Do higher unemployment benefits reduce job search?

Very preliminary, please do not cite.

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1 Introduction

The empirical and theoretical literature that tries to assess the impact of unemployment insurance (UI) designs on economic outcomes such as unemployment incidence, unemployment duration, and post unemployment job characteristics is vast. Some of the more convincing econometric approaches use policy variations inducing large changes in potential benefit duration or replacement rates to identify their causal effect on the behavior of unemployed workers[1].

Carling et. al. (2001) use caps in the benefit level in the Swedish unemployment insurance system to estimate the effect of lower replacement rates on job finding using individuals that receive the highest possible value of UI benefits before and after the reform as a control group. Therefore treatment is varying depending on the income of unemployed individuals. However, this setting does not allow to distinguish the effect of varying replacement rates for a significant part of the population because only high income earners were affected by the policy change. In fact, the effect for low and medium income earners, for whom the welfare gain of higher benefits could be especially high because of the importance of the insurance effect, cannot be assessed using caps on benefit levels. This leads to an underestimation of the benefits of unemployment insurance.

This work is going to address this issue using a specific feature of the Swiss Unemployment Insurance System and a policy change that took place in July 2003. In Switzerland, the level of the benefit replacement rate for unemployed workers without children in the household varies depending on previous insured earnings. Until July 2003, low income earners received 80% of their previous earnings up to a daily benefit level of 130.- Sfr (previous earnings of 3526.- Sfr.). Then they stayed with a constant benefit level until the replacement rate reached 70% (previous earnings of 4030.- Sfr.). Higher income earners received 70% of previous insured earnings. In July 2003 the maximum daily benefit that could be reached with a replacement rate of 80% was raised to 140.-

[1] i.e. Card and Levine (2000), Jurajda and Tannery (2003), Lalive et. al. (2006), van Ours and Vodopivec (2005)

Sfr.[2] This reform in the Swiss unemployment insurance system allows us to estimate the effect of varying UI benefits on unemployment durations.

For the analysis, a large and informative administrative database will be used, containing a full sample of unemployed individuals entering unemployment between 1992 and 2007 in Switzerland. For each individual that gets registered at a local employment service, we observe with daily precision the entry end exit date of the unemployment spell, along with several individual characteristics, such as gender, age, nationality, but also information on schooling, qualification, function at the previous job, mother tongue, and some more. This yields an outstanding source of information to study the incentive effects of unemployment insurance.

Even though the issue of the effect of unemployment benefits on unemployment durations and job market outcomes has been extensively studied, this study wants to go further and study the following issues. *First,* the effect of unemployment benefits on unemployment durations can be accurately estimated using two econometric designs. A policy change in July 2003 allows us to measure the effect using a standard difference-in-difference design, closely related to the study of Carling et. al. (2001). Furthermore, the kink in the benefit replacement rate allows us to locally identify unemployment differences that arise because of changes in the benefits as opposed to changes in previous insured earnings. Contrary to benefit schedules of other countries (i.e. Sweden) this kink arises for medium income levels in Switzerland. That is, we can identify the effect of changes in unemployment benefits for a special population of interest.

Second, the possibility to link our unemployment register data with individual data from the social security database allows us to identify the effect of varying benefit rates on later labor market success. That is, we try to answer the question if longer unemployment durations because of higher benefits lead people to search for a more appropriate job (with the same search intensity) or if they lower their search intensity (and get the same quality of job). This is very important for policy makers, as the first case can involve

[2]Note that unemployed individuals with children always get 80% of previous earnings, independent of their income (up to a ceiling).

a higher welfare, whereas the second outcome only causes higher costs of the social insurance system.

Finally, it is still an unsolved question if individuals react different on financial incentives, depending on their cultural background. Following Bruegger et. al. (2009), we can study this issue using the language border as an instrument. That is, we can compare individuals that share the same labor markets but have different cultural backgrounds with respect to their reaction to changes in benefit levels.

2 Previous literature

There is an ongoing debate in the economic literature on optimal unemployment insurance about the optimal magnitude of replacement rates. One important element in determining optimal unemployment insurance is the dependence of unemployment durations on benefit replacement rates. Moral hazard induces unemployed individuals to search less for a new job if the replacement rate is high. The theoretical literature on optimal unemployment duration serves as the guideline for the expected results from this project. Karni (1999) highlights in his survey theoretical issues concerning optimal unemployment insurance. Atkinson and Micklewright (1991) provide an early critical survey over the effect of unemployment compensation on labor market transitions. They state that the effect of unemployment compensation levels on unemployment duration is far from robust and typically very small. Holmlund (1998) provides a survey over the theoretical and econometric analysis of unemployment insurance from the 1970s onwards. Estimations of the elasticity of expected duration with respect to benefits range from 0.2 to 0.9, depending on the state of the labor market and the country concerned. He also states that some studies found rather surprisingly negative effects of unemployment compensation on unemployment durations. One conclusion of Holmlund is, that there is still scope for empirical research on this question, especially for studies that can identify the parameters using natural experiments.

Furthermore, there is a lot of previous empirical research on the effects of increases in

unemployment benefits and potential benefit duration on the duration of unemployment and other economic outcomes. Meyer (1990) tests the effects of the level and length of UI benefits on unemployment durations with an emphasis to the weeks just prior to when benefits lapse. He finds that higher UI benefits strongly reduce the probability of leaving unemployment. Roed and Zhang (2003) use a flexible hazard rate model and a quasi-experiment in the Norwegian labor market. They find that a marginal increase in compensation reduced the escape rate from unemployment. Furthermore, the exit rate rises in the months just prior to benefit exhaustion. Finally, Lalive (2008) estimates the sensitivity of unemployment duration on extended benefit durations in Austria using a targeted program that extends benefit durations from 30 weeks to 209 weeks. He finds that unemployment duration is increased by at least 0.09 weeks per additional week of potential benefit duration among men.[3] These results suggest that financial incentives for unemployed individuals do indeed significantly alter their behavior.

