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PhD Date: May 2021

My research merges rigorous applied microeconomics research on labor markets and public economics with targeted policy analyses to inform policymakers. In general, my work aims to combine quasi-experimental identification strategies, rich microdata, and cutting-edge theory to tackle major questions in economics such as wage determination and inequality, capital–skill interactions, human capital specialization, the value of social insurance programs, and the effects of unemployment.

My research spans four interrelated areas in applied microeconomics. First, in the area of labor-market dynamics, I investigate the existence and degree of capital–skill complementarity in manufacturing. While the question is not new, my approach to answering it is. In "Manufacturing Capital-Skill Complementarity: Lessons from the US Shale Boom", I use fracking-induced low-skill wage shocks to manufacturers in exposed areas to show that firms substitute low-skill labor with capital when the former becomes more expensive. Combining these findings with previous theory on production functions, I demonstrate that my causal findings necessitate the existence of capital-skill complementarity in manufacturing and that manufacturing production must take a specific form that differs from that previously employed in the more macro-quantitative literature.

Second, I study the effects of human capital specialization on displaced workers. In "The Hedgehog's Curse: Knowledge Specialization and Displacement Loss" (joint with Roberto Pinheiro and Hans Holter), we propose a novel, data-driven approach to measure human capital specificity. We document significant negative effects of specialization pre-displacement on post-displacement earnings and provide two novel mechanisms through which specialization affects re-employment earnings: Longer non-employment duration after involuntary separations and occupation downgrading. To connect all our empirical results, we introduce an extension of Mortensen and Pissarides' (1994) job search model with heterogeneous worker and job types and show how the empirical predictions of the model corroborate our empirical findings.

My third and primary line of work examines the effects of unemployment on workers and the reasons why unemployment insurance (UI) alleviates these effects. With this goal in mind, "The Value of UI: Liquidity vs. Insurance" (joint with Kaixin Liu), we demonstrate how to decompose the value of UI into its liquidity and insurance components. The former captures the friction that workers experience in trying to bring income from the future to the present, while the latter represents a precautionary savings motive due to uncertainty about future labor market outcomes. Using a sufficient statistics approach and quasi-experimental evidence, we show that the majority of the value of UI is driven by its liquidity value. Furthermore, across different groups of workers, there is significant heterogeneity in the relevance of liquidity and insurance to the value of UI. Our decomposition opens the door for heterogeneous insurance policies that could increase welfare while reducing the distortions usually associated with UI. Furthermore, it also facilitates understanding whether increases in UI benefits or extensions of the potential duration of UI benefits present more value to workers.

In "Estimating Duration Dependence on Re-employment Wages When Reservation Wages Are Binding" (joint with Kaixin Liu and Richard Grice and attached at the end of this document), we study the causal effect of the non-employment duration on workers' re-employment outcomes. To achieve this goal, we employ a sufficient statistics approach to estimate the duration dependence on re-employment wages. We demonstrate that it is possible to estimate duration dependence using easily accessible administrative data, even if the worker's wage choices respond to UI, a feat that previous work had not achieved. Furthermore, we show that neglecting the responses of the worker's wage choices to UI would result in an understimation of the true duration dependence. We estimate that duration dependence is large, at around 0.8 percent per month. Furthermore, we document that a meaningful degree of duration dependence is driven by the worker's increasing difficulty in finding full-time work as the duration of non-employment increases. As such, duration dependence in hourly wages is much more limited, at just 0.25 percent per month.

My current work in progress builds upon my previous lines of research, while also expanding to new topics, such as the effect of trade on wage inequality. In the area of unemployment, in "Positive Selection During the Unemployed

Job Search: Nature of Heterogeneity and Limited Predictability" (joint with Kaixin Liu) we study how unobserved information on workers – their risk-aversion and ability –present a non-linear relation with observed workers' characteristics, and how this affects the predictability of important policy variables such as the likelihood of long-term unemployment.

In "Fighting Income Inequality with International Trade" (joint with Nic Kozeniuskas and Roman Merga), we combine creative identification with cutting-edge econometrics to show that increases in trade exposure (primarily driven by increases in imports) have positive effects on the labor earnings of low-wage workers but negative effects for higher-wage workers. In our paper, the main mechanism behind these effects is an increase in the demand for low-skill labor and employment in small firms. Furthermore, we show that imports primarily drive these effects, and that, conditional on imports, increases in exports do not affect wages at any point in the income distribution. These findings contrast with those in previous work studying the effects of trade. We are currently working on a multisector multiregion CRS Cobb-Douglas trade model that could replicate our findings.

My policy work, in the form of economic commentaries, enables me to translate the empirical findings from my research into more digestible and actionable information. It combines rigorous empirical analysis with creative approaches to explain the consequences of recent developments in labor markets or policy, conceptually linking them with similar shocks experienced in the past that can guide our thinking.

The NBER Summer Institute sessions I would be interested in attending are Labor Studies, Public Economics, and Personnel Economics.

In my view, two dimensions of my research present the most significant sources of distinct contribution to the NBER SI. First, my work combines creative empirical strategies with the goal of causal identification and cutting-edge econometrics. This has made me well-versed in various tools, methods, and the limitations of identification and data. In this sense, my presence can foster conversations about issues or opportunities related to the use of certain techniques or identification strategies that might otherwise go overlooked. Second, my work spans multiple topics in labor and public economics. One of the goals of my previous research has been to more directly connect these topics, which, while conceptually similar, might not have been formally linked. For instance, my work in UI has consistently drawn on learnings from the displacement literature, trying to reconcile the tension between both literatures regarding the (implicit or explicit) persistence of unemployment effects. In this sense, my presence can help participants consider their work from different angles and reflect on their findings, methods, and assumptions based on learnings from alternative literatures, something that might otherwise go overlooked.

