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Does Extending Unemployment Benefits Improve Job Quality?†

By Arash Nekoei and Andrea Weber*

Contrary to standard search models predictions, past studies have not found a positive effect of unemployment insurance (UI) on reemployment wages. We estimate a positive UI wage effect exploiting an age-based regression discontinuity design in Austria. A search model incorporating duration dependence predicts two countervailing forces: UI induces workers to seek higher-wage jobs, but reduces wages by lengthening unemployment. Matching-function heterogeneity plausibly generates a negative relationship between the UI unemployment-duration and wage effects, which holds empirically in our sample and across studies, reconciling disparate wage-effect estimates. Empirically, UI raises wages by improving reemployment firm quality and attenuating wage drops. (*JEL* J31, J64, J65)

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 benefit duration affects laid-off agents' search decisions, using more than one 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

* Nekoei: IIES-Stockholm University, S-106 91 Stockholm, Sweden (e-mail: arash.nekoei@iies.su.se); Weber: Department of Economics, Vienna University of Economics and Business, Welthandelsplatz 1, 1020, Vienna, Austria (e-mail: andrea.weber@wu.ac.at). This paper is a revised version of the first chapter of Nekoei's PhD thesis at Harvard University. Nekoei is grateful to Raj Chetty, Edward Glaeser, Nathaniel Hendren, and Lawrence Katz for their guidance throughout this project. For helpful suggestions, we thank Timo Boppart, Adam Breuer, David Card, Gary Chamberlain, Jeffrey Clemens, Tom Cunningham, Itzik Fadlon, Stefano Giglio, Joshua Gottlieb, Xavier Jaravel, Per Krusell, Joana Naritomi, Mahan Nekoui, Zhuan Pei, Benjamin Schoefer, David Seim, Julia Schmieder, Robert Shimer, Ali Shourideh, Josef Sigurdsson, Gary Solon, David Strömberg, Alireza Tahbaz-Salehi, Ugo Troiano, Stephen Woodbury, Clara Zverina, three anonymous referees, and numerous participants of 2014 NBER Summer Institute, joint labor and public economics session. This project received funding from the Austrian Science Fund (NRN Labor Economics and the Welfare State) and the Lab for Economic Applications and Policy at Harvard University. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.
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for eligibility for a nine-week extension of the potential UI benefit duration in addition to the base of 30 weeks.

Consistent with prior research, we estimate that a nine-week increase in potential benefit duration causes workers to stay jobless two days (two percent) 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.[1]

Our finding of a positive effect of UI generosity on reemployment wages stands out compared to the previous 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, Chetty, and Weber 2007a, Lalive 2007, and Van Ours and Vodopivec 2008). Moreover, Schmieder, von Wachter, and Bender (2016) find a statistically significant *negative* UI wage effect. To reconcile our result with the literature, 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 duration dependence. Contrary to intuition, in this setting, the UI effect on subsequent wages is not necessarily positive, as it is determined by two countervailing 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.[2] The latter negative force is triggered by the positive UI nonemployment effect, the result of UI causing recipients to both search less and become more selective (the search and selectivity margins). Theory thus 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.[3]

[1] Gruber's (2004, p. 401) 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).

[2] The intuition of two countervailing forces has been present in discussions of the effect of UI on job quality (for instance, see Addison and Blackburn 2000, Degen and Lalive 2015, and Schmieder, von Wachter, and Bender 2016). For example, Addison and Blackburn (2000, p. 23) 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 post-unemployment wages with receipt of unemployment insurance. But the general presumption that UI will elevate reservation wages and lead to relatively higher post-unemployment wages as a result of better job matches would appear to be robust and to provide a means of discriminating between the labor-leisure and search models of UI.*

Our theory formalizes these countervailing forces, demonstrates their endogeneity, states the fundamental parameters that determine their balance, and shows that the UI wage effect *can* be negative.

[3] How can a more generous UI lead to lower subsequent job quality? We show that this is theoretically possible since agents are maximizing expected consumption rather than expected wage. UI creates a wedge between the two and can reduce wages but always increases consumption.

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 UI nonemployment and wage elasticities. We show that a negative correlation holds in a meta-analysis across existing estimates: studies that estimate a longer UI nonemployment 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 measures the correlation between two estimated elasticities across subsamples defined by observable characteristics. This evidence suggests that heterogeneity in the search and selectiveness margins dominates the empirical relationship between the two UI effects.

Two additional pieces of evidence are provided that connect to our theoretical model. First, the theoretical model predicts that the UI benefit extension effect should be larger for agents with a higher likelihood of UI benefit exhaustion. To support this prediction, we show that in response to the benefit extension, (i) the largest increase in reemployment wages appears in jobs found closer to the time of benefit exhaustion and (ii) for agents with a relatively higher ex ante likelihood of benefit exhaustion. These findings indicate the forward-looking behavior of the unemployed as their responses are proportional to the likelihood of requiring the benefit extension in the future.

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

To investigate the mechanisms driving the positive UI wage effect, we explore the types of jobs found by unemployed workers. We show that UI generosity does not have an effect on the likelihood of changing regions or industries with respect to pre-unemployment jobs. But interestingly, we also find that agents who are eligible for the benefit extension find jobs in firms that pay higher wages to their (other) employees. The magnitude of this effect suggests that a considerable part of the positive 0.5 percent UI wage effect at the individual level is due to more favorable employer-employee matches. This finding rules out the economic significance of the impact of UI generosity on individual workers' bargaining power.

A nonzero UI wage effect has an important policy implication. Connecting this result to a normative model of UI points to an overlooked fiscal externality since UI affects future tax revenue through higher wages. The conventional view of 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 nonemployment 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 nonemployment duration externality, and this re-employment-wage externality, the sign of which depends on the sign of the UI wage effect. 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.[4] 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 versus 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 I discusses the empirical setting. Section II presents the main estimation results of the UI wage effect. Section III sets up a theoretical model and shows how it can reconcile our result with the previous literature. Section IV studies the mechanisms driving the UI wage effect. Section V presents our empirical findings of the effect of UI on other (nonwage) measures of job quality. Section VI investigates the policy implications of our findings and Section VII concludes. Theoretical derivations, the proofs of propositions, validity tests, and further robustness checks are collected in the online Appendix.

I. Empirical Setting

A. *Institutional Background and Data*

The Austrian unemployment insurance system is less generous than those of most European countries. The benefit replacement rate is 55 percent of net earnings, subject to a maximum and a minimum benefit level.[5] UI benefits are paid for a limited time and the potential benefit duration is a function of previous work experience and age. A baseline eligibility for 20 weeks of benefits is established if an individual has been employed for at least one year during the two years prior to the start of the claim. Repeat claimants are eligible for a new claim of 20 weeks if they have worked for 28 weeks during the last year. UI benefit eligibility is extended to 30 weeks for workers who have been employed for 3 years during the 5 years leading up to the claim. Furthermore, since August 1, 1989, workers aged above 40 at the start of the claim are eligible for a benefit extension to 39 weeks, if they have worked for 6 years during the last 10 years. The UI system is financed by a 6-percent payroll tax with no experience rating. After exhausting the UI benefits, workers can apply for a mean-tested unemployment assistance.[6]

Our empirical analysis exploits the discontinuous jump in benefit eligibility from 30 to 39 weeks at the age of 40. We consider a sample of individuals who pass the experience threshold for either 30 or 39 weeks of benefits, such that age is the only determinant of extended benefit eligibility. This gives us a large sample with a high density around the age cutoff, which is important for the precision of the RD estimates. As outlined above, the Austrian UI system features a series of discontinuities, which can be exploited for identifying the effects of benefit generosity. Most of

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

[5] In addition to the basis UI level, workers with dependent family members receive a family allowance for workers for each family member. However, the total UI replacement rate cannot exceed 60 percent, or 80 percent for a claimant with dependents. The family allowance and the maximum and minimum benefit levels are adjusted annually.

[6] The replacement rate of unemployment assistance is 92 percent of UI, but the effective replacement rate is lower due to means testing based on household income (Card, Chetty, and Weber 2007a and Card et al. 2015).

Table 1—Descriptive Statistics

	Population (1)	Sample 1 (2)	Sample 2 (3)	Final sample (4)
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
	(1,687)	(1,373)	(1,371)	(1,384)
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
	(2,417)	(1,534)	(1,702)	(1,875)
Nonemployment 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
	(1,443)	(962)	(1,016)	(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

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

these discontinuities have already been studied in the literature and we include their results in the meta-analysis in Section IIIC.[7]

Two administrative data registers 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. The social security records cover about 85 percent of the workforce, the most important excluded groups are civil servants and self-employed. We match the ASSD with individual records from the Austrian Unemployment Registers. The second database records unemployment spells and information on benefit receipt.

To construct our analysis sample, we start by drawing from the ASSD the universe of 18,612,408 job separations that occurred over the years 1980–2010 and among individuals who have established eligibility for a new UI benefit claim via a minimum pre-separation job tenure of 28 weeks. Table 1, column 1 presents the main descriptive

[7] See online Appendix Table A10 for an overview of studies evaluating discontinuities in the Austrian UI system using either RD or difference-in-differences designs.

statistics of the subpopulation of prime-age workers, in addition to three nested subpopulations.[8] In column 2, we narrow the sample to individuals who take up UI benefits within four weeks after job separation, thus eliminating voluntary quitters who are subject to a four-week waiting period (Card, Chetty, and Weber 2007a). Among those workers, the average tenure at layoff is about 2.5 years, and it takes on average 17 weeks before they start a new job. To focus on the eligibility threshold for 30 or 39 weeks of UI benefits, the next subsample in column 3 imposes two further restrictions. We only include individuals who qualify for the work experience criteria, i.e., workers who have been employed 60 percent in the last 5 and 10 years. In addition, we narrow the age range to 30–50 years. The final analysis sample, in column 4, includes all workers who have been laid off after the introduction of the law of August 1, 1989. This sample includes 1,738,787 job separations. Relative to the average laid-off agent in column 2, individuals around the age 40 discontinuity have a longer job tenure at layoff (8 percent) and experience larger wage drops in their post-unemployment jobs.

The main outcome variables are nonemployment durations and wage changes between the pre- and post-unemployment jobs. Based on the register information, we define nonemployment duration as the number of days between two consecutive employment spells (Solon 1979, Card, Chetty, and Weber 2007b, and Rothstein 2011). For each individual, daily wages per employer and calendar year can be constructed by dividing the annual earnings by the number of days worked for the same employer. To measure the wage change between jobs, we compute the log difference between the daily wage in the year of separation and in the year the new job started with the respective employers. Even though daily wages are measured at a high level of precision, the data give rise to two caveats with respect to the interpretation of wage effects. First, the daily wage measure does not include information on hours worked. It is thus unclear whether the wage change between jobs can be attributed to a change in the wage rate, a change in hours worked, or both.[9] To assess the importance of this problem, we complement the data with information on full versus part-time employment that is available from pay slips through which employers report information on tax withholdings to the tax authority. Starting in 2002, these pay slips contain an indicator equal to one if the employee held a full-time job for the majority of the reporting period (i.e., calendar year). See Section V for further discussion. Second, for workers recalled to their pre-unemployment employer within the same calendar year, the wage change is equal to zero by definition. Therefore, we exclude this group from the analysis of wage changes. Our results are robust to recovering the wage of this group from the previous or following calendar year.

