Why Re-employment Wages Decay During An Unemployment Spell: New Evidence from Spain and Policy Implications

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Abstract

Unemployed workers who remain longer in unemployment typically find a job with lower earnings. This paper studies the causes and policy implications of this observation, using Spanish Social Security data. In the first part of this paper, we separately identify the two major causes behind this negative relation: workers' reduction of their wage selectivity in response to the exhaustion of unemployment insurance (UI) and the deterioration of labor market opportunities. We find that re-employment wages sharply decrease by 3.2% when UI expires. Building upon this result, we identify the deterioration rate of labor market opportunities using exogenous UI extensions as an instrument that affects re-employment wages indirectly, through increases in the duration of unemployment, while controlling for the simultaneous direct wage increase they create through the workers' wage selectivity response. Using our approach, taking advantage of the quasi-experimental variation in potential duration of UI in Spain, we find that workers suffer a 1.1% decline in re-employment earnings per month of unemployment experience, due to the deterioration rate of labor market opportunities. In the second part, to understand the policy implications of this result, we estimate a structural model to answer whether it is better to increase the duration or the level of UI. We find that UI extensions can generate larger expected wages for workers with short UI duration, while increases in the level of benefits are more effective at increasing the re-employment wages of workers with longer UI potential duration. We highlight that UI extensions act as "mandatory savings" tools, helping workers find a job with better labor market outcomes during the early stages of unemployment.

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1 Introduction

Unemployed workers typically suffer large earning losses after finding a new job. The severity and persistence of these wage declines has prompted significant efforts to identify the causes of this pattern. Among these efforts, one well established finding is the negative relationship between re-employment earnings and unemployment duration.[1] While important micro[2] and macro[3] implications of this negative relationship have been theoretically discussed, the empirical relevance of its causes and their consequences have not been fully understood. Why do re-employment earnings deteriorate when jobless durations increase? What are the corresponding implications for policy?

This paper answers these questions by quantifying the causes behind this negative relationship and showing their relevance to the efficient construction of unemployment insurance policies. To do so, we carefully solve two empirical challenges. The first challenge is separating the causal impact of unemployment duration on re-employment earnings from the selection effect, where workers with worse potential labor market outcomes might stay unemployed longer. The second challenge, and the main contribution of this paper, is to decompose this causal effect further into two major mechanisms considered by the literature and assess their policy implications. First, potential earnings deteriorate during unemployment because the labor market opportunities of a worker decays over time.[4] Second, workers lower their wage selectivity when they approach the exhaustion of unemployment insurance (UI). Empirically disentangling the relevance of these two mechanisms is difficult using observational studies, since both are highly co-integrated with each other, since

This paper achieves the following two goals using administrative data from Spain. First, we separately identify the magnitude of the two mechanisms that are responsible for re-employment earnings declines. Using a set of rich policy variations within the Spanish UI system, we show that the mechanism – workers exhausting their UI benefits and lowering down their wage selectivity – is sizable, and provide novel estimates of the degree by which a worker lowers their wage selectivity. We find that workers sharply reduce their wage by 3.2% on average in response to the exhaustion of UI. To quantify the wage decline rate due to the deterioration of labor market opportunities, we propose a sufficient statistics approach that requires causal estimates of both the wage impact of UI

[1] Recent literature finds that a large portion of the heterogeneity in re-employment earning losses can be explained purely by the heterogeneity in jobless durations (Schmieder and Heining (2021)), regardless of whether such transitions are voluntary or involuntary (Fallick et al. (2021)).

[2] Wage subsidies (Pavoni (2009)) and job training programs (Spinnewijn (2013)).

[3] A key macro implication is the amplification of recessionary outcomes. See Bernanke (2012) and Kroft and Notowidigdo (2016) for the consequences on long-term unemployment.

[4] This mechanism is consistent with human capital depreciation, where the worker's stock of skills deteriorates over time when idle (Pavoni (2009)), with a signalling effect, where employers statistically believe that workers with longer unemployment duration tend to be less productive (Jiang et al. (2019), Jarosch and Pilossoph (2019)), or with job ladder models where workers can only access lower productivity firms as they remain unemployed (Jarosch (2021)). We don't have an intention to disentangle them but treat them as a whole parameter to be estimated.

extension and, more importantly, workers' wage selectivity response when expiring UI [5]. Using this estimator, we find that Spanish workers suffer a 1.1% decline in re-employment earnings per month through the deterioration of labor market opportunities. The estimates of these two mechanisms means that, on average, 10% of the causal impact of unemployment duration on re-employment wages is driven by the exhaustion of UI, while the remaining 90% is caused by the deterioration of labor market opportunities. [6]

Second, we structurally estimate a model to examine the policy debate on whether to extend UI duration or to increase UI level when the government wants to expand their UI generosity. A cost-equivalent comparison between the effects of these two policy interventions on the expected re-employment wages suggests that an extension works as a "mandatory savings" tool, benefiting workers with short potential durations relative to an increase in the benefit level. There are diminishing returns to UI durations such that for workers with long potential duration, UI extensions are less effective than UI level increases. These results from our policy experiments imply that policymakers should consider this "mandatory savings" gain as an ingrediant in their optimal design of UI system.

We use administrative data from Spain, and take advantage of the rich policy variations within the Spanish UI system. Shown in Section 2, our main source of identification comes from the policy schedule of potential duration in the Spanish UI system. The potential duration of UI is determined by the number of days worked in the previous 6 years, using an increasing but discontinuous policy function. Workers with at least 360 days worked in the previous 6 years are eligible to collect up to 4 months of UI benefits. Starting from the eligibility point, workers receive an additional two months of UI for every additional 180 days worked in the previous 6 years. This unique feature implies that workers with very similar characteristics can claim UI for different amounts of time, depending on whether their past working experiences cross one of the discontinuities within a short window.

We show how we achieve the goals of this paper in Section 3 to Section 5. In Section 3, we estimate two causal moments — the impact on re-employment earnings of both the exhaustion and extension of UI benefits. First, we document a sudden drop in re-employment earnings upon they expiring their UI and argue it represents the causal impact of UI exhaustion. Our baseline measure of re-employment earnings is the average daily wage during the first month of re-employment. We find that from a half month before the exhaustion of UI to a half month after the exhaustion of UI, the average re-employment wage suffers a steep drop of approximately 3-4%. Observed workers' characteristics are balanced within a short window around the exhaustion of benefits and cannot

[5]Without successfully quantifying the wage selectivity response to UI exhaustion as the first step, we would underestimate the magnitude of the deterioration rate of labor market opportunities by 40%.

[6]The relevance of UI exhaustion increases as potential duration shortens, responsible for over 20% of the total drop in re-employment wages for workers with 4 months of UI. This suggests that for UI systems with short potential durations, like the US, the relevance of the exhaustion of UI could be relatively large in explaining the causal impact of time on re-employment earnings.

explain any part of the observed wage drop.

To causally estimate the impact of the exhaustion of UI on re-employment earnings, free from selection on unobservables, we employ a strategy combining difference-in-differences and regression discontinuity design. This strategy first defines the treatment and control groups using an RD design that exploits the discontinuous jumps in the policy schedule of potential UI duration. The treatment group is "exogenously" given two fewer months of UI compared to the control group, and will therefore first encounter benefit exhaustion. In the end, our strategy differences between how re-employment wages change across the treatment group, who have exhausted their UI, and the control group, who have not. Using this strategy, we find that the exhaustion of UI causes a significant decrease of 3.2% in daily re-employment earnings. When we replace our outcome variable of re-employment earnings by observed worker characteristics or time in unemployment to test for balance, we find no differential changes across groups around the exhaustion of UI. These pieces of evidence suggest that the sudden re-employment wage drop represents that the same group of workers suffering from a wage drop when expiring their UI, i.e., the causal effect of the exhaustion of benefits on re-employment earnings.

Second, we estimate the effect of a 2-month extension of UI duration on re-employment earnings and on time unemployed. We adopt a regression discontinuity design that again uses the discontinuities of the policy function that determine potential UI duration. Crossing a cutoff discontinuity is a quasi-experimental treatment that provides workers with two extra months of potential UI duration. We find that two extra months of potential duration have zero effect on re-employment earnings while increase time unemployed by approximately 27 days. Our balance test of observed characteristics finds no evidence of significant differences between workers in the treatment and control groups, or of workers' manipulation across the discontinuities.

In Section 4, we situate our empirical results in a standard direct job search model, and derive a sufficient statistics approach that links the two key mechanisms behind the decay of re-employment earnings with the causal impacts related to UI in Section 3. In this model, workers are hand-to-mouth and select their target wage and search effort to maximize their discounted expected utility. Workers are also subject to a time-dependent job-arrival production function, reflecting an ongoing deterioration of labor market opportunities. The optimal target wage in the model is affected only by the UI schedule and the arrival process of labor market opportunities. Within this model, we assume that the deterioration of labor market opportunities is smooth over the exhaustion of benefits. This assumption implies that firms do not discriminate against workers who have just exhausted their unemployment benefits[7] and that human capital does not depreciate discretely upon the exhaustion of benefits.

We use our model as a convenient illustrative tool to show the construction of our sufficient

[7]This does not imply that firms do not discriminate by unemployment duration, just that there is no discrete change in discriminatory behavior when UI exhausts.

statistics approach, adopting the following ideas. First, there is an equivalence between the sharp wage drop when workers expire their UI and increased wage selectivity in response to a UI extension.[8] The key to this equivalence is the assumption that regulates the smoothness of the deterioration rate of labor market opportunities, leaving target wage reactions as the only possible explanation for the sudden wage drop. Therefore, workers' react to the exhaustion of UI by sharply reducing their wage around 3.2%. Second, we express the deterioration rate of labor market opportunities as a function that takes as inputs the causal impact of UI extension and, more importantly, workers' wage selectivity response when expiring UI. The key idea behind this function, as shown in Schmieder et al. (2016) and Nekoei and Weber (2017), is that a UI extension affects workers' expected wages through two offsetting[9] channels: firstly, the wage selectivity increase to the expansion of UI benefits[10]; secondly, the decline in labor market opportunities as workers are induced by the UI extension to spend more time unemployed.[11] In our case, pinning down the workers' target wage reaction to a change in benefits allows us to identify, in isolation, the deterioration rate of labor market opportunities. When applied to our data, our function finds a 1.1% decrease per month in re-employment daily wages due to the deterioration of labor market opportunities.

At last, in Section 5, we structurally estimate the model, including heterogeneous agents and capturing the institutional features of the Spanish UI system, to recover the fundamental parameters and to evaluate policy implications. Our model estimation shows that 40% of workers have a discount rate of around 0.86 per half month. The estimated average target wage reaction following UI exhaustion is 3.2%, and it is primarily driven by myopic workers. Our estimate of the deterioration rate of labor market opportunities, directly from the model, is around 1.1% per month.

Using the estimated structural model, we conduct policy experiments that evaluate the effect on re-employment earnings of extending UI versus that of increasing UI. While we find that, in the aggregate, both types of policies have very small impacts on re-employment wages, the heterogeneity of these effects over the distribution of potential duration (entitlement) is very large. Extensions of UI have large positive impacts on re-employment wages for workers with short UI entitlements, while having negligible effects for workers with long UI entitlements. The opposite is true in the case of increases in the replacement rate, where wages for long entitlement workers increase significantly while workers with short entitlements see no changes. The rationale behind these heterogeneous effects comes from the behavior of myopic workers, for whom UI extensions work as a "mandatory savings tool", allowing them to sustain higher target wages for longer. In the case of replacement rate increases, myopic workers first increase their target wages dramatically while they receive UI, significantly lowering their probability of finding a job, but then drastically decrease their target wage once UI is set to exhaust. For long entitlement workers, this behavior pays off, since the

[8]The magnitudes are the same, the sign is flipped.

[9]The direction of the aggregate effect will depend on the relative size of the two components.

[10]A benefit extension implies a higher outside option, increasing target wages today and in the future.

[11]If longer periods of unemployment affect re-employment wages negatively, this would lower re-employment wages.

wage gains from increased target wages dominate the slightly increased probability of exhausting benefits and the resulting labor market opportunity harm. But for short entitlement workers, the components offset each other, since their probability of reaching UI exhaustion dramatically increases due to their short potential duration.

Literature. Our paper contributes to three strands of literature. First, it contributes to the empirical literature that studies unemployment dynamics and the value of non-employment. We are the first to document a sudden wage drop driven by UI benefit exhaustion and to argue it represents workers changing their target wages in response to the benefit ending. Previous work documents a spike in the hazard rate (Card et al. (2007)) and a consumption drop (Ganong and Noel (2019)) when unemployment insurance ends, but the evidence pertaining to re-employment wages is scarce.[12] On the other hand, the empirical literature related to the elasticity of wages to the value of non-employment is limited, and usually finds that changes in the value of non-employment have an insignificant effect on re-employment wages.[13] We contribute to this empirical literature by showing a significant and economically sizable causal effect of the exhaustion of UI on re-employment earnings, in the range of a 3% decline, within a month of UI exhaustion.

Second, our paper contributes to the literature that studies the causes behind the decline in re-employment earnings as unemployment duration increases, regardless of UI exhaustion. Both theoretically and empirically, we show that isolating the effect of UI exhaustion is essential to identifying the deterioration of labor market opportunities. As in Schmieder et al. (2016), the identification strategy comes from analyzing the causal impact of UI extensions 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 versus labor market opportunities. The former is policy-structure dependent, while we understand the latter as a primitive of the human capital depreciation (or signalling) process. We propose a novel estimator of the deterioration rate of labor market opportunities and argue that if we ignored the effect of the exhaustion of UI, we would underestimate the deterioration rate of labor market opportunities by as much as 50%.

Finally, our paper contributes to understanding the consequences of time-dependent UI interventions in two ways. First, we attempt to answer the policy debate of which policy tool is the most cost-effective at generating larger re-employment earnings: extending UI or increasing UI generosity. We highlight that policymakers can manipulate the timing of UI subsidies to reach better re-employment outcomes without necessarily incurring additional expenditure. Second, we show that even when the aggregate impacts on wages of both policies are similar, they vastly differ on the importance of each channel - the change in target wages and labor market opportunities - in

[12] Nekoei and Weber (2017) finds some evidence that re-employment wages decrease faster around the exhaustion of benefits, but falls short of empirically showing it or explaining why.

[13] Jäger et al. (2020) finds a positive but very low wage elasticity to UI replacement rate changes for both employed and unemployed workers. A notable exception is Nekoei and Weber (2017) that finds UI extensions significantly increase re-employment wages for workers who found a job near the exhaustion of UI.

creating the aggregate effect. We argue that separating the relative importance of these channels is critical for understanding the normative implications of different UI interventions.

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 two quasi-experiments to estimate the causal impact of UI exhaustion and the causal impact of UI extension. Section 4 outlines an illustrative model, introduces the channels underlying the decline in the re-employment wage over unemployment duration, and establishes the connection between them and the causal effects in Section 3. In Section 5, we estimate a structural model and conduct policy experiments. Section 6 concludes.

2 Data and Institutional Features

2.1 The Unemployment Insurance System in Spain

Unemployment insurance (UI) in Spain is characterized by two variables: the benefit level and the potential duration. Workers are entitled to receive unemployment benefits if they lose their previous job involuntarily and have worked at least one year during the previous six years.

By the potential UI duration, we mean the maximum duration for which one is allowed to receive unemployment benefits from the social security office. The potential duration is determined by one input, the number of days worked in the previous six years, regardless of whether it is full- or part-time work. Its value takes a wide range, from 4 months to 24 months. The relationship between the number of days worked in the previous six years and potential duration is not smooth, but based on multiple large discrete changes. For instance, if an individual works 539 days, they will have a potential duration of 4 months, while if they work 540 days, they will be entitled to a potential duration of 6 months. Table 1 summarizes potential duration as a function of tenure. After the exhaustion of UI, workers who are still unemployed can apply for unemployment assistance (UA). 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% of the minimum wage, for 6-20 months. For more details on UA, see Domènech-Arumí and Vannutelli (2021).

Table 1: **Unemployment insurance in Spain: potential duration**

	Days Worked in Previous 6 Years ($T_{tenure,i}$)										
From	360	540	720	900	1080	1260	1440	1620	1800	1980	>2160
To	539	719	899	1079	1259	1439	1619	1799	1979	2159	
	Potential Duration (B) (Months)										
	4	6	8	10	12	14	16	18	20	22	24

The unemployment benefit level is determined as a replacement rate of a 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 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, 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: Spanish Social Security Registry

In this project, 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 all individuals who have had any relationship with the Social Security Administration in the past year (i.e., workers, unemployed, retired individuals, and other benefit recipients). If an individual is selected for a given MCVL year, both her daily lifetime record of Social Security affiliations (i.e., all periods of employment, unemployment, self-employment, retirement, and benefit receipt up to the sample year) and her lifetime record of monthly Social Security wages per employer are provided. The combination of daily labor histories and monthly compensation allows us to create measures of tenure and daily compensation for every job. However, our measures of tenure and daily compensation are still subject to measurement error. This is because the Spanish Social Security registry is not specifically designed to provide researchers with the exact information – the work experience of the past 6 years and the average wage of the past 6 months – needed to compute the potential UI duration and the benefit level according to the exact definition of the eligibility criteria. We discuss the details of our solution to measurement error and its limitation in the next subsection.

[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.

[15] If a worker that enters unemployment has been unemployed in the previous six years, the worker is given two choices for benefit level and potential duration. Option one is the benefit level and potential duration defined above. But if the worker did not exhaust her benefits during her previous unemployment spell, she can choose to enjoy the remaining 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 afterwards (i.e., a previous salary of 1,500 EUR). The worker spends 4 months unemployed 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. Alternatively, she could choose to create a new bundle of benefit level and potential duration (i.e., a new claim) based on her last 3 years of employment. Her second choice would have 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.

2.3 Sample Restrictions

Using the historical records of the MCVL we build a sample of displacements (i.e., entries into unemployment that were preceded by a working spell) for the years 1994 to 2016. Our sample contains information for over 419,767 different unemployment claims corresponding to approximately 279,536 different individuals. Several restrictions are applied to reach this sample. First, we do not include any individual who has been self-employed for the past six years or who has had an affiliation that does not belong to the general regime of the Social Security since 2006[16]. Second, we discard any individual recorded as receiving negative wages[17] or holding a job in the public administration at any point during their lifetimes. Third, we discard those who simultaneously show a job (full- or part-time) and unemployment benefits, something that was possible at certain points due to very specific programs implemented by Social Security. Fourth, we eliminate those who never return to the labor market after an unemployment spell, as well as those spells in the top 2% of unemployment length. Fifth, we remove all temporary dismissals or contract suspensions, where firms can easily recall the workers back to their previous jobs.

We apply two additional restrictions to our sample. First, we remove workers who enter unemployment from a temporary contract whose previous tenure is an exact multiple of 6 months (i.e., half-year, one year, one and a half years, etc.). We apply this restriction for two reasons. First, workers whose previous contract had a predetermined length do not see the job separation as an exogenous shock. In expectation of the job separation, they may have held multiple jobs at the same time, they may have arranged a new job prior to the separation, or they may intend to use all UI benefits before job searching. Therefore, this group of workers has very different incentives, constraints, and information sets than does the rest of our sample, for who separation from their job came as an exogenous shock. Second, by design, workers whose previous temporary contract has a predetermined length systemically appear to the right-hand side of the discontinuities in Table 1. For instance, workers whose previous tenure is exactly 2 years (one of the most common types of temporary contract in our sample) appear 10 (or 11) days to the right of the policy discontinuity. This bunching of workers creates large spikes at certain points in the distribution of previous experience. We can clearly see this in the distribution of working experience (in the last 6 years), shown in panels (a) and (b) of Figure 1. One may argue that this spike is the outcome of workers manipulating their prior work experience to move from the left-hand side to the right-hand side of the discontinuity, in order to qualify for additional UI. We argue that these observed spikes do not arise as a consequence of manipulation, but by design, as a consequence of the typical duration of temporary contracts being a multiple of half a year (distinct from 180 days). We provide two direct pieces of evidence supporting our claim. First, we do not find any mirrored absence of workers ending their employment spells before the discontinuities. If workers could manipulate their prior

[16]Different unemployment benefit schedules apply to those individuals

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

work experience, one would expect not only an increased mass to the right of the discontinuity but also a missing mass to the left. Second, our observed spike does not happen directly after the cut-off, but approximately two weeks after. If workers could manipulate their prior work experience, they'd have no reason to continue working once they had crossed the discontinuities, at which point they are automatically entitled to 2 additional months of UI. One small caveat to removing these workers entering unemployment from temporary contracts with a predetermined length is that we eliminate temporary workers who are naturally dismissed exactly at the half-year multiples, even if their contracts were not expiring at that point. This occurs because we do not directly know the original length of a contract, but only observe its realized length.[18]. Removing these workers from our sample changes the distribution of previous work experience to that in panels (a) and (b) of Figure 2.

Last, we identify and discard a sample of unemployment spells for which their time collecting UI benefits is not consistent with their assigned potential duration. Specifically, we identify as measurement errors a group of unemployment spells where the worker claims UI for a duration that corresponds to the maximum UI of a different potential duration group. For instance, for a group of workers with an assigned potential duration of 240 days, we generate an indicator variable taking a value of 1 if a worker collects unemployment insurance for 120 days, 180 days, 300 days, 360 days etc., and after claiming unemployment benefits for that period the worker remains searching for a job. We remove these unemployment spells since they represent measurement error. For instance, for a worker with a (calculated) potential duration of 240 days, two options are possible: a) the worker might find a job prior to claiming her maximum length of unemployment insurance, but in that case, we would see her directly moving from collecting UI to working; b) the worker does not find a job within her (assigned) potential UI duration, but continues collecting UI up to a different maximum potential duration. Both these cases are the result of measurement error in calculating the length of the previous work experience, resulting in assigning the worker to a different maximum UI than she can actually claim.[19] Removing these spells does not significantly affect the distribution of the work experience variable. We can see the final distribution of our sample, after this restriction, in Figure 3.

Tables 3 and 4 present, respectively, the summary statistics of our key variables for the complete

[18] This problem should only affect a marginal part of our sample. Using the years after 2012, in which we can observe whether the worker actually enters unemployment after exhausting the length of her previous temporary contract, we find that over 80% of workers entering unemployment from temporary contracts do so after their contracts expire. For the other 20% to be wrongly removed from our sample, we would need them to be dismissed exactly at the duration of a typical temporary contract (1/2 year, 1 year, etc.), which is highly unlikely

[19] The underlying assumption here is that, as long as they remain unemployed, workers will always claim UI for an amount of time equal to their potential duration. There are scenarios in which it is possible that a worker does not claim UI for the entire length of her potential duration, yet she still remains unemployed. For instance, the worker might be denied UI based on not complying with some requirements imposed by the Social Security Administration, such as actively looking for a job, or if she has rejected more than 3 different jobs offered to her through the Social Security Administration. The Social Security Administration does not provide statistics on the relevance of this phenomenon, but anecdotal evidence suggests it is extremely uncommon.

(restricted) sample and for the final sample used for estimation.

3 The Effect on Re-employment Wages of UI Exhaustion and UI Extension

3.1 The Causal Impact of UI Exhaustion

The Sudden Wage Drop at UI Exhaustion: We begin our analysis by presenting the relationship between time unemployed and the associated re-employment wage. Our baseline measure of the re-employment wage is the average daily wage during the first month of the first re-employment job. [20]

Figure 5(a) presents average re-employment wages (y-axis) against time unemployed (x-axis) at a 15-day frequency, pooling together individuals with different potential durations of unemployment benefits. The y-axis is the average log re-employment daily wage. Overall, re-employment wages decline over the entire support of the time in unemployment. However, 5(a) hides the effect of UI exhaustion (marked by the red dashed lines: 4 months, 6 months, $\cdots$, 24 months, etc) on re-employment wages, since workers with different potential durations are pooled together as long as the realized unemployment duration is the same, regardless of whether they exhaust UI benefits or not.

To clearly show the evolution of re-employment wages around the expiration of benefits, we re-plot in Figure 5(b) the average log re-employment daily wage against time unemployed, but relative to each worker's assigned potential duration. Each point on the x-axis contains workers that exit unemployment with the same duration to UI exhaustion, regardless of their total time unemployed. For each point on the y-axis, we calculate their average re-employment wages. We observe an instantaneous wage drop of approximately 3.0–4.0% when UI is about to expire.

To see if the observed worker characteristics have any ability to explain how wages evolve at benefit exhaustion, we show in Figure 5(c) the residualized re-employment wage against time unemployed (relative to each worker's assigned potential duration). We create the residualized re-employment wage by running an OLS regression of log re-employment daily wage on worker and economy-wide characteristics, controlling for time unemployed [21] and taking its residual term. We find that the residualized re-employment wage still exhibits a steep drop of approximately 3.0–4.0 % right after UI expires, and conclude that worker characteristics cannot explain any part of the observed wage drop.

[20] For additional robustness tests based on other measures, Appendix B presents results using average daily wage from first year and first five years re-employed.

[21] In the graph, we control for time unemployed with a linear control. However, these results are robust to a wide range of specifications, including controls for time unemployed in different degrees of polynomials.

Identification and Estimation: While the previous evidence suggests that re-employment wages react to the exhaustion of UI benefits, it falls short of showing a causal impact of UI exhaustion on re-employment wages. Time in unemployment is an endogenous choice, subject to workers' self-selection. If workers select when to exit unemployment based on unobserved characteristics that also affect their re-employment wages, we could find this documented pattern, even if UI expiration does not affect the re-employment wage for any worker.

To attach a causal interpretation to our findings, we employ a difference-in-differences (D-in-D) estimation, where one of the differences arises from a regression discontinuity design (RD). The intuition of our identification strategy is as follows. We use RD to create a treatment group that exhausts UI first and a control group that exhausts UI later. As explained in Table 1, potential duration is a discontinuous function of the individual's work experience during the previous 6 years T_i^{exp}. When a worker's past work experience crosses certain cutoff thresholds $cutoff_i$, their potential duration increases discretely by two months. Our treatment group contains workers whose past work experience places them on the left-hand side of these thresholds, while workers in our control group have a slightly more work experience when they enter unemployment, and are located to the right-hand side of these thresholds. Control group workers are similar to treatment group workers except they receive two additional months before exhausting UI. We restrict both the treatment group and control group to be close to the corresponding cutoff thresholds, such that $|T_i^{exp} - cutoff_i| < h$. Therefore, these two groups of workers are very similar. Since individuals cannot choose their dismissal date[22], whether a worker receives 2 extra months of potential duration due to crossing the cutoffs provides us with "quasi-random" variation in potential duration.