In addition to the ways my research can contribute to the NBER SI, I believe my life experiences are also relevant to it. As a microeconomist at a mostly macro institution (the Federal Reserve), one is exposed to a wide variety of topics, data, and policy information. This brings up, in my view, two advantages from a research perspective. First, it widens the set of information available when creating new research. It forces you to reconcile your choices and findings with their implications for other topics that, without this exposure, one might not have considered. Furthermore, it helps create bridges between ideas and results in multiple areas, helping to make the contributions of the work broader and more relevant. Second, the policy orientation of my institution typically translates into research with one of the objectives being the usability of its findings or tools. I believe this gives me a different perspective when considering others' research, and allows me to contribute to other authors' work by providing insights on how to approach questions and offer answers and tools that can be of policy relevance.

Attending the NBER SI would be extremely beneficial to my career for many reasons, but three are of special relevance. First, the NBER SI provides a fantastic avenue to publicise and discuss research with the most renowned economists in the profession. This allows for the exposure of one's work and the highest-quality feedback on how to approach a new project, which projects to prioritize or not, and how to elevate existing projects to a higher level of quality. Furthermore, because everyone is involved in similar ideas, finding specific feedback about technical questions or details becomes significantly easier than at one's institution, where the number of researchers working on the exact topics tends to be very limited.

Second, the NBER SI is at the frontier of research. Attending the session and speaking with the different speakers allows for a much better understanding of the questions and advancements relevant to one's field. This is not something that can be easily done outside of these circles. The NBER SI, in this sense, provides the perfect avenue to understand which topics or ideas to pursue and their relevance for the field.

Finally, as someone trained in a non-top-20 institution and working in a federal institution, my ability to interact with the type of participants attending the NBER SI on an ongoing basis is limited. By attending the NBER SI, I

would not only significantly boost the exposure of my research, but I would also have the opportunity to introduce new ideas and find collaborators interested in these topics. It would allow me to directly explain how I contribute to the field and to find researchers interested in making similar or complementary contributions.

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Current Position

- Research Economist, *Federal Reserve Bank of Cleveland* 2021-Present

Research Fields

- Labor Economics, Public Economics, Inequality, Applied Microeconomics.

Education

- Ph.D. Economics, *University of Rochester* 2016-2021
- M.A. Economics, *University of Rochester* 2016-2018
- M.Sc. Economics & Public Policy, *Barcelona Graduate School of Economics* 2014-2015
- B.Sc. Economics, *University Carlos III* 2010-2014

Publications

- "The Value of Unemployment Insurance: Liquidity vs. Insurance Value" with Kaixin Liu, ***Labour Economics*** *(2025) 93: 102691, EEA Young Economist Award 2021*
- "Social distancing merely stabilized COVID-19 in the US" (with Aaron B. Wagner, Elaine L. Hill, Sean E. Ryan, Ziteng Sun, Grace Deng, Sourbh Bhadane, Peter Wu, Dongmei Li, Ajay Anand, Jayadev Acharya, David S. Matteson), ***STAT*** *(2020) 9(1) e302*

Other Publications

- "Can the IRA and CHIPS Act Reduce Labor Earnings Inequality? Lessons from the US Shale Boom" with Geena Panzitta, ***Economic Commentary, Federal Reserve Bank of Cleveland*** (2024)
- "The Effect of Higher Financing Costs on Job Openings and Online Job Postings" with Adiah Bailey, ***Economic Commentary, Federal Reserve Bank of Cleveland*** (2023)

Working Papers

- "The Hedgehog's Curse: Knowledge Specificity and Displacement Loss" with Hans Holter and Roberto Pinheiro, *Review and Resubmit to* ***Labour Economics***
- "Manufacturing Capital-Skill Complementarity: Lessons from the US Shale Boom", *Reviewed and Resubmitted to the* ***European Economic Review***
- "Estimating Duration Dependence on Re-employment Wages When Reservation Wages Are Binding" with Kaixin Liu

Selected Work In Progress

- "Fighting Income Inequality with International Trade"
  with Nicholas Kozeniauskas, and Roman Merga
- "Selection in the Duration of Unemployment: Heterogeneity and Limited Predictability"
  with Kaixin Liu

Conference & Seminar Presentations

- 2025: ASSA, System Heterogeneity Conference, EALE SOLE AASLE World Conference.
- 2024: Southern Economic Association, European Commission Annual Research Conference, System Regional, MEA-SOLE Sessions, Society of Labor Economics, System Applied Micro.
- 2023: Midwest Economics Association, SOLE Sessions (MEA), Midwest International Trade Conference, Society of Labor Economics, System Applied Micro, European Association of Labor Economics.
- 2022: Southern Economic Association, Kent State University, Southeastern Micro Labor Workshop, System Applied Micro, European Economic Association, Ohio State University, FAM Seminar.
- 2021: Cleveland FRB, Bank of Portugal, University of Stockholm SOFI, Smith College, Econometric Society North American Summer Meeting, European Economic Association, European Association of Labor Economics.
- 2020: EALE SOLE AASLE World Conference.

Research Grants

- River Campus Data Grant, *University of Rochester* 2020.

Teaching Experience

Instructor

- Econometrics (Undergraduate ECO231W), *University of Rochester*, Summer 2020
  Evaluations: Teaching Skills: 4.7/5 Overall Teacher Rating 4.5/5

Teaching Assistant

- Econometrics (Undergraduate ECO231W), *University of Rochester*, Fall 2020, Fall 2019, Fall 2018. Instructor: Prof. Ronni Pavan.
- Econometrics (Undergraduate ECO231W), *University of Rochester*, Spring 2020, Spring 2019. Instructor: Prof. Kegon Tan.
- Research in Applied Econometrics (Graduate, ECO 487), *University of Rochester*, Spring 2019. Instructor: Prof. Ronni Pavan.

