

# Frontloading the Unemployment Benefit: An Empirical Assessment

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## Abstract

In November 2005, the Hungarian government frontloaded the unemployment benefit path, while keeping the total benefit amount that could be collected over the unemployment spell constant. We estimate the effect of this reform on non-employment duration using a regression discontinuity research design, where we exploit the sharp change in the benefit-path for those who claimed benefit after 1st of November 2005. We find that non-employment duration fell by 1.5 weeks after November 2005, while reemployment wages and the duration of new jobs remained the same. We show that the decrease in non-employment duration was large enough to make the benefit reform revenue neutral. Our welfare evaluation for this reform is positive: frontloading increased job finding, it made some of the unemployed better off, and did not cost anything to the taxpayers.

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# 1 Introduction

Unemployment insurance programs aim to protect against financial distress at job loss while maintaining incentives for jobs search. Unfortunately, these two goals are usually in conflict: an insurance instrument that provides better protection often distorts behavior and leads to longer unemployment spell.

The optimal level of unemployment insurance is determined by the empirical relevance of the insurance value and the moral hazard cost (Baily, 1978; Chetty, 2008), and the recent empirical literature made considerable progress in estimating these factors Krueger and Meyer 2002; Chetty and Finkelstein 2013; Schmieder and von Wachter 2016. However, most of the empirical work assumes that the level of UI benefits is constant throughout the unemployment spell or only exhibits a single step down (as for example in Chetty 2008).

A mainly theoretical strand of the literature, on the other hand, allows a more flexible benefit structure and focuses on the shape of optimal benefit path Shavell and Weiss 1979; Hopenhayn and Nicolini 1997; Cahuc and Lehmann 2000; Werning 2002; Shimer and Werning 2008. Changing the benefit path, in principle, can maintain the insurance aspects of UI and can provide more incentives to search for a job at the same time. For instance, consider a change that frontloads the benefit profile by raising the unemployment benefit by \$1 in the first period and by cutting it by \$1 in the second period. Under this benefit change, the short-term unemployed can collect more benefits, while the long-term unemployed collect the same amount of benefit throughout their unemployment spell. Therefore, benefit frontloading makes none of the unemployed worse off and it is likely to make some off them better off.<sup>1</sup>

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<sup>1</sup>Unemployed in the new system can replicate the old consumption profile by saving some of the extra dollars they got at the beginning of their unemployment spell. So the old budget constraint is feasible under the frontloaded

The potential downside of such a policy is that the total expenditure of the UI system might increase. Such an increase in costs would eventually increase taxes and make taxpayers worse off. However, the effect of frontloading on government spending is ambiguous. On the one hand, the cost of UI increases mechanically as some of the unemployed collect more benefits. On the other hand, frontloading might speed up the transition to employment which leads to less benefit pay-outs and more tax revenues. In principle, this behavioral response can be large enough to fully offset the mechanical cost increase caused by benefit frontloading.

Therefore, the benefit frontloading described here can lead to a win-win situation where some of the unemployed are made better off without making taxpayers worse off. However, it remains an empirical question whether cost savings from the elevated job search is large enough to offset the mechanical cost increase induced by the reform. This paper provides the first empirical assessment to answer this question. We exploit a unique Hungarian reform that changed radically the time profile of UI payments (see Figure 1). The unemployed who claimed benefit before 1st of November 2005 could rely on a constant benefit for 270 days. Those who claimed UI after November 1st were faced with a frontloaded benefit profile while the total UI benefit that the unemployed were eligible for remained almost the same.<sup>2</sup> . In particular, the benefit was around 50% higher in the first 90 days and then around 25% lower in the next 180 days.

We assess the effect of this unique policy change on non-employment duration using administrative data on UI claimants and social security contributions. We use a regression discontinuity research design, where we exploit that the benefit-path changes sharply at 1st of November 2005.

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benefit structure, and so a forward looking rational unemployed cannot be made worse-off. Moreover, we also show in the model section whenever frontloading implemented as a small deviation from a constant benefit profile even a hand-to-mouth unemployed will not be made worse-off.

<sup>2</sup>The total UI benefit that the unemployed were eligible for before the reform was only 3.5% higher than after it.

We show that the average non-employment duration was stable around 195 days preceeding the reform. However, non-employment duration drops sharply for those who claim benefit after 1st of November 2005. We estimate that non-employment duration decreased by 10 days, or 1.5 weeks after the reform. We conduct a number of robustness tests to verify that our results do not rely on any specific implementation design or functional form.

We also examine the effect of the benefit change on the quality of jobs found. We do not find any sharp change in reemployment wages or in the duration of new jobs. Therefore, our estimates suggest that the shortened unemployment duration did not lead workers to accept worse (or better) jobs.

We then translate the estimated effects into changes in the government's budget . The new benefit mechanically increased governmental spending, because short-term unemployed collected more benefits. However, 50% of this mechanical cost increase is offset by the lower spending on unemployment benefits caused by earlier exit from UI. In addition to that, unemployed who found a job earlier also paid more personal income tax and social security contributions. This latter channel offset another 70% of the mechanical cost increases, and so the behavioral response was large enough to counterbalance the mechanical cost increase caused by the reform. Moreover, the standard errors around the estimated budget consequences of the UI reform allows us to rule out that government's expenditure increased at the conventional significance levels.

These estimates highlight that the burden on taxpayers did not increase as a result of the reform. This suggests that benefit frontloading was a Pareto improving policy change in Hungary as short-term unemployed made better off by collecting more benefit; the long-term unemployed were unaffected as their total benefit remained the same, while the employed were also unaffected as the spending need of the UI system and taxes did not increase. Therefore, the change in benefit

path in Hungary broke the trade-off between moral hazard and insurance value: the new declining benefit profile not just kept generosity of the UI system , but alleviated moral hazard cost of the UI insurance as well.

The key assumption behind our empirical strategy is that there were no other policy or economic changes that affect non-employment duration simultaneous to the reform. The aggregate unemployment rate was stable in this period and the composition of the unemployed who claimed benefit was similar before and after the reform suggesting that economic changes do not explain the change in non-employment duration. The only important policy change that could affect our results is a voluntary reemployment bonus scheme (RB), which was introduced parallel with the benefit reform.

To separate the effect of the reemployment bonus scheme from benefit frontloading, we exploit the local variation in knowledge about the availability of the new bonus scheme similarly to Chetty et al. (2013). While UI offices provided clear and straightforward information to all newly unemployed about the level and the timing of their benefit, the availability of the reemployment bonus scheme was less salient. Moreover, the reemployment bonus scheme was quite complicated and it was also associated with substantial hassle costs as one needed to go through a burdensome administrative process to claim the benefit. Therefore, the role of local UI offices was crucial to advocate the scheme and to help claiming it.

We infer the unemployed access to information from the average bonus take-up rate at the local UI office where the benefit was claimed. There are a large and persistent differences in take-up rates across UI offices that are not related to observable characteristics of the unemployed. In some locations the take-up rate was close to zero, while in others it was above 10%. We show that the size of the drop in non-employment duration after the reform was very similar in zero or very

low take-up and high take-up locations. This suggest that access to information on the voluntary RB scheme is unlikely to have had any significant effect on non-employment duration.

This paper is related to the literature on estimating moral hazard implications of unemployment insurance. Numerous studies scrutinized the effect of changing the benefit level (e.g. Meyer 1990; Lalive et al. 2006; Landais 2015; Card et al. 2007) and most papers found that there is a considerable effect of unemployment benefits on job search behavior (see a survey of this literature by Krueger and Meyer 2002; Chetty and Finkelstein 2013; Schmieder and von Wachter 2016). Other aspects of unemployment insurance systems have been examined, such as reemployment bonuses (Van der Klaauw and Van Ours, 2013) and enforcement (Van den Berg and Van der Klaauw, 2006; Cockx and Picchio, 2013). However, the empirical evidence on the effect of changing the benefit path is surprisingly limited. A notable exception is Kolsrud et al. (2015), who empirically estimate the moral hazard costs of unemployment benefits paid at different times during the unemployment spell. They find that the unemployed respond more to benefit changes at the beginning of the UI spell than towards the end. Our results imply the opposite: the effect of increasing the benefit at the beginning has a smaller effect than the decrease later on. One possible explanation for this discrepancy is that the reform in Hungary is more radical and more salient than the one analyzed in Kolsrud et al. (2015). In our setup, therefore, the unemployed are more likely to be aware of future drops in their benefits and so they will respond more to them.

Our results also contribute to the extensive theoretical literature on the optimal time profile of unemployment insurance (e.g. Shavell and Weiss 1979; Hopenhayn and Nicolini 1997; Cahuc and Lehmann 2000; Werning 2002; Shimer and Werning 2008). These papers derive the fully optimal UI profile but they need to make strong assumptions about the environment in which the unemployed make their decisions (e.g. borrowing constraints). Unfortunately, the optimal

UI profile is very sensitive to these assumptions (Hopenhayn and Nicolini, 1997; Werning, 2002). Moreover, the fully optimal benefit schedule is often quite complicated and hard to implement. Therefore, instead of searching for the fully optimal UI benefit schedule, we look at the welfare implication of an easily implementable reform that moves away from the standard constant benefit schedule to a frontloaded one. Our approach will not come up with the first-best benefit profile, but may help to inform policy makers as to which direction they should deviate in order to find it.

We also contribute to the effect of unemployment insurance on job quality. Recent research shows mixed results on the effect of changing the UI benefit-level on reemployment wages and on the tenure at the new job (Schmieder et al., 2016; Nekoei and Weber, 2015). Our key contribution to this literature is that our Hungarian reform allows us to consider the effect of changing the benefit path on job quality. Similarly to Card et al. (2007), Lalive (2007), Johnston and Mas (2016) and Van Ours and Vodopivec (2008) we do not find evidence for the effect of UI benefit profile on job quality.

Finally, this paper is also related to DellaVigna et al. (2017) that exploits the same reform to better understand job search behavior. In DellaVigna et al. (2017) we contrast the predictions of the standard search models to a model of job search with reference-dependent preferences and show that the latter model performs better in explaining the observed hazard rates in the data. While the DellaVigna et al. (2017) paper's main focus is to structurally estimate different job search models, our aim in this paper is to provide compelling estimates on the effect of the reform on outcomes that are the primary interest to policy makers and to the literature on optimal unemployment insurance (Chetty and Finkelstein 2013; Schmieder and von Wachter 2016). Therefore, we focus here on non-employment duration, job quality alongside the effect on the government budget.

The paper is set out as follows. Section 2 describes the data and institutional details of the

unemployment insurance reform. Section 3 presents the empirical results. In Section 4 we use our empirical estimates to assess the welfare implications of reform. Section 5 concludes.

## 2 Institutional Background and Data

### 2.1 The Benefit Reform in Hungary

Hungary had a two-tier unemployment insurance system around 2005. In the first tier the unemployment benefit depended on the length and amount of contributions<sup>3</sup>. After exhausting the first tier, the unemployed were eligible for additional unemployment assistance. The amount of the benefit in the second tier was the same for all unemployed and the duration depended on the age of the UI claimants. After both tiers were exhausted, the unemployed were eligible for welfare payment in perpetuity. However, welfare payments, unlike the UI benefit, depended on family income and were generally lower than the unemployment benefit.

The UI reform in 2005 changed the benefit schedule dramatically in the first tier for those who claimed benefit after November 1st, 2005, while it preserved the length of unemployment benefit. In the Appendix Figure A-1 we show the new and old benefit schedule in relation to the UI base. In our analysis we concentrate on the unemployed who experienced a particularly interesting benefit path change as a result of the reform. These unemployed are individuals who worked more or less uninterrupted in the preceding four years of their job loss and whose (monthly) earnings base was above HUF108,000 (\$504) in 2005 (around the 70th percentile of UI claimants). Figure 1 summarizes the benefit path for this group before and after the reform. Unemployed individuals

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<sup>3</sup>The length of eligibility was the number of working days in the last four years divided by 5 and it was capped at 270 days. The amount of the benefit was based on the average monthly taxable income in the last year before unemployment and it was also capped.

who claimed benefits before November 1st were eligible for HUF44,460 (\$222) for the first 270 days. Those who claimed benefit after November 1st got HUF68,400 (\$342) in the first 90 days and HUF34,200 (\$171) in the next 180 days. An important feature of the reform was that the total UI benefits paid out throughout the unemployment spell remained approximately the same<sup>4</sup> and only the timing of the benefit payouts changed.

