform. After the reform, this proportion slightly increases to 82 % for individuals in the control group, and to 87 % for those in 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 before the reform. Numbers stay similar for job seekers who became unemployed after the reform.

Table 2 also discusses demographics. The share of female job seekers varies between 43 % and 47 %. The proportion of Swiss citizens is very well balanced across groups for unemployment spells starting before the reform and amounts to 70 %. After the reform, the share of Swiss citizens in the control group increases to around 76 %, and to 72 % for the treatment group. There are no large differences between the

[22] 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.

[23] We adopt an approximation suggested in Acemoglu and Autor (2011) to classify occupations by task content.

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.

Table 2 reports statistics on education. The largest differences between unemployment starts before and after the reform are found for years of schooling. The share of individuals with less than 7 years of schooling, and between 10 and 11 years of schooling remains fairly stable over time and across treatment and control groups. The share of individuals with 8 to 9, 12 to 13, and 14+ years of schooling, however, doubles after the reform. At the same time the share of individuals for whom the attained education level is unknown decreases from around 61 to 63 % to 30 to 31 % over time. Changes in data quality account for this substantial shift in measured education levels. This shift affected treated and untreated individuals in a similar way.

Column 5 of table 2 discusses whether the composition of the treated and the control groups is affected by the reform. Statistically, we report DiD estimates of the reform on the control variables. The test rejects the null hypothesis that the composition of the treated group did not change for cognitive occupations, experience, Swiss nationality, marital status, and education.

Table 2: Selected descriptive statistics

	Before reform		After reform			
Treatment status	$D_i = 1$	$D_i = 0$	$D_i = 1$	$D_i = 0$	DiD	
A. Dependent variables						
UE benefits	93.32	75.25	51.32	34.86	-1.60	
Employment	0.92	0.91	0.91	0.91	-0.01	
Earnings	5,356.51	5,079.46	5,538.72	5,361.44	-99.78	
B. Control variables						
R&D intensity	0.53	0.55	0.46	0.50	-0.01	
Cognitive	0.50	0.51	0.48	0.47	0.02	*
Experience	0.75	0.77	0.82	0.87	-0.02	**
Leader position	0.72	0.74	0.73	0.75	0.00	
Female	0.44	0.43	0.47	0.46	-0.01	
Swiss	0.70	0.69	0.72	0.76	-0.05	***
Marital status						
Single	0.12	0.10	0.13	0.11	0.01	
Married	0.63	0.64	0.63	0.64	0.02	
Widow	0.03	0.05	0.02	0.04	0.00	
Divorced	0.23	0.21	0.22	0.22	-0.03	***
Years of schooling						
$\leq$ *7 years*	0.04	0.04	0.03	0.03	0.01	
8-9 years	0.06	0.07	0.15	0.15	0.01	*
10-11 years	0.03	0.03	0.05	0.05	0.00	
12-13 years	0.14	0.16	0.36	0.37	0.01	

Table 2 – continued

	Before reform		After reform			
Treatment status	$D_i = 1$	$D_i = 0$	$D_i = 1$	$D_i = 0$	DiD	
$\geq$ 14 years	0.04	0.04	0.10	0.08	0.02	***
Other	0.70	0.66	0.32	0.33	-0.05	***
No. of spells	5,736	4,458	19,888	13,161		

Notes: The table shows means of selected variable for the treatment and control group for individuals with only one unemployment spell, who registered before February 1 2001 or after October 1 2003, respectively. Column 5 shows differences in differences. R&D intensity is a dummy that equals 1 if the R&D intensity of the previous employers' industry is above median. Cognitive is a dummy that equals 1 if a job seekers' previous occupation is mainly cognitive. Experience 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.
Source: Own calculations based on merged UIR-SSA database.

To probe further how composition effects might affect our estimates, we report average pre-unemployment earnings for job seekers in treated and control groups. Pre-unemployment earnings measure the pre-unemployment labor market success of job seekers and are affected by a number of characteristics we show in Table 2, e.g. education, gender, and age, etc. Pre-unemployment earnings provide indications on whether the changes in composition of job seekers also translate into changes in labor market success. Figure 2 shows pre-unemployment earnings for job seekers in the treated and control groups, by date of start of their unemployment spell. Pre-unemployment earnings are very similar in both groups, both in terms of their level and evolution over time. This evidence suggests that observed compositional changes do not create strong imbalances in pre-unemployment earnings.[24]

4 Descriptive evidence

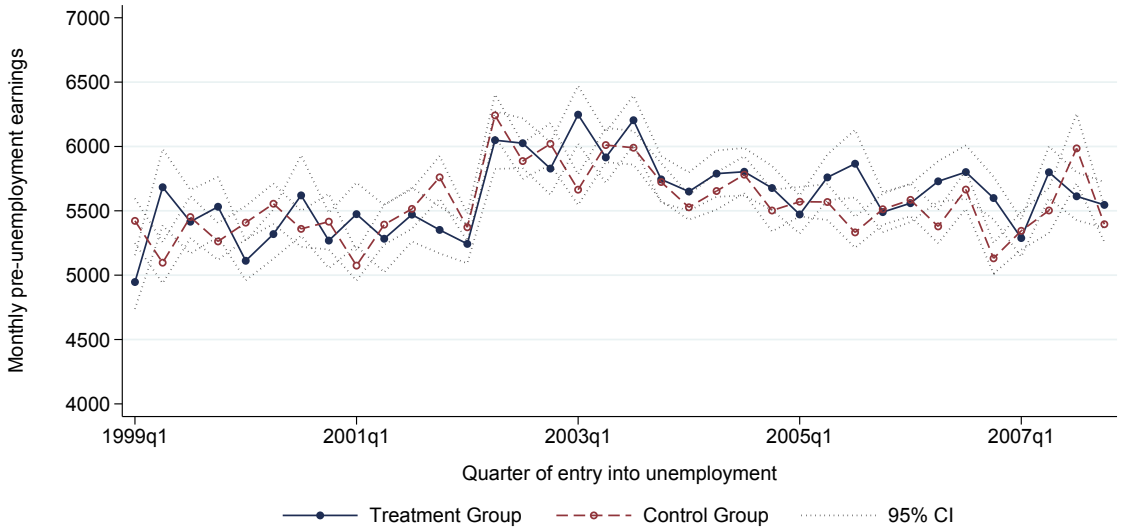
We present first descriptive evidence on the effects of reducing PBD on three outcomes. Unemployment benefit receipt, employment, and earnings. Next we discuss stability of time trends in outcomes, and reform effects on the inflow into unemployment.

4.1 Outcomes

Unemployment benefit receipt. We start by reporting effects of the reduction in PBD on unemployment benefit receipt. Figure 3 shows average benefit receipt, i.e. the proportion of treated (50 to 54 years old) and untreated (55 to 59 years old) receiving

[24] In a similar way, we have assessed evolution of composition with respect to nationality, gender, and education. We find similar evidence as for pre-unemployment earnings. Results are available upon request from the authors.

