



**Does Extending Unemployment
Benefits Improve Job Quality?**

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Does Extending Unemployment Benefits Improve Job Quality?

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Contrary to standard search model predictions, prior studies failed to estimate a positive effect of unemployment insurance (UI) on reemployment wages. This paper estimates a positive UI wage effect exploiting an age-based regression discontinuity in Austrian administrative data. A search model incorporating duration dependence determines the UI wage effect as the balance between two offsetting forces: UI causes agents to seek higher-wage jobs, but also reduces wages by lengthening unemployment. This implies a negative relationship between the UI unemployment-duration and wage effects, which holds empirically both in our sample and across studies, reconciling disparate wage-effect estimates. Empirically, UI raises wages by improving reemployment firms' quality and attenuating wage drops.

The positive effect of unemployment insurance (UI) on unemployment duration is one of the most robust empirical findings in economics. However, the literature has not reached a consensus on a fundamental question: Does UI induce a simple delay in job acceptance as the unemployed enjoy subsidized leisure? Or do the unemployed use benefits to actively improve their job opportunities, so that subsidizing a longer search results in better jobs? This question has significant implications for our understanding of unemployment and the design of UI.

This paper investigates post-UI job quality from both an empirical and a theoretical perspective. We begin by studying how an extension of the potential UI benefits duration affects laid-off agents' search decisions, using 19 million job separations recorded in Austrian administrative data. We adopt a regression discontinuity (RD) design comparing individuals older and younger than 40, the age cutoff for eligibility for a nine-week

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extension of the potential UI benefit in addition to the base of 30 weeks. This empirical setting has two distinct features: first, it allows us to identify even subtle UI effects with a high degree of precision since the Austrian Social Security Database includes daily records of employment status, earnings, and UI benefit receipt for the universe of private sector workers. Second, Austria offers an institutional setting where both the base and the extended UI durations are similar to those in the U.S.

Consistent with prior research, we estimate that a nine-week increase in potential benefit duration causes workers to stay jobless two days longer. But in contrast to previous studies, we find that the benefit extension also causes workers to obtain jobs that pay on average 0.5 percent higher wages. Moreover, the positive wage effect persists over time and does not substitute other desirable job characteristics. The evidence of the positive UI wage effect suggests that UI subsidizes search and not just leisure.²

How can we reconcile our result with the prior literature? A large body of existing work has not found any UI effect on job quality. For instance, three prominent papers that use quasi-experimental designs and administrative data provide estimates of the UI wage effects that are not significantly different from zero (Card et al. (2007a), Lalive (2007) and Van Ours and Vodopivec (2008)). Moreover, Schmieder et al. (2014) find a statistically significant *negative* UI wage effect. We show that these different empirical findings are not in contradiction with theory once we take into account that an unemployed agent's job opportunities, skills, and UI benefits decrease the longer she remains without a job (duration dependence).

We introduce a tractable directed-search model that incorporates such duration dependence. Contrary to intuition, in this setting, the UI effect on subsequent wages is not necessarily positive, as it is determined by two offsetting forces. On the one hand, an increase in UI generosity causes UI recipients to become more selective in their job search, which raises subsequent wages. On the other hand, increased UI generosity also causes them to stay unemployed longer and thus experience a larger decline in job opportunities, which reduces subsequent wages.³ The latter negative force is triggered by

²Gruber (2004)'s textbook mentions "How can we distinguish whether UI subsidizes unproductive leisure or productive job search? The best way to do so is to study the quality of post-UI job matches". An earlier mention of this idea is contained in Burgess and Kingston (1976), Classen (1977), Ehrenberg and Oaxaca (1976) and Holen (1977).

³The intuition of two offsetting forces has been present in discussions of the effect of UI on job quality (For instance, see Addison and Blackburn (2000), Degen and Lalive (2013), and Schmieder et al. (2014)). For example, Addison and Blackburn (2000) mention: "we tend to expect reservation wages to decline with spell length ... as a result of stigmatization or human capital depreciation effects. Such effects may counter the prediction of rising postunemployment wages with receipt of unemployment insurance. But the general presumption that UI will elevate reservation wages and lead to relatively higher postunemployment wages as a result of better job matches would appear to be robust and to provide a means of

the positive UI non-employment effect, the result of UI causing recipients to both search less and to become more selective (the search and selectivity margins). This implies that a stronger search margin leads to a more prominent negative force, without affecting the positive counterpart in our model, and thus reduces the total UI wage effect.⁴

The above-mentioned differences in empirical estimates can thus originate from variation in the relative importance of search and selectivity margins across different studied populations. Such heterogeneity would be reflected in a negative correlation between the UI non-employment and wage elasticities. We show that such a correlation holds in a meta-analysis across existing estimates: studies that estimate a longer UI non-employment effect also tend to find a lower UI wage effect. Furthermore, we provide an empirical test that confirms this negative relationship within our population. This test essentially estimates the correlation between two elasticities across sub-samples created by resampling based on predetermined observable characteristics.

What are the mechanisms driving the positive UI wage effect? We provide three empirical findings to shed light on this question. First, we ask whether UI generosity affects the type of firms that unemployed workers join. We investigate the UI benefit extension effect on post-unemployment firm characteristics based on the same RD design as we use in the analysis of the individual-level wage effect. Interestingly, we find that agents who are eligible for the benefit extension end up working for firms that pay higher wages to their (other) employees. The magnitude of this effect suggests that a considerable part of the positive 0.5% UI wage effect at the individual level comes from the UI effect on employer-employee matches, which rules out the economic significance of the effect on workers' bargaining power.

Second, we show that the average UI wage effect of 0.5% is due to an attenuation of wage declines between pre- and post-unemployment jobs. In response to the nine-week benefit extension, the likelihood of experiencing a wage loss that is larger than 40% is reduced by 0.5%, while the likelihood of achieving a wage increase between 0 and 10% is increased by 0.5%. Such a non-uniform UI effect across the wage distribution can be explained within our model as a result of limited benefit duration.

Third, our theoretical model predicts that the UI benefit extension effect should be larger for agents with a higher likelihood of UI benefit exhaustion. We provide two

discriminating between the labor-leisure and search models of UI." Our theory formalizes these offsetting forces, demonstrates their endogeneity, states the fundamental parameters that determine their balance, and shows that the UI wage effect *can* be negative.

⁴How can a more generous UI lead to lower subsequent job quality? This is theoretically possible as agents are maximizing expected consumption rather than expected wage. UI creates a wedge between the two and can reduce wages but always increases consumption.

pieces of evidence for this prediction: in response to the benefit extension, the largest increase in reemployment wages appears in jobs found closer to benefit exhaustion time and for agents with a relatively higher ex-ante likelihood of benefit exhaustion.

A non-zero UI wage effect has an important policy implication. Connecting these results to a normative model of UI points to an overlooked fiscal externality due to the UI wage effect: UI affects future tax revenue through higher wages. The conventional outlook on the fiscal externality of UI has focused on the unemployment-duration effect of UI, measuring the fiscal effect of benefit payments and lost tax revenues due to longer non-employment durations. However, if UI affects reemployment wages, it will also change future tax revenues. The fiscal externality of UI should thus be calculated as the sum of the traditional negative non-employment-duration externality, and this re-employment-wage externality, the sign of which depends on the sign of the UI wage effect.⁵ In our sample, the wage externality is positive, and has the same order of magnitude as the traditional duration externality, but with the opposite sign. Based on our theoretical insights and this empirical estimate, we conclude that the optimal level of UI varies depending on the relative importance of the effort vs. selectivity margins in job search. These results suggest that taking gains in job quality into account could significantly change the optimal generosity of UI.

The outline of the paper is as follows. **Section 1** discusses the empirical setting. **Section 2** presents the main estimation results of the UI wage effect. **Section 3** sets up a theoretical model and shows how it can reconcile our result with the previous literature. **Section 4** studies the mechanisms driving the UI wage effect. **Section 5** investigate the policy implications of our findings and **Section 6** concludes. Theoretical derivations, the proofs of propositions, validity tests, and further robustness checks are collected in an online Appendix.

1 Empirical Setting

1.1 Data and Institutional Background

The Austrian unemployment insurance system is less generous than those of most European countries. The potential benefits duration is a function of previous work experience and age. As a baseline, all workers are eligible for 20 weeks of benefits provided that they have been employed for more than a year during the two years prior to layoff. UI

⁵Similar to the optimal UI literature, we here assume that UI only affects eligible workers, and neglect the macro effect of UI. Section 4 provides supportive empirical evidence for this assumption.

benefit eligibility is extended to 30 weeks for workers who have been employed for 3 years during the 5 years leading up to the layoff date. Furthermore, since August 1, 1989, workers ages above 40 at the time of layoff have been eligible for a benefit extension to 39 weeks, if they have worked for 6 years during the last 10 years (Lalive et al. (2006)). The benefit replacement rate is 55% of net earnings, subject to a maximum and a minimum benefit levels that is adjusted annually.⁶ The UI system is financed by a 6-percent payroll tax with no experience rating. After exhausting UI benefits, workers can apply for mean-tested unemployment assistance.⁷

Two administrative data sets constitute the main source of our empirical analysis. The Austrian Social Security Database (ASSD) provides daily employment records and annual earnings by employer for the universe of private sector employees. These records cover 85 percent of the workforce, excluding civil servants and the self-employed. We match ASSD with Austrian Unemployment Registers at the individual level. The second dataset includes unemployment spells and benefit receipts. Non-employment duration is defined as the number of days between two consecutive employment spells (Solon (1979), Card et al. (2007b) and Rothstein (2011)). We measure the daily wage rate—one of our measures of job quality—as a worker’s annual earnings per employer divided by the number of days she has worked for this employer.⁸

Over the period of 1980-2011, we consider 18,612,408 job separations of individuals who are eligible for UI, i.e. with a minimum pre-unemployment tenure of 28 weeks at their pre-unemployment job. Table 1 presents descriptive statistics of the subpopulation of prime-age workers, in addition to three nested subpopulations. Column 2 excludes individuals who take up UI benefits more than 28 days after the date of job separation, thus eliminating voluntary quitters who are subject to a 4-week waiting period (Card et al. (2007a)). The average tenure at layoff is around 2.5 years, and the average non-employment duration is 17 weeks. In order to isolate the change in UI duration from 30 to 39 weeks, column 3 includes all agents eligible for 30(39) weeks of UI if they are below(above) the age of 40, i.e. workers who have been employed 60% of the last 5 and 10 years. Our estimate sample, Column 4, includes all workers who have been laid off after

⁶A family allowance for workers with dependent family members could be added to the basis level of UI. However, total UI replacement rate can not exceed 60%, or 80% for a claimant with dependents.

⁷The replacement rate of unemployment assistance is 92%, but the actual replacement rate is much smaller because of means testing based on household income. For such evidence see Card et al. (2012) and Card et al. (2007a).

⁸For individuals recalled by their previous employer within the same calendar year, the data does distinguish between last pre-unemployment wage and first post-unemployment wage because of annual recording of earnings. This group is thus excluded from the main analysis. Our results are robust to recovering the wage of this group from the previous or following calendar year.

the introduction of the law of August 1, 1989. This includes 1,738,787 job separations. Relative to the average laid-off agent in column 2, they have 11 weeks (8 percent) longer tenure at the time of layoff and they experience a wage drop of 4.6 percent.