Newly unemployed individuals who wished to collect unemployment benefits had to go to the local UI office and attend a 30-minute session which explained their rights and obligations as a claimant. Then each individual received a personalized letter which characterized their benefit schedule in the first tier. Figure A-2 shows an example of the first page of such a letter for an unemployed individual who claimed benefit under the new rules. The benefits are highlighted in the table in the middle of the page, where the length of the disbursement period in days and the daily amount are shown. This letter made salient the benefit schedule from the beginning of the unemployment spell.

There were two other changes that were implemented in 2005. First, unemployment assistance (UA- the second tier) was shortened from 180 days to 90 for those who claimed benefit after February 5th, 2005. Second, the government introduced a voluntary reemployment bonus (RB) scheme in parallel with the benefit reform. Under this new scheme, the unemployed who claimed benefit after November 1st, 2005 and found a job in the first 270 days could claim 50 percent of the remaining unemployment benefit as a lump sum. The take-up rate of the RB scheme was very low as only 6 percent of the unemployed took advantage of this new scheme. Claiming UI benefit had two important drawbacks. First, the default option was not to take up RB and if someone

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<sup>4</sup>The total UI benefit was 3.6% in the new system.

decided to make use of it, she had to go through a complicated administrative process<sup>5</sup>. Second, claiming RB also meant that the remaining benefit eligibility was lost. Therefore, RB claimants had to start to collect benefit eligibility from zero again, and this may have seemed a risky step to take for many newly employed worker on probation. In Section 3 we do a couple of robustness checks to show that the changes in non-employment durations were unlikely to be driven by the shorter UA benefits in the second tier or by the voluntary RB scheme.

Finally, it is worth highlighting that the economy was growing at 3-4% before the reform and a somewhat lower rate afterwards (see Figure A-3 Panel a). Nevertheless, aggregate labor market conditions were not affected by the lower performance of the economy and unemployment was stable around the analysis period (see Figure A-3 Panel b).<sup>6</sup>

## 2.2 Database and sample definition

We observe a 50 percent random sample of the unemployed registered by the Hungarian National Employment Service between March 2004 and 2008<sup>7</sup>. During this time period we have information on benefit for which one is eligible and the starting and ending date of benefit provision. We also observe employment history and the earnings from social security contributions between 2002 and 2008.

We restrict attention to prime age workers (25-49 years) who had 270 days benefit eligibility. To analyze the effect of the reform, we compare the average length of benefit duration before

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<sup>5</sup>RB could only be claimed in person at the local unemployment office when 270 days elapsed after the benefit claim. Moreover, the employment status had to be continuous between the reemployment and the RB claim.

<sup>6</sup>The lower GDP growth rate would predict that non-employment duration is higher after the reform. However, in Section 3 we show that the average length of employment was in fact lower after the reform. Therefore, if the change in GDP had some effect on our results, then we are likely to underestimate the “true” effect of the reform.

<sup>7</sup>The sample includes individuals who were born every other day after January 1st, 1927.

and after the reform. As figure Figure 1 shows, the before group consists of the unemployed who claimed benefit between 15th March 2004 and 31st October 2005. The after group is made up by the unemployed who claimed benefit between November 1st, 2005 and August 15th, 2007. We <sup>8</sup> .

The basic descriptive statistics are shown in Table 1. We observe approximately 15000 unemployed both before and after the reform. The observable characteristics of the two groups are very similar. The share of women and the average year of education is slightly larger in the after sample but the average income before unemployment was the same in both groups. The average time spent between job loss and benefit claim was 31 days both before and after the reform, which indicates that people who lost their jobs before the reform did not postpone their benefit claim to become eligible for the new benefit schedule. Finally, less than 6 percent of the unemployed claimed reemployment bonus.

### 3 Results

In this section we evaluate the impact of the reform on non-employment duration and on the quality of jobs found. Figure 5 shows the Kaplan-Meier survivor rate for those who claimed benefit before (between March 15th, 2004 and October 31st, 2005) and after the reform (between November 1st, 2005 and August 15th, 2007). In the first 90 days, the two job survivor functions are very similar. After 3 months the job finding rate of the after group rises compared to the before group. As a result, a significantly higher share of workers finds a job during the first 270 days after the reform than before the reform.

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<sup>8</sup>The results are robust to leaving out workers who claimed benefit around 1st of November. So workers who postponed their benefit claims in order to get into the new system does not bias the results.

To estimate the effect of the reform on the length of unemployment, we estimate the following regression:

$$NonEmpDur_i = \alpha + \beta after_i + \gamma X_i + \varepsilon_i \quad (1)$$

where the dependent variable shows the time elapsed between benefit claim and re-employment. We cap the length of unemployment at 270 days because the reform affected the benefit eligibility only in the first 270 days. However, capping at a higher level does not substantially change the results. The main variable of interest is the  $after_i$  dummy which indicates whether the unemployed individual claimed benefit after the reform.  $X_i$  denotes the control variables that include age, age square, years of education and its square, log income in 2002, log income in 2003, sex, dummies that control for the day of the month the benefit was claimed, one digit occupation and location dummies.

We also re-estimate the effect of the reform using regression discontinuity design to test the robustness of our identification strategy. By using this approach we only consider unemployed in our sample who claimed benefit around the benefit reform. More specifically we follow Lee and Lemieux (2010) estimate the following equation:

$$NonEmpDur_i = \alpha + \beta after_i + f(T_i) + \varepsilon_i \quad (2)$$

Similarly to equation 1, the dependent variable is the length of unemployment (caped at 270

days) and the explanatory variables are the  $after_i$  dummy and individual level control variables. The main difference is that we only use a short time window around the reform and we also control for the date of the benefit claim with a flexible non-linear function<sup>9</sup> and . One possible threat to the identification is that the benefit claims which are farther from the benefit reform may bias the estimation if  $f(T_i)$  is incorrectly specified. To avoid these kind of difficulties we use the optimal bandwidth proposed by Imbens and Kalyanaraman (2011).

Table 2 summarizes the main findings of the paper. According to Column 1, the length of non-employment decreased by 11.17(s.e. 1.10) days after the reform. In Column 2 we take into account the fact that the characteristics of UI claimants differ slightly before and after the reform. The results show that the decline in duration is even bigger now: 12.42 (s.e. 1.09) days. In Column 3 we also control for the the time of benefit claim by adding 3rd order polynomial of the date of benefit claim at each side of the benefit reform . Our results are robust against the inclusion of flexible time trends. The estimated effect of the reform is somewhat larger now (12.46 s.e. 4.37) and statistically not different from the previous columns.

The regression discontinuity estimates in Column 4-6 show similar results. We use the optimal bandwidth of Imbens and Kalyanaraman (2011) in this regression and only consider 6440 unemployed who claimed benefit maximum 4 month before or after the reform. The estimated partial effects are less precise in this case but they are significantly not different from the results using the whole sample. The estimated effect of the reform is 10.15 (s.e. 2.,0). In Column 5 and 6, we add separate linear and 3rd order polynomial trend before and after the reform. The estimated partial effect further increases (18.16 and 21.50) but these point estimates are statistically not different

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<sup>9</sup>In the linear case we estimate a separate linear time trend before and after the reform. In the polynomial case, we estimate different 3rd order polynomial time trend before and after the reform .

from the previous results.

As robustness check for the functional form we also estimate the effect of the reform using the Cox proportional hazard model:

$$h_d = \delta_d \exp(\lambda \text{after}_i + \kappa X_i) \quad (3)$$

where  $h_d$  denotes the re-employment hazard  $d$  days after the benefit has been claimed,  $\delta_d$  is an unrestricted day effect (baseline hazard), and the control variables,  $X$ , are the same as in equation 1. The Cox hazard model shows similar effects. According to the right panel of Table 2, the reemployment hazard increased with 17-20 percent after the reform and the inclusion of control variables do not significantly alter the point estimates. The results does not change significantly if we use only the only the optimal bandwidth of Imbens and Kalyanaraman (2011). The unconditional increase in the job finding hazards is exactly the same on the full sample and on the restricted sample. Similarly to the linear case, the addition of flexible time trends increases the point estimates and the standard errors as well.

Our estimates indicate that after the reform, non-employment duration was lower by 10-12 days. Figure 3 panel (a) plots the average length of non-employment by two month periods relative to the benefit change. The gap shows that non-employment duration was around 197 days in the preceding 6 month periods and that has been dropped to 187 days immediately after the reform. In Figure 3 panel (b) we show the average non-employment duration after controlling for observables and location fixed effects. Again the the change in non-employment duration is very much coincided with the implementation of the new benefit schedule. The figures also highlight

that the average length of non-employment was very similar in the last 18 month before the reform. Therefore, the change in the second tier after February 5th, 2005 had at most a small effect on non-employment duration. Given that the benefit level in the second tier is quite low (HUF22,800 or \$114 per month) this is not surprising.

*How did the composition of unemployed changed over time?* A crucial assumption of our identification strategy is that the composition of unemployed did not change over time. E.g. the unemployed who can find job faster are not overrepresented after the reform. To test this hypothesis, we calculate how the expected length of unemployment changed over time based on observable characteristics. First, we use the before sample to regress the individual level characteristics on the actual length of unemployment (capped at 270 days). Second, we use the estimated parameters to predict the expected length of unemployment for every individual. Finally, Figure 4 plots the average predicted length of unemployment by two month periods. The figure clearly shows that the reform did not affected the selection of unemployed based on observable characteristics: the expected length of unemployment is around 198 days, it is constant over time and there is no break at the timing of the reform either.

### **3.1 The effect of the reform on job quality**

*Did the faster reemployment hurt job quality?* To answer this question we analyze other outcomes besides the non-employment. duration. For example worker may accept a less stable job after the reform to exit unemployment earlier (*Jarosch, 2014*). In Table 3 Column (1) to (3) we estimate equation 1, where the outcome variable is the tenure at the new job. Similarly to the previous

tables, in Column (4) to (6) we only consider unemployed who claimed benefit maximum 4 month before or after the reform. All columns show a small effect on job tenure (e.g. less than 5 days in Column 3) which are negligible in statistical and economic sense. Similarly to Table 2, the estimated partial effect turns to imprecise if we control for for separate 3rd order polynomial before and after the reform. The lack of effect on job tenure at the new job has been also confirmed in Figure 6 where we plot the average tenure by two month periods relative to the benefit change (in Panel a without controls in Panel b with controlling for observables and UI location fixed effects).

Figure 7 shows that frontloading did not affect the reemployment wages either<sup>10</sup>. We plot the log-ratio of the reemployment wage and the unemployment benefit base wage by six month periods.<sup>11</sup> We control for a linear time trend to rule out the effect of the inflation and economic growth. The figure shows that the average reemployment monthly wage is 46-48 log-point lower<sup>12</sup> As the unemployment benefit base wage calculated based on the average earnings in the last four years, this measure overestimates the income loss after unemployment (Card et al., 2007; Schmieder et al., 2016). In any case, Figure 7 highlights that reemployment wages are not affected around the time of the unemployment benefit reform.

Table 4 Panel A reports the point estimates for the log-ratio of the reemployment wage and the base wage. According to Column 1, the reemployment wage ratio was identical before and after the reform; the estimated effect of the reform is 0.3 percent (s.e. 1.46 percent) increase in

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<sup>10</sup>We calculate the daily reemployment wage from the social security data by dividing the monthly earnings by the number of days worked in that month.

<sup>11</sup>The unemployment benefit base wage was calculated by the unemployment insurance office based on the average (daily) wage in the last four years. The unemployment benefit base wage was not affected by severance payment, which was 1 to 6 months' salary depending on the tenure. The average daily wage calculated from the social security data also include severance payments. This means that the log-ratio of the reemployment wage and the wage in the last job overestimates the true wage loss for those who received severance payments.