Closest to this work with respect to the exploited benefit change is Carling et. al. (2001), where the authors exploit a change in the replacement rate in the Swedish UI system to identify the effect on unemployment duration. They use unaffected individuals that receive the highest possible level of unemployment benefits as a control group. This is however not without critics, as these unemployed are high income earners that are generally less affected by the insurance effect of UI benefits. In contrast, our work looks especially at low to medium income earners. Identification will be achieved by exploiting the quasi-experimental setting of the Swiss UI and the policy change in July 2003.

3 The Swiss Unemployment Insurance System

Swiss unemployment insurance is organized at the federal level and insurance rules are relatively generous. Job seekers are entitled to benefits if they have paid unemployment

[3]Berk and Rauma (1983) use a regression discontinuity design to evaluate the impact of longer potential unemployment benefit durations for prisoners after their release on an alternative economic outcome, namely recidivism rates. They find that increasing benefit duration can lower recidivism rates by up to 13%.

insurance taxes for at least six months in the two years prior to registering at the public employment service and only if they are capable of doing a regular job. Entitlement criteria also include compliance with job search requirements and participation in active labor market programs. Additionally, UI recipients have to write a minimum number of applications each month. Potential job offers are supplied by the public vacancy information system of the public employment service, or private. Non-compliance with any of the above obligations is sanctioned by complete withdrawal of benefits for a period that can last up to 30 work days. The maximum potential benefit duration is 2 years. UI recipients have to pay income and social insurance taxes except for the unemployment insurance contribution.

The most relevant regulations for this work concern the UI benefits unemployed workers can receive. Before July 2003, unemployed persons with children receive 80% of previous income up to an income of 3536.- Sfr (daily benefit of 130.- Sfr). Unemployed that have higher earnings receive the maximum of either 130.- Sfr in daily benefits or 70% of previous earnings. In July 2004, the maximum daily benefits were expanded to 140.- Sfr. Table 1 summarizes the benefits an unemployed worker can receive depending on his family status and his previous income.

Table 1: Replacement rate and benefit level in the Swiss UI System

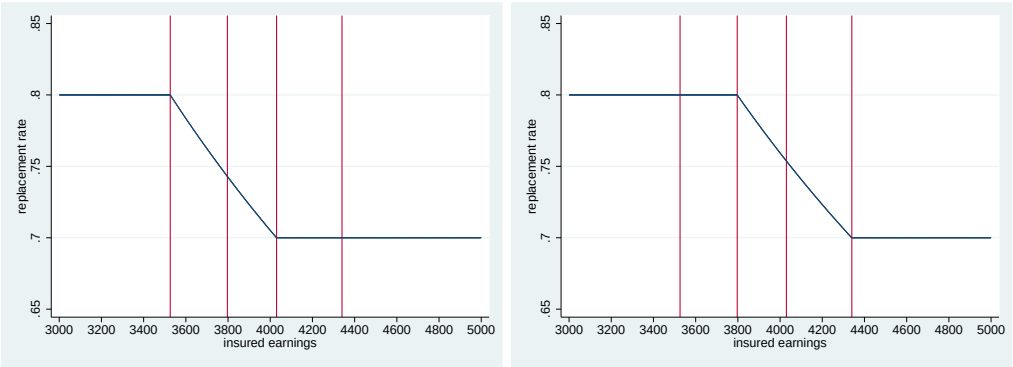
	Previous insured earnings					
	0 - 3526	3526 - 3797	3797 - 4030	4030 - 4340	4340 - 8010	8010 - ∞
Without children	70%	70%	70%	70%	70%	$\frac{260 \cdot 100\%}{21.7 \cdot E}$
With children (before 1.7.2003)	80%	$\frac{130 \cdot 100\% \cdot 21.7}{E}$	$\frac{130 \cdot 100\% \cdot 21.7}{E}$	70%	70%	$\frac{260 \cdot 100\% \cdot 21.7}{E}$
With children (after 1.7.2003)	80%	80%	$\frac{140 \cdot 100\% \cdot 21.7}{E}$	$\frac{140 \cdot 100\% \cdot 21.7}{E}$	70%	$\frac{260 \cdot 100\% \cdot 21.7}{E}$

Notes: E is insured earnings. 21.7 daily benefits can be received per month.

Figure 1 visualizes the same facts as table 1

Note that these kinks in the replacement rate schedule provide an interesting way to identify the effect of the benefit replacement rate on unemployment duration. A general objection against simple regressions of unemployment duration on benefit replacement

Figure 1: Replacement rate in the Swiss UI System before and after July 2003



rates is, that benefit levels vary with previous income, and many unobserved characteristics that vary also with income could yield spurious results for such a regression. Including previous income as a control variable is often not possible as benefit levels and income are perfectly collinear. We can provide two reasons why such a regression is possible in the special case of Switzerland. *First,* unemployment benefits are not perfectly collinear with previous earnings. Even more, the relationship between the two has arbitrary kinks. There is no obvious reason why unobserved characteristics should vary in a similar fashion as the replacement rate with previous earnings. That is, we can include insured earnings as a regressor and as a proxy for several unobserved characteristics. *Second,* instead of estimating the relationship using all observations, we can estimate the coefficients very close to the respective kinks. This is possible because we have a full sample of unemployed and therefore enough observations to estimate very locally. Therefore, even though we might not be able to control for unobserved characteristics in the whole sample, this is much easier for local regression. Individuals that earn 3500.- SFr. are reasonably similar to individuals that earn 3600.- SFr., and the assumption that we can control for still existing unobserved differences by including insured earnings as a regressor is not unbelievable.