1.2 Research Design

To evaluate the effects of UI benefit extension, we posit the following model that exploits the age-base discontinuity in the Austrian UI eligibility rule:

$$y_i = \gamma \times 1(\text{age}_i \geq 40) + f^a(\text{age}_i) + f^b(\text{age}_i) \times 1(\text{age}_i \geq 40) + \eta_i, \quad (1)$$

where age is measured at the time of layoff. The design is a sharp RD since the running variable *age* strictly determines UI duration in our sample. In fact, the potential benefit duration is 9 weeks longer for agents older than 40, conditional on having been employed at least 60% of the time within both the last 3 and the last 5 years. The two unknown functions f^1 and f^2 are assumed to be smooth. Under the identification assumption that $\eta_{t,i}$ does not change discontinuously at age 40, the estimate of γ is unbiased even in the absence of controls for observable factors X_i . In the result section, we report the estimates with and without controls to increase precision. Following [Lee and Lemieux \(2010\)](#), all figures contain both the true underlying functions and several realizations of η .

The estimation results presented in the paper focus on specifications where f^a and f^b are polynomials of degree 2 over a ten-year bandwidth. In the online Appendix, we present several robustness checks. Table A2 assesses the robustness with respect to the choice of polynomial degree and bandwidths. It further provides the two set of additional results when the bandwidth are selected according to [Imbens and Kalyanaraman \(2011\)](#) and [Calonico et al. \(ming\)](#). For each optimal bandwidth, we provide the conventional and bias-corrected RD estimates, as well as the conventional and robust variance estimators following [Calonico et al. \(ming\)](#). For each method, Table A2 reports the results as well as the optimal bandwidth and the order of the local-polynomial used.

If there is strategic timing of layoff, one might be concerned about the validity of the identification assumption. Following standard practice, we implement two sets of tests. First, we look for evidence of bunching in the frequency of layoffs around the 40-year age threshold. Second, we investigate changes in the sample composition at the threshold using observable characteristics. In this regard, we investigate both the existence of a discontinuity in pre-determined observables and examine whether predicted outcomes evolve smoothly with respect to age. In general, none of our tests detect a sign of strategic

timing of layoff.

Figure Ia plots the histogram of the age distribution at layoff at the annual level. As we can see in the figure, the distribution evolves smoothly over the threshold.⁹ Figure Ib plots the mean logarithm of monthly wages in the pre-unemployment jobs against age to assess that pre-determined observables evolve smoothly around the 40-year threshold. Appendix Table A3 reports regression results checking for discontinuities with other observables. Most coefficients are estimated to be a precise zero, thus confirming the visual perception. As a more concise statistics, Figures A1 and A2 in the online Appendix plot composite covariates indices, derived from predicting our main outcome variables – non-employment duration and wage change – against age. The associated estimates of the RD specifications are reported in Appendix Table A4. The results for all covariates in Appendix Table A2 and composite covariates indices (predicted outcomes) are mostly non-significant and somewhat sensitive to the choice of bandwidth and polynomial degree. Moreover, any statistically significant coefficients are small in economic magnitude. In sum, there is no evidence of detectable manipulation in the timing of layoff around the cutoff that would invalidate our design and the tests support our identification assumption underlying the RD design. This is aligned with prior evidence on the absence of strategic layoffs in Austria (Card et al. (2007a)).

One advantage of the policy discontinuity we are studying is that data predating the age-based UI eligibility rule, which was installed in 1989, is available. Figure II exploits the pre-reform period to verify that there was no discontinuity in non-employment duration or wage growth at the cutoff before the rule was implemented. Online Appendix Table A5 presents the corresponding regression estimates focusing on two control groups: (i) agents laid-off before 1989 shown in Figure II, and (ii) workers who were laid off after the 1989 reform, but are not eligible for the nine-week benefit extension at the age of 40 because of their relatively short work history. For both control groups, we find no evidence of a discontinuity in any of the outcome variables.

2 Positive UI Effect on Reemployment Wage

Figure III illustrates the effect of the extension in the potential duration of UI benefits from 30 to 39 weeks on non-employment duration. Figure IIIa plots average non-employment duration against age at layoff. The two lines shown in the graph represent

⁹We also investigate the potential manipulation at monthly level. We find no evidence of manipulation. The age distribution at monthly level exhibits seasonal patterns stem from seasonality in both timing of birth and layoff. Table A6 in the Appendix verifies that our results are not affected by these seasonal patterns.

quadratic fits. The discontinuity observed at the thresholds corresponds to an approximate increase of 2 days in the average non-employment duration in response to the increase in UI duration. Column 1 of Table 2 presents the corresponding coefficient estimate.¹⁰

An alternative way of measuring the UI effect on non-employment duration is via the hazard rate of finding a job. Figure IIIb illustrates the effect of the benefit extension on the probability of finding a job within the first 39 weeks after layoff. As confirmed by the regression estimates in Table 2, the benefit extension decreases the chance of finding job within the first 30 weeks by one percentage point, and within the first 39 weeks by 1.3 percentage points (Columns 2 and 3 of Table 2). The fact that the job-finding rates decreases during the first 30 weeks, where the UI generosity has not been changed, suggests that workers are forward looking. Table 2 also presents result with non-parametric controls for a set of calendar time, firm and individual characteristics. These results confirm the previous findings that agents stay jobless longer, once they are eligible for a longer UI.

Does the UI benefit extension affect the quality of jobs that workers eventually find? Figure IV illustrates the effect of the benefit extension on wage changes between the pre- and post-unemployment jobs. Figure IVa shows a positive discontinuity at the cutoff, corresponding to a 0.45 percentage point increase (Column 4 of Table 2). Since pre-unemployment wages evolve smoothly at the threshold (Section 1), the effect of the benefit extension on wages can also be detected as a discontinuity in reemployment wages. Consistently, the corresponding RD regressions lead to a similar, but less precise estimate (Column 5 of Table 2).

Once we control for the full set of covariates, the coefficient estimates in columns 4 and 5 become very similar. Column 6 of Table 2 further studies the wage effect of the UI benefit extension by focusing on the nominal wage effect, comparing the reemployment wage with the 50% of the pre-unemployment wage. Given a replacement of 55%, this is similar to comparing the reemployment wage to the UI benefit level. We find that having access to nine additional weeks of UI benefits increases the chance of accepting a job with a wage above the UI benefit level by 0.39 percentage points. This result is in the spirit of predictions from job ladder models à la Burdett where unemployed workers will accept the first job offer above the UI benefit level and then search for a better paying

¹⁰Unemployment spells are censored at 2 years when the UI effects are stabilized (Appendix Figure A5). In contrast to common practice, the mere fact that the UI extension does not have a statistically significant effect on the survival rate by time t is not sufficient for censoring the spells at t . The reason is that a censored average is the sum of survival rates, that is $\mathbb{E}(X|X < t) = \int_0^t (S(x)/S(t)) dx$, where X is non-employment duration, $S(x)$ is the survival function, and t is the censoring level.

job (Burdett (1978)). We will investigate this further in [Section 4](#).

In sum, our results document that additional weeks of UI benefit eligibility increase reemployment wages. The most important implication of this finding is that it rejects the hypothesis that unemployment is a state of leisure consumption and the unemployed can find a job whenever they desire. In this case, a more generous UI would have no effect on job quality. Our findings thus provide direct evidence for the existence of significant search frictions in the labor market, which motivate a need for insurance.

Is the order of magnitude of the wage effect reasonable?

Consider the problem faced by an unemployed worker in our sample, who decides whether she should be more selective and search less intensively during the UI period. The worker knows that this strategy will lead to longer unemployment, but it might result in a better job. In this benchmark case, the agent maximizes her expected income (risk-neutral and no labor/search disutility). More importantly, we abstract from any duration dependence, i.e. stationary environment. The unemployed agent weighs the benefit and cost of an additional day of search: She would lose the daily net wage, $(1 - \tau)w$, part of which will be compensated by the UI benefit, b , and, potentially she finds a job paying a higher wage, that is

$$(1 - \tau)w = b + L(1 - \tau)\Delta w \tag{2}$$

where L is the post-unemployment job duration in days.¹¹ Equation (2) implies that the wage gain from marginal search should be $\frac{\Delta w}{w} = \frac{1-\rho}{L}$. Using an average net replacement rate of $\rho = \frac{b}{w(1-\tau)} = 55\%$ and an average post-unemployment job tenure of $L = 567$ days from the sample, the marginal wage gain should, on average, be 0.07%. This leads to a marginal wage gain of 0.14% for 2 additional days of search, which should be compared to 0.5% from Table 2. Although this benchmark case is based on several simplifying assumptions, it suggests that the estimated wage effect is of the same order of magnitude as the one expected from an optimizing agent.

3 Reconciling Empirical Findings

In standard job search models, a more generous UI system leads to higher job quality, since the UI benefit allows agents to be more selective and look for better jobs ([ROGER-](#)

¹¹Table 3 shows that the wage effect lasts within the first post-unemployment job and there is no effect on tenure. We thus assume L to be equal to the new job tenure and ignore the term $(1 - \tau)w\Delta L$. More precisely, L should be equal to how long the increase in wage will last. We are also ignoring discounting given the short horizon of the problem, i.e. $\frac{1-\exp(-rL)}{r} \simeq L$ when $r = 5\%$.

SON et al. (2005)). This prediction is in line with the estimated positive UI wage effect in Section 2, but it is at odds with the prior empirical literature, which mainly finds zero and, at times, negative wage effect. In this section, we solve this apparent puzzle. In Section 3.1, we develop a partial-equilibrium directed-search model with duration dependence where the UI wage effect is the result of two offsetting forces, so that its magnitude and direction are not theoretically determined. The model further guides us to empirically explain different estimates of the UI wage effect across studies (Section 3.2) and predicts who are the agents most affected by a UI benefit extension (Section 3.3).

3.1 Search Model with Duration Dependence

In our directed search model, unemployed workers choose the type of job they want to apply for in each period among a set of posted offers. They know that the job-finding rate, λ , depends on their selectivity, i.e. is decreasing in the value function of being employed V , and is increasing in search effort, s . For now, we assume workers live hand-to-mouth. In this case, the value of being unemployed, denoted by U , is given by

$$U(t) = \max_{\substack{V,s \\ \lambda=E(V,s,t)}} \lambda V + (1 - \lambda) (u(b(t)) + \beta U(t+1)) - \psi(s), \quad (3)$$

where $u(\cdot)$ is the flow utility of consuming benefit $b(t)$, $\psi(s)$ the disutility of search, t the time since layoff and β the discount factor. Note that jobs can differ in many dimensions. If we assume that jobs differ only on one dimension, e.g. wage, selectivity is equivalent to choosing a target wage, w^τ .

Similar to McCall's random search model, this is a partial equilibrium framework.¹² However, our setting is the polar opposite to random search models with respect to the information available about vacancies: here the agent applies to a specific known job, whereas in McCall she draw a random vacancy. The directed-search model thus matches the empirical fact that job-seekers often accept the first offer they receive.¹³ In reality, job advertisements do indicate wage ranges or other information about job quality. Unemployed workers apply for jobs, but they do not have a fixed minimum acceptable wage. The target wage is thus real, whereas reservation wage is merely a

¹²Similar partial-equilibrium directed-search models are rarely used (for the only example that we are aware of see Baily (1978)). The focus of the literature on directed search has been the frictional unemployment created by the lack of coordination among the unemployed (Montgomery (1991), Moen (1997), and Shimer (1996)).