<sup>12</sup>This difference is equivalent to a 37 percent decrease.

reemployment wages. The point estimates increases up to 6 percent (s.e. 2.49) once we control for observable characteristics of the unemployed but the point estimate drops to 2.9 percent (s.e. 4.0) if we add separate 3rd order polynomial trend before and after the reform. We cannot identify the effect of the reform on reemployment wages using the the optimal bandwidth of Imbens and Kalyanaraman (2011). The point estimates are between 2.9 and 9.4 percent but these estimates are not statistically different from zero at five percent significance level..

While these point estimates are significant in economic terms, we should be cautious in drawing strong conclusions. First, most of these estimates are statistically insignificant at the conventional levels and the results are very sensitive on the inclusion of flexible time trends. Moreover, as it has been shown in Figure 7, the timing of the increase in reemployment wages does not perfectly align with the implementation of the reform.

In Table 4 Panel B and C we also explore alternative definitions of reemployment wages. Results with log-ratio of the reemployment wage and the wage in the last job are shown in Panel B. The results are similar to the results in Panel A: most of the point estimates are positive but only one estimate differs statistically from zero at 5 percent level. However, the wage in the last job is also is affected by severance payments, and so these estimate might be biased. Therefore, in Panel C we show the results for log-ratio of reemployment wage and the average wage in 2002. Again, the point estimates tend to be positive and imprecisely estimated.

Overall, these results suggest that the effect on reemployment wages might be positive or zero, but it is unlikely to be negative. Therefore, we find no evidence that the reform hurt job quality.

## 3.2 Robustness to reemployment bonus

*Can reemployment bonus explain the decrease in non-employment duration?* As we discussed in the previous section, those who claimed benefit after November 1st, 2005 was not only faced with the frontloaded benefit schedule but were also eligible to claim voluntary reemployment bonus if they found a job within 270 days. The reemployment bonus was associated with substantial hassle costs and it was a less salient policy than the benefit frontloading. Still, it is possible that the parallel introduction of the reemployment bonus explains part of the decline in non-employment duration. To separate the effect of benefit frontloading from the reemployment bonus, we exploit the anecdotal evidence that at some local UI offices the reemployment bonus was advertised more by UI officials than at other ones. While we do not observe directly which UI offices have been more keen on advocating the reemployment bonus scheme, we use the local level take-up rate of the reemployment bonus as a proxy for information provided to the unemployed.

Two empirical observations motivate that the take-up rate is related to access to information and not to other factors. First, Figure 8 panel (b) shows a scatter plot between the take-up rate one year after the reform and the take-up rate 2 years after. The figure uncovers a strong correlation (0.64) between take-up rate in the two periods. Therefore, the take-up rate differences across locations are persistent. Second, and more importantly, Figure 9 shows scatter plots between different measures of the composition of the unemployed and the take-up rate by UI locations. Panel (a) measures the composition of the unemployed by the average *pre-reform* non-employment duration. We use the *pre-reform* non-employment duration and not the *post-reform* one, because the post-reform does not just measure the composition of the unemployed but the effect of the

reemployment bonus as well.<sup>13</sup> Figure 9 Panel (b) measures the composition of unemployed by the predicted non-employment duration for those who claim benefit after the reform. To get the predicted values we run a regression of non-employment duration on observable characteristics (age, age square, years of education and its square, log income in 2002, log income in 2003, sex, dummies that control for the day UI claimed, one digit occupation) in the pre-reform sample and predict the average non-employment duration for the post-reform.

Both Panel (a) and Panel (b) in Figure 9 depicts the Kernel-weighted local polynomial smoothing to show the non-parametric relationship between composition and take-up rate. In both panels we see no relationship between these two variables if we abstract away from the few outliers with very high take-up rates. This indicates that the reemployment bonus take-up rate is persistently higher at some locations and the differences are not related to the composition of the unemployed. This empirical pattern across UI locations is what we would expect to emerge if the take-up rate was determined by the behavior of local UI officers and not some underlying economic factors.

The effect of reemployment bonus on non-employment duration is likely to vary by the access to information on the scheme. Similarly to Chetty et al. (2013), the variation in access to information across locations can be used to better understand how reemployment bonus affects our baseline results. To do that, we compare low take-up rate (limited information) and high take-up rate (more information) locations that experienced differences in non-employment duration. In particular, we estimate the following regression:

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<sup>13</sup>The measure of pre-reform non-employment duration is a good proxy of the post-reform composition of the unemployed if the composition is stable over time. The correlation in non-employment duration between 1 year before and 2 years before the reform is 0.31. Moreover, with all the caveats of using *post-reform* non-employment duration to measure the composition of the unemployed, it is worth highlighting that there is no relationship between non-employment duration and reemployment bonus take-up rate in the post-reform sample (results available on request).

$$unemployment_i = \beta_1 + \beta_2 after_i + \beta_3 high_i + \beta_4 high_i * after_i + \gamma X_i + \varepsilon_i \quad (4)$$

where the dummy variable  $high_i$  takes the value of 1 if the location is above the median (average take-up rate is 12.5%) and 0 if the location is below the median (average take-up rate is 1.29%) with respect to the reemployment bonus take-up rate<sup>14</sup>. While this is a common difference-in-difference type regression, our main parameter of interest is not  $\beta_4$ , namely the effect of reemployment bonus on non-employment duration, but  $\beta_1$ , the effect of the reform on non-employment duration at locations with close to zero take-up rate and limited information access.

Column (1) to (4) in the first panel of Table 5 summarizes the estimation results. In Column (1) and (3) we saw the baseline results for the sample that includes the lowest and highest quartile locations with low reemployment bonus take-up rate. The point estimates are slightly different here than in the baseline Table 2 (-11.14 vs. -11.17 in the specification with no control and -11.09 vs. -12.46 in the specification with controls and pairwise 3rd order polynomial trend) but the differences are not statistically significant. In Column (2) and (4) we show the results on the same sample but estimate equation 4. The results show that the effect of the after dummy is two days lower in with and without control as well but the differences are statistically not different. According to our results the reform has a 3.7-4.5 days larger effect at locations where the take-up rate was higher than the median. The results using regression discontinuity design show qualitatively similar results (Column 5-8) but the point estimates are more noisy due to the smaller sample size. The point estimates for locations with below median bonus take up rate are somewhat smaller than the average effect of the reform and the interaction term is insignificant.

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<sup>14</sup>We omit locations where we observe less than 30 reemployment claims to measure the bonus take-up more precisely.

As a robustness check, we report the estimates using Cox proportional hazard models. The results are presented in Table 5 Panel B. Each column in Panel B mimics the corresponding column in the upper panel. Similarly to the linear specifications, the estimated effect of the reform at low bonus take-up rate locations are very close to the total effect of the reform. The Cox-model shows also significant geographical heterogeneity in the effect of the benefit reform: The reemployment hazards increased 7.1-7.8 percentage more at locations where the share of reemployment bonus claims were larger than the median.

Using the interaction term of equation 5, we can compute the total effect of the reemployment bonus. The average take-up rate of the reemployment bonus was 6 percent while the difference in take up rate between low and high take-up rate locations was 11.2 percent. This suggests that the average effect of reemployment bonus is approximately  $3.7/11.2*6=1.98$  days which corresponds to a  $7.1/11.2*6=3.8$  percent increase in reemployment hazards. As this point estimates are a magnitude lower than the total effect of the reform, we conclude that our results were not driven by the introduction of reemployment bonus. .

We also estimate the effect of the reemployment bonus by using directly the share of unemployed who claimed reemployment bonus.

$$unemployment_i = \beta_1 + \beta_2 after_i + \beta_3 takeup_i + \beta_4 high_i * takeup_i + \gamma X_i + \varepsilon_i \quad (5)$$

where  $takeup_i$  measures the share of workers who claimed reemployment bonus. Again, the main variable of interest is  $\beta_2$  which measures the predicted effect of the reform if no one had claimed reemployment bonus. Similarly to equation 4,  $\beta_4$  estimates the effect of the reemployment bonus on the average length of unemployment. We only consider the full sample at this exercise

as we cannot precisely measure the share of actual take-up rate on the location level if we use the optimal bandwidth of Imbens and Kalyanaraman (2011)<sup>15</sup>

Table A-2 summarizes the results. According to Column 2, the predicted effect of the reform without reemployment bonus is 9.28 days (s.e. 1.15) which is 1.83 days lower than the estimated total effect of the reform in Column 1. The interaction term means that one percentage point increase in the take-up rate of the reemployment bonus decreases the average length of unemployment with 0.34 days. As the average take-up rate was 6 percent, this means that the total effect of the reemployment bonus is approximately  $6 \times 0.34 = 2$  days. The results remain similar if we control for composition effect and 3rd order polynomial trend. Here the estimated effect of the reform without reemployment bonus is 8.87 days (s.e. 4.92) and one percentage point increase in reemployment bonus take-up decreases the average length of unemployment with approximately 0.38 days (s.e. 0.11).

In Figure 10 we plot the relationship between the before-after change in non-employment duration and take-up rate across locations. We also plot the Kernel-weighted local polynomial smoothing to show the non-parametric relationship between these two variables. The figure supports our regression results in Table 5: there is no strong relationship between the effect of the reform on non-employment duration and the take-up rate.

The results presented here underline that access to information (measured by variation in take-up rate) on the reemployment bonus does not affect extensively the estimates in non-employment duration. This is not surprising given that the reemployment bonus scheme was a very complicated, non-salient policy with some substantial drawbacks, such as losing the remaining benefit eligibility if claimed. Therefore, our estimates indicate the the effect of the reemployment bonus was negli-

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<sup>15</sup>Using a 4 month time window after the reform, the average observations per location is less than 25 unemployed.



shows that an unemployed individual who finds a job quickly collects more benefit under the new system and this mechanically increases the government spending on UI. The second term captures the budget consequences of the behavioral responses to the reform: due to faster reemployment, spending on UI decreases and tax revenues increase. It remains an empirical question whether the mechanical or the behavioral effect has a larger influence on the budget.

Table 6 summarizes the key effects of the reform on the budget. It shows that in the absence of behavioral responses, benefit frontloading would have increased mechanically benefit payments by \$118. However, benefit frontloading sped up reemployment, which decreased spending on UI benefits by \$65. Moreover, finding jobs earlier also lead to higher UI contributions, which is equivalent to an additional \$9.

{As noted by Lawson (2014) and Nekoei and Weber (2015), this is likely to understate the budget shortfall in practice, since workers pay additional taxes on earnings to finance other government expenditures. from Schmider et al} From the government point of view, revenues outside the UI budget should also be taken into account. The wage related taxes and contributions paid because the unemployed find jobs quicker increased the revenue of the budget with an additional \$98.

To sum up, the mechanical increase of UI expenditures were \$118 while the behavioral response of the unemployed improved the balance of the budget by \$172, which suggests that frontloading improved the budget by \$54 per unemployed. We also calculated the standard errors around these estimates by bootstrapping and the gain of the government is significantly different from zero at every conventional significance level.<sup>16</sup>

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<sup>16</sup>We take 1000 random sample with replacement, then calculate the Kaplan-Meire survival rates and the implied UI budget.

## 4 Welfare Assessment

Our estimates in the previous section can be used to assess the welfare implications of frontloading. We use the stylized job search model of Chetty (2008) and Kolsrud et al. (2015) to highlight the key channels through which benefit frontloading affect welfare.

### 4.1 Set-up

We consider a discrete time model of job search in which agents live for  $T$  periods. The representative agent starts as unemployed and searches for jobs in each period. Employment is an absorbing state<sup>17</sup>, and so once a job is found, the unemployed will be employed at wage  $w$  for the rest of her life.<sup>18</sup>

In each periods agents make two decisions: they choose search intensity  $s_t$  and consumption level  $c_t$ . Search intensity is costly and these costs are represented by  $c(s_t)$ . We assume that the cost function is convex, strictly increasing and twice differentiable. The value function of the employed if  $t < T$  is

$$V_t^E(A_t) = \max_{A_{t+1}} u(c_t^e) + v(G) + \delta V_{t+1}^E(A_{t+1}),$$

where  $\delta$  is the discount factor, and  $V_T^E(A_t) = \max_{A_{t+1}} u(w + A_T) + v(G)$ . The value of employment depends on private consumption,  $u(c_t^e)$ , and on the consumption of public goods  $v(G)$ . Both  $u()$  and  $v()$  are strictly increasing, concave, twice differentiable functions. Assets earn a return

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<sup>17</sup>Relaxing this assumption complicates the calculation of the value of employment, but the main conclusions of this section are not affected.

