Does Reducing Unemployment Insurance Generosity Reduce Job Match Quality?

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Abstract

This paper analyzes how a change in Slovenia's unemployment insurance law affected the quality of jobs workers found after periods of unemployment. Taking advantage the "natural experiment" we show through difference-in-differences estimation results that reducing the potential duration of unemployment benefits had no detectable effect on wages, on the probability of securing a permanent rather than a temporary job, or on the duration of the post-unemployment job.

The impact of unemployment benefits on the quality of post-unemployment jobs is theoretically ambiguous and insufficiently researched empirically. According to job search theory, an increase in the level of benefits raises the reservation wage at the beginning of the covered spell of unemployment, leading to post-unemployment wage gains (and gains in stability and other aspects of newly found jobs). According to another line of reasoning, which also suggests that unemployment benefits increase the probability of finding a job and/or the probability of finding a better job, such payments increase the resources available for a job search, thereby facilitating more efficient job matching. According to the moral hazard argument, however, longer-lasting benefits

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may depress the intensity of the job search and may just prolong unemployment without improving the quality of post-unemployment jobs.[1] Longer spells of unemployment may also worsen post-unemployment outcomes through loss of human capital, as workers' skills become obsolete.

To shed light on these opposing views, we analyzed the impact of unemployment benefits on the quality of job matches by taking advantage of a "natural experiment": the reform in 1998 of Slovenia's unemployment insurance law. The reform radically shortened the potential duration of benefits for most – but not all – groups of workers, which allowed us to use a difference-in-differences estimation strategy to compare pre- and post-reform outcomes. Exceptionally rich administrative datasets on both unemployed and employed workers permitted us to examine not only post-unemployment wages, but also the duration and type of employment in the post-unemployment job.

We find that shortening the duration of benefits did not affect the quality of post-unemployment job matches. None of the quality indicators was negatively affected by the reform: the pre-reform wage increases in new jobs stayed unchanged, as did the duration of the newly found jobs and the distribution between fixed-term and permanent jobs. These findings are particularly important combined with the findings of our companion paper (Van Ours and Vodopivec, 2006a), which showed that the same benefit reform strongly shortened the spell of beneficiaries' unemployment. Taken together, the results show that once the potential duration of benefits was shortened, benefit recipients found a job more quickly with no loss in the quality of the post-unemployment job match – an indication that reducing benefits reduced the moral hazard created by unemployment insurance.

How do our results compare with the findings of other studies? Ehrenberg and Oaxaca (1976), Burgess and Kingston (1976), Hoelen (1977), and Barron and Mellow (1979) find a statistically significant and positive relationship between benefit levels and post-unemployment wages.[2] But Classen (1977), Blau and Robins (1986), Kiefer and Neumann (1989), and Addison and Blackburn (2000) find a weak or negligible effect on post-unemployment wages. Meyer (1995) finds that re-employment bonuses shortened the duration of compensated unemployment without

[1] Particularly credible evidence on the effects of unemployment insurance on duration of recipients' spell is provided by micro-studies (for a recent summary, see Vodopivec, 2004a).

[2] Ehrenberg and Oaxaca (1976) estimate that a 10-percentage-point increase in the benefit replacement rate increases post-unemployment wages by 7% for older men and 1.5% for older women. Using New Zealand data, Maani (1993) finds that a 10-percentage-point increase in the benefit replacement rate is associated with a 4.5% increase in post-unemployment wages.

affecting post-unemployment wages. There is also mixed evidence about the effects of unemployment benefits on the duration of post-unemployment jobs. Using U.S. data, Centeno (2004) finds that a more generous unemployment insurance (UI) system is positively related to post-unemployment job tenure, and Tatsiramos (2006) reaches the same conclusion using data for ten European countries. Similarly, Böheim and Taylor (2002) find that longer spells of unemployment are rewarded by longer job tenure. But Portugal and Addison (2003) find no evidence that unemployment benefits facilitated entry into stable jobs in Portugal, and Belzil (2001) finds a negative correlation between unemployment duration and subsequent job duration for Canada. Moreover, Card et al. (2006), using Austrian data, find that extending the duration of potential benefits lengthens the spell of unemployment but has little or no effect on the quality of subsequent job matches.

Our paper is set up as follows. In section 1 we describe the 1998 change in Slovenia's unemployment insurance system. In section 2 we explain our research strategy and present data. In section 3 we present the results of our analysis of post-unemployment wages, of the probability of obtaining a permanent rather than a temporary job, and of the duration of the post-unemployment job. We present our conclusions in section 4.