The observation period ends with December 2011 in the ASSD, which implies that new jobs started after this date cannot be observed. In the empirical analysis of nonemployment durations and wages we exclude observations of individuals who do not find a new job within two years or by the end of the observation period. Online Appendix A.3 provides a detailed discussion to assess the sensitivity of our results with respect to this choice.[10]

[8] See online Appendix Table A1 for a more detailed list of variables.

[9] This shortcoming is common to studies estimating UI effects in administrative data, e.g., Card, Chetty, and Weber (2007a) and Schmieder, von Wachter, and Bender (2016).

[10] An often-used condition for unbiasedness is that the UI benefit extension does not affect the probability of remaining unemployment for more than two years. However, we show that this condition, although necessary to

B. *Research Design*

To evaluate the effects of the UI benefit extension, we adopt a sharp regression discontinuity (RD) design around the age cutoff of 40 years. In particular, we estimate the following model:

$$(1) \quad y_i = \gamma \times 1(age_i \geq 40) + f^a(age_i) + f^b(age_i) \times 1(age_i \geq 40) + \eta_i,$$

where age is measured at the time of layoff and the two unknown functions f^a and f^b are assumed to be smooth in age.[11] Under the identification assumption that η_i does not change discontinuously at age 40, γ provides an unbiased estimate of the causal effect of the UI extension 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 the precision.

To empirically implement the model in equation (1), we adopt a local polynomial approximation approach (Lee and Lemieux 2010). Our main results are estimated with an age bandwidth of 10 years to the left and the right of the threshold and a quadratic polynomial degree. The graphical analysis further uses a four-month bin width to show the data nonparametrically. To check the robustness of our results with respect to the specification choices, we add a full set of sensitivity analyses in the online Appendix, presenting estimates with varying polynomial degrees, bandwidths, implementing optimal bandwidth choices and bias corrections following Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014).

Tests for Validity of the RD Design.—An advantage of the RD design is that the identification assumptions offer testable predictions. To validate our design we present three types of tests. First, we investigate the distribution of layoffs around the age cutoff. If individuals or employers respond strategically to the age-based eligibility rule, we might expect to see bunching in layoffs above the threshold. Second, we test for discontinuity in predetermined covariates around the age threshold. The Austrian setup offers a third validity check by examining the period before the age-based UI extension was introduced. If there are discontinuities in the outcome variables at the age threshold for individuals losing their job prior to the August 1989 reform, this indicates that other determinants of the outcomes change discontinuously at age 40.

Figure 1, panel A plots the histogram of the age distribution at layoff. As we can see, the distribution evolves smoothly through the threshold.[12] This result is in line with previous evidence on the absence of strategic layoff behavior in response

avoid selection, is not a *sufficient* condition for unbiasedness, since it does not rule out a potential UI effect on censored observations. To confirm the robustness of our results, online Appendix A.3 provides a bias-corrected measure of both UI effects and includes sensitivity tests.

[11] In our sample, the date of the UI claim start, which determines eligibility for extended benefits, is the day following the layoff date for 70 percent. The maximum difference between both dates is 28 days. We can also show that the probability of delaying claim start by more than one day after layoff does not change discontinuously at the age threshold.

[12] There is a strong seasonal pattern in the frequency of layoffs. This pattern results from a combination of seasonality in births and seasonality in the probability of layoff over the calendar year. Online Appendix Figure A7 shows that when we weight layoff frequencies by the probability of being born in a certain month, the graph with monthly age bins is also smooth.

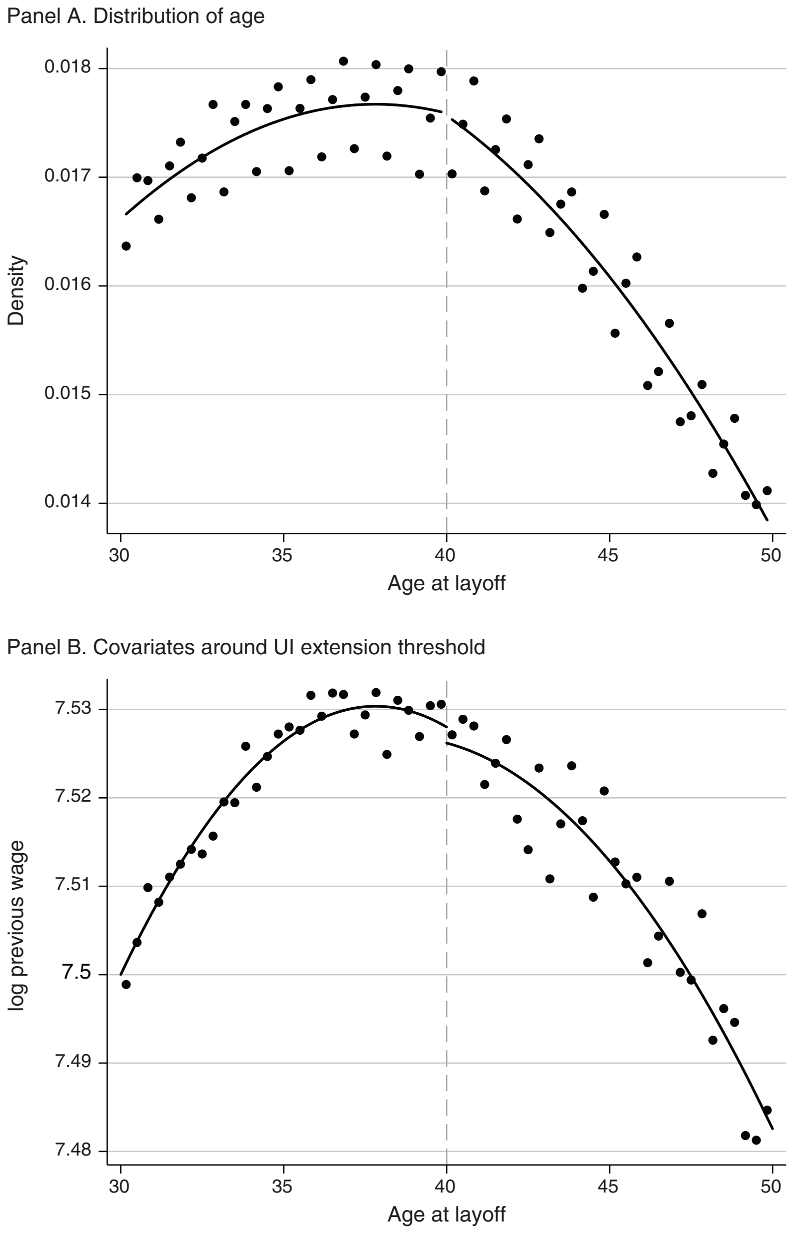

Figure 1. RDD Validity Tests

Notes: This figure presents two examples of validity tests of the RD design. Panel A plots the frequency of job separations by age. Panel B plots the average previous wage by age. The dashed line denotes the cutoff for extended UI benefits eligibility from 30 to 39 weeks. The solid line represents quadratic fits on each side of age cutoff. Age bins correspond to 4-month intervals.

to UI policy rules in Austria (Card, Chetty, and Weber 2007a). There are several potential reasons for the absence of manipulation in the timing of the layoff. From the worker's perspective, the gain from manipulating the layoff date is related to the probability of remaining unemployed for 30 weeks, as discussed in Section III. In our sample, this probability is on average around 20 percent. Furthermore, the perceived likelihood of long unemployment spells by employees is perhaps even smaller (Spinnewijn 2015). However, layoffs are decided by the employer, who is obliged to inform the Works Council in larger firms, and the employee practically has little influence on the timing. From the employer's perspective, manipulation of the timing of layoff has a financial cost equal to the extra payroll taxes.

To assess that predetermined observable characteristics evolve smoothly through the age 40 threshold, Figure 1, panel B plots mean log monthly wages in the pre-unemployment jobs by age. We provide further tests based on a larger set of covariates in the online Appendix. In particular, online Appendix Table A3 reports regression results checking for discontinuities in other observables. Presenting more concise statistics, Figures A1 and A2 in the online Appendix plot composite covariates indices, derived from predicting the main outcome variables—nonemployment duration and wage change—by age. The associated estimates of the RD specifications are reported in online Appendix Table A4. The results for single covariates in online Appendix Table A3 and composite covariates indices (predicted outcomes) in online Appendix Table A4 are mostly insignificant and sensitive to the choice of bandwidth and polynomial degree. Moreover, all statistically significant coefficient estimates are small in economic magnitude. In sum, these tests provide further evidence for the absence of manipulation in the timing of layoff around the age cutoff.

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 2 exploits the pre-reform period to verify that there was no discontinuity in nonemployment 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 2, and (ii) workers laid off after the 1989 reform, but who 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. In general none of our tests detect a sign of strategic timing of layoff.

II. Positive UI Effect on Reemployment Wage

This section investigates the effects of the UI benefit extension from 30 to 39 weeks on nonemployment durations and reemployment wages. We present graphical evidence on changes in the outcome variables at the discontinuity in Figure 3 and Table 2 reports estimation results from the model introduced in Section IB. All estimates are presented for two model specifications with and without including observed covariates.

Figure 3, panel A plots nonemployment durations in days for the sample of individuals who find a new job within two years after layoff. While nonemployment durations are generally increasing in age, the figure also shows a clear upward jump in average nonemployment durations at the age cutoff. This discontinuity corresponds to about 2 more days of average nonemployment in response to the extension of UI benefits. Column 1 in Table 2 presents the corresponding coefficient estimates.