<sup>18</sup>We assume that the change in benefit profile does not affect reemployment wages, which is confirmed by our empirical analysis in Section 3.

$r$  per period so that consumers face a per-period budget constraint  $c_t^e = w + A_t - \frac{A_{t+1}}{1+r}$  and a borrowing constraint  $A_t \geq L$ .<sup>19</sup>

The value function of the unemployed if  $t < T$  is

$$V_t^U(A_t) = \max_{A_{t+1}, s_t} u(c_t^u) - c(s_t) + v(G) + \delta [s_t V_{t+1}^E(A_{t+1}) + (1 - s_t) V_{t+1}^U(A_{t+1})],$$

where  $c_t^u = b_t + A_t - \frac{A_{t+1}}{1+r}$  and  $V_T^U(A_t) = u(b_t + A_T) + v(G)$ . Again the value of employment depends on public and private consumption.

Spending on the unemployment insurance system depends on the fraction of agents that stay unemployed at period,  $S_t$ , and the benefit paid out to these workers,  $b_t$ . The total unemployment benefit payout equals  $\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t S_t b_t$ . The tax that can be collected depends on the fraction of workers who are employed,  $1 - S_t$ , and on the tax rate,  $\tau$ <sup>20</sup>. Finally, the government spends  $G$  on public goods and so the government deficit,  $D$ , is defined by the following formula:

$$D = \sum_{t=1}^T \left(\frac{1}{1+r}\right)^t G + \sum_{t=1}^T \left(\frac{1}{1+r}\right)^t S_t b_t - \sum_{t=1}^T \left(\frac{1}{1+r}\right)^t (1 - S_t) \tau w.$$

We assume that the deficit must be kept constant, and so more spending on unemployment insurance (while keeping constant the tax revenue), will decrease the amount of public goods provided in the economy.

The UI benefit was constant before November 1st, 2015 and so  $b_t = b$ .<sup>21</sup> The Hungarian reform increased the benefit by  $\widetilde{\Delta}b$  in the first  $N$  periods and decreased by  $\underline{\Delta}b$  afterwards, while the total

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<sup>19</sup>The presence of borrowing constraints does not affect our results.

<sup>20</sup>We also include also social security contributions in taxes, because the link between contributions and future benefits is very weak for most workers (Summers, 1998).

<sup>21</sup>If the interest rate,  $r$ , is positive, then this benefit path is slightly declining in present value terms.

benefit that can be collected throughout the unemployment spell remained constant, formally,

$$\sum_{k=0}^N \Delta b_1 + \sum_{k=N+1}^T \Delta b_2 = \sum_{k=1}^T \Delta b_k = 0. \quad (7)$$

Notice that we require here that the total benefit is kept constant in nominal terms and not in present value terms. These two differ if the interest rate,  $r$ , is positive. We make this assumption to stick to the exact reform that occurred, however, the results are unaffected if the present value of the total benefit is kept constant instead.

## 4.2 Welfare implications

The value of unemployment at period 0 captures the expected utility of a newly unemployed agent. We examine the effect of benefit change on this measure to understand the welfare implications of frontloading.

Proposition 1. Suppose that the unemployment benefit is increased by  $\widetilde{\Delta b}$  in the first  $N$  periods and decreased by  $\underline{\Delta b}$  afterwards, while the total benefit that can be collected throughout the unemployment spell remained constant and so equation 7 applies.

Then the effect of benefit change on the value of unemployment at the beginning of the UI spell is determined by the following formula:

$$\Delta V_0^U(A_0) = \underbrace{u'(c_0^{u*}) \Delta b_0 + \sum_{k=1}^N \delta^k \prod_{i=1}^k (1 - s_i^*) u'(c_k^{u*}) \Delta b_k}_{\substack{\text{welfare effect caused by} \\ \text{change in the benefit} \\ \geq 0}} \quad \underbrace{- \sum \delta^k v'(G) \Delta G}_{\substack{\text{welfare effect caused by} \\ \text{change in public spending} \\ \leq 0}}$$

The first part of this expression, welfare effect caused by change in the benefit, is always non-zero and it becomes positive if optimal search  $s_t^*$  is positive for at least one period throughout the unemployment spell or if the interest rate,  $r$ , is positive. Moreover, the second part of this expression, the welfare effect caused by the change in public spending, can be positive, negative or zero depending on the sign of  $\Delta G$ . This  $\Delta G$  is the following:

$$\Delta G = \underbrace{\frac{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t S_t \Delta b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease} \\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t \tau}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax revenue} \\ \text{caused by finding job sooner}}}$$

Proposition 1 highlights that the benefit change induced by frontloading increases the welfare of the unemployed by increasing private consumption. This is because under the new UI benefit schedule the consumption profile under the old rules can be replicated by saving the benefit increase in the first  $N$  periods and consuming them later. The new benefit schedule, therefore, must provide at least as high consumption utility as the old one, and as the proposition highlights, under some weak conditions it will be strictly higher.

However, the new benefit schedule can increase the funding need of the UI system, which can lead to cutting back spending on public goods,  $\Delta G$ . In principle, lowering public goods can offset the welfare gain caused by the consumption increase of the unemployed, but this is not necessarily the case. Proposition 1 shows that the effect on public spending is ambiguous and determined by three different factors. First, benefit frontloading mechanically increases the spending on UI, because the unemployed individuals who find jobs relatively quickly collect more

benefits under the new rule. Second, a sizable decline in non-employment duration decreases spending on UI benefits. Third, unemployed individuals who find jobs quicker pay more taxes and increase government revenue. While the first effect increases the cost of the unemployment insurance system, the latter two effects decrease it. It remains an empirical question, therefore, which of these effects dominates.

The results in Section 3.1 calculate the change in  $\Delta G$  and show that in the Hungarian case the behavioral responses were large enough to offset the mechanical cost increase in the UI. This implies that, in fact,  $\Delta G$  in fact increased and not decreased after the reform. Therefore, the Hungarian benefit change was clearly welfare improving, because not only did it increase private consumption of the unemployed, but it also saved some money for the government.

It is worth highlighting that the result presented in Proposition 1 is very robust to alternative modeling assumptions. The presence of borrowing limits, unobserved heterogeneity among the unemployed, or hand-to-mouth consumers do not influence the welfare implications presented here.

## 5 Conclusion

This paper presented the Hungarian unemployment benefit reform where a new frontloaded benefit path replaced the flat benefit system. The virtue of the reform was that the timing of the benefit was changed while the total amount of the benefit that could have been collected stayed constant. We provided evidence that benefit frontloading speeded up reemployment and did not increase the cost of the unemployment insurance system. This implies that the new benefit schedule made some unemployed definitely better off and none of them worse off. Moreover, given that the reform

increased government revenue , we conclude here that the Hungarian reform was welfare increasing.

Our results are in stark contrast with Kolsrud et al. (2015), who conclude that increasing the benefit profile is likely to be welfare improving. The key difference between their findings and ours is that they find that the behavior response to a benefit change at the beginning of the UI spell does not differ substantially from benefit changes happening latter on. If this were true, we should have found that the benefit increase at the beginning of the UI offsets the effect of the benefit decrease that happened towards the end of the UI, and so the behavioral responses to frontloading should be limited. As we showed above, our results does not support this prediction. While more studies are needed to understand better the behavioral responses to a benefit change, the key advantage of our setup relative to Kolsrud et al. (2015) is that we analyze here a very transparent and radical change in the UI benefit that is likely to induce responses in job search even in the presence of some adjustment costs (Chetty et al., 2013).

Finally, while this paper aims to evaluate the welfare implication of this reform, , in a related paper DellaVigna et al. (2016) we exploit the same reform to evaluate competing job search models. In that paper we show that a behavioral search model does a better job explaining the hazard rate to employment than the standard search models in the literature. Both papers highlight the importance of the benefit path, and suggest that redesigning the UI systems can sometimes break the classic trade-off between moral hazard and insurance.

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# Tables

Table 1: Descriptive Statistics: Comparing Means of Main Variables Pre- and Post UI Reform

	before	after	diff	t-stat
Percent Women	42.65% (0.41%)	44.08% (0.41%)	1.42%	2.42
Age in Years	37.08 (0.05)	36.91 (0.05)	-0.17	-2.03
Imputed Education (years) (based on occupation in the last job)	11.88 (0.01)	12.17 (0.01)	0.29	10.70
UI base/ average wage	0.98 (0.00)	1.04 (0.00)	0.06	9.62
Waiting period*	29.59 (0.31)	29.98 (0.30)	0.39	0.89
Reemployment bonus claimed	0.00 (0.00)	0.06 (0.00)	0.06	30.67
Number of observations**	13,994	14,336		

\* number of days between job loss and UI claim

\*\*there are some missing values for log earnings in 2002, 2003, 2004.

Table 2: Baseline results: Effect of the Reform on Non-Employment Duration

<b>Panel A: linear regression</b>						
	Non-employment duration (OLS)			Regression discontinuity		
	(1)	(2)	(3)	(4)	(5)	(6)
After	-11.17*** (1.101)	-12.42*** (1.098)	-12.46*** (4.367)	-10.15*** (2.324)	-18.16*** (4.507)	-21.50** (9.265)
Controls	no	yes	yes	-	-	-
control function			poly 3rd		linear	poly 3rd
Observations	29,050	29,050	29,050	6,440	6,440	6,440
R-squared	0.004	0.050	0.051	0.003	0.004	0.005
<b>Panel B - Cox hazard model</b>						
	Full sample			RD sample		
	(1)	(2)	(3)	(4)	(5)	(6)
After	0.173*** (0.0166)	0.201*** (0.0171)	0.191*** (0.0670)	0.171*** (0.0351)	0.261*** (0.0694)	0.313** (0.156)
Controls	no	yes	yes	-	-	-
control function			4th poly		linear	poly 4th
Observations	29,050	29,050	29,050	6,440	6,440	6,440

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

Note: This table shows the effect of the reform on non-employment duration. Column 1-3 of Panel A estimate regression in equation 1. Column 4-6 estimates regression discontinuity approach. These columns uses the optimal bandwidth of Imbens and Kalyanaraman and contains 8 months. Panel B estimates regression in equation 3. The non-employment duration is capped at 270 days in all columns. After is a dummy, which is 1 if the unemployed individual claimed benefit after the benefit reform. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed individual claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

Table 3: Job Quality: Effect of the Reform on Job Tenure in the New Job

<b>Panel A: linear regression</b>						
	Non-employment duration (OLS)			Regression discontinuity		
	(1)	(2)	(3)	(4)	(5)	(6)
After	0.278 (1.278)	-0.255 (1.295)	4.345 (5.130)	0.277 (2.695)	-0.221 (5.355)	-0.549 (12.56)
Controls	no	yes	yes	-	-	-
control function			poly 3rd		linear	poly 3rd
Observations	17,787	17,787	17,787	4,000	4,000	4,000
R-squared	0.000	0.030	0.030	0.000	0.000	0.002
<b>Panel B - Cox hazard model</b>						
	Full sample			RD sample		
	(1)	(2)	(3)	(4)	(5)	(6)
After	0.0351 (0.0275)	0.0170 (0.0521)	-0.000189 (0.105)	0.0308 (0.0542)	0.114 (0.110)	-0.0156 (0.248)
Controls	no	yes	yes	-	-	-
control function			poly 3rd		linear	poly 3rd
Observations	17,787	17,787	17,787	4,000	4,000	4,000

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

Note: This table shows the effect of the reform on the duration of the new job (measured in days). Column 1-3 of Panel A estimate regression in equation 1. Column 4-6 estimates regression discontinuity approach. These columns uses the optimal bandwidth of Imbens and Kalyanaraman (2012) and contains 8 months. Panel B estimates regression in equation 3 using the job tenure upon reemployment. The tenure is capped at 360 days in all columns. After is a dummy, which is 1 if the unemployed individual claimed benefit after the benefit reform. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed individual claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