Figure 2: Monthly pre-unemployment earnings (in Sfr)

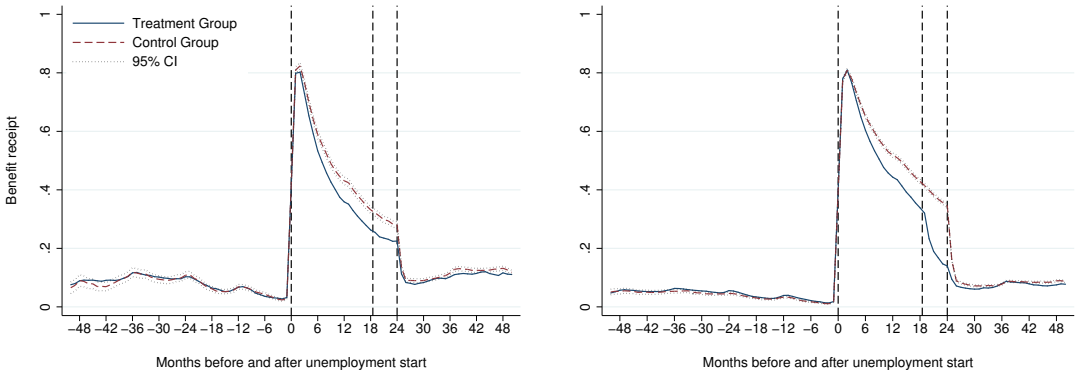


Notes: The figure shows monthly pre-unemployment earnings (in Sfr) for the treatment and control groups, by quarter of entry into unemployment. The sample only includes individuals with one unemployment spell. The dotted lines indicate the 95 % confidence interval.

unemployment benefits, up to 50 months around their unemployment start date. The vertical line at time 0 identifies the start of unemployment. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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 3a depicts benefit receipt for individuals who registered before the policy change was discussed in February 2001 and figure 3b shows the same for individuals who registered after the reform in October 2003.

Figure 3: Unemployment benefit receipt before and after the reform

(a) UE start before February 1 2001 (b) UE start after October 1 2003



Notes: The figure shows unemployment benefit receipt for treatment and control group 50 months before and 50 months after unemployment start for spells that started before February 1 2001 (subfigure 3a) and for spells that started after October 1 2003 (subfigure 3b). The sample only includes individuals with one unemployment spell. The dotted lines around the benefit receipt of the control group indicate the 95 % confidence interval. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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.

Benefit receipt is low, about 10 %, but not exactly zero before unemployment spells start.[25] Benefit receipt does not differ between the treated and the untreated before the start of the unemployment spell. After registering at the PES, unemployment benefit receipt increases substantially.[26] Average benefit receipt is around 80 % in the first month after unemployment start, the remaining 20 % not taking-up benefits for various reasons. Benefit receipt drops as time passes because job seekers gradually leave unemployment. After 24 months, there is a sharp drop in benefit receipt as job seekers exhaust their benefits. But benefit receipt does not drop to zero as job seekers, by working, can re-establish eligibility for unemployment benefits.[27]

Benefit receipts of treated and untreated start 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. For the treated group, there is another a kink after 18.5 months (equivalent to 400 days) after the beginning

[25]Benefit eligibility is low but not exactly zero in our sample for at least two reasons. First, unemployment insurance data do not cover the period before 1999, and some job seekers might have acquired eligibility for unemployment benefits before the observation period. Second, when constructing unemployment spells, we removed very short spells. The first spell in our sample may not be the very first spell known to the UI administration. We will explore in a sensitivity analysis how results are affected by excluding job seekers who had benefit payments before the unemployment spell.

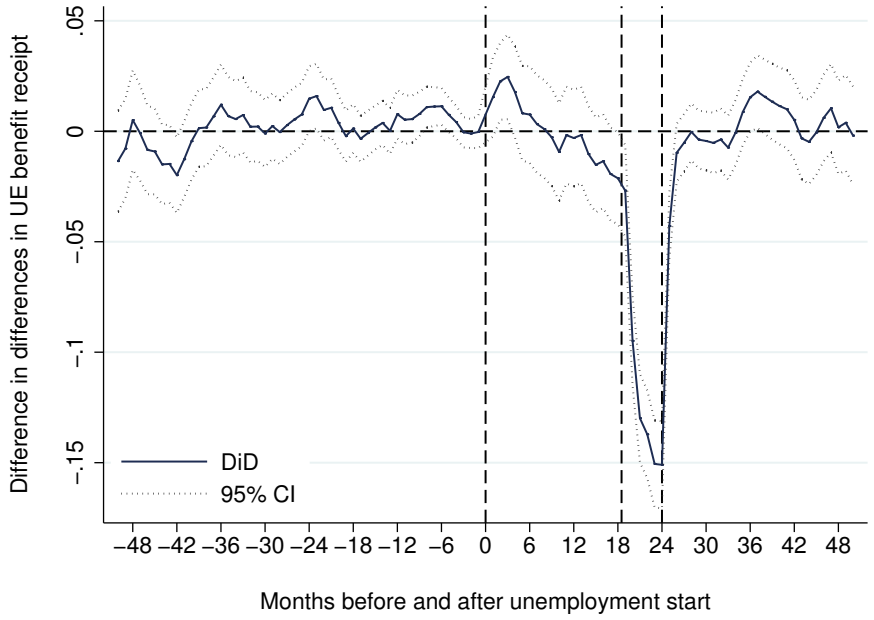
[26]Note that the unemployment start date is defined as the potential entry date for the next job. According to our sample definition, individuals thus fulfill the eligibility for daily benefit payments, conditional on being "employable". Indeed, 85 % of the sample claims unemployment benefits within 3 months after unemployment start. We have explored conditioning on actual benefit receipt but prefer not to do so as take-up could be endogenous to potential benefit duration.

[27]Benefit receipt is akin to survival plot in unemployment, except that benefit receipt does not need to be 100%, benefit receipt decreases due to unemployment exits but increases due to returns to unemployment.

of unemployment in subfigure 3b. This marks the benefit exhaustion date for the treated group after the reform. No such kink is observed at 18.5 moths before the reform (see subfigure 3a). 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 4 shows the difference in differences between the treated and the control group before and after unemployment start. In the period prior to entering unemployment, benefit receipt has evolved in the same way for treated and control groups, the DiD 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 an unemployment spell, the DiD starts to turn negative, reaching its minimum in the treatment period 18 to 24 months after the start of the spell where benefit receipt of treated job seekers is on average around 13 percentage points lower compared to the untreated individuals, lowering benefit receipt from about 35 percent to about 15 percent, 23 months after entering unemployment (see subfigure 3b). This is the mechanical 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 DiD turns not significantly different from zero.

Figure 4: Difference in differences in unemployment benefit receipt



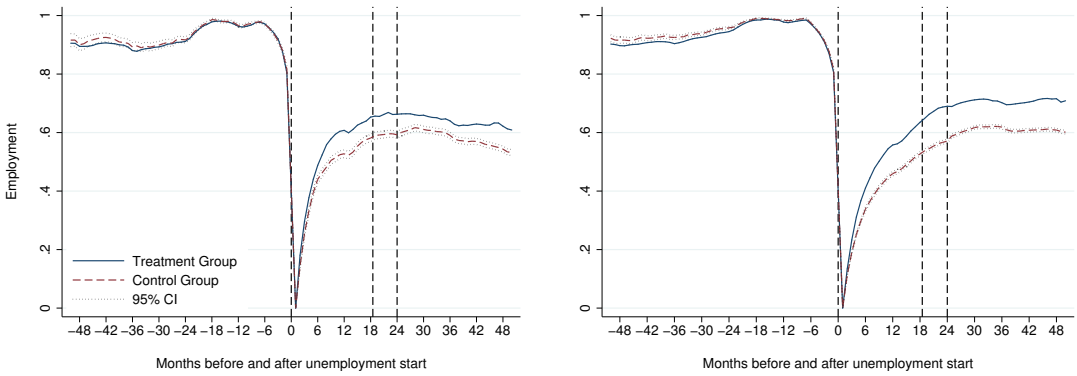
Notes: The figure shows the difference in differences for unemployment benefit receipt for the 50 months before and 50 months after unemployment start. The dotted lines around the difference in differences indicate the 95 % confidence interval. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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.