¹³For empirical evidence see Clark and Summers (1979). For a discussion of information structure differences, see Salop (1973).

"theoretical construct ... not observed in the data" (Cox and Oaxaca (1990)). This leads us to believe that the target wage should be easier to measure than the reservation wage.¹⁴ From a theoretical perspective, a random search model would deliver similar results as presented here, but our target-wage model has the advantage of being more tractable (see online Appendix A3).

Two sources of duration dependence are incorporated in the agent's maximization problem (3). The job-finding rate can vary over the unemployment spell, namely $\lambda = E(V, s, t)$. This is a reduced form modeling of decreasing job opportunities, which we refer to as *structural duration dependence*.¹⁵ Duration dependence can also be caused by the UI system if the benefit level is a function of time, $b(t)$. In practice, this is almost always the case given that most UI systems have a limited benefit duration.

How does a change in UI generosity affect expected job quality?

To answer this question, it is useful to write the expected re-employment job quality V^e in recursive form as:

$$V^e(t) = \lambda(t) V^\tau(t) + (1 - \lambda(t)) V^e(t+1), \quad (4)$$

where $V^\tau(t)$ denotes the optimal target job at time t after layoff, the solution of optimization (3), and $\lambda(t)$ the resulting job-finding rate. If we assume that jobs only differ in one dimension, i.e. wage, the same equation holds for the wage. As job quality is synonymous with wage in this case, we use both interchangeably from here on.

We start by considering a case without duration dependence, $b(t) = b$ and $E_t = 0$. Here a worker targets the same job independently of her unemployment duration, $V^e(t) = V^\tau(t) = V^*$ for all t , implying that a change in the timing of finding a job has no effect on the expected wage. In response to an increase in the benefit level in the first period after layoff, agents become more selective and increase their target wages. This leads to a change in expected wage of $\lambda(0) V_{b(0)}^\tau(0)$, which is always positive.

¹⁴In fact, the accepted wage observed in survey or administrative data is the target wage of the last period of unemployment. However, reservation wages are never captured in administrative data. In order to measure target wages during the unemployment spell, a survey should ask the unemployed about their most recent job application. To measure reservation wages, surveys ask about the minimum acceptable wage in case of an offer. In practice, the latter question cannot be asked without reference to the target job. For instance, the May 1976 Supplement to the Current Population Survey first asks "What kind of work were you looking for?", before asking "What is the lowest wage you would accept for this type of work?" (For other examples, see Rosenfeld (1977) and Krueger and Mueller (2011)).

¹⁵The theoretical literature has considered to many examples of such duration dependence, such as losing social contacts, diminishing idle human capital (e.g. Pissarides (1992), Acemoglu (1995), Ljungqvist and Sargent (1998)), screening (e.g. Lockwood (1991), Moscarini 1997), ranking models (Blanchard and Diamond (1994)), and stock-flow approach (e.g. Coles and Smith). For related empirical evidence, see Kroft et al. (2013), Autor et al. (2013), and Kudlyak et al. (2013).

With a negative duration dependence, $b_t \leq 0$ or $E_t \leq 0$, the target job quality will be decreasing over the unemployment spell, so that $V^\tau(0) > V^e(1)$. A benefits extension still increases the expected wage through the selectiveness effect. But duration dependence reduces the expected wage because longer unemployment duration leads to lower target wages. The effect of an increase in the initial period benefit level on expected wage is the sum of two offsetting forces:

$$\underbrace{V_{b(0)}^e(0)}_{\text{UI Wage Effect}} = \underbrace{\lambda(0) V_{b(0)}^\tau(0)}_{\text{Selectivity Effect}} + \underbrace{\lambda_{b(0)}(0) (V^\tau(0) - V^e(1))}_{\text{Duration Effect} \quad \text{Duration Depen.}}, \quad (5)$$

where lower case letters refers to partial derivative. Equation (5) reveals two insights about the UI wage effect: First, any duration dependence contributes to the negative force, diminishing the positive part of the UI wage effect through higher selectivity. In particular, a limited UI duration creates a negative duration dependence and thus a negative force. We will show that the negative force can theoretically prevail even in the absence of structural duration dependence. Second, keeping the selectivity constant, a higher search margin increases the negative force, and thus reduces the UI wage effect by increasing the UI duration effect. We will return to this point, as duration and wage effects are two main UI effects measured in the literature, and the predicted negative relation between them can thus be tested across prior studies and within our sample (Section 5). But first, let us generalize these intuitions for the effect of a change in initial UI benefits to the case of any change in the UI schedule, including the case of UI benefit extension.

Proposition 1 *The simultaneous effect of a change in the UI benefits in period t after layoff on expected job quality in the same period can be written as*

$$V_{b(t)}^e(t) = \lambda(t) V_{b(t)}^\tau(t) + \lambda_{b(t)}(t) (V^\tau(t) - V^e(t+1)). \quad (6)$$

The forward-looking effect of a change in UI benefits at time t on expected job quality at time k ($t \geq k$) can be written as the discounted sum of the simultaneous effects of UI changes on job quality,

$$\frac{\partial V^e(k)}{\partial u(b(t))} = \sum_{k \leq i \leq t} \beta^{t-i} S(k, i-1) S(i+1, t) \frac{\partial V^e(i)}{\partial u(b(i))}. \quad (7)$$

where the discounting is based on discount factor β , as well as $S(k, t)$, the survival likelihood between k and t .

To see the intuition, we focus on the case where $k = 0$, so that the left-hand side

of (7) is the effect of a change in benefits at time t on initial expected job quality. This corresponds exactly to the effect we measure empirically in [Section 3](#). A change in benefits at time t affects the agents' search decisions – the quality of targeted jobs, as well as the job-finding rate – in each preceding period. Each of these changes then is discounted by the likelihood of reaching that period, $S(0, i - 1)$. [Proposition 1](#) shows that the effect of these changes on the initial expected job quality can be written as a weighted sum of simultaneous UI-benefit effects on the expected job quality of each period, $\frac{\partial V^e(i)}{\partial u(b(t))}$, where the weights are related to the importance of the change in UI benefits at time t from the eyes of the unemployed agent at time i . More precisely, the weights take into account both the time-discounting, β^{t-i} , and the survival likelihood until time t , $S(i + 1, t)$.

The simultaneous effects are weighted by the survival likelihood because an agent is less likely to benefit from a change in future UI benefits, the further she is from the affected period(change). UI benefit extension creates an option value of search, since if a worker finds a job, the option of future UI benefits is lost. Continuing to search preserves that option.¹⁶ For this reason, the effect of an UI benefit extension on job quality is proportional to the survival likelihood until benefit exhaustion. This implies that the discussed negative correlation between the wage and duration effects of a change in the initial level of UI also holds true for an UI benefit extension once the survival likelihood of exhaustion is kept constant. [Section 5](#) investigates this prediction. The option value of search depicted in [Proposition 1](#) also implies that in response to a benefit extension, agents' reactions should be stronger, the closer they get to the time of UI exhaustion. [Section 6](#) will investigate this prediction.

Can more generous UI lead to lower job quality?

This will be the case if the negative force due to duration dependence prevails. At a first glance, this appears counter-intuitive since agents internalize the cost of longer unemployment, lowering their job opportunities and future earnings. However, the negative force can theoretically prevail because agents are not maximizing future earnings. We show this intuition with an example. Assume that the job-finding rate is given by $\lambda = as^{1-\sigma} \exp\left(-\frac{V}{\rho}\right)$ with $\rho > 0, \sigma \in (0, 1]$, and $a = ((1 - \sigma)\rho)^{-(1-\sigma)}$. Further assume that agents are risk-neutral, $u(c) = c$, and that the environment is stationary after the initial period, $b(t) = b1_{t=0} - \Delta b$, and $\rho(t) = \rho1_{t=0} - \Delta\rho$. In the online appendix, we shows that using the [Proposition 1](#) the UI wage effect can in this case be written as:

¹⁶This is similar to the "option value of work" idea of [Stock and Wise \(1990\)](#) in the case of retirement decisions.

$$V_{b(0)}^e(0) = \lambda(0) \left(1 - \frac{\Delta b + \Delta \rho}{\sigma \rho} \right). \quad (8)$$

As jobs only differ in wage here, the left-hand side can simply be written as $\frac{1}{1-\beta} w_{b(0)}^e(0)$.

Equation (8) implies that the UI wage effect is negative whenever the total duration dependence (UI-driven or structural) is strong enough, i.e. when $\Delta b + \Delta \rho > \sigma \rho$. In the absence of structural duration dependence, the UI wage effect can still be negative if the duration dependence created by a time-varying UI benefit is strong enough, i.e. $\Delta b > \sigma \rho$. In other words, the UI wage effect is negative when the base UI system creates enough non-stationarity. The reason for a negative wage effect is that agents maximize expected consumption, which takes into account wage when employed but also UI benefits when unemployed.

Furthermore, Equation (8) reconfirms that a higher search margin, *ceteris paribus*, leads to a lower UI wage effect. More formally, the more elastic is the hazard rate with respect to search effort, i.e. the lower is σ , the higher is the negative force relative to the positive one. It is important to note that all these results based on Equation (8) are unchanged, if risk-neutral agents are able to save. They also hold if agents are risk averse but face complete markets, i.e. the existence of employment-status contingent loans. This is due to that fact that in all these cases agents are maximizing discounted income.

3.2 Prediction I: Wage vs. Non-employment Duration Effects

How can we reconcile the presented positive UI wage effect with the fact that prior literature finds mainly zero and, at times, negative effects? In fact, the main body of literature has not found any UI effect on job quality. For instance, three recent papers using quasi-experiment designs and administrative data provide estimates of the UI wage effect that are not significantly different from zero (Card et al. (2007a), Lalive (2007) and Van Ours and Vodopivec (2008)). In contrast, Schmieder et al. (2014) find a *negative* UI wage effect.¹⁷

Can the negative relation between wage and non-employment duration effects reconcile different estimates of the UI wage effect in the literature? The first panel in Figure V

¹⁷Using Austrian data, Card et al. (2007a) results are based on a tenure-based RD design, while Lalive (2007) is the first paper exploiting an age-based RD design in this context. Van Ours and Vodopivec (2008) use an experience-based difference-in-difference approach in Slovenia. Schmieder et al. (2014) use an age-based RDD in Germany. For a review of the pre-2000 literature, see Addison and Blackburn (2000), and for a more recent contribution, see Le Barbanchon (2012).

provides a meta-analysis of a set of relatively precise estimates of the UI wage effect.¹⁸ All these studies also provide estimates of UI effects on non-employment duration. Figure Va confirms that studies with a higher estimated UI non-employment effect also estimate a smaller UI wage effect.

We also provide a test for the negative relation between the UI effect on non-employment duration and wage in our population. Ideally, we would like to estimate both UI effects for each agent, and then investigate the correlation between these two effects. However, given our RD design, we can only estimate each UI effect in a subpopulation. We first use pre-determined observable characteristics to generate subpopulations. More precisely, partitions are based on either using categorical variables, e.g. gender, occupation, industry, etc., or quantiles of continuous variables, e.g. pre-unemployment tenure, work experience, etc. For each subsample, we replicate the RD estimate of the UI effects on non-employment duration and wage. The correlation between these two sets of estimates informs us about the potential correlation at the individual level.