Table 4: Job Quality: Effect of the Reform on Reemployment Wages

<b>Panel A: Log - reemployment wage</b>					
VARIABLES	full sample		RD sample		
	(1)	(2)	(3)	(4)	(5)
After	0.00369 (0.0146)	0.0628** (0.0249)	0.0299* (0.0154)	0.0568* (0.0307)	0.0943 (0.0658)
Controls	no	yes	-	-	-
control function	linear	3rd poly		linear	poly 3rd
Observations	17,660	17,660	3,976	3,976	3,976
R-squared	0.007	0.220	0.001	0.004	0.004
<b>Panel B: Log(reemployment wage/last wage)</b>					
VARIABLES	full sample		RD sample		
	(1)	(2)	(3)	(4)	(5)
After	0.0259* (0.0144)	0.0485* (0.0280)	-0.00609 (0.0154)	0.0873*** (0.0315)	0.103 (0.0682)
Controls	no	yes	-	-	-
control function	linear	linear		linear	poly 3rd
Observations	17,660	17,660	3,976	3,976	3,976
R-squared	0.016	0.090	0.000	0.004	0.004
<b>Panel C: Log(reemployment wage/wage in 2002)</b>					
VARIABLES	full sample		RD sample		
	(1)	(2)	(3)	(4)	(5)
After	0.0156 (0.0164)	0.0695*** (0.0247)	0.0389** (0.0176)	0.0875** (0.0347)	0.114 (0.0731)
Controls	no	yes	-	-	-
control function	linear	linear		linear	poly 3rd
Observations	16,609	16,609	3,767	3,767	3,767
R-squared	0.009	0.336	0.001	0.003	0.003

Note: This table shows the effect of the reform on reemployment wages. All Panels estimate equation 1 using various version of reemployment wages as an outcome variables (see text for the details): Panel A uses the the log-reemployment wage, Panel B uses the log-ratio of reemployment wage and the unemployment benefit base and Panel C uses the ratio of reemployment wage and average wage in 2002. We deflate wages with the nominal GDP growth and we control for a linear time trend in the full sample. Columns 3-5 use the optimal bandwidth of Imbens and Kalyanaraman and contains 8 months. Only workers who found a job in 360 days included in the sample. In Panel C we only use workers with non-missing information on the average wage in 2002. The after dummy is 1 if the unemployed individual claimed benefit after the benefit reform. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed benefit. Standard errors in parentheses are clustered at the local UI office level.



Table 5: The Effect of Frontloading by the Reemployment Bonus Take-up Rate

<b>Panel A: linear regression</b>								
	Non-employment duration (OLS)				RD sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After	-11.14*** (1.084)	-9.273*** (1.583)	-11.09** (4.843)	-8.805* (4.927)	-9.952*** (2.717)	-6.558 (4.208)	-22.34** (10.39)	-16.24* (9.116)
High take-up		-0.379 (2.656)				-2.868 (4.504)		-2.843 (4.465)
High take-up*after		-3.757* (2.182)		-4.532** (2.276)		-6.686 (5.405)		-6.726 (5.383)
Controls	no	no	yes	yes	-	-	-	-
control function			poly 3rd	poly 3rd			poly 3rd	poly 3rd
Observations	26,049	26,049	26,049	26,049	5,783	5,783	5,783	5,783
R-squared	0.004	0.004	0.048	0.048	0.003	0.004	0.043	0.006
<b>Panel B - Cox hazard model</b>								
	Full sample				RD sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After	0.173*** (0.0175)	0.137*** (0.0251)	0.167** (0.0746)	0.127* (0.0756)	0.165*** (0.0372)	0.116** (0.0535)	0.270* (0.157)	0.230 (0.152)
High take-up		0.00240 (0.0258)				0.0702 (0.0560)		0.0704 (0.0740)
High take-up*after		0.0719** (0.0350)		0.0788** (0.0364)		0.0933 (0.0743)		0.0925 (0.0889)
Controls	no	no	yes	yes	-	-	-	-
control function			poly 3rd	poly 3rd			poly 3rd	poly 3rd
Observations	26,049	26,049	26,049	26,049	5,783	5,783	5,783	5,783

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

Note: This table shows the effect of the reform on non-employment duration by the local reemployment bonus take-up rate. The sample in all columns includes unemployed who claim benefit in the UI locations with the lowest quartile take-up rate and in the UI locations with the highest quartile take-up rate. Column 1, 3, 5 and 7 show the baseline results for this particular sample. Column 2, 4, 6 and 8 estimate equation 4. Column 1-4 use the the full sample sample while Column 5- 8 use the optimal bandwidth of Imbens and Kalyanaraman (2012). a Cox proportional hazard model. Panel B uses Cox hazard model and repeats the columns of panel A. The length of non-employment is capped at 270 days in all Columns. The after dummy is 1 if the unemployed claimed benefit after the benefit reform. The high take-up is a dummy denoting that the unemployed claimed benefit at a location with above median reemployment bonus take up. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

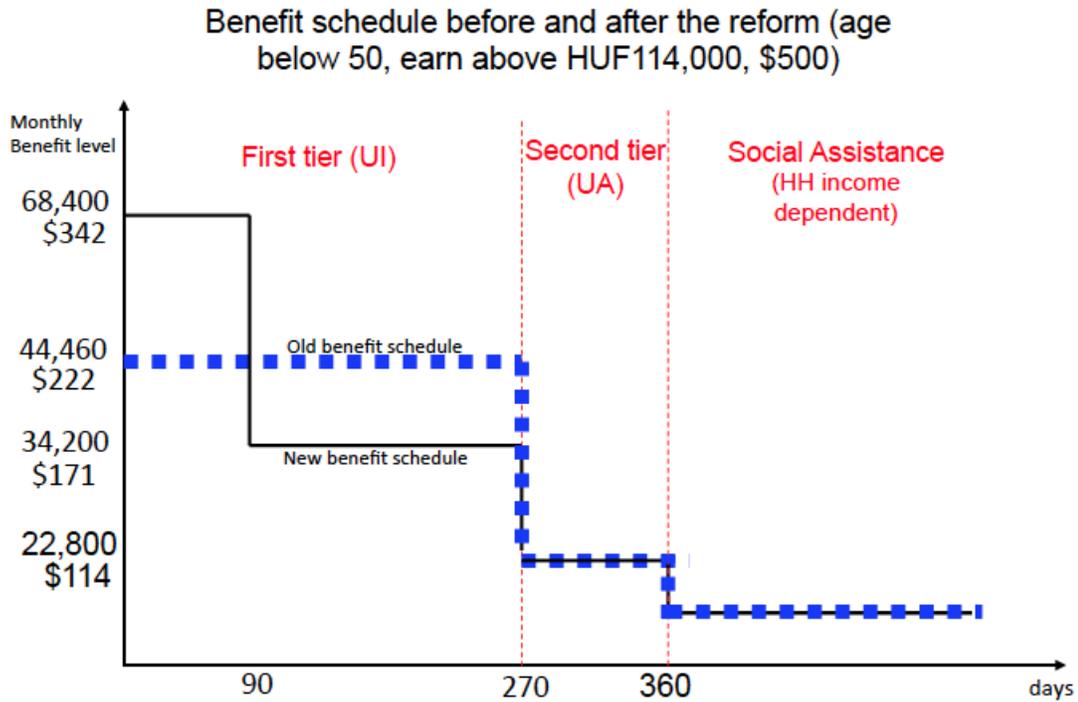
Table 6: The Effect of the Reform on the Budget

		s. e.**
<b>Balance of the unemployment benefit system</b>		
before*	\$1466	(5.78)
Mechanical cost change	\$118	(0.81)
Change in benefit spending because faster reemployment	-\$65	(7.58)
Change in UI contribution because more time in work	-\$9	(.85)
after*	\$1510	(5.88)
Net increase in UI cost	\$44	(8.76)
<b>Net gain in tax revenue</b>		
Taxes and contributions paid by the worker because more time in work	\$42	(4.22)
Contributions paid by the firm because more time in work	\$56	(5.672)
Change in government revenue	\$98	(9.90)
(Net gain in tax revenue - Net increase in UI cost)	\$54	(18.66)
*in the 1st year after UI claimed **bootstrapped standard errors in parenthesis		

Note: This table shows the effect of the reform on the government budget. We decompose the effect of the reform into different components based on equation 6 (see the text for details). Bootstrapped standard errors in parentheses are reported in the right column.

# Figures

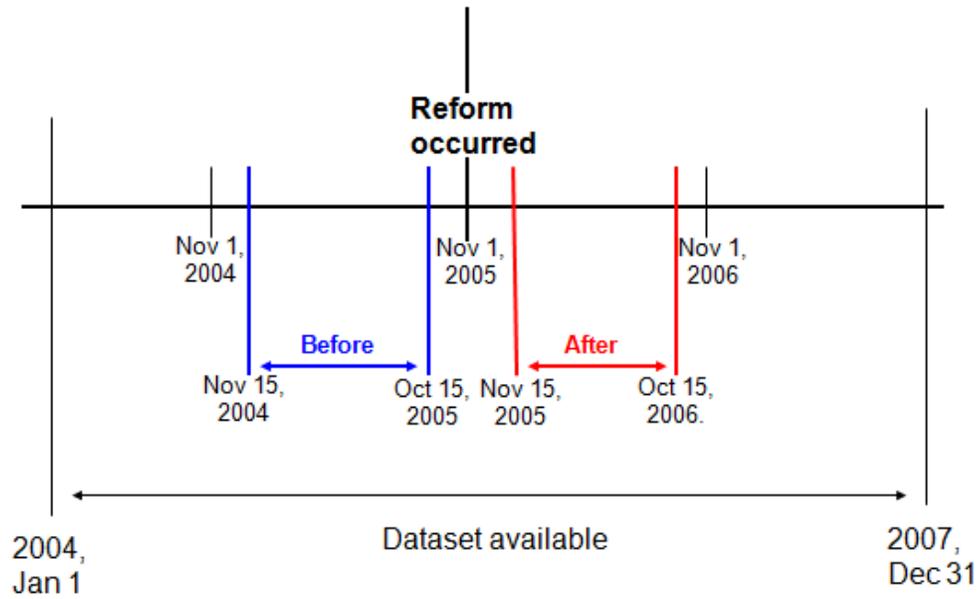
Figure 1: Benefit Schedule Before and After the Reform



Eligible for 270 days, base salary is higher than 114,000HUF

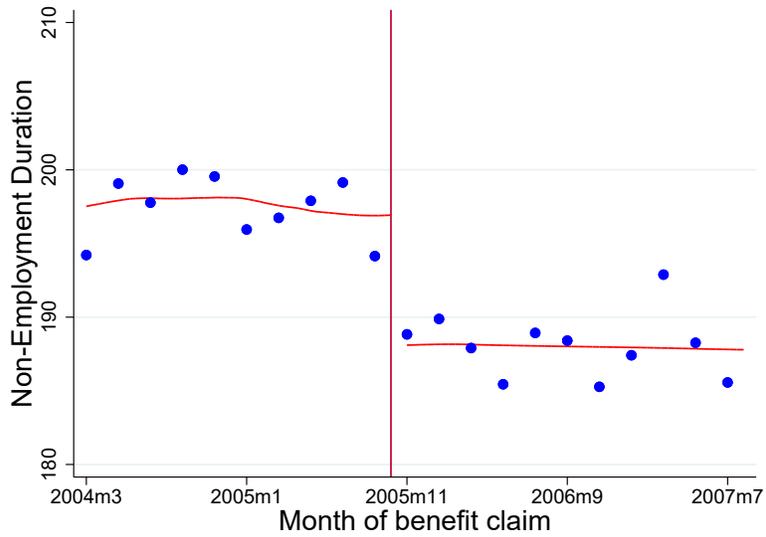
The figure shows the benefit schedule if UI is claimed on October 31, 2005 (old benefit schedule, dashed blue line) and the benefit schedule if UI is claimed on November 1st, 2005 (new benefit schedule, solid red line) for individuals who had 270 potential durations in the first-tier, were less than 50 years old and earned more than 114,000 HUF (\$570) prior to entering the UI scheme. The hypothetical benefit level is shown under social assistance. Benefit levels of social assistance depended on family income, household size and wealth and we do not observe these variables in our data.