1. The reform in Slovenia's unemployment insurance system

Like most OECD countries, Slovenia provides unemployment benefits through a social insurance program covering all formal sector workers.[3] To qualify for benefits, a worker must be enrolled in the program and the job loss must have been involuntary (disciplinary dismissals are excluded). To maintain the entitlement, applicants must be capable of, available for, and looking for work. The level of benefits depends on the earnings on the previous job, and the replacement rate is 70% in the first three months and 60% thereafter, subject to a minimum and maximum. Benefits may last from 3 to 24 months, depending on the individual's work experience. After exhausting earnings-related benefits, workers may qualify for "unemployment assistance" – that is, means-tested, flat-fee benefits available to those with per capita family incomes below a certain threshold. In principle, benefits are financed by contributions from employers and workers, but because

[3] Formerly a part of Yugoslavia, Slovenia – a country with population of only 2 million – became independent in 1991 and joined the European Union in 2004.

contribution rates are low (the combined contribution rate of employers and workers is just 0.2% of gross wages) significant budget subsidies are required. Workers registered as unemployed are also entitled to employment services (such as job referrals and vocational counseling) and have the opportunity to participate in active labor market programs.

In October 1998, when Slovenia reformed its unemployment benefit system, the most significant change was a reduction in the potential duration of benefits. Under the new system, the length of the entitlement period for unemployment insurance was shortened roughly by half for most groups of recipients. Before reform, for example, workers with 5 to 10 years of work experience were eligible for 9 months of benefits, and workers with 10 to 15 years of experience were eligible for 12 months. After reform, both groups were eligible for only 6 months of benefits. A notable feature of reform was that different groups of beneficiaries were treated differently – a trait we take advantage of in testing reform's effects.

Reform also called for several measures aimed at speeding up benefit recipients' reemployment, including improvements in employment services, the obligatory preparation of a re-employment plan for each benefit recipient, and more frequent contact between counselors and recipients. Moreover, reform called for stricter monitoring of eligibility and introduced stiffer sanctions for refusing job offers. The definition of a suitable job was also broadened and, after 4 months, benefit recipients could be asked to take lower-paying jobs or jobs requiring a substantial commute.

2. Exploiting the natural experiment of unemployment benefit reform

Empirically testing how unemployment benefits affect post-unemployment job matches is complicated by the fact that factors responsible for variations in benefit generosity among workers also often affect post-unemployment job characteristics and wages. Our strategy for isolating these effects was to exploit the exogenous variation in the generosity of benefits introduced by the 1998 reform of Slovenia's unemployment insurance law, a variation that warrants a difference-in-differences approach.

2.1 Strategy for identifying reform's effects

To exploit the natural experiment offered by the1998 reform of Slovenia's unemployment insurance law, we adopted a difference-in-differences approach, to isolate the effects of changes in potential benefit duration (PBD) on the quality of post-unemployment job matches. Indeed, the specific design of the legislative change created a natural control group, for which the PBD remained unchanged, as well as four treatment groups – groups for which reform sharply reduced the PBD. For treatment groups, this change introduced an exogenous variation in PBD. We estimated the impact of the varied changes in PBD on post-unemployment job outcomes by comparing the difference between the outcomes of control and treatment groups before and after reform. We studied outcomes for the period from mid-1997 until the end of 2001.[4]

Did the difference-in-differences approach provide unbiased estimates of the effects of program changes that we studied? Did it adequately separate the effects of changes in macroeconomic and labor market conditions from the effects of the legislative change on PBDs? The difference-in-differences approach would work only if macroeconomic shocks and changes in labor market conditions and policies affected treatment and control groups in the same or very similar ways, so we also looked at macroeconomic and labor market conditions for the period studied.

Macroeconomic conditions in Slovenia from 1997 through 2001 were fairly stable. Writing about Slovenia's transition (1991-2003) , Mrak, Rojec and Silva-Jauregui (2004) note that "after initial transitional recession (…) Slovenia succeeded very quickly in regaining growth momentum" and that "throughout the transition period Slovenia sustained favorable positions in its fiscal and external accounts. One can therefore say that the country's macroeconomic policies aimed at stabilization and liberalization have been successful". Indeed, in the period 1997-2001, Slovenia recorded stable growth of 3.0% to 5.2% a year, and moderate inflation (compared with other transition countries) from 6.1% to 8.9%. Unemployment rates in the same period were relatively low and stable, ranging from 7.1% to 7.7% before dropping to 5.9% in 2001. Worker and job flows surged in the beginning of 1990s, before stabilizing (Vodopivec 2004b).[5]

[4] Another feature of the Slovenian unemployment insurance system – that PBDs vary across groups of workers – cannot be used to estimate the effects of PBD on quality of post-unemployment job matches. Workers with a longer work history – those who are, under the law, entitled to longer PBDs – are also older than workers with shorter work history, so age, rather than longer PBD, may be what determines the quality of post-unemployment jobs.

