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Front-Loading the Unemployment Benefit: An Empirical Assessment†

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We estimate the effect of front-loading unemployment benefit payments on nonemployment duration and reemployment wages. Exploiting a sharp change in the path of benefits for those who claimed unemployment benefits after November 1, 2005 in Hungary, we show that nonemployment duration fell by two weeks, while reemployment wages rose by 1.4 percent as a result of front-loading. We show that these behavioral responses were large enough to offset the mechanical cost increase of the unemployment insurance. We argue that our results indicate that benefit front-loading was a Pareto improving policy reform as both unemployed and employed workers were made better off. (*JEL* D91, J31, J64, J65)

Unemployment insurance programs aim to protect against financial distress at job loss while maintaining incentives for job search. Unfortunately, these two goals are usually in conflict: an insurance instrument that provides better protection often induces moral hazard.

The optimal level of unemployment insurance (UI) is determined by a trade-off between its insurance value and its moral hazard cost (Baily 1978, Chetty 2008), and the recent empirical literature has made considerable progress in estimating the relative size of these effects (Krueger and Meyer 2002, Chetty and Finkelstein 2013, Schmieder and von Wachter 2016). Most empirical studies examine responses to changes in the level (e.g., Card et al. 2015) or duration of unemployment insurance (e.g., Card, Chetty, and Weber 2007) that did not fundamentally change the path of the benefit payments throughout a period of unemployment.

A mainly theoretical literature, on the other hand, allows for a more flexible benefit structure and focuses on the shape of the optimal benefit path (Shavell and Weiss

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1979, Hopenhayn and Nicolini 1997, Cahuc and Lehmann 2000, Werning 2002, Shimer and Werning 2008). Changing the benefit path can maintain the insurance aspects of UI while providing better incentives to search for a job. For instance, a change that front-loads the benefit profile—by raising the unemployment benefit early in an unemployment spell and then cutting it by the same amount later in the spell—allows short-term unemployed workers to collect more benefits while leaving the amount collected by the long-term unemployed throughout their unemployment spell unaffected. Therefore, benefit front-loading is likely to make some unemployed better off without detriment to the others.[1]

The potential downside of such a policy is that total expenditure in the UI system might increase, since all unemployed workers collect the extra benefit payments at the start of their unemployment spell, but only those with relatively long unemployment durations experience the benefit reduction. Such an increase in UI costs might eventually increase taxes and make taxpayers worse off. However, the effect of front-loading on government spending is ambiguous. While the cost of UI increases mechanically as some of the unemployed collect more benefits, front-loading may speed up the transition to employment and so reduce benefit payments and increase tax revenue. In principle, this behavioral response can be large enough to fully offset the mechanical cost increase caused by benefit front-loading.

Therefore, the benefit front-loading described here can be potentially Pareto improving with some unemployed workers made better off without making taxpayers worse off. However, whether the cost savings from elevated job search are large enough to offset the mechanical cost increase remains an empirical question.

This paper provides the first empirical evidence to answer this question. We exploit a unique Hungarian reform that changed the time profile of UI payments radically (see Figure 1). Unemployed workers who claimed benefits before November 1, 2005 could rely on a constant payment for 270 days. Those who claimed after November 1, 2005 were faced with a front-loaded benefit profile with the total UI benefit that the unemployed were eligible for remaining almost the same.[2] In particular, the benefit was around 50 percent higher in the first 90 days and then around 25 percent lower in the next 180 days. Another key feature of the Hungarian UI system was that the benefit profile, both before and after the reform, was very clear from the beginning of the unemployment spell as all claimants received a personalized letter about their future benefit payments.

We assess the effect of this unique policy change on nonemployment duration using administrative data on UI claimants and social security contributions. We use a regression discontinuity research design in which we exploit that the benefit path changes sharply on November 1, 2005. We show that the average nonemployment duration was stable at around 239 days preceding the reform. However, the average nonemployment duration dropped sharply by 14 days (SE 1.5) for those who claimed after November 1, 2005. We conduct a number of robustness tests to verify

[1] Unemployed workers in the new system can replicate their old consumption profile by saving some of the extra benefits they receive at the beginning of their unemployment spell. As the old budget constraint is feasible under the front-loaded benefit structure, a forward-looking rational unemployed worker cannot be made worse off.

[2] The total UI benefit eligibility for the unemployed after the reform was 2.4 percent higher than before so, in fact, the overall level of UI was slightly more generous after the reform.

that our results do not rely on any specific implementation design or functional form and are not driven by manipulation in the timing of the benefit claim around the reform.

We also examine the effect of the benefit front-loading on the quality of jobs found. Faster reemployment might reduce the quality of new jobs if people rush into worse matches. However, shorter unemployment duration could also improve unemployed agents' job opportunities if it means their skills deteriorate less during their unemployment spell (Nekoei and Weber 2017). We find that the second channel is more important in our setting, since reemployment wages increased by 1.4 percent (SE 0.7 percent) after the reform. These results suggest that the shorter unemployment duration, if anything, helped workers to find higher-paying jobs.

We then translate the estimated effects into changes in the government's budget. The new benefit mechanically increased government spending by around US$119 (SE 0.8) per unemployed worker because the short-term unemployed, who exit unemployment before 270 days, receive higher payments under the new rules. However, to assess the budget consequences, we also need to take into account that the reform led workers to exit unemployment sooner, and so claimed UI for a shorter time. We find that nearly 65 percent of the mechanical cost increase is offset by lower government spending caused by earlier exit from unemployment. Moreover, unemployed workers who found a job earlier at a higher reemployment wage also paid more personal income tax and social security contributions, adding US$194 per unemployed worker to government revenue. Therefore, the increase in the government's revenue was more than enough to offset the mechanical spending increase.

The positive fiscal impact of the benefit reform implies that the overall tax burden of UI shrank after front-loading. This is particularly surprising given that the UI system became more generous as a result of the front-loading reform.[3] Our findings highlight that changing the benefit path can make both taxpayers and the unemployed better off and so it can be a potentially welfare-improving policy change.

The key assumption behind our empirical strategy is that there were no other policy or economic changes that affected nonemployment duration or reemployment wages at the time of the reform. The composition of the unemployed who claimed benefits was similar before and after the reform, and while there was a slight slowdown in the Hungarian economy two years after the UI reform, the aggregate unemployment rate remained stable.[4] This suggests that our results are not driven by economic fluctuations. The only important policy change that could affect our results is a voluntary reemployment bonus scheme that was introduced in parallel with the benefit reform.

To separate the effect of the reemployment bonus scheme from benefit front-loading, we exploit the local variation in knowledge about the availability of the new bonus scheme similarly to Chetty, Friedman, and Saez (2013). While UI offices

[3] Such a nonintuitive finding is similar to the Laffer-curve relationship between taxes and revenue: reducing taxes can increase government revenue if behavioral responses are large. Here we find that front-loading benefit payments can decrease the spending on UI and make the unemployed better off at the same time.

[4] The reform only affected a subset of the unemployed who claimed benefits around the reform, had a close to continuous working history, and had earnings above the average wage. So even if the length of unemployment spell in our sample decreased, the effect on the aggregate unemployment rate was small.

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 communicated less clearly. Moreover, the reemployment bonus scheme was complicated and associated with extra hassle costs as claimants needed to go through an additional administrative process on top of the standard UI claims procedure to receive it. Therefore, the role of local UI offices was crucial in advocating the scheme and in helping eligible claimants to apply for it.

We infer the access to information about the reemployment bonus from the average bonus take-up rate at the UI office where an individual claimed their benefit. There are large and persistent differences in reemployment bonus take-up rates across UI offices that are not related to observable characteristics of the unemployed workers they serve. In some locations, the take-up rate was close to 0, while in others it was above 10 percent. We show that the drop in nonemployment duration and the increase in reemployment wages after the reform are not related to the local take-up rate of the reemployment bonus. Moreover, even if we restrict our analysis to locations with a low take-up rate, the point estimates are very close to our benchmark estimates. This evidence underlines that the reform effect is mainly driven by the changes in the UI benefit path, and the introduction of the reemployment bonus scheme played only a minor role, if any, in generating our results.

This paper is related to the literature on estimating the moral hazard implications of unemployment insurance. Numerous studies have estimated the effect of changing the benefit level on unemployment durations (e.g., Meyer 1990; Lalive, Van Ours, and Zweimüller 2006; Card, Chetty, and Weber 2007; Landais 2015), and most papers have found that there is a considerable effect of unemployment benefits on job search behavior (see surveys of this literature by Krueger and Meyer 2002, Chetty and Finkelstein 2013, and Schmieder and von Wachter 2016). Other aspects of unemployment insurance systems have also 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, empirical evidence on the effect of changing the benefit path is surprisingly limited. A notable exception is Kolsrud et al. (2018), who estimate the effect of unemployment benefits paid at different parts of the unemployment spell on nonemployment duration in Sweden but do not examine the effect on reemployment wages. Our results highlight that the effect on reemployment wages is important as it accounts for around 20 percent of the fiscal impact of the reform. Our results on nonemployment duration also differ from Kolsrud et al. (2018), who find that unemployed workers respond more to changes in the benefit level at the beginning of the UI spell than toward the end. This finding of Kolsrud et al. (2018) implies that benefit front-loading should *increase* nonemployment duration;[5] but, in stark contrast to this, we find a large fall in nonemployment duration in response to benefit front-loading.

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

[5] Kolsrud et al.'s (2018) benchmark estimates imply that unemployment duration should increase by five days in response to the policy change we study.

2008). These papers derive the fully optimal UI profile under strong assumptions about the environment in which the unemployed make their decisions, and the optimal UI profile is very sensitive to these assumptions (Hopenhayn and Nicolini 1997, Werning 2002). Moreover, the fully optimal benefit schedule is often complicated and hard to implement. Therefore, instead of searching for the fully optimal UI benefit schedule, we look at the welfare implications of an easily implementable reform that moves away from the standard constant benefit schedule to a front-loaded one.

We also contribute evidence on the effect of unemployment insurance on job quality. Recent research shows mixed results on the effect of increasing the UI replacement rate or benefit duration on reemployment wages and on job quality (Card, Chetty, and Weber 2007; Lalive 2007; van Ours and Vodopivec 2008; Nekoei and Weber 2017; Schmieder, von Wachter, and Bender 2016; Johnston and Mas 2018). In the settings studied in these papers, negative effects on the duration of unemployment would be implausible, and so the only empirical question is about the sign of the effect on reemployment wages. On the other hand, our paper is the first to look at the impact of front-loading (rather than an increase in the replacement rate or the potential benefit duration) on reemployment wages. This policy change is different as the direction of the effect on both duration and wages is ambiguous theoretically, and so the trade-off between waiting for a better job match and being subject to skill depreciation is starker. We find that changing the benefit path can achieve both shorter nonemployment duration and higher reemployment wages, which provides direct evidence for negative duration dependence in wages (Kroft, Lange, and Notowidigdo 2013).

Finally, this paper is also related to DellaVigna et al. (2017), which exploits the same reform. DellaVigna et al. (2017) contrast the predictions of 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. In this paper, we focus on the welfare implications of front-loading with the goal of informing policymakers about the optimal design of unemployment insurance. This exercise requires us to establish several new facts about the impact of the reform, including the effect on nonemployment duration, on reemployment wages and, most importantly, on the budget consequences of the reform. A key contribution here is to show that the government's budget is improved after front-loading, and so we can assess the welfare implications of front-loading in a general framework that embeds most models used in the theoretical literature on the optimal unemployment insurance.

The paper is set out as follows. Section I provides a theoretical framework that allows us to assess the effect of front-loading on job search, on the government's budget, and on welfare. Section II describes the data and institutional details of the unemployment insurance reform. Section III presents the empirical results on nonemployment duration, reemployment wages, and on the government's budget, and assesses the welfare implications of the front-loading policy based on these estimates. Section IV concludes.