So far we have discussed changes in the unemployment benefit levels due to the policy reform. Note however, that the reform changed one more key parameter, namely the eligibility duration for unemployment benefits. After the reform, prime age individuals

that fulfill all eligibility requirements and that are younger than 56 can get up to 400 days of unemployment compensation, that is up to 1.5 years. Individuals older than 55 can get up to 520 days of compensation. Before the policy change there was a maximum duration of 150 days for individuals with age 50 or younger, and 250 days for unemployed aged 51 to 60. This is a strong extension of unemployment benefit durations. In our analysis below, we control for this extension using time fixed effects. This is valid as long as we can assume that individuals did react in the same way to prolonged benefit durations, independent of their previous earnings. This is a valid assumption if we compare individuals with similar earnings with each other. However, the analysis of the effect of benefit durations would be another interesting research goal.

4 Data

The data used is constructed from two sources and contains a full inflow sample of all unemployed individuals in Switzerland from January 1998 to December 2007. Individuals were followed until end of March 2008.

The first source of information is the AVAM database. Once a job seeker files a claim for unemployment benefits, the case worker enters this claim into the AVAM/ASAL system of the ministry of labor. He then fills in information on the unemployment spell (daily precision) along with information on the unemployed that is important for job search (i.e. age, land of origin, marital status, number of dependent persons in the household, education, qualification, previous industry, mobility, community of residence, mother tongue,...). Job seekers then see the caseworker on a regularly basis and any new information is updated in the system, using new records. The second source is the complementary ASAL database. This contains for the same individuals all information that is necessary to calculate the unemployment benefits (i.e. insured earnings, dependent persons,...). AVAM and ASAL can be linked and they contain some overlapping information. There exist several studies[4] that used the same databases and they are generally

[4]i.e. Lalive et. al. (2008), Lalive et. al. (2005), Gerfin and Lechner (2002)

viewed as a qualitatively outstanding source of information on unemployed workers in Switzerland.

Besides the AVAM / ASAL database, we have social security data (ZAS) for a large sub-sample of the unemployed. This data contains information on different sources of incomes, i.e. labor income but also income from social insurance, as for example retirement payments, disability insurance, and some more. This data has for example been used by Arni et. al. (2009).

4.1 Sample selection and group construction

For the purpose of homogeneity, we selected a quite restrictive sample.

Our sample consists of Swiss prime-age males. That is, we selected on the following characteristics: Evidently only males and only Swiss citizens are in the group. Prime-age was defined as being between 25 and 60 when the unemployment spell starts. This is to avoid a large number of unemployed that have not yet finished education or that can find a direct path to early retirement through unemployment insurance. Furthermore, only individuals that are eligible for unemployment benefits are included. This makes sense because job searchers can register at a regional labor office to get job search assistance without being unemployed. The length of the spell does not compare to unemployment periods in this case. Then we only keep individuals that do actually get unemployment benefits. This is only a minor selection once eligibility has been controlled for. Furthermore, we only keep full-time unemployed and not handicapped individuals. As a final selection we only keep unemployed who are either single or married. This selection has only practical reasons. In our data, we don't observe the number of children directly. However, we observe the number of dependent persons, which can be the wife, or the children, or in some special cases even parents that depend on the income of the unemployed. Our assumption regarding this key variable is, that if a person is single, the dependents are necessarily children. However, if the unemployed is married, the first dependent is the wife and only the others are children. The knowledge about children is important to calculate the replacement rate that applies to a specific

unemployed person.

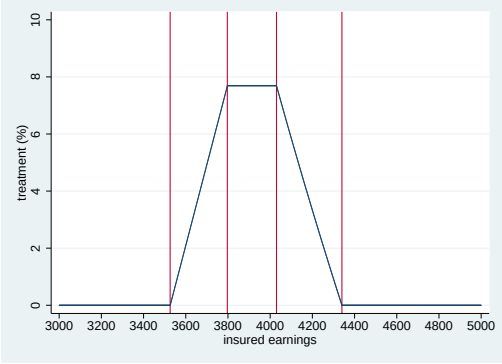
Furthermore, for this first draft, we have focused on individuals without children. The reason is that there is descriptive evidence that individuals with and without children did not develop similarly before and after the policy change. That is, individuals with children are no good control group for individuals without children.

For the descriptive statistics, we have divided the sample into 3 groups which we can observe each before and after the policy change. These groups are chosen to reflect individuals with low income that were not affected by the policy reform in July 2003, individuals with medium income that were affected, and finally individuals with high income, that were again not affected by the policy change.

1. Income $\in (0; 3525) \Rightarrow$ not affected by policy change.
2. Income $\in (3526; 4340) \Rightarrow$ affected by policy change.
3. Income $\in (4341; 8900) \Rightarrow$ not affected by policy change.

Within this second group, which is affected by the policy change, different treatment intensities are present as figure 2 shows. Moreover, treatment doesn't change discontinuously. The treatment, which is defined as the percentage raise in the replacement rate caused by the policy reform in July 2003, varies continuously between 0% and 7.7%.

Figure 2: Treatment intensities by income



For the first approach, using a difference in difference estimator, we used individuals

Table 2: Distribution of Replacement Rates before and after July 1, 2003

	Low income		Medium income		High income	
	Before	After	Before	After	Before	After
Mean	79.98	79.99	72.98	75.60	70.04	70.04
$< 70\%$	0	0	0.08	0.01	0.07	0.01
70%	0.15	0.10	36.40	4.12	99.50	99.58
(70%, 72.5%]	0.01	0.00	20.05	23.55	0.02	0.02
(72.5%, 75%]	0	0.01	15.45	17.86	0.02	0.02
(75%, 77.5%]	0.01	0.01	12.70	16.03	0.01	0.01
(77.5%, 80%)	0.16	0.06	11.85	14.19	0.02	0.01
80%	99.67	99.81	3.46	24.24	0.37	0.36

that had previous earnings between 3119.- and 4747.- Swiss Francs. For the second methodology, we wanted to go somewhat closer to the kinks in the replacement rate schedule and used only individuals than earned between 3326.- and 4540.- Swiss Francs before they got unemployed.

