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Estimating Duration Dependence on Re-employment Wages When Reservation Wages Are Binding

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Abstract

This paper documents a novel finding indicating that re-employment wages are elastic to the level of unemployment insurance (i.e., a binding reservation wage) and adapts the IV estimator for duration dependence in Schmieder et al. (2016) to account for this fact. Using administrative data from Spain, we find that unemployed workers lower their re-employment wages by 3 percent immediately after the exhaustion of unemployment insurance (UI) benefits. Workers' characteristics and permanent unobserved heterogeneity cannot explain this. To estimate duration dependence, we extend the IV framework proposed by Schmieder et al. (2016), whose estimator of duration dependence is proportional to the response of wages to an extension of the potential duration of UI, to account for the response of reservation wages. We find that while extending the potential duration of UI has an insignificant effect on re-employment wages, duration dependence is strongly negative. We estimate that the degree of duration dependence in Spain is approximately 0.8 percent per month in daily wages. Workers' inability to find full-time jobs as the duration of non-employment increases is an important mechanism behind this effect, since the duration dependence of hourly wages is 0.25 percent per month. Failing to account for the fact that reservation wages are binding would underestimate the magnitude of duration dependence by 15 to 20 percent.

Keywords: Unemployment, Duration Dependence, Re-employment Wages

JEL Codes: J65, J64.

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1 Introduction

A well-established fact in the economic literature is that a job loss generates large and persistent earnings and wage losses. The recent literature highlights the important role of the duration of non-employment in accounting for the magnitude of these losses (See Fallick et al. (2021) and Schmieder and Heining (2021)). Whether an increased duration of non-employment following a job loss leads to a larger wage loss has crucial implications for our understanding of the consequences of long-term unemployment, the design of UI policies, and the types of features that models capturing the behavior of unemployed workers should include. However, it remains unclear if this relationship arises because the non-employment duration causally affects re-employment wages (duration dependence[1]). Other forces, such as dynamic selection and the fact that the value of the outside option declines as the duration of non-employment increases because the benefits available to unemployed workers are reduced,[2] could be behind this association.

To answer this question, a handy way of inferring the causal effect of non-employment on wages is to use quasi-experimental variation in UI benefits and to compare its effect on re-employment wages vs. that on non-employment durations.[3] Schmieder et al. (2016) rationalize such instrumental variable estimators, provided that the reservation wage is not binding.[4] However, a large body of literature documents that changes to UI benefits extend non-employment durations but have no effect[5] or a positive effect[6] on re-employment wages. This evidence is difficult to reconcile with a non-binding reservation wage because it would imply that duration dependence is zero or even positive, a result that seems unlikely based on the previous literature. This raises an important question of whether reservation wages are generally binding and how we can adapt the IV estimator to identify the duration dependence after taking into account the response of workers' wage choice.

This paper presents novel evidence indicating that reservation wages are binding and characterizes a new estimator (LMOS estimator) to recover the causal effect of the duration of non-employment on wages, net of the effect of changes in UI benefits. Using Spanish administrative data, we find that duration dependence is 0.8 percent per month in daily wages. If we were to ignore the response of reservation wages to changes in UI benefits, we would end up with an insignificant effect whose point estimator underestimates duration dependence by 15 to 20 percent.

To achieve our goals, we take advantage of a feature in the Spanish UI system that grants different potential durations of UI to otherwise almost identical workers based on whether their

[1]Duration dependence encompasses several sources that, as the duration of non-employment increases, can generate a decline in workers' labor market opportunities. These include human capital depreciation, an adverse signaling effect, and a declining matching efficiency, among others.

[2]See Krueger and Mueller (2016)

[3]The empirical evidence on these two moments is abundant (see survey in Schmieder and Heining (2021)), which makes this method handy to implement.

[4]This means that the wage elasticity with respect to the value of the outside option is zero.

[5]See Card et al. (2007b), Lalive (2007), Le Barbanchon (2016), and van Ours and Vodopivec (2008)

[6]See Nekoei and Weber (2017) and Farooq et al. (2022)

previous working experience crosses certain thresholds. This quasi-experimental variation allows us to estimate the causal effect of an extension of the potential duration of UI on time to re-employment[7] and on re-employment wages.

We additionally present novel evidence indicating that reservation wages are binding in the sense that workers' wages respond to the value of the outside option. Taking advantage of our quasi-experimental variation, we document that wage losses for workers with exogenously longer and shorter potential durations of UI evolve identically throughout time to re-employment, except at the point at which workers with a shorter potential duration of UI have just exhausted their UI benefits while those with a longer potential duration of UI can still collect them. In this time frame, the wage losses of workers with shorter potential duration increase by around 3 percentage points relative to the losses of workers with longer potential duration. We provide robust evidence that this result is not driven by dynamic selection,[8] selection in observed characteristics, or selection in permanent unobserved heterogeneity. We interpret this result as the causal impact of the exhaustion of UI benefits on re-employment wage losses. Furthermore, we show that the decline in wages at the point of exhaustion of UI benefits is equivalent to the response of wage choices to an extension of the potential duration of UI and that it is also evidence of binding reservation wages.[9]

To estimate the duration dependence, we extend the random search framework in Schmieder et al. (2016) to a more general directed search framework that accounts for the fact that the wage choice is elastic to the value of unemployment (reservation wages bind in Schmieder et al. (2016)'s case). We refer to this estimator of duration dependence as the LMOS estimator. When reservation wages are not binding, both Schmieder et al. (2016)'s IV and the LMOS estimator are identical. However, when wage choices respond to the exhaustion of UI benefits, as occurs in our case, the IV estimate captures not only the causal effect on the wage losses of the duration of non-employment but also the effects on wages arising from a changed probability of exhausting the UI benefits when UI changes. This is not the case for the LMOS estimator, which, regardless of whether reservation wages bind, only captures the causal effect on the wage losses of the duration of non-employment, net of any UI exhaustion effects.[10]

Our LMOS estimate of duration dependence indicates that each additional month of non-employment deepens daily wage losses by approximately 0.8 percent, significantly different from zero at the 95 percent confidence level. However, an important part of this deterioration appears to

[7]Throughout the paper we use non-employment duration and time to re-employment interchangeably to refer to the period from when the worker starts the unemployment spell until the worker finds a new job.

[8]Our empirical strategy is not affected by dynamic selection as long as this selection only depends on time to re-employment and not on the potential duration of UI. This means that our estimates will not be biased as long as dynamic selection in the treatment and control groups evolves identically throughout the distribution of time to re-employment and it is not a function of the potential duration of UI.

[9]The key behind this equivalence is the assumption that the duration dependence evolves smoothly around the point of exhaustion of UI benefits.

[10]Our goal is to estimate duration dependence and not to identify the relative importance of the different channels behind it (human capital depreciation, signaling, etc.).

be driven by workers being unable to find full-time jobs as the non-employment duration increases. When we instead estimate duration dependence in hourly wages, we find an LMOS estimate of 0.25 percent per month of non-employment, insignificantly different from zero.

One important limitation of previous work has been the inability to account for dynamic selection in unobserved permanent characteristics when determining the degree of duration dependence. This makes it difficult to assess whether the differences (or lack thereof) in wage losses between treatment and control groups at each point of the distribution of time to re-employment represent equal behavior or if they are the result of selection. We overcome this limitation by additionally estimating duration dependence after controlling for selection based on unobserved permanent heterogeneity in all our moments of interest, using a sample of workers with two or more UI claims. Our LMOS estimate of duration dependence after controlling for selection in permanent unobserved heterogeneity is insignificantly different from our main estimate.[11] However, at the same time, our findings suggest that this type of selection, while statistically insignificant, could be economically important, primarily when it comes to our estimates of the LMOS for daily wages.

Our paper contributes to four strands of the literature. First, it adds to the empirical literature that studies unemployment dynamics and the value of non-employment. We document that re-employment wages respond to the value of non-employment, declining by approximately 3 percent just at the time that UI benefits end. While previous work documents a spike in the hazard rate (Card et al. (2007a)) and a drop in consumption (Ganong and Noel (2019)) when UI benefits end, the evidence pertaining to re-employment wages is significantly more scarce. To our knowledge, only one other paper, Nekoei and Weber (2017), documents that wage changes decline in response to the expiration of UI benefits,[12] but their data do not allow them to fully test whether this could be driven by dynamic selection.[13] Whether re-employment wages (or reservation wages) respond to the value of non-employment remains an open empirical question. In that regard, our results contrast with some of the findings in the previous literature (Lalive et al. (2015), Krueger and Mueller (2016), Schmieder et al. (2016), and Jäger et al. (2020)) that show that re-employment wages (or reservation wages) respond little (if at all) to the value of non-employment.

Second, this paper contributes to the literature that studies the effects of the non-employment duration on subsequent outcomes, in particular wages. We provide new duration dependence estimates, an important input in several types of macroeconomic and public finance models. Both theoretically and empirically, we show that isolating the effect of the exhaustion of UI benefits

[11] Differences in sample composition between our main sample and the sample of workers with two or more UI claims do not drive this result.

[12] In their case the decline is approximately 5 percent and occurs not just at the expiration of UI benefits but starting around 1 month earlier.

[13] In an unpublished manuscript, Centeno and Novo (2011) also show that re-employment wages appear to only respond to an extension of the potential duration of UI around the original point of exhaustion of benefits. Additionally, Marinescu and Skandalis (2021) show that the target wages of French workers decline by approximately 2.4 percent in the year prior to the expiration of benefits, but do not present evidence on how this effect translates into re-employment wages.

on wages is essential to identifying the degree of duration dependence. As in Schmieder et al. (2016), our identification strategy comes from analyzing the causal impact of UI extensions on non-employment durations and on re-employment wages. However, we emphasize the importance of separating the effects on re-employment wages over time that come from changes in UI benefits versus those arising purely through duration dependence. The former depends on the UI policy, while we see the latter as a primitive of the evolution of the workers' labor market opportunities over time.

Third, we add to the literature studying the role of dynamic selection in explaining changes in the hazard rate or wage losses over the non-employment spell. A large literature makes assumptions about the importance of unobservables based on that of observables (Krueger et al. (2014), Schmieder et al. (2016), and DellaVigna et al. (2017)), generally concluding that dynamic selection is unimportant. On the other hand, Ahn and Hamilton (2020) and Alvarez et al. (2016) follow a different approach and conclude that the role of dynamic selection in explaining the reduction of the hazard rate over time is significant. Our results, which directly account for dynamic selection based on permanent unobserved heterogeneity, find a middle ground. Both dynamic selection and duration dependence appear to be economically important in explaining wage losses over the non-employment duration, even if we find that dynamic selection is statistically insignificant.

Finally, this paper contributes to the discussion of which type of theoretical model better describes the behavior of unemployed agents. While very limited, the available quasi-experimental evidence, including this paper, indicates that time to re-employment causally affects re-employment wages. This result suggests that models describing the behavior of unemployed workers should incorporate this feature, either directly or, in models in which the wage distribution faced by the workers does not change over the non-employment duration, through other mechanisms such as duration dependence on search costs. For instance, DellaVigna et al. (2022) compare the goodness of fit in hazard rates of the standard partial equilibrium model, a reference-dependent model, and a standard model with duration dependence on search cost using German data. They conclude that the standard model with duration dependence better fits the data (although it does so even better when combined with reference dependence) but that the evidence for the existence of duration dependence on search costs is extremely limited.

The rest of the paper is organized as follows. Section 2 introduces the Spanish Social Security data and the institutional design of the UI system in Spain. Section 3 exploits our quasi-experimental variation to estimate the causal impact of an extension of the potential duration of UI and that of the exhaustion of UI benefits. Section 4 outlines an illustrative model, introduces the channels underlying the decline in the re-employment wage over the duration of non-employment, and establishes the connection between these channels and the causal effects estimated in Section 3. Section 5 presents our LMOS estimates of duration dependence and discusses their implications. Section 6 concludes.

2 Data and Institutional Features

2.1 The Unemployment Insurance System in Spain

Unemployment insurance in Spain is characterized by two variables: the benefit level and the potential duration. Workers can collect UI benefits if they lose their previous job involuntarily (workers who quit their jobs are not entitled to UI benefits) and have worked for at least one year during the previous six years.

By the potential duration of UI, we mean the maximum duration a worker is allowed to receive unemployment insurance benefits. The potential duration is completely determined by the number of days worked in the previous six years, regardless of whether it was full- or part-time work, and ranges from 4 months to 24 months. The relationship between the number of days worked in the previous six years and the potential duration of UI is not smooth but has multiple large, discrete changes. For instance, if an individual works 539 days, the potential duration of her UI will be 4 months, while if she works 540 days, the potential duration of her UI will be 6 months. Table 1 summarizes the potential duration of UI as a function of the number of days worked in the previous 6 years. Once individuals have exhausted their UI benefits, they can apply for unemployment assistance (UA) if they are still unemployed. UA has a set of very stringent eligibility rules. Workers who are eligible for UA can claim roughly 430 euros per month (in 2016), equivalent to 50 percent of the minimum wage, for 6-20 months. For more details on UA, see Domènech-Arumí and Vannutelli (2023).

Table 1: Unemployment Insurance in Spain: Potential Duration

	Days Worked in Previous 6 Years											
From	0	360	540	720	900	1080	1260	1440	1620	1800	1980	2160
To	359	539	719	899	1079	1259	1439	1619	1799	1979	2159	-
	Potential Duration of UI (Months)											
	0	4	6	8	10	12	14	16	18	20	22	24

The unemployment benefit level is determined as a replacement rate of the worker's previous wage, and it is paid monthly until a) the worker finds a new job or b) the worker reaches the potential duration she is entitled to. During the first 6 months that the worker is collecting unemployment benefits, the replacement rate is 70 percent, decreasing to 50 percent afterward.[14] Benefit levels are subject to minimum and maximum amounts that vary by year and number of children.

Finally, the Spanish unemployment insurance system provides workers with the *right to choose* whether to create a new potential duration and benefit level bundle when entering unemployment

[14] The replacement rate after 6 months of collecting unemployment benefits was lowered to 50 percent in October 2012. Prior to that, it was 60 percent.

or to carry over an unfinished old bundle.[15] To avoid this complication, we restrict our sample to unemployment spells based on new work histories, ignoring carryovers.

2.2 Data Description and Data-Cleaning Process

We take advantage of the *Muestra Continua de Vidas Laborales* (MCVL) for the years 2006 to 2017. Each year, the MCVL randomly selects 4 percent of the individuals with a relationship with the Social Security Administration during the year (i.e., employed and unemployed workers, retired individuals, and recipients of other subsidies).

If an individual is selected for a given MCVL year, both her daily lifetime record of Social Security affiliations (i.e., work spells, unemployment spells, self-employment, retirement spell, and other subsidy spells up to the sample year) and her lifetime record of monthly wages per employer are provided.[16] The combination of daily labor histories and monthly compensation allows us to create precise measures of tenure and daily compensation by job, even if the individuals change jobs (and contracts) within the same firm. The MCVL also provides demographic information, both at the individual and the household level. We observe workers' age, household composition, location, migration status, and educational level.