With our treatment and control groups defined, we calculate the re-employment wage change for the treatment group before and after UI exhaustion. Similarly, we calculate the re-employment wage change for the control group before and after at the same length of unemployment duration. Both differences in wages are calculated in the time window in which the treated group has exhausted their unemployment benefits and the control group would have exhausted their unemployment benefits had they not qualified for two extra months of UI. The re-employment wage change for the control group is used by us as the counterfactual wage change for the treated group, had they not experienced UI exhaustion. In the end, our RD-in-Difference estimator is a result of a double difference between the two before-and-after wage changes.

The key assumption of our empirical strategy is that there is no selection process that is specific to the exhaustion of UI. This assumption rules out the case where some workers exit unemployment right before, or after, the exhaustion of UI, based on characteristics that also affect re-employment wages. Using this assumption, the global time indicator of before and after the exhaustion of UI can capture both the selection effect and other duration effects not due to the exhaustion of UI (for

[22] Only involuntarily dismissed workers can claim UI. Figure 1 to Figure 3 present our manipulation analysis in Section 2.3. There is no evidence of workers manipulating their dismissal date to gain two more months of UI. In Section 3.2, we also test the balance of worker characteristics around the different cutoffs.

example, the deterioration of labor market opportunities).

We first present the graphical evidence related to our empirical strategy. In Figure 11, we show the evolution of the residualized re-employment wage against time unemployed for workers in the treatment group and the control group. The residualized re-employment wage is generated from regressing the log re-employment wage (relative to the log daily wage of the pre-displacement job) on all observed characteristics of the workers and the economy. In panel (a) of Figure 11, we can clearly see that workers in the treatment group experience significantly lower re-employment wages when they are affected by benefit exhaustion. The pattern is clearer if we additionally control for time unemployed when we generate the residualized wages, shown in Figure 12. In panels (a) and (b) of Figure 12, starting 15 days prior to the exhaustion of benefits, workers in the treatment group see their relative re-employment wages fall until 45 days after the exhaustion of UI. At the 45-day mark, control group re-employment wages begin falling as they reach UI exhaustion. The largest re-employment wage gap between the treatment and control groups is 3.6% and happens exactly at the exhaustion of treatment group UI benefits. On average, the re-employment wage gap is approximately 2.5% for the period from -15 days to 45 days. Everywhere else (i.e., the past 105 days and prior 15 days), the re-employment wage pattern for the two groups of workers is very similar, indicating no differential pre-trend prior to the exhaustion of UI (and no clear differential post-trend either).

In practice, we estimate the following regression to recover the causal impact of UI exhaustion:

$$y_{it} = \gamma_0 \cdot E_{0,t} \times Treat_i + \sum_{j=1,2} \gamma_j E_{j,t} \times Treat_i + \theta Treat_i + \sum_{j=0,1,2} \beta_j^E \ E_{j,t} + \beta_b \cdot B_i + X_{it}\beta_X + \epsilon_{it} \quad (1)$$

for the sample $|T_i^{exp} - cutoff_i| < h$, where h denotes the RD bandwidth choice. In equation (1), y_{it} refers to the re-employment wage a worker i finds at time t, $E_{0,t}$ denotes whether the worker found a job prior to -15 days, relative to the timing of treatment group UI exhaustion, $E_{1,t}$ denotes whether the worker found a job prior to +45 days, again relative to the timing of treatment group UI exhaustion, and $E_{2,t}$ denotes whether the worker found a job prior to +105 days. B_i controls for potential duration fixed effects. Finally, X_i is a matrix that contains worker characteristics and economy-wide variables.

γ_0, associated with $E_{0,t} \times Treat_i$, is our estimate of the causal impact of UI exhaustion. More precisely, γ_0 captures the average effect on re-employment wages of exiting unemployment around the UI exhaustion cutoff, in a window from - 15 days to + 45 days around UI exhaustion. Here, we specify the timing of UI exhaustion to be 15 days before exhaustion of benefits, instead of at the exact point the benefits exhaust, consistent with the finding that the workers started reducing their targeted re-employment wage around 15 days prior to the exhaustion of UI. We include $E_{1,t}$ and $E_{2,t}$ as additional controls to ensure our analysis is not confounded by the exhaustion of UI within the control group, since their re-employment wages start falling two months after UI exhaustion of the treatment group, when their own UI benefits expire. In Table 8, we test the robustness of our D-in-D estimates to alternative definitions of the exhaustion window.

Table 8(a) presents the estimated γ_0 when using the largest possible RD bandwidth of 85 days (shown in columns (1)-(6)) and a bandwidth of MSE optimal (shown in columns (7)-(8)). Our results show that the causal impact of the exhaustion of benefits is a significant decrease in the daily re-employment wage of 3.2 percent. Adding controls for worker characteristics does not change the point estimate nor the standard error. The magnitude of this estimate is robust to the addition of time unemployed as a control (both parametrically and non-parametrically), to changes in the RD bandwidth, and to changes in the selected window around the exhaustion of UI.

Our identification assumption is that there is no self-selection of workers specific to UI exhaustion. In other words, if there is any selection on when a worker exits unemployment, this selection is the same in our treatment and control groups, and does not change when one of the groups approaches the expiration of UI. This is especially relevant in our setup, since the time when a worker re-enters the labor market is not exogenously determined, and incentives vary greatly around the exhaustion of benefits, making dynamic selection an important consideration. To relieve this concern for the exhaustion-specific selection problem, we estimate the same difference in differences specification described above, but replacing the outcome variables with worker characteristics. The idea is to check whether the observed discrete change in re-employment wages across groups could be driven by similar discrete changes in the observed worker characteristics across groups, instead of the causal effect of benefit exhaustion. Table 9 presents our results of the balance test. We find almost no significant differences in the change of any observed characteristic across groups, suggesting that the role of dynamic selection in explaining our results, if any, is limited[23]. In particular, when we have time unemployed as the outcome variable in the difference-in-differences regression, γ_0 returns an insignificant zero effect as shown in panel (c) of Table 8. This suggests that neither differences in observed characteristics nor the selection on who exits unemployment around the exhaustion of UI can explain the sudden wage drop. Moreover, the selection across any unobserved worker characteristics would need to affect re-employment wages, but not affect the total time in unemployment (locally), in order to invalidate our identification assumption.

Robustness: Table 8 examines whether the sudden drop in average re-employment wages at the point of UI exhaustion is driven by changes in hourly wages or by changes in hours of work. We find that at least 2/3 of the effect is a consequence of changes to hours worked, with less than 1/3 resulting from changes in hourly wages. Appendix B produces additional robustness tests for our results on the causal impact of UI exhaustion on re-employment wages. In Table B.3, we examine the long-term effects of the expiration of UI on workers' re-employment outcomes. Specfically, we examine the effect of UI exhaustion on the average re-employment wage during the first year after

[23] The only exception here is gender, but it plays a negligible role in driving the wage drop at the exhaustion of benefits. Evolution in the gender composition of the unemployed does not generate the sudden wage drop since we estimate the wage drop by gender and find them to be the same.

unemployment, and the average re-employment wage during the first five years after unemployment. We find the effect to be very similar to our main estimate for the first case, but 30 to 40% smaller when using average re-employment daily earnings over the first five years after unemployment. This suggests that the scarring effect of exiting unemployment around the exhaustion of benefits ends up fading after a certain time. Finally, we consider a subsample of workers that we observe collecting UI more than once. In Table B.4, we repeat our main exercise using this subsample, with and without the use of individual fixed effects. Our DiD estimates of the effect of the exhaustion of benefits on re-employment wages indicate that the inclusion of individual FE slightly increases (in absolute terms) our estimate (from 2.9% to 3.3%). This result suggests that dynamic selection (around the exhaustion of benefits) based on constant unobserved heterogeneity is not an important mechanism behind the wage drop observed when UI expires.

3.2 The Causal Impact of an Extension of UI

Identification of the causal effect of UI benefit extensions on the re-employment wage (and on time unemployed) is provided by discontinuous increases in the potential duration schedule. These discontinuous increases of two months' potential duration are generated when past work experience during the previous six years crosses one of the threshold cutoffs. Table 1 presents the detailed schedule, giving 10 discontinuities for 10 cutoffs.[24] These threshold cutoffs are multiples of 180 days, ranging from the first cutoff at 360 days to the last cutoff at 2160 days. Once work experience crosses a cutoff, potential duration discretely extends by two months, compared to a worker who does not cross the cutoff.

We start by combining all our discontinuities in one single specification, graphically showing in Figure 9 the effects of crossing the discontinuities on the re-employment wage (and on time unemployed). Panel (a) shows the average log re-employment wage (relative to the log of the previous wage) for individuals who do not cross the discontinuity versus those who cross the discontinuity and enjoy an additional two months of potential duration. The results show no clear change in the re-employment wage when crossing the thresholds. Panel (b) shows that workers close to the cut-offs and entitled to two extra months potential duration spend an additional 28 days in unemployment compared to those who do not cross the discontinuity.

Formally, to estimate the causal impact of a two-month UI extension, we estimate the following parametric RD regression:

$$y_{it} = \alpha \cdot \mathbb{1}(T_i^{exp} > cutoff_i) + \theta_1 f_l(T_i^{exp}) + \theta_2 f_r(T_i^{exp}) \times \mathbb{1}(T_i^{exp} > cutoff_i) + X_{it}\beta + \epsilon_{it} \qquad (2)$$

for the sample $|T_i^{exp} - cutoff_i| < h$. In equation (2), y_{it} denotes re-employment wages (and time

[24] We do not exploit the discontinuity at 359–360 days worked in the previous 6 years. The reason is the multiple changes in the last decade to the rules governing the subsidies for those without enough tenure SS to qualify for unemployment benefits.

unemployed), and $f^l(\cdot)$ and $f^r(\cdot)$ are smooth functions that control for the continuous relation between y_{it} and the running variable T_i^{exp}. X_{it} is a control matrix that contains worker characteristics and economy-wide variables. h denotes the bandwidth of choice, restricting our analysis to populations whose distance of T_i^{exp} to the closest cutoff threshold $cutoff_i$ is smaller than h days.

Table 5 presents the baseline empirical results from estimating the RD model of Equation (2). Consistent with what panel (a) and (b) of Figure 9 show, we find that an additional two months of potential duration does not have a significant effect on the re-employment wage but does significantly increase time unemployed by 30 days (RD robust estimator with the MSE-optimal bandwidth). Columns (3) and (6) of Table 5 present the same set of results but include all possible observed worker characteristics as control variables. For both outcomes, the estimated coefficients remain within each other's range of the standard error. We also present the same specification but force the bandwidth to be 85 days, aiming to include as many observations as possible in our estimation. Our results regarding both variables are very similar to the estimates using RD regressions with MSE-optimal bandwidth (columns (7)-(12)).

The identification assumptions of our estimation are that workers on the margin of the discontinuity cutoffs have very similar characteristics regardless of whether they cross the cutoffs or not, and that workers do not manipulate their job end dates to just cross one of the discontinuities. For the former, in Table 6, we present our balance test for different observed characteristics (age, gender, education, benefit level, etc.). We do not find a significant difference between workers who cross the discontinuity and those who do not when the bandwidth is MSE optimal.[25] For the latter, we discussed in Section 2.3 that we do not find systemic manipulation of workers from the left-hand side of the discontinuities to the right-hand side.

Robustness: Table 5 presents RD estimates using a non-parametric method. These estimates of the causal impact of a UI extension stay in a reasonable range of the baseline result. Second, we test the robustness of our estimates to changes in the choice of RD bandwidths. As shown in Table 5, our RDD estimates are robust to the choice of bandwidth. Third, in Appendix A.2 we present a discussion regarding measurement error of potential duration and its possible effects on our estimates. Fourth, Appendix B shows additional robustness results regarding the causal impact of a benefit extension. We examine whether a UI extension has a long term effect on re-employment outcomes. The average wage during the first year after re-employment and the average wage during the first five years after re-employment are used as the long-term outcome variables. In Table B.1, our results suggest that an additional two months of UI have a very similar impact on re-employment wages regardless of whether we consider the short term, medium term (1 year), or long term (5 years) outcome. Finally, we consider a sample of workers that collect unemployment insurance more than once. Using this sample we re-estimate the main results in the paper with

[25] Except for College, which is significant at the 10% level. However, if we further reduce the bandwidth to 18 days, the significance disappears, while the effect on time unemployed remains unaltered.

and without individual fixed effects. As shown in Table B.2, our RD estimates of the effect of UI extension on wages and unemployment duration remain unchanged.

4 A Conceptual Model

In this section, we present an illustrative framework that helps us connect the causal impacts of UI exhaustion/extension with the channels behind the negative relationship between re-employment wages and unemployment duration. First, we develop a representative agent model to introduce the two channels - the workers' reaction to UI exhaustion and the deterioration of labor market opportunities - that governs the optimal re-employment wage path. The model is based on Nekoei and Weber (2017) and DellaVigna et al. (2017). We borrow the direct job search set-up from Nekoei and Weber (2017) and combine it with the idea of present-bias/myopia from DellaVigna et al. (2017). In the model, dismissed workers, who are myopic, will set a target wage and exert effort to search for jobs, receiving UI benefits with limited duration and facing a deterioration of labor market opportunities over time. Then, we show how to map the effect of UI exhaustion/extension on re-employment wages into our proposed two channels.

4.1 Model Set-up

We consider a representative worker who was exogenously displaced from their previous job. Let $t = 0, 1, 2, \cdots$ denote the calendar time since entering unemployment. For each period t, a job seeker decides the amount of effort s_t used to search for a job and sets a target wage w_t. λ, the probability of finding a job for a given (w_t, s_t, t), is decreasing in wage selectivity, increasing in search effort, and also decreasing over time unemployed. Searching has a cost of $\Phi(s_t)$. We assume $\Phi(\cdot)$ is a weakly increasing, weakly convex, and twice differentiable function. Let c denote the level of consumption. The utility function from consuming while unemployed is $u(c)$, and utility while working is $v(c)$. We assume $u(c) \geq v(c)$ for the same c since workers prefers enjoying leisure. Let $b_t = \bar{b}$ when $t \leq B$ and $b_t = \underline{b}$ when $t > B$, where $\bar{b}$ denotes the level of unemployment benefit (UI), $\underline{b}$ denotes the level of unemployment assistance (UA) ($\underline{b} < \bar{b}$), and B denotes the potential duration of UI. Workers' static utility is connected intertemporally by a discount factor $\beta \in [0, 1)$. β can be arbitrary small to model the behaviour of myopia or present bias. Additionally, we assume that workers are hand-to-mouth.[26] Therefore, they will consume $c_t = b_t$ when unemployed and $c_t = w_t$ when working.

The value of being unemployed at t is:

$$U_t = \max_{w,s}\{-\Phi(s) + \lambda(w, s, t) \cdot V(w) + (1 - \lambda(w, s, t)) \cdot (u(b_t) + \beta U_{t+1})\} \tag{3}$$

[26] This is a natural steady state outcome of myopic workers' consumption choices.

where $V(w)$ denotes the value from working at wage w. For now, we assume that the worker starts working immediately once she finds a job. We will discuss the relaxation of this assumption in the appendix and consider the possibility of storing job offers.

$$V(w) = 1/(1-\beta) \cdot v(w) \tag{4}$$

To get analytical solutions, we make several structural assumptions. First, without loss of generality, we assume that $\Phi(s) = s$. Second, we assume that $\lambda(w, s, t)$ adopts the following form, given by Nekoei and Weber (2017):

$$\lambda(w, s, t) = a\left(t\right) \cdot s^{1-1/\sigma(t)} \cdot \exp\left(-\frac{V(w)}{\rho(t)}\right) \tag{5}$$

$\sigma(t) \in (1, +\infty)$ informs the elasticity of the job finding rate with respect to search effort. $\rho(t) \in (0, +\infty)$ is the (minus) semi-elasticity of the job finding rate with respect to wage selectivity $V(w)$. As we will show later, $\rho(t)$ is the parameter governing the labor market opportunities for a given re-employment wage, and is set to decrease with t.

The optimal search problem is now equivalent to choosing the target value from working, V, and search cost, s, such that the object in Equation (3) is maximized subject to Equation (5). Equation (6) shows the first order condition with respect to V:

$$V\left(w^*(t)\right) = u(b_t) + \beta U_{t+1} + \rho(t) \tag{6}$$

The target value from working depends on the value of non-employment, $u(b_t) + \beta U_{t+1}$, plus $\rho(t)$, the stock of labor market opportunities for different re-employment wages.

The first order condition with respect to s is:

$$1/\frac{\partial \lambda}{\partial s} = \left(V\left(w^*(t)\right) - u(b_t) - \beta U_{t+1}\right) \tag{7}$$

Combined with equation (7) and the definition of λ, we can solve for the optimal λ_t^* as a function of V^*:

$$\lambda_t^* = a(t)^{\sigma(t)} \cdot \left(\rho(t)(1 - 1/\sigma(t))\right)^{\sigma(t)-1} \cdot \exp\left(-\frac{V\left(w^*(t)\right)}{\rho(t)}\right) \tag{8}$$

Let's focus on Equation (6) to understand the evolution of the re-employment wage over time. Substituting (3) into (6) repetitively over time, we can express $V(w^*(t))$ as:

$$V\left(w^*(t)\right) = \underbrace{[u(b(t)) + \sum_{j=0}^{\infty} \beta^j \left(u(b_{t+(j+1)}) - s^*_{t+(j+1)}\right)]}_{\textbf{The effect of unemployment insurance}} + \underbrace{[\rho(t) + \sum_{j=1}^{\infty} \beta^j \lambda^*_{t+j} \rho(t+j)]}_{\textbf{The effect of labor market opportunities}} \tag{9}$$

Equation (9) shows that the optimal target wage can be decomposed into two components. First, the worker's utility from consuming UI benefits while continuing to search for jobs. Workers react by decreasing (increasing) their target wage when UI duration decreases (increases). Second, their labor market opportunity condition (both today and in the future, conditional on finding a job). Assuming that $\rho'(t) < 0$, then the longer one's unemployment experience is, the worse their labor market opportunities are. Therefore, the target wage decreases over time. To regulate $\rho(t)$ as a function of t, we make the following assumption:

Assumption 1. *The deterioration of labor market opportunities is smooth at the point when UI exhausts.* $\lim_{\Delta \to 0} \rho(t = B + \Delta) = lim_{\Delta \to 0} \rho(t = B - \Delta)$.

This assumption regulates how labor market opportunities evolve around the expiration of UI. It implies that, first, the worker's human capital does not depreciate discretely upon the exhaustion of benefits. Second, employers do not discriminate against workers based on their UI status, specifically, depending on whether they have finished claiming UI or not. Regarding the first implication, it is reasonable that the process through which human capital depreciates is continuous since, by staying idle in unemployment and not practicing their skills, workers can lose their skill over time. Therefore, human capital should be a smooth function of time unemployed, instead of a function of time left to UI exhaustion. The literature on skill formation and depreciation also uses this assumption underlying their empirical testing and model construction (e.g., Topel (1991), Ljungqvist and Sargent (1998), Nakajima (2012)).

For the second implication, we propose two reasons. First, how long one's UI lasts and whether one's UI has expired are usually a worker's private information. In field experiments from Kroft and Notowidigdo (2016) and Eriksson and Rooth (2014), no information on the length of potential UI duration or UI status is given by workers or reviewed by employers. It is hard to argue that, in the real world, resumes contain anything related to whether workers have just exhausted UI. To confirm, we collected information in both Spanish and English on "how to write a CV" and find that none of the writing guidance nor any sample CV contains information related to the worker's UI status. The only information that firms and employers have access to from CVs is the start and end dates for the historical jobs workers chose to list. Even if employers could deduce UI status based on this information, none of the field experiments finds any evidence of employers acting based on UI status. For instance, Eriksson and Rooth (2014) finds a continuous negative relationship between employers' job call rate and workers' unemployment duration in Sweden, notably smooth at the UI exhaustion threshold (300 days for Swedish workers). Therefore, it is unlikely that employers discriminate against those who have just exhausted UI. Second, if employer discrimination is an endogenous response to human capital depreciation (Jarosch and Pilossoph (2019)), it is hard to justify that there is an abrupt change in their beliefs over worker productivity if the true human capital of workers does not decline discretely at the exhaustion of UI benefits. Throughout the rest of this paper, we will maintain Assumption 1, the key assumption for our identification.

4.2 A Sufficient-Statistics Approach to Recover the Channels from UI Effects

In this section, we show how we recover the impact of each channel causing the re-employment wage decline using the effects of UI exhaustion and extension on re-employment wages.

Proposition 1. *Suppose Assumption 1 holds. Then the causal impact on re-employment wages of UI exhaustion equals the workers' reaction to the UI reduction at UI exhaustion. Mathematically,* $\lim_{\Delta \to 0} V^*_{B+\Delta|B+\Delta} - \lim_{\Delta \to 0} V^*_{B-\Delta|B-\Delta} = u(\bar{b}) - u(\underline{b})$.

Proposition 1 shows that the drop in the re-employment wage due to the exhaustion of benefits is equivalent to the reaction of the target re-employment wage to a benefit reduction, under Assumption 1. The intuition is simple. From Equation (7), we know that UI and labor market opportunities are the only two channels governing the evolution of re-employment wages. If labor market opportunities deteriorate continuously at the point of UI exhaustion as implied by Assumption 1, then the only factor that can generate a discontinuous re-employment wage drop has to be workers' target wage responding to the expiration of UI. Therefore, we can conclude that workers react to the exhaustion of UI by reducing their target wage by 2.8-3.5%.

Why is the re-employment wage drop so large at the exhaustion of UI? We argue that there exist myopic workers who have a small β to rationalize this finding. The workers' myopia can explain why they respond to UI exhaustion at the last moment. In expectation of a future income drop, a forward-looking agent will react well beforehand by saving more, putting more effort into searching for jobs, and decreasing their target wage. This reasoning has been used by Ganong and Noel (2019) to support the existence of myopic or present-biased agents, in order to explain the sudden spending drop at UI exhaustion. Studies that explain the hazard rate spike at UI exhaustion, such as Paserman (2008), DellaVigna et al. (2017), and DellaVigna et al. (2020), also use present-bias or hyperbolic discounting to model their findings. Our paper adopts the same idea and tries to use one small discounting factor to explain the sudden re-employment wage drop at UI exhaustion. At the same time, as illustrated in Figure 7(b), we find a large spike in the job finding hazard rate when UI exhauts, consistent with the literature that argues in favor of the existence of myopic workers.

With the workers' reaction to the exhaustion of UI pinned down, how can we estimate the deterioration rate of labor market opportunities? The key intuition behind identifying this is the idea posited by Nekoei and Weber (2017) and Schmieder et al. (2016). The impact of an extension of UI on expected re-employment wages represents the sum of two offsetting channels. First, in response to a UI duration increase, workers increase their target wage at each point of time, since they now enjoy a higher value of unemployment.[27]

$$\underbrace{\frac{dE(ln(w^*(t)))}{dB}}_{\text{The wage effect of a benefit extension}} = \underbrace{\sum_0^\infty (\frac{\partial ln(w^*(t))}{\partial B}\text{pr}(t))}_{\text{Increased target wage}} + \underbrace{\sum_0^\infty (ln(w^*(t))\frac{\partial \text{pr}(t)}{\partial B})}_{\text{The decline in wages over time}} \quad (10)$$

Second, an increase in UI duration lowers the relative price of unemployment, increasing time spent unemployed, reducing future wages due to either the deterioration of labor market opportunities or the exhaustion of UI.

Equation (11) illustrates this decomposition, which holds by definition of the total derivative. The two channels work in an offsetting way, such that the final effect on re-employment wages of a benefit extension can take either sign. In our empirical estimation in Section 2, we find a zero wage effect of a benefit extension. This implies that the channels cancel each other out, generating a null effect on the expected re-employment wage.

A nice implication of this decomposition is that, as long as we know the effect of UI exhaustion on re-employment earnings, we can identify the deterioration rate of labor market opportunities from Equation (10). To see how, we add some structure by assuming log-utility and linear $\rho(t)$. We can transform Equation (9) to the following parametric form of the re-employment wage function:

$$ln(w^*(t)) = ln(w^*(0)) - \Delta ln(\mathbf{w}(0)) + \frac{dln(w)}{dt} \cdot t + \sum_{\tau=0}^{B} \Delta ln(\mathbf{w}(\tau)) \cdot 1(t=\tau) \quad (11)$$

where $\Delta ln(\mathbf{w}(t))$ denotes the wage premium created by the existence of UI benefits at $t \geq B$, and $\frac{dln(w)}{dt}$ denotes the deterioration rate of labor market opportunities.

Combining Equation (10) and (11), Proposition 2 shows how to identify the deterioration rate of labor market opportunities, and how it is related to the causal impacts of UI extensions and exhaustions.

Proposition 2. *Suppose Assumption 1, log-utility, and linearity of decline in labor market opportunities hold.* ***Then, the deterioration rate of labor market opportunities*** $\frac{dln(w)}{dt}$ ***can be expressed as:***

$$\frac{dln(w)}{dt} \equiv f\left(\frac{dE(D^*)}{dB}, \frac{dE(ln(w^*(t)))}{dB}, \Delta ln(\mathbf{w}(B+dB))\right) + \mathbf{o}(\beta) \quad (12)$$

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

where

$$f\left(\frac{dE(D^*)}{dB}, \frac{dE(ln(w^*(t)))}{dB}, \Delta ln(\mathbf{w}(B+dB))\right)$$

$$\equiv \left(\frac{dE(D^*)}{dB}\right)^{-1} \cdot \left\{\frac{dE(ln(w^*(t)))}{dB} - \Delta ln(\mathbf{w}(B+dB)) \cdot pr(D^* \in (B, B+dB])\right\} \qquad (13)$$

$\frac{dE(D^*)}{dB}$ denotes the effect on unemployment duration (D^*) of a UI extension, $\frac{dE(ln(w^*(t)))}{dB}$ denotes the effect on re-employment wages of a UI extension, and $\Delta ln(\mathbf{w}(B+dB))$ denotes the target wage increase at $(B, B+dB]$ following a UI extension. $\mathbf{o}(\beta)$ is defined as following:

$$\mathbf{o}(\beta) \equiv -\left(\frac{dE(D^*)}{dB}\right)^{-1} \cdot \left(\sum_{\tau=0}^{B} \frac{\partial \Delta ln(w(t))}{\partial B} \cdot \mathrm{pr}(D^* = \tau) + \sum_{\tau=0}^{B} \Delta ln(\mathbf{w}(\tau)) \cdot \frac{\partial \mathrm{pr}(D^*=\tau)}{\partial B}\right) \qquad (14)$$

$\frac{\partial \Delta ln(w(t))}{\partial B}$ denotes the target wage increase at $t = 1, \cdots, B$ from a UI extension, and $\frac{\partial pr(D^*=\tau)}{\partial B}$ denotes the probability decrease of finding a job at $t = 1, \cdots, B$ due to a UI extension.