I. Theoretical Framework

We begin by presenting a simple model to highlight the effect of benefit front-loading on job search, the UI budget, and welfare. Our model is closely related to Card, Chetty, and Weber (2007); Lentz and Tranæs (2005); and Kolsrud et al. (2018). In the online Appendix, we present a more elaborate version of the model with reservation wage decisions.

Model: Consider a discrete-time setting where agents have a finite planning horizon and become unemployed at time 0. Each period, the unemployed decide on their consumption c_t and their job search intensity s_t. Normalize s_t to equal the probability of finding a job in the current period. The flow utility is given by $u(c_t) - \psi(s_t)$, where the functions u and ψ are strictly concave and convex, respectively. The value function of the agent who finds a job at time t is the following:

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

where $A_{t+1} = (1+r)A_t + (1-\tau)w_t - c_t$. This formulation of V_t^e assumes that finding a job is an absorbing state, and there is no on-the-job search.

The value function for the unemployed is given by the following equation:

$$V_t^u(A_t) = \max_{s_t, A_{t+1}} u(c_t^u) - \psi(s_t) + \delta\left[s_t V_{t+1}^e(A_{t+1}) + (1-s_t)V_{t+1}^u(A_{t+1})\right],$$

where $A_{t+1} = (1+r)A_t + b_t - c_t^u$.

Optimal Search Intensity: The optimal search effort, s_t^*, is given by

$$s_t^* = \psi'^{-1}\left(\delta\left[V_{t+1}^e(A_{t+1}^*) - V_{t+1}^u(A_{t+1}^*)\right]\right),$$

while the optimal consumption profile of the unemployed is determined by the Euler equation

$$u'\left(c_1^{u^*}\right) = (1+r)\delta\left[s_1^* u'\left(c_2^{e^*}\right) + (1-s_1^*)u'\left(c_2^{u^*}\right)\right].$$

One can easily derive the effect of benefit change on the optimal search effort by applying the envelope condition:

$$\frac{\partial s_t^*}{\partial b_{t+j}} = -\frac{u'\left(c_{t+j}^{u^*}\right)}{\psi''(s_t^*)}\delta^j \prod_k^j (1 - s_{t+k}^*),$$

where $j > 0$.

Front-loading: We define front-loading as a benefit change, where the first period benefit increases by a dollar, $\Delta b_1 =$ US\$1, and the second period benefit falls by the same amount, $\Delta b_2 = -$US\$1.[6] Note that this benefit change ensures that the total amount that can be collected by the unemployed, $b_1 + b_2$, remains the same.[7]

[6] We provide results for a more general formulation of front-loading in the online Appendix, where the unemployment benefit is higher in the first few weeks and then is lower afterwards.

[7] We require 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 ensure our analysis corresponds closely with the exact reform that occurred, but the results are unaffected if the present value of the total benefit is kept constant instead.

The effect of benefit front-loading on the initial period job search, s_0, is negative in this model:

$$\frac{\partial s_0^*}{\partial b_1} - \frac{\partial s_0^*}{\partial b_2} = -\frac{\delta(1 - s_0^*)}{\psi''(s_0^*)}\left[u'\left(c_1^{u^*}\right) - \delta(1 - s_1^*)u'\left(c_2^{u^*}\right)\right] < 0,$$

where the inequality is implied from the Euler equation. At the same time, the effect of benefit front-loading on the first-period job search, s_1, is positive:

$$\frac{\partial s_1^*}{\partial b_1} - \frac{\partial s_1^*}{\partial b_2} = -\frac{\delta(1 - s_1^*)}{\psi''(s_1^*)}u'\left(c_2^{u^*}\right) > 0.$$

Since the initial period job search effort is reduced while the later period job search effort is increased, the effect of front-loading on nonemployment duration is ambiguous.

The government budget in this model is given by the following equation:

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

where $S_t^* = \prod_{i=0}^{t-1}(1 - s_i^*)$ is the fraction of agents who are still unemployed at time t. The first part is the government's tax revenue coming from earnings. The second part is the spending on UI benefit. The effect of front-loading on the government budget is given by the following equation:

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

$$= \underbrace{-\frac{1}{(1 + r)}S_1^* + \frac{1}{(1 + r)^2}S_2^*}_{\text{mechanical effect}} + \underbrace{\sum_{t=0}^{T}\frac{1}{(1 + r)^t}\left(\frac{\partial S_t^*}{\partial b_1} - \frac{\partial S_t^*}{\partial b_2}\right)(\tau w + b_t)}_{\text{behavioral effect}}.$$

The first part is a mechanical spending increase that always deteriorates the budget in case of front-loading. The second part is the effect of behavioral change on the budget. Since the effect of front-loading on job search is ambiguous, the sign of the behavioral effect on the government's budget is also ambiguous. If job search intensity increases in response to front-loading, then the sign is positive as unemployment benefit payments fall and tax revenues increase. Moreover, if the increase in job search is large enough, the behavioral response can potentially offset the mechanical cost increase, and the budget might improve after front-loading.

Finally, in determining the welfare implications of UI in this model, we make an important simplifying assumption motivated by our empirical findings: taxes do not need to be increased in order to finance the benefit front-loading.[8] As usual, we measure the welfare of the unemployed by their expected utility at the initial period.

[8] If the budget balance worsens after front-loading, tax hikes would be needed to finance the policy change. It is easy to see that front-loading cannot be a Pareto improving policy change if taxes are increased. In that case, the marginal utility of consumption at work and in unemployment should be taken into account in the welfare analysis (see Kolsrud et al. 2018 for the details).

Using the envelope condition again, we get the following expression for the effect of front-loading on welfare:

$$\Delta V_0^u = u'\left(c_1^{u^*}\right) - \delta\left[\left(1 - s_1^*\right)u'\left(c_2^{u^*}\right)\right] \geq 0.$$

Note that the Euler equation ensures that the change in welfare is (weakly) positive, and the inequality holds strictly if there is a positive job search in equilibrium $s_1^* > 0$. These results highlight that front-loading does indeed improve the welfare of the unemployed.

Welfare Effects in Other Models: We can also assess the welfare implications of front-loading in a more general framework (but maintaining the assumption that there is no need to increase taxes). The following proposition shows that one can derive the welfare effects of front-loading by making minimal assumptions about the structure of job search.

PROPOSITION 1: *Suppose the government institutes a benefit change that front-loads the unemployment benefit payments by increasing the benefit in the first N periods and decreasing it in subsequent periods such that the total benefit entitlement throughout the unemployment spell remains unchanged (formally,* $\sum_{k=1}^{\infty} \Delta b_k = 0$*).Assume that (i) the unemployed do not have saving constraints, (ii) the actual behavior of the unemployed reveals their (normative) preferences, and (iii) the unemployed's preferences are not directly affected by the UI benefit level. Then benefit front-loading (weakly) improves the welfare of the unemployed.*

PROOF:

The proposition is a simple consequence of the observations that if $\sum_{k=1}^{T} \Delta b_k = 0$, the unemployed can simply undo front-loading by saving the extra money collected in the first N periods and consuming it when the benefit drops. As a result, the old consumption profile, search behavior, and reservation wage are still feasible under the front-loaded benefit profile. If the benefit level does not enter into the utility function directly, then persisting with prereform behavior has no impact on welfare. Moreover, if a new consumption profile, search effort, or reservation wage is chosen even if the old was feasible, this reveals that the unemployed are better off following the benefit change. ∎

Proposition 1 highlights that front-loading makes some unemployed better off without detriment to others. Moreover, if no tax increase is required to fund the UI budget (which we find to be the case empirically in Hungary), front-loading is a Pareto improving policy change: it makes some agents better off without negatively affecting others.

We note the following two points. First, the proof of Proposition 1 relies on *revealed preference* reasoning, and we do not need to make any further assumptions about the structure of job search or about the size of the policy change (e.g., we do not need the envelope condition as in Chetty 2008 and Kolsrud et al. 2018). Even if there is duration dependence in job finding rates or unobservable heterogeneity in assets, ability, or preferences, the unemployed cannot be made worse off under the new rules as every type of worker could choose the same consumption, search, and reservation wage as before.

Second, the conditions needed for Proposition 1 are quite weak and hold in most theoretical models in the optimal unemployment insurance literature (see, e.g., Shavell and Weiss 1979, Werning 2002, Shimer and Werning 2008).[9] Nevertheless, the condition that the preferences of the unemployed are not directly affected by the UI benefit level is violated in certain job search models with nonstandard preferences. DellaVigna et al. (2017) propose a job search model with reference-dependent preferences, with loss aversion relative to recent income (the reference point). In this model, the benefit level enters the utility function directly by affecting recent income. This model, similarly to the standard job search model, predicts that the unemployed search more as a result of front-loading, and the effect on the government's budget is ambiguous.

However, the welfare implications of front-loading are more complicated in DellaVigna et al. (2017). Even if the unemployed worker replicates his old consumption-search effort profile, their welfare can fall due to the direct effect of the benefit change on the reference point. The size and magnitude of such a direct effect depend on the extent of loss aversion and on the speed at which the worker updates his reference point. To understand the welfare implications in the reference-dependence type of search model, we take the estimated parameters from DellaVigna et al. (2017) and simulate the effect of front-loading on welfare (measured by the value of unemployment at the beginning of the UI spell) under various front-loading scenarios. We vary the length and the size of the first period increase and ensure that the initial increase is compensated by a similarly sized drop in the second period. Under all simulated scenarios—including one corresponding to the Hungarian reform—we find that front-loading increases welfare. This highlights that front-loading is welfare-improving even in the presence of reference dependence (see the details in online Appendix Figure A.1).

To sum up, front-loading has an ambiguous effect on job search effort and on government spending. Moreover, when the behavioral responses to the policy are large enough to offset the mechanical cost increase on the government's budget, benefit front-loading is a Pareto improving policy change as some unemployed are made better off while others are unaffected. This highlights that it is important to evaluate the budget consequences of front-loading, which we will do next.

[9] All models in this literature assume that agents choose optimally, and so their choices reveal their true preferences. Some papers assume hand-to-mouth consumers, mainly because of tractability (see, e.g., Hopenhayn and Nicolini 1997). Since hand-to-mouth agents cannot undo front-loading, these unemployed could be potentially made worse off by front-loading. Nevertheless, in the online Appendix, we show that a small benefit front-loading from a constant benefit profile will still improve the welfare of hand-to-mouth unemployed workers in the type of job search models presented above. The intuition for this result is simple. For hand-to-mouth agents, the change in utility is directly related to the change in the benefit level in each period. When we change marginally from a constant benefit profile to one with front-loading, the marginal utility of the US$1 increase at the beginning is the same as the marginal utility of US$1 loss at the end of the UI. However, the marginal utility increase at the beginning is valued more than the same-size loss in the marginal utility at the end because there is a chance that the unemployed worker will find a job before reaching the end of their benefit entitlement.

II. Institutional Background and Data

A. *The Benefit Reform in Hungary*

Hungary had a two-tier unemployment insurance system during 2005. In the first tier, the unemployment benefit depended on the length of an individual's working history and his average monthly taxable income.[10] After exhausting the first tier, the unemployed were eligible for additional unemployment assistance. The benefit in the second tier was the same for all unemployed workers, and the duration depended on the age of the claimant. After both tiers were exhausted, the unemployed were eligible for a welfare payment in perpetuity. However, these 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 first tier of the benefit schedule dramatically for those who claimed benefits after November 1, 2005, while it preserved the length of eligibility for first-tier benefit. In online Appendix Figure A.2, we show the new and old benefit schedules 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 are individuals who worked essentially uninterruptedly in the four years before their job loss and whose (monthly) earnings were above 108,000 forint (US$504) in 2005 (around the seventieth percentile of UI claimants). Figure 1 summarizes the benefit path for this group before and after the reform. Unemployed individuals who claimed benefits before November 1 were eligible for 44,460 forint (US$222) for the first 270 days. Those who claimed benefit after November 1 received 68,400 forint (US$342) in the first 90 days and 34,200 forint (US$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, and only the timing of the benefit payments changed.[11]

The UI reform was announced in March and was enacted two months later on June 27, 2005. The stated goal of the Hungarian government was to "provide a more generous government support and greater personal interest in finding a new job in the shortest possible time." In practice, a large fraction of the unemployed (those with lower UI base earnings) were made worse off by the reform as their benefits were cut. However, for the group that we study here—the unemployed with previously stable employment and monthly earnings above 108,000 forint (US$504)—the reform increased generosity and incentives at the same time.