Figure Vb plots 530 estimates of the UI effects on non-employment duration and wage from the different subpopulations. The binned scatter plot shows a negative correlation and the fitted line represents a slope of statistically significant $-.00087$. This confirms the negative correlation between UI wage and non-employment effect in the meta-analysis of Panel a. We conclude from our analysis of Figure V that the heterogeneity in relative importance of search and selectivity margins seems to create enough variation to explain the different UI wage effect effects in the literature.

3.3 Prediction II: Option Value of Search

We presented two testable implications of the option value of search idea in [Section 4](#). First, as unemployed workers approach week 30 of unemployment, the UI effect on the job-finding rate and wages should increase. Figure VII and Figure VIII investigate this prediction by illustrating the evolution of the UI effect on the job-finding hazard and accepted wages during unemployment spell, respectively. Figure VII shows that as a result of the benefit extension, the job-finding hazard decreases during the first 30 weeks. This suggests that workers are forward-looking: the value of finding a job depends on the time to benefit exhaustion. We will discuss these results further in the

¹⁸Interestingly, all these relatively precise estimates are based on RDD methodology (see the above footnote). [Card et al. \(2007a\)](#) investigate an UI extension from 20 to 30 weeks, whereas [Lalive \(2007\)](#) investigates an extension from 39 to 52 weeks. The latter provides separate estimate for male and females. [Schmieder et al. \(2014\)](#) investigate the effect of two UI extensions, both for 6 months, one starting from 1 year, another from 1 year and a half. Figure V refers to these studies as CCW, Lalive, and SWB, respectively, while AW denotes our own estimates from Table 2. See section B2 in the online Appendix for more details.

online Appendix Section B2. Here we will focus on the UI wage effect.¹⁹

Figure VIIIa graphically illustrates the wage change around the age of 40. The figure shows a clearly discernible jump of about 5 percentage points for wages of jobs started between 30 and 39 weeks after layoff. Figure VIIIb plots the RD coefficients from different regressions of wage changes for each month of non-employment duration. It suggests that an extension of UI benefit from 30 to 39 weeks increases the target wage of the agent, not only within that period but also immediately before the extension. This pattern reflects a combination of two factors: true responses in search decision and dynamic selection (Heckman critique). In fact, as shown in [Section 2](#), the non-employment duration itself is responsive to UI. If this response is correlated with wage changes, we are concerned that compositional changes drive the dynamic wage effects. As a partial remedy for this concern, Figure VIII shows that controlling for a rich set of observables barely changes the RD coefficients.

The second implication of the option value of search is that an UI extension should have a stronger effect on agents with a higher chance of using the UI extension, i.e. a higher survival rate until benefit exhaustion. This pre-determined probability of exhaustion leads to different option values of the search. We use an out-of-sample prediction of benefit exhaustion, based on a non-parametric regression of ex-ante characteristics of workers and firms.²⁰ Table A7 in the online Appendix shows that the UI wage effect in our sample stems entirely from the group with a large predicted benefit exhaustion likelihood. In sum, we interpret Figures VIII as suggesting that the effect of UI extension is more pronounced for workers getting close to benefit exhaustion.

4 Mechanisms Underlying the Positive UI Wage Effect

Which mechanisms drive the positive UI wage effect estimated in Section 3? We provide three empirical findings to shed light on this question.

4.1 UI Benefit Extension Attenuates Wage Drops

We have shown that a more generous UI system leads to jobs with higher wages on average. Now we would like to know whether this positive UI wage effect is due to a lower likelihood of a wage drop or an increase in the likelihood of a wage raise relative

¹⁹For other investigation of the UI effect on search decisions conditional on unemployment duration see [Centeno and Novo \(2011\)](#), [Caliendo et al. \(2013\)](#), [Schmieder et al. \(2014\)](#).

²⁰The prediction is based on observations out of the estimate sample in order to avoid the problem of over-fitting ([Abadie et al. \(2013\)](#)). See Appendix B for more details.

to the pre-unemployment job. Figure VI shows that the benefit extension attenuates the probability of large wage drops, and increases the likelihood of obtaining mild wage raises, but it has no effect on the likelihood of substantial wage raises. In particular, we estimate a series of RD models for the probability of experiencing wage-growth in specific intervals (PDF) across the distribution and plot the coefficient estimates along with the confidence intervals in panel b. Panel a replicates the same exercise for the CDF of the wage growth distribution. For instance, panel b shows that the UI benefit extension affects the likelihood of experiencing a wage drop of 40 to 60 percent by around 0.3% (left y-axis) from a base of 4% (right y-axis).

Figure VI shows that the likelihood of experiencing a wage drop of more than 40% decreases by 0.5% due to the benefit extension, while the likelihood of achieving a wage raise of below 10% increased by the same amount, with no effect on the distribution of larger wage raises.

The non-uniform UI effect across the wage distribution is consistent with our theoretical model of [Section 3.1](#). For each agent, the benefit extension effect on the wage distribution is not uniform due to the option value of search and the non-marginal change in duration. Across-agent heterogeneity can also cause a non-uniform UI effect. In fact, the benefit extension affects more intensely agents with higher unemployment duration, which is negatively correlated with between-job wage change. Another plausible explanation for the non-uniform UI effect is that workers are not seeking jobs with wages below their UI benefit level while on UI, either because of relatively easy on-the-job search ([Burdett \(1978\)](#)) or reference dependence ([DellaVigna et al. \(2014\)](#)). In this case, the 55% average net replacement creates the pattern observed in Figure VI. This is also consistent with the fact that the UI effect is more substantial between 30-39 weeks after layoff ([Section 3.3](#)).

4.2 UI Benefit Extension Affects Firm-Sorting

Do workers use the extended UI benefits to find better-paying firms, or do they find a better-paid job within the same type of firm? Our approach to answer this question is to compare post-unemployment firm characteristics of workers below and above the age cutoff in our RD model.²¹ Workers who are eligible for a UI benefit extension are shifting toward 'better' firms: they find jobs in larger firms, with higher proportions of male and older workers, which on average pay higher wages to their other workers (Columns 1,

²¹For example, instead of using a worker's post-unemployment wage in the new job as the dependent variable, in our RD design, we now use the average wage of workers of the new firm in the last 2 years prior to her hiring.

3, 4, 5 and 6 of Table 3, respectively).²² However, most of the estimates are not precise enough to allow firm conclusions about the degree to which the UI wage effect is driven by firm sorting. We conclude that a more generous UI affects the sorting of employees across firms.²³

To interpret our results, we divide the potential drivers of the UI wage effect into three categories: (i) UI affects wages without changing the assignment of workers and jobs, e.g. by changing workers' bargaining power at the expense of capital or other workers, (ii) UI affects employee-employer assignments, i.e. subsidizing search leads to better matches (Marimon and Zilibotti (1999)), (iii) UI leads to the creation of better jobs, e.g. by changing the capital-labor ratio (Acemoglu and Shimer (1999)) or by increasing the size of more productive firms (Acemoglu (2001)). The presented evidence in Table 3 suggests that UI affects the type of firms that the unemployed will join. This is evidence for the second and third channels, and suggests that the UI wage effect is accompanied by the creation of better matches or better jobs, rather than an increase in workers' bargaining power.

4.3 Other Measures of Job Quality

Although wage is arguably the main and most salient characteristic of a job, the prediction of the theory presented in Section 4 should theoretically apply to all job characteristics. Following the same RDD method, we now investigate the UI effect on other job quality measures observed in our administrative data. The UI benefit extension from 30 to 39 weeks does not have a statistically significant effect on either of those measures. In particular, the benefit extension has no effect on the duration of the new job (columns 1 and 2 in Table 4).²⁴ This is pertinent, as matching models predict that better jobs should, on average, last longer (Jovanovic (1979)). Furthermore, we find a precise zero effect of the benefit extension on wage growth in the new job (column 3 in Table 4). The last four

²²Male workers are more likely to bargain with their employers, perhaps resulting in firms with higher proportions of male workers to pay on average higher salaries (For recent evidence and references see Card et al. (2014)).

²³A more precise, but computationally demanding, approach would be to estimate the firms' component of the UI wage effect using the Abowd et al. (1999) methodology. This would allow us to decompose our estimated UI wage effect into three parts: The UI effect on individual fixed effects, on firm fixed effects and the residual. The first effect in this decomposition should be zero, given the validity of the RD design. In the current exercise, we increase the precision of within-firm averages by focusing on workers laid-off from firms with more than 10 employees, and exclude temporary layoffs, who often re-join their previous employer. Appendix Table A8 shows that this sample restriction does not change the point estimates and increases the precision.

²⁴We further investigate the UI effect on post-unemployment tenure by looking at the effect on tenure distribution instead of average. The results confirmed the absence of UI effect on tenure.

columns of Table 4 demonstrate that there is no significant effect of extended UI neither on the likelihood that unemployed agents are recalled, nor on the likelihood that they change occupation, industry, or geographical location.

The estimates in Table 4 suggest that UI has no economically significant effect on non-wage measure of job quality in our sample. Moreover, Austrian data provide us with refined measures of all presented job-quality characteristics, except occupation (measures as blue vs. white collar). We conclude that the potential benefit duration, in our setting, has two main effects only: it lengthens the period of non-employment and increases the wage in the new job.

5 Policy Implications

Does the effect of UI on job quality change the optimal generosity (level and duration) of UI? This section answers this question using a sufficient statistic approach based on our estimations of [Section 2](#). Following the literature on optimal UI design, we neglect potential general-equilibrium effects of UI on non-UI-recipient workers.²⁵ The last assumption holds true, for instance, if the wage gains stem from the creation of better matches or higher-wage jobs, but not if laid-off workers find higher-wage jobs at the expense of other workers. [Section 4.2](#) suggests that empirical evidence supports this assumption.

Using our model in [Section 3](#), we consider a UI design problem in the presence of two real-world features of UI. First, a UI system with a fixed benefit level and limited benefit duration, b vs. B .²⁶ Second, benefits are financed through a proportional tax on earnings. This is the case in most of the countries, in contrast to a lump-sum tax assumed in the previous literature. The planner's budget constraint is

$$\tau (1 - n) w^e - b\tilde{n} = 0, \tag{9}$$

where n stands for the expected duration of non-employment, \tilde{n} stands for the expected

²⁵Following the pioneering work of [Baily \(1978\)](#), prior literature mainly focused on UI effect on unemployment duration of eligible workers, neglecting both the effect on wage of eligible workers and UI effect on non-eligible workers. In contrast, we focus on *both* partial equilibrium effect of UI, namely on eligible workers' unemployment duration and job quality. However, still we abstract from potential general equilibrium effect. Recent papers studied general equilibrium effect [Hagedorn et al. \(2013\)](#), [Lalive et al. \(2013\)](#), [Landais et al. \(2014\)](#), [Marinescu \(2014\)](#), and [Di Maggio and Kermani \(2015\)](#).