Using the historical records of the MCVL we build a sample of displacements (i.e., unemployment insurance claims preceded by a working spell) for workers ages 25 to 50 between 1994 and 2016. We impose several restrictions when constructing our sample. We limit our sample to workers who are re-employed within 5 years of losing their job. We focus on workers whose previous job had a part-time coefficient larger than 0.88 during the period used to determine the potential duration of UI (i.e., equivalent to a job that requires 35 hours or more per week).[17] Additionally, we eliminate individuals who have been self-employed at some point in the 6 years prior to unem-

[15] If a worker who enters unemployment has been unemployed in the previous 6 years, the worker can be given two choices for benefit level and potential duration. She can choose between the benefit level and potential duration that was generated since the last time she left unemployment. On the other hand, if the worker did not exhaust her potential benefit duration during the previous unemployment spell, she can choose to enjoy the leftover amount of the previous claim. For instance, suppose a worker in 2013 enters unemployment with a potential duration of 24 months and a benefit level of 1,050 EUR during the first 6 months and 750 EUR afterward (i.e., a previous salary of 1,500 EUR). The worker spends 4 months on unemployment and finds a new job. She works in the new job for 3 years with a wage of 1,400 EUR and is dismissed again. She now has the *"right to choose"* which bundle of benefits she wants to use. She can choose to reuse the leftover amount of the old claim and enjoy 20 months of potential duration, with a benefit level of 1,050 EUR for two months and 750 EUR for the remaining potential duration. On the other hand, she could choose to create a new bundle of benefit level and potential duration (i.e., new claim) based on the last 3 years of employment. Therefore, her second choice has a potential duration of 12 months but a benefit level of 980 EUR during the first 6 months and 700 EUR for the remaining potential duration. The worker is free to choose whichever bundle she considers best but cannot combine them in any way.

[16] Observed wages are capped at a maximum, which varies by year. This is not a problem in our setting since, for workers entering unemployment, less than 1 percent of observations show a previous wage at the maximum cap.

[17] We additionally discard UI claims from workers who, upon re-employment, have a job with a part-time coefficient smaller than 0.25 (equivalent to 10 or fewer hours per week on average). Note that this restriction allows workers to work less than 10 hours per week, but conditional on working on a given day, the worker needs to work for at least 2 hours during that day. This restriction affects less than 0.5 percent of our sample.

ployment or who exit unemployment into self-employment. Furthermore, we discard any individual who presents negative wages.[18] We also discard those who simultaneously work and collect unemployment benefits, something that was possible at certain points due to very specific programs implemented by Social Security.

We discard the spells of workers whose unemployment insurance records are not consistent with their "calculated" previous tenure. Specifically, we discard any unemployment spell where: a) the worker collects unemployment insurance for a period of time longer than what we would expect based on the policy schedule and her "calculated" working experience in the previous 6 years, and the amount of time the worker collects UI benefits corresponds to the maximum potential duration of UI of a different tenure group; and the worker does not start a job right after she stops collecting UI (but eventually starts a new job); and b) the worker collects unemployment insurance for a period of time shorter than what we would expect based on the policy schedule and her "calculated" working experience in the previous 6 years; and the amount of time the worker collects UI benefits corresponds to the maximum potential duration of UI of a different tenure group; and the worker does not start a job right after she stops collecting UI (but eventually starts a new job). Approximately 3 percent of the UI claims in our sample fall under a), while another 4 percent correspond to b).

We exclude all the unemployment spells of workers whose tenure in the previous 6 years is less than 450 days or more than 1,970 days. By doing this, we do not take advantage of two of the policy discontinuities (359-360 days and 2,159-2,160 days) available in our data. We avoid the former discontinuity because, during our sample period, there were several changes to the rules governing the subsidies for those without enough tenure to qualify for unemployment benefits, which would complicate the analysis even further. In the case of the latter, the policy schedule dictates that only tenure in the previous 6 years should be considered, which creates a mechanical bunching of workers to the right of the discontinuity. Extending the window where we count the previous tenure as seven years would solve the mechanical bunching but at the cost of misclassification of workers across the discontinuities.

To reach our final sample, we make one additional sample restriction. We remove workers entering unemployment after exhausting the predetermined length of certain temporary contracts. We impose this restriction for two reasons. First, these workers are aware of the expiration date of their contracts and are more likely to start searching for new jobs before their previous employment spell finishes. Second, workers exhausting temporary contracts have a much higher probability of having a previous tenure of exactly 6, 12, 18, 24, or 36 months. While this is not a problem in itself, when combined with the UI schedule in Spain, this results in these workers usually being located just to the right of our discontinuities of interest and receiving an additional two months of

[18]While no individual receives negative wages, corrections to the Social Security records show up as negative wages in some instances. Moreover, manual entry of data can result in typos showing negative wages.

potential duration of UI.[19] As shown in Figure A.1 (b), keeping these workers in our sample would create manipulation in the running variable. Removing these workers results in a much smoother distribution of the running variable, as shown in Figure A.2 (b).

Our final sample contains over 132,000 unemployment spells corresponding to 106,000 different workers. Table A.1 provides summary statistics of our main variables of interest.

3 The Causal Effect of Extending the Potential Duration of UI and of Exhausting UI Benefits on Wages

This section estimates the causal impact of an extension of the potential duration of UI on time to re-employment and on the change in wages between the pre-displacement and the re-employment job. From there, our analysis focuses on understanding the effects of an extension of the potential duration of UI on the wage losses throughout the distribution of time to re-employment, where we primarily focus on the effects around the exhaustion of UI benefits.

3.1 The Causal Impact of an Extension of the Potential Duration of UI

Identification of the causal effect of an extension of the potential duration of UI on the wage losses and on the time to re-employment is provided by discontinuous increases in the potential duration schedule. These extensions of the potential duration of UI take place when the workers' past work experience during the previous 6 years crosses one of the threshold cutoffs. Table 1, shown above, presents the detailed schedule and shows the 11 different cutoffs.[20] These threshold cutoffs are multiples of 180 days, ranging from the first cutoff at 360 days to the last cutoff at 2160 days. When the worker's prior work experience exceeds the cutoff value, the worker's potential duration of UI increases by two months. This creates quasi-random variation in the potential durations of UI for similar workers based on whether they crossed a threshold, forming the basis of our regression discontinuity (RD) design below.

We focus on two main outcomes of interest. The first one is the time to re-employment. We define it as the length (in days) from the day the worker loses the job that leads to the UI claim to the day the worker starts a new job. The second one is the wage change (i.e., wage losses). We define it as the difference in log daily wages between pre-displacement and re-employment jobs. We define the wage in the re-employment job as the average daily wage during the first month of the

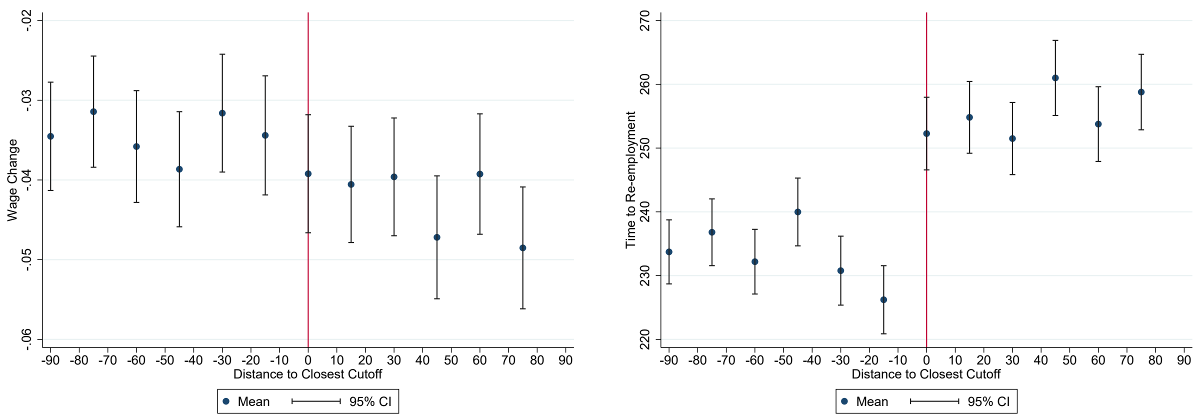
[19] For instance, a worker exhausting a 2-year contract would have 730 days of previous tenure. Since the cutoff between 8 and 10 months of UI is located at 720 days of previous tenure, this results in bunching 10 days to the right of the discontinuity (as shown in Figure A.1(a)).

[20] As explained above, we do not exploit the discontinuities at 359–360 and 2159-2160 days worked in the previous 6 years.

first re-employment job.[21]

We start by combining all our discontinuities in one single specification, showing in Figure 1 the difference a two-month extension of the potential duration of UI makes for the wage losses and the time to re-employment. Panel (a) shows the average wage loss for individuals along the distribution of the distance to the closest potential duration cutoff. Individuals to the left of zero have very similar prior working experience compared to those to the right but have two fewer months of potential duration of UI. The figure shows no clear difference in wage losses for individuals close to but on different sides of the discontinuities. Panel (b) shows that the average time to re-employment for workers close to the cutoffs and entitled to two extra months of potential duration of UI is 25 to 30 days longer compared to those who do not cross the discontinuity.

Figure 1: The Effect of a Two-Month Increase in Potential Duration: All Cutoffs Combined



(a) Wage Losses (b) Time to Re-employment

Note: These figures non-parametrically show the impact of crossing the cutoff threshold on the change in daily wage (panel (a)) and on time to re-employment (panel (b)). The daily wage change (i.e., wage loss) is the difference between the daily log pre-displacement wage and the daily log re-employment wage. We pool workers with different potential durations together. The red line at 0 on the x-axis marks the threshold where workers start receiving two additional months of potential duration.

Formally, we estimate the effects on the time to re-employment and on the change in wages of a two-month extension in the potential duration of UI using an RD design. We follow the ideas in Hahn et al. (2001) and Porter (2003), and construct a popular estimator of τ using kernel-based local polynomials on either side of the threshold. The local polynomial RD estimator of order p is:

$$\hat{\tau}(h_n) = \hat{\mu}_{+,p}(h_n) - \hat{\mu}_{-,p}(h_n) \tag{1}$$

where $\hat{\mu}_{+,p}(h_n)$ and $\hat{\mu}_{-,p}(h_n)$ denote the intercept (at the discontinuity point) of a weighted local p^{th}-order polynomial regression for only treated and only control units, respectively (see Calonico et al. (2014) for further detail). Our main specifications use a first-degree polynomial with a triangular kernel and the maximum possible bandwidth (90 days) that locates workers exclusively

[21]Occasionally, we use different wage change measures, such as hourly or daily wages over a longer period. When that is the case, we specify the chosen measure in the main text.

on one of the sides of the discontinuities.[22] For additional robustness, we also estimate the results of alternative specifications using a bandwidth choice that minimizes an approximation to the mean squared error of the point estimator, as shown in Calonico et al. (2020).

Table 2 presents the results from estimating the RD model of equation (1) where we combine all discontinuities into one single specification. Columns (1) and (2) show the estimated impact of a two-month extension in the potential duration of UI on the time to re-employment. Column (1) includes only discontinuity fixed effects as a control variable (Controls: "Disc"), while column (2) additionally controls for age, gender, education, wealth, previous tenure, previous experience, type of contract, previous wage, part-time coefficient, and month-year fixed effects (Controls: "All"). A two-month extension of the potential duration of UI increases the time to re-employment by 27 days, significantly different from zero at the 1 percent confidence level. The inclusion of controls in column (2) changes little the point estimator, suggesting that differences in observed characteristics play a very limited role in generating the observed effects. These estimates translate into a marginal effect of 0.45. Compared to the previous literature, our results are right in line with the median estimate for European countries shown in the review of previous literature by Schmieder and von Wachter (2016). Our estimated marginal effect is slightly higher than those in Schmieder et al. (2016) and Nekoei and Weber (2017) for Germany and Austria, respectively, but similar to that in Huang and Yang (2021) for Taiwan.

Table 2: Effect of a Two-Month Extension of the Potential Duration of UI. All Discontinuities

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	27.107***	27.369***	-0.007	-0.006	-0.003	-0.002
	[3.488]	[3.407]	[0.005]	[0.004]	[0.004]	[0.003]
Controls	Disc	All	Disc	All	Disc	All
N	132152	130142	122396	122027	122396	122027

Note: Table 2 presents the estimation of the causal effect of a two-month extension of the potential duration of UI on the time to re-employment (columns (1) and (2)), on the wage change (columns (3) and (4)), and on the hourly wage change (columns (5) and (6)). Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (including discontinuity fixed effects, see text). All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Columns (3) and (4) show the estimated impact of a two-month extension in the potential duration of UI on the change in wage between the pre-displacement and re-employment jobs (i.e., wage losses).[23] The results suggest that a two-month extension of the potential duration of UI has no significant impact on wage losses, even if the point estimates are slightly negative (-0.6 to -0.7 percent). This is true for the specification that only includes discontinuity fixed effects in column (3) and for that including all controls in column (4). In columns (5) and (6), we use an alternative

[22] Implementing a bandwidth longer than 90 days would imply that workers are located both to the right and to the left of the discontinuities. For instance, a worker with 640 days of working experience in the previous 6 years would be simultaneously located 100 days to the right of the 539-540 days discontinuity and 80 days to the left of the 719-720 days discontinuity.

[23] Table A.2 examines the effects of a two-month extension of the potential duration of UI on longer-term measures of daily re-employment wages. Specifically, we examine the effect on the change in wage when the re-employment wage is measured as a) the average re-employment wage during the first year after unemployment and b) the average re-employment wage during the first five years after unemployment. In general, we find similar conclusions.

measure of the wage change that, instead of daily wages, uses hourly wages.[24] Our estimates are negative but very close to zero (-0.2 to -0.3 percent) and insignificant, suggesting no effect of an extension of the potential duration of UI on hourly wage losses.[25]

In combination, our estimates of the effect of an extension of the potential duration of UI on non-employment durations and wages are consistent with the results in previous work. As shown in Lindner and Reizer (2020) there is a strong negative relation between the response of non-employment durations and that of wages to a change in UI benefits. The larger is the former, the smaller is the latter. Our estimates fit right into this linear relationship. Our findings are close to those in Schmieder et al. (2016), who find that an extension of the potential duration of UI has a strong positive effect on non-employment durations and a negative impact on re-employment wages. Other papers, such as Card et al. (2007b), Lalive (2007), van Ours and Vodopivec (2008), and Le Barbanchon (2016) find weaker positive responses of non-employment durations and, consequently, wage responses that are closer to zero. On the other hand, Nekoei and Weber (2017) find a very small positive effect on non-employment durations but a strong positive effect on wages, a result similar to that in Farooq et al. (2022) for the US.[26] Similarly, Lindner and Reizer (2020) exploit a policy change frontloading UI benefits to find a negative effect on non-employment durations and a strong positive effect on wages.

Validity: The validity of the RD results shown above relies on all other factors being continuous at the different thresholds. We test this assumption in three different ways. First, we test the balance of several observed covariates around the cutoff. Second, we check for manipulation of individuals around the discontinuity. Third, in Section 3.3, we restrict our sample to workers for whom we observe two or more UI claims and estimate our results after controlling for unobserved permanent individual heterogeneity.

First, we test the balance of observed covariates at the discontinuity. The results are shown in column (2) of Table A.4.[27] Our results indicate that workers in the margin of the discontinuity cutoffs have very similar characteristics regardless of whether they cross the cutoffs or not. We do not find any significant differences in any of the 10 different covariates tested. We summarize the results from the balance tests in Figure 2 (a). We follow Landais and Spinnewijn (2021) and use the RD design above to test the evolution of a covariate index around the policy discontinuities. The index is a linear combination of a vector of characteristics correlating with time to re-employment, including wealth, previous wage (hourly and daily), experience, tenure, gender, education (high school and college), age, part-time coefficient, and type of contract. The coefficients are obtained

[24]We define it as the difference in log hourly wages between the pre-displacement and re-employment jobs. We define the hourly wage in the re-employment job as the average hourly wage during the first month of the first re-employment job.

[25]Table A.3 presents equivalent results using an MSE optimal bandwidth as suggested in Calonico et al. (2020). Our conclusions remain unchanged.