Employment. How does the reduction in PBD affect employment (Figure 5)? Before the unemployment spell, employment increases from 90 % to 98 %, reflecting our focus on job seekers who have worked 18 out of the 24 months prior to entering unemployment. 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, since we focus on job losers. 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 5a) and after (figure 5b) 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 5: Employment before and after the reform

(a) UE start before February 1, 2001 (b) UE start after October 1, 2003

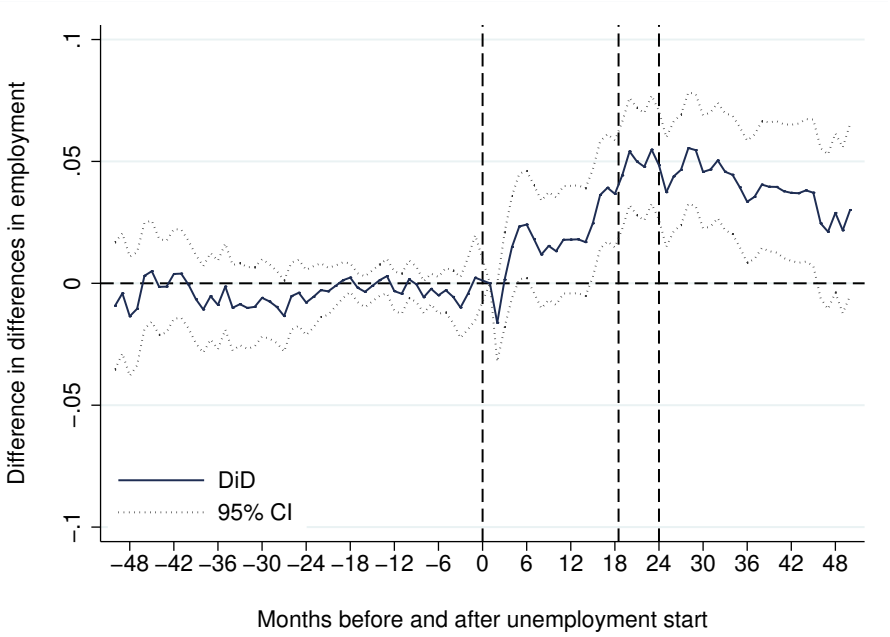


Notes: The figure shows aggregate employment for treatment and control group 50 months before and 50 months after unemployment start for spells that started before February 1, 2001 (subfigure 5a) and for spells that started after October 1, 2003 (subfigure 5b). The sample only includes individuals with one unemployment spell. The dotted lines around the employment share of the control group indicate the 95 % confidence interval. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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 6 provides the effect of reducing PBD on employment. In the period before the unemployment spell, we detect no treatment effect and the difference in differences is not statistically different from zero. The employment effect rises up to around 5 percentage points 20 months after entering unemployment and is statistically different from zero in the anticipation period (13 to 17 months after entry into unemployment) and in the direct treatment period 18 to 24 months after the start of

the spell. The positive employment effects gradually taper off thereafter.[28]

Figure 6: Difference in differences in employment



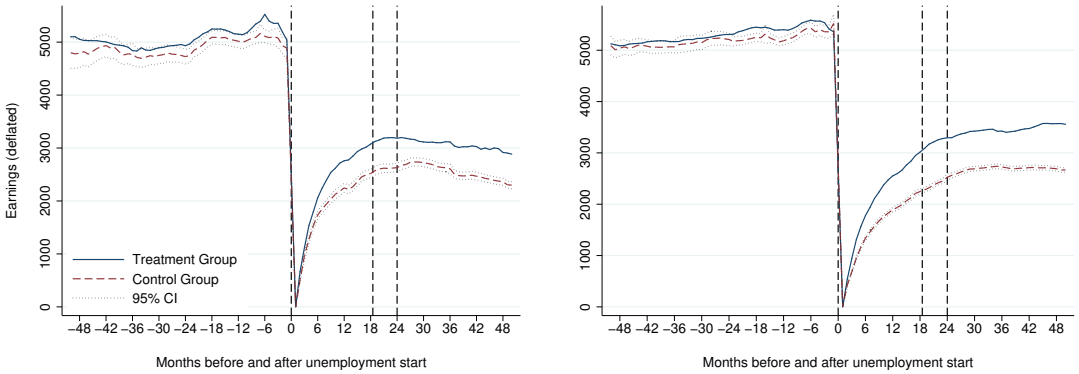
Notes: The figure shows the difference in differences for employment for the 50 months before and 50 months after entering unemployment. The dotted lines around the difference in differences indicate the 95 % confidence interval. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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.

Earnings. How does reducing PBD affect earnings? Figure 7 shows that pre-unemployment earnings are around 5,000 Sfr (about 4,500 EUR) before and slightly above 5,000 Sfr after the reform, and drop to zero at unemployment start. Note that we set earnings to zero for anyone who is not in a job to deal with selection into employment. Like the employment share, earnings rise again, but do no longer reach the pre-unemployment levels, and stay around 2,500 Sfr for the control group, and around 3,000 Sfr 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 7a) or *after* (figure 7b) the reform, earnings increase more for the treated than for the untreated in the *after* reform period.

[28]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) and the average quality of younger unemployed is likely to be lower, the effects on earnings and employment are likely to be negatively biased.

Figure 7: Earnings before and after the reform

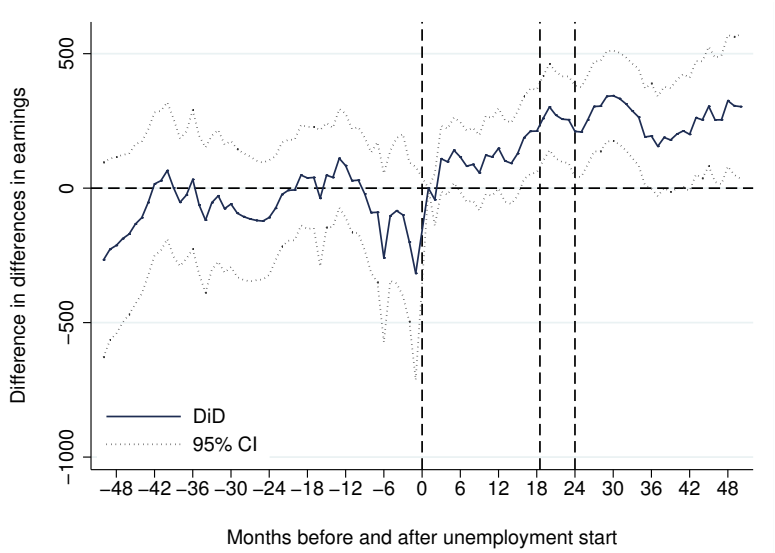
(a) UE start before February 1, 2001 (b) UE start after October 1, 2003



Notes: The figure shows aggregate earnings for treatment and control group 50 months before and 50 months after unemployment start for spells that started before February 1, 2001 (subfigure 7a) and for spells that started after October 1, 2003 (subfigure 7b). The sample only includes individuals with one unemployment spell. The dotted lines around earnings of the control group indicate the 95 % confidence interval. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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.

The difference in differences graph for earnings completes the picture (Figure 8). Prior to entering unemployment, earnings are relatively well balanced across the treatment and control groups. The earnings difference starts to rise significantly after the beginning of a spell to around 200 Sfr in the beginning of the treatment period (18 to 24 months after unemployment start), and it remains relatively stable and in most periods significantly different from zero also in the medium run period (25 to 50 months after unemployment start). In contrast to the result for employment, shortened PBD therefore increases earnings permanently.