The reform affected all unemployed workers who claimed UI after November 1, 2005. Therefore, after the reform was enacted in June, firms and workers had three months to prepare. In principle, this time window allowed firms and workers to alter the timing of their layoffs and claiming decisions. However, whether an individual is better or worse off under the new system depended on the length of working history and the average monthly taxable income, and so determining the optimal

[10] The length of eligibility was the number of working days in the last 4 years divided by 5, and it was capped at 270 days. The benefit schedule is shown in online Appendix Figure A.2.

[11] The total UI benefit was 2.4 percent higher in the new system.

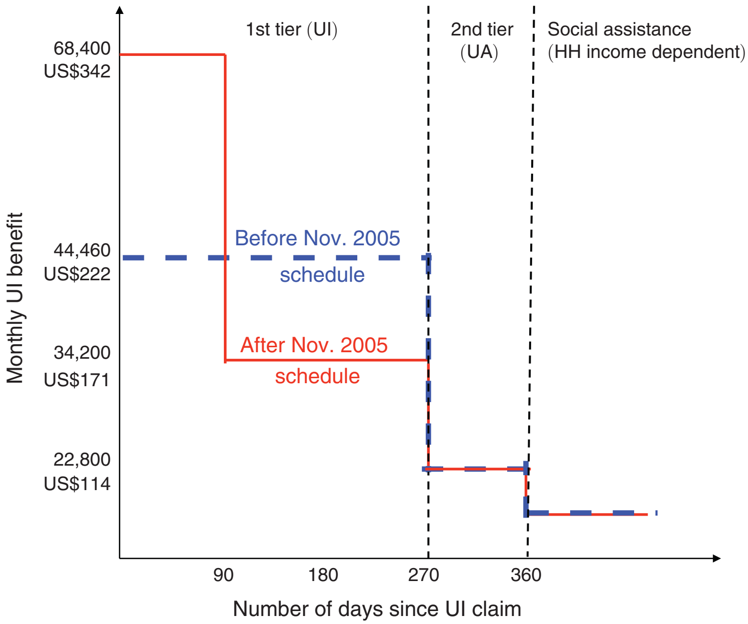

Figure 1. The Benefit Path before and after the Reform

Notes: The figure shows the benefit schedule if UI is claimed between February 1, 2005 and October 31, 2005 (old benefit schedule, dashed blue line) and the benefit schedule if UI is claimed after November 1, 2005 (new benefit schedule, solid red line) for individuals whose potential benefit duration is at the maximum (270 days) in the first tier, who were less than 50 years old, and who earned more than 114,000 forint (US$570) prior to entering the UI scheme. Benefit levels in social assistance are approximate as they depended on family income, household size, and wealth. This figure also appears in DellaVigna et al. (2017, see panel (a) in figure II).

timing required detailed knowledge of both the old and new UI systems.[12] In online Appendix Figure A.3, panel (a), we show that the number of claimants in October was slightly lower than in November (695 versus 774), but such a small variation between months is not unusual in other years. Moreover, panel (b) highlights that the average number of days between job loss and claiming the benefit was slightly lower in October than in November in 2005, but the same difference was present in 2004. Therefore, we find no indication that unemployed workers retimed their claiming decision as a result of the reform.[13]

Newly unemployed individuals who wished to collect unemployment benefits had to visit the local UI office and attend a 30-minute information session that explained their rights and obligations as a claimant. Then each individual received a personalized letter that characterized their benefit schedule in the first tier. Online Appendix Figure A.4 shows an example of the first page of such a letter for an unemployed

[12] Finding the relevant rules and the way the government calculated benefit eligibility was hard even for us. However, once an unemployed worker claimed UI, the government calculated their eligibility and explained their entitlement and the structure of payments to them in a personalized letter, as shown in online Appendix Figure A.4. This means that while the benefits of retiming may have been difficult to understand, unemployed workers are likely to have been aware of their benefit schedule once they claimed UI.

[13] Figure 4, panel B formally tests the presence of timing manipulation by implementing McCrary's (2008) test and finds no evidence of UI claims retiming.

individual who claimed benefit under the new rules. The daily benefit amount and length of the disbursement period are highlighted in the table in the middle of the page. This letter made salient the whole benefit profile from the beginning of the unemployment spell.

There were two other changes implemented in 2005. First, unemployment assistance (UA, the second benefit tier) was shortened from 180 days to 90 days for those who claimed benefit after February 5, 2005. Second, the government introduced a voluntary reemployment bonus scheme in parallel with the benefit reform. Under this new scheme, the unemployed who claimed benefit after November 1, 2005 and found a job in the first 270 days could claim 50 percent of the remaining unemployment benefit as a lump sum.

Only 11.1 percent of the unemployed who found a job in the first 270 days took advantage of the reemployment bonus scheme. The low take-up rate partly reflects that the default option was to not claim the reemployment bonus (Madrian and Shea 2001) and also that information about the reemployment bonus scheme was not easy to understand.[14] Moreover, claiming the reemployment bonus had two important drawbacks that may have prevented people from using it. First, claimants needed to go through a burdensome administrative process, which involved waiting for 270 days after the benefit claim and then submitting a lengthy application in person at the local UI office. Second, claiming the reemployment bonus also meant that the unused benefit eligibility was reset to zero.[15] This may have seemed a risky step to take for many recently unemployed workers who are often on probation periods. In Section III, we show that the changes in nonemployment duration at the time of the reforms were not driven by the shorter UA benefits in the second tier or by the voluntary reemployment bonus scheme.[16]

Finally, it is worth highlighting that the economy was growing at 3 to 4 percent a year before the reform and a somewhat lower rate afterward (see online Appendix Figure A.5, panel (a)). Nevertheless, aggregate labor market conditions were not affected by the lower performance of the economy, and unemployment was stable in the periods studied (see online Appendix Figure A.5, panel (b)).[17]

B. *Database and Sample Definition*

We observe a 50 percent random sample of the unemployed registered by the Hungarian National Employment Service between mid-March 2004 and mid-August 2008.[18] During this time period, we have information on the start and end dates of the

[14] Since we use administrative data, the low take-up rate is not due to underreporting or measurement error.

[15] For instance, someone who is eligible for 270 days of UI benefits but finds a job after 180 days still has 90 days of unused benefit. If this person is laid off a few months later, she can claim unemployment benefits again for these 90 days. However, if she had claimed the reemployment bonus, she would lose the remaining 90 days of eligibility and would not be able to claim UI benefits.

[16] The shorter UA (90 days instead of 180 days) is only relevant for those who claimed benefits before February 5, 2005. We do not see changes in nonemployment duration around that date.

[17] The lower GDP growth rate would predict higher nonemployment duration after the reform. However, in Section III, we show that the average length of nonemployment was in fact lower after the reform. Therefore, if the lower GDP growth rate had any effect on our results, then we are likely to underestimate the true effect of the reform.

[18] The sample includes individuals who were born every other day after January 1, 1927.

benefit provision and the UI base earnings, which determine the level of UI benefit. We also observe social security contributions between 2002 and 2008, which we use to infer employment history and earnings. This allows us to calculate nonemployment durations as the time elapsed between claiming UI benefit and starting a new job.

We restrict attention to prime age workers (25–49 years old) who had the maximum benefit eligibility (270 days). We focus on workers whose monthly UI base earnings were above the seventieth percentile of UI claimants in a given year (i.e., 108,000 forint in 2005). The UI base earnings are the average monthly taxable earnings without severance payments in the 12 months leading up to the benefit claim. These are the unemployed whose benefit changed according to Figure 1. In our benchmark analysis, we use the largest possible sample, which consists of UI claimants who claimed benefit between March 15, 2004 and August 15, 2007. This period covers 20 months before and 23 months after the reform. We drop unemployed who claimed benefit 15 days before and 15 days after November 1, 2005 to ensure that potential retiming of claims does not influence our estimates.[19]

The basic descriptive statistics are shown in Table 1.[20] We observe approximately 14,500 unemployed individuals both before and after the reform. The observable characteristics of the two groups are very similar. The share of women, the average years of schooling, and the average UI base earnings (relative to the average wage in the economy) are slightly higher in the period after the reform, but these differences are small in economic terms. The table also highlights that around 6 percent of the unemployed and 11.1 percent of the eligible population claimed the reemployment bonus. The descriptive statistics are similar to the full sample if we consider only a five-month interval around the reform: the average differences in sample characteristics before and after the reform are very small and are not statistically significant.

III. Results

A. *The Effect of the Reform on Nonemployment Duration*

In this section, we evaluate the impact of the reform on nonemployment duration and on the quality of jobs found. To estimate the effect of the reform on various outcomes, we implement a regression discontinuity design. More specifically, we follow Lee and Lemieux (2010) and estimate the following regression equation:

$$NonEmpDur_i = \alpha + \beta after_i + \gamma X_i + f(T_i) + \varepsilon_i, \tag{2}$$

[19] Our results are robust to including the periods between October 15, 2005 and November 15, 2005. Moreover, as described above, we do not find evidence that unemployed workers retimed their claiming behavior as a result of the reform.

[20] In online Appendix Table A.1, we also report the descriptive statistics separately for the locations with low reemployment bonus take-up rates.

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

	Full sample				Five-month window around the reform			
	Before	After	Diff.	*t*-stat	Before	After	Diff.	*t*-stat
Observed for all unemp.								
Percent women	0.42	0.44	0.01	2.56	0.43	0.42	−0.004	−0.33
	(0.004)	(0.004)			(0.008)	(0.008)		
Age in years	37.1	36.9	−0.16	−2.00	37.1	37.26	0.16	0.97
	(0.06)	(0.06)			(0.113)	(0.119)		
Education (years)	11.88	12.17	0.28	10.62	11.99	11.95	−0.05	−0.84
	(0.02)	(0.02)			(0.037)	(0.039)		
UI base/average wage	0.98	1.04	0.06	9.62	0.99	0.98	−0.004	−0.37
	(0.004)	(0.005)			(0.008)	(0.009)		
Waiting period[a]	29.6	30.0	0.39	0.89	30.27	31.73	1.46	1.64
	(0.31)	(0.3)			(0.612)	(0.648)		
Fraction claimed reemp. bonus	n.a.	0.06			n.a.	0.056		
		(0.002)				(0.004)		
Fraction of eligible unemp. claimed reemp. bonus	n.a.	0.11			n.a.	0.099		
		(0.01				−0.007		
Nonemployment duration (in days)	239.18	224.95	−14.23	−9.39	237.83	224.28	−13.56	−4.48
	(1.07)	(1.07)			(2.09)	(2.19)		
Observations	14,288	14,762			7,264	6,727		
Observed if reemployed								
Prob. new job lasts more than one year	0.81	0.81	0.002	0.03	0.8	0.8	0.002	0.16
	(0.004)	(0.004)			(0.008)	(0.009)		
Reemployment wage	−0.15	−0.13	0.021	2.85	−0.16	−0.14	0.018	1.26
	(0.005)	(0.005)			(0.01)	(0.01)		
Observations	8,562	9,098			2,286	2,170		

[a]Number of days between job loss and UI claim.

where the dependent variable is the time elapsed between the UI benefit claim and the date of reemployment. In this regression, we cap the length of unemployment at 360 days because, due to the time period covered by our data, we cannot observe nonemployment durations longer than that after the reform for some claimants. However, capping at a different 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. We also report estimates with the inclusion of a rich set of control variables X_i. The control variables are age and its square, years of education and its square, log income in 2002 and 2003, sex, dummies for the day of the month the benefit was claimed, one-digit occupation and location dummies, and flexible time trends, $f(T_i)$, at each side of the reform.[21]

[21] In the full sample, we estimate a separate third-order polynomial time trend before and after the reform. In the regressions using the optimal bandwidth or short bandwidth, we estimate separate kernel-weighted local linear regressions before and after the reform.