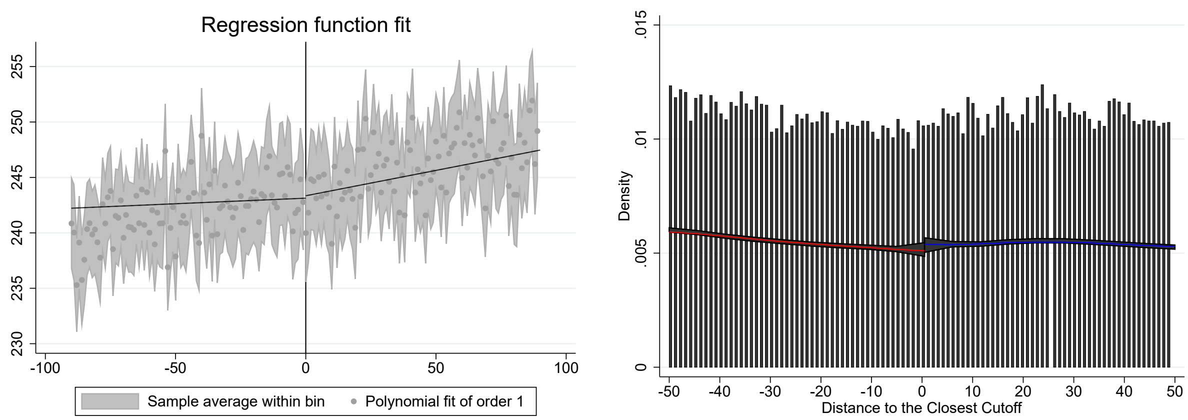
[26]Farooq et al. (2022) only analyze the effect of an extension of the potential duration of UI on re-employment wages but not its effect on non-employment durations.

[27]For additional robustness, in column (1) of Table A.4 we show the balance test for the MSE optimal bandwidth.

from a regression of the time to re-employment on these covariates, where the first five enter the regression linearly and the remaining non-parametrically. As shown in Figure 2 (a), we do not find any significant difference in the covariate index across the discontinuity.[28]

Second, we test for manipulation in the running variable around the thresholds. If workers can manipulate their prior working experience so that they receive two additional months of potential duration of UI, our estimates could be biased. We follow the work of Cattaneo et al. (2018) to test for manipulation. Figure 2(b) shows the density (relative to the closest discontinuity) overlapped by the point estimators and confidence intervals on both sides of the discontinuity. The manipulation test with optimal bandwidth shows a t-statistic of 0.78 (p-value=0.43) for the robust estimate. Therefore, we cannot reject that there is no manipulation.

Figure 2: Validity Tests



(a) Covariate Index Balance Test

(b) Manipulation Test

Note: Figure 2 shows two different validity tests. Panel (a) shows the balance test of the covariate index. The index is a linear combination of a vector of characteristics correlating with the duration of unemployment, including wealth, previous wage (hourly and daily), experience, tenure, gender, education (high school and college), age, part-time coefficient, and type of contract. The estimate shows insignificant differences in the covariate index at the discontinuity (Estimate = 0.217, SE = 0.705). Panel (b) shows the manipulation test. It presents the distribution around the closest policy discontinuity for all thresholds combined and adds the point estimates and confidence intervals of the manipulation test from Cattaneo et al. (2018). The bias-corrected robust estimate does not reject no manipulation (T = 0.86, p-value = 0.39).

3.2 The Causal Effect of the Exhaustion of UI Benefits on Re-employment Wages

Our previous results indicate that a two-month extension of the potential duration of UI has a small, negative, and insignificant impact on the wage losses between the pre-displacement and re-employment jobs. However, that an extension does not have a strong effect on wage losses on average does not necessarily imply that its effects throughout the entire distribution of time to re-employment are zero. For instance, it is possible that wage losses are less negative at each point of the distribution of time to re-employment for workers with an exogenous extension of the

[28]The exercise in 2 (a) uses a bandwidth of 90 days. We additionally test the covariate index balance using the MSE optimal bandwidth (in the last row of column (1) in Table A.4) and find insignificant differences at the discontinuity.

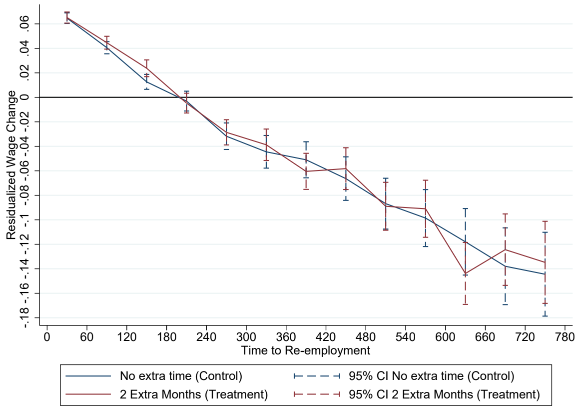
potential duration of UI, but on average, their wage losses are equivalent to those in the control group because their time to re-employment is also increasing.

To understand these dynamics, we start by plotting the evolution of the average residualized change in wage for our sample of workers in the treatment and control groups, separately, throughout the distribution of time to re-employment. This is shown in Figure 3 (a). The residualized wage change is generated from regressing the wage change on a vast array of observed characteristics of the worker and of the economy. As in the previous section, we define a worker as part of the treatment (control) group if her previous working experience is at most 90 days above (below) one of the policy thresholds. The evolution of wage losses is almost identical in the treatment and control groups, and we do not find significant differences at any point of the distribution. However, Figure 3 (a) combines workers with multiple potential durations in both the treatment and the control groups, which hides important differences in the evolution of wage losses, specifically around the point at which workers exhaust UI benefits.

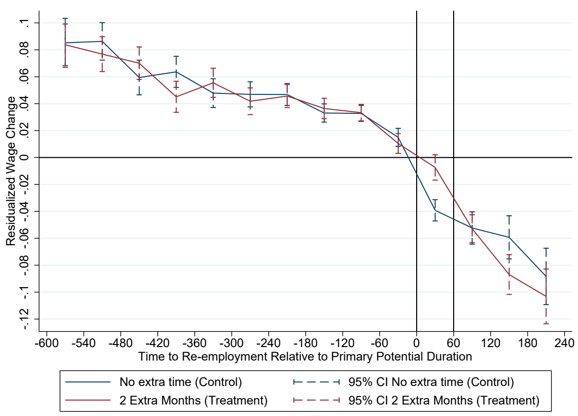
To highlight this, we define the variable "time to re-employment relative to primary potential duration." For workers in the control group, we define it as the difference between the time to re-employment and the potential duration of UI. For workers in the treatment group, we define it as the difference between time to re-employment and the potential duration of UI the worker would have had if she had not crossed the discontinuity, obtaining two extra months of potential duration of UI (i.e., her actual potential duration minus 60 days). Defining time to re-employment relative to the primary potential duration allows us to compare the evolution of wage losses in the treatment and control groups for workers with a very similar time to re-employment while highlighting how the wage losses evolve before, during, and after the point of expiration of UI benefits in each group.

Figure 3 (b) shows the evolution of the average residualized wage change against time to re-employment relative to the primary potential duration for workers in the treatment and control groups. Panel (b) shows that there are no significant differences in wage losses between workers in the treatment and control groups anywhere in the distribution except at the point at which the group with shorter potential duration has just exhausted their UI benefits while the treatment group still can collect UI benefits (i.e., from 0 to 60 on the x-axis). Within this range, the control group sees significantly larger wage losses that do not equalize across groups until those in the treatment group exhaust their UI benefits. This result is even more clear if we additionally control for time to re-employment when we generate the residualized wage change, as shown in Figure 3 (c). Starting at the point of exhaustion of UI benefits in the control group (x=0), workers in the control group see significantly larger wage losses that do not equalize with the losses in the treatment group until workers in the treatment group exhaust their benefits (x=60). During this period, the gap in wage losses between the treatment and control groups is approximately 3 percentage points. This evidence suggests that an extension of the potential duration of UI not only increases the time to re-employment but, conditional on time to re-employment, also impacts changes in re-employment

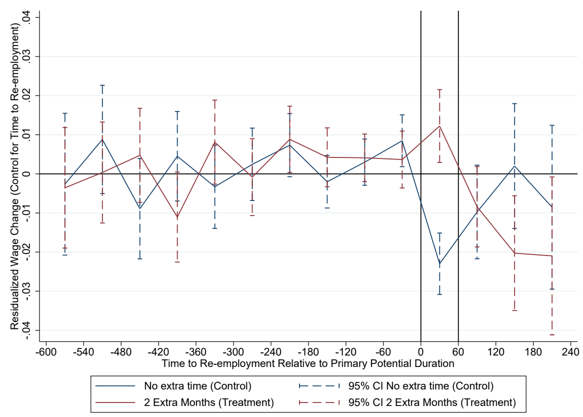
Figure 3: Wage Change Evolution over Time to Re-employment



(a) Residualized Wage Change over Time to Re-employment



(b) Residualized Wage Change over Time to Re-employment Relative to Primary Potential Duration



(c) Residualized Wage Change over Time to Re-employment Relative to Primary Potential Duration

Note: Figure 3 presents the evolution of the average residualized wage change for workers in the treatment and control groups. The residualized wage change is generated from regressing the wage change on a vast array of observed characteristics of the worker and of the economy. In panel (a), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment. In panel (b), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment relative to primary potential duration. In panel (c), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment relative to primary potential duration. Here, we additionally include time to re-employment as a control variable when residualizing the wage change. In all the panels, the average is calculated using a 60-day window. We analyze the workers whose tenure is close to a cutoff, such that whether they belong to the treatment group (receiving an additional two-months of potential duration of UI) or to the control group is close to randomization. The window we choose to classify a worker into either treatment or control group is 90 days from or past the discontinuity. The red (blue) line shows the residualized wage change path for workers with (without) two extra months of potential duration of UI.

wages, specifically in the time range in which workers would have already exhausted the UI benefits had they not received a two-month extension.

To estimate the effect of the exhaustion of UI benefits on wage changes, we propose an empirical strategy that combines our RD designs with a difference in differences empirical strategy based on the figure above. Our estimate of the causal effect of the exhaustion of UI benefits on wage losses will then be the difference in wage changes between treatment and control groups in the time window in which the control group has just exhausted their unemployment benefits and the treatment group would have exhausted their unemployment benefits had they not qualified for two extra months of

UI, relative to the difference between these two groups prior to this point in time.

$$y_i = \gamma_0 E_0 \times Treat_i + \gamma_{Post} E_{Post} \times Treat_i + \theta Treat_i + \beta_0^E \; E_0 + \beta_{Post}^E \; E_{Post} + \beta_b B_i + X_i \beta_X + \epsilon_i \quad (2)$$

In equation (2), y_i refers to the wage change between the pre-displacement and re-employment job of a worker i. $Treat_i$ takes the value 1 for workers in the treatment group and zero for those in the control group. As before, workers will belong to the treatment (control) group if they are located within 90 days to the right (left) of one of the discontinuities. E_0 and E_{Post} are defined based on the variable time to re-employment relative to primary potential duration. E_0 takes the value 1 if the time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60. For workers in the control group, it denotes whether the worker found a job within 60 days after the expiration of UI benefits. For workers in the treatment group, it denotes whether the worker found a job in the 60 days prior to the expiration of her UI benefits.[29] E_{Post} takes the value 1 if the time to re-employment relative to the primary potential duration is above 60. For workers in the control group, it denotes whether a worker's time to re-employment is at least 60 days after the expiration of UI benefits, while for workers in the treatment group, it denotes whether the worker's time to re-employment is after the expiration of UI benefits. Finally, B_i controls for discontinuity fixed effects, and X_i is a matrix that contains worker characteristics and economy-wide variables (age, gender, education, wealth, previous tenure, previous experience, type of contract, previous wage, part-time coefficient, location, and month-year fixed effects).

γ_0, associated with $E_0 \times Treat_i$, is our estimate of the causal impact of the exhaustion of UI benefits. More precisely, γ_0 captures the average effect on the change in wages for workers in the treatment group relative to those in the control group of exiting unemployment in a window starting the day after the control group exhausts their UI benefits and finishing 60 days after it (the point at which the treatment group will then be affected by the exhaustion of UI[30]), relative to the difference between the two groups prior to the expiration of benefits for the control group (captured by θ). We include E_{Post} as an additional control to ensure that our analysis is not confounded by differences in the evolution of wages between treatment and control once both groups have passed the exhaustion of UI benefits.

The key assumption of our empirical strategy is that there is no selection process that is specific to the exhaustion of UI benefits. This assumption rules out the case where some workers are re-employed right before (or after) the exhaustion of UI based on characteristics that also affect re-employment wages. This empirical specification allows for the dynamic selection of workers, in

[29]Table A.5 presents additional robustness results where we allow for some anticipation in the reaction to the exhaustion of UI benefits by moving forward the starting and end points of our estimates by 8 and 15 days. We show this in panels (A) and (B) of Table A.5, respectively. Slight variations in the start and end points do not have a strong impact on our estimates, which remain very similar to the main ones shown in the text.

[30]Later in this section we test the robustness of our difference in differences estimates to alternative definitions of the exhaustion window.

both observed and unobserved characteristics, that determines when they exit non-employment, as long as this selection is the same throughout the distribution of time to re-employment in the treatment and control groups. A different way to understand this assumption is that, conditional on the duration of non-employment, selection into re-employment is not a function of whether the worker reaches the exhaustion of UI benefits. Further in this section, we extensively test whether this assumption is likely to hold.

Table 3 presents different estimates of γ_0. In column (1), we do not include any additional covariates as controls, and the results suggest that passing the point of exhaustion of UI benefits exacerbates the daily wage losses by 2.6 percent. Our identification assumption is that there is no self-selection of workers specific to the exhaustion of UI benefits. In other words, if there is any selection on when a worker starts a new job, this selection is the same in our treatment and control groups and does not change differentially across groups when the control group reaches the expiration of UI benefits. This is especially relevant in our set-up because the time to re-employment is not exogenously determined, and incentives vary greatly around the exhaustion of UI benefits, making dynamic selection an important consideration. To account for the possible differences in observed characteristics, columns (2) and (5) of Table 3 include a vast array of controls in the estimation.[31] Even after accounting for differences in observed characteristics, we estimate that exhausting UI benefits deepens the daily wage losses by 3.2 percent and the hourly losses by 1.3 percent. For additional robustness, columns (3) and (6) additionally control non-parametrically for time to re-employment.[32] Our estimate remains very similar, at 3.5 percent for daily wages and 1.4 percent for hourly wages.[33]

The above specification compares the average wage losses of workers in the treatment and control groups, conditional on covariates, at the point at which the group with shorter potential duration exhausts their unemployment insurance benefits. However, a limitation of this specification is that it does not consider the distance to the RD threshold of different workers. Despite the short bandwidth that we impose to classify workers as part of the treatment or the control group, it is possible that the differences across groups in wage losses around the point of exhaustion of UI

[31] Later in this section, we provide evidence that predicted wage changes based on observed worker characteristics are not estimated to be different between treatment and control groups around the point of UI exhaustion.

[32] If we only had two potential duration groups (one as a control and one as treatment), controlling for time to re-employment would result in collinearity. However, our estimation includes workers with different potential durations of UI in both the treatment and the control groups. If the time to re-employment is shorter for some potential duration groups within the control group relative to the treatment group, this could create an imbalance. This means that for a given point of time to re-employment relative to primary potential duration, we could have an average time to re-employment that is different across treatment and control groups, which could explain the differences in re-employment wage changes we observe.

[33] Table A.6 presents additional results on the causal impact of the exhaustion of UI benefits on re-employment wages, where we examine the long-term effects of the expiration of UI on workers' re-employment outcomes. Specifically, we examine the effect of UI exhaustion on the average re-employment wage during the first year after unemployment and the average re-employment wage during the first five years after unemployment. The negative effect on re-employment wages of the expiration of UI benefits remains unchanged when we consider the average wage during the first re-employment year. However, after five years, the negative effect softens by around 25-30 percent.

benefits are driven by those workers within the treatment and control groups who are further from the discontinuity, and that at the threshold there are no differences between groups.