An alternative way of measuring the UI effect on nonemployment duration is via the hazard rate to employment. Figure 3, panel B illustrates the effect of the benefit extension on the probability of finding a job within 39 weeks after layoff. As confirmed by the regression estimates in Table 2, the benefit extension decreases the probability

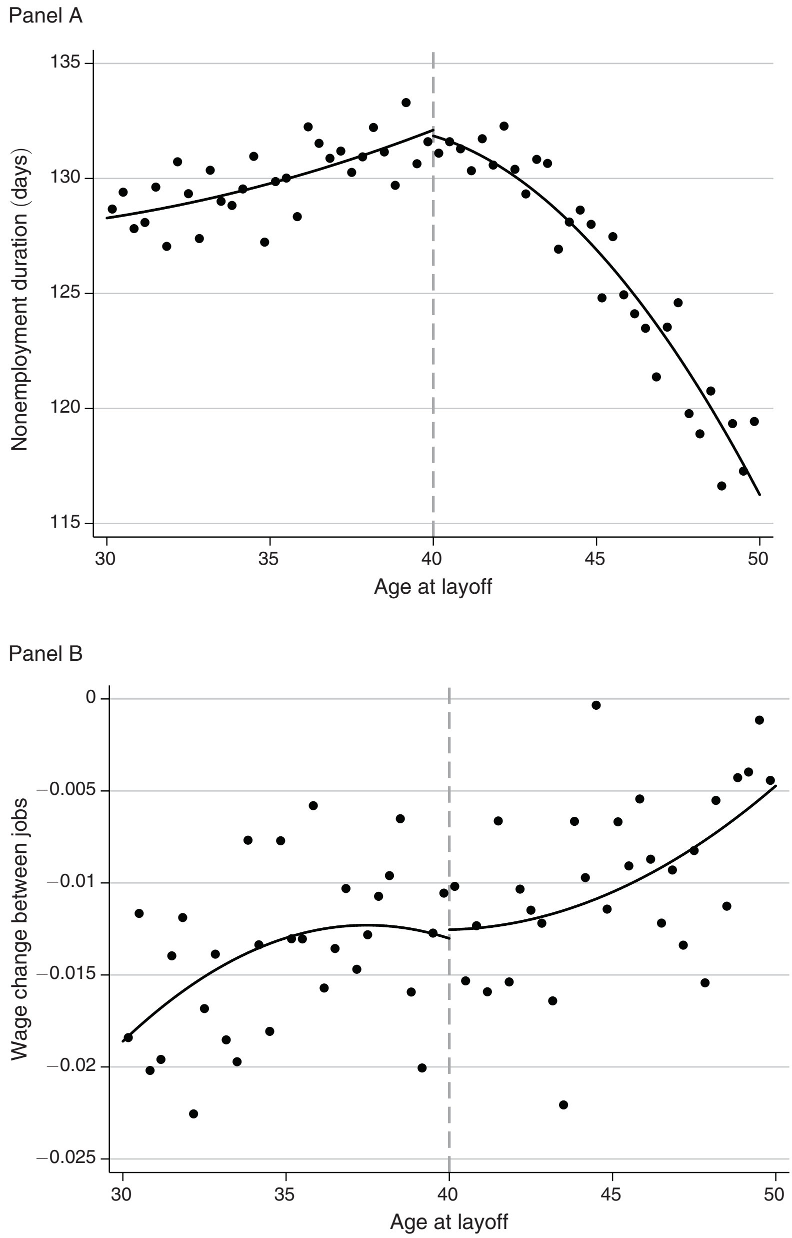

Figure 2. Placebo Tests of Pre-Reform Discontinuity

Notes: Panel A plots the average nonemployment duration by age at layoff for a sample of layoffs between 1980:1 and 1989:7 (pre-reform period). Panel B plots the average change in log wage between pre- and post-unemployment jobs by age at layoff for the same sample. Observations with nonemployment durations of more than two years are excluded. The dashed line denotes the cutoff for extended UI benefits eligibility. The solid line represents quadratic fits on each side of age cutoff. Age bins correspond to 4-month intervals.

of finding a job within 30 or 39 weeks by 1 percentage point and 1.3 percentage points, respectively (columns 2 and 3 of Table 2). The finding that the job-finding rate already decreases in the first 30 weeks, when the UI generosity is unchanged, suggests that workers are forward looking (Card, Chetty, and Weber 2007a).

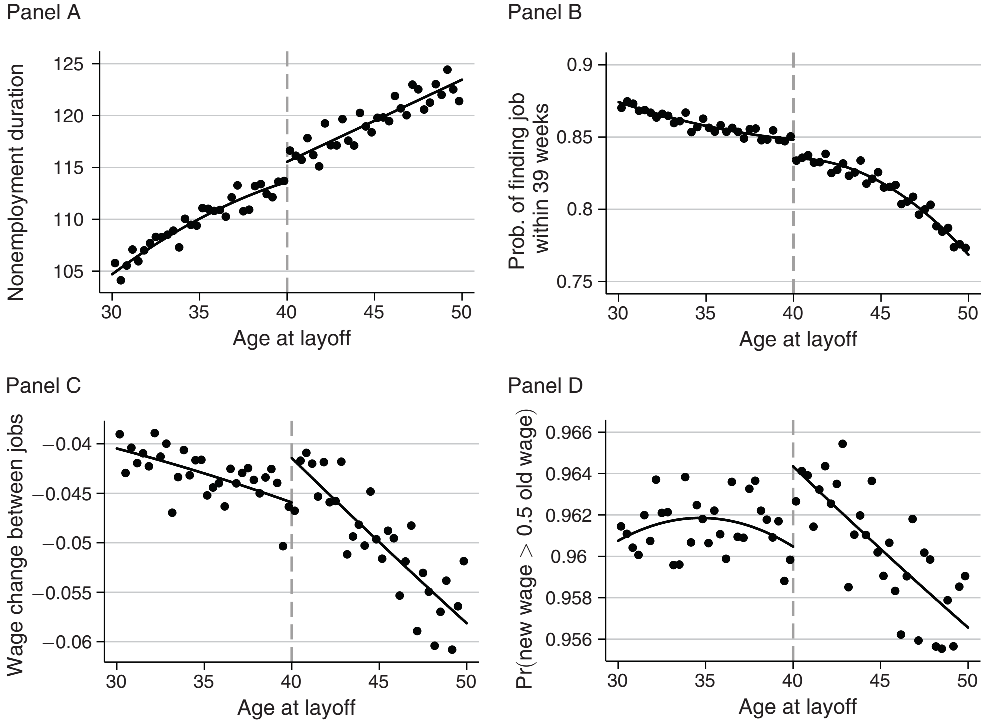

FIGURE 3. UI EFFECTS ON NONEMPLOYMENT DURATION AND WAGE

Notes: Panel A plots average nonemployment durations (time to next job) for each age. Panel B plots the probability of finding a job within 39 weeks of layoff for each age. Panel C plots the average change in log wage between post- and pre- unemployment jobs for each age. Panel D plots the probability that the new wage is higher than 50 percent of the previous wage (proxy for UI benefit level). In panels A, B, and D, observations with nonemployment durations of more than two years are excluded. The dashed line denotes the cutoff for extended UI benefits eligibility. The solid line represents quadratic fits on each side of age cutoff. Age bins correspond to 4-month intervals.

How does the UI extension affect the quality of post-unemployment jobs? Figure 3, panel C plots wage changes between the pre- and post-unemployment jobs by age. We can see a positive discontinuity at the cutoff, which corresponds to an estimated 0.45 percentage point wage increase (column 4 of Table 2). Since pre-unemployment wages evolve smoothly at the threshold, as discussed in Section I, the effect of the benefit extension on wages can also be detected as a discontinuity in reemployment wages. Consistent with this argument, 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 estimates in columns 4 and 5 become very similar.

Figure 3, panel D studies the wage effect of the UI benefit extension by focusing on the nominal wage effect, comparing the reemployment wage with half of the pre-unemployment wage. Given that the net benefit replacement rate is 55 percent, this comparison can be seen as proxy for the probability of accepting a wage above the UI benefit level. The graphical analysis shows a drop in the probability of accepting a job paying more than half of the previous wage at the threshold. The corresponding point estimate in column 6 of Table 2 indicates an increase of 0.39 percentage points. This result is in line with predictions from job ladder models

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

	Covariates	Nonemployment duration (1)	Find job within 30 weeks (2)	Find job within 39 weeks (3)	Wage change between jobs (4)	log re-employment wage (5)	New wage > UI benefit (6)
Discontinuity at age 40	No	1.932	−0.00988	−0.0131	0.00449	0.00350	0.00388
		(0.526)	(0.00178)	(0.00164)	(0.00170)	(0.00234)	(0.00105)
	Yes	1.898	−0.00842	−0.0119	0.00459	0.00506	0.00386
		(0.466)	(0.00153)	(0.00146)	(0.00146)	(0.00154)	(0.00102)
Mean of dependent variable 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

Notes: 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. We exclude observations with nonemployment durations of more than two years, except when studying hazard rates in columns 2 and 3. The unit of time for nonemployment 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 versus 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 I). The covariates used are individual characteristics, such as gender, marital status, a dummy for Austrian citizenship, education, tenure, experience during the last two and five years, month of layoff, calendar week of layoff; and previous firm's characteristics such as industry, frequency of layoff, and proportion of recalls.

à la Burdett where unemployed workers will accept the first job offer above the UI benefit level and then search for a better paying job (Burdett 1978). We will investigate this further in Section IV.

A comparison of the two specifications in rows 1 and 2 in Table 2 shows that all estimation results are highly robust to including a range of predetermined covariates in the RD regression model. This further confirms the validity of the design. Online Appendix Table A2 presents additional specification checks for the models estimated in Table 2. Our results are robust to variations in the polynomial degrees of the supporting functions or the choice of bandwidth. We further present results based on the optimal bandwidth selection following the algorithms suggested by Calonico, Cattaneo, and Titiunik (2014) and Imbens and Kalyanaraman (2012), along with bias corrected estimates and robust confidence intervals. As we can see from Table A2, the coefficient estimates vary very little across specifications. Another concern is the sensitivity of our results to seasonal variation. We have discussed seasonality in birth and layoff dates over the calendar year (Section IA). In addition, Figure 3 shows a seasonal pattern in nonemployment durations by age. To assess the role of seasonality in our estimates, we present estimates corrected for seasonality in online Appendix Figures A11 and A12, and Table A6. These checks further confirm the robustness of our main results.

In sum, our results document that nine 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 the need for insurance.

Is the Order of Magnitude of the Wage Effect Reasonable?—Before presenting our formal model of job search, we give a brief assessment of the magnitude of the estimated UI wage effect, which is based on a simple benchmark comparison of marginal costs and benefits of prolonged job search.[13] We consider an unemployed worker who maximizes expected income and lives in a stationary environment.

This agent weighs the benefit and cost of an additional day of search: she will 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 denotes the post-unemployment job duration.[14] Equation (2) implies that the wage gain from marginal search should equal $\frac{\Delta w}{w} = \frac{1-\rho}{L}$, where ρ stands for the net benefit replacement rate. Plugging in the Austrian net replacement rate of 55 percent and an average post-unemployment job tenure of 567 days in the sample results in an average marginal wage gain of 0.07 percent per day of search. This implies a corresponding wage gain of 0.14 percent for 2 additional days of unemployment, which can be compared to our estimate of a 0.5 percent UI wage effect from Table 2. The simplified benchmark calculations suggest that the estimated wage effect is of the same order of magnitude as the expected wage gain by the optimizing agent. The expected wage gain also provides a benchmark for the degree of precision that is needed to detect a significant UI wage effect. We will return to this issue in Section IIIA.

III. 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 (Rogerson, Shimer, and Wright 2005). This prediction is in line with the estimated positive UI wage effect in Section II, but it is at odds with the prior empirical literature, which mainly finds a zero and, at times, negative wage effect. In this section, we address this apparent puzzle. In Section IIIA, we develop a partial-equilibrium directed-search model with duration dependence where the UI wage effect is the result of two countervailing 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 IIIB and Section IIIC), to identify agents who are most affected by a UI benefit extension (Section IIID) and to investigate the UI effect on the distribution of reemployment wages (Section IIIE).

[13] The magnitude of the estimated UI wage effect may intuitively seem small. In fact, the recent *Handbook of Public Economics* chapter on the issue assesses that current "estimates are sufficiently precise to rule out even a 1 percent increase in the wage rate at the upper bound of the 95 percent confidence interval" (Chetty and Finkelstein 2013, p. 161–162).