Honors and Awards

- Young Economist Award, *European Economics Association* 2021
- Tapan Mitra Prize: Best 5^{th} Year Paper, Empirical Economics, *University of Rochester* 2020
- Economics Department Ph.D. Fellowship and Scholarship, *University of Rochester* 2016-2021
- Tuition Scholarship, *Barcelona Graduate School of Economics* 2014-2015

Previous Employment

- Trainee in Monetary Policy Strategy, *European Central Bank* 2016
- Research Associate in Economics, *INSEAD* 2015-2016

Others

- Computer Skills: Stata, Matlab, Julia, Phyton, R, LaTeX
- Languages: English (Fluent), Spanish (Native), French (Basic)

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

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March 2024

Abstract

We show that unemployed workers' wage choices respond to the level of unemployment insurance, indicating that the reservation wages of unemployed job-seekers bind. That is, we use comprehensive Spanish social security data to show that the exhaustion of unemployment benefits causes an immediate 3 percent drop in job-seekers' re-employment wages. We then derive an estimator of "duration dependence" – the causal effect of unemployment duration on re-employment wages – that accounts for binding reservation wages. Our estimator generalizes and nests the IV estimator of Schmieder et al. (2016). Applied to the Spanish data, our estimator finds that an extra month of unemployment reduces re-employment wages by 0.75 percent in daily wages and 0.23 percent in hourly wages. Finally, we calculate we would underestimate duration dependence by 17 percent if we presumed reservations wages do not bind.

Keywords: Unemployment, Duration Dependence, Re-employment Wages
JEL Codes: J65, J64.

We wish to thank Travis Baseler, Mark Bils, Gaston Chaumont, Bruce Fallick, Elaine Hill, Narayana Kocherlakota, Pawel Krolikowski, John Singleton, and Nesse Yildiz for their inspiring suggestions for this paper. We are especially thankful to our advisors, Lisa Kahn, Ronni Pavan, and Kegon Tan, for their patience and fantastic guidance during this project. The views in this paper are those of the authors and do not necessarily reflect the views of the Federal Reserve System, the Federal Reserve Bank of Cleveland, or the Board of Governors.

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1 Introduction

A spell of unemployment decreases the wages a worker earns upon re-employment compared to their pre-separation job. These wage losses are large and persist well into re-employment. Further, recent research shows they are greater the longer the worker spends out of employment (Fallick et al. (2021) and Schmieder and Heining (2021)). However, to understand their consequences for the long-term unemployed and design unemployment insurance (UI) policies to address them, we need first to understand whether greater wage losses following longer spells of non-employment are a *causal* effect of non-employment duration. Such a causal effect is termed the "duration dependence" of re-employment wages, and needs to be distinguished from the simultaneous effects of dynamic selection, which is the propensity for more employable workers to be re-employed both earlier and into better-paying jobs.[1]

A leading approach to distinguishing these two effects exploits widely available administrative data on workers' wages and UI benefits (see Schmieder and Heining (2021)) and discontinuities common to many UI policies. Schmieder et al. (2016) show how quasi-experimental extensions in UI benefits can be used as instrumental variables (IVs) for estimating the effect of unemployment duration on re-employment wages net of dynamic selection effects. The validity of these IVs rests on benefit extensions only affecting how long workers remain unemployed, not the wages they are willing to accept given their non-employment duration. Yet numerous studies show benefit extensions result in longer spells of unemployment but have no effect – or a positive effect – on wages.[2] That is, if UI benefit extensions only affect the length of unemployment spells, numerous studies imply duration dependence is non-negative, which is theoretically implausible and conflicts with existing estimates. Therefore, we ask whether re-employment wages are directly affected by changes in UI benefits – an effect reflecting binding reservation wages – and, if so, how the IV approach to estimating duration dependence can be adapted to retain its validity.[3]

[1] Duration dependence refers to the aggregate effect of several different mechanisms that can erode workers' labor market opportunities during periods of non-employment. These mechanisms include human capital depreciation, an adverse signaling effect, and declining matching efficiency.

[2] Studies finding no effect of increasing UI benefits on re-employment wages include Card et al. (2007b), Lalive (2007), Le Barbanchon (2016), and van Ours and Vodopivec (2008), while Nekoei and Weber (2017) and Farooq et al. (2022) find a positive effect.

[3] Binding reservation wages are the mechanism by which the worker's wage choices respond to the decreasing

In answer to these questions, we present new, causal evidence showing the wage choices of unemployed workers respond to the value of their UI benefits – indicating reservation wages do bind – and then characterize a new estimator for the causal effect of the duration of non-employment on wages, net of the effect of changes in UI benefits. Using Spanish administrative data – and taking advantage of a feature of the Spanish UI system granting different potential durations of UI to otherwise identical workers based on whether their previous working experience crosses certain thresholds – we estimate the effect of duration dependence as a 0.75 percent loss per month in daily wages. Ignoring the response of the workers' wage choice to changes in UI benefits would underestimate the magnitude of the duration dependence effect by 17 percent.

To show that workers' wage choices respond to the value of their outside options, we first show that re-employment wages causally decline at the point of exhaustion of UI benefits and then theoretically connect this result to the response of the worker's wage choices to an extension of the UI duration. Using quasi-experimental variation, we document that wage losses for workers with exogenously shorter and longer potential durations of UI evolve identically throughout time to re-employment except at one point: when the former group has just exhausted their UI benefits while the latter can still collect them. Around this point, the wage losses of workers with shorter potential duration increase by roughly 3 percentage points relative to the losses of workers with longer potential duration. We provide evidence that this result is not driven by dynamic selection or selection in observed or permanent unobserved characteristics.[4] We interpret this result as the causal impact of the exhaustion of UI benefits on re-employment wage losses. Finally, we show that the decline in wages at the point of exhaustion of UI benefits is equivalent to the response of wage choices to an extension of the potential duration of UI. The key assumption underpinning this equivalence is that duration dependence evolves smoothly around the point of exhaustion of UI benefits.

To characterize an estimator of duration dependence, we extend the random search framework in Schmieder et al. (2016) to a more general directed search framework in which

value of the outside option as UI benefits expire (Schmieder et al. (2016)). A non-binding reservation wage would imply the elasticity of re-employment wages with respect to the value of the outside option is zero.