²⁶This restriction is realistic. "The duration of benefits is one of the most easily accessible policy tools for dealing with unemployment. While it is often difficult for Congress to agree on what to do about benefit levels or general eligibility rules when unemployment is high, benefit entitlement is frequently extended." [Holen \(1977\)](#). For study of an optimal dynamic of benefits, see [Shavell and Weiss \(1979\)](#), [Hopenhayn and Nicolini \(1997\)](#) and [Shimer and Werning \(2008\)](#).

non-employment duration covered by UI. The wage effect of UI only affects the welfare through its effect on the planner's budget constraint (fiscal externality) because of individual optimization, i.e. the envelope theorem. Traditionally, the main source of fiscal externality is through the UI non-employment duration effect. It is of interest to compare this fiscal externality with the externality arising from the new channel, i.e. the UI wage effect.

The welfare cost of a change in potential benefit duration is the fiscal externality, which can be decomposed into two parts:

$$Fiscal\ Externality = \underbrace{\tau (1 - n) \Delta w^e}_{\text{Fiscal Externality due to wage effect}} - \underbrace{(\tau w^e \Delta n + b \Delta \tilde{n})}_{\text{Fiscal Externality due to duration effect}} \quad (10)$$

The first term is the effect of the UI wage effect on the government budget, a positive fiscal externality. Agents do not internalize that their search decisions has an externality: a change in reemployment wage implies a change in future labor income tax. This externality is directly due to the proportionality of UI tax. The second and third terms represent the traditional negative fiscal externality in case of limited UI duration: lower tax revenue due to longer non-employment and higher UI expenditure.

Our estimates from Table 2 show that a nine-week extension of UI benefit eligibility increases reemployment wages by .5 percent, and increases the average non-employment spell, Δn , by two days. The change in \tilde{n} has two components, marginal and infra-marginal, equal to 1.5 and 6.4 days, respectively. Finally, the average post-unemployment job tenure is 81 weeks (see Section 1). Inserting these values, we get $\Delta GB = (20\% - 45\%) w^e$. This implies that the positive fiscal externality of the nine-week UI benefit extension is equal to 20% of the average weekly wage, as compared to the traditional negative moral-hazard externality of -45% . The overall fiscal externality is thus equal to -25% , only half as big as the externality of -45% if we had ignored the wage effect.

More generally, we can characterize the optimal UI design in the presence of two real-world features of UI: limited duration and proportional UI tax.

Proposition 2 (*Optimal UI with limited duration*) Suppose that the agent has a separable utility between consumption and leisure ($u(c) - v(l)$), and does not discount the future.²⁷ Then optimal

²⁷As discussed in footnote 33, this is a good approximation for realistic values of the discount rate (Shimer and Werning (2007)).

UI satisfies the following conditions:

$$\mathbb{E} \left(\frac{w}{w^e} \frac{u_c((1-\tau)w)}{u_c(b)} \right) = \frac{1}{\varepsilon_{\tau,b}} \quad (11)$$

$$\kappa \frac{u(b) - u(0)}{bu_c(b)} = \frac{\varepsilon_{\tau,B}}{\varepsilon_{\tau,b}}, \quad (12)$$

where κ is the proportion of total UI benefits received by agents who exhaust their benefits, $\kappa = \frac{S(B)B}{\bar{n}}$.²⁸

The first condition trades off the cost of fiscal externality and the benefits of consumption smoothing. It nests the Baily–Chetty formula (Baily (1978), Chetty (2006)) as a special case, under stationarity and the absence of the UI wage effect. More importantly, the second condition weighs the trade-off between the benefit level and duration. The relative welfare gain from benefit duration and level, the left-hand side, is the gain from increasing the utility of agents who receive benefits until exhaustion from $u(0)$ to $u(b)$ normalized by the marginal utility of consumption. The parameter κ is a measure of the option value of search as the extension is more valuable to agents who are more likely to exhaust benefits (Section 3).

Following the consumption-based approach (Baily (1978), Gruber (1997), Chetty (2006)), we can write our optimal UI conditions as a function of consumptions in different states.

Corollary 1 *The UI system is optimal iff*

$$\gamma \left(\frac{1}{\rho} - 1 \right) \approx 1 - \frac{1}{\varepsilon_{\tau,b}}, \text{ and} \quad (13)$$

$$\kappa \left(1 + \frac{\gamma}{2} \right) \approx \frac{\varepsilon_{\tau,B}}{\varepsilon_{\tau,b}}. \quad (14)$$

where γ stands for the coefficient of relative risk aversion.

Two insights illustrated in this Corollary are worth emphasizing. First, a higher degree of risk aversion implies longer (lower) UI duration (benefit) at the optimum. As there is no consensus on the empirical value of risk aversion, we will instead use the two conditions to eliminate the coefficient of relative risk aversion (see the online Appendix). This implies, for instance, that for the optimal replacement rate to be below 2/3, which is the case in most countries, the fiscal externality of the UI benefit level should be higher than that of UI duration ($\varepsilon_{\tau,b} > \varepsilon_{\tau,B} \iff \frac{2}{3} > \rho$).

²⁸ $\varepsilon_{y,x} \equiv \frac{\partial \ln(y)}{\partial \ln(x)}$ represents the elasticity of y with respect to x .

6 Conclusion

For more than three decades, the effect of UI on job quality has been a controversial topic. While the early institutional literature on UI and theoretical search models suggest that UI allows the unemployed to find jobs that are better suited to their skills, empirical work has not found evidence of a positive causal effect of UI generosity on the quality of re-employment jobs. This did not change economists' beliefs, however. [Layard et al. \(2005\)](#) wrote in their classic text on unemployment: "It is clear that we should expect to see significant benefit effects on wages. However, the evidence here is very thin, not least because in many countries important changes in the benefit system are very infrequent".

This paper has taken advantage of a discontinuity in the Austrian UI system and identified a positive UI effect on re-employment wages. We reconcile this finding with previous literature using a theoretical model that shows that the UI wage effect is the result of two off-setting forces and thus it can, in theory, take any sign and magnitude. The model predicts a negative relation between the UI wage effect and the UI effect on non-employment duration that holds across estimates in the literature. We also provide a direct test of this prediction in our data.

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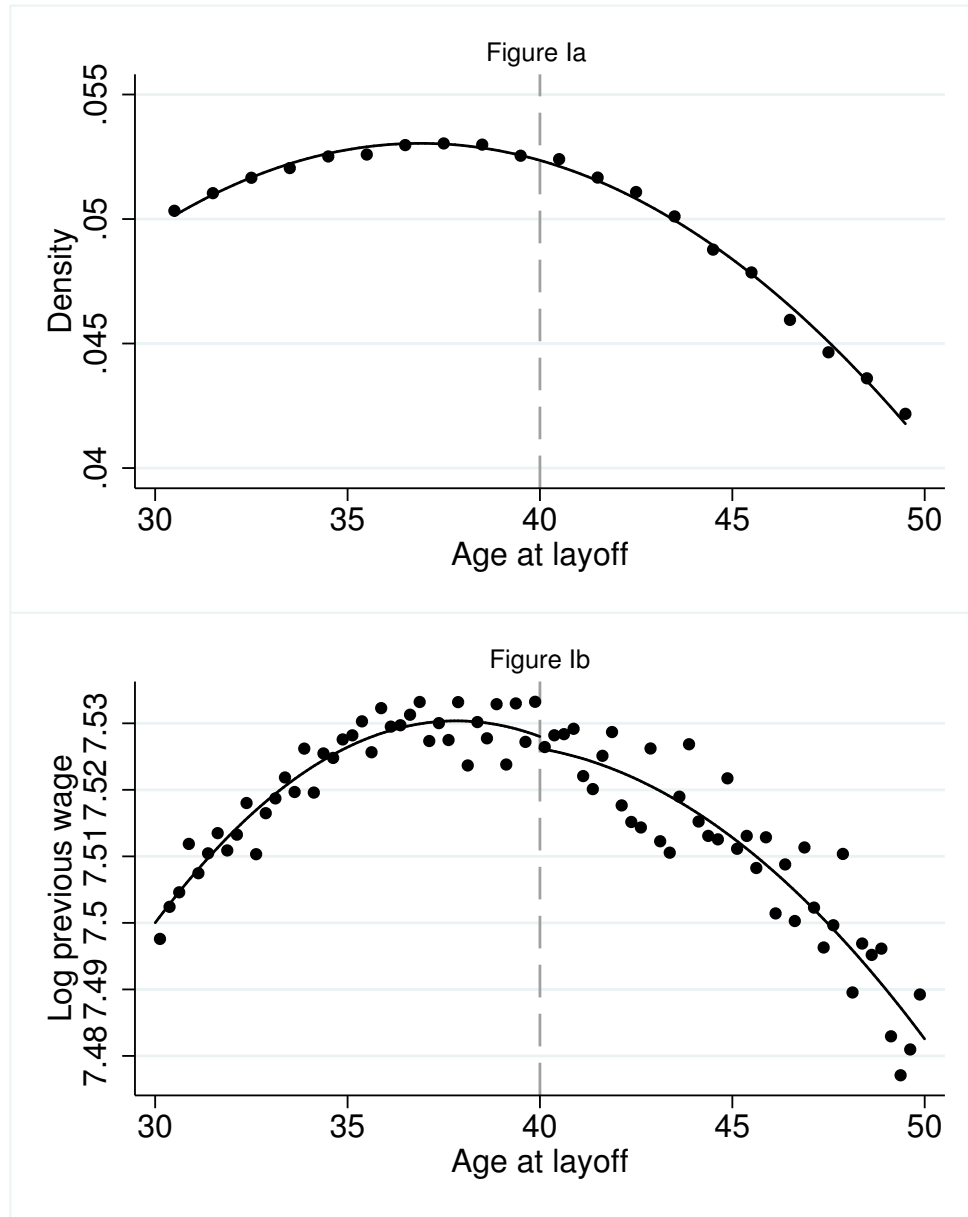
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Figure I: RDD Validity Tests