[14] Section V and Table 4, panel B show that the wage effect persists over the first post-unemployment job and there is no UI effect on job tenure. Therefore, L equal to the new job tenure implies that the UI wage effect, $\frac{1-\rho}{L}$, is an upper bound. We also ignore the term $(1 - \tau) w \Delta L$ and assume $\Delta L = 0$. We also ignore discounting, given the short horizon of the problem, i.e., $\frac{1-\exp(-rL)}{r} \simeq L$ when $r = 5$ percent.

A. *Search Model with Duration Dependence*

We consider a directed search model where unemployed workers choose the type of job they are looking for among a set of posted vacancies. Unemployed workers know that the success of the application or the job finding rate, λ, is jointly determined by search effort and target job quality (selectivity). Formally, this mechanism is captured by a matching function E, decreasing in the value function of being employed at the target job, denoted by V, and increasing in search effort, s. For now, we assume that workers live hand-to-mouth. The value of being unemployed at time t after layoff, $U(t)$, is given by

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

where $u(\cdot)$ is the flow utility of consuming benefit b, ψ the disutility of search, t the time since layoff, and β the discount factor. In general, we assume that jobs differ along several dimensions. In the case where job quality only differs in the wage, selectivity is equivalent to choosing a target wage. We denote by $V(t)$ and $s(t)$ the optimal target job quality and search effort at time t after layoff, the solution of optimization (3), and by $\lambda(t)$ the resulting job-finding rate, $\lambda(t) = E(V(t), s(t), t)$.

Similar to McCall's (1970) random search model, this is a partial equilibrium framework.[15] However, our setting is the opposite of random search models with respect to the information available about vacancies: here agents apply to a specific job of known quality, while in the McCall framework they draw random job offers from a known quality distribution. The directed search framework matches several empirical observations. Job advertisements typically indicate wage ranges or other information about job quality.[16] Unemployed workers apply for posted vacancies, but they probably do not base their decisions on a fixed minimum acceptable wage. The target wage is thus a realistic concept, while the reservation wage reflects a "theoretical construct . . . not observed in the data" (Cox and Oaxaca 1990, p. 20). This leads us to believe that the target wage should be easier to measure than the reservation wage.[17] Furthermore,

[15] Similar partial-equilibrium directed-search models are rarely used, with the only exception of Baily (1978). The main application of the directed search models has been the frictional unemployment created by the lack of coordination among the unemployed in such models (Montgomery 1991, Moen 1997, and Shimer 1996).

[16] In Austria, antidiscrimination legislation regulates the information provided in vacancy postings in the media or at the public employment office. Among others, the minimum hourly wage according to collective bargaining agreements or similar rules has to be clearly stated along with an indication of the employer's willingness to negotiate overpay.

[17] In fact, the accepted wage observed in survey or administrative data corresponds to the job seeker's target wage immediately before leaving unemployment. However, the reservation wage is never captured in administrative data. In order to measure target wages over the unemployment spell, a survey could ask the unemployed about their most recent job applications. 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 a 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).

the directed search model matches the empirical observation that job seekers often accept the first offer they receive.[18]

Two sources of duration dependence are incorporated in the agent's maximization problem (3). First, the likelihood of finding a certain job given that the search effort can vary over the unemployment spell. This is a reduced form representation of decreasing job opportunities, which we refer to as *structural duration dependence*.[19] Second, duration dependence can also be caused by the UI system if the benefit level changes over time, $b(t)$. In practice, this is the case in most countries where the potential benefit duration is limited.

Both types of duration dependence imply nonstationary search behavior of the unemployed, e.g., a decreasing target job quality over the unemployment spell, $V(t)$. Empirically, the path of target job quality over the unemployment spell is not observed due to dynamic selection (Section IIID). The main outcome of interest is therefore the average quality of re-employment jobs or, more precisely, the expected value of the target job quality, $E_{\tilde{T}}(V(\tilde{T}))$, where $\tilde{T}$ is the random variable of nonemployment duration. In the same vein, we define the expected re-employment job quality at unemployment duration t by: $V^e(t) = E_{\tilde{T}}(V(\tilde{T}) \mid \tilde{T} \geq t)$.

How Does a Change in UI Generosity Affect Expected Job Quality?—To answer this question, it is helpful to write the expected re-employment job quality at unemployment duration t in recursive form as

$$V^e(t) = \lambda(t)\, V(t) + \big(1 - \lambda(t)\big)\, V^e(t+1). \tag{4}$$

If we assume that jobs only differ in one dimension, i.e., the wage, the same equation (4) holds for the expected re-employment wage. As job quality is synonymous with wage in this case, we use both interchangeably from here on.

Start by considering a case without duration dependence, i.e., a time-independent unemployment benefit and matching function, $b_t = 0$ and $E_t = 0$ (the lowercase letters refer to partial derivatives). Here, the unemployed worker chooses a fixed target job quality V^* and a fixed level of search effort irrespective of the elapsed unemployment duration, implying $V^e(t) = V(t) = V^*$ for all t. In response to an increase in the benefit level of the initial period after layoff, agents become more selective and increase their target wages in this period. Moreover, they decrease their search effort during the first period. Higher selectivity and lower search lead to a change in the timing of finding a job, but longer unemployment has no direct effect on the expected wage due to the absence of duration dependence. However, the higher selectivity in the initial period changes the expected wage proportionally to the likelihood of finding a job in that period, $V^e_{b(0)}(0) = \lambda(0)\, V_{b(0)}(0)$. This illustrates that the probability of finding a job in the initial period $\lambda(0)$ and the change

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

[19] The theoretical literature has considered to many examples of such duration dependence, such as 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 1998). For related empirical evidence, see Kroft, Lange, and Notowidigdo (2013), Autor et al. (2015), and Kudlyak, Lkhagvasuren, and Sysuyev (2013).

in selectivity $V_{b(0)}(0)$ jointly determine the change in expected wages. Importantly, the UI wage effect is positive, as it is purely driven by selectivity and it is independent of the choice of search effort. Next, we will show how this result changes once we allow for duration dependence.

In the setting with negative duration dependence, either due to decreasing unemployment benefits or job-finding opportunities, with $b_t \leq 0$ or $E_t \leq 0$, the selectivity decreases over the unemployment spell. Thus, the target job quality at any unemployment duration t is higher than the expected job quality and $V(t) > V^e(t+1)$. A benefit extension still increases the expected wage through selectiveness, similarly to the case without duration dependence. In addition, duration dependence leads to a decline in expected job quality since longer unemployment durations lead to lower target wages. The overall effect of the initial-period benefit increase on expected job quality can thus be expressed as the sum of two countervailing forces,

$$\underbrace{V^e_{b(0)}(0)}_{\text{UI Wage Effect}} = \overbrace{\lambda(0)\underbrace{V_{b(0)}(0)}_{\substack{\text{Selectivity}\\(+)}}}^{\text{Positive Force}} + \overbrace{\underbrace{\lambda_{b(0)}(0)}_{\substack{\text{Duration Effect}\\(-)}}\underbrace{\big(V(0) - V^e(1)\big)}_{\substack{\text{Duration Dependence}\\(+)}}}^{\text{Negative Force}}, \tag{5}$$

where lowercase letters refer to partial derivatives. The positive force results from increased selectivity, while the negative force is proportional to the effect of the change in benefit generosity on the job finding rate and a measure of duration dependence given by the difference between target job quality in the initial period and the expected job quality after this period.[20]

Proposition 1 extends the result of a benefit change in the initial period to a general case which considers the effects of a change in benefit generosity in a future period t on the expected job quality in any of the preceding periods $k \leq t$. Importantly, the generalized result covers the effect of a UI benefit extension on mean reemployment wages, which we are estimating in our empirical application.[21]

PROPOSITION 1: *The instantaneous effect of a change in the UI benefit level in period t after layoff on expected job quality in the same period is given by*

$$V^e_{b(t)}(t) = \lambda(t)\, V_{b(t)}(t) + \lambda_{b(t)}(t)\big(V(t) - V^e(t+1)\big). \tag{6}$$

If UI generosity is changed at a future period t, the response in expected job quality in period $k \leq t$ to the change in utility associated with the benefit change can be expressed as a weighted sum of instantaneous benefit responses in the following way:

$$\frac{\partial V^e(k)}{\partial u\big(b(t)\big)} = \sum\nolimits_{k \leq i \leq t} \beta^{t-i} \frac{S(k,t)}{1-\lambda(i)} \frac{\partial V^e(i)}{\partial u\big(b(i)\big)}, \tag{7}$$

[20] In the empirical applications, the effect of UI on the job search success is typically measured as the effect on nonemployment durations. Here, we refer to the UI effect on the hazard of exiting unemployment λ_b as the UI duration effect. These two are closely related measures of the duration effect of UI, but with opposite signs.

[21] Qualitatively, a random search model delivers similar results on the UI effect on job quality as the ones presented here, since the two countervailing forces are present in any search model incorporating duration dependence. Our target-wage model has the advantage of being more tractable and arguably more general since it nests the usual random-search model (see online Appendix A.1.3).

where the weights are determined by the discount factor β, as well as $S(k,t)$, the probability of remaining unemployed from time k to time t, i.e., $S(k,t) = \prod_{k \leq j \leq t}(1-\lambda(j))$.

A change in benefit level in period t affects the agents' search decisions—the target job quality and the job-finding rate—in each preceding period. Proposition 1 shows that we can write the response in expected job quality at k as a weighted sum of responses to instantaneous benefit changes $\frac{\partial V^e(i)}{\partial u(b(i))}$.

To understand the intuition and link the result in Proposition 1 to the empirical application, we focus on the case where $k = 0$ and consider the effect of a change in benefit generosity at time t on expected job quality at the time of layoff. In Section II we have estimated the empirical equivalent of this effect as the effect of a UI benefit extension on the expected wage, which we call the UI wage effect. The results in Proposition 1 allow us to decompose the UI effect on expected job quality in the initial period of unemployment as follows:

$$(8)\quad V^e_{b(t)}(0) = \sum_{i \leq t} \beta^{t-i} \frac{S(0,t)}{1-\lambda(i)} \frac{u_c(b(t))}{u_c(b(i))} \Big(\lambda(i)\, V^{\tau}_{b(i)}(i) + \lambda_{b(i)}(i)\left(V^{\tau}(i) - V^e(i+1)\right)\Big).$$

This equation highlights that the instantaneous response in each period i can be decomposed—analogously to equation (5)—into a component reflecting the UI benefit effect on the target wage path, $V_{b(i)}(i)$, plus a component reflecting the instantaneous effect of a UI benefit change on the job finding rate, $\lambda_{b(i)}(i)$, in proportion to the measure of contemporaneous duration dependence $(V(i) - V^e(i+1))$. The effect of the benefit change in period t on expected job quality at the start of the unemployment spell is given by the weighted sum of the instantaneous effects, where the weight for each intermediary period is the product of two factors. The first factor discounts the instantaneous effect by the likelihood of remaining unemployed until this period, $S(0, i-1)$. The second factor discounts it by the importance of the change in UI benefits at time t from the perspective of an unemployed agent at time i. More precisely, the second weighting factor takes into account time-discounting, β^{t-i}, and the survival likelihood from period i until t, $S(i+1, t)$.