Figure 8: Difference in differences in earnings



Notes: Figure 8 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. The vertical dashed line at 18.5 months indicates the benefit exhaustion for the treatment group after the reform, and the vertical dashed 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.

4.2 Time trends

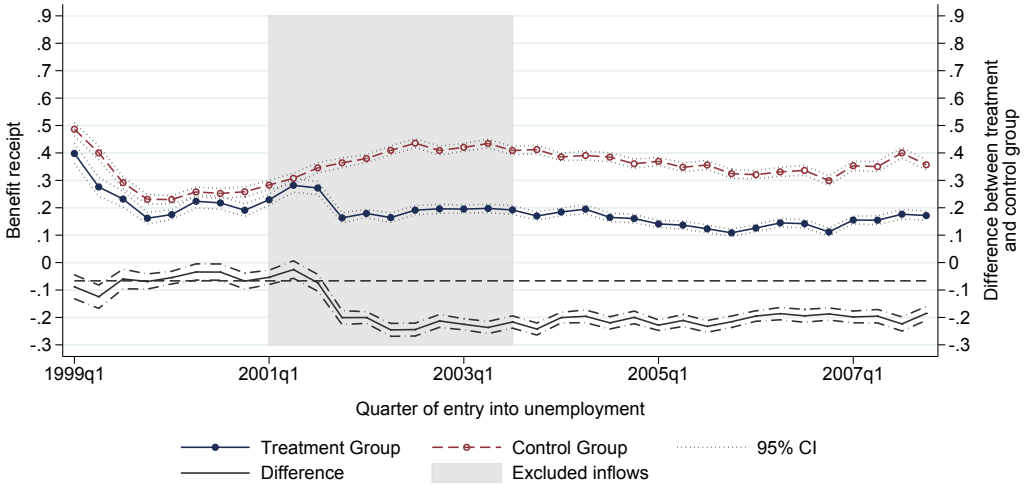
We now assess how benefit receipt, employment, and earnings change over time for job seekers in the treated and control groups. We focus on unemployment benefit receipt in 22 to 24 months after job seekers enter unemployment, i.e. benefit receipt in the last quarter of a job seeker's framework period of two years. The last quarter of a job seeker's framework period is mechanically affected by the reform from July 2003 onward. 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.

Figure 9 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 y-axis measures the share of job seekers who claim benefits. The right y-axis measures the difference between treatment and control groups. The shaded area depicts the excluded inflows from February 1 2001 to October 1 2003. The dashed horizontal line indicates the mean difference between the treated and control group before the reform.

Figure 9 also shows that benefit is about 5 to 10 percentage points higher in the

control group, compared to the treated group. Control job seekers are older than the treated job seekers, will remain unemployed longer and be more likely to draw benefits in the last quarter of their framework period. Before the reform, benefit receipt varies quite strongly and quite similarly in both groups.

Figure 9: Benefit receipt over time



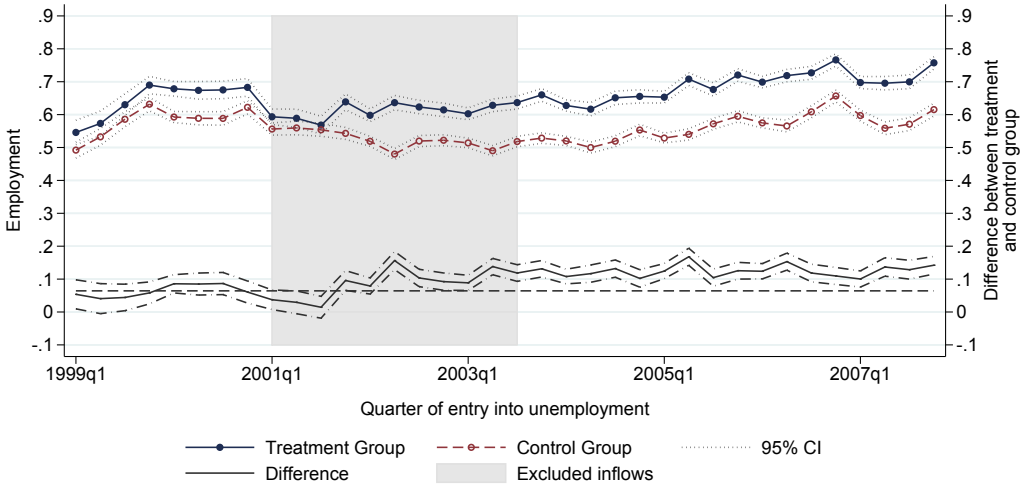
Notes: The figure shows the time trends for benefit receipt in the last quarter of the framework period, 22 to 24 months after entering unemployment, together with the 95 % confidence interval. On the right hand axis, the solid line at the bottom shows the difference between treatment and control group together with the 95 % confidence interval. The dashed horizontal line shows the mean difference between the treated and the control groups before the reform. The shaded area indicates that no data is available for that time period (inflows between February 1 2001 and October 1 2003 were omitted from the analysis).
Source: Own estimations based on merged UIR-SSA database.

Yet from the fourth quarter 2001 onwards, Figure 9 shows that benefit receipt in the treated group drops strongly, by about 15 percentage points, and permanently. This drop in benefit receipt at the very end of the framework period shows the mechanical effect of the reform. Every job seeker who enters unemployment in the last quarter of 2001 will loose all of her or his last benefits in the framework period.

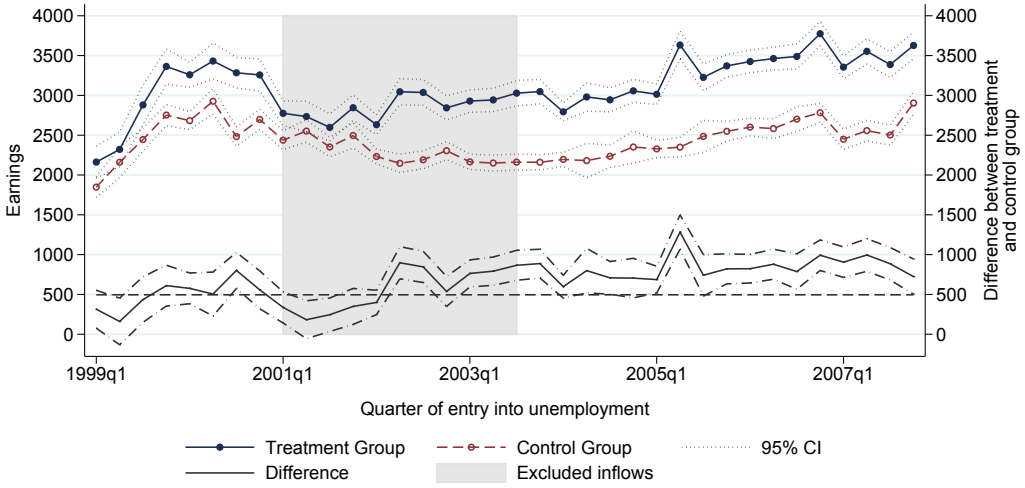
Figure 10 reports a similar analysis for employment and earnings in the last quarter of a job seekers' framework period. Effects of the reform should be visible starting from job seekers entering unemployment in the last quarter of 2001 who loose their benefits. Time trends are parallel for both outcomes for spells that start before the fourth quarter of 2001. Employment and earnings start to diverge between treated group and control for job seekers who enter unemployment at the end of 2001 or early 2002. Figure 10 visually suggest that the assumption of parallel trends is plausible for both employment and earnings before the reform. Whether the differences in employment and earnings for job seekers entering from 2002 are due solely due to the reform is unsettled. We return to this key issue below in a sensitivity analysis.