Table 3: Effect of the exhaustion of UI benefit. All discontinuities

	Daily Wage change			Hourly Wage change		
θ	0.000	0.001	0.000	-0.002	0.001	0.000
	[0.003]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
γ_0	0.026***	0.032***	0.035***	0.006	0.013**	0.014***
	[0.008]	[0.007]	[0.007]	[0.006]	[0.005]	[0.005]
γ_{Post}	-0.009*	-0.011**	-0.012***	-0.009*	-0.008*	-0.007*
	[0.005]	[0.005]	[0.004]	[0.005]	[0.004]	[0.004]
Controls	D	All	All	D	All	All
Time to Re-emp Ctrl	No	No	Yes	No	No	Yes
N	122396	122027	122027	122396	122027	122027

Note: Table 3 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) to (3) is the change in daily wages, while in columns (4) to (6) the outcome is the change in hourly wages. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

To test that our above specification is not providing biased estimates of the effect of the exhaustion of UI benefits on re-employment wage losses, we follow the methods in Schmieder et al. (2016) and Nekoei and Weber (2017) and estimate our original RD specification for the sample of workers whose time to re-employment falls in the period after the control group has exhausted their UI benefits but before the treatment group does. This allows us to focus on workers just around the discontinuity and isolate their response from the response coming from workers further away from the threshold. We present this result in column (2) of Table 4. Despite the small sample size, our RD estimate indicates that, at the discontinuity, wage losses are 2.6 percent larger for workers who just exhausted their UI benefits compared to workers who, having an almost identical time to re-employment, still have 60 more days of UI benefits. This result is significantly different from zero at the 5 percent confidence level. The conclusion is similar for hourly wage changes, shown in column (5) of Table 4. Our RD estimate indicates that, at the discontinuity, hourly wage losses are 1.4 percent larger for workers who have just exhausted their UI benefits compared to workers who, having an almost identical time to re-employment, still can collect UI benefits for 60 more days.

So far, we have focused our attention on the evolution of wage losses in the treatment and control groups around the point of exhaustion of UI benefits. However, panels (b) and (c) of Figure 3 also suggest that wage losses in the treatment and control groups at any other point (prior to the exhaustion of UI benefits in the control group and after the exhaustion of UI benefits in the treatment group) appear very similar. The remaining estimates displayed in Table 3, for both daily

Table 4: Effect of a Two-Month Extension of the Potential Duration of UI. All Discontinuities

	Time to Re-employment relative to Primary PD					
	≤ 0	> 0 and ≤ 60	> 60	≤ 0	> 0 and ≤ 60	> 60
	Daily Wage Change			Hourly Wage Change		
RD Estimate	0.000	0.026**	-0.005	0.002	0.014*	-0.007
	[0.005]	[0.013]	[0.011]	[0.004]	[0.009]	[0.007]
Controls	All	All	All	All	All	All
N	81961	11482	28584	81961	11482	28584

Note: Table 4 presents the estimation of the causal effect of a two-month extension of the potential duration of UI on the wage change for three different groups of workers. The outcome in columns (1) to (3) is the change in daily wages while in columns (4) to (6) it is the change in hourly wages. In column (1) we restrict the sample to workers whose time to re-employment is shorter than their potential duration of UI if they are part of the control group, or, if they are part of the treatment group, their time to re-employment is shorter than what would have been their potential duration of UI had they not crossed the discontinuity (i.e., time to re-employment relative to primary potential duration shorter than zero). In column (2) we restrict the sample to workers whose time to re-employment is between 1 day and 60 days longer than their potential duration of UI if they are part of the control group, or, if they are part of the treatment group, their time to re-employment is shorter than their potential duration of UI (i.e., time to re-employment relative to primary potential duration is larger than zero but smaller or equal to 60). In column (2) we restrict the sample to workers whose time to re-employment is more than 60 days longer than their potential duration of UI if they are part of the control group, or, if they are part of the treatment group, their time to re-employment is longer than their potential duration of UI (i.e., time to re-employment relative to primary potential duration larger than 60). Controls "All": All controls included (including discontinuity fixed effects; see text). All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

and hourly wage changes, confirm this intuition. Looking first at the coefficient θ, which captures the differences in wage changes between treatment and control groups prior to the expiration of UI benefits in the control group, all specifications show a very close to zero and insignificant coefficient. Moving to the period after the exhaustion of UI benefits for both groups, the coefficient γ_{Post} suggests that workers in the control group see slightly less negative changes in wages, although the magnitude is small (around 1 percent difference between groups for daily wage changes and 0.7 percent for hourly wage changes).[34]

To confirm that our difference in differences results pre- and post-benefit exhaustion are not driven by workers further away from the discontinuity, we rely again upon our original RD specification. First, we focus on the sample of workers whose time to re-employment occurs before the expiration of benefits in the control group (i.e., time to re-employment relative to primary potential duration is smaller than or equal to zero). Second, we focus on workers whose time to re-employment takes place after both groups have exhausted their UI benefits (i.e., time to re-employment relative to primary potential duration is larger than 60). The results for daily wages are shown in columns (1) and (3) of Table 4, while the results for hourly wages are displayed in columns (4) and (6). The estimates for both sub-samples are insignificantly different from zero for both daily and hourly wage changes. Therefore, both the difference in differences and RD results indicate that wage losses differ between treatment and control groups at the point of exhaustion of benefits only in the control group. Before that point and after it, wage losses are identical across groups.

For additional robustness, we further decompose the period prior to the exhaustion of benefits for workers in the control group and the period after the expiration of UI benefits in the treatment

[34] As we show below, once we use the RD empirical strategy instead of the difference in differences this negative effect disappears. Furthermore, in our robustness exercises displayed later in this section, we show that once we control by individual fixed effects, this estimate becomes positive and insignificant, while all the remaining estimates in Table 4 remain unchanged.

group in smaller time intervals. This allows us to more accurately fix the time to re-employment within each sub-sample when estimating our difference in differences and RD empirical strategies. The results for the difference in differences and RD specifications are shown in Figure 4(a) and (c), respectively. At no other period other than at the exhaustion of UI benefits in the control group (but prior to the exhaustion of UI benefits for the treatment group) do we consistently find significant differences across groups in wage losses. Importantly, comparing these results to those in Figure 4(b) and (d), where the outcome is the predicted wage change based on observed worker and economy-wide pre-displacement characteristics, we see that workers in the treatment and control groups showed no differences in their predicted wage losses at any point of the distribution. This includes the point after the exhaustion of UI benefits in the control group but before the treatment group exhausts theirs.

3.3 Robustness: Dynamic Selection on Permanent Unobserved Heterogeneity

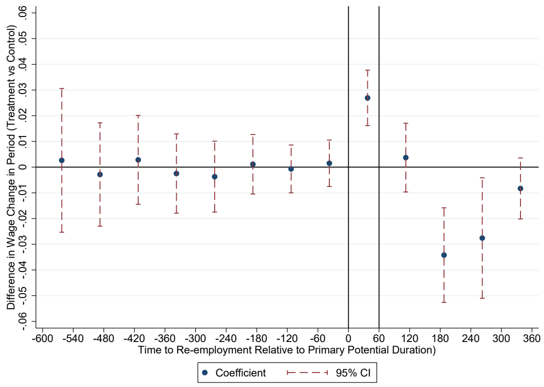
The results above strongly suggest that workers' non-employment duration significantly increases after an extension of the potential duration of UI, while their re-employment wage losses are unaffected. Furthermore, we also find robust evidence that workers react to the value of the outside option. Compared to workers exogenously granted an additional two months of UI, workers with a shorter potential duration of UI but identical time in non-employment see significantly larger wage losses when their UI benefits expire but see identical wage losses at every other point in time.

However, even after the different validity tests we display above, it is possible that workers are manipulating their previous experience in ways that remain unobserved in the data, which would invalidate our RD design. Furthermore, even if there is no manipulation, our analysis of the evolution of wage losses for workers with exogenously shorter and longer potential durations of UI along the distribution of time to re-employment assumes that dynamic selection is identical in both groups. This closes the door to patterns of selection that depend on the duration of UI benefits. It is possible that the type of workers leaving non-employment in the treatment and control groups after the former reaches the exhaustion of benefits (but before the latter does) differs in unobserved characteristics that could affect their re-employment wages (i.e., dynamic selection could be different between treatment and control groups around this point).

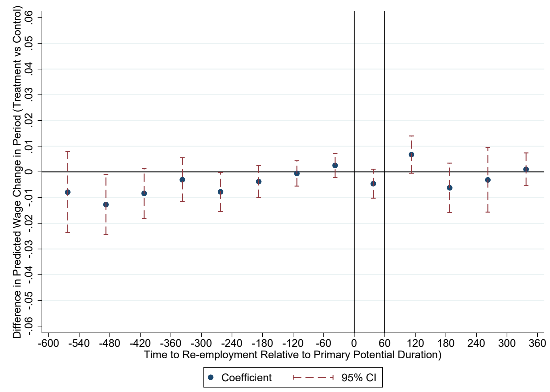
Fortunately, our data are extensive enough that we observe more than one entry into unemployment for a relatively large sample of workers. This allows us to re-estimate our previous exercises for a sample of workers for whom we observe two or more UI claims, enabling us to control for permanent unobserved heterogeneity in all our estimates.

We start by replicating our RD results for this sample of workers in Table A.7. Column (1) restricts our sample to workers with two or more UI claims but does not include individual fixed effects, while column (2) includes them. Our results in column (1) strongly indicate that the

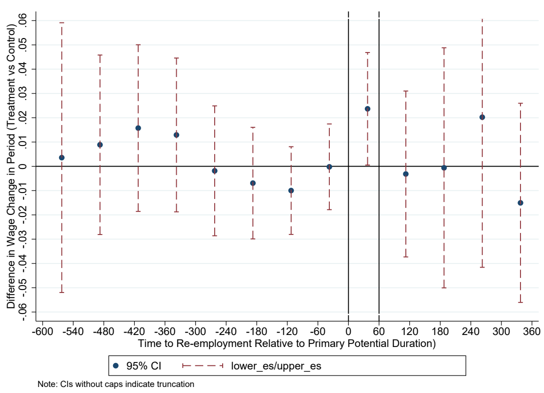
Figure 4: Period by Period Differences in the Evolution of Wage Changes



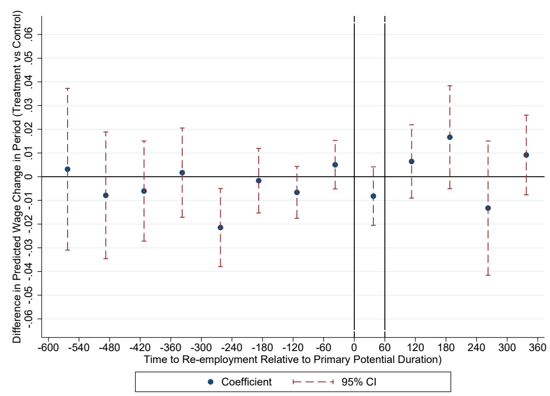
(a) Wage Change (DiD)



(b) Predicted Wage Change (DiD)



(c) Wage Change (RD)



(d) Predicted Wage Change (RD)

Note: Figure 4(a) presents the estimated coefficients of an extension of equation (2) where we divide the period prior to the exhaustion of UI benefits in the control group, and the period after the exhaustion of UI benefits in the treatment group in multiple 60-day periods. Each coefficient represents the difference in wage changes between treatment and control groups in the corresponding 60-day period of the time to re-employment relative to primary potential duration distribution. We analyze the workers whose tenure is close to a cutoff, such that whether they belong to the treatment group (receiving an additional two-months potential duration of UI) or the control group is close to randomization. The window we choose to classify a worker into either treatment or control group is 90 days from or past the discontinuity. Figure 4(b) is equivalent to Figure 4 (a) but replaces the outcome variable with the predicted wage change from a regression on pre-displacement worker and economy-wide characteristics. Figure 4 (c) presents the estimated coefficients of separate RD regressions in the form of equation (1) that include all control variables and discontinuity fixed effects (see text). In each regression, the sample comprises workers whose time to re-employment relative to primary potential duration is within the corresponding 60-day window. The RD estimates are conventional estimates as shown in Calonico et al. (2014) using a polynomial of degree one, triangular kernel, and a bandwidth of 90 days. Figure 4(d) is equivalent to Figure 4 (c) but replaces the outcome variable in each RD regression with the predicted wage change from a regression on pre-displacement worker and economy-wide characteristics. The 95 percent confidence intervals are constructed from robust standard errors.

response of workers with two or more UI claims to a two-month extension of the potential duration of UI is not significantly different from that of our complete sample. In our restricted sample, workers entitled to collect UI benefits for two additional months increase their non-employment duration by 36 days. This estimate is significantly different from zero at the 1 percent confidence level. Controlling for unobserved permanent heterogeneity in column (2) (i.e., adding individual fixed effects to our estimation) does not change our conclusions.[35] These results suggest that there is no manipulation of the running variable based on unobserved permanent characteristics, a fact

[35] We show the balance test for this sample, with and without individual fixed effects, in columns (3) and (4) of Table A.4. We do not find evidence of an imbalance in observed characteristics across workers close to but on different sides of the discontinuities.

that strengthens the credibility of our main estimates.

We next move on to analyzing the evolution of wage losses across the distribution of time to re-employment. The results of our difference in differences empirical strategy in equation (2) for this sub-sample are shown in Table A.8. Compared to our main results, our conclusions remain unchanged. Columns (1) and (2) (respectively (5) and (6) for hourly wage losses) do not use individual fixed effects, while columns (3) and (4) (respectively (7) and (8) for hourly wage changes) do. The results show that compared to our complete sample, our sample of workers with two or more UI claims sees an extremely similar increase in wage losses around the point of exhaustion of UI benefits (3 percent in the restricted sample vs. 3.5 percent in the complete sample). Furthermore, even after we control for unobserved individual heterogeneity, our results indicate that exhausting the UI benefits exacerbates the wage losses by 2.5 to 2.7 percent. This result strongly suggests that differential dynamic selection in the treatment and control groups around the point of exhaustion of UI benefits based on permanent unobserved worker characteristics does not appear to be a concern in our sample. We see this as further evidence that the recovered estimates in the previous section represent the causal impact of the exhaustion of UI benefits on re-employment wages.

Similarly, when we compare the wage losses in the treatment and control groups at any other point in time other than when the control group exhausts their UI benefits but before the treatment group does, we find no significant differences. The remaining estimates (θ and γ_{Post}) displayed in Table A.8 for both daily and hourly wage changes show this. The difference in wage losses between treatment and control groups prior to the expiration of UI benefits in the control group, and after both groups have exhausted their benefits, remains insignificant even after controlling for individual fixed effects. In combination with the results in the previous paragraph, these results suggest that dynamic selection based on permanent unobserved characteristics appears unimportant in explaining the evolution of wage losses over time, including at the point of exhaustion of UI benefits.

In summary, the results in this section strongly indicate that selection into treatment based on unobserved permanent heterogeneity is not a concern in our RD empirical strategy. More importantly, the concern that dynamic selection based on fixed unobserved worker characteristics could be behind the differences (or lack thereof) in wage losses across treatment and control groups across the distribution of time to re-employment also appears unfounded.

4 A Conceptual Model

This section presents an illustrative framework that helps us connect the impact on wages of the exhaustion of UI benefits and that of the extension of the potential duration of UI with the channels behind the negative relationship between re-employment wages and the duration of non-employment. Here, we present an outline of the main theoretical results of interest. The detailed model and derivations are shown in Appendix B.