Table 2—Effect of the Reform on Nonemployment Duration and Job Quality

	Full sample		Optimal bandwidth		Short bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Nonemployment duration (number of days)						
After	−14.24	−13.98	−16.67	−15.99	−20.57	−19.78
	(1.49)	(1.51)	(4.85)	(4.85)	(5.38)	(5.40)
R^2	0.003	0.056				
Observations	29,050	29,050	13,991	13,991	7,183	7,183
Bandwidth	20 before	20 before	10 before	10 before	5 before	5 before
	23 after	23 after	10 after	10 after	5 after	5 after
Panel B. Probability of the new job lasting more than a year						
After	0.0002	−0.004	−0.052	−0.05	−0.04	−0.037
	(0.006)	(0.007)	(0.037)	(0.038)	(0.025)	(0.025)
R^2	0.0002	0.033				
Observations	17,787	17,787	1,773	1,773	909	909
Bandwidth	20 before	20 before	2 before	2 before	1 before	1 before
	23 after	23 after	2 after	2 after	1 after	1 after
Panel C. Reemployment wage: log(reemployment wage/UI base earnings)						
After	0.021	0.014	0.090	0.092	0.071	0.069
	(0.007)	(0.007)	(0.05)	(0.051)	(0.034)	(0.034)
R^2	0.0001	0.074				
Observations	17,660	17,660	1,765	1,765	903	903
Bandwidth	20 before	20 before	2 before	2 before	1 before	1 before
	23 after	23 after	2 after	2 after	1 after	1 after
Controls	No	Yes	No	Yes	No	Yes
$f(T_i)$	No	3rd poly	Kernel	Kernel	Kernel	Kernel

Notes: This table shows the effect of the reform on nonemployment duration (panel A), on the probability that the new job lasts more than a year (panel B), and on reemployment wages (panel C). Columns 1 and 2 use the full sample, and columns 3 and 4 restrict the sample to the optimal bandwidth, while columns 5 and 6 use a half of the optimal bandwidth. In panel A, the nonemployment duration is capped at 360 days in all columns. In panels B and C, only workers who found a job in 360 days are included in the sample. "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, education, occupation (one digit) in the last job, and log earnings in 2002 and 2003. Columns 1 and 2 also include 12-month dummies and location fixed effects in the regression. "Bandwidth" shows the number of before and after months used in the estimations. The standard errors in parentheses are clustered at the local UI office level.

We conduct our analysis using data from three differently sized windows around the reform date. The first bandwidth is chosen to maximize our sample size and minimize the standard errors around our estimates. As mentioned in Section IIB, this sample selection leads us to cover 20 months before and 23 months after the reform. Second, we present estimates using the optimal bandwidth procedure of Imbens and Kalyanaraman (2012). Third, we present estimates using only half of this optimal bandwidth.

Panel A of Table 2 summarizes the main findings. According to column 1, the length of nonemployment fell by 14.24 days (SE 1.49) after the reform. In column 2, we include controls for individual characteristics and find a slightly lower effect of the reform (a reduction of 13.98 (SE 1.51) days instead of 14.24), reflecting that the characteristics of UI claimants differ slightly before and after the reform.

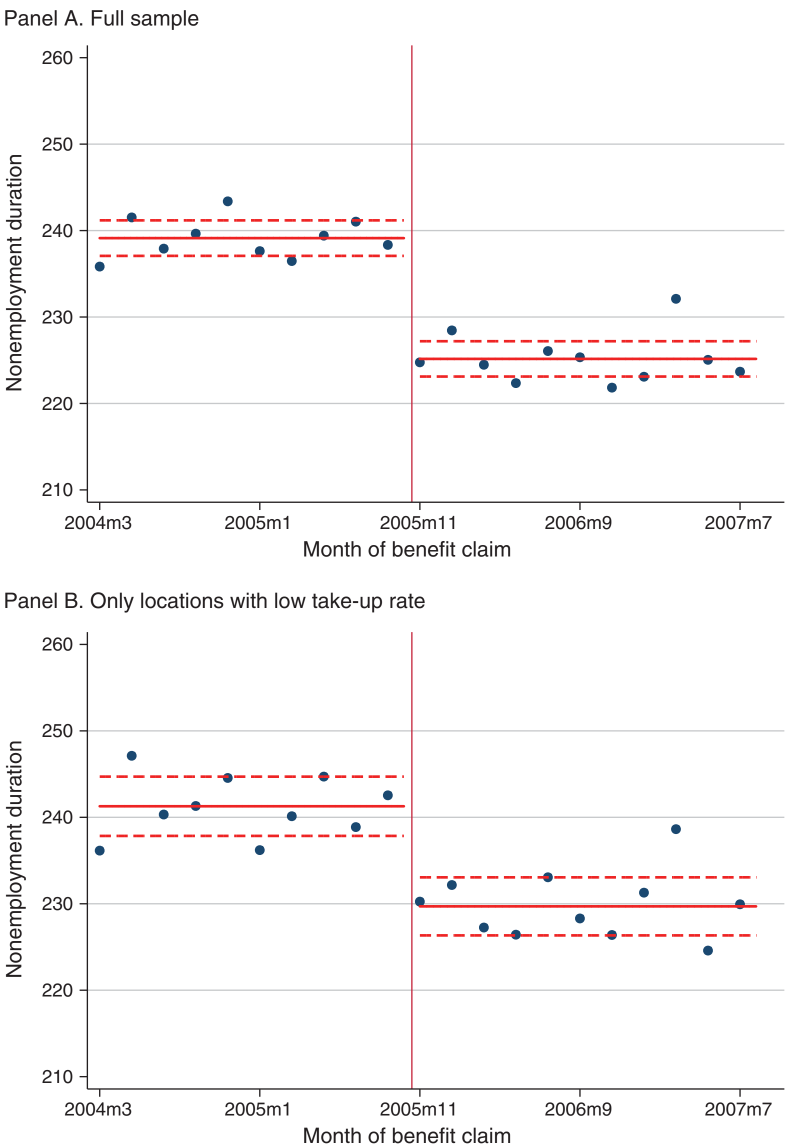

FIGURE 2. BASELINE RESULTS: NONEMPLOYMENT DURATION BY THE DATE OF BENEFIT CLAIM

Notes: The figure shows average length of nonemployment spells in two-month periods (blue dots) and the kernel-weighted local polynomial smoothing (red line). Panel A shows the results on the full sample, while panel B uses only locations where the reemployment bonus take-up rate was lower than the median (6 percent). Both panels control for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, education, occupation (one digit) of the last job, and log earnings in 2002 and 2003 and the month of the year. The vertical red lines show the timing of the reform. The figure highlights that the average length of nonemployment duration dropped immediately after the reform.

Figure 2, panel A shows the average nonemployment duration by the time of claiming UI benefits. To take into consideration that claimants in different months vary in their observable characteristics, we report the averages conditional on the full set of covariates X_i.[22] We also report a kernel-weighted local polynomial (red line) that is estimated separately before and after the reform. The figure highlights that the change in nonemployment duration coincided with the introduction of the new benefit schedule: the average nonemployment duration was around 239 days for those who claimed benefit before the reform and fell to 226 days immediately for those who claimed after November 1, 2005. The figure also shows that the average length of nonemployment was very similar throughout the period preceding the reform, which indicates that our results are not driven by preexisting trends. Moreover, the lack of a change in nonemployment duration around February 5, 2005 indicates that the small change in the second tier introduced on that date does not affect the results presented here.

In columns 3 and 4 of Table 2, we report the point estimates with the optimal bandwidth. The decrease in nonemployment duration is even larger, around 16.67 days (15.99 when we include controls), if we restrict our analysis to 10 months before and after the reform. This increases further to around 20 days when we use a shorter bandwidth of 5 months before and after the reform (columns 5 and 6). While the point estimates are larger (in absolute value), if we focus on the months closer to the reform, the standard errors are also larger since the sample size shrinks. As a result, the estimates using alternative bandwidths are not statistically different from each other at the conventional levels.

Figure 3, panel A provides additional evidence on the effect of the reform on nonemployment duration. The figure shows the Kaplan-Meier survival rate in nonemployment for those who claimed the benefit before the reform (between March 15, 2004 and October 15, 2005) and after it (between November 15, 2005, and August 15, 2007). In the first 60 days, the two survival functions are very similar. But then the survival rates move apart, with the survival rate dropping more in the period after the reform than before it, indicating that unemployed workers found jobs more quickly after the reform. The difference in survival rates widens up to 270 days, and then it starts to shrink. However, a small difference between survival rates remains even after 400 days. We will use these estimates of the survival rates when we calculate budget consequences of this reform in Section IIID. In online Appendix Figure A.6, we also report the hazard rate to employment. In line with the evidence on the survival rates, after the reform, the hazards are higher between 70 and 200 days but lower between 250 and 350 days. The effect of the reform on hazard rates is also explored further in DellaVigna et al. (2017), where a job search model is estimated to fit the observed hazard rates.

In panel A of Table 3 and in online Appendix Table A.2, we also explore heterogeneous responses to front-loading. We find no indication that responses differ by gender or education level. However, older unemployed and those living in regions where the unemployment rate is lower responded more to the reform. The latter is in line with existing literature finding that the unemployed are more responsive to changes in benefits when unemployment is low (see, e.g., Kroft and Notowidigdo 2016; Schmieder, von Wachter, and Bender 2012). Finally, we also find stronger

[22] The results are very similar if we use the raw averages of the nonemployment durations.

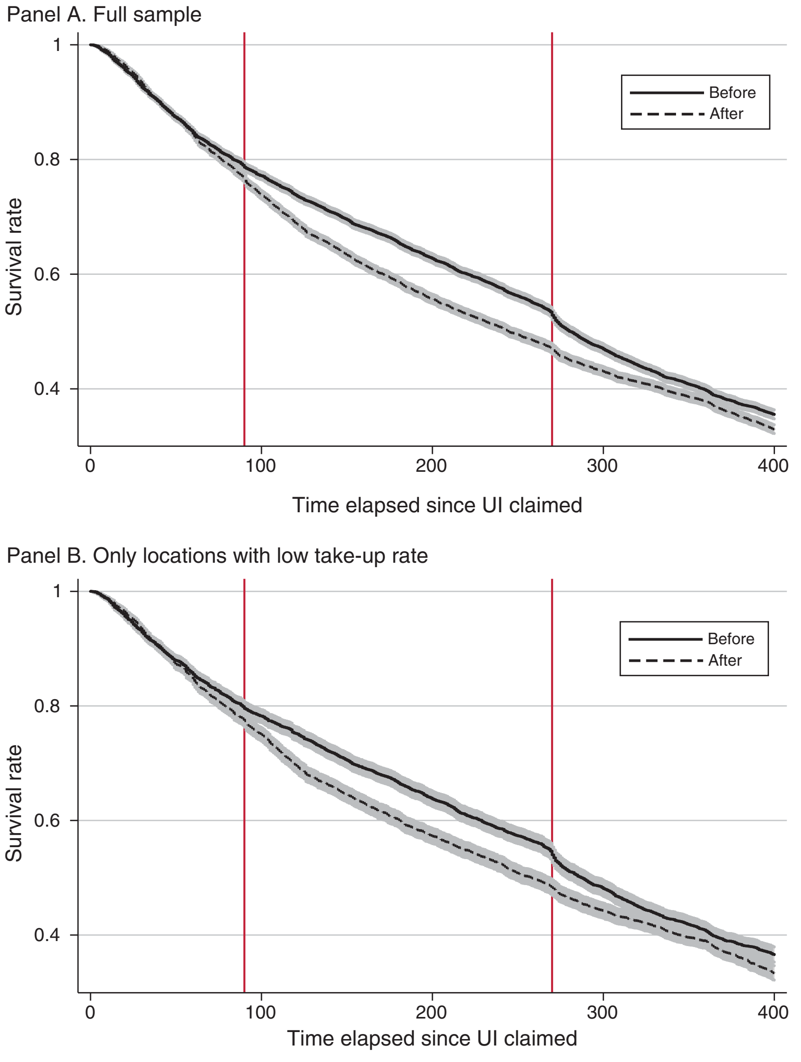

Figure 3. Kaplan-Meier Survival Rates before and after the Reform

Notes: The figure shows the Kaplan-Meier survival rates of the unemployed before and after the reform. Panel A shows the results on the full sample, while panel B uses only locations where the reemployment bonus take-up rate was less than the median (6 percent). The vertical red lines show the drop in the benefits after the reform at 90 and 270 days. The shaded area shows the confidence intervals of the survival estimates.

responses to front-loading in areas where average income is higher (as measured by higher UI base earnings for an average claimant).