[5] Unemployment insurance law enforcement and monitoring regulations (Official Gazette of Slovenia, No. 17/99) apply to all groups of beneficiaries indiscriminately, so one should not expect that the impact of monitoring and enforcement of search requirement was group-specific.

2.2 Using a natural control group and four treatment groups

Our empirical analysis rests on data about five groups of unemployed workers: one control group and four treatment groups. Within each group, some unemployed workers started to collect benefits before the law changed, and some after. Had the law not changed, all members of a group would be entitled to benefits of the same potential duration. Because some of the recipients in a group registered after the law changed, their entitlement was much reduced in duration. The exception is the control group, for whom the PBD was unchanged.

For these five groups, 'old' and 'new' benefit entitlements are presented in Table 1. The first group, which has limited work experience (12 to 30 months), is also the only group for which the potential duration of benefits (3 months in their case) did not change. For the second group, with work experience of 2.5 to 5 years, the PBD was reduced from 6 to 3 months. All the other groups also faced a reduction in the PBD. Implicitly, the formation of groups is also strongly correlated with age. The older workers are, the more work experience they have and the longer their potential benefit duration when they lose their job.

To avoid selectivity, we avoided data on the inflow into unemployment around the date reform was introduced. Our sample is of data on the inflow into unemployment for two periods: August 1, 1997 – July 31, 1998 and January 1, 1999 – December 31, 1999. Both time periods cover a year of inflow so we do not have to worry about seasonal differences in the composition of the inflow.

Table 1 shows how the change in the PBD affected median unemployment durations. For the group of workers eligible for 3 months of benefits before and after reform (the control group), the median duration of unemployment was reduced by 0.3 months (for men) and by 0.8 months (for women). For the other groups of workers, the PBD and the median duration of unemployment were reduced more greatly than for the control group. For the group for which the PBD was reduced from 18 to 9 months, for example, the median duration of unemployment was reduced by 3.6 months (for men) and by 6.3 months (for women). From this we reached two conclusions: First, the median duration of unemployment was longer for women than for men. Second and more important, the greater the reduction in potential benefit duration, the greater the reduction in median duration of unemployment. [6]

[6] See for more details Van Ours and Vodopivec (2006a).

2.3 Data sources

Our analysis is based on administrative records of unemployment spells for the recipients of unemployment benefits, as well as records of their post-unemployment employment spells, for all unemployment spells that started during August 1, 1997 – July 31, 1998 and during the period January 1, 1999 – December 31, 1999 (with censoring on December 31, 2001). We used data sets from the following sources:

- The data set on registered unemployed is from the National Employment Office of Slovenia. For each spell of unemployment, data includes starting and ending dates of registered unemployment, destination of exit, and information on the receipt of unemployment insurance benefits (starting and ending date of the eligibility and actual ending date of receipt). Personal and family characteristics of recipients are also included.
- The work history data set is from the Statistical Office of Slovenia. For all formal sector workers, the data includes information about their employment spells. The data, obtained from social insurance records, include the starting and ending date of employment, the type of appointment, occupation, and personal characteristics (gender, age, education).
- The workers' earnings data set, from the Pension and Disability Fund, contains information on earnings associated with each employment spell for workers employed in the formal sector. For each year (or part of the employment spell within a year) the information collected includes the amount of earnings, the number of hours worked in regular time and overtime, and the starting and ending date of the earnings period.

The data sources provide exceptionally rich high-quality information. First, they provide complete coverage; all registered unemployed workers in the selected period were included. Second, being administrative in nature, the information is free of problems common in survey data (such as non-response and interviewer bias). Third, because we combined information about unemployment and subsequent employment spells, the information at our disposal not only covers the whole unemployment spell but also provides accurate information about the timing of transitions from unemployment to employment. Unlike many studies of unemployment in which administrative data on the job-finding date is based on unreliable reporting by unemployed workers themselves (who have little incentive to be accurate), for our study we had independent, employer-reported information about the start of post-unemployment jobs. And fourth, we have information about wages the unemployed earn in their post-unemployment jobs.