Proposition 2 shows that the deterioration rate of labor market opportunities can be expressed as the sum of two pieces. First, a function $f()$ of $\frac{dE(D^*)}{dB}$, $\frac{dE(ln(w^*(t)))}{dB}$, and $\Delta ln(\mathbf{w}(B+dB))$, which are the causal effects of a UI extension and exhaustion, estimated in Section 3. Second, $\mathbf{o}(\beta)$, which denotes the sum of two terms. Within $\mathbf{o}(\beta)$, the term $\sum_{\tau=0}^{B} \frac{\partial \Delta ln(w(t))}{\partial B} \cdot \mathrm{pr}(t = \tau) \geq 0$ captures the response of the target wage to a future extension of UI benefits in all periods prior to the extension of benefits. The second term of $\mathbf{o}(\beta)$, $\sum_{\tau=0}^{B} \Delta ln(w(\tau)) \cdot \frac{\partial \mathrm{pr}(t=\tau)}{\partial B} \leq 0$, captures the decline in re-employment wages over time due after an extension of UI, comprised of a decrease in search effort in periods before UI exhausts and a higher probability of exhausting UI benefits. The two terms have different signs, and leave the direction of $\mathbf{o}(\beta)$ undetermined.

4.3 Estimation and Implications

Equation (12) allows us to estimate the deterioration rate of labor market opportunities.

4.3.1 Estimation of $\mathbf{o}(\beta)$:

We start by estimating $\mathbf{o}(\beta)$. For the left-hand side term $\sum_{\tau=0}^{B} \frac{\partial \Delta ln(w(t))}{\partial B} \cdot \mathrm{pr}(D^* = \tau)$, the key is to estimate $\frac{\partial \Delta ln(w(t))}{\partial B}$. Taking advantage of the theoretical equivalence that $\frac{\partial \Delta ln(w^*(t))}{\partial B} = \frac{\partial ln(w(t))}{\partial B}$, we estimate the change in wage selectivity at τ to UI extensions.

Empirically, to estimate the changes to workers' wage selectivity in periods before unemployment insurance exhausts, we use a similar empirical specification to the one shown in Section 3.1. Specifically, we re-estimate an updated version of Equation (1) where, for each specification, we add a dummy, $E_{-p,t}$, and its interaction with the dummy for the treatment group, $E_{-p,t} \times Treat_{i,t}$.

[28] $E_{-p,t}$ takes a value of 1 if the worker is re-employed in month $-p$ prior to the exhaustion of benefits (i.e. -1,-2... etc.), and 0 otherwise. Our coefficient of interest is γ_{-p}, which we recover for each period in a separate specification. The estimates are plotted in Figure 13 and given in Table 10. All the coefficients are very close to zero and insignificant.

All assumptions outlined and tested in Section 3.1 need to be satisfied for γ_{-p} to recover the causal effect of the exhaustion of UI on wage selectivity in month $-p$. Furthermore, we require one additional assumption. Workers exiting unemployment in month $-p$ need to be identical except for their assigned potential duration. Thus, as in Section 3.1, our empirical specification is robust to the dynamic selection of workers in when they exit unemployment, as long as that dynamic selection over time is the same in the treatment and controls groups.

To test this assumption, we proceed in two ways. First, we estimate Equation (14) with and without controls.[29] We find that both with and without controls, the coefficient of γ_{-p} barely changes and remains insignificant for all periods (column (3)). Second, we replace the outcome variable and instead use unemployment duration (column (1)). In this case, we can interpret the coefficient of γ_{-p} as the difference in unemployment duration, for workers re-employed in month $-p$, between the treatment and control groups. While we find limited differences in the last 6 months prior to UI exhaustion, we observe significant differences in the months before. To account for this difference, we add unemployment duration fixed effects to our specification in Equation (14). We find that the effects on wages (column (4)) change little and remain insignificant, sliding even closer to zero.[30]

For the right-hand side term $\sum_{\tau=0}^{B} \Delta ln(\mathbf{w}(\tau)) \cdot \frac{\partial \text{pr}(D^*=\tau)}{\partial B}$, we need to estimate both $\Delta ln(\mathbf{w}(\tau))$ and $\frac{\partial \text{pr}(D^*=\tau)}{\partial B}$. Starting with $\frac{\partial \text{pr}(D^*=\tau)}{\partial B}$ (i.e., the change in the probability of exiting unemployment in period τ prior to exhaustion of UI), we first calculate separately the hazard rates (relative to the exhaustion of UI in the treatment group) for the treatment and control groups. This is shown in Figure 14 (a). We find very similar hazard rates for both groups over the distribution of time unemployed, for periods before UI benefits expire. To test whether the month by month difference

[28]

$$y_{it} = \beta_{-p}^{E}\ E_{-p,t} + \gamma_{-p} \cdot E_{-p,t} \times Treat_i + \gamma_0 \cdot E_{0,t} \times Treat_i + \sum_{j=1,2} \gamma_j E_{j,t} \times Treat_i + \theta Treat_i + \sum_{j=0,1,2} \beta_j^{E}\ E_{j,t} + \beta_b \cdot B_i + X_{it}\beta_X + \epsilon_{it} \quad (15)$$

[29] See Appendix B, Table **??**

[30] While insignificant differences period by period suggest that there is no effect of UI exhaustion on re-employment wages prior to the exhaustion of benefits, it is possible that, cumulatively, those differences become relevant over time. We test for this using a modified version of Equation (1) where we add a dummy, $CE_{-p,t}$, that takes a value of 1 if the worker is re-employed between month $-p$ and the last month, -1, (relative to the exhaustion of benefits), and 0 otherwise. As before, we also add the interaction of this dummy with the dummy for the treatment group $CE_{-p,t} \times Treat_{i,t}$. Our coefficient of interest now captures the cumulative difference in the re-employment wage between the treatment and control groups, from month $-p$ to the last month prior to the exhaustion of benefits. The results are shown in Appendix B, Table B.5 and Figure 13. We find some small (cumulative) differences between groups when we combine the last 4 or 5 months prior to UI exhaustion, in the range of 1% lower re-employment wages for workers that reached UI exhaustion 2 months earlier. These differences disappear if we consider any other number of months.

in the hazard rate is significant, we bootstrap our sample 200 times and calculate the difference in the hazard rates for each sample. Figure 14 presents the estimated differences, along with 95% CIs. The period differences are small and insignificant up to 240 days prior to the exhaustion of benefits. On the other hand, in the final months (-240 days to -30 days) we find significant differences, with the hazard rate in the treatment group being 6% (or 0.03pp on average) larger than in the control group.

While the previous evidence suggests that there are some small differences in the hazard rates (prior to UI exhaustion) between the treatment and control groups, it does not allow us to control for differences in observables across groups. Similarly, the period by period differences do not provide an answer as to whether these differences accumulate over time or cancel each other out. To get a sense of the cumulative magnitude of the difference in hazard rates between groups prior to UI exhaustion, in Table 11 we estimate Equation (2), using the cumulative probability of exiting unemployment from period τ to period -1 as the outcome of interest. Our preferred specification, column (2), suggests that (cumulatively) there is a 3pp difference between groups in the probability of exiting unemployment prior to UI exhaustion in the treatment group.

α recovers the causal impact of an additional 2 months of potential UI duration on the cumulative probability of exiting unemployment prior to the exhaustion of benefits if the assumptions outlined in Section 3.2 are satisfied and there's no differential dynamic selection across groups prior to the exhaustion of UI in the treatment group. This latter assumption is required here to avoid selection in the outcome variable, something previously unnecessary since we did not have any "timing" restrictions on our outcomes of interest.

Turning to $\Delta ln(\mathbf{w}(\tau))$, (i.e., the change in the re-employment wage in period τ in the absence of UI), we lack the type of variation that would allow us to provide a causal estimate of its value in our sample. Nevertheless, we can infer its approximate value from different moments in our data, and we test the sensitivity of our results to the chosen value (see the sensitivity subsection at the end of Section 4.2). Our preferred approximation of $\Delta ln(\mathbf{w}(\tau))$ comes from the wage drop upon the exhaustion of UI. The peak decline reaches -4%, as shown in Figure 13(b). Furthermore, we consider two alternative options. First, we estimate a linear regression to recover the effect of the exhaustion of benefits on re-employment wages, after linearly controlling for time unemployed and all other covariates we observe. In this case we obtain a value for $\Delta ln(\mathbf{w}(\tau))$ of -8.2%. Second, we take the residuals of the previous specification and regress them against time unemployed (relative to the exhaustion of UI). This gives us a slope of -3.8%, that we add to our previous estimate (-8.2%-3.8%=-12%). We consider this case the worst-case scenario, since we assign all the unexplained decrease in re-employment wages over time, but prior to UI exhaustion, to the effect on re-employment wages of the absence of UI.

Using our preferred estimates for each of the different components[31] of $\mathbf{o}(\beta)$, we find that

[31] See the sensitivity subsection at the end of Section 4.2, for our estimates of the DLMO in the other cases.

$\mathbf{o}(\beta) = -1.2e^{-3}$. This number is one order of magnitude smaller than the remaining pieces in Equation (12). Empirically, workers respond very little beforehand in their target wage and search effort to the UI extensions in the future. We argue that this is the natural outcome from having a fraction of the workers almost fully myopic $\beta = 0$ (at least for workers who cannot find a job prior to the exhaustion of UI). A nice property associated with $\mathbf{o}(\beta)$ is that, theoretically, it approximates 0 as β approximates 0. The argument is simply that when workers are completely myopic, $\beta = 0$, so they don't respond at all beforehand, ($\frac{\partial \mathrm{pr}(D^*=\tau)}{\partial B} = 0$ and $\frac{\partial w^*_\tau}{\partial B} = 0$ for $\beta = 0$), since a UI extension is a wholly unpredictable shock to them.

4.3.2 Estimation of $f()$:

Therefore, as suggested by our empirical evidence, we can express the deterioration rate of labor market opportunities as that of a worker with $\beta = 0$:

$$\frac{dln(w)}{dt} = \left(\frac{dE(D^*)}{dB}\right)^{-1} \cdot \left\{\frac{dE(ln(w^*(t)))}{dB} - \Delta ln(\mathbf{w}(B+dB)) \cdot \mathrm{pr}(D^* \in (B, B+dB])\right\} \quad (16)$$

Combining all the causal impacts related to UI extension into Equation (14), we recover the deterioration rate of labor market opportunities. The results are shown in Table 12 (point-estimates) and Table 13 (bootstrap distribution). Our findings suggest that one additional month of time unemployed leads to a 1.2 percent decrease in the re-employment wage. This is equivalent to a 14.0 percent reduction in the re-employment wage for 1 additional year of unemployment experience.

How does this estimater speaks to the prior literature? Schmieder et al. (2016) proposes an IV estimator that uses UI benefit extensions as an instrument to calculate the causal effect of unemployment duration on re-employment wages. There are two major differences between their work and our paper. First, we intend to separate the deterioration of labor market opportunities and the effect of UI exhaustion. Schmieder et al. (2016) seeks to only estimate an average causal effect of unemployment duration on re-employment wages, which consists of the two component channels we separate out. Further, while their IV estimate is a lower bound for the causal effect of unemployment duration on re-employment wages, if we are interested in the deterioration rate of labor market opportunities isolated from the effect of UI exhaustion, the IV estimate no longer acts as a lower bound of the deterioration rate of market opportunities. Second, in their study, the increased target wage $\Delta ln(\mathbb{w})$ is taken as zero.[32] Their theoretical framework centers around workers respond little to UI changes since the reservation wage is not binding. In this case, the estimator of the deterioration rate of labor market opportunities in Equation (12) degenerates to an IV estimator that uses UI extensions as an instrument. In comparison to their work, we do not put any restriction on the size of $\Delta ln(\mathbb{w})$ and recover it from establishing the relationship between

[32]This assumption in Schmieder et al. (2016) might be reasonable since they do not find a significant and persistent wage drop in their empirical set-up.

the sudden wage drop upon UI exhaustion and the causal effect on re-employment wages of UI exhaustion. For our empirical set-up, in Table 14, we compare the estimates derived from Equation (14) versus those using the IV estimator proposed by Schmieder et al. (2016), derived by assuming no worker target wage responses. We find that their IV estimator underestimates the deterioration rate of labor market opportunities by 28 - 55% compared to our estimates.

Robustness: We test the sensitivity of our estimate of the deterioration rate of labor market opportunities to the approximation of $\sum_{\tau=1}^{B} \Delta ln(\mathbb{w}(\tau)) \cdot \frac{\partial \mathrm{pr}(D^*=\tau)}{\partial B}$. As explained above, since we cannot causally estimate $\Delta ln(\mathbb{w}(\tau))$, we proposed different alternatives to approximate its value. Here, we pay special attention to the worst-case scenario. Using our estimate of $\Delta ln(\mathbb{w}(\tau))$ for the worst-case scenario, (12%), we find that our obtained deterioration rate of labor market opportunities changes from -1.2% to -1.0%. If we further decide to use the estimated value for $\sum_{\tau=0}^{B} \frac{\partial \Delta ln(w(t))}{\partial B} \cdot \mathrm{pr}(D^* = \tau)$ (even if it is insignificantly different from zero) and add it to our estimation of the deterioration rate of labor market opportunities, we find $o(\beta) = 3e-4$, leaving the estimated rate of deterioration of labor market opportunities unchanged at 1.2%.[33]

5 Summary Of Reduced-Form Results

Combining Proposition 1 and the empirical estimates from Section 3.1, we can directly conclude that the workers react to benefit exhaustion by reducing their target wage by around 2.8 - 3.5%. Our estimated re-employment wage elasticity to a change in UI benefits (dollar-to-dollar) is around 0.07-0.08, around 2-3 times larger than the point estimates from Jäger et al. (2020), and much larger than the estimates of Marinescu and Skandalis (2021), who finds that re-employment wages decline by 2.4% within the year before the exhaustion of UI.

Using Proposition 2 and the empirical results of Section 3, we find that one additional month of unemployment leads to a 1.0 to 1.2 percent decrease in the re-employment wage. This is equivalent to a 12-14 percent reduction in the re-employment wage for 1 additional year of unemployment experience.

6 Structural Model Estimation and Policy Experiments

6.1 Model Set-up

In this section, we structurally estimate the theoretical model introduced above by matching it to the key moments from the data. While our primary goal is to conduct policy experiments to understand the effect of UI interventions at different points in time, the structural estimation

[33] From a theoretical perspective it is more reasonable to think that both terms in $ø(\beta)$ are either zero or different from zero.

of the model also allows us to relax some of the previous assumptions. First, our reduced-form estimate of the deterioration rate of labor market opportunities relies on arguing that the last two pieces in Proposition 2 are negligible. Even though our data suggests this is a reasonable empirical approximation, from a modelling perspective this implies that agents behave as if they were almost fully myopic. Our structural estimation allows us to relax this assumption by directly estimating the discount rates of workers, while still independently recovering the depreciation rate of labor market opportunities.

Second, in the model, we allow heterogeneity in the discount rate, β. Workers' self-selection on β over time can help us rationalize the wage drop at UI exhaustion, since myopic workers will tend to find a job around the exhaustion of UI, while forward-looking workers will exit unemployment earlier. In this case, the wage drop at the end of UI will depend only on the response to the exhaustion of UI among myopic workers. However, conducting policy experiments other than UI extensions requires us to understand the overall distribution of target wage elasticity, not just the local DiD estimate upon exhaustion of UI. We explicitly model the heterogeneity over β and the associated consumption mode.

Third, Proposition 2 implicitly assumes that the deterioration rate of labor market opportunities is homogeneous across time and agents. If this is not true, similarly to Schmieder et al. (2016), our proposed estimator is a weighted average of the deterioration rate of labor market opportunities. The weight is larger for those whose probability of finding a job responds more to an extension of UI. This implies that alternative policy experiments, that induce different compliers, could result in a different reduced form estimate of the deterioration rate of labor market opportunities. Any policy that alters the composition of compliers such that the response of the job finding probability changes would result in a different reduced-form estimate, even if the underlying deterioration of labor market opportunities within groups remains the same. We deal with this issue by considering heterogeneity in the deterioration rate of labor market opportunities across agents, based on their prior work experience.[34] The identification of these parameters comes from the differential effects of UI extensions and exhaustions at each policy discontinuity, that generate different estimates of the deterioration rate of labor market opportunities associated with each instrumental variable.

Finally, Proposition 2 applies to a marginal change in potential duration. In reality, the exogenous variation from crossing the threshold cutoffs increases potential UI duration by two months, a large discrete magnitude. Since utility is concave in the level of consumption, our reduced form approximation, using discrete policy changes, might not be correct.

Table 16 summarizes the parametric assumptions we make on top of the system of Equations (1), (2), and (3). As mentioned, we incorporate two sources of heterogeneity into the model. First,

[34] For now, we do not specify a nonlinear deterioration rate over time, since we have not found a way to separately identify it. For the purpose of estimating aggregate re-employment wage gains, without trying to distinguish whether they are driven by individual sorting or by changes to the depreciation rate of human capital, allowing heterogeneity across workers or across time are isomorphic to each other.

we add a layer of heterogeneity in workers' consumption behavior: myopic or forward-looking. There are two types of workers, L and H. An L-type worker has a smaller discount factor, β_L, and consumes hand-to-mouth as described by our model. The lower β_L, the more myopic the individual is, and the larger the magnitude of the re-employment wage reaction to UI exhaustion. An H-type worker has a larger discount factor, $\beta_H = 0.99$,[35] and will perfectly smooth their consumption. However, our model lacks the ability to model consumption smoothing since we are making a "hand-to-mouth" assumption. To compensate, we assume that workers with β_H will always consume $\underline{b} + \frac{B(\bar{b}-\underline{b})}{T}$ as long as they remain unemployed, regardless of whether they are claiming UI or not. The idea of this assumption is that unemployed workers will see unemployment assistance income as permanent income, treating $\bar{b}-\underline{b}$ as the temporary income shock to be smoothed out.[36] Fraction pr_L of the workers are L-type and fraction $\text{pr}_H = 1 - \text{pr}_L$ of the workers are H-type.

Adding heterogeneity in the discount factor and the consumption mode requires us to have different $(a, \sigma, \boldsymbol{\rho})$ for β_L and β_H. The reason is that the search technology function is linked to the discount factor, since it directly targets the expected future value of unemployment (holding future opportunities the same, a higher discount factor implies a larger value of unemployment), instead of the re-employment wage. In practice, if we force the two sets of parameters to be identical, we find that for the same search effort and target wage, the probability of finding a job is vastly different for each type of worker. Therefore, we will have a set of parameters $(\beta_L, a_L, \sigma_L, \rho_{0,L}, \rho_{1,L})$ for L-types and a set $(\beta_H, a_H, \sigma_H, \rho_{0,H}, \rho_{1,H})$ for H-types.

Second, we introduce some sparse heterogeneity in the human capital parameters (ρ_0, ρ_1), by allowing workers with short entitlements (potential durations of 6, 8, 10, 12, or 14 months) and long entitlements (potential durations of 16, 18, or 20 months) to have a different set of parameters.

6.2 Estimation

With this structural model, we aim to fit the model to match three series of moments $(\hat{m})$. First, the hazard rate over time for each potential duration (entitlement) group. Similar to studies that consider the evolution of the hazard rate over the unemployment spell, we aim to recover the spike in the hazard rate around the exhaustion of UI. Second, the re-employment wage over time for each entitlement group. Here, notice that the sudden wage drop at UI exhaustion is naturally included in the evolution of re-employment wages within each potential duration group. For the first two sets of moments — hazard rate and re-employment wage — we calculate them at the 15 -day level. Last, we consider the causal impact of UI extensions on unemployment duration and on re-employment wages, estimated in Section 3.

[35] We chose 0.99 by setting $\beta_H \cdot (1 + r) = 1.0$, where $r = 1.013$ (monthly) comes from Hernandez-Martinez and Liu (2021)

[36] In our preferred estimation of the model, forward-looking types will smooth out the income shock from UI over a number of periods corresponding to an individual in the 90th percentile (by potential duration) of time unemployed in our sample.

We estimate the parameters denoted by θ to best fit our chosen moments $\hat{m}$. Our estimator solves the following problem:

$$\hat{\theta} = \underset{\theta}{argmin}(m(\theta) - \hat{m})'W(m(\theta) - \hat{m})$$

Figure 18 and 17 present the model-predicted re-employment wage evolution and the hazard rate, relative to the exhaustion of UI, and compare it with the data. Table 18 presents the model-predicted effect of UI extensions on time unemployed and re-employment wages, compared to the RD estimates from the data. Overall, the model produces a good match to the data, with reasonable estimates of the evolution of the hazard rate and the re-employment wage for each potential duration group.

Figure 19 presents the predicted evolution of the hazard rate for each potential duration group, and Figure 20 presents the predicted evolution of re-employment wages. The model captures the empirical fact that the re-employment wage drop is larger for higher duration groups. In Figure 20, we illustrate how the two types of workers — forward-looking and myopic — shape the evolution of the hazard rate and re-employment wage. Workers who are forward-looking exert a large amount of search effort from the beginning, set a lower target wage, and exit unemployment early. On the other hand, myopic workers set a higher target wage at the beginning of their unemployment spell, because of the outside option of UI, while exerting less search effort (compared to non-myopic workers). Once they approach the exhaustion of UI, myopic workers drastically reduce their target wages and increase their search effort, generating a spike in the hazard rate. The separate dynamic selection of the two groups implies that the re-employment wage and hazard rate early in the unemployment spell are governed by forward-looking workers. Conversely, myopic workers determine these moments later in the spell, especially around the exhaustion of UI. This result suggests that a model with homogeneous workers will not fit the data correctly, since it will either underestimate the hazard rate early in the spell, or underestimate the spike in the hazard rate around the exhaustion of UI (as well as the wage drop upon UI exhaustion).

6.3 Model Finding

Table 17 presents our estimated parameters. Our estimation shows that 40% of the population in our sample has a half-month discount factor of 0.86. Our discount factor is at the lower end of the literature, though is of a similar magnitude as in Ganong and Noel (2019). We find that the ATE for the deterioration rate of labor market opportunities is 1.2% per month, within the range of our bound estimator. Type-L workers experience a 0.46% re-employment wage decline per month. Type-H workers have a 1.71% re-employment decline per month. The target wage reaction at UI exhaustion is around 3.2%. The ATE for the target wage reaction at UI exhaustion is around 1.28%.

6.4 Policy Experiments

We use the estimated model to conduct policy experiments analyzing different UI interventions. We evaluate the effectiveness of these UI interventions based on the re-employment wage gains each creates, given the same amount of fiscal expenditure. Policymakers care about re-employment wage gains for two reasons. First, it directly affects the well-being of the unemployed. For a previously unemployed worker, their first several months' earnings from their re-employment job might be the most essential source of income to support their daily consumption, and might also severely affect their self-reported happiness. Second, larger wage gains lead to a positive fiscal externalities (Nekoei and Weber (2017)), and this tax revenue is not internalized by workers when they search for jobs. [37]

We compare the effectiveness of two different policy instruments: UI extensions and increases to the UI replacement rate (RR), since these two policy instruments are the most common forms of UI interventions. Each type of policy has received significant attention empirically and theoretically in isolation, primarily from a moral hazard perspective (Chetty (2008)), but comparisons between the two policy instruments in the same framework are far less common.[38] Despite the similarity of these two interventions, in that both provide additional income to the unemployed, UI extensions should be treated as an income transfer from the relatively distant future, while replacement rate increases should be treated as an income transfer from the relatively near future (Landais (2015)).

To compare the effects the different policies have on the re-employment wage, we force ex-post fiscal expenditure to be identical across policy interventions. The ex-post fiscal expenditure considers not only the additional expenditure that arises mechanically from increasing benefits, but also the workers' changing search behavior. Specifically, in policy experiment 1, we first extend the potential duration of UI by two months for every worker and calculate the resulting additional fiscal cost, after the workers have optimized their behavior according to the new policy. On average, this policy experiment costs an additional 885 euros per worker, although it varies significantly across potential duration groups. Next, in policy experiment 2, we increase the replacement rate for each potential duration ("entitlement" for short) group. We do this by increasing the replacement rate such that it generates the same ex-post expenditure, within each group, as policy experiment 1. Note that means different groups of workers with different potential durations thus receive different increases to benefits. Figure 21 presents the effects on the re-employment wage of the different policy experiments.

Our first finding from the policy experiments comparison is that the aggregate effects of both

[37] Shimer and Werning (2007) shows that in a certain model without duration dependence, the effect of UI on wages is a sufficient statistic for designing an optimal UI system. However, this optimality claim requires assumptions, including a functional form for welfare, so we instead only present positive statements regarding the effectiveness of UI, instead of discussing its normative implications.

[38] Within our knowledge, only Ganong and Noel (2019) compares the effect on consumption smoothing of a UI extension and a replacement rate increase.

policies are small[39], with both policies yielding negligible effects on re-employment wages. While the aggregate effects of both policy experiments are similar, there is a large degree of heterogeneity in their effects on re-employment wages for workers of different entitlements. For instance, for workers with short UI entitlements, UI extensions have small positive impacts on re-employment earnings (0.3%), while equivalent changes to the UI replacement rate have a small negative impact (-0.5%). The opposite is true for workers with long UI entitlements, for whom UI extensions have almost no effect on re-employment earnings (0.1%), while increases to the replacement rate have large positive effects (1.0%).