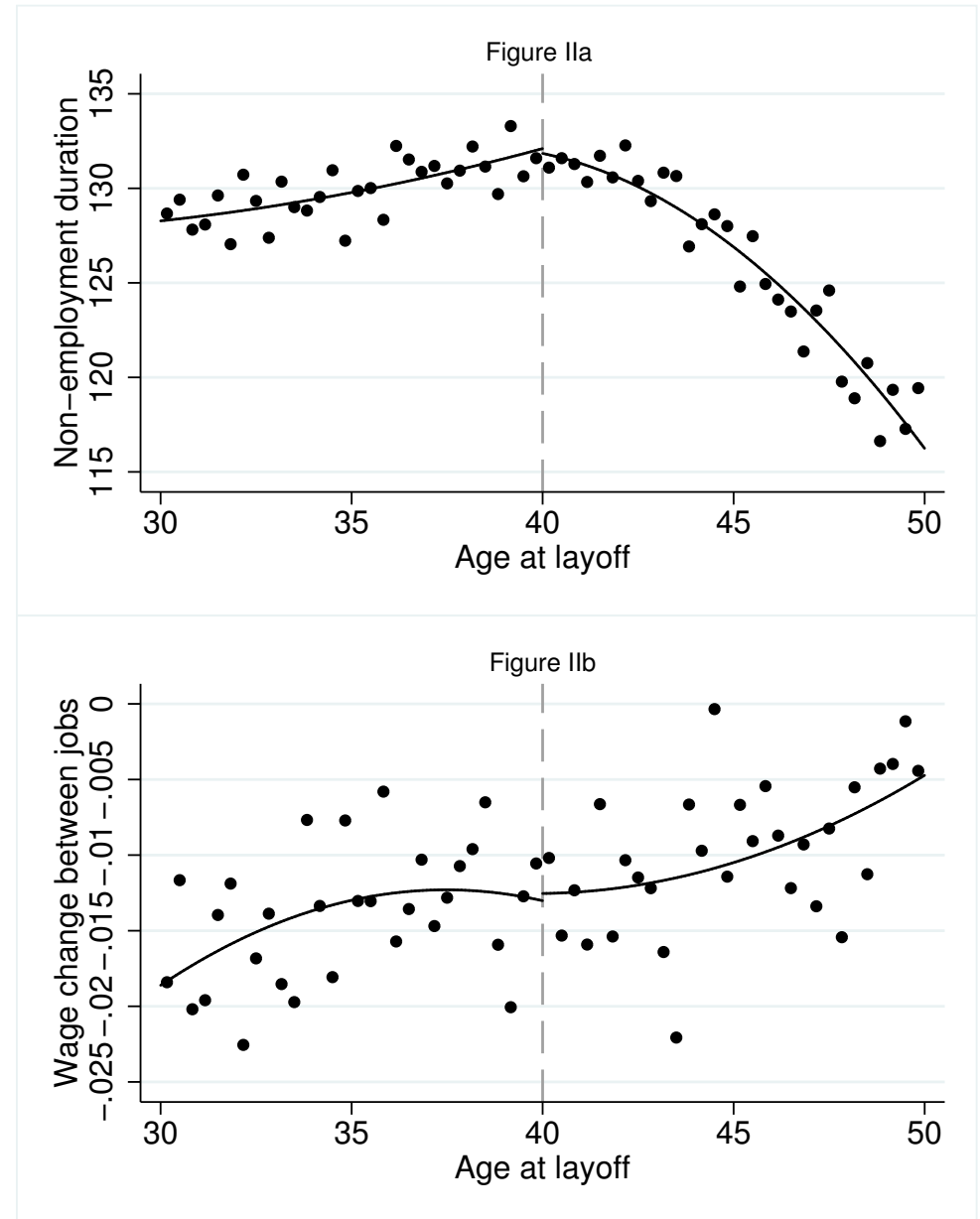
Distribution of Age & Covariates Around UI Extension Threshold



Note: This figure provides two of several RDD validity tests. The dashed vertical line denotes the cutoff for UI eligibility extension from 30 to 39 weeks at the age-40 threshold. The solid lines represent quadratic fits. The subfigure Ia plots the distribution of age at layoff (the assignment variable). Figure Ib shows how previous wage, the key observable characteristics, evolve around the UI extension eligibility threshold.

Figure II: Placebo Test

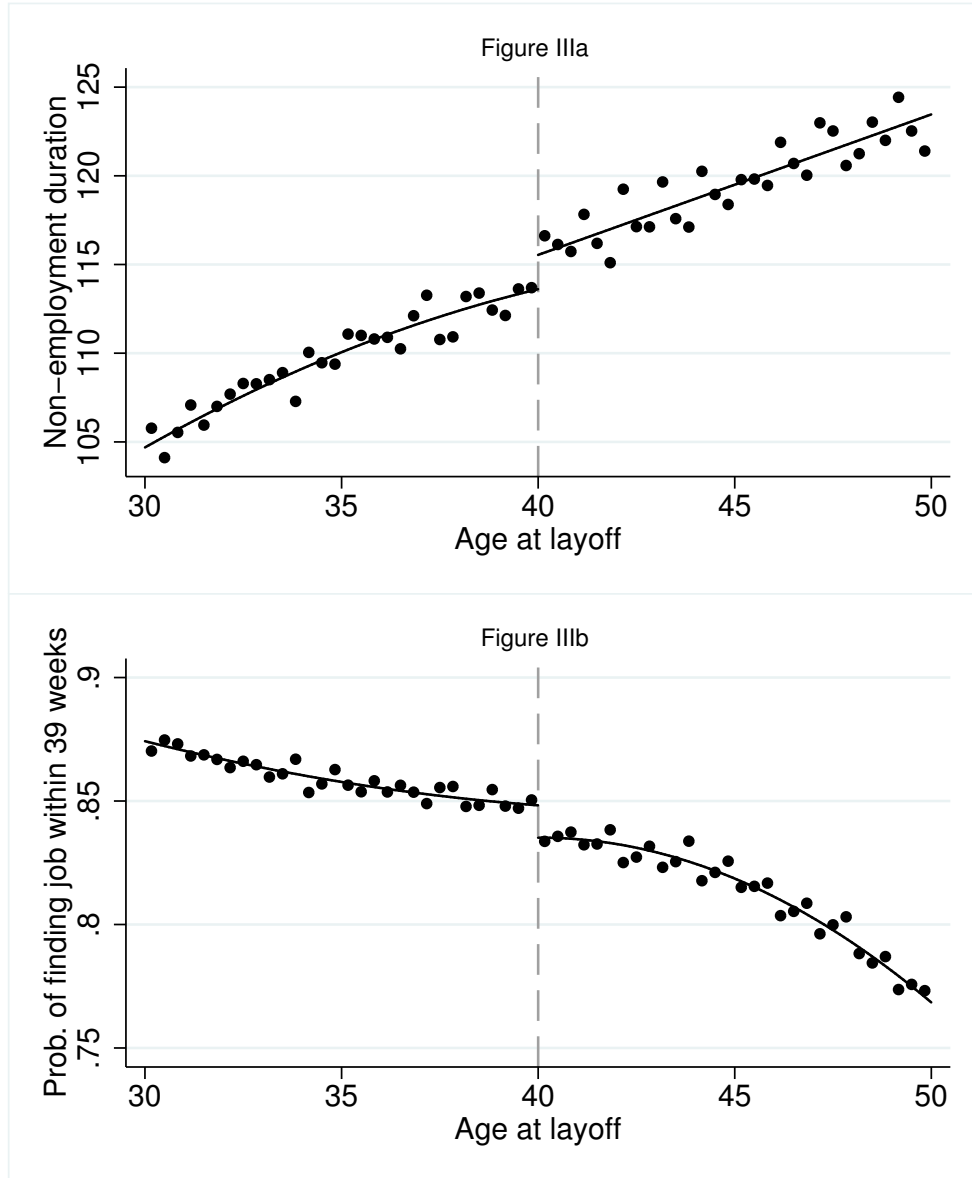
Pre-Reform Discontinuity



Note: Figure IIa plots non-employment duration for each age for pre-reform period when there is no discontinuity in UI benefit duration at age 40. Figure IIb plots average change in log wage between pre- and post-unemployment jobs for each age. For both sub-figures observations with non-employment durations of more than two years are excluded. The dashed vertical line denotes the cutoff for UI eligibility extension from 30 to 39 weeks at the age-40 threshold. The solid lines represent quadratic fits.

Figure III

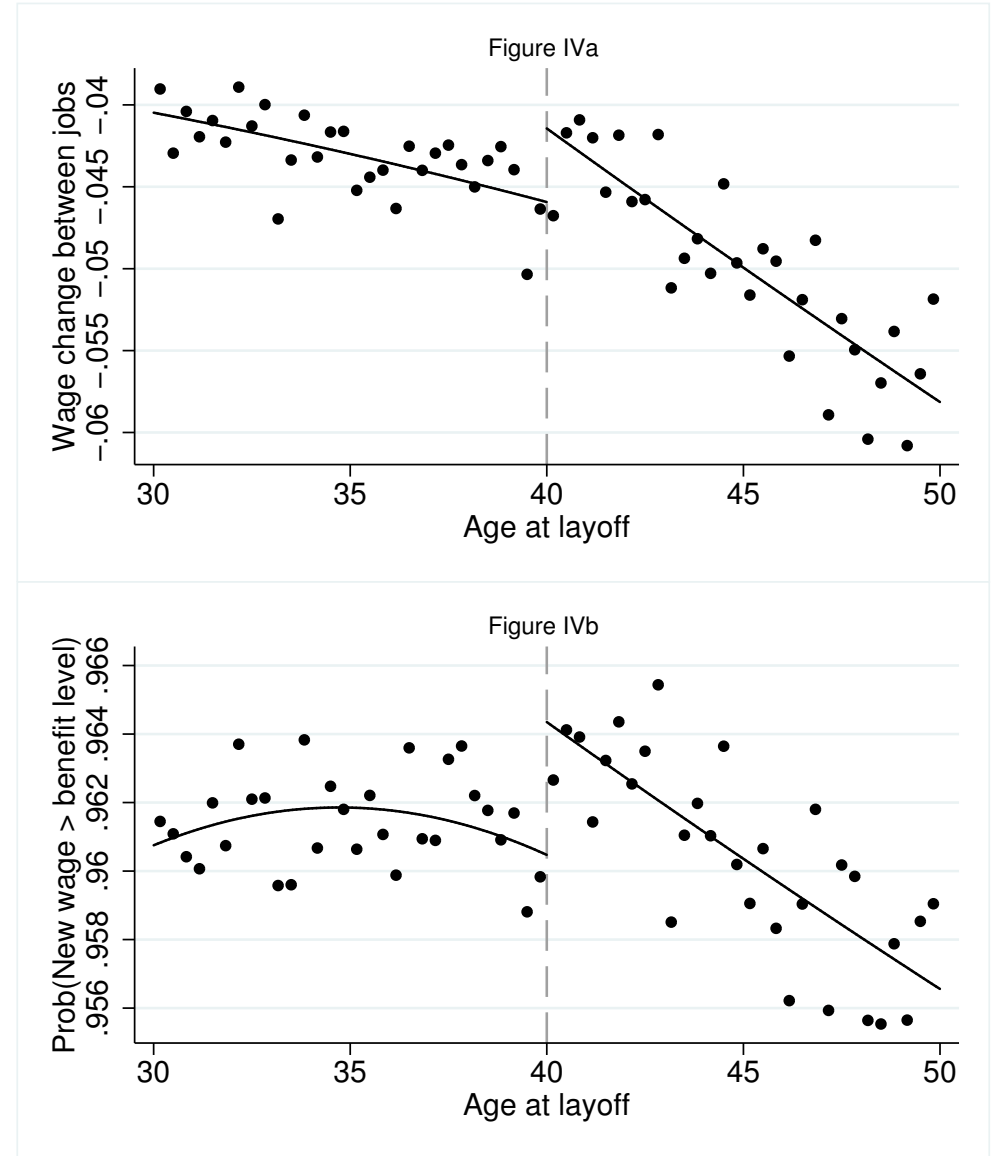
UI Effect on Non-Employment Duration



Note: Figure IIIa plots average non-employment durations (time to next job) for each age. Observations with non-employment durations of more than two years are excluded. Figure IIIb plots the probability of finding a job within 39 weeks of layoff for each age. The dashed vertical line denotes the cutoff for UI benefit eligibility extension from 30 to 39 weeks at the age-40 threshold. The solid lines represent quadratic fits.