Our identification strategy relies on three crucial assumptions. First, we assume that unemployed workers do not bring forward (or delay) their benefit claims as a result of the reform. If this was the case, we would expect the number of benefit claims to be significantly larger (or smaller) during the months before the reform than in the months after it. We test for retiming of benefit claims using McCrary's

Table 3—Heterogeneity in the Effect of the Reform

	Gender		Age between		Local unemployment rate[a]		Share of unemployed with high UI base[b]	
	Women	Men	25–40	40–55	Above median	Below median	Above median	Below median
Panel A. Nonemployment duration								
After	−13.67	−14.50	−12.81	−17.41	−12.66	−15.45	−15.55	−11.39
	(2.152)	(2.157)	(1.877)	(2.567)	(2.905)	(1.372)	(1.611)	(2.875)
R^2	12,561	16,391	19,141	9,811	13,157	15,795	17,606	11,297
Observations	0.066	0.063	0.06	0.071	0.063	0.052	0.051	0.067
Bandwidth	20 before	20 before	20 before	20 before	20 before	20 before	20 before	20 before
	20 before	23 after	23 after	23 after	23 after	23 after	23 after	23 after
Panel B. Reemployment wage: $log(reemployment\ wage/UI\ base\ earnings)$								
After	−0.00	0.0239	0.025	0.001	0.006	0.027	0.019	0.005
	(0.012)	(0.011)	(0.009)	(0.01)	(0.011)	(0.01)	(0.009)	(0.011)
R^2	4,636	5,870	6,938	3,568	5,050	5,456	5,072	5,434
Observations	0.062	0.059	0.056	0.07	0.055	0.057	0.06	0.048
Bandwidth	20 before	20 before	20 before	20 before	20 before	20 before	20 before	20 before
	20 before	23 after	23 after	23 after	23 after	23 after	23 after	23 after
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(T_i)$	3rd poly	3rd poly	3rd poly	3rd poly	3rd poly	3rd poly	3rd poly	3rd poly

Notes: This table shows the effect of the reform on nonemployment duration (panel A) and on reemployment wages (panel B) for various subgroups. In all specifications, we use the full sample, and we control for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003, 12 month dummies, and location fixed effects (so we use the same controls as in specifications reported in column 2 in Table 2). In panel A, the nonemployment duration is capped at 360 days in all columns. In panel B, only workers who found a job in 360 days are included in the sample. "After" is a dummy, which is 1 if the unemployed individual claimed benefit after the benefit reform. “Bandwidth” shows the number of before and after months used in the estimations. The standard errors in parentheses are clustered at the local UI office level.

[a] The average unemployment rate is 3.3 percent below the median and 9.3 percent above the median.

[b] The average share of unemployed with high benefit base is 22.6 percent below the median and 40.2 percent above the median.

(2008) method and present the results in Figure 4, panel A. The figure shows that the number of benefit claims are roughly constant around the time of the reform. Similarly, the McCrary test statistic (0.044, SE 0.046) is not significantly different from zero, which suggests that there was no systematic retiming of benefit claims as a result of the reform.

Our second assumption is that the composition of the unemployed did not change over time. For instance, if those unemployed workers who are able to find a job more quickly are overrepresented after the reform, our results would overstate the effect of the reform on reducing unemployment duration. To test this hypothesis, we estimate how the expected nonemployment duration changed over time based on observable characteristics. First, we use the before sample to regress the individual level characteristics on the actual nonemployment duration (capped at 360 days). Second, we use the estimated parameters to predict the expected nonemployment duration for every individual. Figure 4, panel C plots the average predicted nonemployment duration by the time of the benefit claim. The figure shows that the effects we find are not due to selection among the

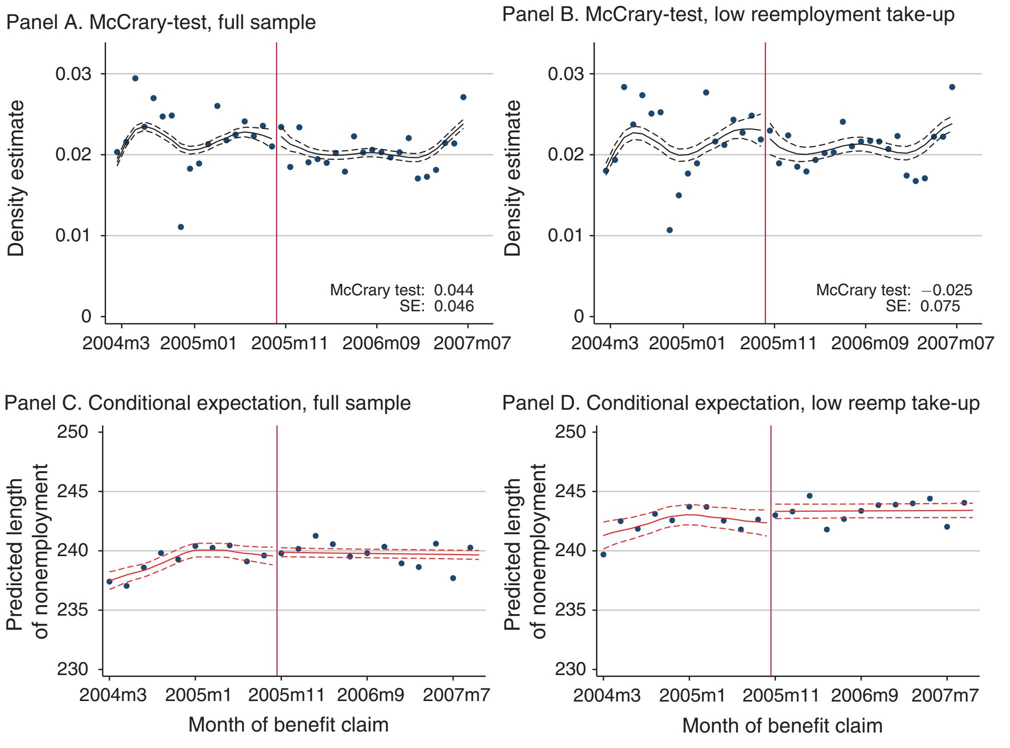

Figure 4. Density of UI Claimants and Expected Length of Nonemployment Duration

Notes: These figures provide a formal test of sorting around the reform. Panels A and B show the density of UI claimants by the date of claiming and test for manipulation using McCrary (2008) for the full sample and for the sample using locations with low reemployment bonus take-up rate, respectively. Panels C and D show the expected nonemployment duration by the date of benefit claim for the full sample and for the sample using locations with low reemployment bonus take-up rate, respectively. We calculate the expected nonemployment duration by regressing nonemployment duration (capped at 360 days) on sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, education, occupation (1 digit) of the last job, and log earnings in 2002 and 2003 using the before reform sample. Then we predict nonemployment duration and plot the predictions for two-month periods. The vertical red lines show the timing of the reform. The figure indicates that no manipulation occurred around the reform.

unemployed: the expected nonemployment duration is constant over time, and there is no break at the time of the reform.[23]

The third important assumption is that the effect of the reform is driven by the change in the benefit schedule and not some other contemporaneous change. We will present evidence in Section IIIC to support this assumption.

[23] In online Appendix Figure A.7, we also show the histogram of estimates with alternative placebo timings of the reform. The histogram shows that the estimates around placebo reform dates are often smaller, which underlines that we capture here the effect of the reform and not something else.

B. *The Effect of the Reform on Job Quality*

Did the faster reemployment reduce job quality? On one hand, faster reemployment might reflect some unemployed individuals rushing into lower-quality jobs. But, on the other hand, the shorter unemployment spell could raise reemployment wages if workers' skills deteriorate or job opportunities are worsened during unemployment. In this section, we analyze the effect of the reform on job tenure at the new job and on reemployment wages.

In panel B of Table 2, we estimate the effect of the reform on the probability that the new job lasts at least one year. The point estimates in columns 1 and 2 are close to zero, suggesting that there is no significant change in separation rates at the new job after the reform. Figure 5, panel A shows the effect on job tenure by the timing of the benefit claim. The probability that the new job lasts a year is around 80 percent, and there is no significant change around the introduction of the benefit change. We explore the robustness of these estimates to alternative bandwidth selection in columns 3 to 6 in Table 2, panel B. The optimal bandwidth is only two months when the outcome variable is the job tenure at the new job. Under alternative bandwidth selections, the estimated effects on the probability that the new job lasts a year are negative, but these estimates are not significantly different from zero.[24]

In Table 2, panel C, we report the effect of the reform on daily reemployment earnings relative to the UI base earnings.[25] The estimates in column 1 indicate that reemployment wages were 2.1 percent (SE 0.07 percent) higher after the reform. The point estimate shrinks to 1.4 percent (SE 0.7 percent) when we control for observable characteristics, but the effect is nonetheless statistically significant at the 5 percent level. Figure 6, panel A plots the reemployment wages by the timing of the benefit claim. The figure shows that reemployment wages are around 15 percent below the UI base earnings on average, and they are more noisily estimated than nonemployment duration. Still, there is a clear upward shift in reemployment wages after the benefit reform. In columns 3 to 6 of Table 2, we report results using alternative bandwidth selections. The increase in reemployment wages is 9.2 percent (SE 5.1 percent) with the optimal bandwidth (2 months) and 6.9 percent (SE 3.4 percent) with the shorter bandwidth (1 month).[26] These point estimates are large compared to the previous findings in the literature, although the 90 percent confidence intervals include modest positive effects and the point estimates using the full sample (columns 1 and 2) as well.

[24] The tenure of the new job is unaffected even on a longer time horizon. In online Appendix Figure A.8, we show that the Kaplan-Meier survival rates of the new job are very similar before and after the reform even three years after reemployment.

[25] The UI base earnings reflect the average taxable earnings without severance payments in the last year before the benefit claim. In online Appendix Table A.3, we explore the robustness to alternative reemployment wage definitions, where we calculate the reemployment wages relative to the average earnings (including severance payments) in the last year (panel A) and relative to the average earnings in 2002 (panel B). All results presented in the main text hold under these alternative wage definitions.

[26] Surprisingly, the standard errors decline from columns 3 and 4 to 5 and 6 in panels B and C even though the sample sizes are lower. This drop in standard errors is caused by clustering at the level of the local UI office. Without clustering, the standard errors increase as we expect.