Figure 10: Employment and earnings over time

(a) Employment



(b) Earnings



Notes: The figure shows the time trends for employment and earnings in the last quarter of the framework period, 22 to 24 months after entering unemployment, together with the 95 % confidence interval. On the right hand axis, the solid line at the bottom shows the difference between treatment and control group together with the 95 % confidence interval. The dashed horizontal line shows the mean difference between the treated and the control groups before the reform. The shaded area indicates that no data is available for that time period (inflows between February 1 2001 and October 1 2003 were omitted from the analysis).
Source: Own estimations based on merged UIR-SSA database.

5 Results

This section discusses the estimation results. Subsection 5.1 presents the main estimation, subsection 5.2 discusses sensitivity checks, and subsection 5.3 presents results by previous job. Subsection 5.4 relates our results to the existing literature on job-match quality.

5.1 Main results

We adopt a DiD specification to estimate the causal effect of PBD on benefit receipt, employment and earnings. We first introduce the notation we use in the specification below. Y_{itc} is the outcome variable, that is unemployment benefit receipt, employment, or earnings. Subscript i is an indicator for the individual, t indicates the month after unemployment start, and c denotes calendar time. D_{itc} is the treatment dummy which is equal to 1 if an individual is below 55 years old in the current month, and 0 otherwise. A_c is a dummy that takes the value 1 for unemployment spells that start after October 1 2003, and A_c takes the value 0 otherwise. The indicators τ_{1t} to τ_{4t} identify different periods after unemployment start, i.e. $\tau_{1t} = \mathbb{1}(1 \leq t < 13 \text{ months})$, $\tau_{2t} = \mathbb{1}(13 \leq t < 18 \text{ months})$, $\tau_{3t} = \mathbb{1}(18 \leq t < 24 \text{ months})$, and $\tau_{4t} = \mathbb{1}(24 \leq t \leq 50 \text{ months})$ respectively. X_i is a vector of control variables, and $\tilde{X}_i$ is vector of the same control variables expressed as the deviation from the sample mean.[29]

We adopt the following DiD specification:

$$\begin{aligned} Y_{itc} =& \alpha + \sum_{p=2}^{4} \beta_{1p}\tau_{pt} + \sum_{p=1}^{4} \beta_{2p}(\tau_{pt} \cdot D_{itc}) + \sum_{p=1}^{4} \beta_{3p}(\tau_{pt} \cdot A_c) + \sum_{p=1}^{4} \delta_p(\tau_{pt} \cdot D_{itc} \cdot A_c) \\ &+ \sum_{p=1}^{4} \gamma_{1p}(\tau_{pt} \cdot \tilde{X}_i) + \sum_{p=1}^{4} \gamma_{2p}(\tau_{pt} \cdot D_{itc} \cdot \tilde{X}_i) + \sum_{p=1}^{4} \gamma_{3p}(\tau_{pt} \cdot A_c \cdot \tilde{X}_i) \\ &+ \sum_{p=1}^{4} \gamma_{4p}(\tau_{pt} \cdot D_{itc} \cdot A_c \cdot \tilde{X}_i) + \eta X_i + \varepsilon_{itc} \end{aligned} \tag{1}$$

The first line in this specification is a standard DiD model where the parameters δ_p, identified by the interaction between the treatment dummy and the time dummy, measure the treatment effect. This standard specification does not deal very well with heterogeneity as time trends, treatment control differences, and treatment effects are assumed to be homogenous. We allow for full heterogeneity in all dimensions by interacting the control vector with the treatment dummy, the time dummy, and the interaction of the two. By expressing the control variables in deviations of the sample mean, the treatment parameters δ_p give the effects for the job seeker with average

[29] These are 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 DiD in unemployment benefit receipt during months 7 to 5 prior to the spell we analyze (see Figure 4).

characteristics.

Table 3 presents the baseline estimates of the treatment effects (Table A1 reports all coefficient estimates). In all cases, we base inference on standard errors that are clustered at the individual level. In columns 1, 3, and 5, we estimate the treatment effects using equation (1) without controls. Columns 2, 4, and 6 show estimates based on our main specification (1) that allows for full flexibility of the effect of extended PBD on medium-run earnings and employment, and conditions on control variables. Because the composition of the inflow changes over time, we consider estimates that include control variables as our baseline estimates.

Table 3: Does reducing PBD affect benefit receipt, employment and earnings?

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
... 1-12 mths after	0.004	-0.014	0.013*	0.028***	0.017*	0.026**
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
... 13-17 mths after	-0.012	-0.029***	0.027***	0.039***	0.027**	0.029**
	(0.010)	(0.010)	(0.010)	(0.011)	(0.013)	(0.013)
... 18-24 mths after	-0.099***	-0.116***	0.047***	0.059***	0.046***	0.043***
	(0.009)	(0.010)	(0.010)	(0.011)	(0.014)	(0.014)
... 25-50 mths after	0.005	-0.013**	0.031***	0.042***	0.036**	0.039***
	(0.005)	(0.007)	(0.010)	(0.011)	(0.014)	(0.014)
Controls	No	Yes	No	Yes	No	Yes
Avg. of dep. var.	0.81	0.81	0.91	0.91	5413.25	5413.25
R-squared	0.25	0.25	0.06	0.10	0.03	0.19
Obs.	2,115,055	2,115,055	2,115,055	2,115,055	2,115,055	2,115,055
Clusters	43,241	43,241	43,241	43,241	43,241	43,241

Notes: This table 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 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.
Source: Own estimations based on merged UIR-SSA database.

The estimates for unemployment benefit receipt in Table 3 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 2.9 pp. This treatment effect is driven by changes in behavior in anticipation of benefit exhaustion. In the period between 18 and 24 months after unemployment start, benefit receipt is around 11.6 pp lower for the treated, the mechanical effect of reducing benefits. In the medium run, 25 to 50 months after entering unemployment, treated job seekers are 1.3 pp less likely to receive unemployment benefits than control job seekers.

The estimates for employment in Table 3 column 4 show an anticipation effect of 2.8 pp, 1-12 months after unemployment entry, and 3.9 pp, 13 to 17 months after

unemployment start. Treated job seekers re-enter employment faster than the untreated in anticipation of the end of benefits. The direct effect of the reform, increases employment by 5.9 pp, in the period 18-24 months after entry into unemployment. This effect is smaller than the effect of the benefit cut on benefit receipt, suggesting that treated job seekers either have alternative sources of income, or take up social assistance. Employment is also 4.2 pp higher for the treated in the medium-run, 25 to 50 months after entering unemployment, so the effects on job finding appear to be permanent.

Effects on earnings are relative to average earnings 3 months prior to unemployment start. Table 3 column 6 shows a statistically significant anticipation effect of around 2.6 pp, 1-12 months after entry, and 2.9 pp, 13-18 months after entering unemployment. The direct effect for earnings amounts to 4.3 pp, 18-24 months after entering unemployment, and slightly falls to 3.9 pp in the medium run, 25 to 50 months after entering unemployment. 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 and employment effects persist in the medium run.

These baseline findings suggest that reducing potential benefit duration induces job seekers to leave unemployment faster. Job seekers on shorter PBD accept jobs more quickly thereby increasing employment. Earnings increase in tandem with employment but remain higher until the end of our observation period, substantially beyond the period covered by unemployment insurance. This positive earnings effect is suggestive that faster job acceptance may improve labor market chances.