Figure IV

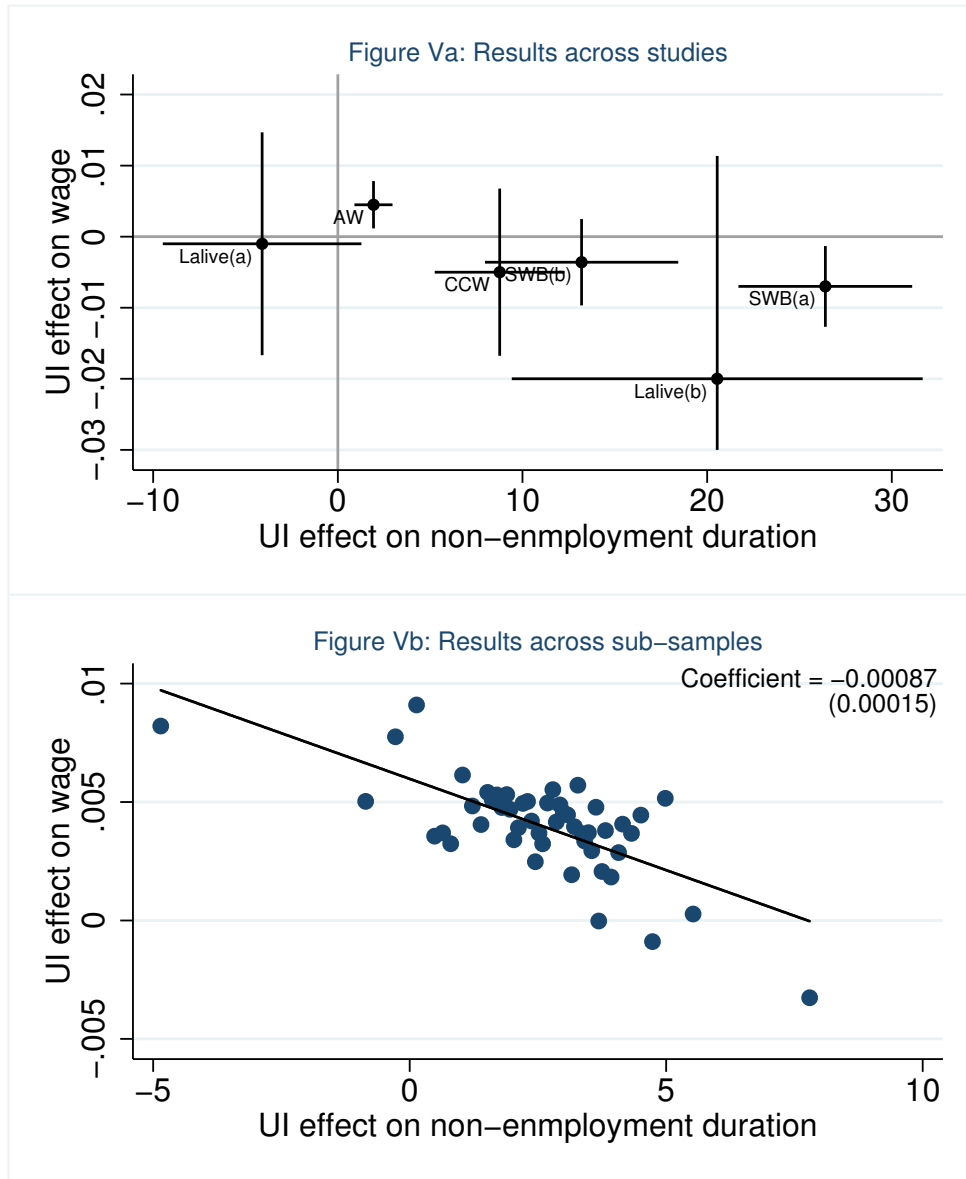
UI Effect on Wage



Note: Figure IVa plots average change in log wage between pre- and post-unemployment jobs for each age. Figure IVb plots the probability that the new wage is higher than the UI benefit level. For both sub-figures observations with non-employment durations of more than two years are excluded. The dashed vertical line denotes the cutoff for UI eligibility extension from 30 to 39 weeks at the age-40 threshold. The solid lines represent quadratic fits.

Figure V

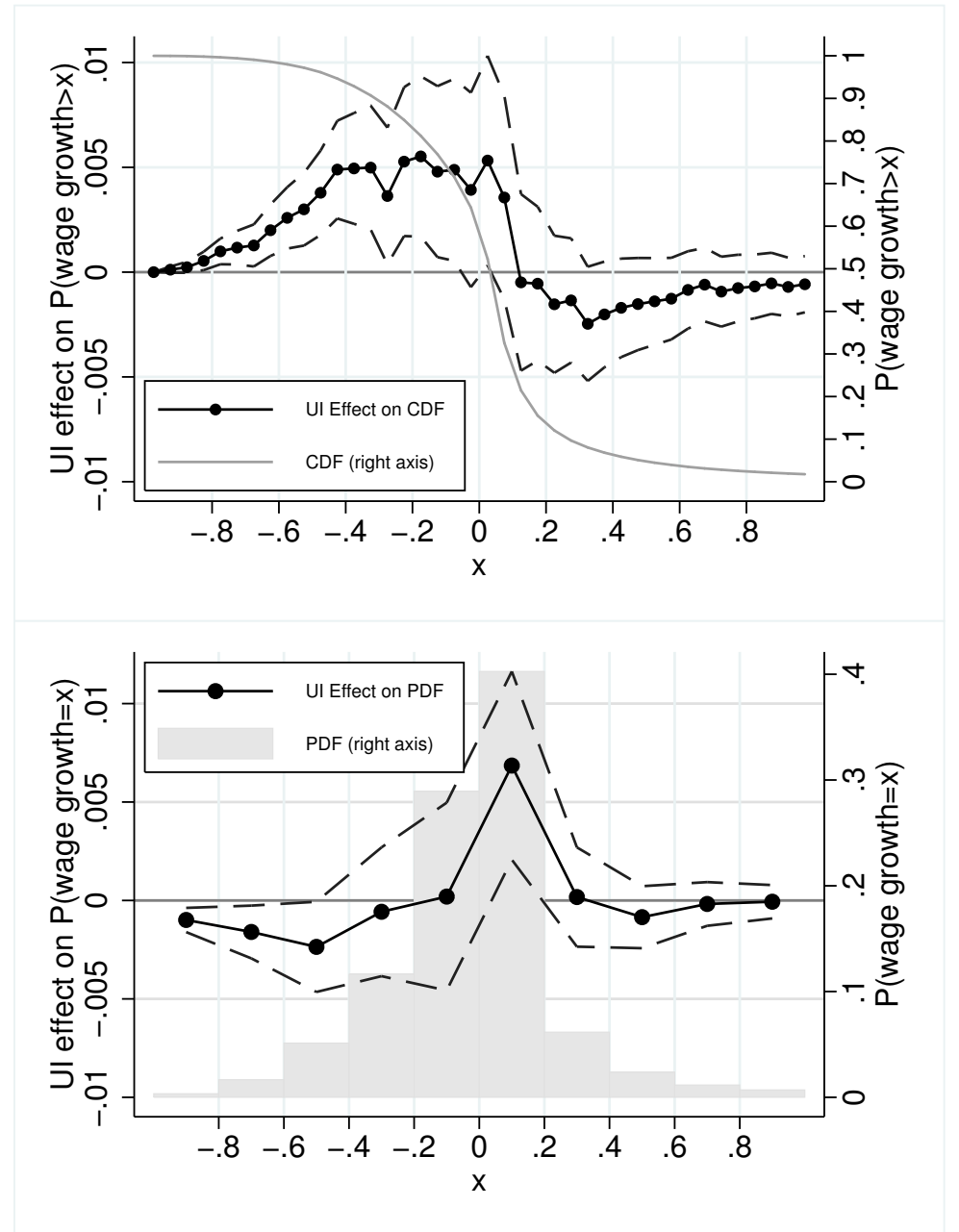
Wage vs. Non-employment Duration Effects



Note: This figure provides empirical evidence for a negative relation between the UI extension effect on non-employment duration and its effect on post-employment wage. The top sub-figure offers a meta-analysis and the bottom sub-figure investigate the relationship within subsamples of our population. Sub-samples are selected using ex-ante pre-determined observables, e.g. industry, occupation, tenure, etc. Panel b is a binned scatter plot of two elasticities for subsamples with more than 100,000 observations. The solid line and the coefficient correspond to the best linear fit on the underlying data using OLS.

Figure VI

UI Benefit Extension Attenuates Wage Drops



Note: This figure investigate the effect of UI extension from 30 to 39 weeks on the between-job wage growth. The first subfigure plots the effect on CDF of wage growth distribution on left axis (), the CDF itself on right axis (solid line). The bottom subfigure plots the same way for PDF of wage growth instead of CDF.

Figure VII

Dynamic Effect of UI Benefit Extension on Hazard Rate

Figure VIIa

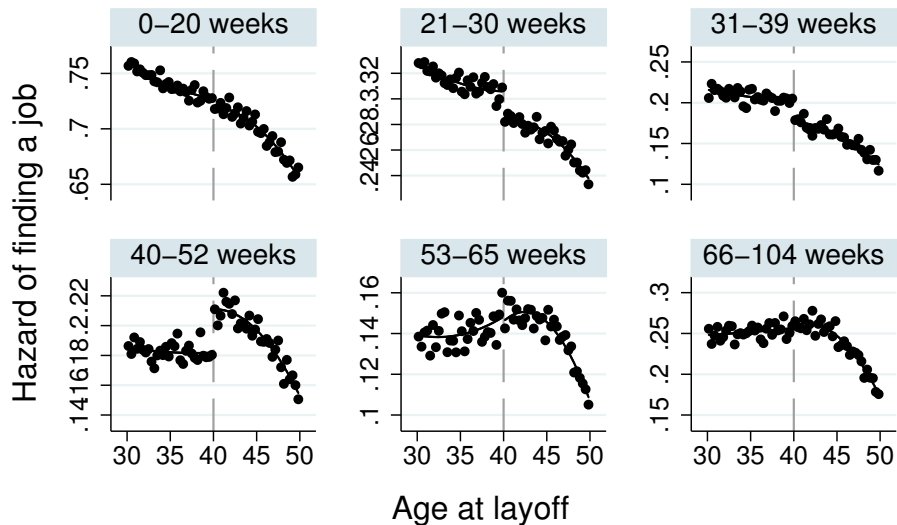
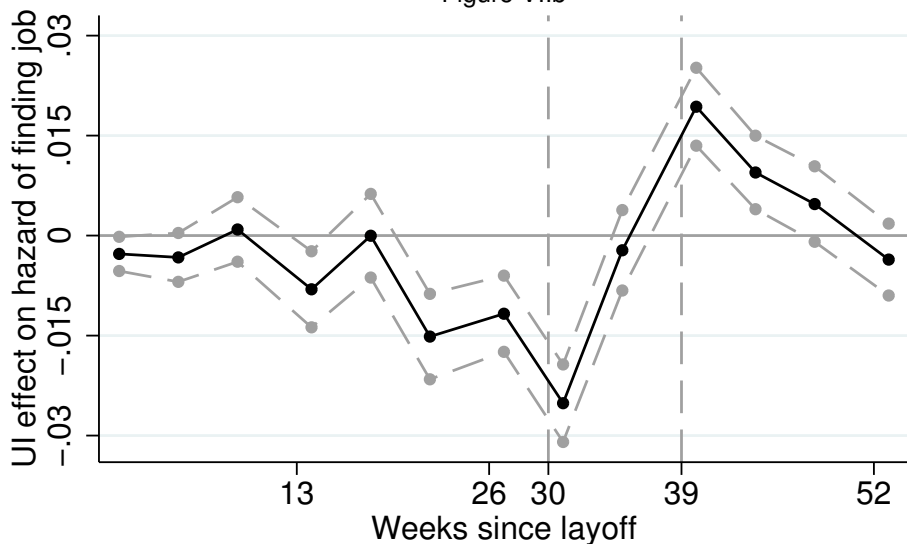


Figure VIIb



Note: Figure VIIa investigates the dynamic effect of a UI benefit extension from 30 to 39 weeks on job-finding rate. It plots the job-finding rate across the non-employment period against the age at layoff. Figure VIIb plots the RD coefficients from different regressions for monthly hazard rates.