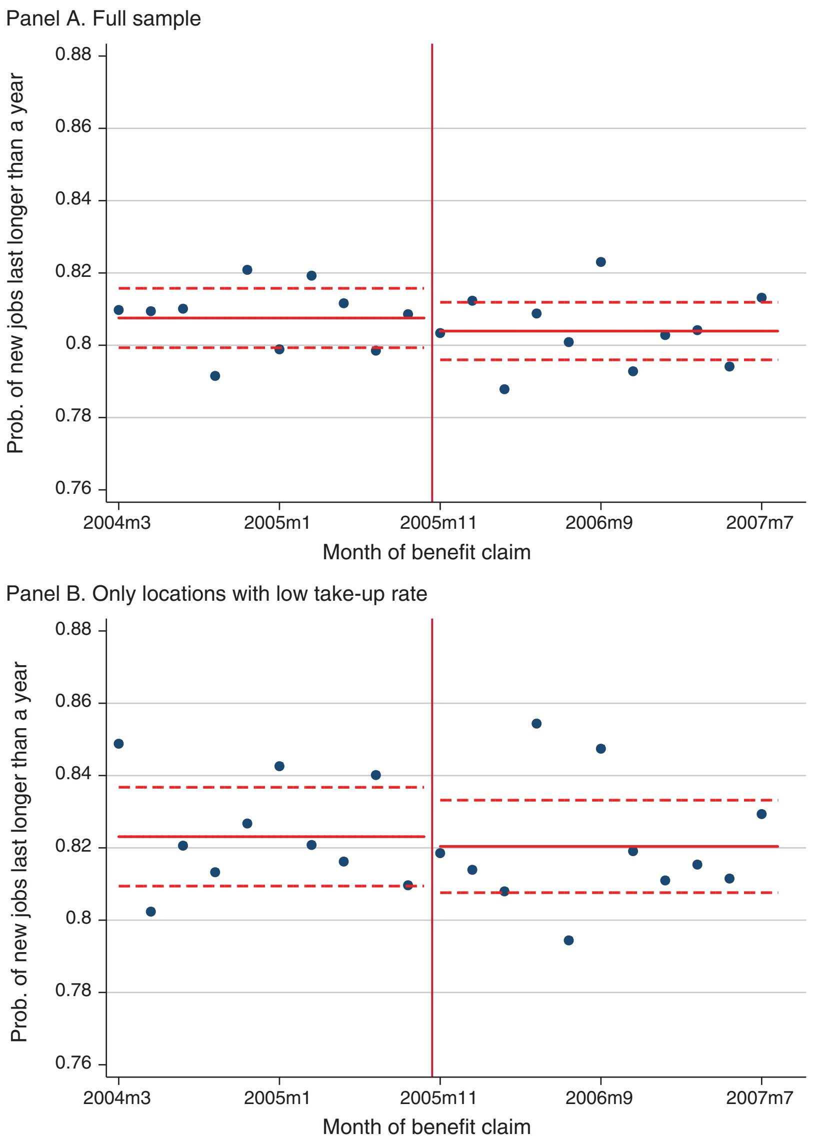

Figure 5. Job Quality: Job Tenure at the New Job before and after the Reform

Notes: The figure shows the probability of the new employment spells lasting at least one year by two-month periods (blue dots) and the kernel-weighted local polynomial smoothing (red line). Panel A shows the results on the full sample, while panel B uses only locations where the reemployment bonus take-up rate was lower than the median (6 percent). Both panels control 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 (one digit) in the last job, log earnings in 2002 and 2003, and the month of the year (e.g., January, February, etc.). The vertical red lines show the timing of the reform. The figure shows that the reform had no effect on job tenure.

In panel B of Table 3, we also explore heterogeneous responses to front-loading, although reemployment wages are more noisily estimated, and so it is harder to interpret those results. We find that male and young unemployed benefit more from the reform in terms of higher reemployment wages, which might reflect that the

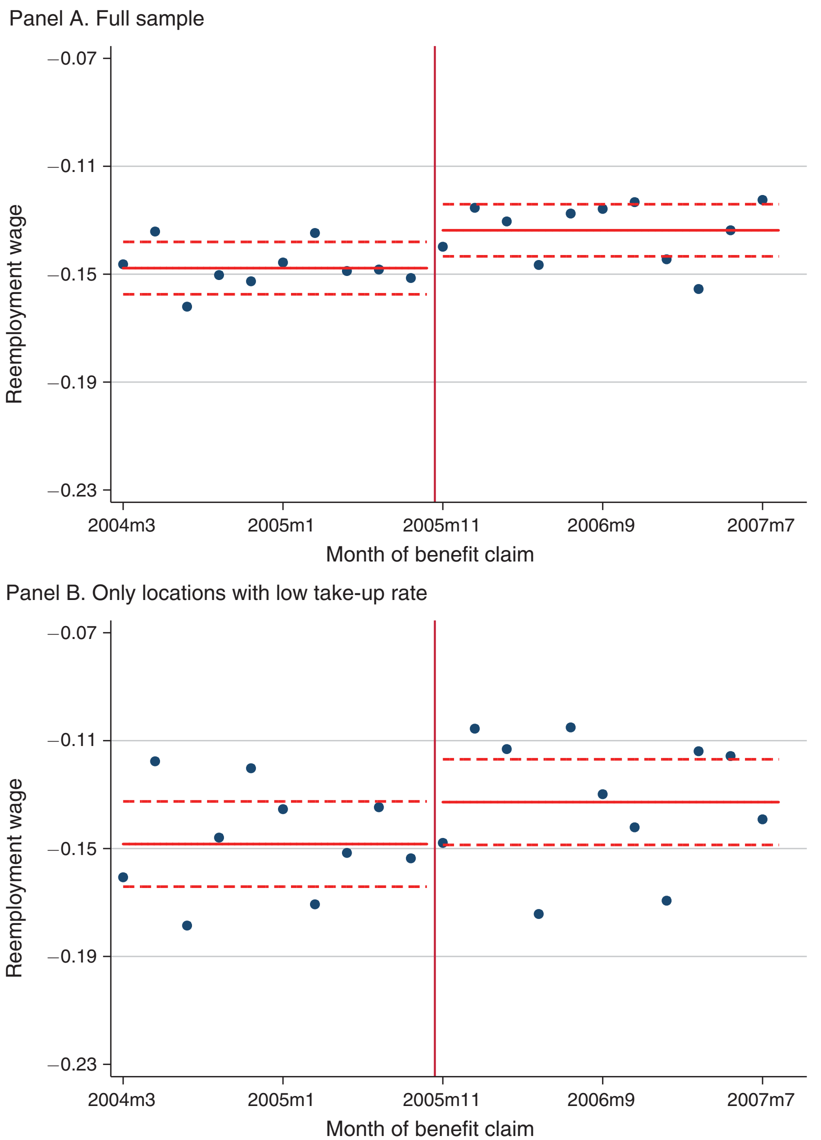

Figure 6. Job Quality: Reemployment Wages before and after the Reform

Notes: The figure shows the reemployment wage by two-month periods (blue dots) and the kernel-weighted local polynomial smoothing (red line). Panel A shows the results on the full sample, while panel B uses only locations where the reemployment bonus take-up rate was lower than the median (6 percent). Both panels control 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 (one digit) in the last job, log earnings in 2002 and 2003, and the month of the year (e.g., January, February, etc.). The vertical red lines show the timing of the reform. The figure shows that reemployment wages were slightly higher after the reform.

signaling value of longer unemployment is larger for these groups. Moreover, the positive reemployment wage effects mainly come from areas with low unemployment. These findings are in line with Kroft, Lange, and Notowidigdo (2013), who show that duration dependence in job recall rates is weaker in recessions.

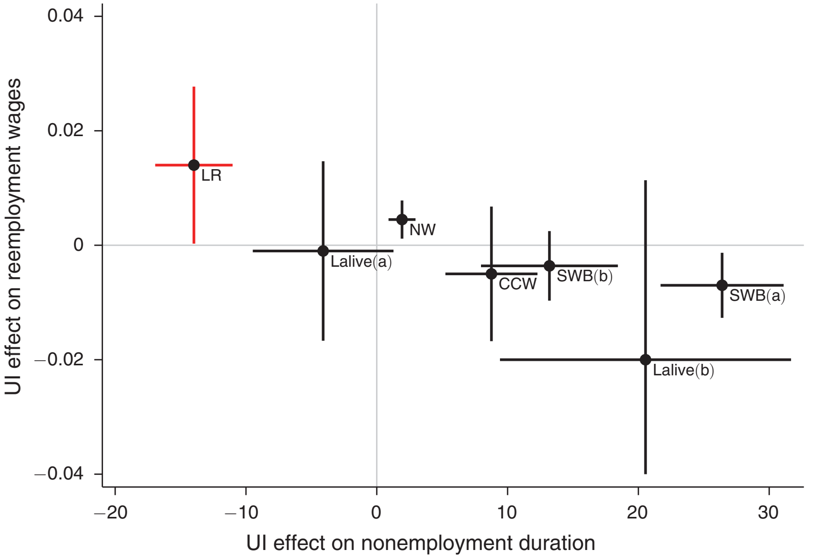

Figure 7. The Effect on Nonemployment Duration and Reemployment Wages in Relation to the Previous Literature

Notes: The dot with the LR label shows our estimates of the effect of the reform on nonemployment duration (*x*-axis) and on the reemployment wages (*y*-axis). We report the results from column 2, Table 2. The vertical and horizontal lines around the estimate show the 95 percent confidence intervals. We take the estimates from the literature from figure 4, panel A of Nekoei and Weber (2017).

Overall, our estimates suggest that reemployment wages increased significantly in response to the reform, which suggests that any negative effects from unemployed individuals rushing into worse jobs were small relative to the benefits of returning to work after a shorter unemployment spell. The positive reemployment effect and the shorter nonemployment duration suggest that negative duration dependence plays an important role: the unemployed who find a new job earlier can earn higher wages as their skills have deteriorated less (Kroft, Lange, and Notowidigdo 2013).[27]

In Figure 7, we compare the magnitude of our estimates to the previous literature. We plot the estimates of nonemployment duration on the horizontal axis, with the estimates of reemployment wages on the vertical axis similarly to Nekoei and Weber (2017). We also add our main estimates based on the full sample with controls (column 2 of Table 2).

Estimates from the previous literature exploit increases in the potential benefit duration. In these settings, negative duration effects would be implausible, and so most estimates are located in the right quadrants (I and IV). On the other hand, the policy analyzed in this paper is different as both the wage and duration effects could be positive or negative. We find that employment effects are negative (horizontal

[27] Front-loading may have a different effect on reemployment wages at the beginning of UI spells where the benefit is increased than at later periods when the benefit is cut. Online Appendix Figure A.9 estimates the reemployment wages by 15-day benefit periods. The sample size is not large enough to estimate the effect of front-loading by the length of unemployment precisely, but the figure nonetheless suggests that reemployment wages conditional on unemployment duration are not significantly different before and after the reform.

axis), and the wage effects are positive (vertical axis), which places our estimates in the top left quadrant. As a result, our estimates support the negative relationship between nonemployment duration and reemployment wages. Our evidence is therefore in line with the previous literature suggesting that strong negative duration dependence is present in reemployment wages.

C. *Robustness to Alternative Explanations*

In the previous sections, we showed that nonemployment duration fell and reemployment wages increased when the benefit reform was introduced on November 1, 2005. We interpret these behavioral responses as evidence for the causal effect of front-loading shown in Figure 1. In this section, we discuss in detail alternative explanations that might threaten such an interpretation and conclude that none played a major role.

Reemployment Bonus: Those who claimed benefit after the reform were not only faced with the front-loaded benefit schedule but were also eligible to claim a voluntary reemployment bonus if they found a job within 270 days. The reemployment bonus was associated with extra hassle costs and other disadvantages described in Section IIA that resulted in the take-up rate being low. Still, it is possible that the parallel introduction of the reemployment bonus explains part of the decline in nonemployment duration. To separate the effect of benefit front-loading from the reemployment bonus, we exploit the anecdotal evidence that the reemployment bonus scheme was advertised more at some UI local offices than at others. While we do not observe directly which UI offices advertised the reemployment bonus more actively, we use the local level take-up rate of the reemployment bonus as a proxy for the information provided to the unemployed about the scheme.