5.2 Sensitivity analyses

Labor market outcomes might react very differently to changes in the state of the labor market. We estimate models that include the unemployment rate in the canton to proxy for the state of the labor market.[30] These models allow for a correlation between outcomes and the unemployment rate, and the correlation is allowed to differ by treatment status, and time since a job seeker started unemployment.

Table 4, Panel "Interactions", shows that the local unemployment rate is an important predictor of outcomes, especially in the first year after entering unemployment. Benefit receipt increases, while employment and earnings decrease, when the local unemployment rate increases (see row "Unemployment rate"). Effects of unemploy-

[30] In an earlier version of this paper, we estimated a model where job seekers aged 55 years were the Placebo treated group, and job seekers aged 56 years were the Placebo control group. The reform did not have an effect on the Placebo treatment group.

Table 4: Does local unemployment drive the results?

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effects						
... 1-12 mths after	-0.014	-0.028**	0.028**	0.038***	0.026*	0.038***
	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
... 13-17 mths after	-0.029**	-0.036**	0.039***	0.044***	0.029*	0.038*
	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)	(0.015)
... 18-24 mths after	-0.116***	-0.115***	0.059***	0.052***	0.043**	0.035*
	(0.010)	(0.010)	(0.011)	(0.012)	(0.014)	(0.015)
... 25-50 mths after	-0.013*	-0.016*	0.042***	0.045***	0.039**	0.038**
	(0.007)	(0.007)	(0.011)	(0.011)	(0.014)	(0.014)
Interactions						
1-12 mths × treat × unempl.		0.011***		-0.008**		-0.010**
		(0.003)		(0.003)		(0.003)
13-17 mths × treat × unempl.		0.006		-0.005		-0.008
		(0.004)		(0.004)		(0.005)
18-24 mths × treat × unempl.		-0.000		0.006		0.006
		(0.003)		(0.004)		(0.005)
25-50 mths × treat × unempl.		-0.002		0.002		-0.004
		(0.002)		(0.003)		(0.004)
13-17 mths × unempl.		-0.004*		0.008***		0.015***
		(0.002)		(0.002)		(0.003)
18-24 mths × unempl.		-0.008***		0.010***		0.016***
		(0.002)		(0.002)		(0.003)
25-50 mths × unempl.		-0.019***		0.030***		0.039***
		(0.002)		(0.002)		(0.003)
Unemployment rate		0.034***		-0.046***		-0.042***
		(0.002)		(0.002)		(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Avg. of dep. var.	0.81	0.81	0.91	0.91	5413.21	5413.21
Treated interaction		0.001		0.000		0.000
R-squared	0.25	0.26	0.10	0.10	0.19	0.19
Obs.	2,115,005	2,115,005	2,115,005	2,115,005	2,115,005	2,115,005
Clusters	43,240	43,240	43,240	43,240	43,240	43,240

Notes: This table 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 relative to average earnings 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *Treated interaction* in the table footer reports a joint F-test of significance of treated unemployment interactions. *** P<0.01 ** P<0.05 * P<0.1.

Source: Own estimations based on merged UIR-SSA database.

ment become weaker from the second year onwards, as job seekers leave unemployment for jobs, but outcomes remain sensitive to unemployment (rows "13-17 mths x unempl" to "25-50 mths x unempl").

Table 4, Panel "Interactions", shows that job seekers in the treated group are more sensitive to the unemployment rate than job seekers in the control group, a joint test of significance of treated unemployment interactions rejects the null of equal sensitivity strongly.[31] Treated job seekers are more sensitive to the labor market during the first year after entering unemployment (row "1-12 mths x treat x unempl"), but from the second year onward, treated job seekers are as sensitive to local unemployment

[31]Row "Treated interaction" reports the p-value of joint test that interactions "1-12 mths x treat x unempl" to "25-50 mths x treat x unempl" are zero in Table 4.

as control group job seekers (rows "13-17 mths x treat x unempl" to "25-50 mths x treat x unempl"). As a result, treatment effects are very similar to baseline results from the second year onward.

Estimates of the effects of reducing PBD could be biased because of disability insurance offering a way to exit the labor force after unemployment to older job seekers.[32] A cut in PBD could affect disability pensions in mainly two ways. First, reducing PBD could amplify the adverse health effects of job-loss[33] and thereby increase disability pensions, and second, reducing PBD could induce a substitution of unemployment benefits with disability pensions. Table 5 shows the effects of reducing PBD on disability retirement pensions.[34] Point estimates are negative and not statistically significant. Entry into disability is unlikely to confound our estimates of the effects of reducing PBD for older workers.

Table 5: DiD estimates for disability retirement

	Disability pensions
Treatment effect in the period ...	
... 1-12 mths after	-0.207
	(0.181)
... 13-17 mths after	-0.263
	(0.213)
... 18-24 mths after	-0.150
	(0.226)
... 25-50 mths after	-0.125
	(0.248)
Avg. of dep. var.	99.10
R-squared	0.06
Obs.	769,612
Clusters	15,584

Notes: This table shows the difference in differences estimates for disability pensions normalized by the average disability pension 3 months prior to unemployment start. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.
Source: Own estimations based on merged UIR-SSA database.

Some job seekers in our sample receive unemployment benefits before their unemployment spell starts. We now explore whether excluding these job seekers affects results. Table 6 shows results when we exclude job seekers with prior benefit receipt,

[32]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.
[33]Kuhn et al. (2009) find important health effects of job loss, particularly for men.
[34]Disability pension data is only available for a random subsample of around 35 % of job seekers.

Table 6: Job seekers with no prior unemployment

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	No UB before	Baseline	No UB before	Baseline	No UB before
Treatment Effects						
... 1-12 mths after	-0.014	-0.005	0.028**	0.031**	0.026*	0.029*
	(0.008)	(0.010)	(0.009)	(0.012)	(0.010)	(0.014)
... 13-17 mths after	-0.029**	-0.017	0.039***	0.042**	0.029*	0.033
	(0.010)	(0.012)	(0.011)	(0.014)	(0.013)	(0.017)
... 18-24 mths after	-0.116***	-0.113***	0.059***	0.061***	0.043**	0.044*
	(0.010)	(0.012)	(0.011)	(0.014)	(0.014)	(0.017)
... 25-50 mths after	-0.013*	-0.010	0.042***	0.039**	0.039**	0.037*
	(0.007)	(0.008)	(0.011)	(0.014)	(0.014)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Avg. of dep. var.	0.81	0.80	0.91	0.92	5413.21	5651.10
R-squared	0.25	0.27	0.10	0.11	0.19	0.19
Obs.	2,115,005	1,781,212	2,115,005	1,781,212	2,115,005	1,781,212
Clusters	43,240	36,422	43,240	36,422	43,240	36,422

Notes: This table 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 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.

Source: Own estimations based on merged UIR-SSA database.

removing about 6,500 individuals. Columns 1, 3, and 5 reproduce the baseline estimates, while columns 2, 4, and 6 report results in the sub-sample of job seekers without prior benefit receipt. In this sub-sample, all job seekers start a new framework period when their unemployment spell starts. Treatment effects are very similar to baseline results both in terms of magnitude and statistical significance.