Figure VIII

Dynamic Effect of UI Benefit Extension on Wage

Figure VIIIa

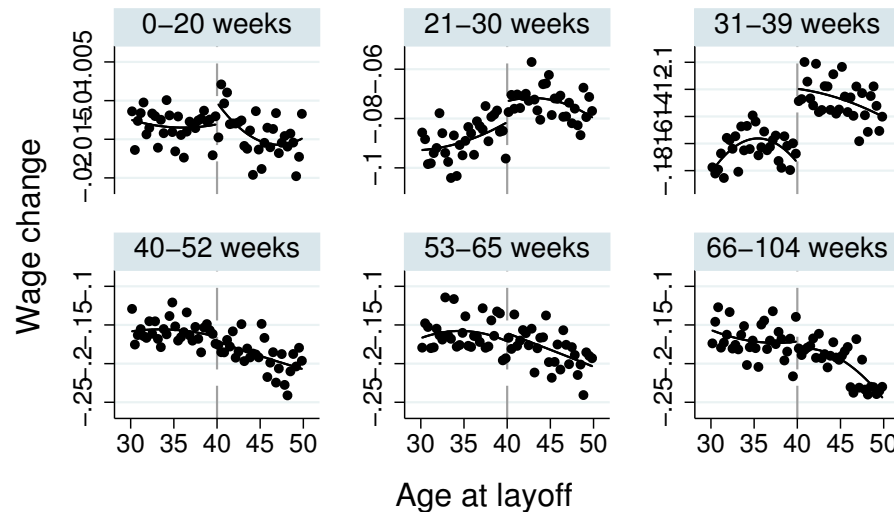
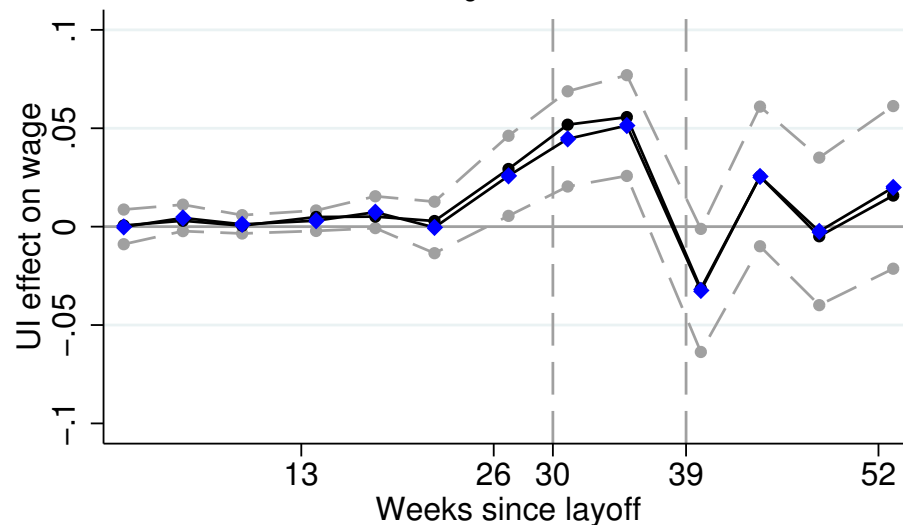


Figure VIIIb



Note: Figure VIIIa investigates the dynamic effect of a UI benefit extension from 30 to 39 weeks on wages. It plots log wage changes for individuals who exit unemployment in different periods against the age at layoff. Figure VIIIb plots the RD coefficients from different regressions for wage change in each non-employment month, with and without controlling for covariates, black dots and blue squares, respectively.

Table 1: Descriptive Statistics

	Population	Sample 1	Sample 2	Final Sample
	(1)	(2)	(3)	(5)
Female	0.40	0.33	0.26	0.25
Married	0.33	0.46	0.54	0.53
Age	36	37	40	40
	(11)	(11)	(6)	(6)
Education, more than compulsory	0.57	0.52	0.51	0.53
Blue-collar	0.56	0.74	0.77	0.76
Tenure (days)	1 290	907	1 008	984
	(1687)	(1373)	(1371)	(1384)
Share of time employed				
Last 2 years	0.87	0.83	0.88	0.88
Last 5 years	0.77	0.75	0.87	0.87
Monthly wage (real Euros)	1 663	1 614	1 798	2 007
	(2417)	(1534)	(1702)	(1875)
Non-employment duration (days)	87	122	116	114
	(133)	(117)	(111)	(113)
Wage change	0.020	-0.019	-0.041	-0.046
	(0.376)	(0.343)	(0.323)	(0.315)
Post-unemployment tenure (days)	938	554	606	558
	(1443)	(962)	(1016)	(868)
Observations	17 192 624	5 942 834	2 261 089	1 738 787
Sample restrictions:				
Age	20-60	20-60	30-50	30-50
Minimum tenure of 28 weeks	Yes	Yes	Yes	Yes
Laid-off workers		Yes	Yes	Yes
Experience 3 years over 5 years			Yes	Yes
6 years over 10 years			Yes	Yes
Layoff after August 1, 1989				Yes

Note: The sample covers the universe of private-sector job separations in Austria for the period of 1980-2011. Non-employment duration is the duration of the period between the end of a lost job and the start of a new job. Non-employment duration and wage growth represent averages for workers who find a job within 2 years of separation.

Table 2: Effect of UI Benefit Extension from 30 to 39 Weeks

		Dependent variable					
Covariates		Non-employment duration	Find job within 30 weeks	Find job within 39 weeks	Wage change between jobs	Log re-employment wage	New wage > UI benefit
		(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity at age 40	No	1.932*** (0.526)	-0.00988*** (0.00178)	-0.0131*** (0.00164)	0.00449*** (0.00170)	0.00350 (0.00234)	0.00388*** (0.00105)
	Yes	1.918*** (0.466)	-0.00843*** (0.00153)	-0.0119*** (0.00146)	0.00455*** (0.00146)	0.00503*** (0.00155)	0.00385*** (0.00102)
Mean of dep. var. around cutoff		114.7	0.806	0.842	-0.0440	7.468	0.962
Observations		1,589,178	1,738,787	1,738,787	1,187,476	1,189,446	1,187,476

Note: This table reports the coefficient of the age-above-40 indicator controlling for a quadratic polynomial, which allows for different coefficients on each side of the cutoff. Unemployment spells are censored at 2 years, except when studying hazard rates in columns 2 and 3. The unit of time for non-employment duration is days. The mean of the dependent variable for three years around the cutoff is reported. "Wage change between jobs" is defined as the change in the log of the average monthly wage in post vs. pre-employment jobs, where the average is taken over the last (first) calendar year for the pre (post)-unemployment job. The wage effect regressions (columns 4-6) are based on a smaller sample because the re-employment wage is not distinguishable from the previous wage for short recalls falling within the same calendar year (see Section 1). The covariates used are individual characteristics, such as gender, marital status, a dummy for Austrian citizenship, education, tenure, experience during the last 2 and 5 years, month of layoff, calendar week of layoff; and previous firm's characteristics such as industry, frequency of layoff, and proportion of recalls.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 3: Firm Sorting Effect of UI Benefit Extension from 30 to 39 Weeks

		Firm-level Outcomes						Individual level Outcomes
Covariates		Change in Firm size	New firm size > Old firm size	Change in male proportion	Change in average age	Change in log average wage	Change in residual wage	Wage change
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discontinuity at age 40	No	11.04 (14.87)	0.00189 (0.00427)	0.00167 (0.00228)	0.00324*** (0.00125)	0.00482* (0.00272)	0.00704** (0.00316)	0.00680** (0.00313)
	Yes	11.36 (14.65)	0.00292 (0.00406)	0.00115 (0.00194)	0.00269** (0.00107)	0.00491** (0.00237)	0.00751** (0.00307)	0.00494* (0.00266)
Mean of dep. var. around cutoff		382.6	0.400	-0.0254	-0.00387	-0.0621	-0.00988	-0.105
Observations		454,990	456,114	454,547	454,971	454,401	429,504	456,114

Note: This table reports the coefficient of the age-above-40 indicator controlling for a quadratic polynomial, which allows for different coefficients on each side of the cutoff. The sample excludes temporary layoffs as well as workers who are laid-off from a firm with less than 10 workers. The results are based on the same RDD as Table 2 and described in Section 1, but instead of using a worker's outcomes as the dependent variable, this table uses the firm-level outcomes (average over workers in the firm excluding the unemployed herself) in the last 2 years prior to layoff.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 4: Effect of UI Benefit Extension from 30 to 39 Weeks

		Dependent variable								
		Tenure in new job	Separation within a year	Wage growth in new job	Positive wage growth in new job	Probability of changing firm	Probability of changing industry	Probability of changing occupation	Probability of changing region	Probability of changing Zip-code
Covariates		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Discontinuity at age 40	No	1.202 (4.034)	0.000923 (0.00208)	1.03e-06 (0.000347)	-0.00126 (0.00240)	-0.00318 (0.00230)	-0.00101 (0.00206)	-0.000714 (0.00113)	0.00133 (0.00143)	0.000518 (0.00225)
	Yes	0.294 (3.801)	0.000333 (0.00189)	-0.000190 (0.000342)	-0.00203 (0.00238)	-0.00221 (0.00154)	-0.00103 (0.00169)	-0.000555 (0.00108)	0.00136 (0.00134)	0.00136 (0.00162)
Mean of dep. var. around cutoff		567.3	0.724	0.0362	0.725	0.419	0.265	0.0615	0.0990	0.360
Observations		1,589,178	1,589,178	1,192,343	1,193,243	1,589,178	1,589,174	1,589,177	1,502,960	1,566,755

Note: This table reports the coefficient of the age-above-40 indicator controlling for a quadratic polynomial, which allows for different coefficients on each side of the cutoff. Unemployment spells are censored at 2 years, except in column 2. The unit of time for tenure is days. The mean of the dependent variable for three years around the cutoff is reported. "Wage growth in new job" is defined as the change in the log of the average monthly wage in post-employment jobs. The covariates used are individual characteristics, such as gender, marital status, a dummy for Austrian citizenship, education, tenure, experience during the last 2 and 5 years, month of layoff, calendar week of layoff; and previous firm's characteristics such as industry, frequency of layoff, and proportion of recalls.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.