Three empirical observations motivate that the take-up rate is related to access to information and not to other factors. First, there is a large variation in the reemployment bonus take-up rate across UI locations. This is highlighted in online Appendix Figure A.10, panel (a), which shows the histogram of the reemployment bonus take-up rate by UI location. Second, this variation in reemployment bonus take-up rate is not related to prereform unemployment duration or reemployment wages. To show this, in Figure 8, we plot the kernel-weighted local polynomial regression of the average nonemployment duration (panel A) and average reemployment wage (panel B) on the local reemployment bonus take-up rate after the reform. We separate prereform averages (red line) from postreform averages (blue line). The red lines are flat in both panels, which suggests that there is no relationship between prereform nonemployment duration (or reemployment wages) and local reemployment bonus take-up rate after the reform.[28] Third, differences in the take-up rate across locations are persistent. Online Appendix Figure A.10, panel (b) shows a scatterplot between

[28] The average prereform nonemployment duration is slightly higher and the average reemployment wages are slightly lower at locations with a very high (above 25 percent) reemployment bonus take-up rate. However, the 95 percent confidence intervals around these estimates are large, and so we cannot reject that the relationship remains flat.

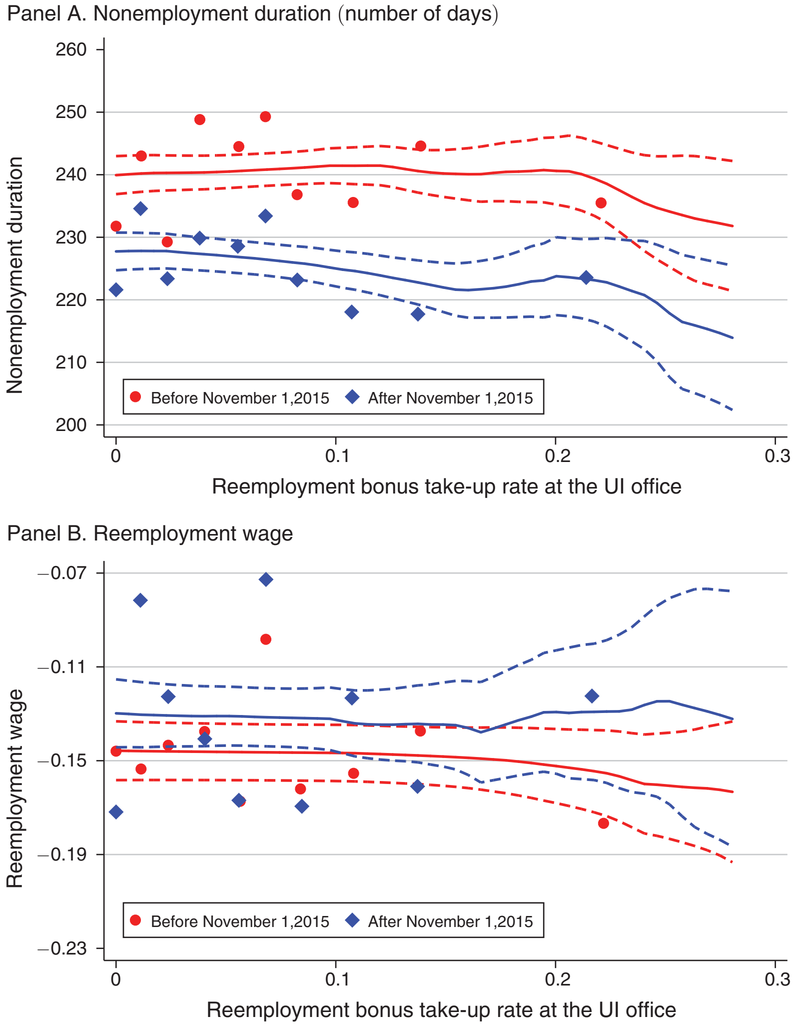

FIGURE 8. THE EFFECT OF THE REFORM BY LOCAL REEMPLOYMENT BONUS TAKE-UP RATE

Notes: This figure shows the kernel-weighted local polynomial regression of the nonemployment duration (panel A) and the reemployment wages (panel B) on the average reemployment bonus take-up rate at local UI office. The red line shows the local polynomial fit for the unemployed who claimed benefit before the reform, while the blue line shows the fit for those who claimed after. The red dots are the nonparametric (bin scattered) relationship between the nonemployment duration (panel A) and the reemployment wages (panel B) on the average reemployment bonus take-up rate at local UI office before the reform, while the blue squares are for after the reform.

the take-up rate one year after the reform and the take-up rate two years after. The figure demonstrates a strong correlation (0.64) between take-up rates in the two years.

Similarly to Chetty, Friedman, and Saez (2013), we use the variation in access to information across locations to better understand how the reemployment bonus affects our baseline results. We estimate the effect of the reform at locations with

Table 4—Effect of the Reform on Nonemployment Duration and Job Quality at Locations with Low Reemployment Bonus Take-up Rate

	Full sample		Optimal bandwidth		Short bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Nonemployment duration (number of days)						
After	−13.97	−11.58	−12.73	−13.14	−17.55	−15.57
	(2.58)	(2.66)	(8.33)	(8.00)	(8.07)	(8.29)
R^2	0.003	0.052				
Observations	10,506	10,506	5,952	5.952	3,050	3.05
Bandwidth	20 before 23 after	20 before 23 after	12 before 12 after	12 before 12 after	6 before 6 after	6 before 6 after
Panel B. Probability that the new job lasts more than a year						
After	0.006	−0.003	−0.063	−0.059	−0.039	−0.036
	(0.01)	(0.01)	(0.06)	(0.06)	(0.03)	(0.04)
R^2	0.0001	0.023				
Observations	6,327	6,327	945	945	310	310
Bandwidth	20 before 23 after	20 before 23 after	2 before 2 after	2 before 2 after	1 before 1 after	1 before 1 after
Panel C. Reemployment wage: log(reemployment wage/UI base earnings)						
After	0.024	0.016	0.017	0.071	0.049	0.073
	(0.012)	(0.012)	(0.08)	(0.09)	(0.05)	(0.06)
R^2	0.001	0.059				
Observations	6,272	6,272	613	613	290	307
Bandwidth	20 before 23 after	20 before 23 after	2 before 2 after	2 before 2 after	1 before 1 after	1 before 1 after
Controls	No	Yes	No	Yes	No	Yes
$f(T_i)$	No	3rd poly	Kernel	Kernel	Kernel	Kernel

Notes: This table shows the effect of the reform on nonemployment duration (panel A), on the probability that the new job lasts more than a year (panel B), and on reemployment wages (panel C). We restrict our sample to locations where the reemployment bonus take-up rate was less than the median (6 percent). We use only locations with at least 30 observations to make sure that reemployment bonus take-up rate is estimated reliably. Columns 1 and 2 use the full sample, and columns 3 and 4 restrict the sample to the optimal bandwidth, while columns 5 and 6 use a half of the optimal bandwidth. In panel A, the nonemployment duration is capped at 360 days in all columns. In panels B and C, only workers who found a job in 360 days are included in the sample. "After" is a dummy that is 1 if the unemployed individual claimed benefit after the benefit reform. The control variables are the same as for Table 2. "Bandwidth" shows the number of before and after months used in the estimations. The standard errors in parentheses are clustered at the local UI office level.

a take-up rate below the median.[29] In this subsample, the mean bonus take-up rate was only 2.7 percent (versus 6.8 percent in the main sample), which suggests that access to information about the reemployment bonus scheme was limited.[30]

Table 4 presents the main analysis from Table 2 estimated using only UI locations with a below-median take-up rate. Column 1 of panel A shows that the fall

[29] We omit locations where we observe fewer than 30 reemployment claims to measure the bonus take-up more precisely.

[30] Among unemployed who were eligible for the reemployment bonus because they found a job within 270 days of their benefit claim, the take-up rate was 5.4 percent in the low take-up rate locations and 12.2 percent across all locations with at least 30 unemployed observed after the reform. In areas with a high reemployment bonus take-up rate, the take-up rate was highest in the first part of the UI spell and fell significantly after six months. The take-up rate was low and fairly constant throughout the UI spell in locations with a low take-up rate (see online Appendix Figure A.11).

in nonemployment duration is 13.97 days (SE 2.58), which is very close to the estimated drop using all UI locations (14.24 days in Table 2). In column 2 we include the control variables in the regression. The absolute value of the point estimates is slightly lower in the low UI take-up locations (11.58 in Table 4 versus 13.98 days in Table 2), but the estimated drop in nonemployment duration remains high and statistically does not differ from the full sample. A similar pattern emerges when we use alternative bandwidth selections (columns 3–6 of Table 4): the point estimates are a little lower when we use only UI locations with low reemployment bonus take-up, but they are still close to our main estimates. The difference in the point estimates is around two days, which is smaller than the standard errors around the estimates.

In panels B and C of Table 4, we show the effects on match quality using only locations with a low reemployment bonus take-up rate. The estimated effect on the probability of staying unemployed for at least a year (panel B) is virtually unaffected by restricting the sample to locations with the low bonus take-up rate. The effect on reemployment wages is also very similar to the estimates using the main sample (Table 2) in most specifications: the estimated effects in the low take-up locations (Table 4) are slightly higher than in the full sample (columns 1 and 2) and slightly lower when we use the optimal bandwidth with controls (column 4) or use the short bandwidth (columns 5 and 6). The only big difference emerges in column 3 (optimal bandwidth with no controls), where we estimate only a modest effect on reemployment wages in the low take-up rate sample (1.7 percent, SE 8 percent in Table 4) instead of a large effect in the main sample (9 percent, SE 5 percent in Table 2).

In panel B of Figures 2, 3, 5, and 6, we provide further graphical evidence on the effect of the reform in locations with a low reemployment bonus take-up rate. The graphs show that a similar pattern to the main sample (panel A) emerges before and after the reform: nonemployment duration drops considerably after the reform (Figure 2); Kaplan-Meier survival rates diverge after 70 days, and they almost converge after 350 days (Figure 3); the probability of finding a job within a year is not affected (Figure 5); and the reemployment wages increase modestly (Figure 6). Moreover, in Figure 4, panels B and D we show that the standard regression discontinuity assumptions (no discontinuity in the number of claimants and no selection in observables around the threshold) hold for the sample using low reemployment bonus take-up locations.

Finally, in Figure 8, we plot the kernel-weighted local polynomial regression of average nonemployment duration (panel A) and average reemployment wage (panel B) on the local reemployment bonus take-up rate after the reform. The difference between the prereform averages (red line) and the postreform averages (blue line) shows the effect of the reform by local reemployment bonus take-up rate. Both panels demonstrate that the reform had an effect even in locations where the reemployment bonus take-up rate was zero. This is reassuring since as the reemployment bonus was not utilized in these locations, the difference between the pre- and postreform periods reflects only the effect of front-loading. The impact of the reform was slightly higher in locations where the take-up rate was above the median (6 percent), but the increase relative to areas with a zero take-up rate is not significant economically.[31] Panel B highlights that the local reemployment bonus take-up rate is unrelated to the reemployment wage except in those locations with very high take-up rate.

Overall, the evidence presented in this section underlines that access to information on the reemployment bonus (measured by variation in its take-up rate) had only a limited effect on nonemployment duration and on reemployment wages, and so our results are mainly driven by the changes in the benefit path. This is not surprising given that the reemployment bonus scheme was a complicated and obscure policy with some substantial drawbacks, such as losing the remaining benefit eligibility if claimed.

Spillovers: In the main analysis, we focus on those unemployed whose monthly UI base earnings were above 108,000 forint (US$504) in 2005, since the reform front-loaded the unemployment benefit for this group. As we described in Section IIA, unemployed with a lower UI earnings base experienced the same drop in benefit between 90 and 270 days, but the increase in their benefit in the first 90 days was substantially lower. Since these unemployed people's new benefit schedule was less generous than the old one, their search effort would have increased in response to the reform.[32]

What if the behavioral changes of the low UI earnings base unemployed had an effect on the unemployed in our primary sample? Any spillover effects would work against finding a drop in nonemployment duration if the low UI base unemployed are substitutes with the high UI base unemployed. In this case, all unemployed workers would compete for the same jobs, and the higher search effort of the unemployed with low UI base earnings would crowd out unemployed with a high UI base. This would make it more difficult for high UI earnings base unemployed to find a new job, and we would underestimate the effect of front-loading on the unemployed in our main sample. However, if unemployed workers with a low UI base are complements with high UI base unemployed, the increased search effort of low UI base unemployed would improve the prospects of high UI base unemployed, and we would overestimate the effect of front-loading.