Consider a shortcut to the solution of the optimal wage path of an unemployed worker:

$$ln(w_t) = g(\mathcal{B}(t), \theta) + \rho_h t + \rho_0 + \epsilon_i \tag{3}$$

where $g(\cdot, \theta)$ represents how the value of non-employment affects the wage determination given the structural parameter θ, $\rho_h < 0$ represents the duration dependence rate (that encompasses human capital depreciation, adverse signaling, declining matching efficiency, etc.), and ϵ_{it} represents unobserved factors.

In $g(\cdot, \theta)$, workers only care about the future available benefits, $\mathcal{B}(t) = \{b_t, b_{t+1}, \cdots\}$, representing the future available unemployment insurance or unemployment assistance (UA) benefits. This is especially the feature of hand-to-mouth unemployed workers in that their past income history does not affect their future search decisions. θ denotes the structural parameters, including preference parameters (discount factor β, utility function), search technology parameters, and the state of the economy. We specify $b_t = \bar{b}$ for the period during which the worker collects unemployment insurance benefits ($t < B$) and $b_t = \underline{b}$ later on, once the worker runs out of UI benefits and starts collecting unemployment assistance.[36] Given this structure, g can be essentially collapsed into a function of limited dimensions in (B, t, θ).[37] Furthermore, when $t \geq B$, $g(B, t, \theta)$ is a constant and denoted by g^*. To clarify our exposition, let us consider a prototype parametric example of g that uses the search technology function specified by Nekoei and Weber (2017) (see Appendix B.1 for further details):

$$g(\mathcal{B}(t), \theta) = (1 - \beta) \left(u(b_t) + \sum_{j=1}^{\infty} \beta^j \left(u(b_{t+j}) - e^*_{t+j} \right) \right) \tag{4}$$

In equation (4), θ includes the discount factor β and the static utility function u. e^* denotes the optimal search effort. Thus, a natural implication is that a higher flow of unemployment benefits will increase the optimal wage choice (or reservation wage).

This shortcut representation of the optimal wage path is an extended counterpart of the expression proposed by Schmieder et al. (2016). As shown in Appendix B.1, it takes its micro-model foundation from general directed job search models under two important assumptions. First, it assumes that duration dependence is a continuous function of time in unemployment. This implies

[36] Unemployment assistance benefits start once the standard unemployment insurance benefits expire. The duration and amount of unemployment assistance benefits will depend on different household and individual characteristics. Nevertheless, UA benefits are always lower than UI benefits. We assume that workers can collect unemployment assistance benefits forever. While this is not true, workers without a job and under a certain level of income are still able to collect benefits from other programs once their unemployment assistance benefits expire. All these programs aim to provide a subsistence level of income. Therefore, whether the worker is collecting UA benefits or benefits from some other program after she has exhausted her UI benefits, the amount of benefits received per period does not vary greatly.

[37] We don't consider the variation in $\bar{b}$ and $\underline{b}$ so it is dropped from the function as an argument.

that the only reason why re-employment wages can decline discretely at the point of exhaustion of UI benefits is that reservation wages discretely decline at that point by an equivalent amount. Second, it assumes the separability between the value of the non-employment and the duration dependence. This assumption is weaker than our first one, and we use it to facilitate our exposition. Moreover, even if this assumption were not satisfied, our shortcut representation of the optimal wage path would still represent a first-order approximation to the real optimal wage path as long as the complementarity between the value of non-employment and the duration dependence is not too strong.

With this optimal wage path set-up, we can now think about how the expected wage of a worker changes in response to an extension of UI:

$$\frac{dEln(w^*)}{dB} = \sum_0^\infty \left(\frac{\partial ln(w_t)}{\partial B}\mathrm{pr}(t)\right) + \sum_0^\infty \left(ln(w_t)\frac{\partial \mathrm{pr}(t)}{\partial B}\right) \tag{5}$$

Equation (5), which holds by the definition of the total derivative and does not rely on a specific model, illustrates the decomposition of the impact of an extension of the potential duration of UI on expected wages into the sum of two offsetting channels. First, in response to an extension of the potential duration of UI, workers increase their optimal wage choice (and that translates into re-employment wage changes) at each point in time, since they now enjoy a higher value of unemployment.[38] This effect, when positive, refers to what we call a "binding reservation wage." Even though there is no reservation wage in this framework, we refer to it as such to remain consistent with Schmieder et al. (2016). Second, an extension of the potential duration of UI lowers the relative price of unemployment, increasing time in unemployment. This reduces future wages due to either negative duration dependence or by potentially pushing these workers to find a job past the point at which UI benefits have expired. The two channels work in an offsetting way, such that the final effect on wages of an extension of the potential duration of UI can take a positive or a negative sign.

If $\frac{\partial ln(w_t)}{\partial B} = 0$, i.e., the reservation wage is not binding, and the combination of equations (3) and (5) degenerates to:

$$\frac{dEln(w^*)}{dB} = \rho_h \cdot \frac{dED_i}{dB} \tag{6}$$

Thus, when the reservation wage is not binding, the comparison between the causal effect of an extension of the potential duration of UI on re-employment wages and on the duration of non-employment can identify duration dependence. This logic motivates Schmieder et al. (2016) to construct an IV estimator $IV_h \equiv \frac{dEln(w^*)}{dB} / \cdot \frac{dED_i}{dB}$ for duration dependence.

However, two empirical findings make the assumption of a non-binding reservation wage unreasonable in our data: First, in the empirical estimation in Section 3, we find a negative but

[38] The reaction here is consistent with the wage bargaining process in Jäger et al. (2020)

insignificant effect of an extension of the potential duration of UI on wage losses. This implies that, in our case, the two channels described above almost fully cancel each other out. Since we find that an extension of the potential duration of UI also generates a large positive effect on the duration of non-employment, for both channels to cancel each other, it has to be that either duration dependence is insignificantly different from zero or that reservation wages are binding. Moreover, this situation is not exclusive to our paper. A number of previous studies find similar evidence of a non-significant (or a positive and significant) wage effect and a significant duration effect(Card et al. (2007b), van Ours and Vodopivec (2008), and Nekoei and Weber (2017)), highlighting that a binding reservation wage is not uncommon.

Second, our empirical evidence indicates that wages respond to the expiration of UI benefits but do not respond at any other duration of non-employment. Based on the theoretical results in Krueger and Mueller (2016), this implies that the decline in re-employment wages when UI is exhausted is not only equivalent to the response of the wage choice to the exhaustion of UI benefits but also equivalent to the response of the wages choice to an extension of the potential duration of UI.[39].

This motivates us to derive a new estimator for duration dependence, taking into account that reservation wages are binding. Combing (3), (5), and the properties of $g(\cdot,\theta)$, we can decompose the causal impact of an extension of UI benefits on the expected wage into three components:

$$\frac{dEln(w^*)}{dB} = \rho_h \frac{dED^*}{dB} + \underbrace{\frac{\partial g(B,t,\theta)}{\partial B}\text{pr}(B)}_{\text{Binding Reservation Wage}} \underbrace{+\sum_{t<B}\frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta)-g^*)+\sum_{t<B}\frac{\partial g(B,t,\theta)}{\partial B}\text{pr}(t)}_{\text{Anticipation Effect}} \tag{7}$$

The first component captures duration dependence, $\rho_h \frac{dED^*}{dB} < 0$. The second component, the binding reservation wage effect $\frac{\partial g(B,t,\theta)}{\partial B}\text{pr}(B)$, captures the effect on reservation wages of an extension of the potential duration of UI just at the point at which the original benefits would have ended, weighted by the probability of reaching that point. The final component, the anticipation effect, is the sum of two pieces. The first piece, $\sum_{t<B}\frac{\partial g(B,t,\theta)}{\partial B}\text{pr}(t)$ represents the response of the worker's wage choice in each period prior to the exhaustion of UI benefits to an extension of the potential duration of UI, weighted by the period's probability of re-employment. The second piece, $\sum_{t<B}\frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta)-g^*)$ arises because an extension of the potential duration of UI decreases the hazard rate earlier in the unemployment spell, making workers more likely to reach the original point of exhaustion of UI benefits (i.e., the point at which workers would have exhausted the UI benefits had they not been granted a two-month extension of the potential duration of UI). Reaching the point of exhaustion of UI benefits has a negative impact on the worker's re-employment wage; therefore, if workers are more likely to reach it under a UI extension, not accounting for this

[39]In Krueger and Mueller (2016) this is the wage response to permanently removing UI benefits

term would overestimate duration dependence.[40]

Therefore, as implied by equation (7), the LMOS estimator of duration dependence is:

$$LMOS \equiv \rho_h = (\frac{dED^*}{dB})^{-1} \left(\frac{dEln(w^*)}{dB} - \text{Binding reservation wage} + \text{Anticipation effect} \right) \quad (8)$$

Compared with the duration dependence IV estimator in Schmieder et al. (2016), which applies when reservation wages do not bind, the LMOS duration dependence estimator will diverge drastically from IV if the direct wage effect and the anticipation effect are important. Moreover, when the anticipation effect is close to zero (which could reflect myopic job seekers), but the direct wage effect is sizable, the IV estimator will not represent a lower bound of the duration dependence effect because it will be biased toward zero. This will be our case, as we show below.

5 Estimating Duration Dependence

Estimating duration dependence using the LMOS estimator requires us to estimate the binding reservation wage effect and the anticipation effect. We focus first on the estimation of the binding reservation wage effect.

5.1 Binding Reservation Wage Effect

The binding reservation wage effect exactly equals the causal effect of the exhaustion of UI benefits on wage changes. Conceptually, when duration dependence is assumed to be a smooth process throughout the duration of non-employment, the decline in wages in response to the exhaustion of UI benefits is equivalent to the elasticity of the wage choice to an extension of UI. We formalize this equivalence in Appendix B.2. Based on this equivalence, the empirical results in Section 3 provide us with an estimate of the binding reservation wage effect. From Table 3, this is 3.5 percent.

5.2 Anticipation Effect

The main takeaway of the estimation of the anticipation effect is that, empirically, it is close to zero. When compared to the importance of the binding reservation wage effect in determining the LMOS estimator for duration dependence, the anticipation effect is almost irrelevant. To see this, note that the numerator of the anticipation effect is the sum of two pieces.

[40]Note that an extension of the potential duration of UI can make workers more likely to reach the original point of exhaustion of UI benefits while at the same time making workers less likely to exhaust their UI benefits (since their point of exhaustion of UI benefits is now later). However, the key here is whether workers are more likely to reach the point at which their benefits would have ended in the absence of the extension because the effects for the period starting at the original UI exhaustion point and finishing at the new UI exhaustion point are captured as part of the direct effect.

The first piece is $\sum_{t<B} \frac{\partial g(B,t,\theta)}{\partial B} \mathrm{pr}(t)$. As discussed above, it represents the response of the worker's wage choice in each period prior to the exhaustion of UI benefits to an extension of the potential duration of UI. Our previous empirical results strongly suggest that this term is zero. To see this, note that our estimates in panels (a) and (c) of Figure 4 indicate that wage losses evolve identically for workers in the treatment and control groups for all the periods prior to the exhaustion of UI benefits. This is a consistent result of both the DiD empirical strategy and the RD strategy. Furthermore, both empirical strategies also indicate that predicted re-employment wages at each point of the distribution are the same in the treatment and control groups (panels (b) and (d) of Figure 4). In combination, both pieces of evidence strongly suggest that equality in wage choice, and not differential patterns of dynamic selection between the treatment and control groups in the periods prior to the exhaustion of UI benefits, is the reason why wage losses are identical between both groups, making this first piece in our estimation zero.

The second piece is $\sum_{t<B} \frac{\partial \mathrm{pr}(t)}{\partial B}(g(B,t,\theta)-g^*)$. It captures the negative response of the hazard rate early in the spell to an extension of the potential duration of UI. As discussed above, this response makes workers more likely to reach the original point of exhaustion of UI benefits (i.e. the point at which workers would have exhausted the UI benefits had they not been granted a two-month extension of the potential duration of UI), what has a negative impact on the wage choice. Not accounting for this term would overestimate duration dependence.[41]

Note that the channel through which this effect affects re-employment wages is the same as the channel through which the duration dependence does: by extending the time to re-employment. However, the root cause of each mechanism is different. While duration dependence exists independently from the UI benefits, this effect only exists because the worker's wage choice responds to the existence of UI benefits. If re-employment wages did not respond to UI benefits (i.e., if reservation wages were not binding), this effect would disappear. By increasing the time to re-employment, an extension of the potential duration of UI affects re-employment wages not only through a pure duration dependence but also through an increased likelihood of reaching the original UI exhaustion point, after which the workers' wage choice will decrease.[42]

The relevance of this channel depends on the difference in the hazard rates between treated and control units in all periods prior to the exhaustion of UI benefits. If they are identical, this channel will be irrelevant. On the other hand, if the hazard rate strongly responds to an extension of the potential duration of UI from the start of the non-employment spell, its relevance will be much

[41] Note that an extension of the potential duration of UI can make workers more likely to reach the original point of exhaustion of UI benefits while at the same time making workers less likely to exhaust their UI benefits (since their point of exhaustion of UI benefits is now later). However, the key here is whether workers are more likely to reach the point at which their benefits would have ended in the absence of the extension because the effects for the period starting at the original UI exhaustion point and finishing at the new UI exhaustion point are captured as part of the binding reservation wage effect.

[42] Again, reservation wages will decrease after these workers reach their point of exhaustion of UI benefits. However, the effects arising in the period starting at the original UI exhaustion point and finishing at the new UI exhaustion point are captured as part of the binding reservation wage effect. For more detail, see the previous footnote.

larger.

Figure 5 (a) plots the estimated difference in the hazard rate between the treatment and control groups at each point of the time to re-employment relative to the primary potential duration distribution. The period-by-period difference in hazard rates is zero at the start, but over the last few periods before the original point of exhaustion of UI benefits, this difference becomes negative and significantly different from zero. The conclusions are similar when looking at the cumulative unconditional probability of re-employment, shown in Figure 5 (b). By the time the control group reaches the expiration of UI benefits, the cumulative re-employment probability of workers in the treatment group is 3.7 percentage points lower than for workers in the control group. Differential dynamic selection across groups appears to play no role in explaining these differences in re-employment probabilities. This is evidenced in panels (c) and (d) of Figure 5. The difference between the predicted hazard rates (and the predicted cumulative hazard rates) across groups is estimated to be insignificantly different from zero in all periods of interest.

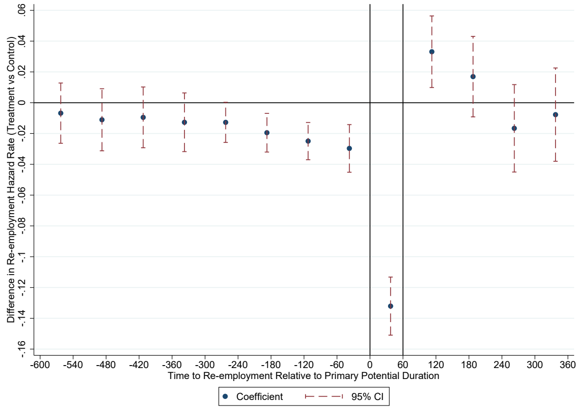
Note that under the assumption that $g(B,t,\theta) - g^*$ is constant for all t smaller than B (more on this below), we can express our term of interest as: $(g(B,t,\theta) - g^*) \cdot \frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$. From Figure 5 (b) we can extract an estimate of $\frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$. This is 3.7 percent.

All that is left now is to estimate $g(B,t,\theta) - g^*$ and assess whether assuming that it is constant for all t smaller than B is reasonable. $g(B,t,\theta) - g^*$ represents how wage losses would change in period t in the absence of UI benefits. Suppose first that $g(B,t,\theta) - g^*$ is constant for all t smaller than B. Since we lack the type of variation that would allow us to provide a causal estimate of its value, our strategy here is to approximate it based on the moments we can recover in our data.