5 Descriptive statistics

The first question to answer is, if there is an evident treatment in the data. Table 2 shows the distribution of the replacement rate before and after the policy change for low, medium, and high income earners, respectively.

Let's have a look at the untreated groups first. The low income earners have in more than 99.6% of all cases a replacement rate of 80 percentage. The high income earners have in more than 99.5% a replacement rate of 70 percentage. Furthermore, these percentages do not change significantly before and after the policy change. However, for the medium income earners, the policy change seems indeed to have an effect. Before

Table 3: Spell characteristics before and after July 1, 2003

	Low income		Medium income		High income	
	Before	After	Before	After	Before	After
Mean duration	215.19	199.22	204.26	199.18	226.51	219.61
% of spells lasting more than						
30 days	91.63	90.62	90.03	89.44	92.38	92.65
60 days	79.13	78.95	76.10	76.95	79.39	81.86
120 days	56.09	55.47	52.41	53.25	55.71	58.35
180 days	40.92	38.81	37.71	37.62	41.01	41.93
360 days	19.64	17.20	17.76	17.40	21.59	20.12
420 days	15.58	12.82	14.77	14.34	18.33	16.48
% censored	5.47	9.06	5.39	9.46	6.99	9.55
No. of spells	26'962	21'876	20'498	16'007	63'585	59'091

the policy change, more than 36% had a replacement rate of only 70 percentage and only about 3.5% got 80 percentage of their previous earnings. However, after the policy change, these proportions seem to inverse. Only 4% have a low replacement rate and about 24% have a replacement rate of 80%. That is, the change in policy did indeed affect the unemployed.

Having showed that the treatment of higher replacement rates after July 1, 2003 really is present in the data, we will next have a look at the main variable of interest, namely unemployment durations. Table 3 contains the mean unemployment duration, and some indications on the distribution of unemployment durations along with the percentage of censored spells and the number of spells in each group.

Note first that the mean unemployment duration is always shorter after the policy change, which comes along with a higher rate of censored observations. Therefore, this reduction is purely technical. However, we can see that before the policy change, the

treated group had an unemployment duration that was about 10 to 20 days shorter than the untreated groups. After the policy change, this difference has vanished when comparing with the low income earners or strongly reduced when comparing to the high income earners. So along with higher replacement rate, we can see a relative augmentation of mean unemployment duration in the treatment group.

Let's now have a look at the background characteristics of the unemployed in our sample. Table 4 shows descriptive statistics for all three groups before and after the policy change respectively.

Because income is rising with age, the mean age is generally higher in the second and the third group. Also, very naturally, mean earnings rise with the income groups. Our data contains rather detailed information about the function in the previous job of the unemployed person. They are differentiated into independent workers, management, skilled workforce, non-skilled workforce, apprentices, home-workers, and students. Note that by the regulation of UI, independent workers are generally not eligible for unemployment benefits. That's why the percentage of independents is very low in all groups. Naturally, there are more managers and skilled workers in the third group and more non-skilled workers, apprentices, and students in the first group.

It is interesting to recognize that German speakers are relatively more present in the third group than in the first two groups. Latin-speakers are over-represented in the low and medium income group.

Unsurprisingly, the highly qualified unemployed are strongly represented in the high income groups. However, qualification levels don't seem to differ very much between the low and medium income earners.

We also know for all unemployed people if they were employed before their unemployment spell or if they search their first job, try to reenter the labor market or are in reeducation. For the medium and high income earners, about 98% of all individuals had a job before they got unemployed. The low income group is somewhat more diversified. There, only 83% had a job before their spell, 10% are in search of their first job, and

Table 4: Descriptive statistics

	Low income		Medium income		High income	
	Before	After	Before	After	Before	After
Age	33.23	32.03	34.06	33.27	38.00	37.62
	(8.91)	(8.21)	(9.01)	(8.53)	(10.09)	(9.77)
Ins. earnings	2854.34	2851.65	3963.97	3973.65	6054.76	6069.50
	(600.95)	(626.99)	(235.97)	(233.36)	(1387.36)	(1399.66)
% independent	0.78	0.66	0.43	0.36	0.52	0.51
% management	2.57	2.19	2.36	1.73	17.10	13.80
% skilled	52.92	50.49	67.93	66.25	74.19	75.75
% non-skilled	29.78	25.88	27.51	29.13	7.72	9.50
% apprentice	1.98	3.76	0.45	0.60	0.12	0.19
% home-worker	0.02	0.03	0.00	0.01	0.00	0.00
% student	11.97	17.00	1.30	1.91	0.35	0.25
% 1st lang. German	56.09	57.78	61.98	61.37	71.80	71.16
% 1st lang. French	30.14	28.44	26.32	25.91	20.99	21.26
% 1st lang. Italian	8.96	7.49	6.77	6.30	3.96	3.49
% 1st lang. Raetho-R.	0.37	0.25	0.66	0.52	0.35	0.33
% qualification high	71.06	69.52	71.60	63.65	86.78	81.11
% qualification medium	13.76	13.34	14.87	17.85	7.95	10.80
% qualification low	15.18	17.14	13.53	18.50	5.27	8.09
% sit. before employed	83.45	85.18	96.78	97.75	97.95	98.92
% sit. first job	10.31	10.71	0.98	0.95	0.32	0.24
% sit. reentry	4.95	3.36	1.47	1.01	1.13	0.60
% sit. reeducation	1.29	0.74	0.77	0.29	0.60	0.25
% placement w/o help	3.60	1.48	3.32	1.41	4.09	1.55
% placement easy	13.51	8.61	14.85	8.63	18.10	9.58
% placement medium	61.28	70.59	65.71	72.32	66.80	75.69
% placement difficult	17.00	16.22	13.49	15.71	9.42	12.07
% placement spec. case	3.76	2.36	1.93	1.54	0.82	0.78
% ex unknown	0.27	6.89	0.29	6.65	0.22	5.98
% ex ES	17.07	11.35	21.60	15.41	18.37	15.07
% ex Self	47.30	49.27	50.64	49.78	60.18	58.89
% ex Other	2.69	2.27	2.67	2.34	2.84	2.47
% ex No Job	32.67	30.22	24.80	25.81	18.39	17.59
Observations	26'962	21'876	20'498	16'007	63'585	59'091