The weights corresponding to the probability of survival are intuitive as an agent is less likely to benefit from a change in future benefit generosity, the further she is from the affected period. A UI benefit extension in the future thus creates an option value of search, in the sense that an unemployed individual loses the option of future benefits, as soon as she accepts a job, while continuing to search preserves the option. This concept is similar to the "option value of work" in the case of retirement decisions (Stock and Wise 1990). Equation (8) shows that the effect of a UI benefit extension on job quality is proportional to the survival likelihood until benefit exhaustion. Thus, the UI wage effect should be stronger for individuals with a higher likelihood of benefit exhaustion. A further implication is that in response to the benefit extension, agents' reactions should be stronger, the closer they get to the period when UI benefits expire. Section IIID will investigate these predictions.

B. *Undetermined Sign of UI Wage Effect*

How can we reconcile the apparent contradiction between the positive UI wage effect estimated in Section II and the results in the existing literature? Our estimate is in contrast to Schmieder, von Wachter, and Bender (2016), who find a *negative* UI wage effect in Germany, while two recent papers using quasi-experimental designs and administrative data provide relatively precise estimates of UI wage effects that are not significantly different from zero (Card, Chetty, and Weber 2007a and Lalive 2007).[22]

The theoretical model presented in Section IIIA clarifies that these findings are, in fact *not* contradictory, since we show that in the presence of negative duration dependence, a UI extension can both increase or decrease post-unemployment wages.

To illustrate the main argument in the above analysis, we focus on equation (5) which decomposes the effect of a change in benefit generosity in the initial period after layoff on job quality into a positive and negative force. The decomposition highlights that an increase in benefit generosity can lead to lower job quality in the presence of a sufficiently large negative force, driven by high duration dependence. At a first glance, this argument appears counter-intuitive since agents internalize the cost of longer unemployment that lowers job opportunities and future earnings. However, the negative force can theoretically prevail, because agents are not maximizing future wage but they are maximizing expected consumption. Since UI benefits create a wedge between expected consumption and labor earnings, UI may reduce expected wages while it increases expected consumption.

To see this, we assume a specific functional form for the job-finding rate given by $E(V,s,t) = a(t)\, s^{\,1-\frac{1}{\sigma(t)}} \exp\left(-\frac{V}{\rho(t)}\right)$. This matching function emphasizes the separate influence of search—via the choice of s—and selectiveness—via the choice of V—on the job finding rate. Moreover, we assume that agents are risk-neutral, $u(c) = c$, and that the environment is stationary after the initial period, that is $b(t) = b + 1_{t=0}\Delta b$, and $\rho(t) = \rho + 1_{t=0}\Delta\rho$. Using equation (5), we show in online Appendix A.1.1 that in this case, the UI wage effect can be written as

$$V^{e}_{b(0)}(0) = \lambda(0)\left(1 - \frac{\sigma(0)}{\rho(0)}(\Delta b + \Delta\rho)\right). \tag{9}$$

Equation (9) implies that the UI wage effect is negative whenever total duration dependence (i.e., the sum of UI-related and structural duration dependence, $\Delta b + \Delta\rho$) is strong enough.[23]

Both types of duration dependence, UI-related and structural, contribute to the negative force, diminishing the positive part of the UI effect on job quality through higher selectivity. In particular, even in the absence of structural duration dependence, a limited potential UI benefit duration creates negative duration dependence,

[22] The empirical strategy of Card, Chetty, and Weber (2007a) is a tenure-based RD design, while Lalive (2007) is the first paper exploiting an age-based RD design in this context. Both papers are using Austrian administrative data. Schmieder, von Wachter, and Bender (2016) use an age-based RDD in Germany. For a review of the pre-2000 literature, see Addison and Blackburn (2000).

[23] See online Appendix A.1.1 for a similar result under a more general set of assumptions.

which can lead to a negative UI effect on job quality. This is the case if the initial benefit schedule (before the extension) creates a large-enough nonstationarity, reflected by a large-enough decrease in the target wage. The latter is captured by Δb in equation (9).

This result rationalizes the different signs of empirical estimates of the UI wage effect in the literature. The following section provides further insights into how the parameters of the matching function drive the magnitude of the UI wage effect.

C. *Wage versus Nonemployment Duration Effects*

While the sign of the UI wage effect is theoretically undetermined, understanding the forces driving the magnitude of the UI wage effect can help connect disparate wage-effect estimates. In this section, we discuss how the parameters of the matching function drive the UI wage effect. We argue that under plausible assumptions, the theory predicts a negative relationship between the UI wage and nonemployment duration effects. We then show empirically that this relationship can explain different estimates of the UI wage effect in the literature.

Equation (5) pins down the role of the positive and negative forces behind the UI wage effect.[24] It also shows that a larger drop in the job finding rate as a result of more generous benefits, ceteris paribus, leads to a lower overall UI wage effect. This implies a negative relation between UI duration and UI wage effects. To see the intuition, note that the UI effect on the hazard of finding a job can be decomposed into search versus selectiveness margins, $E_s s_b$ and $E_V V_b$, respectively. Equation (5) shows that an increase in the UI duration effect due to a higher search margin increases the negative force of duration dependence, leading to a lower UI wage effect. This implies that heterogeneity in search margins across the population creates a negative relationship between duration and wage effects. This heterogeneity is equivalent to heterogeneity in σ using the specific matching function introduced in the previous section. This matching function emphasizes the separate influence of search and selectiveness on the job finding rate, so that the parameter σ expresses the relative importance of search versus selectiveness margins, $\frac{E_s s_b}{E_V V_b}$ (see online Appendix A.1.1).

To grasp the intuition, consider a worker who cannot become more selective in response to a UI extension, facing a fixed wage at each unemployment duration. Absence of the selectivity margin implies that a more generous UI leads to a lower wage, since UI lengthens the unemployment duration and decreases the target wage due to duration dependence. However, the UI wage effect is more negative the larger the UI nonemployment duration effect. This mechanism is responsible for a negative correlation between the UI duration and wage effects.[25]

We can further generalize this result by allowing for the general case of heterogeneity in the matching function, i.e., heterogeneity in both the selectivity and search margins. Online Appendix A.1.1 shows that the correlation between the UI duration and wage effects is negative if the heterogeneity in the UI duration effect

[24] For simplicity, we will focus on the case of a benefit change in the initial period after layoff and base our main arguments on equations (5) and (9). But the results also carry over to the general case covered in Proposition 1.

[25] We thank one of the referees for pointing out this example.

is dominated by the search margin rather than the selectivity margin or if there is a high UI-driven duration dependence.[26]

This result highlights theoretically how the parameters of the matching function drive the UI wage effect. Empirically, we cannot directly apply this result, because the parameters of the matching function are not observable. The previous literature is scant on evidence relating observable characteristics to theoretical parameters. We will instead use theory as the guidance to isolate types of heterogeneity that cause a systematic variation in the UI wage effect.

Empirical Evidence for the Negative Correlation between UI Wage and UI Duration Effects.—As the UI effects on nonemployment durations and reemployment wages constitute the primary focus of empirical research in the UI literature, the relation between the parameter estimates can be tested across studies. We conduct a meta-analysis of empirical studies, which provides estimates both for the UI effect on nonemployment durations and the UI effect on wages within the same sample using quasi experiments.[27] Figure 4, panel A plots the estimates and illustrates that, as predicted, studies reporting larger estimated UI nonemployment duration effects also find smaller UI wage effects.[28]

Heterogeneity in UI duration and UI wage effects across studies can be due to two main factors: variation across treated populations, and differences in the type of treatment. We will argue that the former most plausibly explains the differences in UI duration and wage effect estimates across studies. Then, we show that this source of heterogeneity generates a negative relationship between the UI duration and wage effects in our sample, which is of the same order of magnitude as in the meta-analysis.

First, in the presence of individual heterogeneity in the degree of responsiveness to UI, the estimates across studies differ since they represent diverse local average treatment effects. Empirical designs vary across studies due to differences in the running variables (age, experience, tenure) or the threshold values, which mechanically create a variation in the treated populations (online Appendix Table B2). Moreover, the policy settings differ across studies, as the UI extensions start at unemployment durations varying from 20 weeks to 76 weeks. The extensions starting at longer durations only affect individuals with longer expected unemployment durations, since the intensity of treatment is related to the likelihood of reaching the extension period (see Section IIIC).

[26] These results based on equations (5) and (9) are unchanged, if risk-neutral agents are living hand-to-mouth or are able to save. They also hold if risk-averse agents are living hand-to-mouth or are able to save but face complete markets, i.e., the existence of employment-status contingent loans. This is due to the fact that in all these cases, agents are maximizing discounted income.

[27] Card, Chetty, and Weber (2007a) investigate a UI extension from 20 to 30 weeks, whereas Lalive (2007) studies an extension from 39 to 52 weeks. Schmieder, von Wachter, and Bender (2016) investigate the effect of two UI extensions, both for 6 months, one starting from 1 year, another from 1 year and a half (see also footnote 22). Figure 4 refers to these studies as CCW, Lalive, and SWB, respectively, while NW denotes our own estimates in Table 2. See online Appendix B.2 for more details.

[28] Online Appendix Table A9 shows that the correlations between the UI wage effects and policy characteristics across different studies are all insignificant. The only significant correlation is between the UI wage and duration effects. The caveat is that the correlations are only based on six observations, so we have to interpret them with caution.