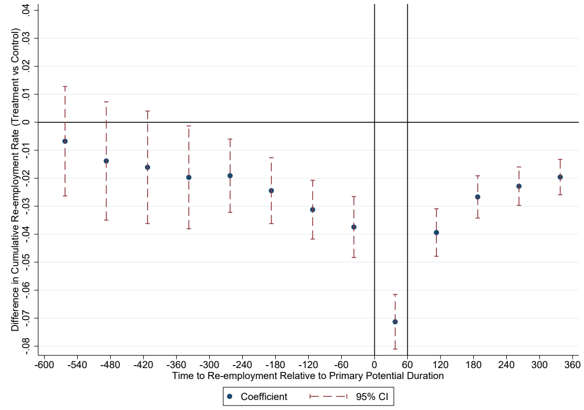
Our preferred approximation of $g(B,t,\theta) - g^*$ comes from the causal effect of the exhaustion of the UI benefits on wage losses. From Table 3, this estimate is 3.5 percent. The assumption here is that the counterfactual wage change we would observe in period t in the absence of UI benefits would decrease by the same amount as we estimate it does at the point of exhaustion of UI benefits. To test how reasonable this assumption is, we take advantage of the fact that our sample combines workers with multiple different potential durations. This allows us to estimate the causal impact of the exhaustion of UI benefits for multiple different groups of workers, using our difference in differences strategy. The key is that by estimating the effect for each discontinuity, we take advantage of the variation in our sample on how long workers have been unemployed once they face the point of exhaustion of UI benefits. For the first discontinuity, the time is only 4 months, while for the last it is 22 months. Finding that the estimated effect of the exhaustion of UI benefits on wage losses is not significantly different across discontinuities would provide support for our assumption that the counterfactual wage loss we would observe in period t in the absence of UI benefits would be the same as the one we estimate at the point of exhaustion of UI benefits.

The results are shown in Figure A.3. We do not find significant differences in the causal impact of the exhaustion of UI benefits on wage losses across discontinuities, even if the point estimates move

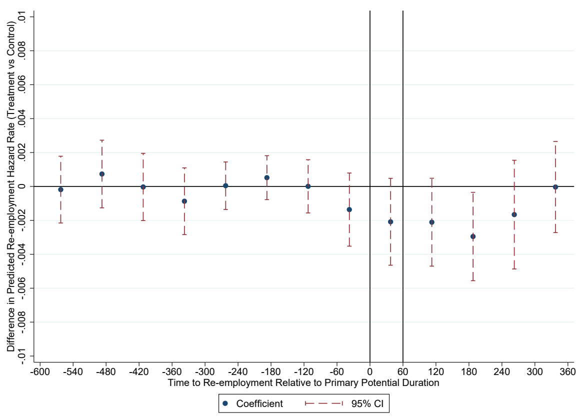
Figure 5: Differences in Re-employment Rates



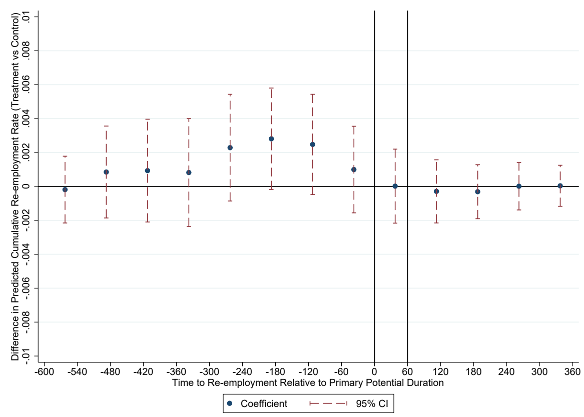
(a) Hazard Rate



(b) Cumulative Hazard Rate



(c) Predicted Hazard Rate



(d) Predicted Cumulative Hazard Rate

Note: Figure 5 (a) presents the estimated coefficients of separate RD regressions in the form of equation (1), including all control variables and discontinuity fixed effects (see text). The outcome in each regression is a dummy variable taking the value of one if the worker is re-employed within that period and the value of zero if she is not (conditional on not being re-employed prior to that period). Figure 5 (b) is equivalent to panel (a) but replaces the outcome variable with a dummy variable taking the value of one if the worker is re-employed within or prior to that period and the value of zero if she is not. Figure 5 (c) and (d) are equivalent to Figure 5 (a) and (b), respectively, but replace the outcome variable with the predicted probability of re-employment from a regression on pre-displacement worker and economy-wide characteristics. The RD estimates are conventional estimates as shown in Calonico et al. (2014) using a polynomial of degree one, triangular kernel, and a bandwidth of 90 days. The 95 percent confidence intervals are constructed from robust standard errors.

toward somewhat higher values after the third discontinuity. This suggests that our assumption that the counterfactual wage loss we would observe in period t in the absence of UI benefits would be equal to that at the point of exhaustion of UI benefits appears reasonable.[43] Furthermore, the results in Figure A.3 also support the assumption that we used to start this exercise: that $\Delta ln(\mathbf{w}(\tau))$ is constant for all t smaller than B. The fact that we do not find significant differences in the effect on wage changes of the exhaustion of UI benefits for workers with different unemployment durations

[43]Suppose we were to take the increase of the point estimates over the distribution of discontinuities in Figure A.3 as evidence that the effect of the exhaustion of UI benefits gets worse the longer the non-employment duration (even if the difference between estimates is insignificant). Then, using the aggregated causal impact of the exhaustion of UI benefits as our approximation would result in an upwardly biased estimate of the real $g(B, t, \theta) - g^*$. This would imply that the anticipation effect is even less relevant for determining duration dependence.

suggests that assuming that $g(B,t,\theta) - g^*$ does not vary over $g(B,t,\theta) - g^*$ could be reasonable.

In summary, of the two different terms that compose the numerator of the anticipation effect, one is zero, and our best approximation of the second one is 0.0013 (0.037*0.035). Therefore, accounting for this effect in our LMOS estimator of duration dependence will have an extremely limited impact on our conclusions. In our results later in this section, we present the estimates of duration dependence including and excluding this term, to make clear its limited relevance and the interpretation of this limited difference from a theoretical perspective.

With the anticipation effect and the binding reservation wage effect pinned down, the LMOS estimator of duration dependence only requires us to additionally estimate the effects of an extension of the potential duration of UI on wage losses and on time to re-employment. How we recover these causal effects is already shown in Section 3.

5.3 Results on Duration Dependence

The estimated duration dependence using the LMOS estimator is shown in the first row of Table 5. Our findings suggest that one additional month of non-employment increases the daily wage losses by 0.8 percent per month. This result is significantly different from zero at the 95 percent confidence level and is equivalent to an additional wage loss of slightly less than 10 percent for one additional year of unemployment duration.

Table 5: Duration Dependence (LMOS) vs
IV Estimates from Schmieder and von Wachter (2016)

	Daily Wage Change	Hourly Wage Change
LMOS	-0.0080**	-0.0024
90% CI	[-0.0157,-0.0007]	[-0.0085, 0.0038]
LMOS (Ignoring Anticipation Effect)	-0.0097**	-0.0030
90% CI	[-0.0175,-0.0021]	[-0.0090, 0.0033]
IV (Schmieder and von Wachter (2016))	-0.0069	-0.0020
90% CI	[-0.0142, 0.0005]	[-0.0079, 0.0044]
Ratio LMOS vs. IV	116%	120%
Controls	All	All
N	122027	122027

Note: Table 5 presents in row (1) the estimated (monthly) duration dependence (LMOS estimator) in daily re-employment wage (column (1)) and in hourly re-employment wage (column (2)). In row (2) we show an estimate of duration dependence (LMOS estimator without anticipation effects) that ignores the anticipation effects. Row (3) shows the IV estimate proposed in Schmieder and von Wachter (2016). Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Bootstrapped 90 percent confidence intervals in brackets. The sample is bootstrapped at the spell level. p-value: * 0.10 ** 0.05, *** 0.01.

As discussed above, the anticipation effects are of very limited relevance. This can be observed by comparing the results in the first and second rows of Table 5. In the latter, we ignore these anticipation effects and find estimates of duration dependence that are only around 0.15 percent more negative (-0.97 percent per month of non-employment). Note that our main estimate of the LMOS assumes that the response of the wage choice in the periods prior to the exhaustion of UI benefits is the same as the one we observe at the point of exhaustion of benefits. We understand this approximation as an upper bound of how much the choice of wage could respond in these periods. If the response during these periods were smaller,[44] the real LMOS estimate that includes the anticipation effects would be more negative.[45] Based on our model, this suggests that workers behave as if their β is significantly lower than one, showing a strong degree of impatience or myopia. This is not an uncommon result in this literature, something previously highlighted in Ganong and Noel (2019) or DellaVigna et al. (2022).

How do these estimates compare to those in the prior literature? The closest point of comparison comes from Schmieder et al. (2016). Their proposed IV estimator, using German data, suggests that one additional month of non-employment decreases re-employment wages by 1 percent. However, there are a few major differences between their work and our paper. First, the IV estimator is only suited to cases in which the reservation wage is not binding. As we discussed at length above, this is not the case in our data. Using the IV estimator in our case would result in an underestimation of the degree of duration dependence. This is shown in Table 5, where we compare the LMOS estimates (row 1) versus those using the IV estimator (row 3). We find that the IV estimator underestimates the deterioration rate of labor market opportunities by 15 to 20 percent.

Second, Schmieder et al. (2016) argue that their IV estimate is a lower bound for the causal effect of non-employment duration on re-employment wages when reservation wages are slightly binding. While this is true under their assumptions, in our case, these assumptions do not hold. Given how reservation wages bind in our data (for the two months after the original point of exhaustion of UI benefits, workers enjoy reservation wages that are around 3 percent higher under an extension of UI), the IV estimate captures not only the causal effect of the duration of non-employment on re-employment wages but also the effect of a higher wage choice. This can be seen comparing rows (2) and (3) in Table 5. While the anticipation effect works to offset this difference[46] (because the re-employment probability prior to the expiration of UI benefits decreases driven by the extension of the UI), its effect is not strong enough. Therefore, if we are interested in estimating duration dependence net of the effect of the exhaustion of UI benefits on re-employment wages (our LMOS estimator), the IV estimate no longer acts as a lower bound.

An important part of the degree of duration dependence in daily wages appears to be driven by workers' inability to find full-time jobs as the duration of non-employment increases. When we ignore hours worked and focus only on hourly wages, the estimated degree of duration dependence becomes significantly weaker. This can be seen in column (2) of Table 5, where we estimate duration dependence by focusing on changes in hourly wages instead of the changes in daily wages. The estimated duration dependence in hourly wages is only 0.25 percent per month of unemployment,

[44]The evidence above suggests that this response is either equal or slightly weaker compared to the one estimated at the point of UI exhaustion

[45]On the other hand, in the unlikely case in which the response of the wage choice in the periods prior to UI exhaustion was stronger than at the point of UI exhaustion, the real LMOS estimate including anticipation effects would be closer to zero.

[46]This can be seen comparing rows (1) and (2).

and it is insignificantly different from zero.

Our analysis of hourly wages is conceptually much closer to that in Schmieder et al. (2016), in that reservation wages are just barely binding. The main reason why reservation wages are barely binding when analyzing hourly wage losses can be seen in Table 3. Hourly wage losses between the pre-displacement and re-employment jobs see a much smaller increase at the point of exhaustion of UI benefits compared to daily wage losses. This results in their IV estimate for hourly wages being almost identical to our LMOS estimate of duration dependence in hourly wages.

5.4 Robustness: The Role of Permanent Unobserved Heterogeneity in the LMOS

Our results above indicate that duration dependence is 0.8 percent in daily wages and 0.25 percent in hourly wages. However, the moments used in the estimation do take into account the potential role of permanent unobserved heterogeneity. To consider this, we repeat our estimation in a sample of workers with two or more UI claims, both with and without including individual fixed effects. The results are shown in Table A.9. Three main points are worth highlighting.

First, we cannot rule out that the estimated LMOS (in both daily wages and hourly wages) is identical to that in our main estimates using the complete sample. Second, there is no strong evidence that dynamic selection based on permanent unobserved heterogeneity plays an important role in biasing our main results, especially when considering our results regarding hourly wage changes. The estimated LMOS in hourly wages is identical (-0.25 percent per month of non-employment) in our complete sample, in our sample of workers with two or more UI claims when we do not include individual fixed effects, and in this same sample after controlling for permanent unobserved heterogeneity. Third, we find some economically meaningful (but statistically insignificant) evidence that dynamic selection based on unobserved permanent heterogeneity is affecting our main results regarding daily wages. Our estimate of the LMOS for the sample with two or more UI claims after controlling for individual fixed effects is -0.4 percent. While this estimate is not statistically significantly different from our main result, the difference between both is meaningful in economic terms. This result suggests that a non-trivial part of the increase in wage losses that we observe as the duration of non-employment accumulates could be driven by selection, specifically in hours worked (workers with a higher propensity to choose fewer hours of work exit non-employment later).

In general, however, we see these results as consistent with our main ones. Duration dependence remains an important source of wage losses as the duration of non-employment accumulates. A meaningful part of duration dependence in daily wages appears to be driven by workers' inability to find full-time jobs, resulting in duration dependence in hourly wages being closer to zero than that in daily earnings. Finally, we find some economically meaningful (but statistically insignificant) evidence of dynamic selection in hours worked. Workers who choose to work fewer hours appear to leave non-employment later. Accounting for this type of selection makes the estimated duration dependence slightly closer to zero but does not invalidate its existence and relevance.

6 Conclusion

This paper estimates the causal effect of the duration of non-employment on re-employment wages. Using exogenous variation in the potential duration of UI, we estimate that duration dependence is approximately 0.8 percent per month in daily wages and 0.25 percent per month in hourly wages.

We show that an extension of the potential duration of UI affects re-employment wages through three channels. First, it gives workers extra time to find a job while still collecting UI benefits. This matters because workers respond to the exhaustion of UI benefits by decreasing their wage choice by approximately 3 percent. Additionally, by increasing the duration of non-employment, an extension of the potential duration of UI affects re-employment wages through a second and third channel: It deteriorates workers' labor market opportunities (duration dependence), and it pushes workers toward re-employment later in the spell, once their UI benefits have expired. We show how to recover the degree of duration dependence separate from the other two channels. This is important because the first and third channels depend on the UI policy choice, while we understand the second as a primitive regarding the prospects of labor market opportunities over time.

Beyond our main goal, our individual results have important implications for policy. We establish a connection between our estimated causal effect of the exhaustion of UI benefits on re-employment wages and the elasticity of wage selectivity to the value of non-employment, an important parameter for wage determination, labor market matching, and UI policy effectiveness. Furthermore, we complement the work of Schmieder et al. (2016) and Nekoei and Weber (2017) to show a more complete picture of how to identify duration dependence in certain cases in which reservation wages bind.

Our work raises several new questions. First, where does the increased wage selectivity in response to an extension of UI come from? Is it reflecting improvements in match quality (a true productivity increase) or is it simply that workers have a better bargaining position in their negotiations with employers? In the former case, the fiscal externality from increased re-employment earnings should be thought of as a true efficiency improvement. In the latter case, changes in relative bargaining power do not necessarily create net benefits for society. The distinction between these cases consequently matters for policymakers.

Second, while identifying and estimating the sources behind duration dependence is beyond the scope of this paper, it remains an open question with important policy implications. We put forward two potential mechanisms (human capital depreciation and signaling), but our data do not allow us to speak to their relative importance. Furthermore, previous evidence on this topic suggests that neither mechanism may be able to fully account for these effects (see Eriksson and Rooth (2014) or Cohen et al. (2023)), leaving the door open to alternative explanations.