about 5% want to reenter the labor market.

An interesting and rather unique variable in our data is the assessment of the caseworker with respect to the placement possibilities of the unemployed. This assessment is done very fine, and we aggregate here to the following five categories: (1) the unemployed can find a job completely without help; (2) it is easy to find a suitable job for the unemployed; (3) it is somewhat difficult to find a job; (4) it is difficult to find an acceptable job; or (5) the unemployed constitutes a special case. First, we can note that after July 2003, it seems generally more difficult to find a job. This yields a shift in the distribution towards worse assessments of the placement possibilities. Furthermore, high income earners are generally more easy to mediate than low income earners, what could be caused by their better education or because they have more labor market experience.

Besides these characteristics of the unemployed, we also have some information about the exit destination after the unemployment spell has finished. We differentiate between exits to unknown destination, exits to jobs that are mediated by the employment service, exits to self found jobs, exits to other jobs, or exits out of the labor market. First note that exits to unknown destinations or to "other jobs" are almost balanced over the income categories. However, individuals that get unemployed after the policy reform are much more likely to exit to an unknown destination. This can be explained by the fact that censoring is higher for these unemployed. Individuals with a medium income are somewhat more likely to get assigned a job by the local labor office. However, the percentage of mediated jobs decreases after the policy reform. This might again be an effect of our sample selection, because employment offices tend to mediate jobs later in the unemployment spell. If spells are censored, we do not observe if later on the unemployed individual got a job assigned by the employment service.

The probability that individuals find a job themselves is higher for individuals with high previous income. This can have several reasons as for example more skills with respect to job search. Interesting is the last exit destination. Men that had a low income before getting unemployed have a very high propensity to leave the labor force. More than 30% of them decide to stay at home or change to another social insurance scheme.

For high income earners, this percentage is only about half as large.

Over all, we can see that the groups do significantly differ in many background characteristics. This does also mean, that we cannot take one of the non-treated groups as a control group for the estimation of the policy effect. However, if we can measure the effect close to the income frontier between treated and non-treated groups, this will assimilate the groups, because we then compare individuals with very similar incomes before their unemployment spell.

5.1 Descriptive graphical analysis

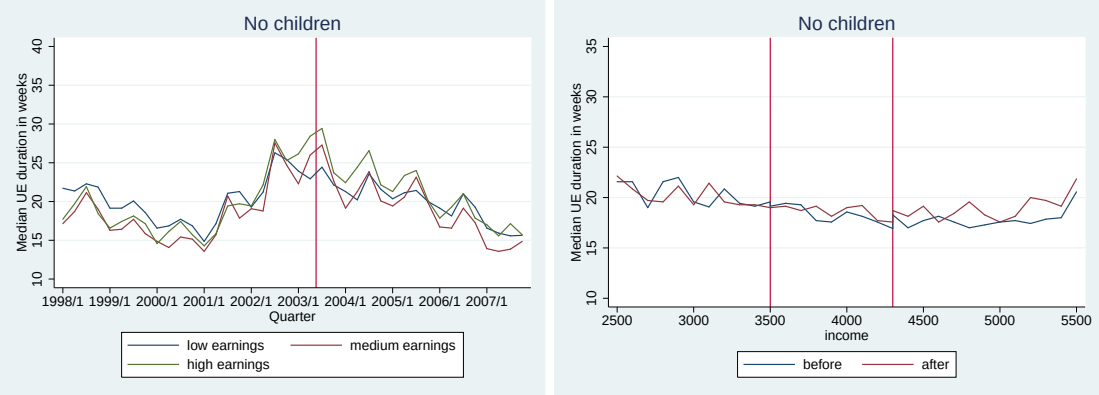
Figure 3 shows median unemployment duration over time and over previous earnings. What would we expect if the raise in the replacement rate had a positive effect on unemployment durations for the treatment group?

When we plot median duration over time, we would expect that the medium income earners would be relatively longer unemployed after the policy change than before. However, the effect could arise already somewhat before the change because of anticipation and because individuals that are already unemployed on July 1, 2003 will switch the regime.

When looking at median duration over previous earnings, we would expect that the treatment group would have higher unemployment durations after the policy change. Furthermore, we would expect this change to be relatively sharp at the income borders, as the treatment intensity rises very fast at these borders.