For the selected period(s) and for our five comparison groups, we have data for 25,416 individuals (11,713 men and 13,703 women). Of these, 8627 men and 9074 women found post-unemployment jobs. Information about wages, however, exists only for 5059 men and 5339 women. For the records on earnings, we merged data from the Pension and Disability Fund with data from the Statistical Office of Slovenia, based on personal identification numbers (PINs). The merging is incomplete, because Pension and Disability Fund records sometimes contain only identifiers issued by the Fund, with PIN numbers missing. There is no reason, as far as we know, that such omissions are non-random. For new earnings, in addition, there are fewer matches because at the time of data collection (the end of 2002), some employers had not yet provided data for 2001. The deadline for submission is June of the following year, but in reality many employers submit data with a delay as great as one year. Wages are calculated as earnings divided by spell hours, taking into account the amount of overtime and the fact that overtime working hours pay 20% more. To reduce the effects of errors in earnings and hours of work in our baseline estimates we limit the wage range from 200 to 1500 Slovenian Tolars.[7] This reduces the sample to about 4176 men and 4217 women.

2.4 Definition of variables

We focus on the quality of the post-unemployment job matches, so our dependent variables are (logarithm) of wages in the post-unemployment job, a binary variable indicating whether the post-unemployment job was permanent, and a binary variable indicating whether the post-unemployment job ended within a year after it started. The analysis is done separately for men and women, to account for possible differences in labor market behavior. The key explanatory variable of interest is the reduction of PBD. In the baseline estimates we use one dummy variable with a value of 1 for the groups of workers with more than 2.5 years of work experience after the change in benefit law; we use a value of 0 otherwise. Alternatively, to generate group-specific estimates of

[7] In 1997, 1 US dollar was approximately 170 Slovenian Tolars, and by 2001, 250 Tolars.

changes in PBD, we use separate dummies for each of the treatment groups. We also introduce a dummy variable “after change of law” to indicate whether a person became unemployed after the reform – to account for possible differences in macroeconomic and labor market conditions after reform that might have affected the quality of the post-unemployment job matches.

Personal characteristics of the unemployed are also taken into account. Previous work experience is represented by dummy variables for four groups (2.5–5 years of work experience, 5–10 years, 10–15 years, and 15–20 years). Age at the time of entrance into unemployment is specified as a continuous variable. For education we use dummy variables: Education2 (elementary school), Education3 (vocational school), Education4 (high school or more); reference group: unfinished elementary school. For family situation we also use dummy variables: Family1 (1 dependent family member), Family2 (more than 1 dependent family member); reference group: no dependent family members. Finally we use a dummy variable for ill health, derived from information employment office counselors got from interviews with benefit recipients.

3. Econometric results

3.1 Wages in post-unemployment jobs

Table 2 provides stylized facts about the evolution of wages. When wages in post-unemployment jobs are compared with wages in pre-unemployment jobs, wage increases are smaller after benefit reform than before. Of course, this may have to do with a change in inflation. What is important is that the differences in wage changes do not seem to be related to how much the potential benefit duration was reduced. For the male control group with no change in PBD, the difference in wage change equals –3.5%, while on average for the other groups the difference in wage changes equals –3.9%. In difference-in-differences terms, this would imply that the reduction in PBD for men led to an average wage decline of 0.4%; for women, to an average wage increase of 1.7%.

The parameter estimates from a simple log wage regression are shown in Table 3. The baseline estimates of the effect of the change in PBD are in line with Table 2. For men there is ~~a~~ small and insignificant decline in wages; for women, a positive and insignificant increase. The middle part of Table 3 shows group-specific parameter estimates for the change in PBD.[8] For men

[8] Because the parameter estimates for the other variables are hardly affected we report only the parameters of interest.

none of the estimated parameters differs significantly from zero. For women in the group for which the potential duration of benefits was reduced from 18 to 9 months there is a significant positive effect (4.9%) on wages. It is this group which is driving the average positive wage effect for women. Since the group closest to the control group has a large but insignificant wage effect (3.3%) we conclude that the change in PBD does not seem to have affected post-unemployment earnings for women, either.

Many of the other parameter estimates are as expected. For men, experience and education have a positive effect on wages; family situation and ill health are irrelevant. A somewhat surprising result is that for men age has a negative effect on the wage – because work experience is included as an explanatory variable. For women work experience and age both have a positive effect on wages.[9] Furthermore, for women education has a positive effect on wages and ill health has a negative effect. Both for men and women, conditional on all observed characteristics, the previous wage has a positive effect on the new wage. We attribute this effect to unobserved characteristics of workers that were captured by the previous wage. The elasticity is 0.2 to 0.3. If conditional on observed characteristics the previous wage was 1% higher, the current wage is 0.2% higher.

To investigate the robustness of our results, we performed a sensitivity analysis, for which the results are summarized in the lower part of Table 3. First, we did not impose any wage limits, and used all available information about wages. For men the negative effect on wages is bigger but is still estimated very imprecisely. The wage effect for women is now much smaller and is also estimated very imprecisely. Second, we trimmed the wage information by declaring all wages below 200 Tolar to be equal to 200, and all wages above 1500 Tolar to be equal to 1500. The relevant parameter estimates do not change because of this.