Third, while we assume that duration dependence is linear in the duration of non-employment, this does not necessarily need to be the case. It is possible that duration dependence is stronger at the beginning of the unemployment spell, for instance, if workers' specific human capital depreciates faster than general human capital. For instance, Cohen et al. (2023) document that skill depreciation in general human capital is unlikely to be a major explanation for duration dependence in re-employment wages or hazard rates, but the evidence regarding specific human capital is scarce. Whether duration dependence is linear in the duration of non-employment has important consequences for the distributional effects of UI, especially for long-term unemployed workers. An important challenge in solving this question is to distinguish the non-linearities in wage depreciation from the sorting of workers with lower depreciation rates to the lengthier side of the non-employment duration distribution. Future research trying to understand the degree of non-linearity of duration dependence and the relative importance of specific and general human capital would be tremendously valuable.

Last, our paper does not consider the extensive margin of finding a job versus leaving the labor force and how it evolves over time. Does the probability of finding any job decline over time? Previous work has found it difficult to provide an answer to this question, even when data on the search behavior of unemployed workers are available (see DellaVigna et al. (2022)). However, this is an important question to explore from the perspective of understanding duration dependence in re-employment wages, since the relevance of the extensive margin may differ across workers. For instance, in low-wage jobs, workers have less space for bargaining over wages, making this extensive margin more relevant. Therefore, a crucial piece to understanding the job market opportunities of long-term unemployed workers relies on understanding the dynamics of the job-finding rate over time.

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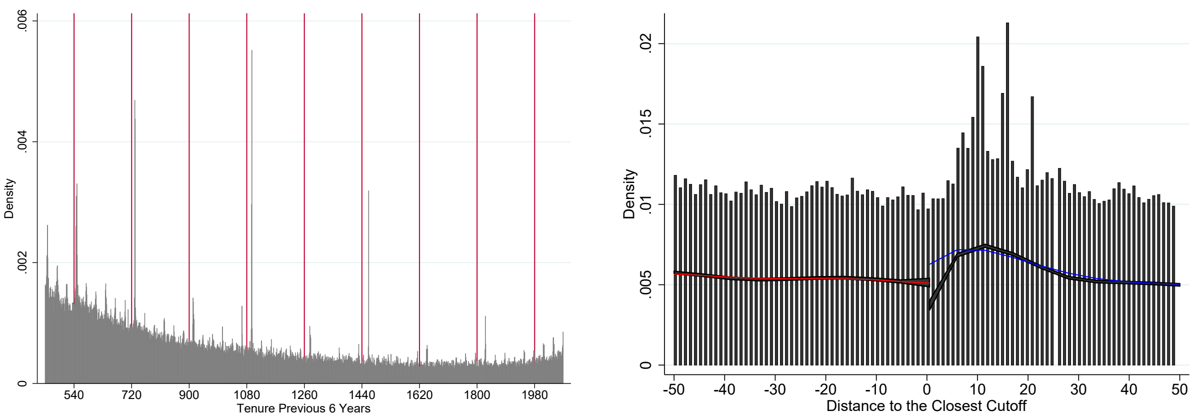
A Additional Figures

Figure A.1: Distribution of the Previous Experience:
Including UI Claims from Temporary Contracts with Predetermined Length

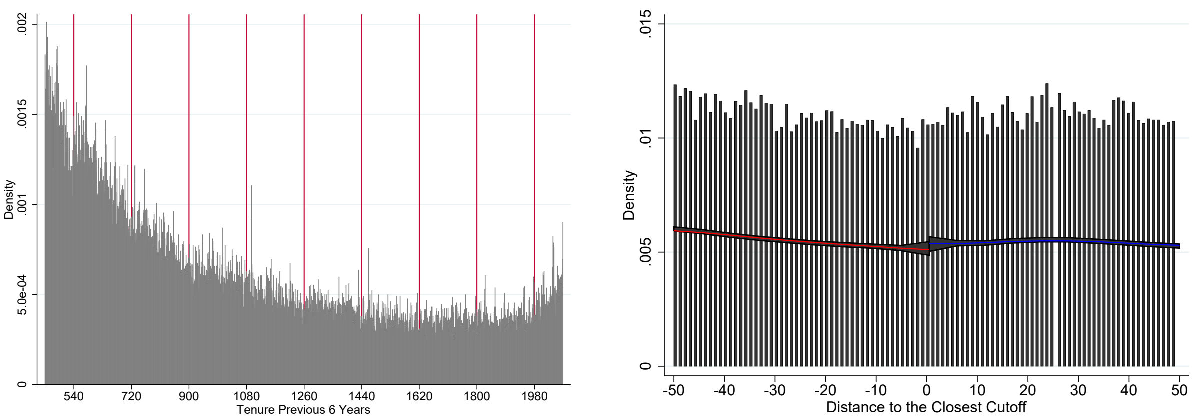


(a) Previous Tenure

(b) Manipulation Test Previous Tenure (Relative to Policy Discontinuities) T = -7.080, p-value = 0.000

Note: These figures plot the distribution of working experience in the previous 6 years (the running variable) for our original sample. Panel (a) presents it separately for each discontinuity, with red bars marking each of the policy thresholds. Panel (b) presents the distribution around the closest policy discontinuity for all thresholds combined and adds the point estimates and confidence intervals of the manipulation test as in Cattaneo et al. (2018). The estimate rejects no manipulation. In the main text we argue this manipulation arises due to the relevance of temporary contracts with predetermined duration, "bunched" systematically to the right of the discontinuity due to those contracts usually being multiples of 1/2 year lengths.

Figure A.2: Distribution of the Previous Experience: Final Sample

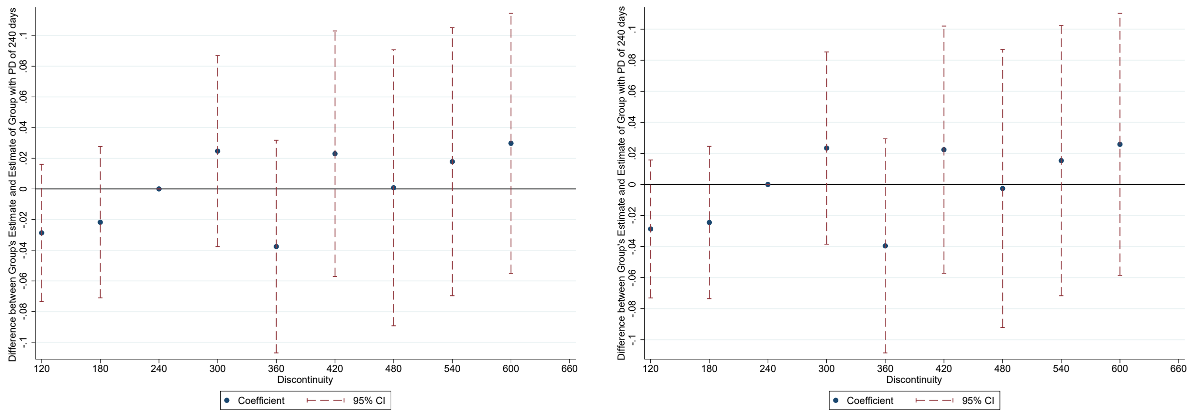


(a) Previous Tenure

(b) Manipulation Test Previous Tenure (Relative to Policy Discontinuities): T = 0.861, p.value = 0.389

Note: These figures plot the distribution of working experience in the previous 6 years (the running variable) for our original sample, after removing unemployment spells where the worker enters unemployment from a temporary contract with previous tenure that is a multiple of a half year. This sample also removes all unemployment spells where our calculated potential duration does not match the worker's time collecting benefits (i.e., the worker collects benefits as if she had a longer or shorter potential duration). Specifically, we remove unemployment spells where (1) the worker collects benefits for a longer time than her (calculated) potential duration, but corresponding to a different potential duration, and then continues searching for a job; (2) the worker collects benefits for a shorter time than her (calculated) potential duration, but corresponding to a different potential duration, and then continues searching for a job. Panel (a) presents tenure separately for each discontinuity, with red bars marking each of the policy thresholds. Panel (b) presents the distribution around the closest policy discontinuity for all thresholds combined and adds the point estimates and confidence intervals of the manipulation test as in Cattaneo et al. (2018). The estimate does not reject no manipulation.

Figure A.3: Effect of the Exhaustion of UI Benefits on Wage Losses



(a) by Discontinuity

(b) by Discontinuity (Addt'l Controls for Time to Re-employment)

Note: Figure A.3 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs, separately for workers around each one of the policy discontinuities (each value on the x-axis represents one discontinuity). Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. In panel (a) the point estimates displayed show the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text) for workers around the discontinuity shown in the x-axis, relative to workers around the discontinuity generated between 899 and 900 days. The specification includes all controls (including discontinuity fixed effects; see text). Panel (b) is identical to panel (a) but additionally controls for time to re-employment. The 95 percent confidence intervals shown are generated from robust standard errors.

Additional Tables

Table A.1: Summary Statistics

Potential Duration	120	180	240	300	360	420	480	540	600	660	All
Days Collecting UI	92.00	110.0	136.3	157.2	183.6	200.8	219.6	240.6	254.6	263.4	161.8
	(49.66)	(69.12)	(90.21)	(112.2)	(134.4)	(154.0)	(174.7)	(196.7)	(214.2)	(229.7)	(142.2)
Time to Re-employment (Days)	184.4	190.5	219.9	235.3	265.9	279.9	295.1	313.9	320.6	329.2	241.8
	(234.3)	(246.2)	(269.2)	(278.3)	(303.7)	(313.9)	(324.7)	(340.6)	(342.5)	(356.5)	(291.1)
Re-employment in 6 months	0.721	0.670	0.608	0.584	0.538	0.521	0.503	0.490	0.482	0.480	0.592
Re-employment in 12 months	0.888	0.878	0.846	0.811	0.753	0.731	0.710	0.682	0.676	0.675	0.799
Share Exhausting UI	0.382	0.262	0.225	0.197	0.185	0.164	0.147	0.144	0.124	0.109	0.220
Real Previous Daily Wage	47.23	48.11	48.41	49.82	50.39	51.43	52.57	52.60	53.11	54.08	49.85
	(16.15)	(17.05)	(16.94)	(18.29)	(19.15)	(19.87)	(20.46)	(20.43)	(20.60)	(21.49)	(18.52)
Real Previous Hourly Wage	5.904	6.014	6.052	6.228	6.298	6.428	6.572	6.574	6.639	6.761	6.232
	(2.019)	(2.131)	(2.117)	(2.286)	(2.394)	(2.483)	(2.557)	(2.554)	(2.576)	(2.687)	(2.315)
Previous (6 Years) Tenure	491.5	622.6	804.1	987.4	1165.1	1348.1	1527.5	1709.7	1891.1	2027.5	1049.1
	(25.59)	(51.11)	(51.53)	(51.32)	(51.98)	(51.86)	(51.96)	(51.36)	(52.72)	(25.98)	(480.1)
Age	35.11	34.69	34.33	34.33	34.35	34.32	34.37	34.65	34.97	35.66	34.63
	(7.237)	(7.104)	(7.018)	(6.995)	(6.958)	(6.911)	(6.913)	(6.918)	(6.844)	(6.803)	(7.025)
Share Male	0.623	0.635	0.641	0.632	0.627	0.627	0.624	0.634	0.633	0.666	0.633
Share College	0.245	0.260	0.270	0.286	0.282	0.295	0.301	0.285	0.274	0.251	0.272
Share High School	0.402	0.423	0.439	0.456	0.456	0.472	0.492	0.468	0.459	0.435	0.443
Wealth*	42.34	43.91	44.72	48.95	51.01	55.18	58.64	64.15	69.94	77.29	51.31
	(36.38)	(36.26)	(35.53)	(35.72)	(35.49)	(36.11)	(36.56)	(37.66)	(38.68)	(39.39)	(37.76)
Change Log Real Daily Wage	-0.0116	-0.00832	-0.0252	-0.0401	-0.0450	-0.0508	-0.0686	-0.0754	-0.0850	-0.0946	-0.0383
	(0.418)	(0.410)	(0.418)	(0.418)	(0.436)	(0.420)	(0.418)	(0.426)	(0.428)	(0.419)	(0.420)
Change Log Real Hourly Wage	0.0520	0.0522	0.0447	0.0305	0.0236	0.0199	0.00359	-0.00405	-0.0152	-0.0283	0.0289
	(0.298)	(0.307)	(0.313)	(0.312)	(0.323)	(0.327)	(0.325)	(0.329)	(0.332)	(0.337)	(0.317)
N	17527	27313	18903	14634	11825	10099	8761	8376	8784	5930	132152

Note: Table A.1 presents the summary statistics of our final sample. Each column displays the summary statistics for workers with a different potential duration of UI. The final column shows summary statistics for the complete sample. Means and standard deviations (in parentheses) are shown.
*Wealth is the discounted sum of real wages throughout the worker's working history.

Table A.2: Effect of a two-month extension of the potential duration of UI. All discontinuities

	Daily Wage Change Wage 1 Year after Re-employment		Daily Wage Change Wage 5 Years after Re-employment	
RD Estimate	-0.008*	-0.008**	-0.008*	-0.008**
	[0.004]	[0.004]	[0.004]	[0.004]
Controls	Disc	All	Disc	All
N	128893	128507	129313	128921

Note: Table A.2 presents the estimated causal effect of a two-month extension of the potential duration of UI on the wage change when we calculate the re-employment wage as the average wage in the first year after re-employment (columns (1) and (2)) and when we calculate the re-employment wage as the average wage in the first five years after re-employment (columns (3) and (4)). Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (including discontinuity fixed effects; see text). All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.3: Effect of a two-month extension of the potential duration of UI. All discontinuities

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	30.955***	28.978***	-0.009	-0.014*	-0.005	-0.010*
	[5.571]	[5.351]	[0.009]	[0.008]	[0.008]	[0.006]
Controls	Disc	All	Disc	All	Disc	All
Bandwidth	35	45	24	33	23	33
N	132152	130142	122396	122027	122396	122027

Note: Table A.3 presents the estimated causal effect of a two-month extension of the potential duration of UI on the time to re-employment (columns (1) and (2)), on the wage change (columns (3) and (4)), and on the hourly wage change (columns (5) and (6)). Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (including discontinuity fixed effects; see text). Bandwidth indicates the MSE optimal bandwidth for that estimate. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.4: Balance Test. All discontinuities

Panel A: Age				
RD Estimate	0.005	0.004	0.071	0.116
	[0.136]	[0.085]	[0.136]	[0.133]
Bandwidth	35 (O)	90	90	90
Panel B: Male				
RD Estimate	-0.005	-0.006	-0.001	-
	[0.011]	[0.006]	[0.009]	[.]
Bandwidth	25 (O)	90	90	-
Panel C: High School				
RD Estimate	-0.011	0.002	0.005	-
	[0.012]	[0.006]	[0.010]	[.]
Bandwidth	22 (O)	90	90	-
Panel D: College				
RD Estimate	-0.022*	0.002	0.002	-
	[0.012]	[0.005]	[0.009]	[.]
Bandwidth	19 (O)	90	90	-
Panel E: *ln* Wealth				
RD Estimate	0.007	0.015	-0.000	0.003
	[0.016]	[0.009]	[0.014]	[0.015]
Bandwidth	28 (O)	90	90	90
Panel F: *ln* Previous Daily Wage				
RD Estimate	-0.013	0.002	0.004	0.011
	[0.008]	[0.004]	[0.007]	[0.007]
Bandwidth	25 (O)	90	90	90
Panel G: *ln* Previous Hourly Wage				
RD Estimate	-0.013	0.002	0.004	0.011
	[0.008]	[0.004]	[0.007]	[0.007]
Bandwidth	25 (O)	90	90	90
Panel H: *ln* Previous Firm Tenure				
RD Estimate	0.010	-0.023	-0.048	0.016
	[0.069]	[0.038]	[0.055]	[0.055]
Bandwidth	27 (O)	90	90	90
Panel I: *ln* Previous Experience				
RD Estimate	0.065	-0.008	0.009	0.000
	[0.055]	[0.031]	[0.013]	[0.003]
Bandwidth	29 (O)	90	90	90
Panel J: Share of Full Time				
RD Estimate	0.000	0.000	-0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	23 (O)	90	90	90
Panel K: Permanent Contract				
RD Estimate	0.016	-0.002	-0.001	-0.009
	[0.012]	[0.005]	[0.009]	[0.011]
Bandwidth	19 (O)	90	90	90
Panel L: Cov. Index - Predicted Time to Re-employment				
RD Estimate	1.152	0.217	-0.466	-1.754
	[1.326]	[0.705]	[1.120]	[1.289]
Bandwidth	25 (O)	90	90	90
Controls	Disc	Disc	Disc	Disc
Worker FE	No	No	No	Yes
Sample	Complete	Complete	2+ Obs	2+ Obs
N	132152	132152	46426	46197