[4]Our empirical strategy is robust to dynamic selection as long as this selection only depends on time to re-employment and not on the potential duration of UI.

wage choices are elastic to the value of unemployment – that is, a framework in which reservation wages can bind. We refer to this estimator of duration dependence as the LMOS (i.e., "Labor Market Opportunity losS") estimator. Our LMOS estimator nests the IV estimator of Schmieder et al. (2016) in the sense that when reservation wages do not bind, the two estimators are identical. However, when wage choices respond to the exhaustion of UI benefits, as occurs in our case, the IV estimator conflates the causal effect of non-employment duration on wage losses with the effects on wages of a changed probability of exhausting the UI benefits when UI changes. This is not the case for the LMOS estimator, which, regardless of the worker's response to the value of the outside option, only captures the causal effect of the duration of non-employment, net of any UI exhaustion effects.[5]

Our LMOS estimate of duration dependence indicates that each additional month of non-employment deepens daily wage losses by approximately 0.75 percent, significantly different from zero at the 90 percent confidence level. However, an important part of this deterioration appears to be driven by workers being unable to find full-time jobs as the non-employment duration increases. Estimating duration dependence in hourly wages results in an insignificant LMOS estimate of 0.23 percent per month of non-employment.

One limitation of previous work has been the inability to account for dynamic selection in unobserved permanent characteristics when determining the degree of duration dependence. This makes it difficult to assess whether the differences (or lack thereof) in wage losses between treatment and control groups at each point of the distribution of time to re-employment represent equal behavior or if they are the result of selection. We overcome this limitation by using a sample of workers with two or more UI claims to re-estimate duration dependence controlling for selection based on unobserved permanent heterogeneity. Our LMOS estimates of duration dependence after controlling for selection in permanent unobserved heterogeneity are insignificantly different from our main estimates.[6]

Our paper contributes to four strands of the literature. First, it adds to the empirical

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

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

literature studying unemployment dynamics and the value of non-employment. We document that re-employment wages respond to the value of non-employment, declining by approximately 3 percent when UI benefits end. While previous work has documented a spike in the hazard rate (Card et al. (2007a)) and a drop in consumption (Ganong and Noel (2019)) when UI benefits end, there is significantly less evidence on the response of re-employment wages. Nekoei and Weber (2017) document that wage changes decline approximately 5 percent, not just at the expiration of UI benefits but starting around 1 month earlier. Compared to their work, we can better rule out dynamic selection as an explanation for this decline, due to our results controlling for worker's permanent unobserved heterogeneity.[7]

A handful of papers have studied the evolution of reservation or target wages around the expiration of UI benefits. Krueger and Mueller (2016) estimate that within-worker reservation wages decline by 2 percent after the exhaustion of UI. However, their small sample size and the exceptionally large UI potential duration in NJ at the time make their estimate insignificant and difficult to interpret. In more recent work, Marinescu and Skandalis (2021) show that the within-worker target wage of French workers declines by 2.4 to 3.5 percent in response to the expiration of benefits. Compared to their work, our exogenous variation in potential duration allows us to relax the assumptions required to identify these effects.[8]

Whether re-employment wages (or reservation wages) respond to the value of non-employment remains an open empirical question. In that regard, our results contrast with some of the findings in the previous literature (Lalive et al. (2015), Schmieder et al. (2016), and Jäger et al. (2020)) that show that re-employment wages (or reservation wages) respond little (if at all) to the value of non-employment.

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

[7] Mario Centeno and Novo (2011) also show that re-employment wages appear to only respond to an extension of the potential duration of UI around the original point of exhaustion of benefits.

[8] Marinescu and Skandalis's (2021) main identification assumption requires the evolution of search effort and target wages during the unemployment spell to be uncorrelated with the worker's potential duration or UI eligibility.

the exhaustion of UI benefits on wages is essential to identifying the degree of duration dependence. While a similar theoretical discussion is present in Nekoei and Weber (2017), their work does not provide a methodology to estimate duration dependence, something we do and implement. In a setting where the worker's wage choices do not respond to the value of the outside option, Schmieder et al. (2016) estimate duration dependence in daily wages at 1 percent per month of non-employment for Germany. As in Schmieder et al. (2016), our identification strategy comes from analyzing the causal impact of UI extensions on non-employment durations and on re-employment wages. However, we emphasize the importance of separating the effects on re-employment wages over time that come from changes in UI benefits versus those arising purely through duration dependence. The former depends on the UI policy, while we see the latter as a primitive of the evolution of the workers' labor market opportunities over time.

Marinescu and Skandalis (2021) estimate duration dependence in monthly target wages at just 1.5 percent per year for France, primarily driven by hourly wages. Their sample is comprised of low-wage workers, which could see lower duration dependence because of the existence of minimum wages. Compared to their work, our sample is more representative of the average unemployed worker, and our identification of duration dependence requires less restrictive assumptions. Furthermore, Marinescu and Skandalis (2021) estimate duration dependence on target wages, using the posted wages of the jobs the workers applied to as their measure target wages. The mapping from the average target wage in the re-employment period to the re-employment wage could change over the duration of non-employment, making the comparison between their estimates of duration dependence on target wages and estimates of duration dependence in re-employment challenging.

Third, we add to the literature studying the role of dynamic selection in explaining changes in the hazard rate or wage losses over the non-employment spell. A large literature makes assumptions about the importance of unobservables based on that of observables (Krueger et al. (2014), Schmieder et al. (2016), and DellaVigna et al. (2017)), generally concluding that dynamic selection is unimportant. On the other hand, Ahn and Hamilton (2020) and Alvarez et al. (2016) follow a different approach and conclude that the role of

dynamic selection in explaining the reduction of the hazard rate over time is significant. Our results indicate that duration dependence remains economically important in explaining wage losses over the non-employment duration even after accounting for dynamic selection based on permanent unobserved heterogeneity.

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

Additionally, our empirical evidence suggests that workers' wage choices do not respond in anticipation of the exhaustion of UI benefits. This finding is difficult to replicate using standard parameter values in a standard or reference-dependent job search model. On the other hand, this finding is more consistent with variations of these models in which workers show an important degree of impatience (Ganong and Noel (2019)), or are over-optimistic regarding their probability of finding a job (Mueller et al. (2021)).