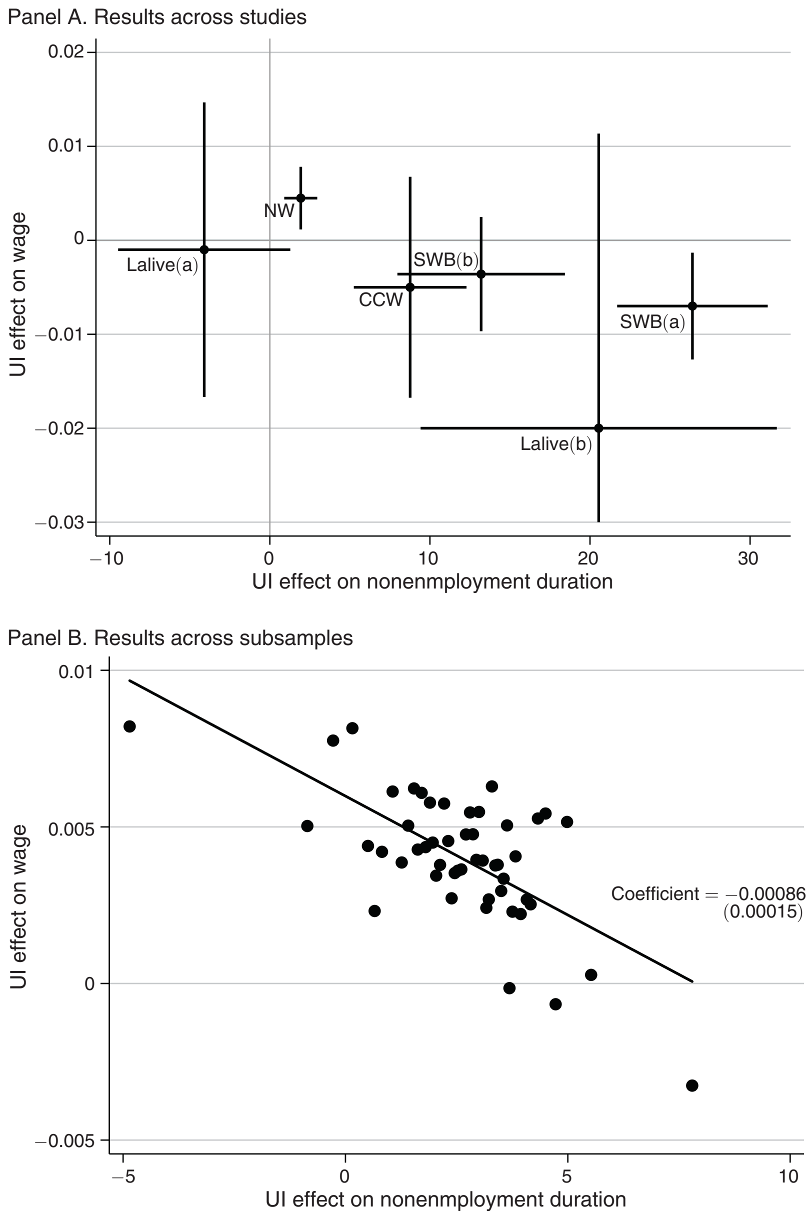

FIGURE 4. WAGE VERSUS NONEMPLOYMENT DURATION EFFECTS

Notes: This figure provides empirical evidence for a negative relation between the UI effect on nonemployment duration and its effect on post-unemployment wage. Panel A offers a meta-analysis (see footnote 27 and online Appendix B.2 for details). Panel B investigates the relationship within our data: It plots the estimated UI effect on wage against its effect on nonemployment duration for 541 subsamples of our sample. Subsamples are defined by predetermined characteristics (see online Appendix B.3); group-specific effects are estimated by applying the RDD approach, as in Table 2, separately to each subsample. Panel B shows a 50-binned scatter plot for subsamples with more than 100,000 observations, where the solid line and the coefficient correspond to the best linear fit on the underlying data using OLS.

Second, another possible explanation for the difference in UI effects across studies is the type of treatment. This would be the case if a specific individual responds differently to a UI benefit extension later in the unemployment spell than early on, which theoretically corresponds to an evolving matching function over the unemployment spell. This type of treatment effect heterogeneity creates a negative correlation between the UI wage and duration effects if, for instance, the relative role of the search margin is increasing over the unemployment spell, implying that the UI duration(wage) effect is increasing (decreasing) over the unemployment spell. This monotonic pattern is not supported by the estimates in the meta-analysis: the highest UI wage effect is estimated for an extension at 30 weeks, whereas the extensions at 20 or 52 weeks show smaller UI wage effects. Empirically it is, however, difficult to distinguish between the two types of treatment effect heterogeneity, and the literature does not offer any quasi-experimental guidance in this respect.[29]

To further provide evidence of a negative relation between UI duration and wage effects, we estimate the correlation within our population. Ideally, we would like to estimate both effects for each agent and then investigate the correlation between them. This is not possible given our RD design identification strategy. Instead, we divide the population into subsamples based on observable individual characteristics, estimate the UI wage and duration effects for each subsample, and then measure the correlation between these estimates.[30]

To define subsamples, we discretize the main observable covariates, using quartiles for non-categorical variables. The groups created by the discretized covariates and a set of pairwise interactions of covariates defining larger groups determine the set of 541 non-disjoint subsamples. For each subsample, we then estimate the RD specification in equation (1). As the theoretical results suggest a comparison of UI effects conditional on job finding hazards, we weight the regression for each subsample by the distribution of predicted nonemployment durations in the overall population. Figure 4, panel B plots the resulting estimates in a binned scatter plot similarly to panel A. The regression line and the binned scatter plot visualize a pronounced negative correlation between the estimated UI wage and duration effects.[31]

Three additional pieces of evidence further confirm the result of a negative correlation between the UI wage and duration effects. First, we estimate a heterogeneous treatment model allowing the UI effects to vary nonparametrically by observable characteristics. This provides us with estimates of both UI effects for each individual. Online Appendix Figure A13, panel A plots these two estimates and illustrates a negative correlation. Second, we replicate the re-sampling method similar to Figure 4, panel B but with full interactions among covariates. As this is only possible for a smaller set of covariates, a machine learning algorithm has been used to identify the most important predictors of nonemployment durations from the initial set of covariates. This procedure results in 378 estimates, which are plotted in online Appendix Figure A13, panel B and confirms the existence of a negative correlation. Third, we replicate our re-sampling methods for the UI benefit

[29] Business cycle fluctuations can generate heterogeneity in treated populations and in treatment characteristics. UI duration and wage effects may vary across RBC within the same individual, or due to variation in the unemployed pool (Mueller 2015). Schmieder, von Wachter, and Bender (2012) documented such evidence for UI duration effect variation across RBC. For the estimates included in the meta-analysis, this should not be quantitatively important, since most studies span long time periods covering several booms and busts.

[30] Although the method used here to estimate the correlation between UI duration and wage effects relies entirely on observables, online Appendix A.4 develops a method to bound the role of unobservable characteristics.

[31] See online Appendix B.3 for a detailed list of included covariates and the numbers of categories and interactions, as well as details on some statistics of the estimates.

extension from 20 to 30 weeks in the Austrian UI system, studied by Card, Chetty, and Weber (2007a). The results are shown in online Appendix Figure A14 once more confirming the negative correlation between UI duration and wage effect at a different discontinuity. We conclude from our analysis of Figure 4 that the individual heterogeneity seems to create enough variation to explain the different UI wage effects in the literature.[32]

D. *Option Value of Search*

We have presented two testable predictions connected with the option value of search in Section IIIA. First, the option value of search declines as unemployed workers approach benefit exhaustion, thus the UI effect on the job-finding rate and on wages should increase closer to the exhaustion point. Second, individuals who are more likely to exhaust their UI benefits should be more affected by a UI benefit extension.

To test the first prediction, Figure 5 illustrates the evolution of the UI effect on job-finding hazards and re-employment wages for different levels of unemployment duration, respectively. Figure 5, panel A shows the discontinuities in hazard rates at the age cutoff at different durations of unemployment, whereas panel B plots coefficients of a series of RD models estimated at monthly durations. The figures show that as a result of the benefit extension, the drop in the job-finding hazard at the age cutoff increases during the first 30 weeks of unemployment. This pattern suggests that workers are forward-looking (Card, Chetty, and Weber 2007a) and the value of finding a job depends on the time to benefit exhaustion. For a further discussion of the dynamic analysis of job finding rates, see online Appendix B.4. Here, our primary interest is the dynamic UI wage effect.

Figure 5, panel C graphically illustrates wage changes around the age 40 discontinuity for different unemployment durations. The figure shows a pronounced jump of about 5 percentage points in wages of jobs started between 30 and 39 weeks after layoff, while there appear to be no visually discernible wage effects at shorter or longer durations. Figure 5, panel D plotting coefficient estimates for a monthly series of RD models suggests that the UI benefit extension from 30 to 39 weeks increases target wages, not only during this period but also in the month before the extension sets in.

The observed pattern of dynamic UI wage effects results from a combination of two factors: true responses in search behavior and dynamic selection. As shown in Section II, nonemployment duration itself responds to the UI extension. If this response is correlated with wage changes, the concern arises that the dynamic UI wage effects are driven by compositional changes over the unemployment duration. In other words, the dynamic RD design might fail the McCrary bunching test or a test of sorting in observables.

We offer two remedies to assess the potential bias due to dynamic selection. First, the series of squares in Figure 5, panel D shows that controlling for a rich set of

[32] See online Appendix A.4 and B.3 for more details. We also provide evidence that the higher observed duration dependence is associated with more negative correlation between UI wage and duration effects, as predicted by theory.

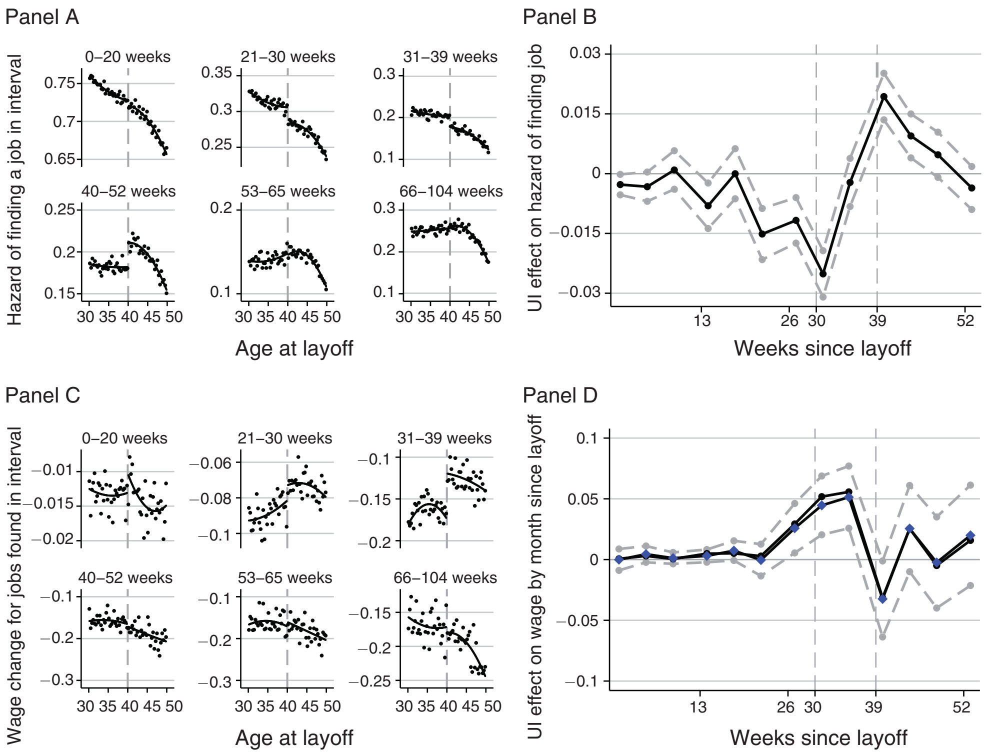

Figure 5. Dynamic Effect of UI Benefit Extension

Notes: Panel A investigates the dynamic effect of a UI benefit extension from 30 to 39 weeks on the job-finding rate. It plots the job-finding rate in different intervals of the nonemployment duration against the age at job separation. The solid line represents quadratic fits on each side of age cutoff. Age bins correspond to 4-month intervals. Panel B plots the RD coefficients from different regressions for monthly hazard rates. Panel C 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 intervals of the nonemployment duration against age at layoff. The solid line represents quadratic fits on each side of age cutoff. Age bins correspond to 4-month intervals. Panel D plots the coefficients from RD regressions for the wage change by month since layoff with and without controlling for covariates, dots and squares, respectively.

observables barely changes the RD coefficients.[33] Second, Figure A10 in the online Appendix shows estimates of nonparametric bounds for the dynamic effect of UI on wages.[34] Both controls for observable covariates and estimated bounds support the interpretation that the dynamic pattern of UI wage effects is due to the choice of lower target wages in the weeks around benefit exhaustion, rather than dynamic selection.