Figure 2: Before and After Comparison Groups

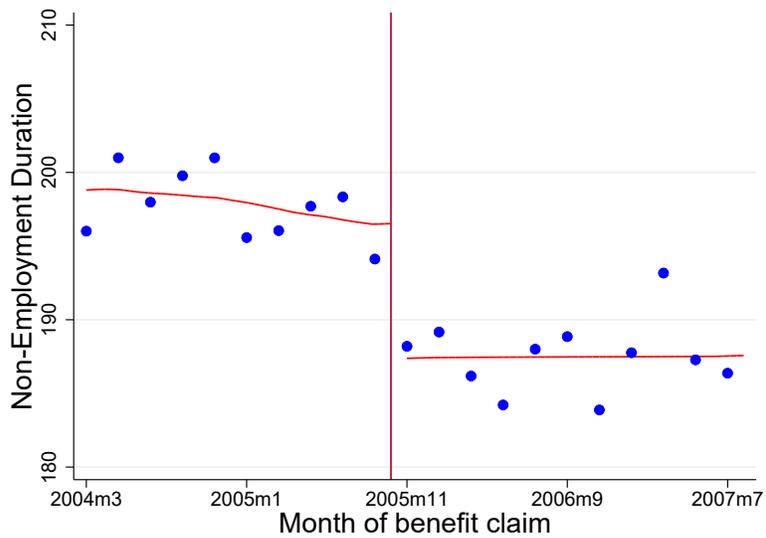


The figure shows the the time of the reform and how we define the before and after periods that we use for our before-after comparison. The before sample consists of those unemployed who claimed UI between November 31st, 2004 and October 31st, 2005, and the after sample consist unemployed who claimed UI between November 1st, 2005 and November 15th, 2007.

Figure 3: Baseline Results: Non-Employment Duration by 6-month Periods Relative to the Reform



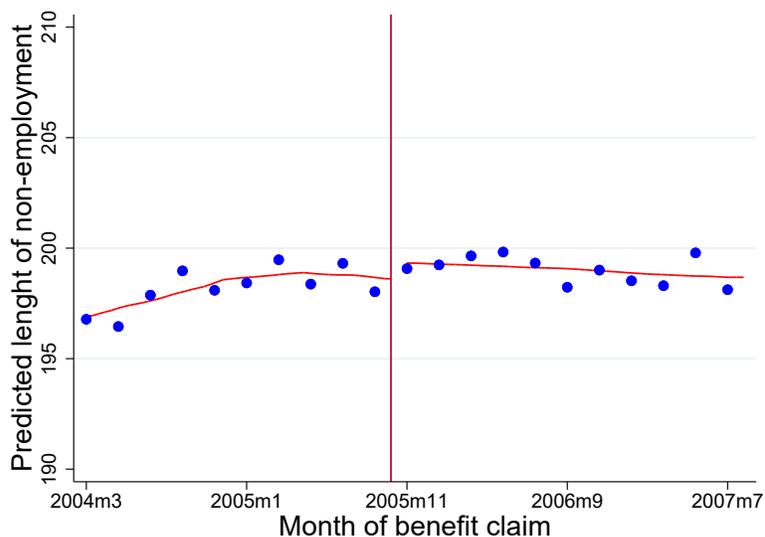
(a) No controls



(b) With controls

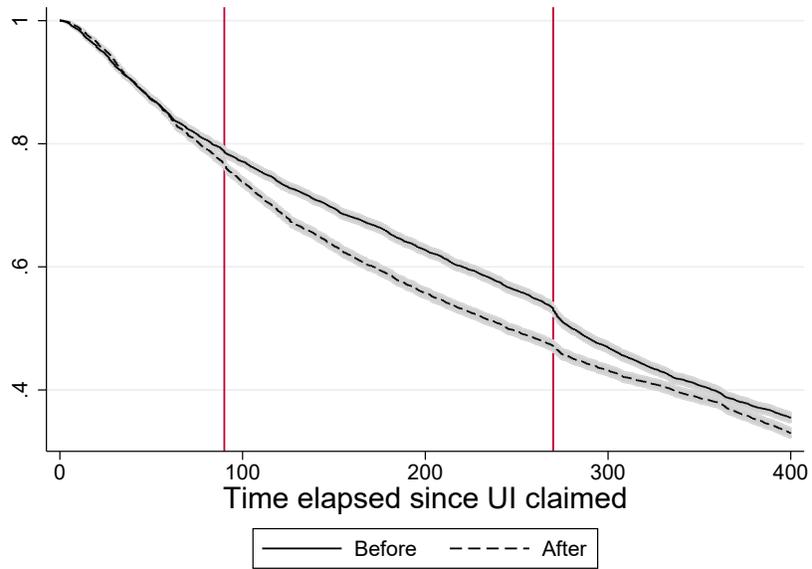
The figure shows the seasonally adjusted average length of unemployment spells by 2-month periods. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) of the last job, and log earnings in 2002 and 2003. The figure highlights that the average length of non-employment duration dropped immediately after the reform. The vertical red line shows the timing of the benefit frontloading.

Figure 4: Expected length of unemployment based on observable characteristics



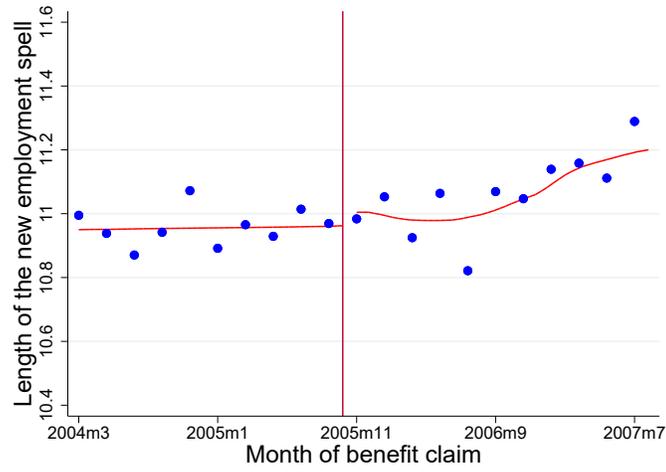
The figure shows the expected length of unemployment by the date of benefit claim. First we regress the actual length of unemployment (caped at 270 days) on sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) of the last job, and log earnings in 2002 and 2003. Second, we predict the expected length of unemployment and plot them by 2-month periods. The figure highlights that the expected length of non-employment duration is constant over time and unaffected by the reform. The vertical red line show the timing of the benefit frontloading.

Figure 5: Kaplan-Meier Survival Rates Before and After the Reform

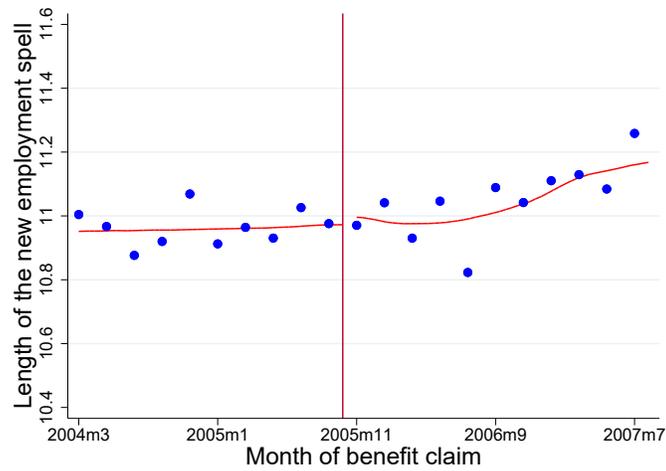


The figure shows the Kaplan-Meier survivor rates of the unemployed before and after the reform. The vertical red line shows the drop in the benefits after the reform at 90 and 270 days. The shaded area shows the confidence intervals of the survivor estimates.

Figure 6: Job Quality: Job Tenure at the New Job Before and After the Reform



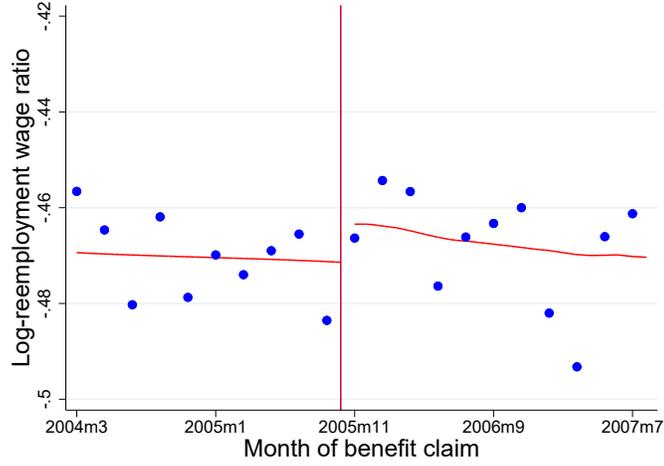
(a) No controls



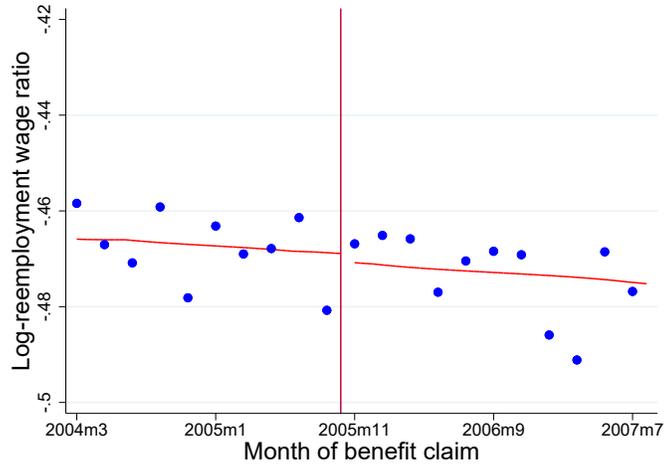
(b) With controls

The figure shows the average length of the new employment spells by 6-month periods. The length of employment is capped at 360 days and only workers who found a job within 360 days are included in the sample. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, and log earnings in 2002 and 2003. The vertical red line shows the timing of the benefit frontloading. The figure highlights that the duration of the new employment spells did not change after the reform.

Figure 7: Job Quality: Reemployment Wages Before and After the Reform



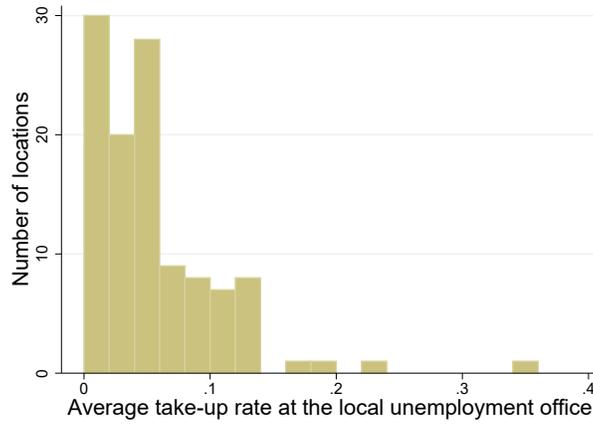
(a) No controls



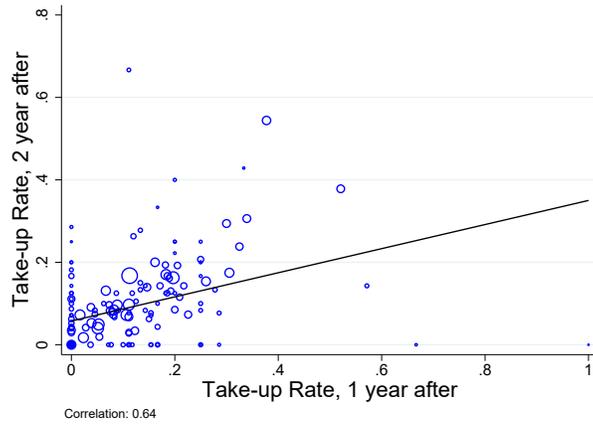
(b) With controls

The figure shows the log ratio of reemployment wage and the benefit base by 6-month periods. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, and log earnings in 2002 and 2003. Both regressions include linear time trends and only workers who found a job within 360 days are included in the sample. The vertical red line show the timing of the benefit frontloading. The figure highlights that reemployment wages did not change after the reform.