Next, we decompose the effect on re-employment earnings into two channels: changes through target wages, and changes through labor market opportunity losses, shown in Panel (a) and (b) of Figure 22. First, we find that a UI extension results in longer unemployment spells, and therefore larger deteriorations of labor market opportunities, than a RR increase. The former increases time in unemployment by almost 25 days, versus 15 days for the latter. This directly translates into larger losses to the re-employment wage via labor market opportunity loss, with a UI extension decreasing re-employment wages by approximately 0.9% through this channel, compared to 0.6% for changes to the RR. Second, as expected, both policies have positive impacts via the wage selectivity channel, but the magnitudes vary significantly across policies. Changes to potential UI duration raise re-employment wages by almost 1% through this channel, against 0.6% in the case of changes to the RR. In summary, while the overall effect on re-employment wages is similar for the different UI interventions, there's an important degree of heterogeneity in a) the mechanisms behind those effects (wage selectivity versus labor market opportunity loss) and b) the effects of each policy across different potential duration groups.

Why do different policies affect wage selectivity differently? It all comes down to the behavior of myopic workers. For them, a UI extension acts as a mandatory savings tool, while an increase in the RR rate does the opposite. Consider first myopic workers with short entitlements. A UI extension increases their wage selectivity a little at the beginning, which generates a very small reduction in the probability of finding a job early in the spell. But at the same time, a UI extension allows them to continue being selective about wages, significantly increasing the re-employment wage for those who would have exited unemployment near to the original exhaustion of benefits, but who now exit within the last few months of UI. In our case, this second force dominates the first, generating a positive impact on re-employment wages for workers with short potential durations. In the case of workers with long entitlements, the forces at play are the same, but, by the time they reach the exhaustion of their benefits, most have already exited unemployment, implying the possibility of remaining selective about wages for longer is almost irrelevant for them. UI extensions work as "forced" savings tools for myopic workers. These "forced" savings help short entitlement workers, since most of them would have reached the expiration of benefits under the original policy system.

[39]Previous work estimates the causal impact of job loss on daily wages to have at least 50 times (in absolute terms) the effect on daily wages than do the different policy changes found in this section.

Saving is almost irrelevant for long entitlement workers, because most of them exit prior to the original exhaustion of benefits.

In the case of a replacement rate increase, the effect is the opposite of a "forced" savings tool. For myopic workers with short entitlements, the large benefit level dramatically raises their wage selectivity early in the spell, severely limiting their chances of finding a job quickly. By the time UI expires, most workers still remain unemployed and have to drastically decrease their wage selectivity. Since most workers exhaust their UI with this new policy, most workers do not see a gain from the increased replacement rate, making the wage selectivity channel smaller in aggregate. For workers with longer entitlements, the situation is reversed. Their replacement rate increases, allowing them to raise their target wage. While this increases the probability that they reach the expiration of UI, their potential duration is long enough that this force is almost irrelevant, making the channel of wage selectivity more relevant for them in creating positive re-employment wage changes.

7 Conclusion

This paper disentangles the driving factors behind the causal impact of unemployment insurance duration on re-employment earnings and assesses their policy implications, using Spanish Social Security data. We find that both the labor demand factor – workers' reactions to UI exhaustion – and the labor supply factor — the decline in labor market opportunities -— matter in characterizing the re-employment wage path and govern the effectiveness of UI policies.

Empirically, we document a sudden wage drop at the exhaustion of UI and provide an identification strategy to recover the causal effect of the exhaustion of unemployment insurance on re-employment wages. We establish its connection with the elasticity of wage selectivity to the value of non-employment, an important parameter for wage determination, labor market matching, and UI policy effectiveness. Combining this novel piece of information with quasi-experimental UI extensions, we estimate the rate of decline in re-employment wages due to decreased labor market opportunities.

Theoretically, we complement Schmieder et al. (2016) to show a complete picture of how to identify the decline in labor market opportunities. We highlight the economic relevance of the change in target wages and show its equivalence to the causal impact of the exhaustion of benefits on the re-employment wage using a simple model. Finally, we highlight how the relative importance of these two time-dependent channels will affect the effectiveness of different UI interventions, that incentivize workers through conditional and unconditional income transfers, depending on the timing of the intervention.

Our work raises several new questions. First, where does the increased wage selectivity in response to UI extension come from? Is it reflecting improvements in match quality (a true productivity increase) or just 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 create net benefits for society. The distinction between these consequently matters for policymakers.

Second, is the deterioration of labor market opportunities a linearly decreasing process over the duration of unemployment? In our analysis of heterogeneous workers, we find that workers with longer unemployment durations have a significantly smaller rate of decline in re-employment earnings; workers also have a smaller rate of decline during the financial crisis. These pieces of evidence are consistent with a non-linear decline in labor market opportunities: decreasing but with diminishing slope over time. It is possible that human capital depreciation (or labor market stigmatization) is stronger at the beginning of the unemployment spell, for instance if workers' specific human capital, a key determinant of the wage premium, depreciates faster than general human capital. This duration-dependent feature of labor market opportunity 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-linearity of the wage depreciation from the sorting of workers with lower depreciation rates to the lengthier side of the unemployment duration distribution, and future research trying to disentangle this will be required to provide an answer to the relative importance of specific and general human capital in the decline of labor market opportunity.

Last, our paper does not consider the extensive margin, that 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? This is an important question to explore since, in low wage jobs, workers have less space to bargain 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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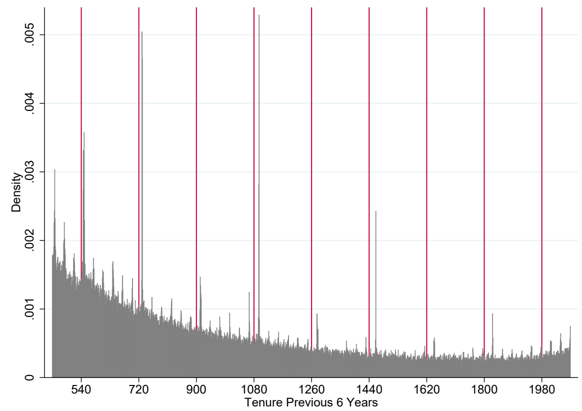
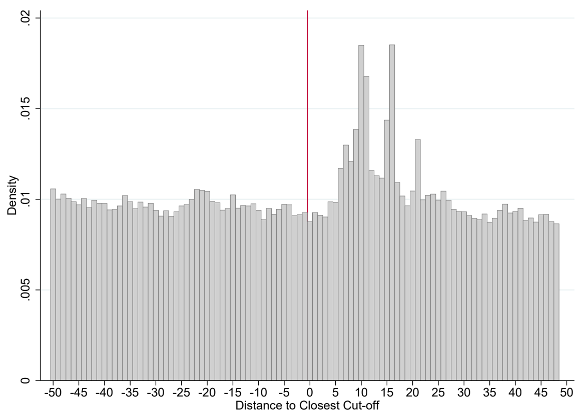
Figures

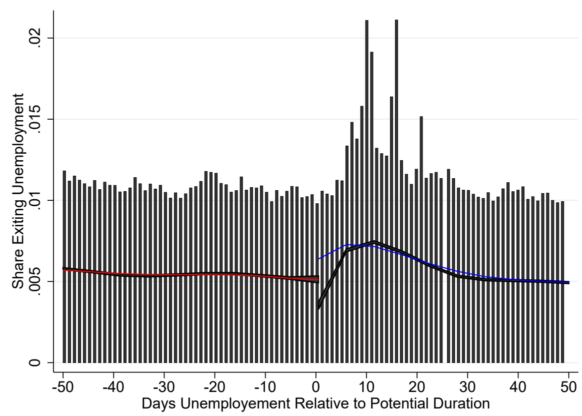
Figure 1: Distribution of previous tenure: Original sample



(a) Previous tenure



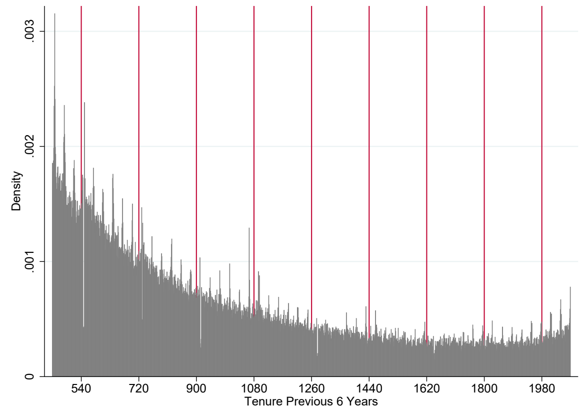
(b) Previous tenure (relative to policy discontinuities)



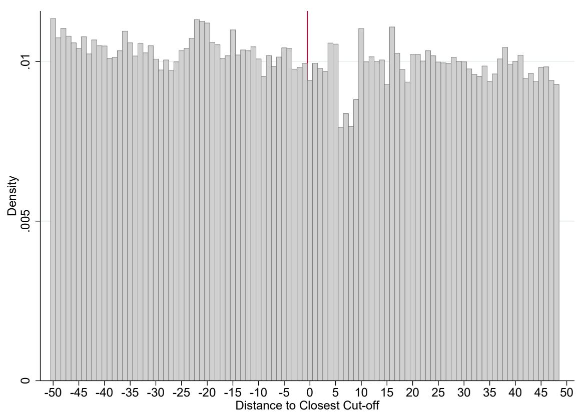
(c) Manipulation test previous tenure (relative to policy discontinuities)

Note: These figures plot the distribution of total work tenure 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. Panel (c) 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). Both the conventional and the bias-corrected robust estimate reject 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.

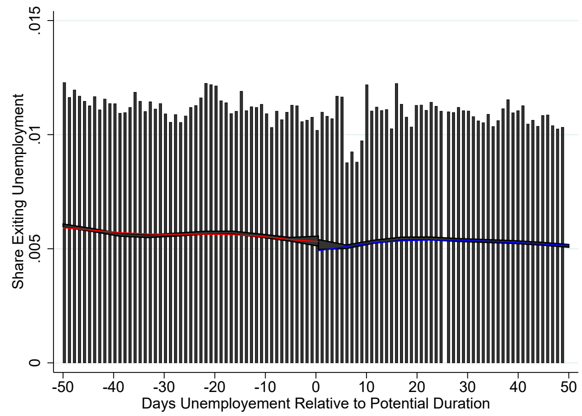
Figure 2: Distribution of previous tenure:
No temporary contracts with predetermined length



(a) Previous tenure



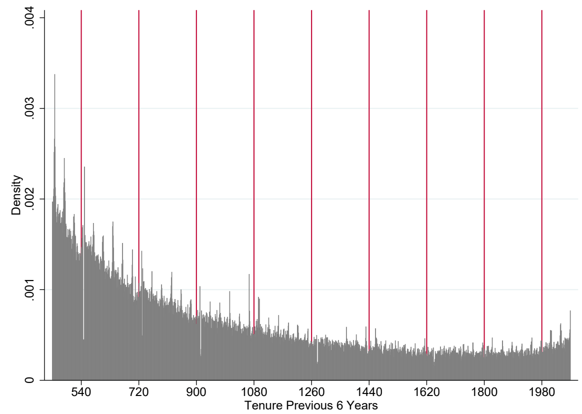
(b) Previous tenure (relative to policy discontinuities)



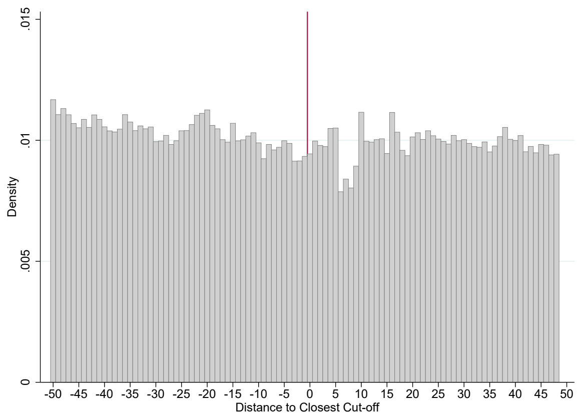
(c) Manipulation test previous tenure (relative to policy discontinuities)

Note: These figures plot the distribution of total work tenure 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 1/2 year. 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. Panel (c) 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). Neither the conventional and the bias-corrected robust estimate reject no manipulation.

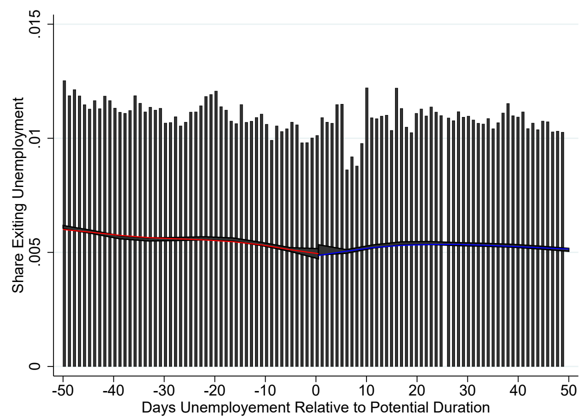
Figure 3: Distribution of previous tenure:
No temporary contracts with predetermined length;
No spells where potential duration and time in unemployment do not match



(a) Previous tenure



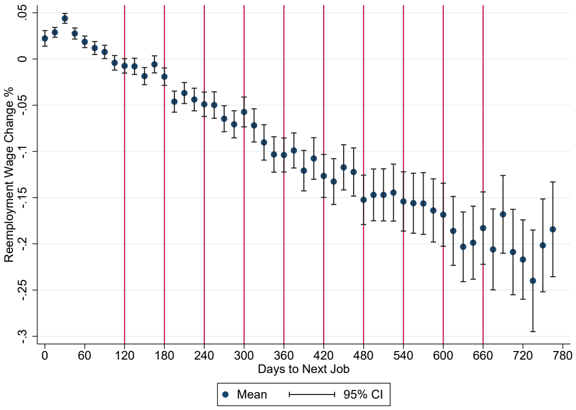
(b) Previous tenure (relative to policy discontinuities)



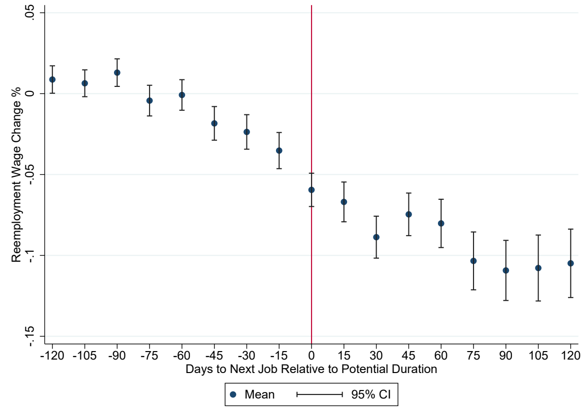
(c) Manipulation test previous tenure (relative to policy discontinuities)

Note: These figures plot the distribution of total work tenure 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 a 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. Panel (c) 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). Neither the conventional and the bias-corrected robust estimate reject no manipulation.

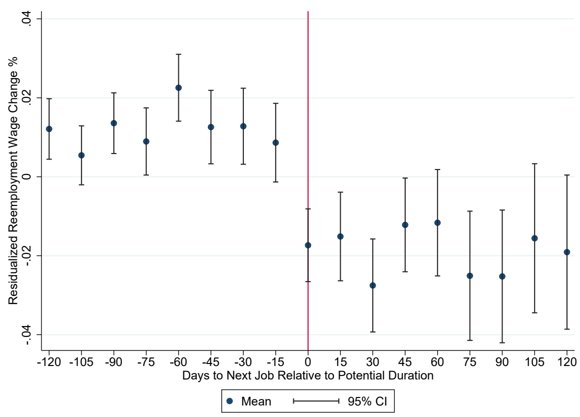
Figure 4: Evolution of re-employment wage changes



(a) Re-employment wage change over time



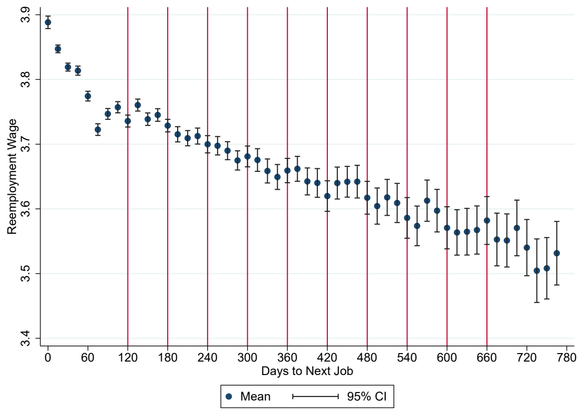
(b) Re-employment wage change over time (relative to benefit exhaustion)



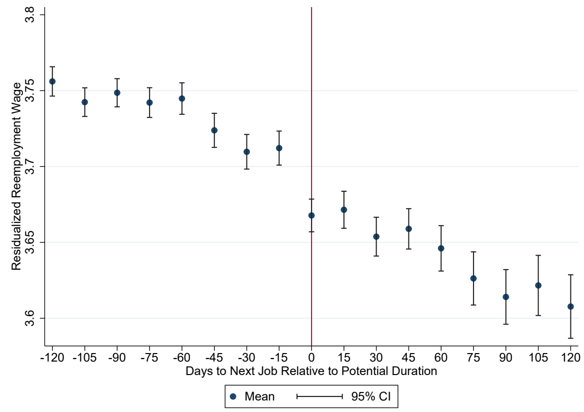
(c) Residualized re-employment wage change (relative to benefit exhaustion)

Note: These figures plot the re-employment wage evolution in different ways. The re-employment wage variables for all figures here is the log of the re-employment wage (relative to log previous wage). Panel (a) plots the re-employment wage change against time until re-employment. Panel (b) plots the residualized re-employment wage change against time until re-employment, relative to benefit exhaustion, pooling together workers with different potential durations (B). We generate the residualized re-employment wage and wage change by taking the residual of the OLS regression of the re-employment wage on worker and economy characteristics. Panel (c) plots the residualized re-employment wage change against time until re-employment, relative to benefit exhaustion, pooling together people with different potential durations (B), using the same residualization as above, adding controls for time unemployed.

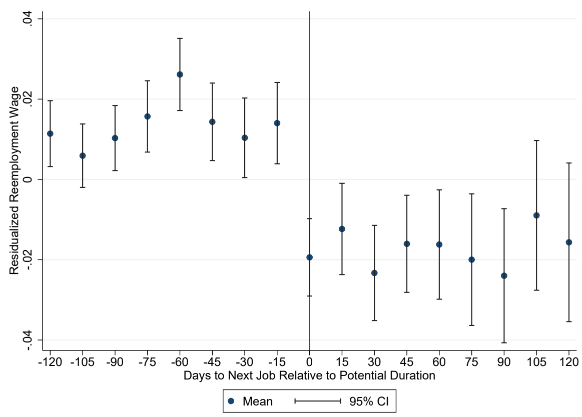
Figure 5: Evolution of re-employment wages



(a) Re-employment wage over time



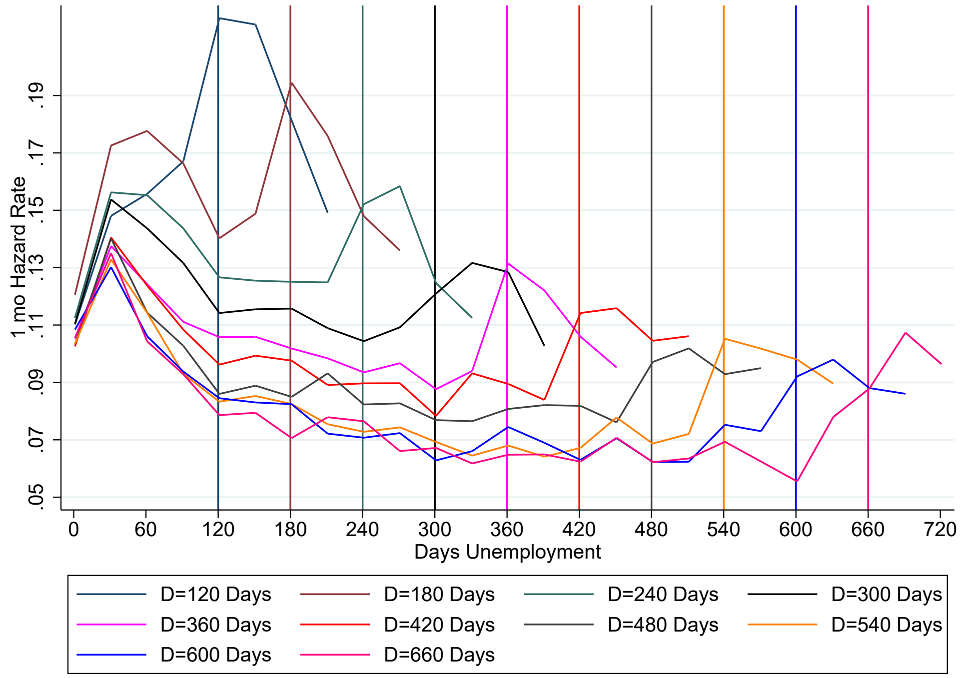
(b) Re-employment wage over time
(relative to benefit exhaustion)



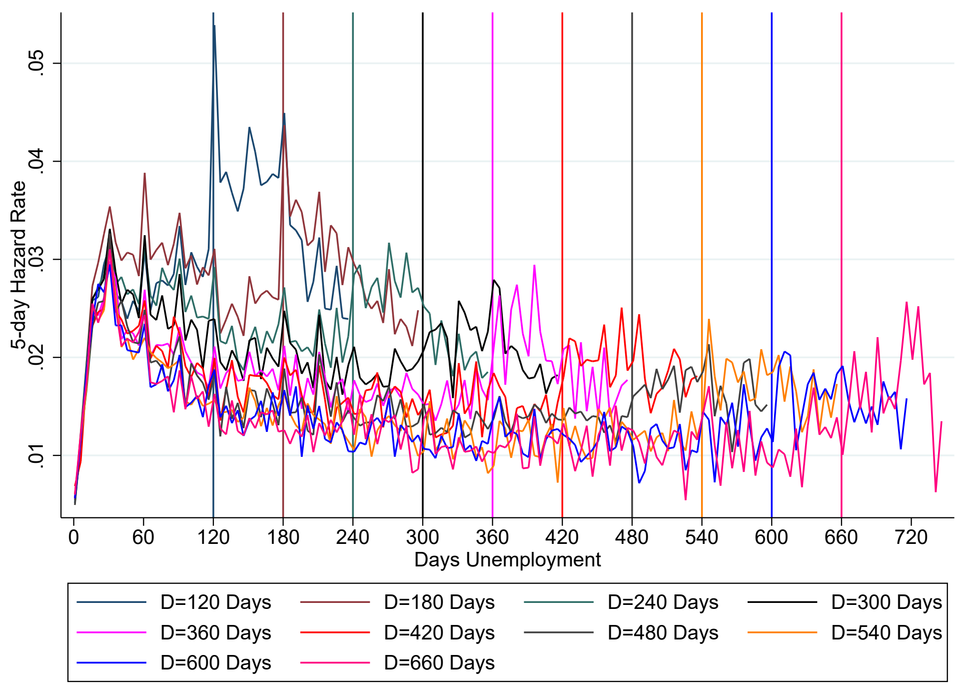
(c) Residualized re-employment wage
(relative to benefit exhaustion)

Note: These figures plot the re-employment wage evolution in different ways. The re-employment wage for all figures here is the log of the re-employment wage. Panel (a) plots the re-employment wage against time until re-employment. Panel (b) plots the residualized re-employment wage against time until re-employment, relative to benefit exhaustion, pooling together people with different potential durations (B). We generate the residualized re-employment wage and wage change by taking the residual of the OLS regression of the re-employment wage on worker and economy characteristics. Panel (c) plots the residualized re-employment wage against time until re-employment, relative to benefit exhaustion, pooling together people with different potential durations (B), using the same residualization as above, adding controls for time in unemployment.

Figure 6: Re-employment hazard rates, by potential duration



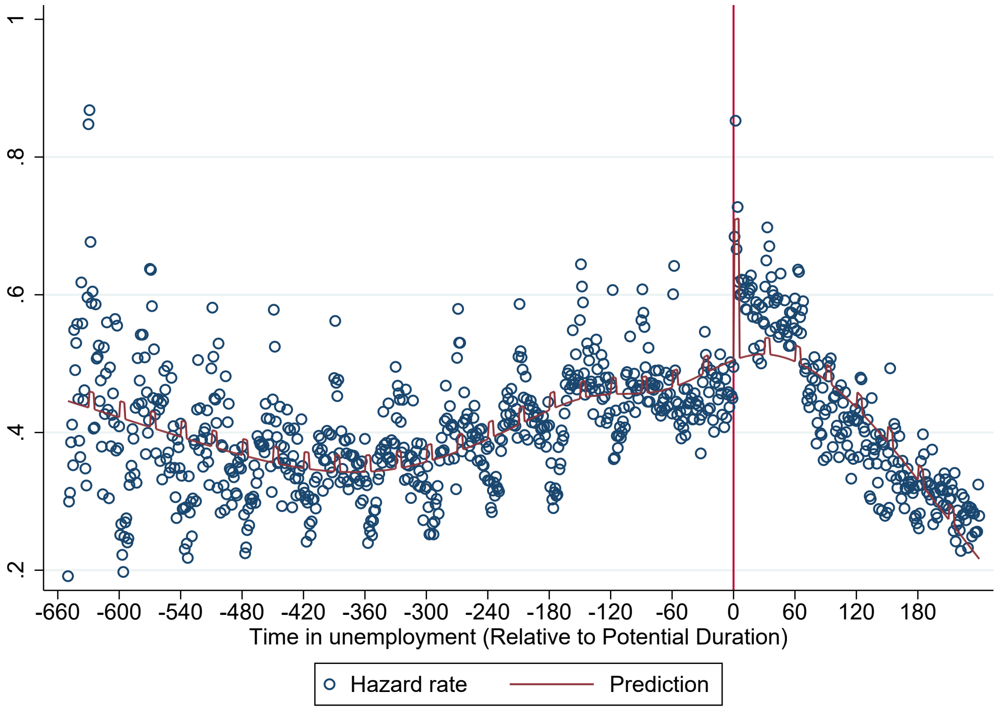
(a) 30-day hazard rate, by potential duration



(b) 5-day hazard rate, by potential duration

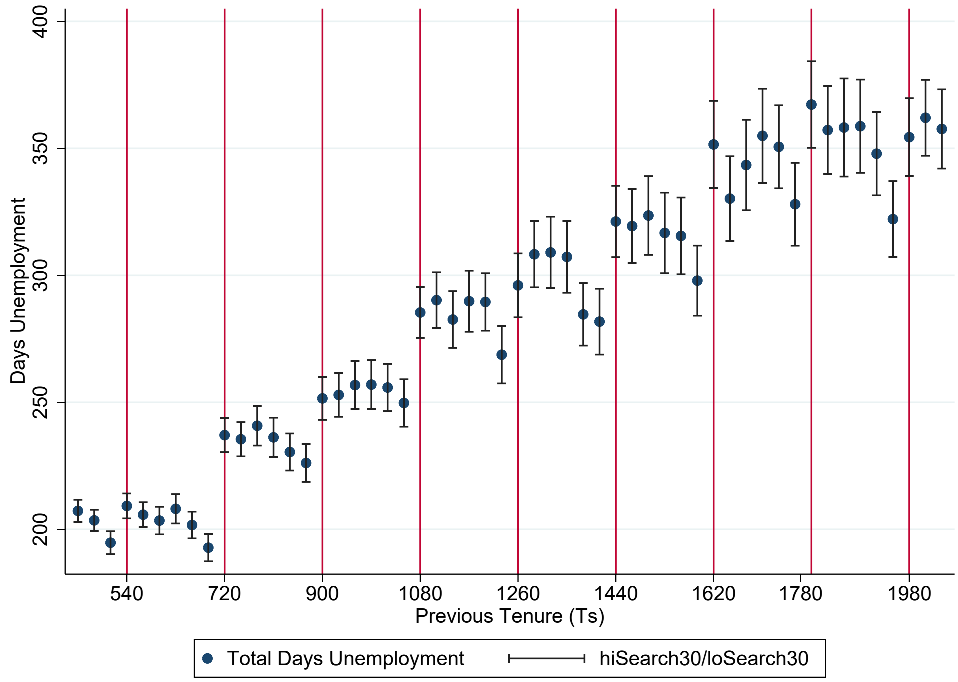
Note: Both panel (a) and (b) plot the hazard rates over time until re-employment for workers with different potential durations. Panel (a) presents the 30-day hazard rate. Panel (b) presents the 5-day hazard rate. Panel (a) only shows the hazard rate up to 90 days post exhaustion of benefits.