To test the second prediction from the option value of search, we form a prediction of the probability that an individual exhausts benefits or does not find a job within 30 weeks, based on a nonparametric regression on a rich set of ex ante worker and firm characteristics. To avoid the problem of over-fitting, we base the regression

[33] Similarly, Centeno and Novo (2011), Caliendo, Tatsiramos, and Uhlendorff (2013), Schmieder, von Wachter, and Bender (2016) investigate the UI effect during the unemployment spell. In particular, Caliendo, Tatsiramos, and Uhlendorff (2013) addresses the dynamic selection by estimating a hazard model jointly with wage equation and find that the UI effects are larger around the benefit exhaustion.

[34] For details on the construction of bounds see online Appendix B.4.

on observations out of the estimation sample (Abadie, Chingos, and West 2013).[35] Then, we split the sample into two groups of individuals with above and below median probability of benefit exhaustion and estimate the RD models from Table 2 for both groups. The results, shown in Table A7 in the online Appendix, confirm that the UI wage effects in our sample are entirely driven by the group with a high predicted probability of benefit exhaustion.

In sum, the empirical tests confirm the interpretation that UI creates an option value of search, which is responsible for more pronounced effects of the benefit extension among workers who are more likely to reach the extension or who are closer to the exhaustion point.

E. *UI Benefit Extension Attenuates Wage Drops*

The empirical results in Section II show that the average reemployment wages increase due to the UI benefit extension from 30 to 39 weeks. Now, we investigate how this effect is reflected in the distribution of reemployment wages. We are particularly interested in distinguishing between a uniform shift in the distribution of reemployment wages due to UI generosity versus shifts in certain parts of the distribution, such as a lower likelihood of experiencing a wage drop or a higher likelihood of a raise with respect to the pre-unemployment wage.

The target wage model predicts nonuniform effects on the reemployment wage distribution. We formally derive this result in online Appendix A.1.2 for the setting without structural duration dependence. Within this framework, we can show that increased UI generosity due to a benefit extension decreases the probability of finding jobs of very low quality and increases the probability of finding higher quality jobs. In the overall population, the UI effect should mainly be concentrated in the lower part of the job quality distribution, and the reduction in the probability of finding lowest quality jobs should be dominant.[36]

More broadly, a nonuniform UI effect across the wage distribution can be interpreted in the lens of the idea of the option value of search, either within or between individuals. For each individual, the UI extension effect is larger, the closer she is to the benefit exhaustion. Given a decreasing target wage during the unemployment spell due to duration dependence, this implies a nonuniform UI wage effect. Between individuals, the benefit extension more intensely affects agents with a longer unemployment duration, which is negatively correlated with a between-job wage change.

To test these predictions empirically, we estimate a series of RD models conditioning on the magnitude of the wage growth, either for the probability of experiencing wage growth in a certain interval (PDF) or the probability of experiencing wage growth above a certain level (CDF). Figure 6 plots the coefficient estimates along with the confidence intervals for the effects of UI on wage changes along the CDF in panel A and in the PDF in panel B. For instance, panel B shows that the UI benefit

[35] For more details, see online Appendix B.4.

[36] Interestingly, this prediction is in contrast with the reservation-wage model. To see the intuition, we consider a static setting, in which an increase in UI generosity leads to higher selectivity. In a target wage model, higher selectivity corresponds to a shift in the overall distribution of target wages. In contrast, in the reservation-wage model, higher selectivity corresponds to a truncation of the job quality distribution from below, which increases the chance of wage gains including very large wage gains.

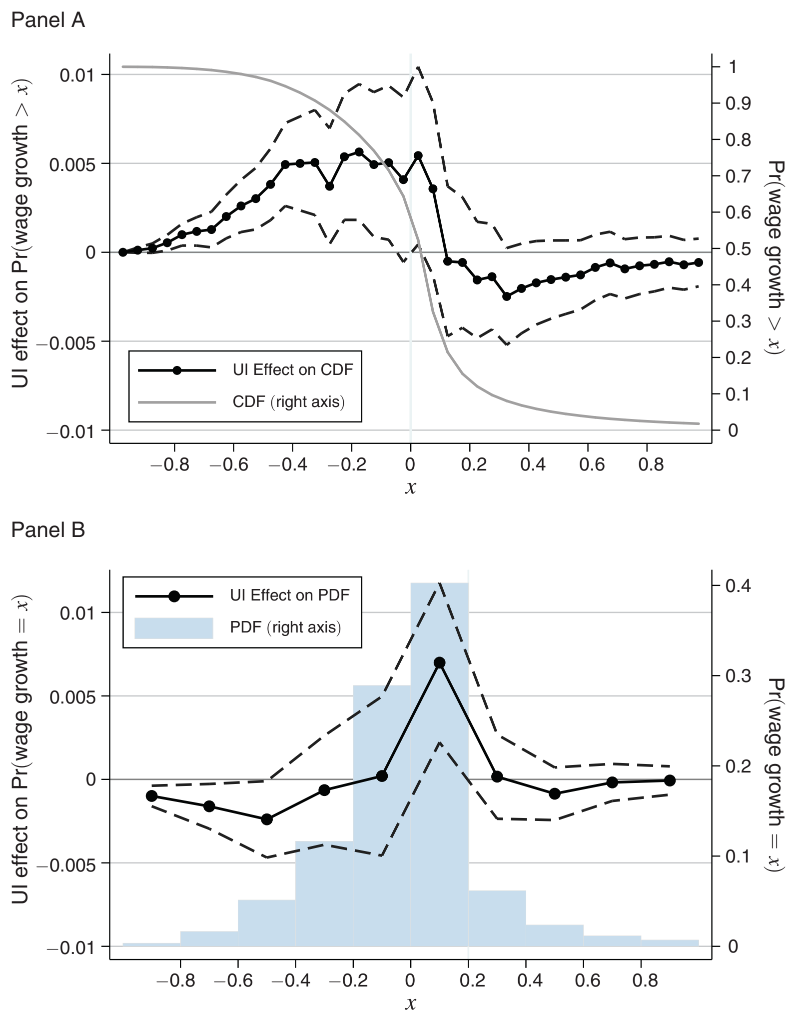

Figure 6. UI Benefit Extension Attenuates Wage Drops

Notes: This figure investigates the effect of a UI extension from 30 to 39 weeks on the between-job wage growth. Panel A plots the UI effect on the CDF of the wage growth distribution on the left axis (marked line) with corresponding confidence intervals (dashed line), the CDF itself on the right axis (solid line). Panel B plots similar result for the PDF of wage growth instead of the CDF.

extension affects the likelihood of experiencing a wage drop of 40 to 60 percent by around −0.3 percent (left y-axis) from a base of 4 percent (right y-axis).

Overall, the graphs confirm the hypothesis of nonuniform changes over the wage distribution. Panel A shows that the UI effects are strongest on wage changes greater than −40 percent to +10 percent. Panel B confirms that the likelihood of experiencing a wage growth between 0 and 20 percent is significantly positively affected, while the probability of wage losses is slightly negatively affected and the probability of experiencing large wage gains is unaffected by the UI extension.

An alternative explanation for the nonuniform UI effect on wage changes is that benefit recipients avoid jobs that pay wages below the UI benefit level, either due to low costs of on-the-job search (Burdett 1978) or due to reference dependence

Table 3—Firm Sorting Effect of Benefit Extension

		Firm-level outcomes						Individual level outcomes
		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
	Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discontinuity at age 40	No	11.04	0.00189	0.00167	0.00324	0.00482	0.00704	0.00680
		(14.87)	(0.00427)	(0.00228)	(0.00125)	(0.00272)	(0.00316)	(0.00313)
	Yes	9.208	0.00336	0.00119	0.00261	0.00507	0.00778	0.00543
		(14.67)	(0.00404)	(0.00193)	(0.00107)	(0.00236)	(0.00306)	(0.00266)
Mean of dependent variable 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

Notes: 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 ten workers. The results are based on the same RDD as Table 2 and described in Section I, 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 two years prior to layoff.

(DellaVigna et al. 2015). In this case, the nonuniform UI effect in Figure 6 can be explained by the average net replacement rate of 55 percent. Reference dependence would also be consistent with the fact that the UI effect is more substantial between 30–39 weeks after layoff (Section IIID).

IV. Mechanisms Underlying the Positive UI Wage Effect

In this section, we investigate the mechanisms through which UI generosity affects re-employment wages by contrasting three main potential drivers. The first channel is bargaining power. By improving the outside options of unemployed workers, benefit generosity could increase their bargaining power vis à vis employers. However, improved bargaining power among workers covered by UI does come at the expense of uncovered workers or capital. The second driver is sorting as workers covered by UI become more selective and are able to wait for better matches (Marimon and Zilibotti 1999). This implies that UI generosity affects employee-employer assignments without necessarily changing the composition of jobs. Third, UI generosity could also change the equilibrium job composition, e.g., by creating more capital-intensive jobs (Acemoglu and Shimer 1999 and Acemoglu 2001).

The three mechanisms are all widely discussed in the theoretical literature, but direct empirical evidence to distinguish between the bargaining and sorting mechanisms is rare. Two empirical findings presented below shed some light on the potential mechanism.

We start by investigating whether UI generosity affects worker mobility across firms, industries, occupations, or regions. Table 4, panel A presents estimation results from RD models where the right-hand-side variables are indicators equal to one if the individual changes firms, industries, occupations, or regions. The findings indicate that UI does not affect worker mobility in an economically or statistically significant

Table 4—Effect of Benefit Extension on Mobility and Job Characteristics

		Probability of changing				
		Firm	Industry	Occupation	Region	Zip code
	Covariates	(1)	(2)	(3)	(4)	(5)
Panel A. Mobility						
Discontinuity at age 40	No	−0.00318	−0.00160	−0.000714	0.00189	0.000518
		(0.00230)	(0.00196)	(0.00113)	(0.00149)	(0.00225)
	Yes	−0.00237	−0.00159	−0.000672	0.00158	0.000985
		(0.00153)	(0.00167)	(0.00108)	(0.00138)	(0.00161)
Mean of dependent variable around cutoff		0.419	0.229	0.0615	0.109	0.360
Observations		1,589,178	1,589,174	1,589,177	1,521,919	1,566,755
		Tenure in new job	Separation within a year	Wage growth in new job	Positive wage growth in new job	New job is full-time
	Covariates	(1)	(2)	(3)	(4)	(5)
Panel B. Nonwage job characteristics						
Discontinuity at age 40	No	1.202	0.000923	0.000001	−0.00126	−0.000420
		(4.034)	(0.00208)	(0.000347)	(0.00240)	(0.00237)
	Yes	0.197	0.000514	−0.000207	−0.00209	−0.00202
		(3.798)	(0.00189)	(0.000342)	(0.00237)	(0.00199)
Mean of dependent variable around cutoff		567.3	0.724	0.0362	0.725	0.845
Observations		1,589,178	1,589,178	1,192,343	1,193,243	755,594

Notes: 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. Tenure is measured in days in column 1 of panel B. Full-time indicator is only available for 2002–2012. Wage growth in new job is defined as the change in the log of the average monthly wage in post-employment jobs between the first two calendar years after hiring. The mean of the dependent variable for three years around the cutoff is reported. The covariates used are individual characteristics, such as gender, marital status, a dummy for Austrian citizenship, education, tenure, experience during the last two and five years, month of layoff, calendar week of layoff; and previous firm's characteristics such as industry, frequency of layoff, and proportion of recalls.

way. The point estimate for the probability of switching firms is negative, indicating that workers eligible for extended benefits are more likely to be recalled, but economically and statistically insignificant. The industry affiliation of the employer is measured in 8 broad categories, but the result remains unchanged if we switch to a finer 4-digit classification. Occupations can only be approximated by a blue- or white-collar indicator and, at this broad level, no mobility across occupations is observable. Like the probability of firm changes, regional mobility is unaffected by the UI benefit extension, whether we measure it at the level of 9 federal regions or at the employer zip code level (corresponding to the workplace address). This echoes the recent findings of Carloni (2015) in the United States, who finds no evidence of an effect of UI duration on the geographical mobility of unemployed workers.