The evidence from figure 3 is not very clear. When comparing the medium group with the low group, we predicted effect could effectively be seen. From 1998 to about mid 2002, the treated group had always lower unemployment durations than the low income group. However, this changes somewhat before July 1, 2003 and from then on, the two groups move almost together. This could be weak evidence for an increasing relative unemployment duration after the policy change. However, taking into account the high earners destroys this evidence. There, the evidence is actually the inverse of

Figure 3: Median unemployment duration

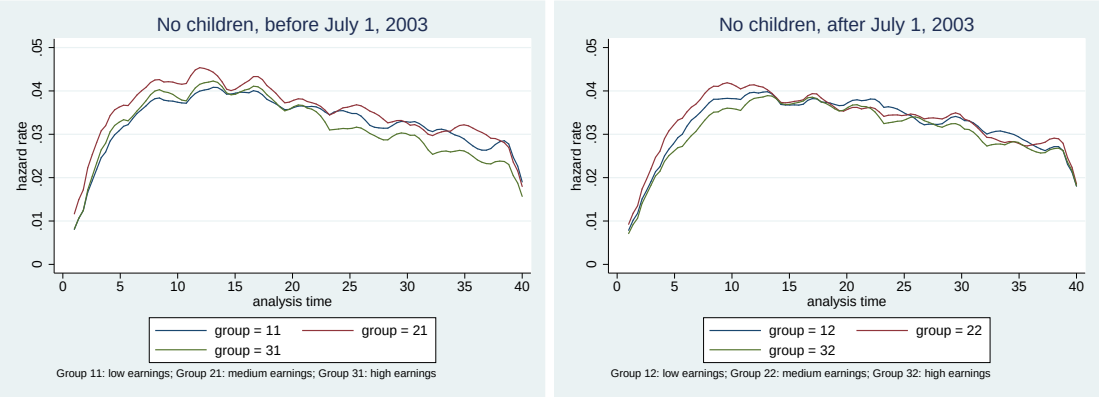


our prediction. So the evidence from this figure is mixed.

The second panel in figure 3 is even less explicative, and we cannot see any sign for a treatment effect.

Despite this negative evidence, we will now have a look at hazard rates for the first 40 weeks of unemployment for the different groups. Figures 4 and 5 contain the results.

Figure 4: Hazard rates for different earning levels

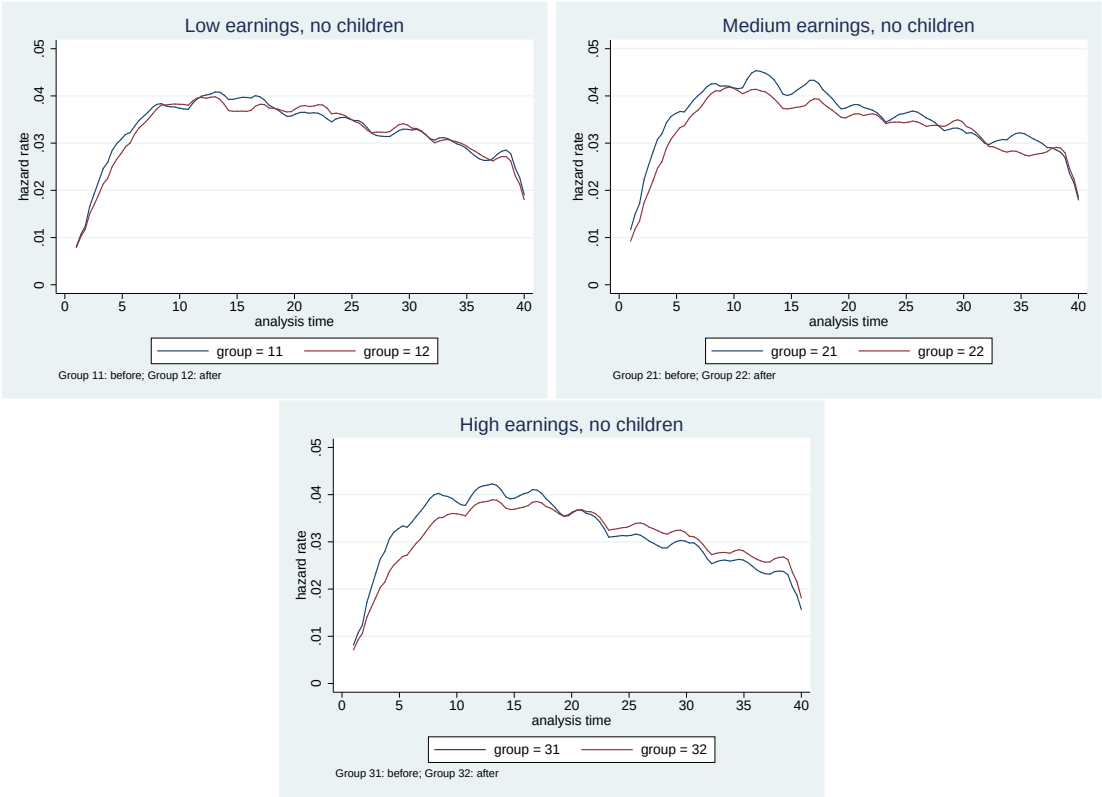


When comparing the hazard rates for the different income levels, before and after the policy respectively, we can see that the hazard rates seem to be more similar after week 15 after the policy change than before. Else we cannot see any evidence for a treatment

effect.

Figure 5 changes the perspective and compares hazard rates before and after the policy change, holding the income group constant.

Figure 5: Hazard rates before and after the policy change



When looking at the first two panels, we can see that the hazard rate for the treated group diminished more than the hazard group for the low income earners. However, the hazard rate for the high income earners diminished even more. This confirms the results from the graphical analysis of median durations.

So up to now we do not find any descriptive evidence for a treatment effect. Could it be that changes in the replacement rate do not have any effect on unemployment duration in this special setting? In the next section, we will treat this question in more detail.

6 First results

6.1 Difference-in-differences

This section presents the first regression results, which follow a rather standard difference-in-difference approach. In fact, we compare the treated and untreated groups before and after the policy change. There are some specialities that are worth to be mentioned:

1. We have excluded individuals that got unemployed between January 1 and June 30, 2003, because of possible anticipation effects and regime switching problems. Note however that, even though this goes in the right direction, it might be not enough exclusion because there are about 40% of individuals that stay longer than 6 months in unemployment.