Figure 7: Observed & predicted hazard rate: All potential durations combined

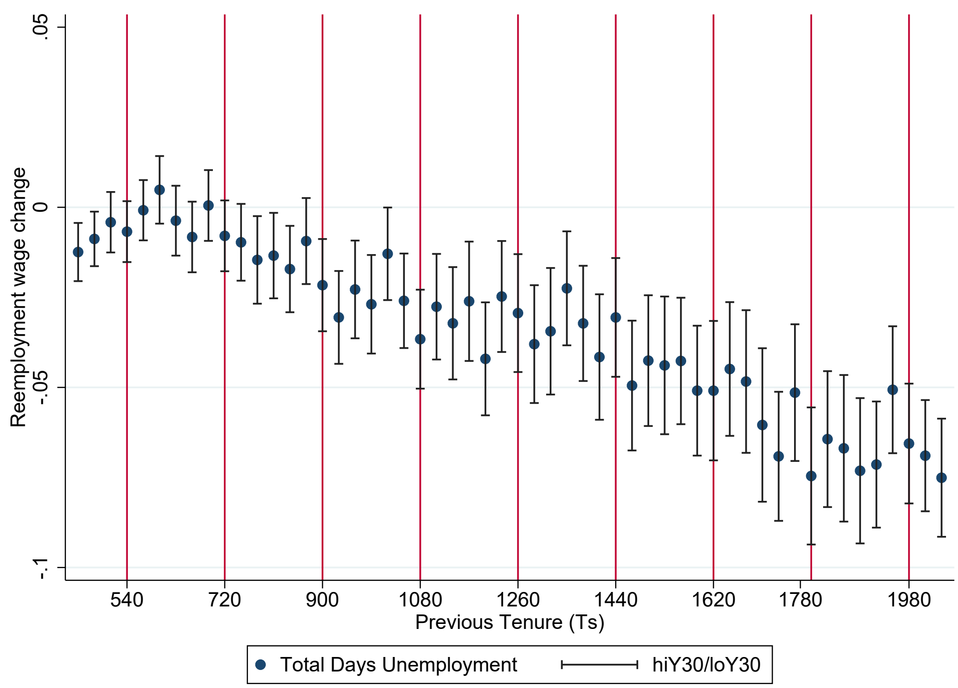


Note: Figure 7 shows the 1-day hazard rate and its non-parametric prediction (red) against time until re-employment (relative to benefit exhaustion). This combines workers with different potential durations together.

Figure 8: The effect of a 2-month increase in potential duration



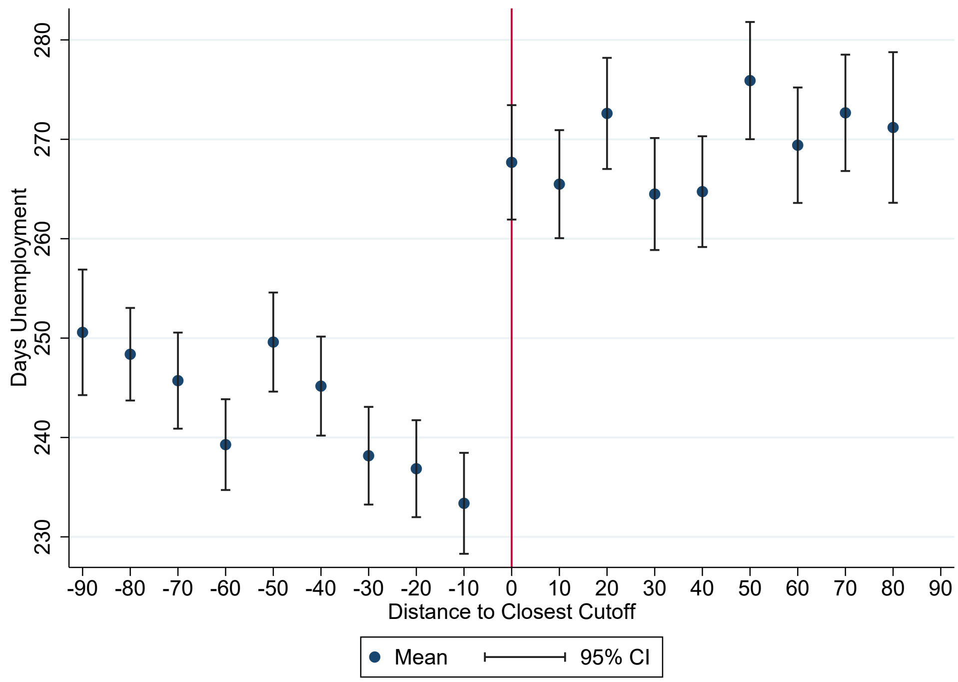
(a) Time in unemployment



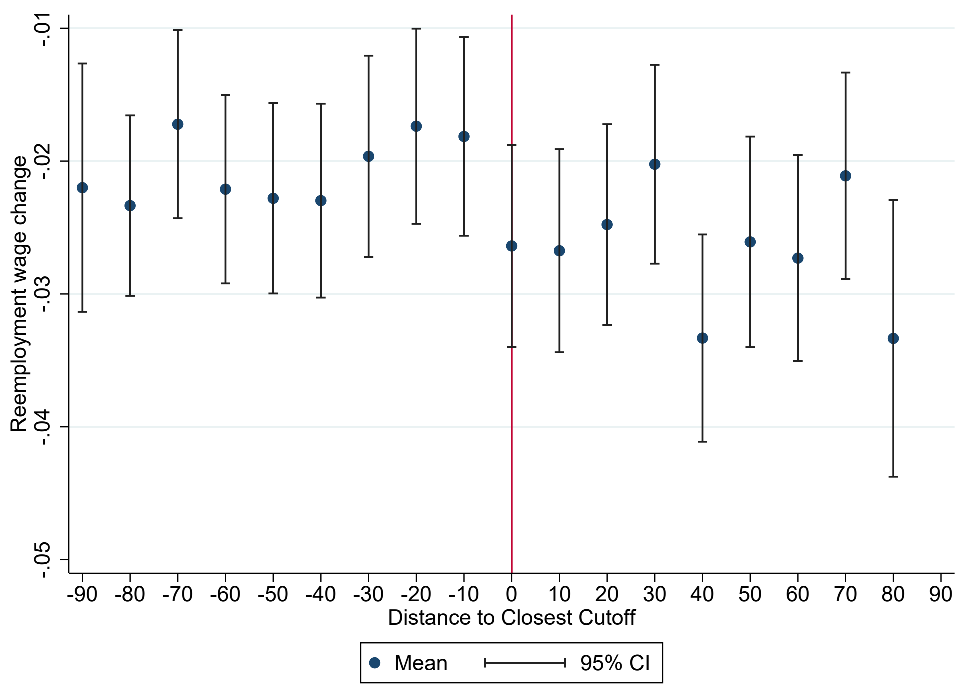
(b) Re-employment wage

Note: These figures non-parametrically show the impact of crossing the cutoff threshold on time unemployed (panel (a)) and re-employment wages (panel (b)). The re-employment wage variable is the log of the re-employment wage (relative to the log previous wage).The red lines on the x-axis mark the thresholds where workers start receiving two additional months of potential duration.

Figure 9: The effect of a 2-month increase in potential duration: All cut-offs combined



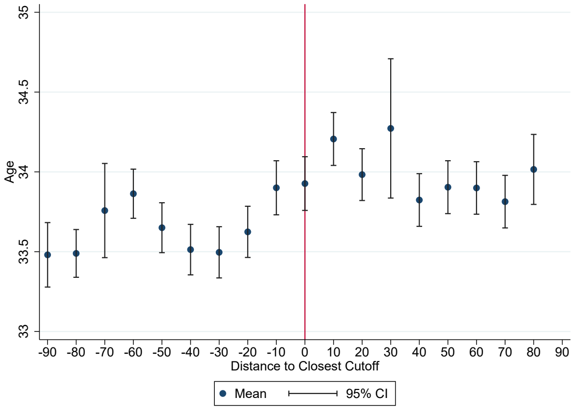
(a) Time in unemployment



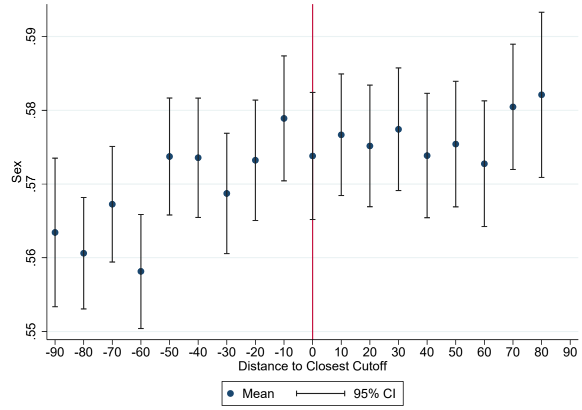
(b) Re-employment wage

Note: These figures non-parametrically show the impact of crossing the cutoff threshold on time unemployed (panel (a)) and re-employment wages (panel (b)). The re-employment wage variable is the log of the re-employment wage (relative to the log previous 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.

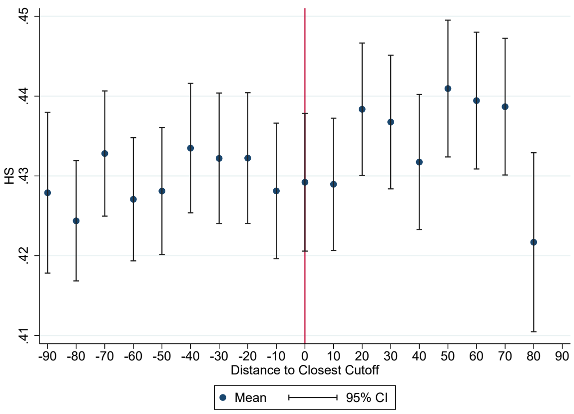
Figure 10: Balance test of observed characteristics



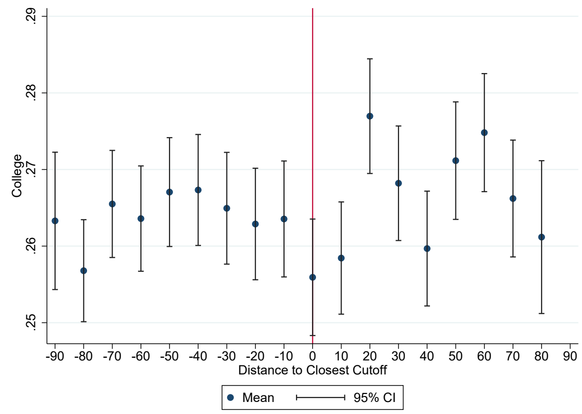
(a) Age



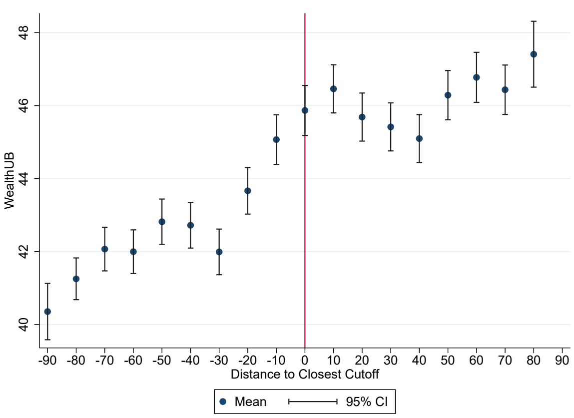
(b) Male



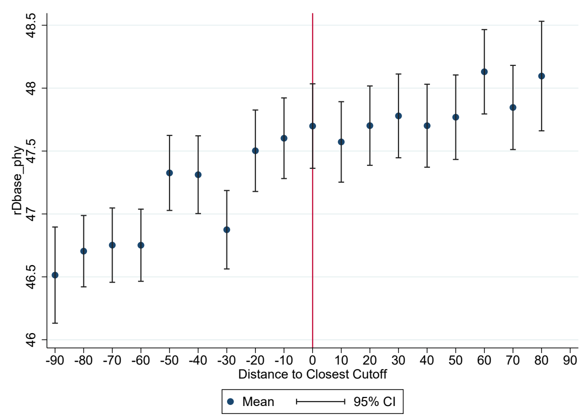
(c) High School



(d) College



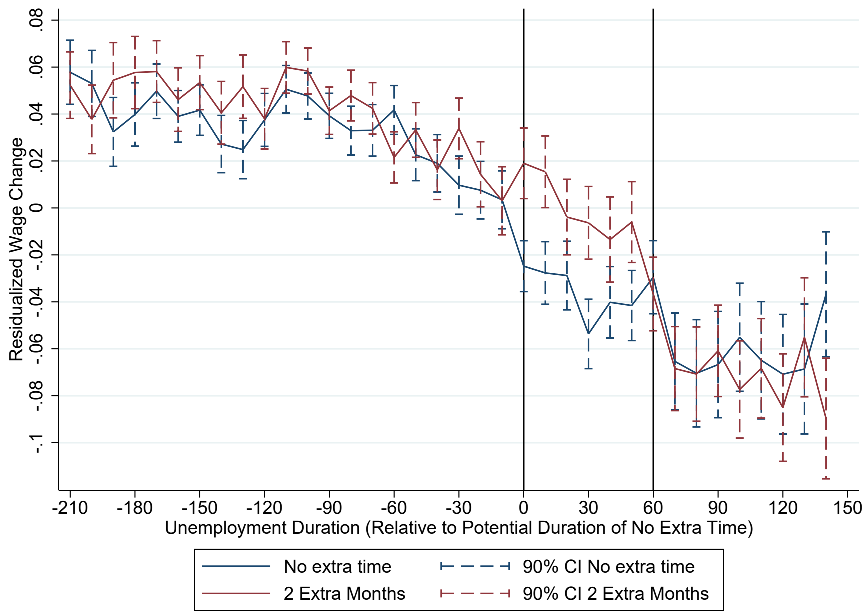
(e) Wealth*



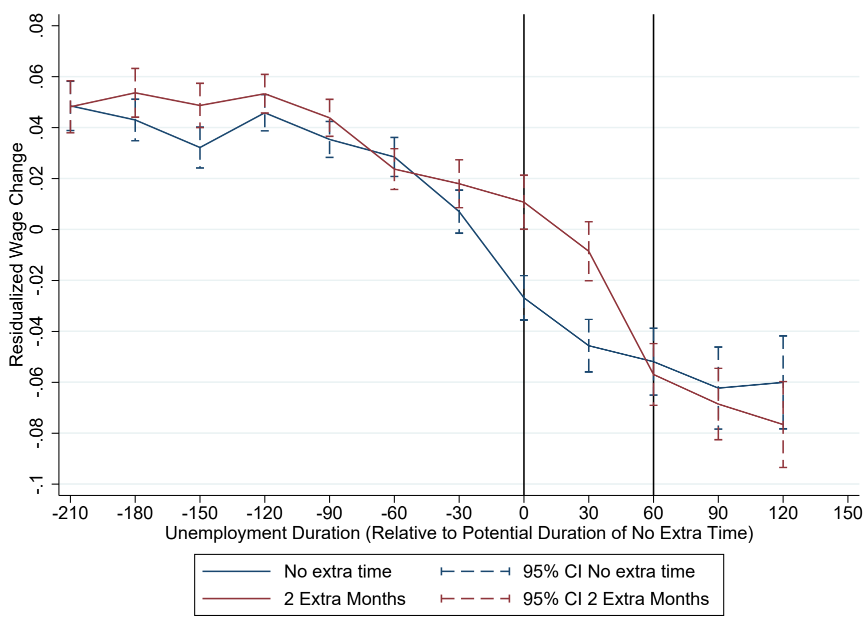
(f) Previous Daily Wage

Note: These figures plot the balance test of observed characteristics for the RD design. 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. *Wealth is the discounted sum of all labor earnings prior to entering unemployment.

Figure 11: Evolution of re-employment wages: Treatment vs control group



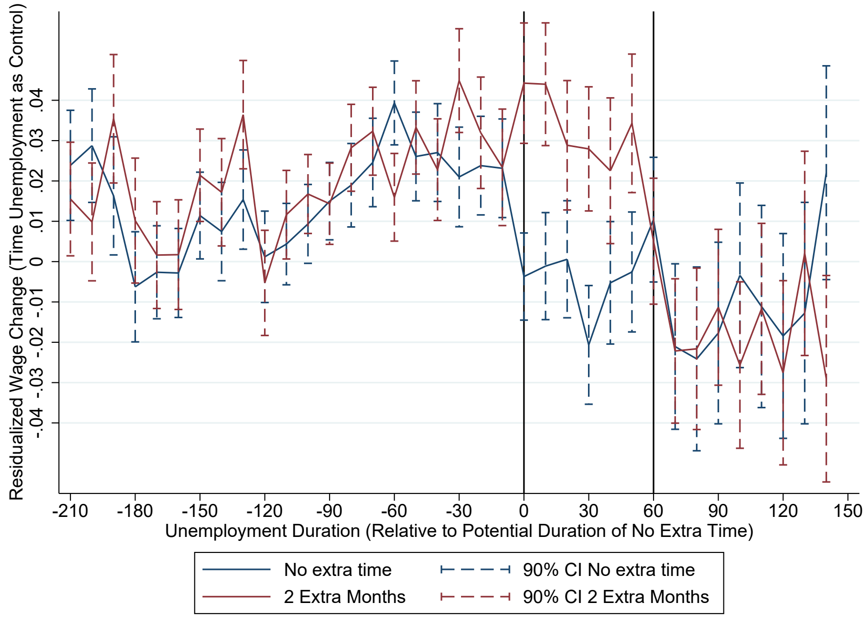
(a) 10-day residualized re-employment wage



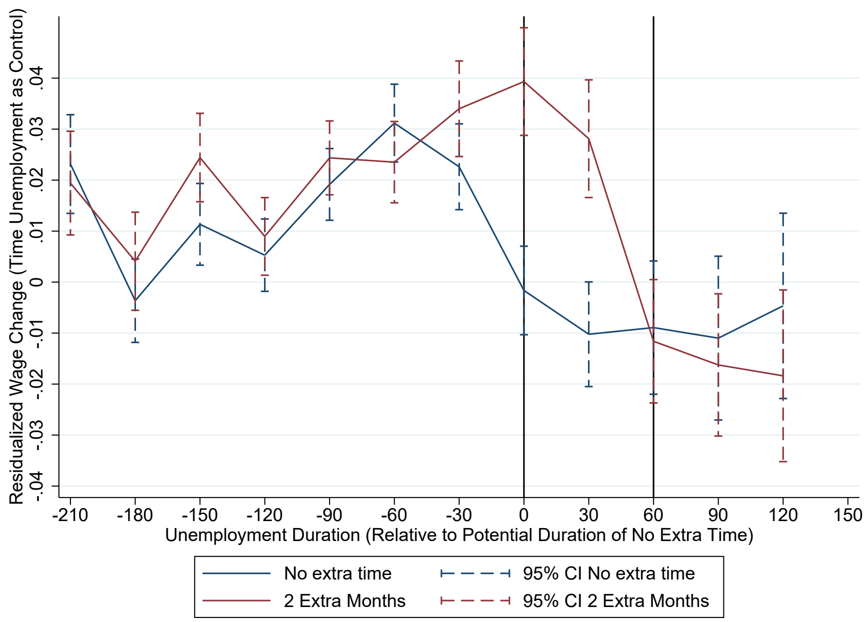
(b) 30-day residualized re-employment wage

Note: Figure 12(b) presents the evolution of residualized wags for workers with and without an exogenous additional two months of UI. In panel (a), we residualize the re-employment wage using worker and economy characteristics, without including time unemployed as a control, using a 10-day window to calculate the average residualized re-employment wage. In panel (b), we residualize the re-employment wage on worker and economy characteristics, without including time unemployed as a control, using a 30-day window to calculate the average residualized re-employment wage. We analyze the workers whose tenure is close to a cut-off, such that the extension of two additional months of UI is close to randomization. The window we choose to classify a worker into either treatment or control is 85 days from or past the discontinuity. The red (blue) line shows the wage path for workers with (without) two extra months of UI.

Figure 12: Evolution of re-employment wages: Treatment vs control group



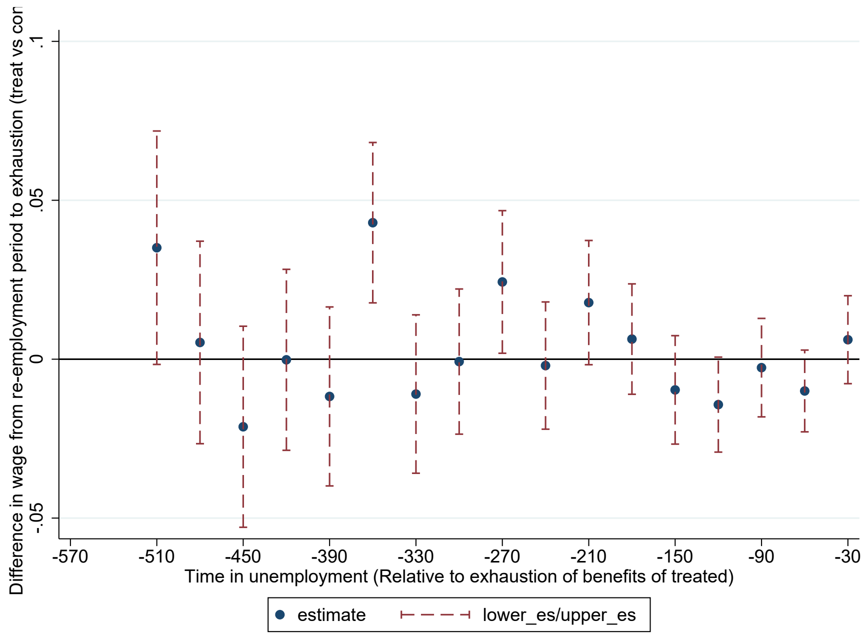
(a) 10-day residualized re-employment wage (control time in unemployment)



(b) 30-day residualized re-employment wage (control time in unemployment)

Note: Figure 12(b) presents the evolution of residualized wages for workers with and without an exogenous additional two months of UI. In panel (a), we residualize the re-employment wage uing worker and economy characteristics including time unemployed as a control, using a 10-day window to calculate the average residualized re-employment wage. In panel (b), we residualize the re-employment wage using worker and economy characteristics, including time unemployed as a control, using a 30-day window to calculate the average residualized re-employment wage. We analyze the workers whose tenure is close to a cut-off, such that the extension of two additional months of UI is close to randomization. The window we choose to classify a worker into either treatment or control is 85 days from or past the discontinuity. The red (blue) line shows the wage path for workers with (without) two extra months of UI.