As a further sensitivity analysis we also investigated whether the availability of wage information was unrelated to the process that determines the wage. We followed a traditional approach, assuming a normal selection process, including the inverse Mill's ratio, to account for possible selectivity in available wage data.[10] Calculation of the inverse Mill's ratio is based on a probit estimate with personal characteristics and the duration of the calendar time period between

[9] Note that if we omit the experience dummies for both males and females we find that age has a significant positive effect.

[10] We also used a discrete factor method to account for selectivity. Unlike the traditional approach, this method does not rely on normality assumptions. The estimated wage effect did not change. See for the method and results: Van Ours and Vodopivec (2006b).

job finding and the censoring date (December 31, 2001) as explanatory variables. The coefficients of the Mill's ratios are significantly positive, indicating that higher wages are more likely to be in the sample. Nevertheless, as shown in the bottom line of Table 3, the estimated effects of the change in PBD do not change.

3.2 Type of contract

As Table 2 shows, men, especially those with little work experience, are less likely to get a permanent job after reform of Slovenia's benefit law. But experienced workers are more likely to get a permanent job after the policy change. For the male control group the probability of getting a permanent job declines 2.3%, while on average for the other groups that probability increases 2.4%. Following a difference-in-differences approach, this would suggest that for men reducing the period of potential benefits increased the probability of finding a permanent job by 4.7%. Similarly, for women the shorter benefits period reduced the probability of finding a permanent job by 5.1%. So for women the change in the benefit law might affect the behavior of unemployed job seekers in that after reform they are more likely to accept a fixed-term contract.

To investigate this in more detail, we estimated a linear probability model explaining the probability of finding a permanent job.[11] The parameter estimates are shown in the first two columns of Table 4. Clearly, the estimated effects of the change in PBD are very much in line with the unconditional difference-in-differences estimates presented in Table 2. For men the reduction in PBD led to a 4.6% increase in the probability of finding a permanent job; for women, it led to a 4.5% decrease.[12] The lower part of Table 4 shows the group-specific effects. For men the positive average effect seems to be driven by the positive effects for the groups of experienced workers. For the groups of experienced workers that are close to the control group, reducing the length of potential benefits had no significant effect on the probability of finding a permanent job. For women there is a negative effect for each of the experience groups, but for the group closest to the control group this estimated effect does not differ significantly from zero.

[11] We also estimated logit and probit models but this does not affect the relevant results.
[12] Because we are not interested in the parameter estimates that determine the transitions to temporary or permanent jobs per se, we do not discuss the other parameter estimates.

Overlooking all the incentive effects generated by reducing PBDs, it is clear that these reductions have not led to unemployed workers being ‘forced’ into temporary jobs. The findings point more to opportunistic behavior than to restricted behavior.

3.3 Job separation rates

Table 2 presents data on the probability of individuals losing their post-unemployment job within a year.[13] For the male control group this probability decreased slightly, from 51.2% before to 48.8% after the change in the unemployment law. On average for the other groups there was a decrease of 0.7% leading to a difference-in-differences estimate of 1.7%. For women this difference-in-differences estimate would be –3.0%.

To analyze the duration of post-unemployment jobs, we first estimated linear probability models for the probability of job loss within 1 year. The parameter estimates are shown in the last two columns of Table 4. Again, the estimated effects of the change in PBD are almost identical to those in Table 2. For men there is an insignificant (1.8%) increase in the probability of job loss; for women, an insignificant (2.9%) decrease. If we compare group-specific effects, for women the results are clearly driven by the effect of the group for which the PBD was reduced from 6 to 3 months. For this group the probability of job loss decreased by 6.4%. For the other groups and for all male groups there are no significant effects.

The estimates of the linear probability model ignore right-censored data on jobs lasting less than 1 year. Moreover, the job separation rate may fluctuate over the spell of unemployment. To account for these phenomena, we analyze the duration of post-unemployment jobs in more detail using hazard rate analysis, with job separation rates now the focus of interest. when calculating job separation rates, one may use information about completed and ongoing job spells, with data on ongoing job spells considered as right-censored data on job durations at the censoring date. To investigate the determinants of job separations we estimate a proportional hazard model (PH) where the job separation rate at employment duration t conditional on observed variables x

$$\theta(t \mid x) = \exp(\beta x + \Sigma_k \mu_k I_k(t)) \tag{1}$$

[13] Ignoring right-censored durations of less than 1 year.