The rest of the paper is organized as follows. Section 2 describes the institutional design of the Spanish UI system and the Spanish Social Security data we use. Section 3 exploits quasi-experimental variation to estimate the causal impacts of extending the potential duration of UI benefits and exhausting benefits. Section 4 outlines an illustrative model, introducing the channels underlying the decline in re-employment wages during the course of non-employment, and establishes the connection between these channels and the causal effects estimated in Section 3. Section 5 presents our LMOS estimates of duration

dependence and discusses their implications. Section 6 concludes.

2 Data and Institutional Features

2.1 The Unemployment Insurance System in Spain

Workers in Spain are eligible for unemployment insurance (UI) benefits if they lose their job involuntarily and have worked at least 360 days in the previous six years.[9] Their UI benefits are characterized by two variables: the level of benefits and the potential duration for which these benefits may be collected.

The potential duration of benefits is determined by the number of days worked in the previous six years, regardless of whether the jobs worked were full- or part-time. It increases in a series of discrete steps from a minimum potential duration of 4 months up to 24 months for 2,160 or more days worked. This mapping from the number of days worked to the potential duration of benefits is depicted in Figure 1.[10]

The level of benefits is determined as a replacement rate of the worker's previous wage and is paid monthly until the worker either a) finds a new job or b) exhausts the potential duration of her benefits. The replacement rate is 70 percent for the first 6 months that the worker is collecting benefits, after which it decreases to 50 percent. This 50 percent rate was lowered from 60 percent in October 2012. Benefit levels are subject to minimum and maximum amounts, which vary across years and depend on the worker's number of children.

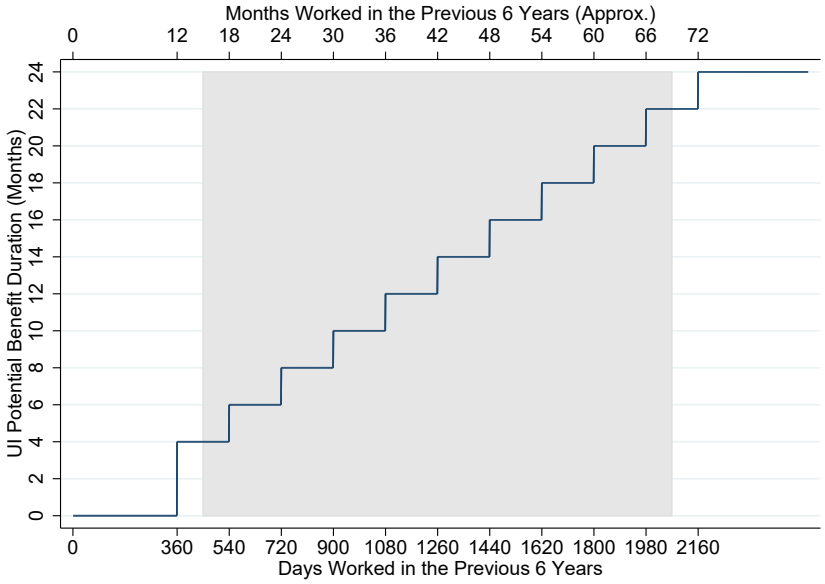
Finally, the Spanish UI system grants workers entering a spell of unemployment the *right to choose* between starting a new potential duration - benefit level bundle or continuing an existing, unfinished bundle.[11] We avoid the complications created by this choice by restricting

[9] Workers who quit their jobs or are fired for misconduct (but not for economic or other reasons) are not eligible for UI in Spain.

[10] If individuals exhaust their UI benefits and are still unemployed, they can apply for unemployment assistance (UA). UA is a means tested program with a set of very stringent eligibility rules. Workers who are eligible for UA can claim 430 euros per month (in 2016) – which is approximately 50 percent of the minimum wage – for an additional 6-20 months. For more details on UA, see Domènech-Arumí and Vannutelli (2023).

[11] If a worker entering unemployment was unemployed and made a UI claim in the preceding 6 years, she can choose between the benefit level and potential duration generated since she last left unemployment, or – if she did not exhaust the potential duration of her benefits during her previous unemployment spell – she can choose to use the remaining amount of her previous claim.

Figure 1: Unemployment Insurance in Spain: Potential Duration



Note: The mapping in the Figures shows the potential duration of UI benefits (horizontal axis) given the number of days worked in the previous 6 years (bottom axis). For the reader's convenience, on the top axis, we show the approximate number of months corresponding to the number of days worked. However, the potential duration of benefits is determined by the exact number of days worked, not months. The area shaded in grey corresponds to the part of the sample used in the analysis, as described in Section 2.2.

our sample to unemployment spells based on new work histories and discarding carryovers.[12]

2.2 Data Description and Data-Cleaning Process

We use the *Muestra Continua de Vidas Laborales* dataset (MCVL) for the years 2006 to 2017. The MCVL is created by randomly selecting 4 percent of the individuals each year who have a relationship with the Social Security Administration (i.e., employed and unemployed workers, retired individuals, and recipients of other subsidies). For each sample year, the MCVL records the individual's lifetime history of daily Social Security affiliations (i.e., work spells, unemployment spells, self-employment, and other subsidy spells) and her lifetime record of monthly wages per employer.[13] We combine these daily labor histories and monthly

[12]We retain unemployment spells for workers who could have used a carryover UI claim but instead chose a new claim. We discard carryovers because the mapping determining the potential duration of carried-over benefits as a function of days worked in the previous six years will have potentially endogenous discontinuity points that are different from those in Figure 1. We report the effect of discarding carryovers on the composition of our final sample in Appendix B.

[13]Wages recorded in the MCVL are right-censored at a maximum, which varies across years. This right-censoring does not create a problem for our purposes because less than 1 percent of the unemployment spells in our sample have a wage at the maximum.

compensation records to create precise measures of workers' tenure and daily compensation by job, even for workers who change jobs (and contracts) within the same firm. The MCVL also records demographic information at the individual and the household level. This allows us to observe workers' age, household composition, location, migration status, and education level.