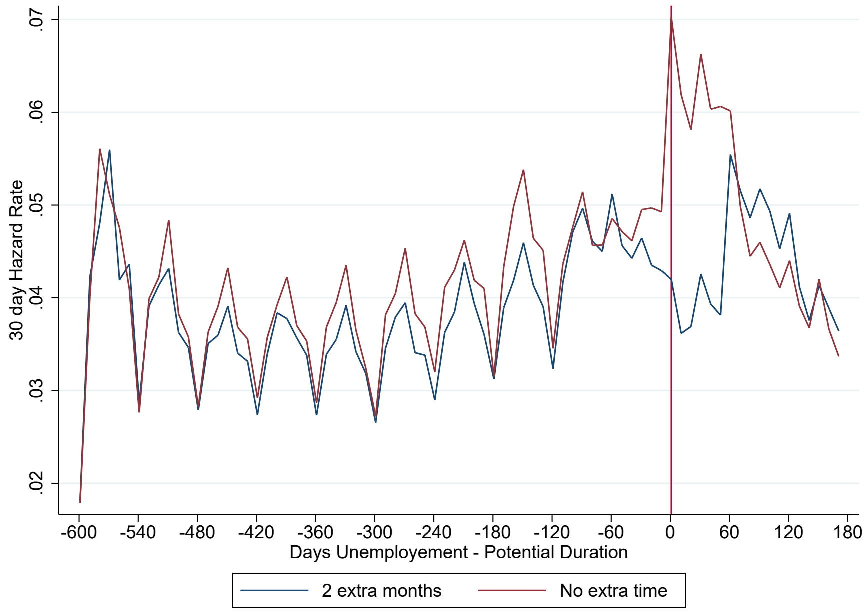
Figure 13: Evolution of re-employment wages: Treatment vs control group, by period



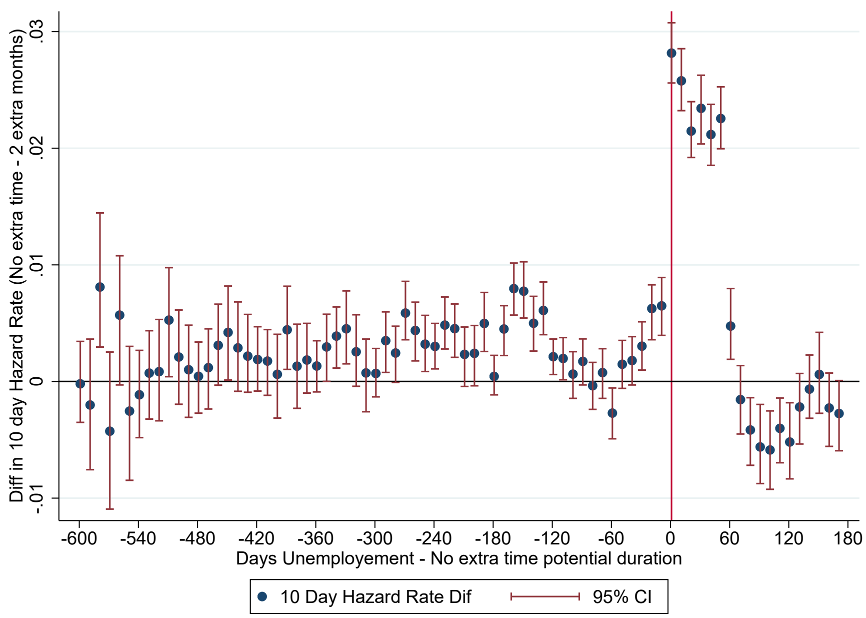
(a) Difference in re-employment wage for period (control time in unemployment)

Note: Figure 13 presents the impacts of two additional months of UI benefits on the re-employment wage prior to the exhaustion of UI. Panel 14(a) shows the effect of a UI extension on wages at τ ($\tau = B - 30, B - 60, \cdots, B - 510$ days).

Figure 14: Evolution of 10-day hazard rate: Treatment vs control group, by period.



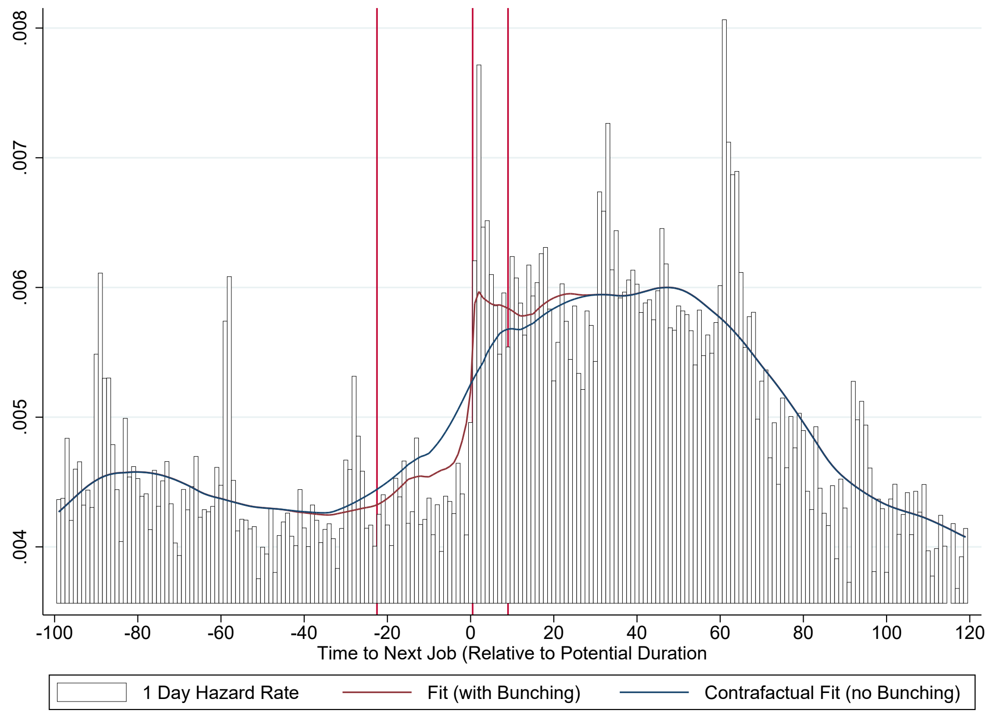
(a) 10-day hazard rate



(b) Difference in 10-day hazard rate

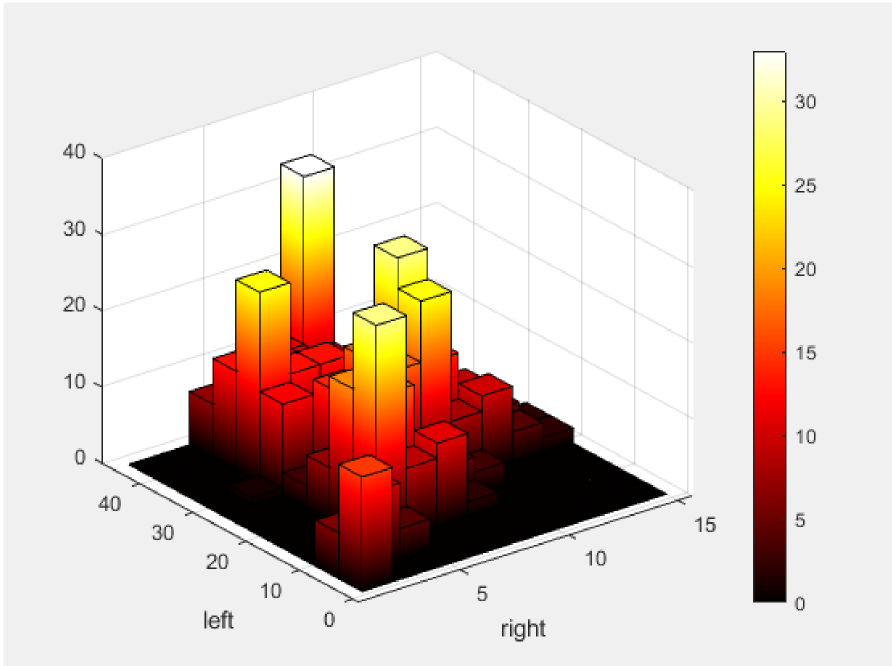
Note: Figure 14 presents the impacts of two additional months of UI benefits on the 10-day hazard rate, with zero corresponding to the exhaustion of UI within the treatment group. Panel 15(a) shows the hazard rate for each group (treatment and control) at τ ($\tau = B - 30, B - 60, \cdots, B - 510$ days). Panel 15(b) shows the difference in the period hazard rate across groups for each $\tau = B - 30, B - 60, \cdots, B - 510$ days.

Figure 15: Manipulation analysis of re-employment timing

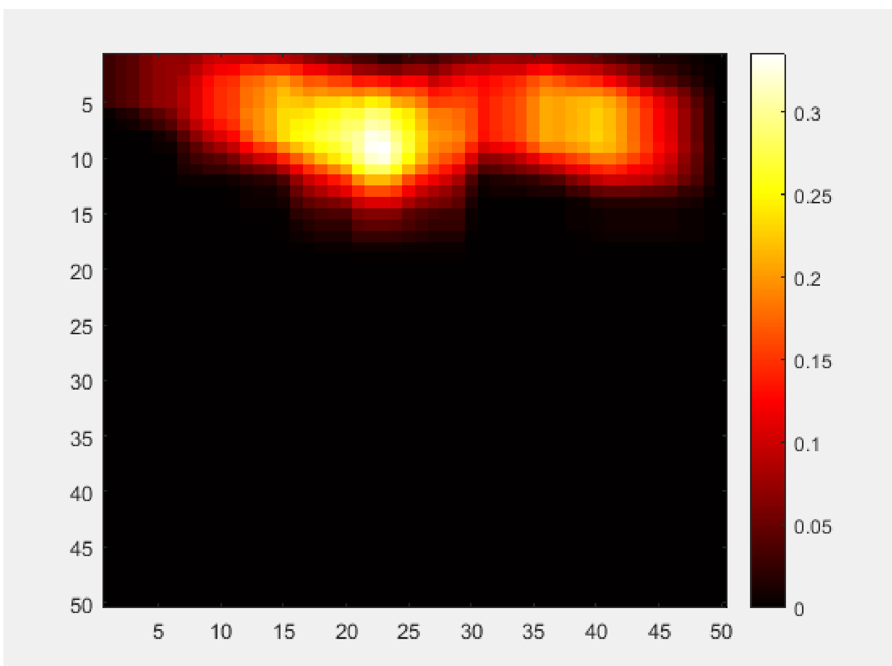


Note: 15 presents the empirical distribution of the 1-day hazard rate around the exhaustion of unemployment benefits. We plot using the blue smooth line the estimated counterfactual distribution of the hazard rate if workers were not allowed to manipulate their working start date. The two extra vertical red lines, to the left and right of 0 are respectively the optimal solution to the minimization problem in Equation 17, $(D_{B,-}, D_{B,+})$.

Figure 16: Solutions of $(D_{B,-}, D_{B,+})$, via bootstrap (100 cycles)



(a) Frequency distribution of the solutions



(b) Heat map of the solutions

Note: This graph presents the distribution of the estimates for $((D_{B,-}, D_{B,+}))$ from 100 bootstrap re-samplings.

Figure 17: Aggregate hazard rate near UI exhaustion: Data and model

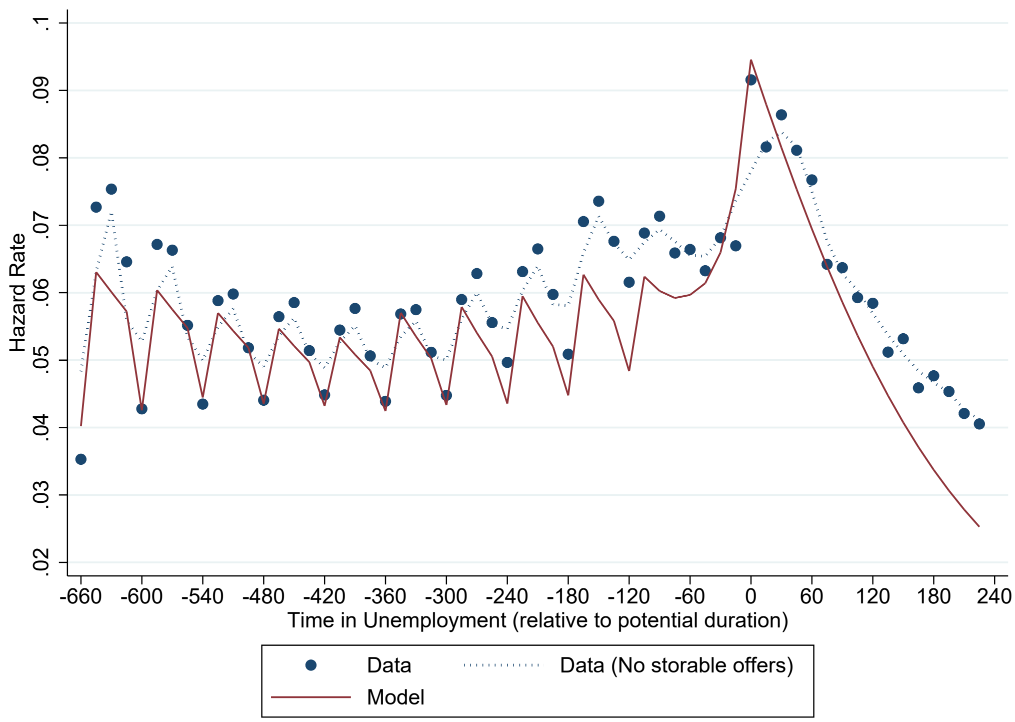


Figure 18: Re-employment wage evolution: Data and model

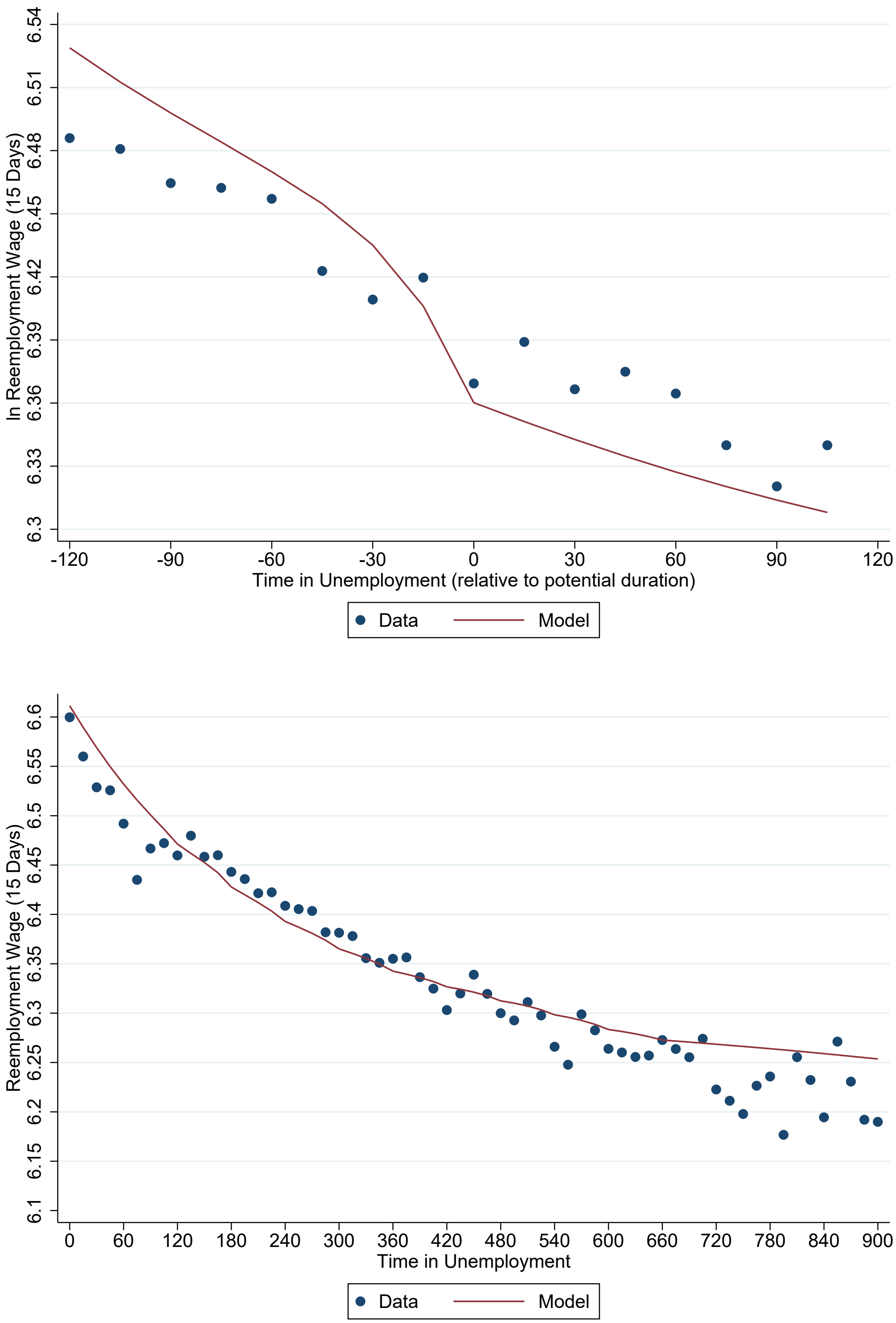


Figure 19: Hazard fit, by entitlement

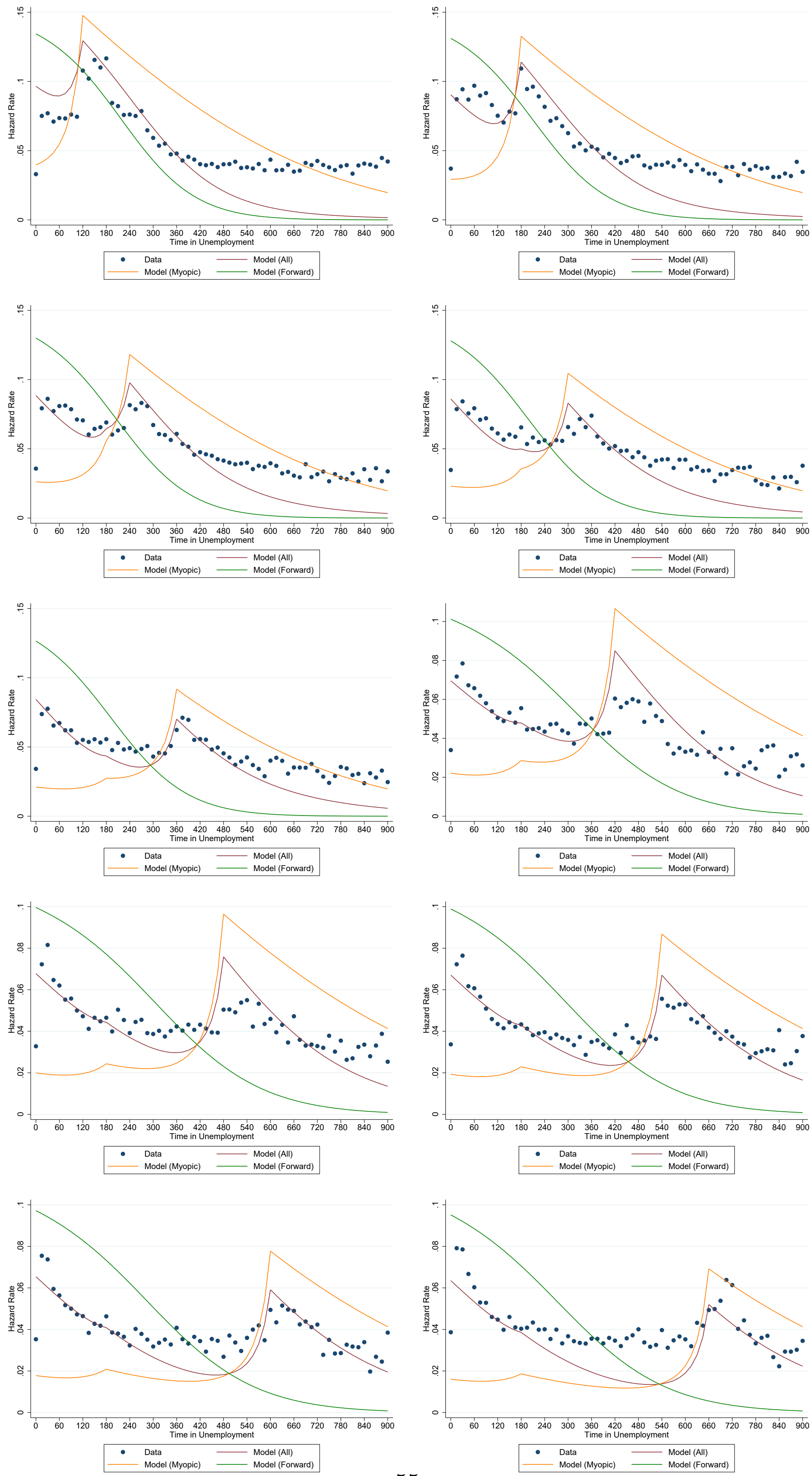


Figure 20: Wage fit, by Entitlement

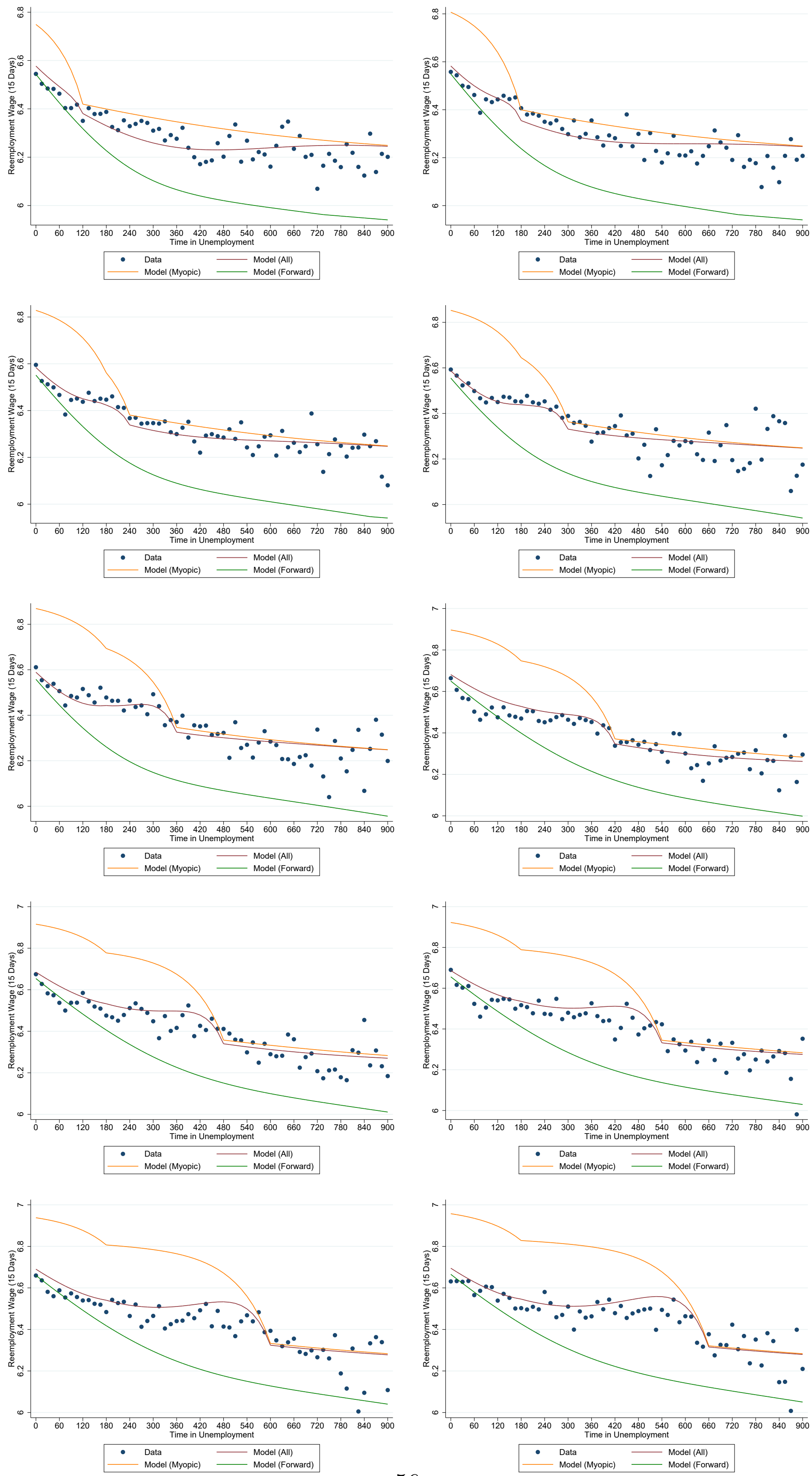


Figure 21: Policy experiments, by entitlement

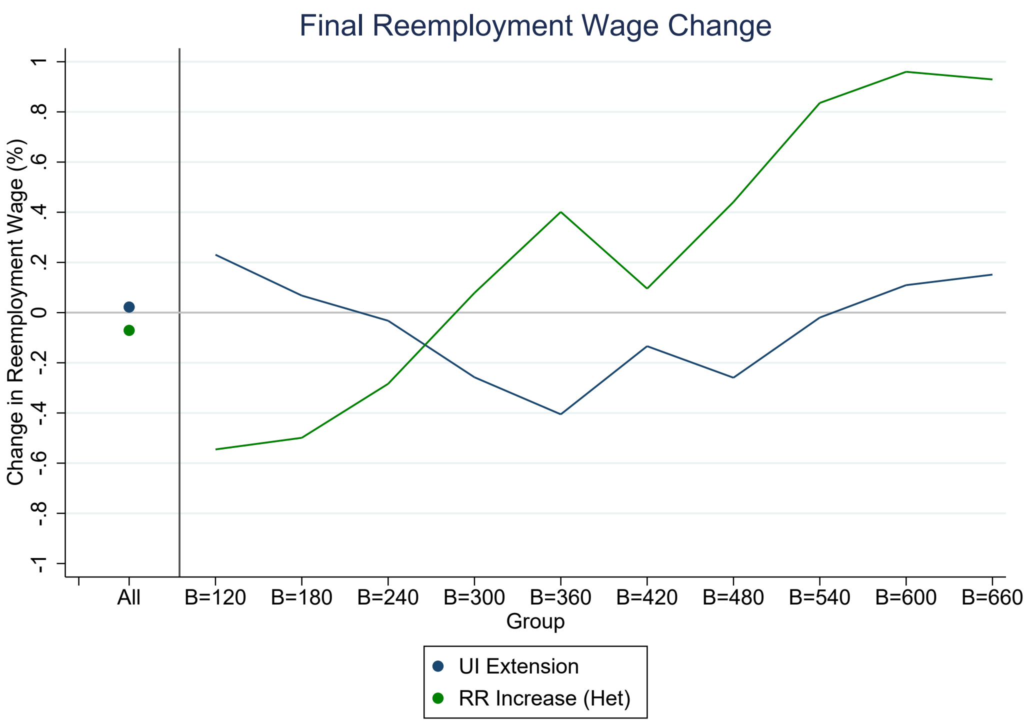
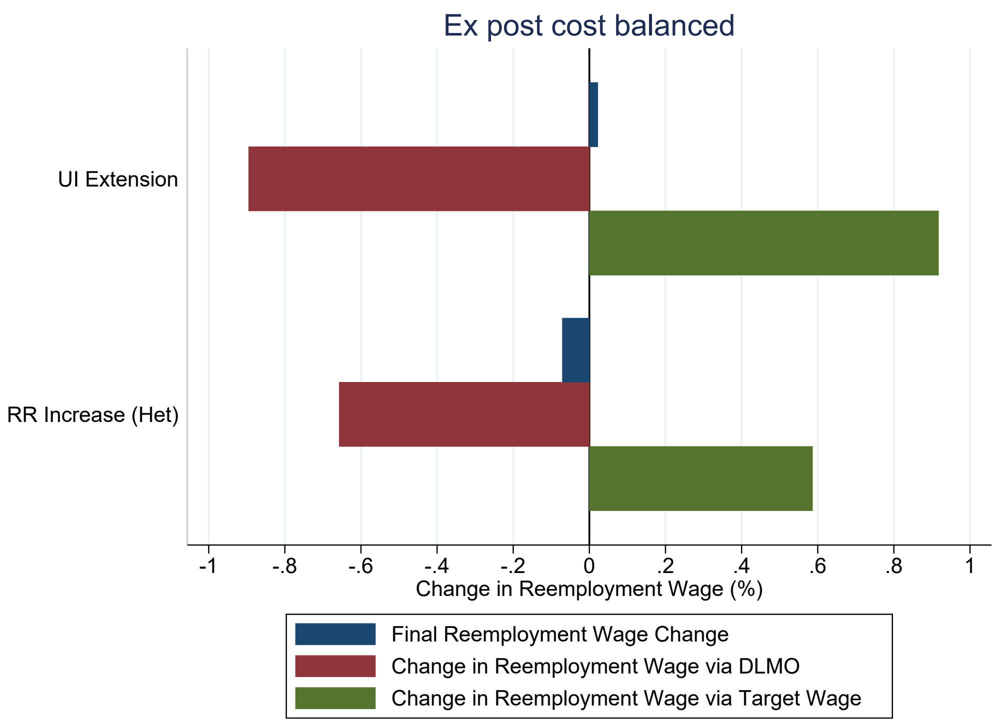


Figure 22: Channel effects of policy experiments, by entitlement

Figure 23: Policy experiments: Aggregate effect



Tables

Table 2: **UI potential duration scheme**

Days Worked in Previous 6 Years ($T_{tenure,i}$)											
From	360	540	720	900	1080	1260	1440	1620	1800	1980	>2160
To	539	719	899	1079	1259	1439	1619	1799	1979	2159	
Potential Duration (D) (Months)											
	4	6	8	10	12	14	16	18	20	22	24

Note: This table summarizes the schedule of the potential duration (B) for UI in Spain. To read the table, let's focus on the first column: the workers whose past 6 years working experience ($T_{tenure,i}$) is in between 360 to 539 days will be given 4 months of UI.