Note: Table A.4 presents the balance test of a two-month extension of the potential duration of UI on different observed worker characteristics. Controls "Disc": Discontinuity fixed effects. Bandwidth: Indicates the length of the bandwidth. The symbol (O) indicates optimal bandwidth following Calonico et al. (2020). Controls "Disc": Discontinuity fixed effects. Sample "Complete": The complete sample is used in the estimation. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.5: Effect of the exhaustion of UI benefit. All discontinuities

	Panel A: Daily Wage change		
θ	-0.000	0.001	0.000
	[0.003]	[0.002]	[0.002]
γ_0	0.028***	0.029***	0.031***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.011**	-0.011***	-0.011**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
Start	-8	-8	-8
End	52	52	52
N	132152	130142	130142

	Panel B: Daily Wage change		
θ	0.000	0.001	0.001
	[0.003]	[0.002]	[0.002]
γ_0	0.025***	0.029***	0.031***
	[0.007]	[0.007]	[0.007]
γ_{Post}	-0.010*	-0.011**	-0.011**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
Start	-15	-15	-15
End	45	45	45
N	132152	130142	130142

Note: Table A.5 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. In both panels, workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. "Start" specifies the lowest value of time to re-employment relative to primary potential duration in which the variable E_0 takes the value one. "End" specifies the highest value of time to re-employment relative to primary potential duration in which the variable E_0 takes the value one. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.6: Effect of the exhaustion of UI benefit. All discontinuities

Panel A: Daily Wage change (First-year average re-employment wage)			
θ	0.000	0.001	0.000
	[0.002]	[0.002]	[0.002]
γ_0	0.031***	0.035***	0.038***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.002	-0.006	-0.008*
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
N	128893	128507	128507

Panel B: Daily Wage change (First 5 years average re-employment wage)			
θ	0.001	0.002	0.001
	[0.002]	[0.002]	[0.002]
γ_0	0.023***	0.026***	0.029***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.003	-0.007*	-0.008**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
N	122396	122027	122027

Note: Table A.6 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs using longer-term definitions of re-employment wages. Panel (A) defines the re-employment job wage as the average daily wage during the first year after re-employment, while panel (B) defines the re-employment job wage as the average daily wage during the first five years after re-employment. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.7: Effect of a two-month extension of the potential duration of UI. All discontinuities

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	35.937***	40.405***	-0.007	-0.005	-0.003	-0.003
	[4.015]	[5.290]	[0.005]	[0.007]	[0.004]	[0.005]
Controls	All	All	All	All	All	All
Worker FE	No	Yes	No	Yes	No	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	45892	45373	43377	41277	43377	41277

Note: Table A.7 presents the estimation of the causal effect of a two-month extension of the potential duration of UI on the time to re-employment (columns (1) and (2)), on the wage change (columns (3) and (4)), and on the hourly wage change (columns (5) and (6)). The results in the table use a sample of workers for whom we observe two or more UI claims. Controls "All": All controls included (including discontinuity fixed effects; see text). Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.8: Effect of the exhaustion of UI benefit. All discontinuities

	Daily Wage change				Hourly Wage change			
θ	-0.003	-0.003	-0.003	-0.002	-0.003	-0.003	-0.002	-0.002
	[0.004]	[0.003]	[0.004]	[0.004]	[0.003]	[0.003]	[0.004]	[0.004]
γ_0	0.027***	0.030***	0.025**	0.027**	0.011	0.013*	0.010	0.012
	[0.010]	[0.009]	[0.012]	[0.012]	[0.008]	[0.008]	[0.009]	[0.009]
γ_{Post}	-0.003	-0.004	0.009	0.008	-0.007	-0.007	0.003	0.003
	[0.007]	[0.007]	[0.009]	[0.009]	[0.006]	[0.006]	[0.007]	[0.007]
Controls	All	All	All	All	All	All	All	All
Time to Re-emp Ctrl	No	Yes	No	Yes	No	Yes	No	Yes
Worker FE	No	No	Yes	Yes	No	No	Yes	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	43671	43671	41771	41771	43671	43671	41771	41771

Note: Table A.8 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) to (4) is the change in daily wages, while in columns (5) to (8) the outcome is the change in hourly wages. The results in the table use a sample of workers for whom we observe two or more UI claims. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control group for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.9: Deterioration rate of labor market opportunities estimates (LMOS) vs IV estimates from Schmieder and von Wachter (2016)

	Daily Wage Change		Hourly Wage Change	
LMOS	-0.0070	-0.0038	-0.0027	-0.0028
90% CI	[-0.0155, 0.0018]	[-0.0123, 0.0069]	[-0.0104, 0.0043]	[-0.0107, 0.0058]
LMOS (Ignoring $\mathbf{o}(\beta)$)	-0.0085	-0.0050	-0.0034	-0.0032
90% CI	[-0.0170, 0.0005]	[-0.0136, 0.0058]	[-0.0110, 0.0038]	[-0.0112, 0.0054]
IV (Schmieder and Von Wachter (2016))	-0.0063	-0.0034	-0.0025	-0.0026
90% CI	[-0.0151, 0.0022]	[-0.0121, 0.0073]	[-0.0102, 0.0045]	[-0.0105, 0.0061]
Ratio LMOS vs. IV	110%	112%	108%	108%
Controls	All	All	All	All
Worker FE	No	Yes	No	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	43377	41277	43377	41277

Note: Table A.9 presents the estimated (monthly) deterioration rate of labor market opportunities in daily re-employment wage (columns (1) and (2)) and in hourly re-employment wage (columns (3) and (4)) (row 1) and compares it with an estimate of the deterioration rate of labor market opportunities that ignores the anticipation effects (row 2) and to the IV estimate suggested in Schmieder and von Wachter (2016) (row 3). The sample is that of workers with two or more UI claims. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Worker FE: Whether the estimation of all moments used to calculate the LMOS includes individual fixed effects or not. All estimates are constructed from conventional RD estimates as shown in Calonico et al. (2014)) using a triangular kernel. Bootstrapped 90 percent confidence intervals in brackets. The sample is bootstrapped at the spell level. p-value: * 0.10 ** 0.05, *** 0.01.

Appendix

B Theoretical Framework

B.1 Separability between the Value of UI Benefits and Duration Dependence

This section presents an illustrative framework to rationalize the shortcut equation in the main text (equation (3) that expresses the optimal wage choice as a sum of two separate pieces: the value of UI benefits and the human capital depreciation.

The following two equations characterize a standard dynamic search model with duration dependence.

$$\mathcal{U}_t = u(b) + \ell + \beta \max_{w_t, \lambda_t, e_t} \{-\mathcal{C}(e_t) + \cdot (\lambda_t \cdot \mathcal{V}(w_t) + (1 - \lambda_t) \cdot \mathcal{U}_{t+1})\} \tag{9}$$

$$\mathcal{V}_t(w) = 1/(1-\beta)u(w) \tag{10}$$

subject to:

$$\mathcal{F}(w, \lambda, e) = y(t) \text{ or } \lambda = \Lambda(w, e, y(t)) \tag{11}$$

Consider a representative worker who was exogenously displaced from her previous job. Let $t = 0, 1, 2, \cdots$ denote the calendar time since entering unemployment. For each period t, an unemployed worker with static utility $u()$ and value of leisure ℓ receives unemployment insurance or unemployment assistance, summarized in $\{b_t\}$. Workers' static utility is connected intertemporally by a discount factor $\beta \in [0, 1)$.

Upon receiving unemployment insurance, workers decide the optimal search effort e_t, wage w, and job-finding rate λ. Search effort e_t incurs a cost of $\Phi(s_t)$. We assume $\mathcal{C}(\cdot)$ is a weakly increasing, weakly convex, and twice differentiable function. The combination of these three choices, (e_t, λ, w_t), will be affected by the worker's productivity, $y(t)$, through the search technology function $\mathcal{F}$. $\mathcal{F}$ is assumed to be convex in λ and w, with monotonic properties $\frac{\partial \mathcal{F}}{\partial w} > 0$, $\frac{\partial \mathcal{F}}{\partial \lambda} > 0$, and $\frac{\partial \mathcal{F}}{\partial e} < 0$.

This expression of the search technology function in equation (11) is very general and includes those derived from directed search models[47] with matching technology (e.g., telephone matching, urn-ball matching, and Cobb-Douglas matching) or from a random search model.[48]. Notice that productivity is decreasing over the duration of non-employment, generating a source for duration dependence. Moreover, we also derive an implicit function Λ from $\mathcal{F}$. This Λ specifies a corresponding job-finding rate, given the wage choice, search effort, and worker's productivity. We use Λ and $\mathcal{F}$ interchangeably to facilitate our exposition.

[47] See Petrongolo and Pissarides (2001) and Wright et al. (2021)

[48] See Rogerson et al. (2005), Eckstein and van den Berg (2007), Marinescu and Skandalis (2021) and Nekoei and Weber (2017). In particular, Marinescu and Skandalis (2021) and Nekoei and Weber (2017) show how to transform the search technology of random search models into the functional form considered in directed job search models.

Taking the first-order condition with respect to w:

$$V(w^*) - U_t = \frac{\lambda}{-\frac{\partial \lambda}{\partial w}} \frac{\partial V}{\partial w} \tag{12}$$

This expression equates the value of working at a wage w with the summation of the value of non-employment and the option value of continuing to search in the future. Let $\rho(t) = \frac{\lambda}{-\frac{\partial \lambda}{\partial w}}$ represent the semi-elasticity of the job-finding rate with respect to the wage choice. In this expression, the source of duration dependence comes from $y(t)$. The previous assumptions made on $\mathcal{F}$ imply that $\rho(t)\frac{\partial V}{\partial w}$ is an increasing function of w and a decreasing function of $y(t)$.

Using equation (12), it is possible to express the optimal wage iteratively:

$$u(w^*) = \underbrace{(1-\beta)\sum_{\tau \geq t} \beta^{\tau - t} \left(u(b_\tau) + \ell\right)}_{\text{Value of UI and leisure}} + \underbrace{(1-\beta)\left(\rho_t \frac{\partial V_t}{\partial w} - \beta \mathcal{C}_t + \sum_{\tau \geq t+1} \beta^{\tau - t + 1} (\lambda_\tau \rho_\tau \frac{\partial V_\tau}{\partial w} - \beta \mathcal{C}_\tau)\right)}_{\text{Option value of searching}} \tag{13}$$

The optimal wage can be decomposed into two pieces. First, the weighted average of flow utility from UI benefits and leisure, with β determining the weight that future benefits receive. Second, the weighted average of the option value from searching for a job, a value that depends on duration dependence $y(t)$. Therefore, there exists a certain degree of separability between the value of unemployment insurance and human capital depreciation.

Let us now discuss two special cases. First, when β goes to zero, workers become extremely impatient/myopic, resulting in $u(w) \to u(b_t) + \ell + \rho(t)u'(w)$. Thus, workers become very sensitive to the value of the outside option, suggesting that changes in the value of the outside option should result in changes in the reservation wage. In this case, the value from UI benefits and the human capital depreciation is fully separable.

Second, consider the parametric search technology function specified by Nekoei and Weber (2017), $\Lambda(w, s, t) = a\,(t) \cdot s^{1-1/\sigma(t)} \cdot \exp\left(-\frac{u(w)}{\rho(t)}\right)$. Provided a log-utility set-up, we reach:

$$ln(w^*(t)) = (1-\beta)\left(u(b_t) + \sum_{j=1}^{\infty} \beta^j \left(u(b_{t+j}) - e_{t+j}\right)\right) + \rho(t) + (1-\beta) \cdot \beta \sum_{j=1}^{\infty} \delta^j \lambda_{t+j} \rho(t+j) \tag{14}$$

Similar to our first example, the first-order term of the above wage equation specifies that the value of non-employment and duration dependence are fully separable.

B.2 Equivalence between the Decline in Wages at the Exhaustion of UI Benefits and the Wage Response to an Extension of the UI Potential Duration

Our goal is to show that the decline in wages at the point of exhaustion of UI benefits is equivalent to the elasticity of the wage choice to an extension of the UI potential duration. This equivalence is similar to that in Krueger et al. (2014), but instead of considering a permanent removal of UI benefits, we consider an extension of the UI potential duration.[49]

First consider how extending the potential duration of UI from B to $B+1$ affects the optimal wage at $t = B+1$:

$$\frac{\partial g(w_{B+1})}{\partial b_{B+1}} \equiv ln(w^*_{B+1})|(B+1) - ln(w^*_{B+1})|(B) \tag{15}$$

Based on equation (14), we can express the above equation as:

$$\frac{\partial g(w_{B+1})}{\partial b_{B+1}} = (1-\beta)(u(\bar{b}) - u(\underline{b})) \tag{16}$$

Second, consider what happens to wages when unemployment insurance (UI) ends after B periods:

$$ln(w^*_B)|(B) - ln(w^*_{B+1})|(B) = (1-\beta)(u(\bar{b}) - u(\underline{b})) \tag{17}$$

$$+\underbrace{\rho(B) - \rho(B+1) + (1-\beta)\left(\sum_{j=1}^{\infty}(\lambda_{B+j+1}\rho(B+j+1) + \mathcal{C}_{B+j+1} - \lambda_{B+j}\rho(B+j) - \mathcal{C}_{B+j})\right)}_{\text{Duration Dependence net of Changes in UI Benefits}}$$

The decline in wages at the point of exhaustion of UI benefits can be decomposed into two parts: first, the effect arising from the reduction in UI benefits because the value of the outside option goes down; second, an effect that represents duration dependence due to the reduced prospect of finding a job (only a function of ρ and $\underline{b}$, because $t > B+1$).

The first piece in equation (17) is of first-order importance because it captures the abrupt change in the value of the outside option. On the other hand, the relevance of the second piece ("Duration Dependence net of Changes in UI Benefits") will be significantly more limited. This occurs because the time frame of the decline can be very small (think about the wage right before and right after the UI exhaustion), pushing the effect coming from duration dependence toward zero.[50]

Therefore, we can establish the approximation that states the equivalence between wage elasticity to an extension of the potential duration of UI with the decline in wages observed at the

[49] They are equivalent under the extreme case where β is close to zero.

[50] Even if the time frame is longer, it is possible to extract the effect coming from duration dependence from the left-hand side of equation (17), provided that duration dependence is a smooth process throughout time in non-employment

point of exhaustion of UI benefits:

$$\underbrace{\frac{\partial g(w_{B+1})}{\partial b_{B+1}}}_{\text{Binding wage}} \underset{\Delta t \to 0}{\to} \underbrace{ln(w_B^*)|(B) - ln(w_{B+1}^*)|(B)}_{\text{Wage drop at UI exhaustion}} \tag{18}$$

where Δt represents the unit of time in this model. The intuition of this equivalence is that duration dependence (encompassing the human capital depreciation, signaling effect, decreasing matching quality, etc.) declines smoothly over time, especially at the point at which UI benefits expire. This is a natural assumption that has been verified in experimental set-ups (Kroft et al. (2013)).