Next, we investigate the type of firms where workers find new jobs. We are particularly interested in the question whether more generous UI enables workers to find jobs in firms that pay higher wages to all their employees, or whether they find a better-paid job within the same type of firm. Evidence in favor of these two channels will allow us to distinguish between the bargaining and sorting mechanisms. Table 3 reports RD estimates of the UI effect on firm-level outcomes for the sample of workers laid off from firms with more than 10 employees, because firm characteristics are not informative for very small firms. To confirm that this restricted sample is comparable to the overall population, column 7 reports the UI wage effect estimates for this sample.

The estimation results indicate that workers who are eligible for the UI benefit extension are moving toward "better" firms: they find jobs in slightly larger firms, with higher proportions of male and older workers which, on average, pay higher wages to their other employees (columns 1, 3, 4, 5, and 6 of Table 3, respectively).[37] However, note that not all effects are estimated precisely enough to allow firm conclusions.[38]

Overall, the evidence presented in Tables 3 and 4 suggests that UI generosity affects the type of firms where unemployed workers find new jobs, but does not affect the sorting of workers to sectors, industries or geographical locations. We can tentatively interpret this result as evidence in favor of the hypothesis that UI increases the size of more productive firms (Acemoglu 2001). An alternative interpretation would be that UI increases the likelihood of unemployed workers being matched to firms in which all employees have a relatively high bargaining power. The latter interpretation would be a combination of first and second mechanisms.

V. UI Effect on Other Measures of Job Quality

The wage is arguably the main and most salient characteristic of a job. However, according to the theory presented in Section IIIA, UI generosity should also affect other measures of job quality. In this section, we exploit the RD design to investigate the effects of the UI benefit extension on a range of job characteristics that are observable in the administrative data: job tenure, within job wage growth, and full-time versus part-time jobs.

Estimation results presented in Table 4 panel B show that the UI benefit extension from 30 to 39 weeks does not have a statistically significant effect on any measure of job quality. In particular, the benefit extension has no statistically or economically significant effect on the duration of the new job (column 1). The point estimate is positive with a magnitude of about one day, which is economically insignificant as compared to an average tenure of 567 days. Since the employment spells are censored by the end of our observation period, we also consider the UI effect on the probability of experiencing a job separation within one year in column 2. This results confirms the absence of an UI effect on tenure, which is surprising, as matching models predict that better jobs should, on average, last longer (Jovanovic 1979). Furthermore, while

[37] Male workers are more likely to bargain with their employers, which might be one explanation for why firms with higher proportions of male workers do, on average, pay higher salaries. For recent evidence and references, see Card, Cardoso, and Kline (2016).

[38] A more precise, but computationally demanding, approach would be to estimate the firms' component of the UI wage effect using the Abowd, Kramarz, and Margolis (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 ten employees, and exclude temporary layoffs, who often rejoin their previous employer. Online Appendix Table A8 shows that this sample restriction does not change the point estimates and increases the precision.

the average annual wage growth in the sample is 3.6 percent, we find a precise zero effect of the benefit extension on wage growth in the new job (columns 3 and 4).

Lastly, we measure the effect of the UI extension on the probability that the post-unemployment job is full-time versus part-time, a variable which is only available as a binary variable for the years 2002 to 2012. The working-time indicator is an important outcome for two reasons. First, unemployed workers might be willing to accept a part-time job if they are in a rush to exit unemployment. From this perspective, the outcome reflects job quality. Second, as discussed in Section II, the ASSD only reports daily wages per employer, but not the hourly wage rate. This implies that our estimate of the UI wage effect potentially captures both a UI effect on the hourly wage rate and an effect on hours worked per day. From this perspective, measuring the UI effect on the likelihood of finding a full-time job sheds some light on the potential effect of UI on hours. The estimate in column 5 of Table 4, panel B shows that while 85 percent of post-unemployment jobs are full-time, the UI extension from 30 to 39 weeks has no statistically significant effect on this likelihood. The magnitude of the effect is economically insignificant, and its sign suggests that the UI extension reduces the hours worked per day. This result confirms that the estimated UI effect of 0.5 percent is a wage response rather than an hours response.[39]

In sum, the estimates in Table 4, panel B suggest that UI generosity has no economically significant effect on other measures of job quality in our setting. Importantly, the results also indicate that the UI effect on starting wages does not persist beyond the duration of the first post-unemployment job. A more generous UI helps workers find better paying jobs, but it has not impact on their longer-term careers.

VI. Policy Implications

Does the effect of UI on job quality change the optimal generosity (level and duration) of UI? The social cost of a change in potential benefit duration is the fiscal externality it creates, which should be weighed against the welfare benefit of the insurance provided (Baily 1978). The UI wage effect attenuates the welfare cost of the UI program. To illustrate this point, we can decompose the fiscal externality of UI into two parts:

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

where n stands for the expected duration of nonemployment, $\tilde{n}$ stands for the expected nonemployment duration covered by UI, and τ is the total tax on labor earnings including UI tax.[40]

The first term is the effect of the UI wage effect on the government budget, a positive fiscal externality which has been overlooked in the prior literature. Agents do not

[39] The UI effect on hourly wage is equal to the UI daily wage effect minus the UI effect on hours worked. The latter can be bounded from above by assuming that changes from full-time to part-time jobs are equivalent to a reduction in hours worked of 25 percent. This implies that the UI hourly wage effect is between 0.5 and 0.51 percent.

[40] Here we rely on the work of Lawson (2016) who points out that the main fiscal externality of a social program occurs through labor income tax.

internalize that their search decisions have an externality: a change in reemployment wage implies a change in the future labor income tax. This externality is directly due to the proportionality of the tax. The second term represents the traditional negative fiscal externality in case of limited UI duration: a lower tax revenue due to longer nonemployment and higher UI expenditure.

Moreover, the fiscal externality of the UI wage effect is quantitatively important in our setting. Replacing our estimates from Table 2 in equation (10), we find that the total fiscal externality is equal to $\left(20\% - 45\%\right) w^e$. This implies that the overall fiscal externality is thus equal to -25 percent of the average weekly wage, only half as big as the externality of -45 percent if we had ignored the wage effect. The UI wage effect thus has a significant welfare implication that should not be ignored in the design of UI systems.[41]

In the online Appendix, we characterize the optimal UI design in the presence of two real-world features of a UI system: limited duration and proportional tax. We find a formula for the optimal amount of UI benefit level and duration. This allows us to quantify how the positive fiscal externality due to the UI wage effect changes the UI optimal design. Namely, we show that taking the UI wage effect into account would increase the optimal UI duration such that the share of unemployed exhausting their benefit decreases by one-fourth. In our setting, this is equivalent to moving the optimal UI duration from 30 to 40 weeks. Two additional insights developed in the online Appendix are worth emphasizing here. A higher degree of risk aversion implies a longer (lower) UI duration (benefit) at the optimum. However, the absence of a consensus on the empirical value of risk aversion hinders any concrete conclusion about optimal UI.[42] Thus, we provide an upper bound for the UI replacement rate independent of the risk aversion parameter. Namely, the optimal replacement rate should be below $2/3$, if the fiscal externality of the benefit level is higher than that of benefit duration. The latter is the case when the average unemployed does not exhaust her benefit, which is the case in the UI systems of many developed countries. In sum, we conclude that while considering the design of the UI system, incorporating the UI wage effect and its implied fiscal externality is quantitatively important.

VII. 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 suggests that UI allows the unemployed to find jobs that are better suited to their skills, empirical work has not found any evidence of a positive causal effect of UI generosity on the quality of re-employment jobs. Layard, Nickell, and Jackman (2005, p. 211) wrote in their classical text on unemployment: "It is clear that we

[41] Following Baily (1978), prior literature on optimal UI mainly focused on UI effect on unemployment duration of eligible workers, neglecting the UI effect on future wage of eligible workers and effect on non-eligible workers. In contrast, we focus on *both* UI effect on eligible workers' unemployment duration and job quality, but abstracting from potential UI effect on non-eligible workers. This is a sensitive abstraction in our setting since the nine-week UI extension affects a small share of the labor force. For recent literature, see Di Maggio and Kermani (2016), Hagedorn et al. (2015), Lalive, Landais, and Zweimüller (2015), Landais, Michaillat, and Saez (forthcoming), and Marinescu (2014).

[42] For example, see Baily (1978), Gruber (1997), and Chetty (2006).

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 argue that the magnitude of this UI wage effect is economically meaningful from both an individual and a social perspective. The magnitude of the effect is consistent with rational behavior of an agent who extends her unemployment by being more selective in her job search, knowing that this can help secure a lasting wage gain upon reemployment. Moreover, from a social perspective, the UI wage effect implies a considerable positive fiscal externality.

We reconcile this finding with the previous literature using a theoretical model that shows that the UI wage effect is the result of two offsetting forces and thus it can, in theory, take any sign and magnitude. We characterize the potential heterogeneity in matching functions that can create a negative relation between the UI wage effect and the UI effect on nonemployment duration that holds across estimates in the literature. Then, we provide a direct test of this prediction in our data.

Soaring unemployment rates during the last recession have brought unemployment benefits back to the center of public attention. The debate rages on whether UI provides a remedy or exacerbates the problem. A key factor missing from this debate has been the effect of UI on job-match quality. Proponents of UI were cautious to make the case for a positive UI effect on job quality. Summarizing this debate, the *New York Times* printed: "Economists expect that the end of the emergency jobless benefits will, surprisingly, lead to a sharp drop in the unemployment rate … because the loss of benefits might spur some workers to intensify their job search, or accept an offer they might have turned down."[43] This statement emphasizes that the expected reduction in unemployment durations could reflect two different reactions of unemployed workers to UI, which we named search and selectivity margins. They are the central piece of our explanation for the disparate UI wage effect estimates in the literature. However, a deeper understanding of these mechanisms and determinants of these margins requires further research.

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