Table 3: Summary statistics. Complete (restricted) sample. Different potential duration groups.

Potential Duration	120	180	240	300	360	420	480	540	600	660	All
Days Collecting UI	89.48	107.2	132.1	151.2	178.4	194.7	214.8	233.8	248.1	249.2	146.9
	(38.50)	(61.91)	(83.70)	(105.2)	(127.3)	(147.5)	(168.7)	(191.2)	(209.0)	(221.9)	(131.6)
Days Unemployment	200.2	190.8	221.6	237.1	267.5	281.8	300.2	324.0	332.9	317.1	238.9
	(241.2)	(239.0)	(276.6)	(296.3)	(320.3)	(339.8)	(355.0)	(387.5)	(395.5)	(379.8)	(299.7)
Re-employment in 6 months	0.662	0.666	0.609	0.585	0.537	0.525	0.502	0.491	0.484	0.501	0.598
Re-employment in 12 months	0.868	0.873	0.841	0.814	0.764	0.744	0.716	0.689	0.681	0.696	0.810
Share Exhausting UI	0.460	0.271	0.224	0.184	0.180	0.153	0.142	0.136	0.119	0.0881	0.266
Previous Daily Wage	44.45	45.46	45.91	47.07	47.58	48.82	50.12	50.43	51.27	53.60	47.05
	(15.34)	(16.36)	(16.66)	(17.54)	(18.19)	(18.76)	(19.80)	(19.66)	(20.07)	(20.99)	(17.63)
Previous (6 Years) Tenure	436.1	622.4	803.3	986.1	1164.0	1347.2	1526.3	1709.7	1890.4	2088.1	963.9
	(51.03)	(50.61)	(51.23)	(51.24)	(51.83)	(51.36)	(51.82)	(51.09)	(52.32)	(52.26)	(544.4)
Age	34.09	33.38	33.06	33.32	33.66	33.96	34.46	34.88	35.83	37.72	34.17
	(10.54)	(13.50)	(14.63)	(9.642)	(9.526)	(9.312)	(9.303)	(9.118)	(9.207)	(9.108)	(11.25)
Share Male	0.567	0.587	0.594	0.597	0.598	0.603	0.606	0.611	0.621	0.672	0.595
Share College	0.236	0.258	0.264	0.270	0.268	0.270	0.277	0.277	0.261	0.236	0.255
Share High School	0.394	0.420	0.427	0.437	0.436	0.443	0.457	0.449	0.437	0.407	0.420
Wealth*	36.54	37.84	38.94	43.66	46.57	50.97	55.99	61.09	68.12	85.46	46.51
	(35.83)	(36.44)	(35.27)	(36.44)	(36.15)	(36.16)	(38.70)	(38.81)	(40.54)	(41.65)	(38.48)
Wage Change	-0.0170	-0.0134	-0.0240	-0.0369	-0.0471	-0.0502	-0.0579	-0.0692	-0.0845	-0.0882	-0.0358
	(0.418)	(0.410)	(0.419)	(0.419)	(0.435)	(0.420)	(0.417)	(0.426)	(0.428)	(0.418)	(0.420)
Avg. Wage Change (1 Year)	0.0196	0.0210	0.0101	-0.000129	-0.0117	-0.0180	-0.0262	-0.0354	-0.0505	-0.0558	-0.00102
	(0.363)	(0.364)	(0.376)	(0.379)	(0.395)	(0.385)	(0.390)	(0.395)	(0.398)	(0.389)	(0.379)
Avg. Wage Change (5 Years)	0.0490	0.0486	0.0409	0.0313	0.0219	0.0128	0.00352	-0.00420	-0.0208	-0.0274	0.0288
	(0.352)	(0.357)	(0.369)	(0.368)	(0.382)	(0.376)	(0.381)	(0.384)	(0.389)	(0.374)	(0.369)
N	96220	55756	38431	28327	22626	17580	15098	13503	14036	25673	327250

Note: Table 3 presents the summary statistics of the complete sample with potential durations from 120 days to 660 days (in this paper, workers with the longest potential duration are not considered). Means and standard deviations (in parentheses) are shown.

Table 4: Summary statistics. Estimation sample. Different potential duration groups.

Potential Duration	120	180	240	300	360	420	480	540	600	660	All
Days Collecting UI	87.02	107.3	132.1	151.2	178.4	194.6	214.4	234.4	247.5	265.3	152.9
	(38.68)	(61.93)	(83.72)	(105.3)	(127.2)	(147.2)	(168.6)	(191.3)	(209.2)	(228.9)	(132.0)
Days Unemployment	190.0	191.3	222.0	237.1	267.3	281.1	300.9	324.8	331.8	346.7	241.8
	(236.9)	(239.7)	(277.4)	(296.1)	(320.0)	(339.0)	(356.8)	(387.4)	(394.3)	(402.2)	(305.2)
Re-employment in 6 months	0.693	0.665	0.609	0.585	0.536	0.525	0.502	0.490	0.486	0.473	0.593
Re-employment in 12 months	0.876	0.872	0.840	0.813	0.765	0.745	0.716	0.688	0.681	0.663	0.804
Share Exhausting UI	0.415	0.272	0.223	0.186	0.179	0.152	0.142	0.137	0.120	0.110	0.229
Previous Daily Wage	44.39	45.40	45.94	47.02	47.61	48.83	50.12	50.45	51.25	52.45	47.11
	(15.41)	(16.29)	(16.68)	(17.50)	(18.26)	(18.79)	(19.83)	(19.64)	(20.09)	(20.93)	(17.72)
Previous (6 Years) Tenure	492.9	622.0	802.9	985.9	1163.7	1347.1	1526.2	1709.7	1890.5	2023.9	1010.1
	(24.39)	(51.97)	(52.70)	(52.75)	(53.28)	(52.83)	(53.31)	(52.67)	(53.93)	(24.32)	(465.2)
Age	33.57	33.40	33.07	33.30	33.67	33.93	34.47	34.90	35.78	36.87	33.83
	(10.33)	(13.68)	(14.88)	(9.632)	(9.533)	(9.298)	(9.302)	(9.100)	(9.186)	(9.133)	(11.55)
Share Male	0.570	0.586	0.595	0.595	0.597	0.604	0.608	0.611	0.620	0.649	0.595
Share College	0.243	0.257	0.263	0.271	0.269	0.270	0.275	0.279	0.261	0.246	0.261
Share High School	0.401	0.420	0.426	0.438	0.436	0.442	0.456	0.450	0.437	0.424	0.428
Wealth*	35.29	37.85	39.00	43.60	46.56	51.01	56.05	61.16	68.10	76.30	45.54
	(35.83)	(36.43)	(35.29)	(36.44)	(36.17)	(36.08)	(38.73)	(38.71)	(40.54)	(41.70)	(38.47)
Wage Change	-0.0209	-0.0136	-0.0242	-0.0368	-0.0476	-0.0495	-0.0586	-0.0696	-0.0837	-0.0876	-0.0370
	(0.418)	(0.410)	(0.418)	(0.418)	(0.436)	(0.420)	(0.418)	(0.426)	(0.428)	(0.419)	(0.420)
Avg. Wage Change (1 Year)	0.0198	0.0211	0.0101	0.000210	-0.0123	-0.0168	-0.0274	-0.0361	-0.0492	-0.0567	-0.00176
	(0.363)	(0.365)	(0.376)	(0.377)	(0.395)	(0.384)	(0.391)	(0.395)	(0.398)	(0.390)	(0.379)
Avg. Wage Change (5 Years)	0.0500	0.0489	0.0409	0.0315	0.0214	0.0134	0.00266	-0.00501	-0.0202	-0.0270	0.0284
	(0.352)	(0.358)	(0.368)	(0.367)	(0.382)	(0.375)	(0.381)	(0.384)	(0.390)	(0.375)	(0.369)
Cross	0	0.560	0.550	0.532	0.545	0.518	0.528	0.501	0.491	1	0.476
N	37004	52793	36317	26735	21414	16633	14260	12713	13230	8383	239482

Note: Table 4 presents the summary statistics of the sample we will use for the remaining of the paper. Workers in the sample are within 85 days of one of the discontinuities we exploit in the paper. Compared to 3 the main difference is in the groups with potential duration 120 and 660, where approximately half of the sample is not used for estimation, since they are not within a short distance of one of the discontinuities exploited int his paper. Means and standard deviations (in parentheses) are shown.

Table 5: Effect of a 2-month UI extension. All discontinuities

						Panel A: Time in Unemployment						
RD Estimate	31.75***	34.65***	29.57***	31.15***	34.15***	29.86***	32.34***	33.50***	29.74***	31.43***	33.17***	29.23***
	[5.39]	[5.33]	[5.30]	[5.00]	[4.94]	[4.91]	[2.79]	[2.75]	[2.73]	[2.63]	[2.60]	[2.61]
Controls	No	Disc	All	No	Disc	All	No	Disc	All	No	Disc	All
Method	NP	NP	NP	P	P	P	NP	NP	NP	P	P	P
Bandwidth	24*	24 *	24 *	24	24	24	85	85	85	85	85	85
N	65003	65003	59905	65003	65003	59905	239482	239482	220855	239482	239482	220849
						Panel B: Re-employment ln daily wage change						
RD Estimate	-0.001	-0.001	-0.006	-0.007	-0.008	-0.010	-0.008*	-0.008**	-0.008**	-0.008**	-0.009**	-0.008**
	[0.008]	[0.008]	[0.007]	[0.007]	[0.007]	[0.006]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]
Controls	No	Disc	All	No	Disc	All	No	Disc	All	No	Disc	All
Method	NP	NP	NP	P	P	P	NP	NP	NP	P	P	P
Bandwidth	24 *	24 *	24 *	24	24	24	85	85	85	85	85	85
N	56226	56226	56071	56226	56226	56071	207457	207457	206918	207457	207457	206913

Note: Table 5 presents the estimation of the causal effect of a 2-month extension of UI on time in unemployment (panel (a)) and change in re-employment wage (panel (b)). Controls “No”: No controls. Controls “Disc”: Discontinuity fixed effects. Controls “All”: All controls included (see text). Method “NP”: Non parametric estimation following Calonico et al. (2019) with local polynomial. Method “P”: Parametric estimation with linear regression. Bandwidth: Indicates the length of the bandwidth. The star (*) indicates optimal bandwidth following Calonico et al. (2020), for a specification without controls, for the effect on time in unemployment of two additional months potential duration. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table 6: Balance Test. All discontinuities

				Panel A: Age				
RD Estimate	-0.135	-0.048	-0.114	-0.041	0.346***	0.364***	0.358***	0.375***
	[0.170]	[0.170]	[0.156]	[0.155]	[0.089]	[0.089]	[0.100]	[0.099]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	65003	65003	65003	65003	239482	239482	239482	239482
				Panel B: Male				
RD Estimate	-0.006	-0.004	-0.005	-0.004	-0.003	-0.003	-0.003	-0.003
	[0.009]	[0.009]	[0.008]	[0.008]	[0.004]	[0.004]	[0.004]	[0.004]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	65003	65003	65003	65003	239482	239482	239482	239482
				Panel B: High School				
RD Estimate	-0.007	-0.007	-0.006	-0.006	-0.003	-0.003	-0.000	0.000
	[0.009]	[0.009]	[0.008]	[0.008]	[0.004]	[0.004]	[0.004]	[0.004]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	64851	64851	64851	64851	238907	238907	238907	238907
				Panel D: College				
RD Estimate	-0.015*	-0.015*	-0.015**	-0.015**	-0.006	-0.006	-0.003	-0.003
	[0.008]	[0.008]	[0.007]	[0.007]	[0.004]	[0.004]	[0.004]	[0.004]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	64851	64851	64851	64851	238907	238907	238907	238907
				Panel E: *ln* Wealth				
RD Estimate	-0.002	0.018	0.010	0.030**	0.038***	0.045***	0.034***	0.044***
	[0.015]	[0.014]	[0.014]	[0.013]	[0.008]	[0.007]	[0.007]	[0.007]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	64971	64971	64971	64971	239363	239363	239363	239363
				Panel F: *ln* Previous Daily Wage				
RD Estimate	-0.009	-0.006	-0.010*	-0.007	-0.002	-0.001	-0.002	-0.000
	[0.006]	[0.006]	[0.006]	[0.006]	[0.003]	[0.003]	[0.003]	[0.003]
Controls	No	Disc	No	Disc	No	Disc	No	Disc
Method	NP	NP	P	P	NP	NP	P	P
Bandwidth	24*	24 *	24	24	85	85	85	85
N	60080	60080	60080	60080	221484	221484	221484	221484

Note: Table 5 presents the balance test of a 2-month extension of UI on different observed worker characteristics. Controls "No": No controls. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (see text). Method "NP": Non parametric estimation following Calonico et al. (2019) with local polynomial. Method "P": Parametric estimation with linear regression. Bandwidth: Indicates the length of the bandwidth. The star (*) indicates optimal bandwidth following Calonico et al. (2020), for a specification without controls, for the effect on time in unemployment of two additional months potential duration. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table 7: **Manipulation tests**

	Conventional Estimates	Robust Estimates
Panel (a): Original sample (Figure 1 (c))		
T-statistics ($P > \|T\|$)	12.41*** (0.00)	-10.04**(0.00)
Panel (b): No temporary contracts with fixed duration (Figure 2 (c))		
T-statistics ($P > \|T\|$)	-5.60*** (0.00)	-0.51 (0.60)
Panel (c): Final sample (Figure 3 (c))		
T-statistics ($P > \|T\|$)	-2.25** (0.02)	2.47** (0.01)

Note: This table presents the manipulation testing using both conventional and robust methods by Calonico et al. (2020). Panel (a) presents the results using our original sample. Panel (b) presents the results when removing unemployment spells where workers enter unemployment from temporary contracts and their previous tenure is a multiple of 1/2 years. Panel (c) presents the results when removing unemployment spells where workers enter unemployment from temporary contract and the previous tenure is a multiple of 1/2 years and unemployment spells whose time collecting UI matches a different potential duration than their (calculated) potential duration. All manipulation tests are conducted with optimal bandwidth, following Calonico et al. (2020). p-value: * 0.10 ** 0.05, *** 0.01

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

	Panel A: Re-employment *ln* daily wage change							
DiD Estimate	-0.026***	-0.027***	-0.028***	-0.032***	-0.024***	-0.028***	-0.019*	-0.021**
	[0.006]	[0.006]	[0.006]	[0.006]	[0.007]	[0.006]	[0.012]	[0.011]
Controls	D	All	D	All	D	All	D	All
Bandwidth	85	85	85	85	85	85	24	24
Start (Days)	-30	-30	-15	-15	-15	-15	-15	-15
End (Days)	30	30	45	45	30	30	45	45
N	207457	206913	207457	206913	207457	206913	56226	56071
	Panel B: Re-employment *ln* hourly wage change							
DiD Estimate	-0.006	-0.009**	-0.000	-0.007*	-0.002	-0.009**	0.004	-0.004
	[0.005]	[0.004]	[0.005]	[0.004]	[0.006]	[0.004]	[0.010]	[0.008]
Controls	D	All	D	All	D	All	D	All
Bandwidth	85	85	85	85	85	85	24	24
Start (Days)	-30	-30	-15	-15	-15	-15	-15	-15
End (Days)	30	30	45	45	30	30	45	45
N	207457	206913	207457	206913	207457	206913	56226	56071
	Panel C: Time in unemployment							
DiD Estimate	3.997	2.122	-0.011	-1.626	0.347	-1.855	6.358	6.001
	[2.796]	[2.840]	[2.804]	[2.846]	[3.201]	[3.247]	[5.319]	[5.401]
Controls	D	All	D	All	D	All	D	All
Bandwidth	85	85	85	85	85	85	24	24
Start (Days)	-30	-30	-15	-15	-15	-15	-15	-15
End (Days)	30	30	45	45	30	30	45	45
N	239482	220849	239482	220849	239482	220849	65003	59905

Note: Table 8 presents the RD-in-Difference estimates that identify the causal effect of benefit exhaustion on re-employment wage drop. Workers are included in the treatment or control group if they are located within a certain bandwidth of one of the discontinuities that extends the potential duration by 2 months. Panel (a) presents the specification of the log of the daily re-employment wage (relative to the log of the previous daily wage). Panel (b) presents the specification of the log of the hourly re-employment wage (relative to the log of the previous hourly wage). Panel (c) presents the specification of time in unemployment. Controls "D": Controls by potential duration of the worker. "Bandwidth" refers to the chosen bandwidth, relative to the RDD discontinuities, to include an individual in the sample, either in the control or treatment group. Control "All": Controls by all observed worker and economy characteristics. "Start (Days)" refers to the initial point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits starts. "End" refers to the final point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits ends. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table 9: Balance test exhaustion of UI. All discontinuities

Bandwidth	85	85	85	24
Start (Days)	-30	-15	-15	-15
End (Days)	30	45	30	45
	Panel A: Age			
DiD Estimate	-0.014	0.071	0.071	-0.199
	[0.168]	[0.172]	[0.172]	[0.268]
Controls	D	D	D	D
N	239482	239482	239482	65003
	Panel B: Male			
DiD Estimate	-0.022***	-0.023***	-0.023***	-0.022
	[0.007]	[0.007]	[0.007]	[0.014]
Controls	D	D	D	D
N	239482	239482	239482	65003
	Panel C: High School			
DiD Estimate	-0.002	0.001	0.001	0.013
	[0.007]	[0.007]	[0.007]	[0.014]
Controls	D	D	D	D
N	238907	238907	238907	64851
	Panel D: College			
DiD Estimate	0.000	0.003	0.003	0.016
	[0.006]	[0.006]	[0.006]	[0.012]
Controls	D	D	D	D
N	239363	239363	239363	64971
	Panel E: ln Wealth			
DiD Estimate	-0.004	-0.013	-0.013	-0.026
	[0.012]	[0.012]	[0.012]	[0.022]
Controls	D	D	D	D
N	240636	240636	240636	60080
	Panel F: ln Previous daily wage			
DiD Estimate	-0.004	-0.010*	-0.010*	-0.004
	[0.005]	[0.005]	[0.005]	[0.010]
Controls	D	D	D	D
N	221484	221484	221484	60080

Note: Table 8 presents the RD-in-Difference estimates for the balance test of observed characteristics around the benefit exhaustion. Workers are included in the treatment or control group if they are located within a certain bandwidth of one of the discontinuities that extends the potential duration by 2 months. Each panel presents a different covariate. "Bandwidth" refers to the chosen bandwidth, relative to the RDD discontinuities, to include an individual in the sample, either in the control or treatment group. Controls "D": Controls by potential duration of the worker. "Start (Days)" refers to the initial point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits starts. "End (Days)" refers to the final point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits ends. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table 10: Re-employment wage change. Treatment vs control. By pre-exhaustion period

	Time Unemployment	Time Unemployment	Re-employment Wage	Re-employment Wage
1 to 2 months prior to exhaustion	8.176**	0.034	0.005	0.008
	[3.487]	[0.079]	[0.007]	[0.007]
2 to 3 months prior to exhaustion	6.175*	0.050	-0.010	-0.008
	[3.253]	[0.074]	[0.007]	[0.006]
3 to 4 months prior to exhaustion	4.540	-0.156**	-0.009	-0.007
	[3.259]	[0.073]	[0.007]	[0.006]
4 to 5 months prior to exhaustion	0.335	-0.059	-0.013*	-0.013
	[3.838]	[0.086]	[0.008]	[0.008]
5 to 6 months prior to exhaustion	-2.666	-0.001	-0.005	-0.007
	[3.854]	[0.086]	[0.008]	[0.008]
6 to 7 months prior to exhaustion	-10.797**	0.325***	0.010	0.007
	[4.477]	[0.100]	[0.009]	[0.009]
7 to 8 months prior to exhaustion	-9.965**	-0.019	0.014	0.012
	[4.455]	[0.100]	[0.009]	[0.009]
8 to 9 months prior to exhaustion	-14.339***	-0.105	0.002	-0.002
	[5.146]	[0.115]	[0.010]	[0.010]
9 to 10 months prior to exhaustion	-15.332***	-0.067	0.023**	0.019*
	[5.108]	[0.115]	[0.010]	[0.010]
10 to 11 months prior to exhaustion	-21.539***	-0.174	0.005	-0.000
	[5.856]	[0.131]	[0.012]	[0.012]
11 to 12 months prior to exhaustion	-23.256***	0.030	-0.005	-0.011
	[5.763]	[0.130]	[0.012]	[0.011]
12 to 13 months prior to exhaustion	-24.234***	-0.153	0.049***	0.042***
	[6.445]	[0.145]	[0.013]	[0.013]
13 to 14 months prior to exhaustion	-18.621***	0.158	-0.001	-0.006
	[6.357]	[0.143]	[0.013]	[0.013]
14 to 15 months prior to exhaustion	-20.624***	0.005	0.006	-0.000
	[7.262]	[0.163]	[0.015]	[0.015]
Controls	All	All	All	All
Time Unemployment	No	Yes	No	Yes
N	220849	220849	206913	206913

Note: Table 10 presents the estimation of the impacts of two additional month UI benefits on the re-employment wage prior to the exhaustion of UI. Columns (3) and (4) show the estimates without/with unemployment duration fixed effects as a control. For each row indexed by τ, the table shows the impact of UI extensions on the re-employment wage at 1 to $\tau + 1$ months prior to the exhaustion of UI in the treatment group vs the control group.

Table 11: Effect of a 2-month UI extension on probability of exiting unemployment

1 to 2 months prior to exhaustion	-0.002	-0.008***
	[0.004]	[0.002]
1 to 3 months prior to exhaustion	-0.011*	-0.012***
	[0.006]	[0.003]
1 to 4 months prior to exhaustion	-0.017**	-0.013***
	[0.007]	[0.004]
1 to 6 months prior to exhaustion	-0.013*	-0.017***
	[0.008]	[0.004]
1 to 7 months prior to exhaustion	-0.027***	-0.026***
	[0.008]	[0.004]
1 to 8 months prior to exhaustion	-0.024**	-0.018***
	[0.010]	[0.005]
1 to 9 months prior to exhaustion	-0.036***	-0.026***
	[0.010]	[0.005]
1 to 10 months prior to exhaustion	-0.024**	-0.027***
	[0.011]	[0.006]
1 to 11 months prior to exhaustion	-0.030**	-0.035***
	[0.012]	[0.006]
1 to 12 months prior to exhaustion	-0.017	-0.025***
	[0.014]	[0.007]
1 to 13 months prior to exhaustion	-0.008	-0.023***
	[0.014]	[0.007]
1 to 14 months prior to exhaustion	-0.013	-0.028***
	[0.016]	[0.008]
1 to 15 months prior to exhaustion	-0.024	-0.030***
	[0.016]	[0.008]
Controls	All	All
Time Unemployment	No	Yes

Note: Table 11 presents the estimation of the impact of two month UI extensions on the probability of finding jobs prior to the original UI potential duration. For each row indexed by τ, we calculate how the probability of finding a job at 1 to $\tau + 1$ months prior to the original UI potential duration decrease when workers are given two months of UI benefits.

Table 12: Labor market opportunity loss estimates (LMOS). In sample

	(1)	(2)	(3)	(4)
LMOS	-0.0078	-0.0125	-0.0117	-0.0121
Controls	All	All	All	All
Method	NP	P	NP	P
Bandwidth	24	24	85	85

Note: Table 12 presents the estimated labor market opportunity loss in re-employment wage (monthly). Controls "All": All controls included in both RDD and DID specifications (see text). Method "NP": Non-parametric estimation of RD results following Calonico et al. (2019) with local polynomial. Method "P": Parametric estimation of RD results with linear regression. Bandwidth: Indicates the length of the bandwidth used for the RD estimation and for the creation of the treatment and control groups in the DID specification (see text for details).