where x includes personal characteristics and characteristics of the unemployment insurance pertaining to the unemployment spell that ended when a person started the current job, β is a vector of parameters, and the μ-parameters represent stepwise individual duration dependence in the separation rates with k (= 1,..,10) as a subscript for the ten duration intervals: 1, 2, 3, 4, 5, 6, 7–9, 10–12, 13–18, 18+ months. For reasons of normalization, we impose μ_1=0. The conditional density functions of the completed job durations can be written as

$$f(t \mid x) = \theta(t \mid x) \exp(- {}_0\int^t \theta(s \mid x)\, ds \qquad (2)$$

The parameters, estimated with the method of maximum likelihood, are presented in Table 5.[14] For both men and women the effects of the changes in PBD are insignificantly different from zero. Again, only if we distinguish between group-specific effects do we find that for the group for which the PBD changed from 6 to 3 months there is an increase in the job separation rate.

The relationship between potential benefit duration and the duration of the first job may be affected by a correlation between unobservables in the job finding rate and the job separation rate. To investigate this we estimated bivariate duration models with correlated error terms. We found that the parameters of the benefit variables hardly changed, so our main conclusions remain the same. [15]

4. Conclusions

The efficiency properties of unemployment insurance are theoretically ambiguous and empirically controversial. This paper sheds light on one aspect of that controversy: how unemployment benefits affect the quality of post-unemployment jobs. Even if unemployment benefits increase the length of time benefit recipients are unemployed – a well-established empirical fact for most unemployment insurance systems – might they at the same time improve the quality of jobs workers find after the period of unemployment? Evidence for such a finding would have important implications for unemployment insurance systems.

[14] Since the type of job – permanent or temporary – is endogenously determined we do not make a distinction according to type.
[15] See Van Ours and Vodopivec (2006b) for details; the parameter estimates are available on request.

Taking advantage of Slovenia's "natural experiment" of 1998, which helped to isolate the impact of unemployment benefits, we investigated whether greatly shortening the potential duration of unemployment benefits affected the quality of post-unemployment jobs. If reducing the potential duration of benefits forces unemployed workers to accept lower-quality jobs, their post-unemployment jobs would be less likely to be permanent and more likely to be temporary, low in pay, and short-term. But we find no evidence of such an impact. Reducing the potential duration of benefits did not affect the likelihood of a worker taking a temporary rather than a permanent job, had hardly any effect on job separation rates (the likelihood of losing a post-unemployment job within a year), and did not affect post-unemployment wages. Interestingly, the results of calculations with a simple difference-in-differences approach are very much in line with the results from regressions in which other exogenous variables are taken into account, confirming that the Slovenian policy change was indeed a "natural experiment."

Our findings, combined with those in a companion paper (Van Ours and Vodopivec 2006a), strongly suggest the presence of strategic opportunistic behavior. The companion paper shows that shortening the potential duration of benefits reduces the length of beneficiaries' unemployment spells. The present paper shows that workers found jobs faster without accepting jobs of lower quality (in terms of stability, lower wages, and type of contract). We conclude that benefits of longer potential duration contributed to longer spells of unemployment without improving the quality of post-unemployment jobs. Having longer to search for jobs had zero marginal effect on productivity. Indeed, benefit recipients might not have spent more time job-hunting at all.

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Table 1
Length of unemployment by potential duration of unemployment-insurance benefits, before and after the 1998 change in Slovenian unemployment insurance law

	Work experience (years)	PBD (months)		Median length of unemployment (months)							
				Men				Women			
		Before	After	Before	After	Δ	ΔΔ	Before	After	Δ	ΔΔ
Group											
1	1–2.5	3	3	3.8	3.5	–0.3		5.0	4.2	–0.8	
2	2.5–5	6	3	4.2	3.7	–0.5	–0.2	5.9	4.1	–1.8	–1.0
3	5–10	9	6	5.8	4.2	–1.6	–1.3	8.2	5.6	–2.6	–1.8
4	10–15	12	6	7.0	4.9	–2.1	–1.8	11.0	5.7	–5.3	–4.5
5	15–20	18	9	9.2	5.6	–3.6	–3.3	13.3	7.0	–6.3	–5.5
Average 2-5				6.0	4.5	–1.5	–1.2	8.7	5.4	–3.3	–2.5

Table 2
Quality of post-unemployment jobs by potential length of unemployment-insurance benefit before and after the 1998 change in Slovenian UI law