2. *Sample restriction:* To make the groups that we compare more similar, we restrict the sample to contain only a specific range of previous incomes. If "all incomes" are included, that means we include all incomes from 3119.- to 4747.- Swiss Francs. If we regress using only "low incomes", that means that we use incomes from 3119.- to 3933.- Swiss Francs. "High incomes" include previous earnings from 3933.- to 4747 Swiss Francs. These restrictions are made to not compare very poor with very rich individuals.

3. *Continuous vs. discrete treatment:* When regressing with *continuous treatment*, we use a treatment dummy that has been scaled according to the treatment intensity. That is, if there is no treatment, the dummy variable will take the value of 0. If there is maximum treatment intensity (7.7%), the dummy takes a value of 1. For all treatment intensities in between, the variable is linearly adjusted. That is, the treatment variable is no longer binary but rather continuous between 0 and 1. For the case of *discrete treatment*, the treatment variable can only take values of 0 and 1. All income groups that have a treatment intensity between 0% and 7.7% were excluded of the regression. That is, only observations with no or with maximum treatment were kept in the regression sample.

Table 5 shows the results of two sets of regressions. We regress unemployment duration in days on a treatment indicator, as explained above, and on a dummy specifying if the individual got unemployed before or after the policy reform. Our coefficient of interest is the interaction term between treatment and getting unemployed after the change in regulations. In panel a. there are continuous treatment regressions, in panel b. there are discrete treatment regressions. Furthermore, the analysis has been done comparing all treated with all untreated, or comparing only low or only high income earners with each other. Finally, we include controls sequentially. First only the treatment variables are included, then cantonal fixed effects are added, and finally individual characteristics. These characteristics include age of the unemployed, number of dependent persons, the function in previous employment, qualification, the case-worker assessment of ease of placement, marital status, and the willingness to commute or relocate.

From our prior expectations, that higher unemployment benefit will enhance unemployment durations, we expect the interaction term between the treatment variable and the time dummy to be positive. We have no a priori believe on the treated variable itself, because it is difficult to say how the treated group is different from the untreated. If unemployment durations decrease with rising income (because of higher search productivity), the variable would be negative for the low income regressions, but positive for the high income regressions. Furthermore, we expect the time variable "After" to be negative because of the censoring that is present in our sample.

The results from panel a. show that unemployment duration has been increased by about 6 days when raising unemployment benefits by 7.7%. On a mean duration of 198 days, this yields an increased unemployment durations by about 3%. Therefore, we estimate an elasticity of about 0.4, which seems to be reasonable, comparing to previous literature. Furthermore, this effect seems to be driven by the comparison of low income individuals, and cannot be found when comparing high income individuals.

Table 5: Regression of UE duration

	All incomes			Low incomes			High incomes		
Duration	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a. *Continuous treatment*									
Treated	.206	4.682*	-.162	-2.435	7.982**	-2.913	3.009	1.264	1.168
	(2.50)	(2.45)	(2.40)	(3.50)	(3.45)	(3.41)	(3.55)	(3.50)	(3.41)
After	-6.188***	-5.295***	-6.228***	-11.713***	-9.411***	-7.019***	-.730	-1.208	-4.960**
	(1.68)	(1.65)	(1.64)	(2.33)	(2.28)	(2.27)	(2.42)	(2.39)	(2.38)
Treat After	8.286**	6.635*	6.038*	13.187***	9.362*	9.169*	2.973	3.494	2.830
	(3.58)	(3.51)	(3.42)	(4.98)	(4.87)	(4.74)	(5.14)	(5.05)	(4.92)
Constant	199.909***	186.821***	-3.679	205.066***	184.915***	-13.690**	194.880***	188.613***	2.083
	(1.21)	(1.89)	(4.50)	(1.71)	(2.68)	(6.65)	(1.73)	(2.66)	(6.14)
Ind. char.	No	No	Yes	No	No	Yes	No	No	Yes
Canton FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	76'113	76'113	76'113	36'971	36'971	36'971	39'173	39'173	39'173
R^2	0.000	0.040	0.093	0.001	0.046	0.101	0.000	0.036	0.087
b. *Discrete treatment*									
Treated	-2.393	1.698	-3.481	-6.224**	3.315	-7.367**	1.873	.045	-.712
	(2.94)	(2.89)	(2.83)	(3.18)	(3.14)	(3.12)	(3.27)	(3.23)	(3.15)
After	-6.168***	-5.083***	-5.054***	-11.228***	-8.652***	-5.515**	-.571	-1.329	-4.825*
	(1.82)	(1.79)	(1.79)	(2.45)	(2.39)	(2.38)	(2.72)	(2.69)	(2.68)
Treat After	10.109**	9.037**	8.673**	15.168***	12.843***	10.320**	4.512	5.004	6.870
	(4.28)	(4.20)	(4.09)	(4.58)	(4.49)	(4.38)	(4.73)	(4.65)	(4.52)
Constant	199.801***	185.730***	-1.208	203.632***	182.191***	-9.612	195.535***	191.064***	6.624
	(1.33)	(2.19)	(5.37)	(1.80)	(2.83)	(6.95)	(1.95)	(3.06)	(7.04)
Ind. char.	No	No	Yes	No	No	Yes	No	No	Yes
Canton FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	51'865	51'865	51'865	32'079	32'079	32'079	30'116	30'116	30'116
R^2	0.000	0.041	0.095	0.001	0.047	0.103	0.000	0.036	0.088

Including only individuals with no or maximum treatment does not change the general conclusions, but it increases the estimated effect somewhat.