Table 13: Labor market opportunity loss estimates (LMOS). Bootstrap sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LMOS	-0.0120**	-0.0117**	-0.0100**	-0.0102**	-0.0116***	-0.0122***	-0.0121***
SD	0.0061	0.0050	0.0042	0.0041	0.0042	0.0041	0.0036
$p5 - p95$	[-0.022, -0.001]	[-0.020, -0.003]	[-0.018, -0.003]	[-0.017, -0.003]	[-0.019, -0.005]	[-0.023, -0.006]	[-0.019, -0.007]
$p1 - p99$	[-0.026, 0.004]	[-0.023, 0.000]	[-0.020, -0.002]	[-0.020, -0.000]	[-0.021, -0.002]	[-0.019, -0.003]	[-0.020, -0.003]
Controls	All	All	All	All	All	All	All
Method	P	P	P	P	P	P	P
Bandwidth	25	35	45	55	65	75	85
Bootstraps	300	300	300	300	300	300	300

Note: Table 13 presents the estimated labor market opportunity loss in re-employment wage (monthly) from 300 bootstraps of the complete sample. The sample is bootstrapped at the spell level. SD: Standard deviation of the LMOS estimate. Controls "All": All controls included in both RDD and DID specifications (see text). Method "P": Parametric estimation of RD results with linear regression. Bandwidth: Indicates the length of the bandwidth used for the RD estimation and for the creation of the treatment and control groups in the DID specification (see text for details).

Table 14: Labor market opportunity loss estimates (LMOS) vs
Duration Dependence (DD) in Schmieder and von Wachter (2016)
Bootstrap sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LMOS (This Paper)	-0.0120	-0.0117	-0.0099	-0.0102	-0.0116	-0.0122	-0.0121
SD	0.0061	0.0050	0.0042	0.0041	0.0042	0.0041	0.0036
DD (Schmieder et al (2016))	-0.0096	-0.0091	-0.0071	-0.0066	-0.0074	-0.0079	-0.0081
SD	0.0059	0.0049	0.0040	0.0041	0.0042	0.0040	0.0035
Difference (abs value)	+27%	+30%	+39%	+54%	+58%	+54%	+51%
Controls	All	All	All	All	All	All	All
Method	P	P	P	P	P	P	P
Bandwidth	25	35	45	55	65	75	85
Bootstraps	300	300	300	300	300	300	300

Note: Table 14 presents the estimated labor market opportunity loss in re-employment wage (monthly) from 300 bootstraps of the complete sample, and compares it to the duration dependence estimate suggested in Schmieder and von Wachter (2016). The sample is bootstrapped at the spell level.SD: Standard deviation of the LMOS estimate. Controls "All": All controls included in both RD and DID (LMOS only) specifications (see text). Method "P": Parametric estimation of RD results with linear regression. Bandwidth: Indicates the length of the bandwidth used for the RD estimation and for the creation of the treatment and control groups in the DID specification (see text for details).

Table 15: **Manipulation estimation**

Parameters	Estimates
$D_{B,-}$	-22.5** (8.2)
$D_{B,+}$	9**(2.3)
$\hat{S}/D_{B,+}$	–
$\hat{M}/D_{B,-}$	–

Table 16: Parametric Assumptions

Functions	**Descriptions**	**Parametric Form**
$u(c)$	Utility function while working	$ln(c) - l$
$v(c)$	Utility function while not working	$ln(c)$
$a(t)$	Search technology - scalar	a
$\sigma(t)$	Search technology - effort share	σ
$\rho(t)$	Labor market opportunity	$\rho_0 + \rho_1 \cdot t$

Table 17: Model Estimates (half-month estimates)

Entitlement Group	1-5	6-10
Discount factors		
(β_L, β_H)	(0.86,0.99)	
Leisure		
l	0.105	
Impatient population share		
pr_L	0.4	
Human capital parameters		
(ρ_{0L}, ρ_{0L})	(4.28,36.0)	(4.30,36.2)
(ρ_{1L}, ρ_{1L})	(-0.0063,-0.20)	(-0.005,-0.15)
Search technology parameters		
(a_L, a_H)	(10500.0,4.2$\times 10^6$	
(σ_L, σ_H)	(3.2,1.5)	
Number of moment sets	4	
Number of estimated parameters	11	

Table 18: Model prediction for RDD sample

RDD on unemployment duration		RDD on re-employment wages	
data	model	data	model
29.6***	24.7	-0.006	0.0006

Appendix

A Robustness Check

A.1 Present-Bias v.s. Other Explanations

We briefly examine whether present bias is the exclusive reason driving the re-employment wage drop upon benefit exhaustion. Two alternative "search-free" explanations have been suggested by the literature to explain this sudden decline in wages[40]. The first explanation is the story of recalled workers. Firms and workers may establish contracts that workers will be re-employed (recalled) once they claimed all the UI from government. These types of contracts are usually associated with low monetary payment. Therefore, the observed re-employment wage drop may only capture a larger proportion of low wage jobs, since the UI exhausts. Second, the story of storable offers can also account for the observed wage drop. Once the workers receive a job offer, they are allowed a certain period of time to store it until they accept it. A typical strategy for workers is to delay the working start date to coincide with the benefit expiration. Therefore, the workers who start working shortly before the UI exhaustion on average find a job much earlier than the workers who starts working shortly after the UI exhaustion. If there is a nonzero labor market opportunity loss, the wage drop around the benefit exhaustion is reflecting a timing difference in receiving a job offer.

We argue that the second story of storable offers is more likely to be a potential completing mechanism in explaining the wage drop at benefit exhaustion. There are three reasons. First, we have already get rid of the most cases where is the re-employment is labeled as "recalled" in social administrative system. Second, the heterogeneous analysis of the wage drop at benefit exhaustion is in less favor of the first story of recalled contract. We don't find any evidence suggesting that among workers with less human capital or with a lower quality of next job will suffer from a less wage drop at the benefit exhaustion. Last, as we will show in the following, the hazard rate of job finding around the benefit exhaustion presents a shape that is supporting the storable offer explanation, instead of the recalled contract.

A.2 Hazard Rate of Job Finding

In figure 7, we graphically present re-employment rates, using 5-day hazard rate as a proxy over the calendar time of unemployment. In panel (a), we first plot it respectively for workers with different potential duration of benefits, as well as for those who are not eligible for any UI. There indeed presents an increasing trend and decreasing trend respectively for before and after benefit

[40] A static model of unemployment (Moffitt (1985)) cannot explain it, since the model requires the wage to be fixed. A reference-dependent model also has no ability to explain the re-employment wage drop. Both have a certain ability to explain the hazard rate spike upon UI exhaustion.

exhaustion. The spike is clear around the assigned benefit potential duration for workers who are eligible for UI, as in the previous literature.

To further analyze the effect of benefit exhaustion, we pool workers with different UI potential duration together and plots the re-employment rate against time in unemployment (relative to the potential duration). Panel (b) of figure 7 shows the evolution of 5-day hazard rate, where the 0 of the x-axis denotes the timing when UI expires. It clearly shows a spike when UI exhausts. The red line of panel (b) in figure 7 presents the fitted prediction from using a Cox regression. However, what is different from the previous literature is that there exists a missing area before benefit exhaustion, and a discontinuous jump in hazard rate just right after benefits exhausts. Compared to our paper, DellaVigna et al. (2020) and Marinescu and Skandalis (2021) found a more continuous spike in hazard rate analysis.

Another way to present the re-employment rate is to directly examine the distribution of the time spent elapsed to re-employment job (time in unemployment). Figure **??** shows the empirical histogram of time spent elapsed to re-employment job (1-day level, in frequency term) around the benefit exhaustion (60 days before and after). We find the odds of re-employment increase by a bulky amount just right after the benefit exhaustion. At the same time, there seems to exist a missing "mass" of workers before the exhaustion of benefits, in companion with the bunching "mass" after the UI expires.

A.3 Manipulation Analysis

Enlightened by this reallocation of working time, we use a bunching analysis approach developed by Chetty et al. (2011) and Kleven and Waseem (2013) to estimate the length and the intensity of this manipulation of re-employment timing behavior. We perform this bunching analysis on the hazard rate sequence, trying to understand which proportion of unemployed workers manipulate themselves to just start working immediately after UI exhausts.

We fit a flexible polynomial or semi-parametric regression to the empirical distribution of the hazard rate $h_{i,t}$, excluding a region around the benefit exhaustion $[D_{B,-}, D_{B,+}]$. With the fitted regression, we predict the counterfactual distribution $\hat{h}_{i,t}$ for the excluded region. We can get a predicted counterfactual distribution for every possible choice of $(D_{B,-}, D_{B,+})$. We iterate over all possible combinations of $(D_{B,-}, D_{B,+})$ such that the difference between the missing "mass" $\hat{M}$ and the spiking "mass" $\hat{S}$ is minimized:

$$(D^*_{B,-}, D^*_{B,+}) = arg\min|\hat{S} - \hat{M}| \tag{17}$$

$$\hat{S} = \sum_{t=B}^{D_{B,+}} (h_{i,t} - \hat{h_{i,t}}) \quad \hat{M} = \sum_{t=D_{B,-}}^{B} (\hat{h_{i,t}} - h_{i,t})$$

Panel (a) of figure 15 presents the empirical distribution of the 1 day hazard rate together with the optimal counterfactual distribution. The two extra vertical red line are respectively the optimal solution to the minimization problem of equation (24), $(D_{B,-}, D_{B,+})$. It is very clear from the graph that the odd of finding job almost doubles right after the exhaustion of the benefit compared to one day before. The counterfactual hazard rate, however, experiences a continuous increase across the time of benefit exhaustion and reaches its peak around half month afterwards. One may be worried that the spikes at $t = B + 30 \cdot \tau, t = B - 30 \cdot \tau, \tau = 1, 2, \cdots$ confounds our analysis. It is unlikely since the results from cox regression shows that the spike at places other than the UI exhaustion turns to be 10 times smaller. Further, to solve this concern, we replace the spiking value at $t = B + 30 \cdot \tau, t = B - 30 \cdot \tau, \tau = 1, 2, \cdots$ by the prediction from the cox hazard rate regression when shutting down the spiking effects. Then we perform the same minimization process defined in (24) on the transformed data. Panel (b) of figure 15 shows the results. We don't find any significant difference between the predicted counterfactual hazard rate path of panel (a) and that of panel (b).

Table 15 presents the estimated results with bootstrapped standard error. Looking at the optimal solution of $(D_{B,-}, D_{B,+})$, it shows that the bunching "mass" after the exhaustion of benefit concentrates in the first 9 days, and can be supplemented by the missing "mass" between $(B-30, B)$ days. If we accept the story of delaying working start date, therefore, 0.27 percent of the worker who found a job at $(B-30, B)$ delay their working start date until benefit exhaustion and contributes to 0.12 percent of the hazard rate spike.

Heterogeneity and Sub-sample Analysis We perform heterogeneity analysis to further understand the nature and the cause of the hazard rate spike. We first analyze the heterogeneity on observed demographics of the worker, for example, age, wealth, gender, education, etc. We present these results in figure **??**. Surprisingly, we don't find any significant difference of the spike across these demographics.

Second, we tend to focus on the group of workers who were not likely to sort themselves to a specific date of unemployed or longer potential duration. The value of the exercise here is to show that the potential issue with the RDD exercise in subsection 6.1 is not generating the hazard rate spike here. We first check the spike of workers whose previous job was temporary contract job v.s permanent contract job since permanent contract tends to end at a predetermined duration. Second, we test if the spike disappear if we only focus on the workers around the tenure cutoffs that do not present a huge bunching. Last, we directly examine if removing the workers at the bunching of the tenure distribution will affect hazard rate spike. In all these three cases, we don't find any significant change in hazard rate when we do the corresponding changes.

Lastly, enlightened by Boone and van Ours (2012), we examine whether the spike at benefit exhaustion differ across the characteristics of the re-employment job. We find that the bunching is much more pronounced, and generating a significantly larger spike, when the next job of the re-employment entails a permanent contract and temporary contract. Present in Figure **??**, we can see that if the next job is temporary contract, we do not find a significant bunching at all. These results are consistent with the theoretical prediction that a job with a longer promised tenure tends to have more scope for allowing workers to spend time deciding whether or not to accept or to postpone the working start date. However, the wage drop before and after keeps the same regardless of whether the next job is temporary contract or a permanent contract. It is suggesting that the delaying offer cannot be the only mechanism generating the wage drop at the end of benefit exhaustion. On the other hand, in Spain, permanent contract are providing much better non-monetary benefit to workers than temporary contract. Since workers are not sacrificing the wage requirement for better non-monetary benefits to pursue a permanent contract job, therefore, according to the prediction from equation (10), it is unlikely that the increased effort will be able to generate a higher spike.

A.4 Implications for Our Main Results

Does including this "search-free" behavior of delaying working start date change our estimation formula for labor market opportunity loss? Does it change our interpretation of wage drop at benefit exhaustion?

For the first question, the answer is that it does not affect our estimation of labor market opportunity loss. We show the proof in Appendix. The intuition is that the delaying offers will change both time spent in unemployment and observed wage path in an off-setting way. In the end, we reach to the same equation and the same estimates for labor market opportunity loss.

However, it does change the interpretation of the re-employment wage drop at benefit exhaustion since it represents a sum of both response in the targeted wage to a benefit cut and monetary value of the leisure workers when delaying working start date. Suppose that the workers can delay their working start date by at most one month (consistent with our bunching analysis and previous empirical results that show that the storing offers behavior does not change around the exhaustion of benefits (DellaVigna et al. (2020)). We find that 77% of the wage drop at the end of benefit exhaustion is generated by the reduced wage selectivity when benefit expires. The rest of the 23% wage drop is due to the mechanism that the workers delay working starting date much less to coincide with the exhaustion of benefit.

B Additional Figures & Tables

Table B.1: Effect of a 2-month UI extension. All discontinuities

Panel A: Re-employment *ln* daily wage change (1Y average)												
RD Estimate	-0.004	-0.006	-0.009	-0.007	-0.009	-0.010*	-0.007**	-0.008**	-0.007**	-0.006*	-0.007**	-0.006**
	[0.007]	[0.007]	[0.006]	[0.006]	[0.006]	[0.006]	[0.004]	[0.004]	[0.003]	[0.003]	[0.003]	[0.003]
Controls	No	Disc	All	No	Disc	All	No	Disc	All	No	Disc	All
Method	NP	NP	NP	P	P	P	NP	NP	NP	P	P	P
Bandwidth	24*	24 *	24 *	24	24	24	85	85	85	85	85	85
N	59287	59287	59126	59287	59287	59126	218736	218736	218172	218736	218736	218167
Panel B: Re-employment *ln* daily wage change (5Y average)												
RD Estimate	-0.003	-0.004	-0.007	-0.004	-0.006	-0.008	-0.007*	-0.007**	-0.007**	-0.006*	-0.007**	-0.006**
	[0.007]	[0.007]	[0.006]	[0.006]	[0.006]	[0.005]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Controls	No	Disc	All	No	Disc	All	No	Disc	All	No	Disc	All
Method	NP	NP	NP	P	P	P	NP	NP	NP	P	P	P
Bandwidth	24 *	24 *	24 *	24	24	24	85	85	85	85	85	85
N	59496	59496	59334	59496	59496	59334	219485	219485	218913	219485	219485	218908

Note: Table 5 presents the estimation of the causal effect of a 2-month extension of UI on time in unemployment (panel (a)) and change in re-employment wage (panel (b)). Controls "No": No controls. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (see text). Method "NP": Non parametric estimation following Calonico et al. (2019) with local polynomial. Method "P": Parametric estimation with linear regression. Bandwidth: Indicates the length of the bandwidth. The star (*) indicates optimal bandwidth following Calonico et al. (2020), for a specification without controls, for the effect on time in unemployment of two additional months potential duration. Outcome 1Y average calculates the average re-employment daily wage over the first year of re-employment, or until the worker re-enters unemployment, if the worker re-enters unemployment during the first year of re-employment. Outcome 5Y average calculates the average re-employment daily wage over the first five years of re-employment, or until the worker re-enters unemployment, if the worker re-enters unemployment during within the first 5 years of re-employment Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table B.2: Effect of a 2-month UI extension. All discontinuities

	Panel A: Time in Unemployment			
RD Estimate	34.227***	22.235	28.361***	30.701***
	[6.671]	[14.832]	[3.490]	[4.337]
Individual FE	No	Yes	No	Yes
Controls	All	All	All	All
Method	P	P	P	P
Bandwidth	24	24	85	85
N	26806	9087	98954	95113
	Panel B: Re-employment ln daily wage change			
RD Estimate	-0.006	-0.007	-0.008	-0.010*
	[0.009]	[0.021]	[0.005]	[0.006]
Individual FE	No	Yes	No	Yes
Controls	All	All	All	All
Method	P	P	P	P
Bandwidth	24	24	85	85
N	25252	8141	93325	86425
	Panel C: Re-employment ln hourly wage change			
RD Estimate	-0.005	-0.010	-0.003	-0.000
	[0.006]	[0.014]	[0.003]	[0.004]
Individual FE	No	Yes	No	Yes
Controls	All	All	All	All
Method	P	P	P	P
Bandwidth	24	24	85	85
N	25252	8141	93325	86425

Note: Table 5 presents the estimation of the causal effect of a 2-month extension of UI on time in unemployment (panel (a)) and change in re-employment wage (panels (b) and (c)) for a sample of workers that enter unemployment more than once. Columns (1) and (3) present the estimated results without individual fixed effects and Columns (2) and (4) with indvidual fixed effects. Controls "No": No controls. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls included (see text). Method "NP": Non parametric estimation following Calonico et al. (2019) with local polynomial. Method "P": Parametric estimation with linear regression. Bandwidth: Indicates the length of the bandwidth. The star (*) indicates optimal bandwidth following Calonico et al. (2020), for a specification without controls, for the effect on time in unemployment of two additional months potential duration. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

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

	Panel A: Re-employment *ln* daily wage change (1Y average)							
DiD Estimate	-0.028***	-0.028***	-0.029***	-0.032***	-0.026***	-0.028***	-0.020**	-0.021**
	[0.006]	[0.005]	[0.006]	[0.005]	[0.006]	[0.006]	[0.011]	[0.010]
Controls	D	All	D	All	D	All	D	All
Bandwidth	85	85	85	85	85	85	24	24
Start (Days)	-30	-30	-15	-15	-15	-15	-15	-15
End (Days)	30	30	45	45	30	30	45	45
N	218736	218167	218736	218167	218736	218167	59287	59126
	Panel B: Re-employment *ln* daily wage change (5Y average)							
DiD Estimate	-0.019***	-0.019***	-0.021***	-0.024***	-0.018***	-0.020***	-0.019**	-0.021**
	[0.005]	[0.005]	[0.006]	[0.005]	[0.006]	[0.005]	[0.010]	[0.009]
Controls	D	All	D	All	D	All	D	All
Bandwidth	85	85	85	85	85	85	24	24
Start (Days)	-30	-30	-15	-15	-15	-15	-15	-15
End (Days)	30	30	45	45	30	30	45	45
N	219485	218908	219485	218908	219485	218908	59496	59334

Note: Table 8 presents the RD-in-Difference estimates that identify the causal effect of benefit exhaustion on re-employment wage drop. Workers are included in the treatment or control group if they are located within a certain bandwidth of one of the discontinuities that extends the potential duration by 2 months. Panel (a) presents the specification of the log of the daily re-employment wage (relative to the log of the previous daily wage). Panel (b) presents the specification of the log of the hourly re-employment wage (relative to the log of the previous hourly wage). Panel (c) presents the specification of time in unemployment. Controls "D": Controls by potential duration of the worker. "Bandwidth" refers to the chosen bandwidth, relative to the RDD discontinuities, to include an individual in the sample, either in the control or treatment group. Control "All": Controls by all observed worker and economy characteristics. "Start (Days)" refers to the initial point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits starts. "End (Days)" refers to the final point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits ends. Outcome 1Y average calculates the average re-employment daily wage over the first year of re-employment, or until the worker re-enters unemployment, if the worker re-enters unemployment during the first year of re-employment. Outcome 5Y average calculates the average re-employment daily wage over the first five years of re-employment, or until the worker re-enters unemployment, if the worker re-enters unemployment during within the first 5 years of re-employment. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table B.4: Effect of the exhaustion of UI. All discontinuities

Panel A: Re-employment ln daily wage change		
DiD Estimate	-0.029***	-0.033***
	[0.008]	[0.010]
Individual FE	No	Yes
Controls	All	All
Bandwidth	85	85
Start (Days)	-15	-15
End (Days)	45	45
N	93325	86425
Panel B: Time in Unemployment (Days)		
DiD Estimate	-1.694	1.135
	[3.620]	[4.743]
Individual FE	No	Yes
Controls	All	All
Bandwidth	85	85
Start (Days)	-15	-15
End (Days)	45	45
N	98954	95113

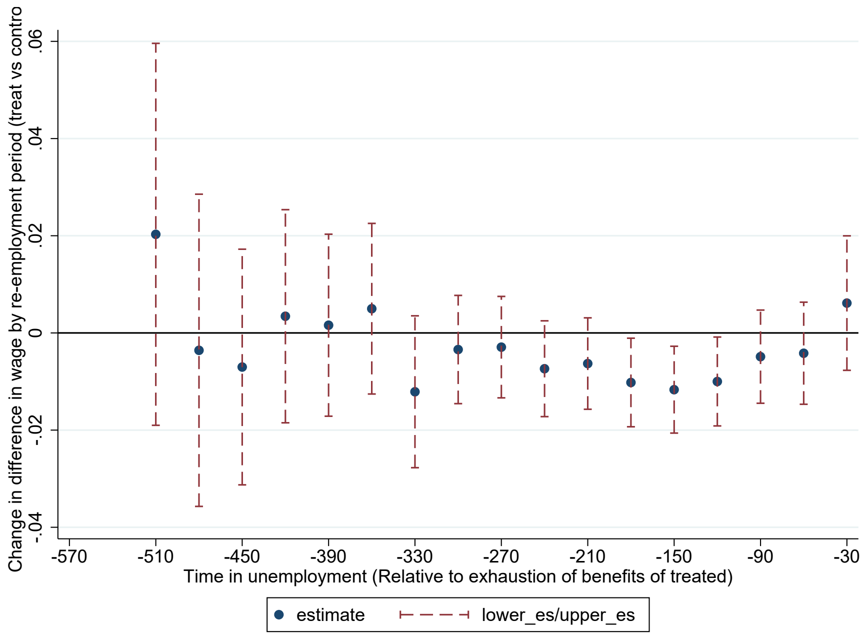
Note: Table 8 presents the RD-in-Difference estimates that identify the causal effect of benefit exhaustion on re-employment wage drop for a sample of workers that enter unemployment more than once. Columns (1) presents the estimated results without individual fixed effects and Column (2) with indvidual fixed effects. Workers are included in the treatment or control group if they are located within a certain bandwidth of one of the discontinuities that extends the potential duration by 2 months. Panel (a) presents the specification of the log of the daily re-employment wage (relative to the log of the previous daily wage). Panel (b) presents the specification of the log of the hourly re-employment wage (relative to the log of the previous hourly wage). Panel (c) presents the specification of time in unemployment. Controls "D": Controls by potential duration of the worker. "Bandwidth" refers to the chosen bandwidth, relative to the RDD discontinuities, to include an individual in the sample, either in the control or treatment group. Control "All": Controls by all observed worker and economy characteristics. "Start (Days)" refers to the initial point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits starts. "End (Days)" refers to the final point of the distribution of previous tenure (relative to the exhaustion of benefits) used to determine where the effect of the exhaustion of benefits ends. Standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table B.5: Re-employment wage change. Treatment vs control. By pre-exhaustion period

	Time Unemployment	Time Unemployment	Re-employment Wage	Re-employment Wage
1 to 2 months prior to exhaustion	8.176**	0.034	0.005	0.008
	[3.487]	[0.079]	[0.007]	[0.007]
1 to 2 months prior to exhaustion	12.914***	0.052	-0.005	-0.001
	[2.563]	[0.059]	[0.005]	[0.005]
1 to 3 months prior to exhaustion	19.506***	-0.036	-0.011**	-0.005
	[2.268]	[0.053]	[0.005]	[0.005]
1 to 4 months prior to exhaustion	13.979***	-0.059	-0.013***	-0.009**
	[2.135]	[0.051]	[0.005]	[0.004]
1 to 5 months prior to exhaustion	11.077***	-0.058	-0.015***	-0.012**
	[2.107]	[0.051]	[0.004]	[0.004]
1 to 6 months prior to exhaustion	7.295***	0.025	-0.012***	-0.010**
	[2.131]	[0.051]	[0.005]	[0.005]
1 to 7 months prior to exhaustion	4.251*	0.022	-0.008*	-0.007
	[2.211]	[0.053]	[0.005]	[0.005]
1 to 8 months prior to exhaustion	2.149	-0.001	-0.009*	-0.008
	[2.308]	[0.056]	[0.005]	[0.005]
1 to 9 months prior to exhaustion	-0.482	-0.020	-0.004	-0.004
	[2.464]	[0.059]	[0.005]	[0.005]
1 to 10 months prior to exhaustion	-3.429	-0.061	-0.004	-0.004
	[2.628]	[0.063]	[0.006]	[0.006]
1 to 11 months prior to exhaustion	-5.664**	-0.063	-0.007	-0.008
	[2.868]	[0.068]	[0.006]	[0.006]
1 to 12 months prior to exhaustion	-8.031**	-0.114	0.003	0.001
	[3.129]	[0.074]	[0.007]	[0.007]
1 to 13 months prior to exhaustion	-11.839***	-0.090	0.002	-0.001
	[3.536]	[0.083]	[0.007]	[0.007]
1 to 14 months prior to exhaustion	-13.018***	-0.108	0.002	-0.001
	[3.970]	[0.092]	[0.008]	[0.008]
1 to 15 months prior to exhaustion	-16.357***	-0.046	-0.006	-0.010
	[4.680]	[0.108]	[0.010]	[0.010]
Controls	All	All	All	All
Time Unemployment	No	Yes	No	Yes
N	220849	220849	206913	206913

Note: Table B.5 presents the estimation of the impacts of two additional month UI benefits on the re-employment wage prior to the exhaustion of UI cumulatively. Columns (3) and (4) present the estimates without/with unemployment duration fixed effects as a control. For each row, indexed by τ, the table shows the cumulative impact of UI extensions on the re-employment wage from 1 to $\tau + 1$ months prior to the exhaustion of UI in the treatment group vs the control group.

Figure B.1: Evolution of re-employment wages. Treatment vs control group. By period



(a) Cumulative difference in re-employment wage for period (control time in unemployment)

Note: Figure 13 presents the impacts of two additional month UI benefits on the re-employment wage prior to the exhaustion of UI. Panel 14(a) shows the effect UI extension on wages, cumulatively, from the month prior to UI exhaustion to τ ($\tau = B - 30, B - 60, \cdots, B - 510$ days).