	Wage change (%) [a]				**Permanent job (%)** [b]				**Job loss (%)** [c]			
Group	**Before**	**After**	**Δ**	**ΔΔ**	**Before**	**After**	**Δ**	**ΔΔ**	**Before**	**After**	**Δ**	**ΔΔ**
Men												
1	12.5	9.0	-3.5		19.4	17.1	-2.3		51.2	48.8	-2.4	
2	17.2	11.4	-5.8	-2.3	19.9	20.3	0.4	2.7	47.2	46.1	-1.1	1.3
3	16.3	12.8	-3.5	0.0	22.9	23.1	0.2	2.5	43.2	44.4	1.2	3.6
4	16.1	12.7	-3.4	0.1	21.6	25.0	3.4	5.7	46.6	43.0	-3.6	-1.2
5	16.6	13.6	-3.0	0.5	22.2	27.7	5.5	7.8	42.1	43.0	0.9	3.3
Av. 2-5	16.5	12.6	-3.9	-0.4	21.7	24.1	2.4	4.7	44.8	44.1	-0.7	1.7
Women												
1	13.8	12.1	-1.7		18.2	19.8	1.6		35.8	40.3	4.5	
2	16.0	14.9	-1.1	0.6	17.7	17.7	0.0	-1.6	39.4	37.2	-2.2	-6.7
3	20.7	20.7	0.0	1.7	23.6	19.8	-3.8	-5.4	36.1	37.0	0.9	3.6
4	21.3	19.4	-1.9	-0.2	23.3	17.0	-6.3	-7.9	36.2	40.8	4.6	0.1
5	16.3	19.8	3.5	5.2	20.4	17.3	-3.1	-4.7	37.1	39.9	2.8	-1.7
Av. 2-5	18.7	18.7	0.0	1.7	21.3	17.8	-3.5	-5.1	37.2	38.7	1.5	-3.0

[a] Wage increase comparing post-unemployment wage with pre-unemployment wage; ignoring hourly wages below 200 Tolar and hourly wages above 1500 Tolar.

[b] Workers finding permanent jobs as percentage of all workers finding jobs.

[c] Probability of job loss within a year; ignoring right-censored spells of less than 1 year.

Table 3
Parameter estimates, log wage models [a)]

	Men	Women
a. Baseline estimate		
Effect of change in PBD	-0.016 (0.9)	0.030 (1.6)
Log(old wage)	0.268 (18.2)**	0.207 (14.4)**
Experience 2.5-5 yrs	0.045 (3.1)**	-0.006 (0.4)
Experience 5-10 yrs	0.053 (3.5)**	0.020 (1.3)
Experience 10-15 yrs	0.082 (4.7)**	0.029 (1.7)*
Experience 15-20 yrs	0.123 (5.8)**	0.006 (0.3)
Education2	0.017 (1.1)	0.023 (1.4)
Education3	0.051 (3.6)**	0.065 (3.8)**
Education4	0.114 (7.2)**	0.182 (10.3)**
Family1	0.009 (0.9)	-0.002 (0.3)
Family2	0.002 (0.2)	-0.003 (0.4)
Ill health	-0.009 (0.6)	0.010 (0.5)
Age/10	-0.024 (2.2)**	0.022 (2.2)**
After change of law	0.061 (3.7)**	0.039 (2.3)**
Constant	4.988 (49.4)**	5.252 (54.9)**
R^2	0.173	0.215
N	4176	4217
b. Effect of change in PBD group-specific		
6 to 3 months	-0.014 (0.6)	0.033 (1.4)
9 to 6 months	-0.009 (0.4)	0.015 (0.7)
12 to 6 months	-0.020 (0.9)	0.025 (1.1)
18 to 9 months	-0.021 (0.9)	0.049 (2.2)**
c. Effect of change in PBD group – sensitivity analysis		
c1. No wage limits	-0.041 (1.5)	-0.005 (0.2)
c2. Trimmed wage data	-0.026 (1.4)	0.019 (1.0)
c3. Accounting for selectivity	-0.016 (0.9)	0.030 (1.6)

[a)] In the estimates without wage limits and trimmed wage data, the estimates are based on samples of 5059 men and 5339 women; absolute t-statistics based on robust standard errors in parentheses; a ** (*) indicates significance at a 95% (90%) level.