About 13 percent of all job seekers enter unemployment in December 2006 or after, so we do not observe a full 50 months history for these job seekers. We explore whether this affects our results by limiting the analysis to job seekers who are all observed for 50 months, those who enter unemployment before December 1 2006. Table 7 shows the result. Columns 1, 3, and 5 reproduce the baseline estimates, while columns 2, 4, and 6 report results in the sub-sample of job seekers with identical observation window. Results for benefit receipt and employment are very consistent across the two samples. The observation window does not appear to affect them. The short run effects of benefit cuts on earnings are less strong in the sample with identical observation windows, but the medium run effects 25 to 50 months after entering unemployment, are virtually identical.

5.3 Effects by previous job

This section analyzes whether the effects of a reduction in potential benefit duration differ between subgroups of job seekers with different previous industry affiliation.

Table 7: Job seekers who enter before December 2006

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Inflow < Dec 06	Baseline	Inflow < Dec 06	Baseline	Inflow < Dec 06
Treatment Effects						
... 1-12 mths after	-0.014	-0.004	0.028**	0.023*	0.026*	0.019
	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
... 13-17 mths after	-0.029**	-0.024*	0.039***	0.035**	0.029*	0.018
	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)
... 18-24 mths after	-0.116***	-0.114***	0.059***	0.054***	0.043**	0.033*
	(0.010)	(0.010)	(0.011)	(0.012)	(0.014)	(0.014)
... 25-50 mths after	-0.013*	-0.014*	0.042***	0.042***	0.039**	0.039**
	(0.007)	(0.007)	(0.011)	(0.011)	(0.014)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Avg. of dep. var.	0.81	0.81	0.91	0.91	5413.21	5381.79
R-squared	0.25	0.27	0.10	0.10	0.19	0.19
Obs.	2,115,005	1,804,700	2,115,005	1,804,700	2,115,005	1,804,700
Clusters	43,240	36,092	43,240	36,092	43,240	36,092

Notes: This table 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 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.

Source: Own estimations based on merged UIR-SSA database.

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 high R&D intensity, with higher than median R&D expenditure, versus job seekers from industries with low R&D intensity, with lower than median R&D expenditure. Skill depreciation could be more important 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. A shortened period of unemployment could 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 reproduces the baseline estimates for the sake of comparison. Columns 4 to 6 report estimates for job seekers from high R&D industries, and columns 7 to 9 for job seekers from industries with low R&D intensity. For both subsamples 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. Job seekers from high R&D industries experience strong and significant anticipation effects already in the period from 1 to 12 months and from 13 to 17 months after unemployment start. In the period 18 to 24 months after unemployment start, employment increases by 9.3 percentage points, and earnings are 4.7 percent higher. In the medium run, 25 to 50 months after unemployment start, reducing PBD by 6 months increases employment by 7.7 percentage points, and earnings by 6.7 percent; the effects of reducing PBD

are persistent. In contrast, treatment effects for job seekers leaving industries with low R&D intensity are mostly absent, except for a marginally significant employment effect in the period from 18 to 24 months after unemployment start, probably directly induced by the cut in benefits in that period.[35]

5.4 Match Quality

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 analyze the effects of the PBD reduction on these direct measures of job-match quality.

Panel A presents the difference-in-difference estimates for the pre-to-post earnings changes. This analysis is based on all spells where job seekers left unemployment and stayed in their job for at least two months. Pre-unemployment earnings are measured in the second month before unemployment start and post-unemployment earnings are measured in the second month after re-employment.[36] Results show no significant effect in the overall sample and both sub-samples. Panel B shows the estimates for the probability of job loss within 12 months after re-employment, a measure for the stability of jobs. This estimation includes individuals which we observe for at least 12 months after re-employment. Reducing PBD does not affect the duration of employment spells. Panel C shows estimates for earnings changes within 12 months after re-employment. Reducing PBD does not affect earnings growth.

[35]Job seekers in industries with high R&D expenditure have higher mean earnings than job seekers in industries with low R&D expenditure. The difference in the effects of PBD could partly be related to the difference in earnings. When we split the sample by previous earnings, we do not find the same pattern of results as we by previous job (see table A2). The job quality effects of reducing PBD appear to be linked to the skill atrophy rather than to earnings levels. We have explored results for men and women, not reported, and find similar effects for both sub-groups.

[36]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. Because we do not observe number of days worked on the job so we can not adjust for this. The same logic applies for the measurement of the pre-unemployment earnings.

Table 8: DiD estimates by R&D intensity of previous industry

	Baseline			High R&D intensity			Low R&D intensity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
... 1-12 mths after	-0.014	0.028***	0.026**	-0.023*	0.063***	0.052***	-0.001	-0.003	-0.013
	(0.008)	(0.009)	(0.010)	(0.012)	(0.013)	(0.014)	(0.012)	(0.014)	(0.018)
... 13-17 mths after	-0.029***	0.039***	0.029**	-0.030**	0.073***	0.056***	-0.027*	0.012	-0.004
	(0.010)	(0.011)	(0.013)	(0.015)	(0.016)	(0.018)	(0.015)	(0.016)	(0.022)
... 18-24 mths after	-0.116***	0.059***	0.043***	-0.123***	0.093***	0.074***	-0.110***	0.031*	0.005
	(0.010)	(0.011)	(0.014)	(0.014)	(0.016)	(0.018)	(0.014)	(0.016)	(0.022)
... 25-50 mths after	-0.013**	0.042***	0.039***	-0.012	0.077***	0.067***	-0.010	0.013	0.004
	(0.007)	(0.011)	(0.014)	(0.010)	(0.016)	(0.018)	(0.010)	(0.016)	(0.022)
Avg. of dep. var.	0.81	0.91	5413.25	0.82	0.92	6182.58	0.80	0.90	4666.74
R-squared	0.25	0.10	0.19	0.28	0.11	0.19	0.24	0.09	0.18
Obs.	2,115,055	2,115,055	2,115,055	1,043,675	1,043,675	1,043,675	1,071,380	1,071,380	1,071,380
Clusters	43,241	43,241	43,241	21,295	21,295	21,295	21,947	21,947	21,947

Notes: This table shows the DiD estimates for subsamples split 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.

Source: Own estimations based on merged UIR-SSA database.

Table 9: Effects on unemployment duration, earnings changes, and job loss

	All	R&D intensity	
		High	*Low*
Panel A: Pre-to-post earnings changes			
$D_i A_c$	-32.500	-237.810	81.244
	(139.721)	(222.062)	(174.385)
Avg. of dep. var.	-1893.16	-2460.76	-1345.99
R-squared	0.31	0.36	0.19
Obs.	1,626,572	798,389	828,183
Clusters	33,275	16,296	16,980
Panel B: Job loss within 12 months			
$D_i A_c$	-0.008	0.007	-0.015
	(0.013)	(0.020)	(0.018)
Avg. of dep. var.	0.38	0.36	0.41
R-squared	0.04	0.04	0.05
Obs.	1,752,055	858,835	893,220
Clusters	35,832	17,527	18,306
Panel C: Δ earnings changes within 12 months after re-employment			
$D_i A_c$	73.196	-28.850	89.457
	(79.743)	(114.844)	(108.501)
Avg. of dep. var.	-126.76	-48.11	-202.58
R-squared	0.01	0.01	0.02
Obs.	1,626,572	798,389	828,183
Clusters	33,275	16,296	16,980

Notes: This table 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 the pre-to-post earnings changes, panel B for the probability of job loss within the first 12 months after re-employment and panel C for the earnings changes within 12 months after re-employment. Standard errors clustered by individual in parentheses. *** P<0.01 ** P<0.05 * P<0.1.