In Table 3, we test for the presence of spillover effects directly. Since spillovers mainly affect unemployed competing in the same local labor market, spillover effects should be larger in areas with a higher local share of unemployed with a low UI earnings base. If low and high UI base unemployed are substitutes, we would expect a smaller reduction in nonemployment duration among high UI base unemployed in these areas. On the contrary, if low and high UI base unemployed are complements, we would expect more of a reduction in these areas. Columns 7 and 8 suggest that if anything, low and high UI base unemployed are substitutes. The

[31] In online Appendix Table A.4, we repeat the main analysis presented in Table 2 using UI locations with an above-median take-up rate (6 percent). In line with Figure 8, the drop in nonemployment duration is slightly larger in that sample: -18.94 in high take-up rate locations (column 1 of Table A.4) versus -13.97 in low take-up rate locations (4). The drop in nonemployment duration in the overall sample restricted to locations with at least 30 observations is -16.4 days. Therefore, the presence of the reemployment bonus can at most explain $(1 - 13.97/16.4) = 15$ percent of the overall response.

[32] We find that nonemployment durations among the low-earnings base unemployed dropped significantly after the reform (results available on request).

reduction in nonemployment duration is larger in locations where the local share of high UI earnings base unemployed is above median (column 7) than in locations where the local share is below median (column 8). Moreover, the increase in reemployment wages is also larger in locations with above-median local share of high UI base unemployed. This suggests that if anything, our results would be even more pronounced if the behavior of low UI earnings base unemployed was not affected by the reform.

D. *Effect of the Reform on the Budget Balance*

Our results presented in the previous sections indicate that nonemployment duration dropped considerably and reemployment wages increased as a result of benefit front-loading. In this section, we use these estimates to understand the consequences of the reform for the government's budget, using equation (1) derived in Section I. For simplicity, we assume that the daily interest rate r is zero and set $T = 360$ because after 360 days, the impact of the reform on survival rates is very small (see Figure 3). Furthermore, we allow for wage adjustment in our calculations since we find that reemployment wages do adjust in response to the reform. The budget effect of front-loading is given by

$$(3)\qquad \Delta G = \underbrace{\sum_{t=1}^{360}\left(-S_t^{pre}\Delta b_t\right)}_{\text{mechanical effect}} + \underbrace{\sum_{t=1}^{360}\left(b_t^{post} + \tau w^{post}\right)\left(-\Delta S_t\right)}_{\text{survival rate change}} + \underbrace{\sum_{t=1}^{360}\left(1 - S_t^{pre}\right)\tau\Delta w}_{\text{reemployment wage change}},$$

where b_t^{post} and b_t^{pre} are the daily postreform and prereform benefit shown in Figure 1, S_t^{post} and S_t^{pre} are the daily postreform and prereform survival rates in nonemployment shown in Figure 3, and w^{post} and w^{pre} are the postreform and prereform reemployment wages. We note that in contrast with the model presented in Section I, we must distinguish between prereform and postreform policy changes in the empirical budget decomposition since the reform we study was not infinitesimal. This formula is an exact decomposition of the total budget effect of the reform, and so the sum of the various elements is equal to the actual impact on the budget.

The Hungarian benefit front-loading raised benefits by around 50 percent in the first 90 days and decreased them by around 25 percent between 91 and 270 days. Table 5 summarizes the key effects of the reform on the government's budget balance. The first pair of columns rely on the estimates based on the full sample, while the second pair use only locations with low reemployment bonus take-up rates. To calculate the budget consequences, we use the Kaplan-Meier estimates on survival rates presented in Section IIIA (see Figure 3) and the reemployment wage estimates presented in Section IIIB (see Figure 6).[33] The details of the tax and benefit system

[33] Since new jobs do not last forever, we take into account the average length of new jobs in the first year when we calculate the budget consequences of higher reemployment wages (see online Appendix A.1. for the details).

Table 5—The Effect of the Reform on the Government's Budget Balance

Total UI benefit payments per unemp. before the reform	−US$1,621	(6.8)	−US$1,642	(11.16)
The effect on UI budget balance (see equation (3))				
I. Mechanical UI spending increase caused by the reform	−US$119	(0.8)	−US$116	(1.6)
II. Change in budget because shorter unemployment				
IIa. UI spending decrease	US$77	(9.01	US$76	(14.7)
IIb. Increase in UI contribution	US$11	(1.2)	US$10	(1.8)
IIc. Gain in tax revenue outside the UI budget	US$126	(13.8)	US$117	(23.1)
III. Change in tax revenue because higher reemployment wages				
IIIa. Increase in UI contribution	US$4	(2)	US$4	(3.39)
IIIb. Gain in tax revenue outside the UI budget	US$50	(26.7)	US$50	(45.6)
IV. Net change in the government's budget balance (I + IIa + IIb + IIc + IIIa + IIIb)	US$148	(39.4)	US$142	(63.3)
V. Reemployment bonus effect on the budget balance	−US$38	(1.5)	−US$17	(1.7)
VI. Net change in the government's budget balance with reemp. bonus (IV + V)	US$110	(39.5)	US$125	(63.3)

Notes: This table shows the effect of the reform on the government's budget balance. All values represent spending and tax revenue per unemployed. We decompose the effect of the reform based on equation (3) (see the text for details). The net change in the government's budget balance adds up all the benefits and costs of the reform. The positive values in the IV and VI rows mean that the budget balance improved after benefit front-loading. Bootstrapped standard errors are in parentheses.

are described in online Appendix A.1. We calculated the standard errors around the estimates by bootstrapping (see columns 2 and 4).[34]

The first line in Table 5 highlights that the UI system spent US$1,621 (SE US$6.80) per unemployed worker in the full sample before the 2005 reform. The line denoted with (I) shows that as a result of benefit front-loading, the cost of UI mechanically increased by US$119 (SE US$0.80) per unemployed.

As we show in Section IIIA, the reform sped up reemployment and shifted down the survival rate in nonemployment. Row IIa in Table 5 shows that spending decreased by US$77 (SE US$9) per unemployed worker as a result. Finding a job earlier also affects the amount of tax collected. As a result, UI contributions increased by US$11 (SE US$1.20) per unemployed worker (row IIb). From the government's perspective, the revenues outside the UI budget should also be taken into account (Schmieder and von Wachter 2016). The quicker reemployment increased wage-related taxes and contributions by an additional US$126 (SE US$13.80) per unemployed worker.

Finally, the reform increased reemployment wages by 1.4 percent, which further increased the revenue of the government. The additional revenue due to higher reemployment wages improved the balance of the UI budget by US$4 (SE US$2) (row IIIa) and improved the government's budget outside the UI system by US$50 (SE US$26.70) in the first year after reemployment.

[34] We take 1,000 random samples with replacement, then we calculate the change in UI budget balance using the Kaplan-Meier survival rates and the reemployment wages in the new sample.

To sum up, the reform would have increased UI expenditures by US$119 per unemployed worker in the absence of any behavioral response, but the change in unemployed behavior was large enough to offset the cost increase. We estimate that the faster reemployment and high reemployment wages increased revenues by US$268 (US$77 + US$11 + US$126 + US$4 + US$50). Therefore, the benefit front-loading improved the government's budget balance by US$148 (SE US$39.40) per unemployed worker, and the standard error around this estimate is tight enough to rule out that the budget balance deteriorated as a result of the benefit change.

Section IIIC showed that our estimates on nonemployment duration and reemployment wages are driven by the change in the benefit schedule and not by the introduction of the reemployment bonus scheme. To make sure that our estimates on the budget consequences of the reform do not change substantially, even if we consider the cost of the reemployment bonus scheme, we also report the government's expenditures on the bonus scheme. Row V of Table 5 shows that the government spent US$38 (SE US$1.50) per unemployed on reemployment bonuses, and so the government's budget improved by US$113 (SE US$39.50) if these costs are taken into account.

In columns 3 and 4 of Table 5, we also report the budget consequences of the reform when we use estimated responses only from locations with a low bonus take-up rate. The point estimates of the various budget items are virtually the same in the low reemployment bonus take-up locations (column 3) and in the full sample (column 1), although the standard errors around the estimates are larger in the low take-up regions. For instance, the UI spending per unemployed worker before the reform is US$1,642 in low take-up versus US$1,621 in the full sample (first line), the change in UI spending is −US$76 in low-take up versus −US$77 in the full sample, and the increase in revenue as a result of higher reemployment wages is US$50 in low take-up versus US$50 in the full sample. A notable exception is line V, the cost of reemployment bonuses scheme, which is mechanically smaller in the low take-up rate locations (US$17 versus US$38).

The fact that the different items in the budget are so close to each other (except the spending on reemployment bonuses) in the two samples provides further evidence that it is the benefit change and not the reemployment bonus scheme that drives our results. In fact, when we concentrate on locations with a low reemployment bonus take-up rate, the budget balance improves even more (by US$130 in low take-up areas versus US$113 in full sample).

E. *Welfare Assessment*

The previous section highlights that the government's budget was improved as a result of front-loading. This implies that benefit front-loading can be implemented without changes in taxes—if anything, taxes can be lowered after front-loading. Proposition 1 in Section I highlights that under these circumstances, the welfare of the unemployed must be (weakly) improved. This is due to the fact that unemployed workers can undo front-loading by putting aside the benefit increase in the first 90 days and supplementing the lower benefits received between 90 and 270 days with this extra saving. They therefore cannot be made worse off, and some of those who choose a different profile are likely to be made better off.[35] This implies that the front-loading implemented in Hungary was a Pareto improving policy change.

IV. Conclusion

This paper assesses the welfare implications of a radical change in the path of UI benefits in Hungary. We show that benefit front-loading reduced nonemployment durations, increased reemployment wages, and improved the government's budget balance. As a result, most (if not all) unemployed workers' welfare improved, while the burden on taxpayers shrank. Therefore, our results indicate that the Hungarian reform was a Pareto improving policy change.

One important implication of our results is that changing the UI benefit path can break the trade-off between the moral hazard and insurance motives of insurance under some circumstances. We believe that these findings are likely to apply to countries other than Hungary: in online Appendix A.3, we estimate that a 1 percent increase in the UI replacement rate lowers the reemployment hazard by 0.21 percent in our sample before the reform. Such a response to the change in replacement rate is in line with the cross-sectional estimates of Moffitt (1985) and recent results of Lalive (2007), Card et al. (2015), and Landais (2015) from Austria and the United States. Therefore, as far as responses to a change in the benefit level are indicative of responses to the change in benefit path, our estimates on front-loading are likely to be relevant for other countries as well.

Our paper also makes a strong case that shifting from a constant benefit profile common in many countries to one that declines in unemployment duration can improve welfare. This implication is in stark contrast with Kolsrud et al. (2018), who conclude that an increasing benefit profile is likely to be welfare improving. The key source of the difference in our conclusion is that our estimates imply that unemployed workers respond more to later drops in benefits than to an equal size earlier increase, while they find the opposite. While further empirical studies are needed to resolve this discrepancy, one key advantage of our analysis is that the reform we study was particularly large,[36] and so it is more likely that unemployed agents who face optimization frictions (e.g., adjustment costs or inattention) responded to the benefit change (Chetty 2012).

Finally, the finding that reemployment wages are higher after front-loading even though nonemployment duration is shorter suggests that there is some negative duration dependence in wages. Such a duration dependence also implies that the fully optimal benefit path should be declining (see Shimer and Werning 2006).

[35] In fact, the new benefit regime was slightly more generous, as the total benefit amount was 2.4 percent higher under the new benefit profile. This means that the unemployed were in fact strictly better off after the reform.

[36] Kolsrud et al. (2018) exploit that benefits drop by 17 percent after 20 weeks (from SEK 680 to SEK 580) in Sweden. In contrast, our reform examines the effects of a 50 percent increase in the first 90 days and 25 percent drop between 90 and 180 days.

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