From the MCVL, we construct a sample of displacements (i.e., UI claims preceded by a spell of employment) occurring between 1994 and 2016 for workers aged between 25 and 50 years. After discarding a small number of spells in which the UI records are inconsistent with the calculated previous tenure, our initial sample contains 677,000 unemployment spells. From there, we discard all carryovers (366,000 spells) and impose three major restrictions on our sample. We briefly summarize these restrictions here and provide additional details – including the compositions of the initial and final samples – in Appendix B.

First, we discard 112,000 spells associated with workers who in the previous 6 years have worked less than 450 days or more than 2,070 days. We impose this restriction to remove the workers around the first and last discontinuities in the potential duration of benefits. We remove the spells close to the first discontinuity because there were several changes during our sample period to the rules governing subsidies for those workers who worked less than 360 days and thus did not qualify for UI benefits. We discard the spells around the last discontinuity because workers end up mechanically bunched to its right due to the six-year window used to determine the potential duration of benefits. That is, there are workers just to the right of the last discontinuity whose last employment spells lasted both just longer and much longer than 6 years.

Second, we discard 44,000 spells of workers entering unemployment after exhausting the predetermined length of certain temporary contracts. These workers know when their contracts will expire and likely start searching for new jobs before this date. Further, the length of these temporary contracts is often exactly half-year, one year, etc. While this pattern is not a problem in and of itself, when combined with the UI schedule in Spain, it results in the bunching of these workers just to the right of the potential duration discontinuities. Keeping these workers in the sample creates artificial manipulation in our running variable

(i.e., number of days worked in the past six years), while discarding these workers results in a smoother running variable distribution. This result can be seen by comparing Figures A.1 (b) and A.2 (b).

Third, we restrict the sample to spells in which the worker worked 35 or more hours per week on average during the period used to determine the potential duration of her UI benefits. We impose this restriction because 40 percent of the unemployment spells of part-time workers in our sample lack information on previous daily wages, compared to just 1 percent for workers working 35 hours or more. This restriction discards 23,000 displacements. We find the paper's conclusions are not sensitive to this restriction, as shown in Appendix C.

Our final sample contains over 132,000 unemployment spells, corresponding to more than 106,000 workers. Tables B.1 and B.2 in Appendix B summarize the composition of our sample and how it is affected by each of the three restrictions. The majority of workers with enough prior experience to generate a new UI claim when entering unemployment do so. The changes in the composition of the sample from our restrictions on prior experience are partially offset by the restrictions related to workers exhausting temporary contracts and part-time workers. Our final sample differs from a representative sample of all workers collecting UI primarily because of the removal of carryover claims. An important part of all workers collecting UI are forced to use carryover claims after a short period of re-employment because they do not have enough previous experience to generate a new UI claim. These workers usually have shorter potential durations, collect UI benefits for a shorter period of time, and find a new job significantly faster. We therefore see our sample as representative of workers with a relatively strong degree of attachment to the labor market entering unemployment after a moderately long spell of employment. We report summary statistics for the main variables in our final sample in Table A.1, both for the entire sample and for each potential duration subgroup.

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

In this section, we estimate the causal impact of extending the potential duration of UI on both the time to re-employment and the wage change between pre-displacement and re-employment jobs. From there, we concentrate on understanding the effect of a potential duration extension on wage losses across the distribution of time to re-employment, where we primarily focus on the effects around the exhaustion of UI benefits.

Our data allow us to estimate these causal impacts for our entire sample and for a sub-sample of workers with two or more UI claims. This latter sub-sample allows us to estimate the causal impacts controlling for permanent unobserved heterogeneity, the results of which we discuss in Section 3.3. We regard these results as complementary and not superior to the estimates from our complete sample for two reasons. First, the size of our complete sample allows us to provide robustness results that are not feasible to estimate precisely in the subsample with two or more UI claims. This is of special relevance for the analysis of the wage losses across the distribution of time to re-employment, a critical input for our estimates of duration dependence. Second, the sample of workers with two or more UI claims does not need to be representative of the complete sample of unemployed workers. Workers with two or more UI claims differ across certain dimensions compared to our complete sample. Our primary goal is to provide a comprehensive understanding of the effects of UI and the duration of non-employment on the unemployed, which is better captured by our main results using the complete sample.[14]

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

Identification of the causal effect of an extension of the potential duration of UI on the wage losses and on the time to re-employment is provided by discontinuous increases in the potential duration schedule. These extensions of the potential duration of UI take place when the workers' past work experience during the previous 6 years crosses one of the threshold cutoffs. Figure 1, shown above, presents the detailed schedule and shows the 11 different

[14] Using the results controlling for individual fixed effects to assess the relevance of permanent unobserved heterogeneity in the complete sample will require additional assumptions, as discussed in Section 3.3.

cutoffs. These threshold cutoffs are multiples of 180 days, ranging from the first cutoff at 360 days to the last cutoff at 2160 days (as discussed above, we ignore the first and last cut-offs in our analysis). When the worker's prior work experience exceeds the cutoff value, the worker's potential duration of UI increases by two months. This creates quasi-random variation in the potential durations of UI for similar workers based on whether they crossed a threshold, forming the basis of our regression discontinuity (RD) design below.[15]

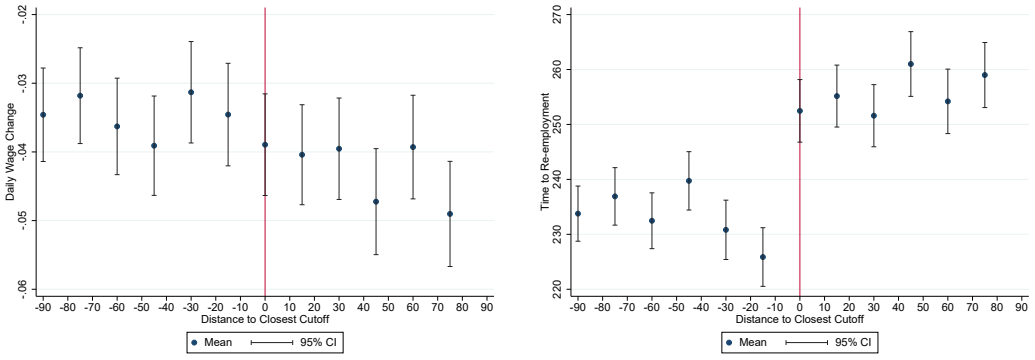
We focus on two outcomes of interest. The first one is the time to re-employment. We define it as the length (in days) from the day the worker loses the job to the day the worker starts a new job. The second one is the wage change (i.e., wage losses). We define it as the difference in log daily wages between pre-displacement and re-employment jobs. We define the wage in the re-employment job as the average daily wage during the first month of the first re-employment job.[16] Occasionally, we use longer-run measures of wage changes. We specify the chosen measure in the main text when that is the case.