6.2 Local regression results

Table 6 presents the first results for the effect of the benefit replacement rate on unemployment durations, estimated using the regime switching points for identification. All presented results are for prime-age males.

One interesting question is, if incentive effects differ depending on cultural background. For a first answer to this question, we estimate the effects also including a dummy for living on the Latin speaking side of Switzerland, and an interaction term between the replacement rate and living on the Latin speaking side. From earlier studies, we know that living on the Latin speaking side significantly raises your unemployment duration, however, we don't know anything about the interaction between the cultural variable and the replacement rate.

As a first step, the estimations are performed using a uniform kernel. That is, we don't weight observations. Later on, we can weight observations according to their distance with respect to previous income to the kink.

Note that all regressions in table 6 include individual characteristics[5], state dummies, as well as quarterly time dummies. Columns (1) and (2) show linear regressions with log unemployment duration as the dependent variable. The effect of the replacement rate on unemployment duration is significantly positive. Furthermore it is very high. An increase in the benefit level by 1 percentage point increases unemployment duration by 3.59%. This yields an elasticity of about 2.5, which is more than twice as large as what estimated Roed and Zhang (2003) in their study. More interesting, this elasticity is about five times as high as the elasticity we got using the treatment approach. Comparing this

[5]Age, number of dependent persons, function in previous employment, qualification, case-worker assessment of ease of placement, marital status, and willingness to commute or relocate.

Table 6: The effect of the benefit replacement rate on unemployment durations using local regressions

	Linear regression		Competing risks				Competing risks with culture			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(duration)		All	ES	Self	NoJob	All	ES	Self	NoJob
Repl. Rate	0.0359***	0.0352***	-0.0276***	-0.0193***	-0.0171***	-0.0600***	-0.0271***	-0.0206***	-0.0166***	-0.0607***
	(0.00372)	(0.00393)	(0.00308)	(0.00702)	(0.00423)	(0.00677)	(0.00319)	(0.00727)	(0.00436)	(0.00692)
Latin		0.0575					-0.0928	-0.178	-0.253	-0.300
		(0.190)					(0.172)	(0.386)	(0.236)	(0.365)
Latin·Repl. Rate		0.00204					-0.00209	0.00373	-0.00255	0.00204
		(0.00254)					(0.00229)	(0.00516)	(0.00317)	(0.00482)
Earnings	0.000933**	0.000958**	-0.000729**	0.000329	-0.000782	-0.000551	-0.000761**	0.000361	-0.000837*	-0.000551
	(0.000429)	(0.000429)	(0.000354)	(0.000830)	(0.000484)	(0.000700)	(0.000355)	(0.000831)	(0.000485)	(0.000700)
Earnings^2	-7.37e-08	-7.65e-08	6.20e-08	-3.85e-08	9.15e-08	-4.27e-08	6.53e-08	-4.22e-08	9.70e-08	-4.30e-08
	(5.25e-08)	(5.25e-08)	(4.37e-08)	(1.01e-07)	(5.96e-08)	(8.71e-08)	(4.37e-08)	(1.01e-07)	(5.96e-08)	(8.72e-08)
Constant	-1.345	-1.351								
	(1.028)	(1.030)								
Observations	52954	52954	52954	52954	52954	52954	52954	52954	52954	52954
R^2	0.092	0.093	.	.	.	.	.	.	.	.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

result to column (2) of the same table, we can see that inclusion of the language region does not alter the estimated effect. Moreover, the language region variables are both insignificant.

Columns (3) to (6) and columns (7) to (10) in table 6 show two sets of competing risks regressions, analogous to the linear regressions discussed above. The first set is estimated without inclusion of language region dummies, whereas the second set of regressions does include them. Again, the coefficients obtained in the two specifications are very similar, therefore we discuss only the first set of estimations. Note also, that language region variables are always insignificant.

One can see that an increasing replacement rate lowers the exit hazard for all exit destinations, namely for all exits (column 3), for exits to jobs mediated by the employment service (column 4), for exits to jobs found by the unemployed individual itself (column 4), and finally also for exits out of the labor force. The reason for this lower hazard rates is not yet completely clear and could be even spurious. But the following ideas could explain some of the lowered hazard rates. Individuals lower their search effort and raise their expectations to a new job, i.e. their reservation wage, if unemployment benefits are higher. This causes hazard rates to self found jobs to decrease. Furthermore, this could also have an influence on the hazard rates for jobs mediated by an employment service. This because perfect monitoring is not possible and unemployed individuals might get "picky" when accepting a job or not. It is interesting to see that the by far highest effect can be found for exits out of the labor force. This makes sense because the higher benefits increase incentives to stay in the unemployment pool, even though one has to comply with several job search requirements.

In sum, the exit hazard diminishes by about 2.7% if the replacement rate raises by 1 percentage point. This effect is somewhat less than 2% for jobs mediated or found by the unemployed, and is as high as 6% for individuals that exit the labor force.

7 Conclusions and caveats

Despite the descriptive statistics, that were not very promising, using econometric methods allows us to find the expected positive effect of unemployment compensation on unemployment durations. However, results differ very much between the first and the second methodology. Furthermore, estimated elasticities amount to as much as 2.5 (local regressions), whereas it is estimated to be 0.4 in the first methodology (treatment effects). Earlier studies have found elasticities between 0.5 and 1 (Roed and Zhang, 2003), which leads me to be very careful in the interpretation of the results. Most probably, identification of the true effect failed when using local regression, and we should reconsider estimation using a new econometric model.

Furthermore, it is troubling that the estimated effects of language regions on unemployment durations are not significant. This is opposite to what found Bruegger et. al. (2009). The final conclusion is, that these results are surely not the end of the story and much work has to be done to find the true causal effect of unemployment compensation on unemployment durations.

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