Figure 8: Take-up Rate of Reemployment Bonus



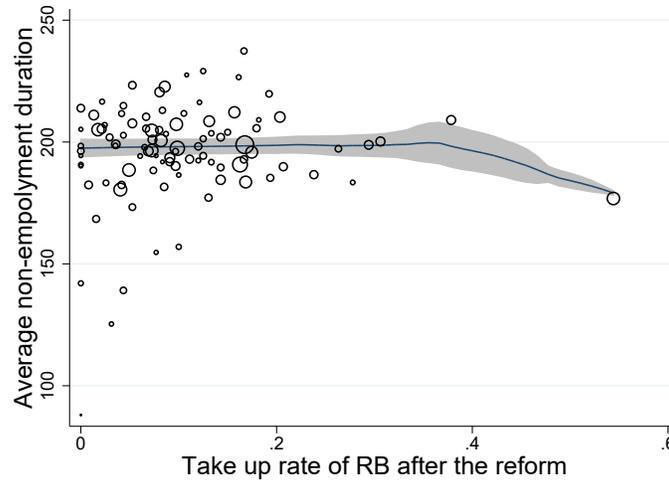
(a) Frequency distribution of the take-up rate across locations



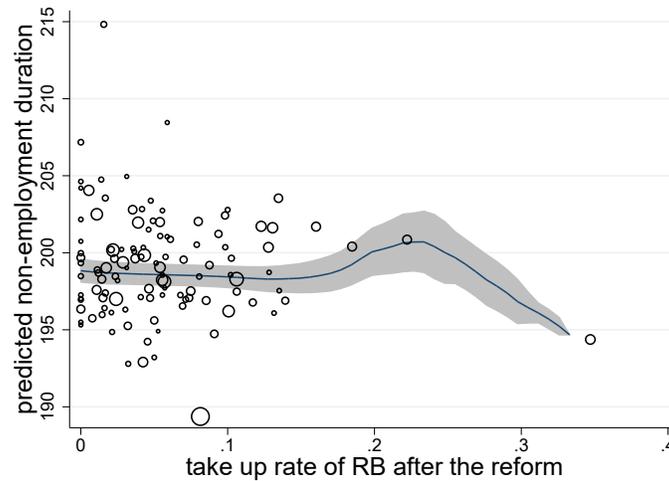
(b) Relationship between take-up rate 1 year and 2 years after the reform

Panel (a) shows the frequency distribution of local UI take-up rates. Panel (b) shows the take-up rate of reemployment bonus at local unemployment offices one year and two year after the reform. The graph highlights that the local take-up rate is persistent over time. In both panels only UI offices with at least 30 UI claimants were used.

Figure 9: Relationship between the Composition of UI Claimants and the Take-up Rate across Locations



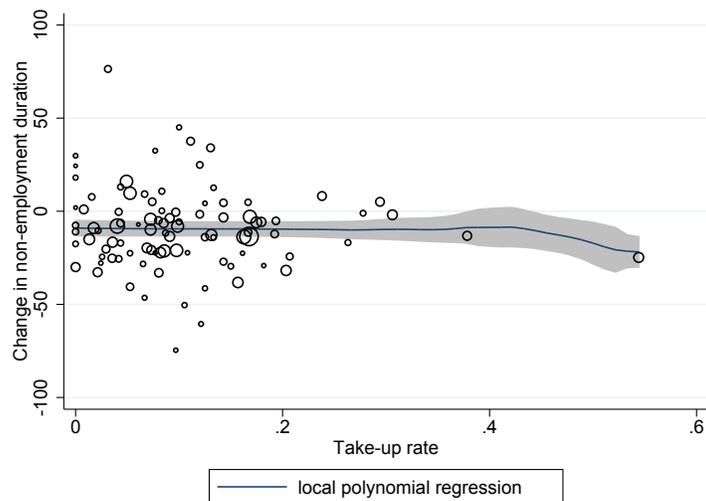
(a) Non-employment duration before the reform



(b) Predicted non-employment duration after the reform

The figure plots the relationship between the composition of UI claimants and the take-up rate of reemployment bonus after the reform at all UI locations. Panel (a) measures the composition of UI claimants with the average non-employment duration before the reform while panel (b) measures the composition of the unemployed by the predicted non-employment duration for those who claimed benefit after the reform. To get these predicted values we run a regression of non-employment duration on observable characteristics in the pre-reform sample and predict the average non-employment duration for the post-reform. The blue line shows the local polynomial fit weighted by the number of benefit claims before the reform. In both panels only UI offices with at least 30 UI claimants were used. The figure shows that the reemployment bonus take-up is uncorrelated with the length of non-employment before the reform.

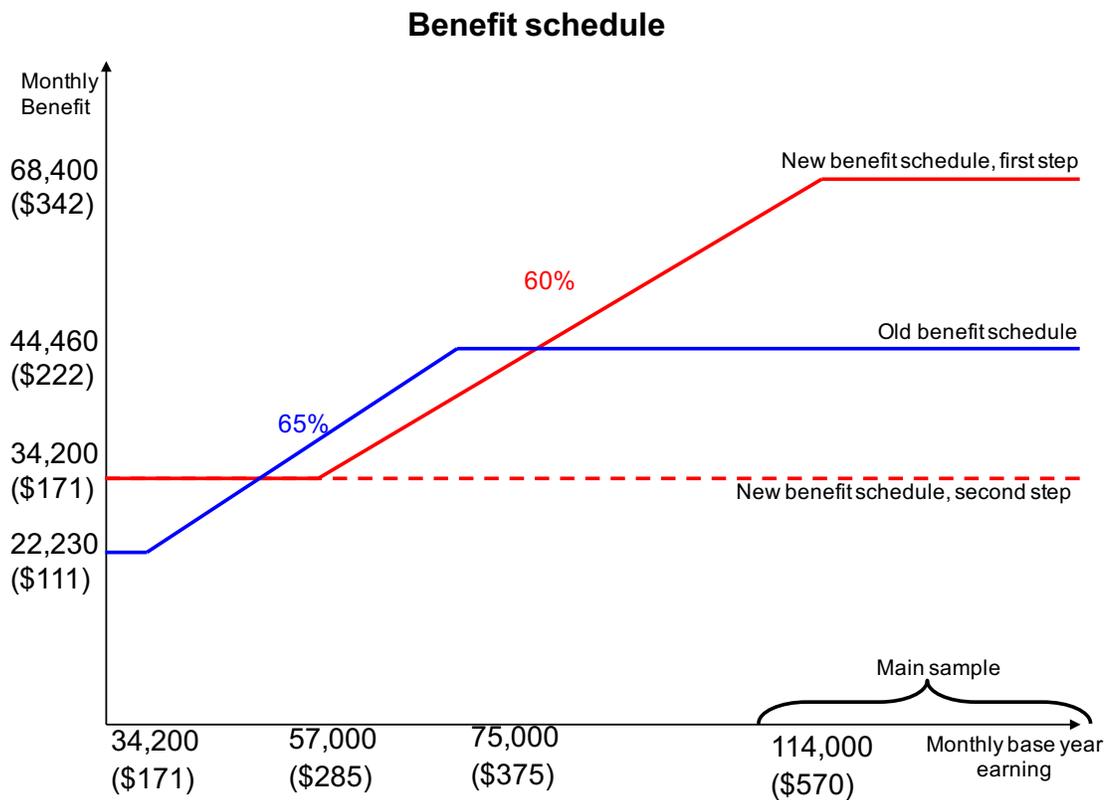
Figure 10: The Effect of the Reform by the Take-up Rate of the Reemployment Bonus



The figure plots the relationship between the before-after change in the average non-employment duration at the local UI office and the reemployment bonus take-up rate. The blue line shows the local polynomial fit weighted by the number of benefit claims before the reform. The figure shows no relationship between the change in non-employment duration and the reemployment bonus take-up rate.

# Appendix

Figure A-1: The UI Benefit Schedule Before and After the 2005 Reform in Hungary



The figure shows monthly UI benefits in the first tier under the old rule (blue solid line) in the first 90 days under the new rules (red solid line) and between 91-270 days under the new rules (red dashed line) as a function of the monthly base salary. The main sample, defined by being above the 70th percentile of the earnings base distribution of the UI claimants in the given year, is indicated by the curly brackets.



Figure A-2: Information Sheet Received by the Unemployed

Közép-Magyarországi Regionális Munkaügyi Központ  
Kerületi Kirendeltség

Ügyiratszám: BP-16/06-2005  
Ügyintéző:  
Telefon: (1) 403-0002

Tárgy: álláskeresési járadék megállapító határozat

**HATÁROZAT**

Ügyfél neve:  
Születési neve:  
Anyja neve:  
Születési helye, ideje: BUDAPEST,  
Levelezési címe: 1223 Budapest ZZ. ker.  
TAJ száma:

Kérelme alapján az **álláskeresési járadékra való jogosultság** 2005.11.14. naptól

**megállapítom,**

és 2005.11.14.-től

Időszak hossza	Összeg
90 napig	naptári nap 2280FT
91-180 napig	naptári nap 1140FT

összegben való **folyósítását rendelem el.**

A döntés ellen a kézhezvételt követő naptól számított 15 napon belül a Közép-Magyarországi Regionális Munkaügyi Központ címzett (címe: 1082 Budapest 08. Ker. Kisfaludy utca 11), de a XVI. Kerületi Kirendeltséghez (címe: ) 2 példányba benyújtott íltékmentes fellebbezéssel lehet élni.  
A határozat a fellebbezésre tekintet nélkül végrehajtható.

**INDOKLÁS**

2005.11.14. napján álláskeresési ellátás iránti kérelmet nyújtott be. Nevezett 2005.11.14. naptól álláskeresőként nyilvántartott.

A rendelkezésre álló iratokból megállapítottam, hogy az álláskeresővé válást megelőző öt éven belül 1728 nap munkaviszonyban töltött idővel rendelkezik.

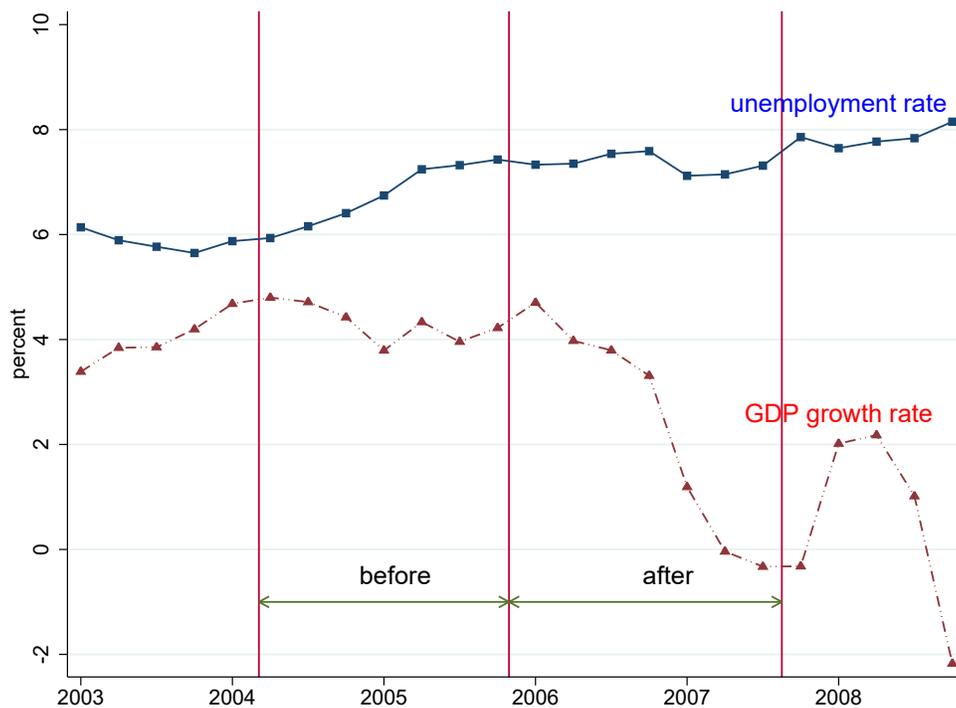
Az álláskeresési járadék napi összegét 902500 Ft a társadalombiztosítás ellátásaira és a magánnyugdíjra jogosultakról, valamint e szolgáltatások fedezetéről szóló 1997. Évi LXXX. Törvény 19. §-ának (3) bekezdésében meghatározott havi átlagos munkaerőpiaci járulékalap figyelembevételével – a rendelkezésekre álló igazolások alapján – állapítottam meg.