Learning about job match quality from changes in PBD is challenging because changes in PBD affect not only when people find jobs, but also whether job seekers are employed or not in our setting. Selection into employment plays a role, and comparisons across treated and control groups are unlikely to yield estimates of the causal effects of PBD. Our approach of focusing on employment and earnings informs on the causal effects of reducing PBD, despite the presence of selection into employment.

6 Conclusions

We discuss the effects of shortening potential benefit duration (PBD) for job seekers aged 50 to 54 years. Shortening PBD induces job seekers to accept jobs during the

period when benefit payments are cut. But these jobs may be of lower quality than those found with longer PBD. Conversely, forcing job seekers to leave unemployment more quickly may help them find jobs before their human capital depreciates or help them avoid the stigma associated with long-term unemployment.

Our results suggest that job seekers who find employment more quickly because of a reduction in PBD earn more not only during the period when benefits are removed but up to 2 years later on. The medium-run benefits are strong for job seekers who left R&D intensive industries and absent for job seekers in low R&D intensive industries.

This evidence is consistent with unemployment insurance potentially having a role in avoiding human capital depreciation, especially for sub-groups that face rapid skill depreciation. Reductions in PBD can improve earnings and employment of these job seekers whereas extensions of PBD could probably also lead to reductions in labor market outcomes.

Compliance with Ethical Standards:

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A Appendix

A.1 Tables

Table A1: DiD 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)	0.004	-0.014	0.013*	0.028***	0.017*	0.026**
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
$\tau_2 D_i A_c$ (13-17 mths after)	-0.012	-0.029***	0.027***	0.039***	0.027**	0.029**
	(0.010)	(0.010)	(0.010)	(0.011)	(0.013)	(0.013)
$\tau_3 D_i A_c$ (18-24 mths after)	-0.099***	-0.116***	0.047***	0.059***	0.046***	0.043***
	(0.009)	(0.010)	(0.010)	(0.011)	(0.014)	(0.014)
$\tau_4 D_i A_c$ (25-50 mths after)	0.005	-0.013**	0.031***	0.042***	0.036**	0.039***
	(0.005)	(0.007)	(0.010)	(0.011)	(0.014)	(0.014)
τ_2	-0.213***	-0.212***	0.161***	0.160***	0.146***	0.144***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
τ_3	-0.293***	-0.293***	0.202***	0.201***	0.190***	0.186***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
τ_4	-0.480***	-0.479***	0.189***	0.187***	0.178***	0.174***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)
$\tau_1 D_i$	-0.035***	-0.016**	0.038***	0.025***	0.047***	0.032***
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)
$\tau_2 D_i$	-0.070***	-0.051***	0.070***	0.059***	0.101***	0.090***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.012)	(0.012)
$\tau_3 D_i$	-0.063***	-0.044***	0.068***	0.057***	0.103***	0.096***
	(0.008)	(0.009)	(0.009)	(0.010)	(0.012)	(0.012)
$\tau_4 D_i$	-0.016***	0.004	0.065***	0.054***	0.101***	0.091***
	(0.005)	(0.006)	(0.009)	(0.010)	(0.013)	(0.012)
$\tau_1 A_c$	0.051***	0.010*	-0.074***	-0.054***	-0.057***	-0.017**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
$\tau_2 A_c$	0.094***	0.052***	-0.059***	-0.038***	-0.054***	-0.012
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)
$\tau_3 A_c$	0.082***	0.040***	-0.039***	-0.017**	-0.041***	0.004
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
$\tau_4 A_c$	-0.031***	-0.073***	0.031***	0.054***	0.029***	0.072***
	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)
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.		-0.001		0.004***		0.000
		(0.001)		(0.001)		(0.001)
$\geq$ 24 mths of work exp.		0.001		-0.082***		-0.068***
		(0.007)		(0.011)		(0.013)
R&D intense industry		0.020***		-0.021**		-0.012
		(0.005)		(0.008)		(0.009)
Cognitive task		0.029***		0.012		0.057***
		(0.006)		(0.010)		(0.012)
Female		0.004		0.003		-0.074***
		(0.005)		(0.009)		(0.016)
Swiss		-0.032***		0.116***		0.084***
		(0.006)		(0.010)		(0.012)
Leader position		-0.005		0.061***		0.075***
		(0.006)		(0.011)		(0.011)
Marital status (reference group are singles)						
Married		-0.021***		0.035***		0.042***
		(0.008)		(0.013)		(0.015)
Widowed		-0.033**		0.042*		0.027
		(0.014)		(0.024)		(0.022)
Divorced		-0.010		0.059***		0.068***
		(0.009)		(0.015)		(0.017)
Education (reference group is "8-9 years of schooling")						

Table A1 – continued

	Benefit receipt		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
$\leq$ 7 years		-0.021		-0.023		0.041**
		(0.015)		(0.026)		(0.019)
10-11 years		-0.011		0.001		0.027
		(0.018)		(0.028)		(0.023)
12-13 years		-0.014		0.024		-0.002
		(0.012)		(0.019)		(0.017)
$\geq$ 14 years		-0.017		0.019		0.007
		(0.017)		(0.027)		(0.039)
Other		-0.141***		0.101***		0.149***
		(0.010)		(0.017)		(0.013)
Avg. of dep. var.	0.81	0.81	0.91	0.91	5413.25	5413.25
R-squared	0.25	0.25	0.06	0.10	0.03	0.19
Obs.	2,115,055	2,115,055	2,115,055	2,115,055	2,115,055	2,115,055
Clusters	43,241	43,241	43,241	43,241	43,241	43,241

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 parameters for the interactions of demeaned controls with the treatment dummies, time trend dummies, and the interaction of the two. The parameters are not shown in this table but available upon request. 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: DiD estimates by previous earnings

	Baseline			Low (≤ 4069 Sfr per month)			High (> 4069 Sfr per month)		
	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings	Benefit receipt	Employment	Earnings
Treatment effect in the period ...									
... 1-12 mths after	-0.014	0.028***	0.026**	-0.010	0.006	0.043	-0.022	0.040**	0.012
	(0.008)	(0.009)	(0.010)	(0.021)	(0.030)	(0.052)	(0.014)	(0.018)	(0.025)
... 13-17 mths after	-0.029***	0.039***	0.029**	-0.024	0.035	0.064	-0.038**	0.037*	0.010
	(0.010)	(0.011)	(0.013)	(0.023)	(0.032)	(0.056)	(0.017)	(0.020)	(0.026)
... 18-24 mths after	-0.116***	0.059***	0.043***	-0.105***	0.052*	0.093	-0.130***	0.056***	0.018
	(0.010)	(0.011)	(0.014)	(0.022)	(0.032)	(0.058)	(0.016)	(0.020)	(0.026)
... 25-50 mths after	-0.013**	0.042***	0.039***	-0.012	0.039	0.106*	-0.020*	0.037*	0.009
	(0.007)	(0.011)	(0.014)	(0.020)	(0.032)	(0.057)	(0.012)	(0.020)	(0.026)
Avg. of dep. var.	0.81	0.91	5413.25	0.79	0.89	2656.21	0.83	0.93	7544.19
R-squared	0.25	0.10	0.19	0.24	0.08	0.11	0.27	0.10	0.14
Obs.	2,115,055	2,115,055	2,115,055	921,479	921,479	921,479	1,193,576	1,193,576	1,193,576
Clusters	43,241	43,241	43,241	18,851	18,851	18,851	24,390	24,390	24,390

Notes: This table shows the difference in differences estimates for subsamples split 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 (≤ 4069 Sfr per month), and columns 7 to 9 include high earnings (> 4069 Sfr 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.
Source: Own estimations based on merged UIR-SSA database.