We start by combining all our discontinuities in one single specification, showing in Figure 2 the difference a two-month extension of the potential duration of UI makes for the wage losses and the time to re-employment. Panel (a) shows the average wage loss for individuals along the distribution of the distance to the closest potential duration cutoff. Individuals to the left of zero have very similar prior working experience compared to those to the right but have two fewer months of potential duration of UI. The figure shows no clear difference in wage losses for individuals close to but on different sides of the discontinuities. Panel (b) shows that the average time to re-employment for workers close to the cutoffs and entitled to two extra months of potential duration of UI is 25 to 30 days longer compared to those who do not cross the discontinuity.

[15]The discontinuity between 719 and 720 and days of past work experience not only extends the potential duration of UI by two months but also lowers the replacement rate for those two additional months by 10 or 20 percentage points. We ignore this fact for two reasons. First, Rebollo-Sanz and Rodríguez-Planas (2020) show in the same data that a 10 percentage point decline in the replacement rate after 6 months of collecting UI did not affect re-employment wages. Second, we find that an exogenous 10 percentage point decline in the replacement rate after 6 months of collecting UI benefits did not affect the time to re-employment or the re-employment wages of workers with 8 months of potential duration of UI (see Section E.3 of the Appendix).

[16]When the first re-employment job lasts less than a month, we calculate the average daily wage over the days worked in the first re-employment job. Because the raw data only contains the total monthly earnings for each job, to calculate daily wages when the job lasts for less than one month, we divide the job's total monthly earnings by the number of days worked in the job during that month.

Figure 2: The Evolution of Time to Re-employment and Daily Wage Changes after a Two-Month Increase in Potential Duration: All Cutoffs Combined



(a) Wage Losses

(b) Time to Re-employment

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

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

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

where $\hat{\mu}_{+,p}(h_n)$ and $\hat{\mu}_{-,p}(h_n)$ denote the intercept (at the discontinuity point) of a weighted local p^{th}-order polynomial regression for only treated and only control units, respectively (see Calonico et al. (2014) for further detail). Our main specifications use a first-degree polynomial with a triangular kernel and the maximum possible bandwidth (90 days) that locates workers exclusively on one of the sides of the discontinuities.[17]

We also estimate the results using the optimal bandwidth in Calonico et al. (2020)

[17] A bandwidth longer than 90 days would place a worker simultaneously to the right and to the left of the discontinuity. For instance, a worker with 640 days of working experience would be simultaneously located 100 days to the right of the 539-540 days discontinuity and 80 days to the left of the 719-720 days discontinuity.

and find them to be extremely similar.[18] However, throughout the rest of the paper, we maintain a bandwidth of 90 days for all our estimates unless specifically stated. We do this for four main reasons. First, the optimal RD bandwidth in separate estimations for each individual discontinuity ranges from 50 to 90 days, depending on the outcome and discontinuity. When estimating the effects of crossing the cut-off for each discontinuity separately, we do not face the limitation of simultaneously classifying workers as part of the control and the treatment group. This increases the support of the estimator to 180 days on each side of the discontinuity, resulting in an optimal bandwidth of up to 90 days. Second, while we eliminate an important number of unemployment spells in which workers exhaust contracts of pre-determined duration, some might remain. These unemployment spells tend to be located to the right of the discontinuities (close to them but not exactly after the cut-off) and could create some sorting. Increasing the bandwidth allows us to reduce their potential impact on our estimates. Third, a linear polynomial combined with a bandwidth of 90 days adequately captures the evolution of our outcomes of interest on each side of the cut-off, showing very little difference compared to more flexible polynomials (see Figure A.3). Fourth, the larger bandwidth uses the full extent of our sample, increasing the statistical precision of the results while still comparing very similar workers who show no evidence of manipulation or covariate imbalance across the discontinuities.

Table 1 presents the results from estimating the RD model of equation (1) where we combine all discontinuities into one single specification. Columns (1) and (2) show the estimated impact of a two-month extension in the potential duration of UI on the time to re-employment. Column (1) includes only discontinuity fixed effects as a control variable, while column (2) additionally controls for age, gender, education, wealth, previous tenure, previous experience, type of contract, previous wage, part-time coefficient, and month-year fixed effects (Controls:"All"). A two-month extension of the potential duration of UI increases the time to re-employment by 28 days, significantly different from zero at the 1 percent confidence level. The inclusion of controls in column (2) changes little the point estimator, suggesting that differences in observed characteristics play a very limited role in generating

[18] See Table A.2.

the observed effects. These estimates translate into a marginal effect of 0.46, meaning the time to re-employment increases by 2 weeks for each additional month of potential duration extension. The estimated duration elasticity is 0.61.