Table 4
Parameter estimates, linear probability models [a)]

	Prob. of finding permanent job		Prob. job loss within 1 year	
	Men	Women	Men	Women
a. Baseline estimate				
Effect of change in PBD	0.046 (1.9)*	-0.045 (2.1)**	0.018 (0.6)	-0.029 (1.1)
Experience 2.5-5 yrs	0.004 (0.2)	0.022 (1.3)	-0.062 (2.9)**	0.007 (0.3)
Experience 5-10 yrs	0.034 (1.8)*	0.058 (3.2)**	-0.127 (5.5)**	-0.025 (1.1)
Experience 10-15 yrs	0.024 (1.1)	0.047 (2.3)**	-0.141 (5.4)**	-0.039 (1.6)
Experience 15-20 yrs	0.033 (1.3)	0.027 (1.1)	-0.204 (6.5)**	-0.074 (2.4)**
Education2	0.047 (2.2)**	0.077 (2.8)**	-0.070 (2.8)**	-0.091 (2.5)**
Education3	0.116 (5.7)**	0.157 (5.8)**	-0.163 (6.7)**	-0.124 (3.5)**
Education4	0.245 (11.6)**	0.224 (8.4)**	-0.255 (10.1)**	-0.179 (5.1)**
Family1	0.021 (1.6)	-0.033 (3.1)**	0.017 (1.1)	-0.016 (1.2)
Family2	0.021 (1.6)	-0.031 (2.9)**	-0.018 (1.2)	-0.027 (2.1)**
Ill health	-0.050 (2.4)**	-0.007 (0.3)	0.002 (0.1)	0.039 (1.1)
Age/10	0.013 (1.0)	0.046 (3.7)**	0.072 (4.5)**	0.054 (3.5)**
After change of law	-0.028 (1.3)	0.016 (0.8)	-0.019 (0.7)	0.044 (1.8)
Constant	0.025 (0.7)	-0.113 (2.6)**	0.504 (10.6)**	0.387 (6.9)**
R^2	0.040	0.028	0.025	0.009
b. Effect of change in PBD group-specific				
6 to 3 months	0.025 (0.9)	-0.018 (0.7)	0.016 (0.4)	-0.064 (1.9)*
9 to 6 months	0.027 (0.9)	-0.052 (1.9)*	0.036 (1.0)	-0.032 (1.0)
12 to 6 months	0.057 (2.0)**	-0.068 (2.6)**	-0.014 (0.4)	-0.002 (0.1)
18 to 9 months	0.070 (2.4)**	-0.040 (1.5)	0.039 (1.1)	-0.015 (0.4)

[a)] Probability of finding permanent job: 8627 men and 9074 women; probability of job loss within 1 year: 8441 men and 8751 women (right-censored durations within 1 year were excluded); absolute t-statistics based on robust standard errors in parentheses; a ** (*) indicates significance at a 95% (90%) level.

Table 5
Parameter estimates, job separation rates [a)]

	Men	Women
a. Baseline estimate		
Effect of change in PBD	0.01 (0.1)	-0.00 (0.0)
Experience 2.5-5 yrs	-0.13 (2.4)**	-0.04 (0.7)
Experience 5-10 yrs	-0.30 (5.1)**	-0.19 (3.4)**
Experience 10-15 yrs	-0.41 (6.3)**	-0.21 (3.3)**
Experience 15-20 yrs	-0.62 (7.8)**	-0.28 (3.6)**
Education2	-0.14 (2.2)**	-0.22 (2.5)**
Education3	-0.35 (5.5)**	-0.26 (3.0)**
Education4	-0.61 (8.9)**	-0.48 (5.5)**
Family1	0.03 (0.6)	-0.02 (0.6)
Family2	-0.11 (2.8)**	-0.08 (2.2)**
Ill health	-0.02 (0.4)	0.22 (2.8)**
Age/10	0.25 (6.2)**	-0.08 (2.1)**
After change of law	0.03 (0.4)	0.10 (1.5)
Duration dependence		
Month 2	0.12 (1.6)	0.29 (3.3)**
Month 3	0.48 (6.7)**	0.75 (9.6)**
Month 4	0.25 (3.2)**	0.52 (6.2)**
Month 5	0.13 (1.6)	0.69 (8.4)**
Month 6	0.21 (2.6)**	0.48 (5.6)**
Month 7-9	0.34 (5.3)**	0.14 (1.9)*
Month 10-12	0.24 (3.6)**	0.40 (5.4)**
Month 13-18	-0.51 (7.3)**	0.09 (1.2)
Month 18+	-0.72 (11.2)**	-0.31 (4.3)**
Constant	-6.78 (51.6)**	-6.82 (44.9)**
–Loglikelihood	41570.0	42017.3
b. Effect of change in PBD group-specific		
6 to 3 months	0.01 (0.1)	-0.14 (1.7)*
9 to 6 months	0.02 (0.3)	0.02 (0.3)
12 to 6 months	-0.07 (0.7)	0.07 (0.8)
18 to 9 months	0.08 (0.8)	0.07 (0.8)
–Loglikelihood	41568.6	42013.1

[a)] Samples of 8627 men and 9074 women; absolute t-statistics based on robust standard errors in parentheses; a ** (*) indicates significance at a 95% (90%) level.