Előzőkre tekintettel megállapítottam, hogy az álláskeresési járadék megállapításának a foglalkoztatás elősegítéséről és a munkanélküliek ellátásáról szóló 1991. évi IV. törvény (Ft.) 25. § (1) bekezdésében foglaltak alapján határoztam meg.

Telefon: 1/403-0002, Fax: 1/403-3433, Holnap: <http://fovaros.munka.hu>; <http://www.afsz.hu>  
E-mail: KMRVivoutca@lab.hu

The figure shows an example of the the first page of the personalized information sheet received by an unemployed individual when UI was claimed. According to the table in the middle, the receiver of the form was eligible for daily HUF2280 for 90 days and daily HUF1140 for another 180 days.

Figure A-3: GDP growth and unemployment rate in Hungary



The figure shows the seasonally adjusted GDP growth rate (dashed red line) and the seasonally adjusted unemployment rate (solid blue) between 2003 and 2008 in Hungary. The major (red) vertical lines indicate the period we use for the before-after comparison. The data was obtained from the Hungarian Central Statistical Office.

Table A-1: The Effect of Replacement Ratio on Reemployment Hazards

	Full sample			RD sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Log-replacement ratio	-0.236*** (0.0214)	-0.222*** (0.0309)	-0.188*** (0.0332)	-0.309*** (0.0462)	-0.305*** (0.0467)	-0.310*** (0.0472)
Controls	no	yes	yes	-	-	-
control function			poly 4th		linear	poly 4th
Observations	28,330	28,330	28,330	5,720	5,720	5,720

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Note: This table estimates equation ?? and shows the effect of the log-replacement ratio on non-employment duration. Column 1-3 uses the full sample and Column 4-6 estimates regression discontinuity approach and uses the optimal bandwidth of Imbens and Kalyanaraman (2012) and contains 8 months. The non-employment duration is capped at 270 days in all columns. The replacement ratio is the ratio of the monthly benefit eligibility and unemployment benefit base. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed individual claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

Table A-2: The effect of Frontloading by the Reemployment Bonus Take-up Rate

VARIABLES	Non-employment duration (OLS) <sup>1</sup>				Reemployment hazards (Cox-estimation)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
after	-11.14*** (1.084)	-9.278*** (1.545)	-11.09** (4.843)	-8.867* (4.972)	0.173*** (0.0175)	0.139*** (0.0263)	0.167** (0.0746)	0.136* (0.0758)
take-up rate		-0.145 (0.281)				0.00272 (0.00214)		
take-up rate*after		-0.342** (0.140)		-0.386*** (0.144)		0.00598** (0.00297)		0.00582*** (0.00212)
controls	no	no	yes	yes	no	no	yes	yes
location FE	no	no	yes	yes	no	no	yes	yes
Observations	26,049	26,049	26,049	26,049	26,049	26,049	26,049	26,049
R-squared	0.004	0.004	0.048	0.048				

Clustered standard errors by UI take-up locations in parentheses

<sup>1</sup>Capped at 270 days.

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

Note: This table shows the effect of the reform on non-employment duration by the local reemployment bonus take-up rate. The sample in all columns includes those unemployed individuals who claimed benefit in a UI office that has had at least 30 RB claimants in our sample. Column 1, 3, 5 and 7 show the baseline results for this particular sample. Column 2 and 4 estimate equation 4 and Column 6 and 8 estimate a Cox proportional hazard model. We use continuous measure of take-up rate instead of using the high take-up rate dummy variable as in Table 5. The length of non-employment is capped at 270 days in all columns. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed the benefit. Standard errors in parentheses are clustered at the local UI office level.

## A.1 The Effect of the Reform on the Budget

Table 6 summarizes the effect on the budget. We use equation 6. The  $b_t^{post}$  and  $b_t^{pre}$  are the daily pre- and post benefits shown on Figure 1.  $S_t^{post}$  and  $S_t^{pre}$  are the daily pre- and post- survival rates shown in Figure 5. The average monthly gross reemployment wage was \$509.

The following items are paid to the government:

1. Unemployment insurance contributions. The UI contribution was 4.5% of the gross wage and paid directly into the budget of the unemployment benefit system. Given that the behavioral effect of the reform was around 10 days, the additional revenue of the benefit budget was around  $\$509 * (10/30) * 4.5\%$ .
2. Personal Income Tax. The income taxes were based on monthly earnings. The tax rate below the minimum wage (\$285) was 0, while above the minimum wage it was 18 percent. This means that around  $(\$509 - \$285) * (10/30) * 18\% = \$13.4$  was paid in taxes.
3. Health insurance contribution. The health insurance contribution was a fixed \$9.75 per month. The additional revenue effect of that item was around  $(10/30) * \$9.75 = 3.25$
4. Social security contribution (employee part). The social security contribution was 12.5 percent of the gross wage, and so the sum of taxes paid by the workers were around  $\$509 * (10/30) * 12.5\% = \$21.2$
5. Social security contribution (employer part). Firms also needs to pay social security contributions which is 30% of the gross wage so the contributions paid by the firm were around  $\$509 * (10/30) * 30\% = \$50.9$

## A.2 Proof of Proposition 1

Proposition 1. Suppose that the unemployment benefit is increased by  $\widetilde{\Delta b}$  in the first  $N$  periods and decreased by  $\underline{\Delta b}$  afterwards, while the total benefit that can be collected throughout the unemployment spell remained constant, formally,

$$\sum_{k=0}^N \widetilde{\Delta b} + \sum_{k=N+1}^T \underline{\Delta b} = \sum_{k=1}^T \Delta b_k = 0. \quad (8)$$

Then the effect of benefit change on the value of unemployment at the beginning of the UI spell is determined by the following formula:

$$\Delta V_0^U(A_0) = \underbrace{u'(c_0^{u*}) \Delta b_0 + \sum_{k=1}^N \delta^k \prod_{i=1}^k (1 - s_i^*) u'(c_k^{u*}) \Delta b_k}_{\substack{\text{welfare effect caused by} \\ \text{change in the benefit} \\ \geq 0}} \quad - \underbrace{\sum \delta^k v'(G) \Delta G}_{\substack{\text{welfare effect caused by} \\ \text{change in public spending} \\ \leq 0}}$$

The first part of this formula, welfare effect caused by change in the benefit, is always non-negative, and it only becomes positive if optimal search  $s_t^*$  is positive for at least one period throughout the unemployment spell or if the interest rate,  $r$ , is positive. The second part of this formula, the welfare effect caused by change in public spending, can be positive, negative or zero depending on the sign of  $\Delta G$ . Moreover,  $\Delta G$  will be determined by the following equation.

$$\Delta G = \underbrace{\frac{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t S_t \Delta b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease} \\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t \tau}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax revenue} \\ \text{caused by finding job sooner}}}$$

Proof:

The value of unemployment is defined by the following equation:

$$V_t^U(A_t) = u(c_t^{u*}) - c(s_t^*) + v(G) + \delta [s_t V_{t+1}^E(A_{t+1}^*) + (1 - s_t) V_{t+1}^U(A_{t+1}^*)]$$

Based on this the value of unemployment in period 0 can be rewritten as

$$V_0^U(A_0) = \sum_{k=0}^T \delta^k v(G) + u(c_0^{u*}) - c(s_0^*) + \sum_{k=1}^T \delta^k \prod_{i=1}^k (1 - s_i^*) [u(c_k^{u*}) - c(s_k^*)] + \sum_{k=1}^T \delta^k \prod_{i=1}^{k-1} (1 - s_i^*) s_k^* V_k^E(A_{t+1}^*)$$

Now we look at the change in benefits described by equation 8. By the envelop theorem the indirect effect on the value function will be second order, and so the effect of benefit change on the value function will be the following:

$$\Delta V_0^U(A_0) = \underbrace{u'(c_0^{u*}) \Delta b_0 + \sum_{k=1}^N \delta^k \prod_{i=1}^k (1 - s_i^*) u'(c_k^{u*}) \Delta b_k}_{\substack{\text{welfare effect caused by} \\ \text{change in the benefit} \\ \geq 0}} \quad \underbrace{- \sum \delta^k v'(G) \Delta G}_{\substack{\text{welfare effect caused by} \\ \text{change in public spending} \\ \leq 0}} \quad (9)$$

As we show next, the first term is always positive, while the second term can be positive or negative depending on the sign of  $\Delta G$ . We will provide the expression for  $\Delta G$  later.

To show that the welfare effect caused by the benefit change is non-negative, we stipulate that the optimal consumption path must satisfy the usual Euler equation:

$$u'(c_t^{u*}) \geq \delta(1 + r) \left[ s_t^* \frac{\partial V_{t+1}^E(A_{t+1}^*)}{\partial A_{t+1}} + (1 - s_t^*) u'(c_{t+1}^{u*}) \right]$$

This equation can be easily derived from the FOC of the value function with respect to  $A_{t+1}$  and from the envelop theorem that indicates that  $\frac{\partial V_{t+1}^U(A_{t+1}^*)}{\partial A_{t+1}} = u'(c_{t+1}^{u*})$ . This equation holds for equality in the absence of borrowing constraints while in the presence of binding borrowing constraints the left hand side is strictly greater than the right hand side.

Given that  $\frac{\partial V_{t+1}^E(A_{t+1}^*)}{\partial A_{t+1}} > 0$ ,  $s_t^* \geq 0$ , and  $r \geq 0$ , the Euler equation implies that  $u'(c_t^{u*}) \geq \delta(1 - s_t^*) u'(c_{t+1}^{u*})$  for all  $t$  and this inequality holds strictly if  $s_t^* > 0$  or  $1 + r > 1$ . This equation also implies that  $\delta^t \prod_{i=1}^t (1 - s_i^*) u'(c_t^{u*}) \geq \delta^T \prod_{i=1}^T (1 - s_i^*) u'(c_T^{u*})$  for all  $t$ . Therefore,

$$u'(c_0^{u*}) \Delta b_0 + \sum_{k=1}^T \delta^k \prod_{i=1}^k (1 - s_i^*) u'(c_k^{u*}) \Delta b_k \geq \prod_{i=1}^T (1 - s_i^*) u'(c_T^{u*}) \sum_{k=1}^T \Delta b_k$$

and whenever  $s_t^* > 0$  for at least one period or  $r > 0$ , this inequality holds strictly. Moreover, given that equation 8  $\sum_{k=1}^T \Delta b_k = 0$ , this inequality implies that the first part of equation 9 is positive:

$$u'(c_0^{u*}) \Delta b_0 + \sum_{k=1}^N \delta^k \prod_{i=1}^k (1 - s_i^*) u'(c_k^{u*}) \Delta b_k \geq 0$$

and the inequality holds strictly if  $s_t^* > 0$  for at least one period or if  $r > 0$ .

Now we derive the expression for  $\Delta G$ . By total differentiating the government budget we get the following expression:

$$0 = \sum_{t=1}^T \left( \frac{1}{1+r} \right)^t \Delta G + \sum_{t=1}^T \left( \frac{1}{1+r} \right)^t \Delta S_t b_t + \sum_{t=1}^T \left( \frac{1}{1+r} \right)^t S_t \Delta b_t + \sum_{t=1}^T \left( \frac{1}{1+r} \right)^t \Delta S_t \tau w.$$

where we specify that taxes and deficit are kept constant. This leads to the expression in the

proposition:

$$\Delta G = \underbrace{\frac{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t S_t \Delta b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t b_t}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease} \\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^T \Delta S_t \tau}{\sum_{t=1}^T \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax revenue} \\ \text{caused by fining job sooner}}}$$