Table 1: Effect of a Two-Month Extension of the Potential Duration of UI. All Discontinuities

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	27.6***	27.9***	-0.006	-0.006	-0.003	-0.002
	[3.5]	[3.4]	[0.005]	[0.004]	[0.004]	[0.003]
Controls	Disc	All	Disc	All	Disc	All
N	132129	130118	122387	122020	122387	122020

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

Our results are slightly larger than those in previous work, although they remain well within reasonable values. After removing outliers, Schmieder and von Wachter's (2016) review of the previous literature finds a mean marginal effect of 0.26 among previous studies, with a range between 0.05 to 0.65. In more recent work, Nekoei and Weber (2017) estimate a marginal effect of just 0.03 for Austria, while Huang and Yang (2021) report a marginal effect of 0.41 for Taiwan. Part of the difference across estimates arises driven by differences in potential duration and non-employment durations across countries. Focusing on the duration elasticity, our estimate is above the median duration elasticity reported in Schmieder and von Wachter (2016) (0.40), but well within their reported range of estimates excluding outliers (from 0.1 to 1). Furthermore, van Ours and Vodopivec (2008), Mário Centeno and Novo (2009), and Le Barbanchon (2016) consider three countries (Slovakia, Portugal, and France) whose labor markets more closely resemble the Spanish one.[19] Their reported elasticities range from 0.4 to 0.72, with a median of 0.53.

Columns (3) and (4) show the estimated impact of a two-month extension in the potential duration of UI on the change in wage between the pre-displacement and re-employment jobs (i.e., wage losses).[20] The results suggest that a two-month extension of the potential duration

[19]Bertheau et al. (2023) shows that the earning losses following a displacement are significantly larger in Spain and Portugal than those in Nordic countries or Austria. An important part of this effect arises as a consequence of longer periods of non-employment in the former countries.

[20]Table A.3 examines the effects of a two-month extension of the potential duration of UI on longer-term

of UI has no significant impact on wage losses, even if the point estimates are slightly negative (-0.6 percent). This is true for the specification that only includes discontinuity fixed effects in column (3) and for that including all controls in column (4). In columns (5) and (6), we use an alternative measure of the wage change that, instead of daily wages, uses hourly wages.[21] Our estimates are negative but very close to zero (-0.3 to -0.2 percent) and insignificant, suggesting no effect of an extension of the potential duration of UI on hourly wage losses.[22]

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

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

measures of daily re-employment wages. Specifically, we examine the effect on the change in wage when the re-employment wage is measured as a) the average re-employment wage during the first year after unemployment and b) the average re-employment wage during the first five years after unemployment. In general, we find similar conclusions.

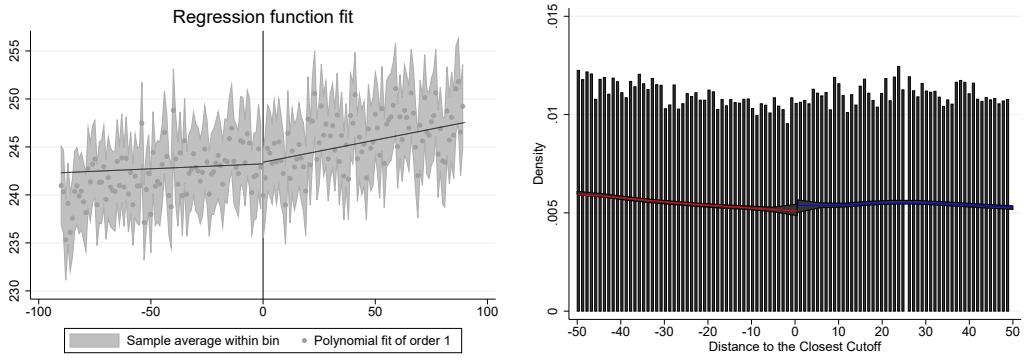
[21] We define it as the difference in log hourly wages between the pre-displacement and re-employment jobs. We define the hourly wage in the re-employment job as the average hourly wage during the first month of the first re-employment job.

[22] Table A.4 shows the RD estimates for each discontinuity separately. In general, we find that the estimated effects are similar across all discontinuities.

controlling for unobserved permanent individual heterogeneity.

First, we test the balance of observed covariates at the discontinuity. The results are shown in column (2) of Table A.5.[23] Our results indicate that workers in the margin of the discontinuity cutoffs have very similar characteristics regardless of whether they cross the cutoffs or not. We do not find any significant differences in any of the 10 different covariates tested. We summarize the results from the balance tests in Figure 3 (a). We follow Landais and Spinnewijn (2021) and use the RD design above to test the evolution of a covariate index around the policy discontinuities.[24] As shown in Figure 3 (a), we do not find any significant difference in the covariate index across the discontinuity.[25]

Figure 3: Validity Tests



(a) Covariate Index Balance Test

(b) Manipulation Test

Note: Figure 3 shows two different validity tests. Panel (a) shows the balance test of the covariate index. The index is a linear combination of a vector of characteristics correlating with the duration of unemployment, including wealth, previous wage (hourly and daily), experience, tenure, gender, education (high school and college), age, part-time coefficient, and type of contract. The estimate shows insignificant differences in the covariate index at the discontinuity (Estimate = 0.173, SE = 0.707). Panel (b) shows the manipulation test. It presents the distribution around the closest policy discontinuity for all thresholds combined and adds the point estimates and confidence intervals of the manipulation test from Cattaneo et al. (2018). The bias-corrected robust estimate does not reject no manipulation (t = 0.86, p-value = 0.39).

Second, we test for manipulation in the running variable around the thresholds. If

[23]For additional robustness, column (1) of Table A.5 shows the balance test for the MSE optimal bandwidth. Additionally, Figure A.4 shows the RD plots for each of the covariates around the discontinuity.

[24]The index is a linear combination of a vector of characteristics correlating with time to re-employment, including wealth, previous wage (hourly and daily), experience, tenure, gender, education (high school and college), age, part-time coefficient, and type of contract. The coefficients are obtained from a regression of the time to re-employment on these covariates, where the first five enter the regression linearly and the remaining non-parametrically.

[25]The exercise in Figure 3 (a) uses a bandwidth of 90 days. We additionally test the covariate index balance using the MSE optimal bandwidth (in the last row of column (1) in Table A.5) and find insignificant differences at the discontinuity. For additional robustness, in Figure A.5, we test the covariate index balance separately for each discontinuity. We find insignificant differences for all discontinuities.

workers can manipulate their prior working experience so that they receive two additional months of potential duration of UI, our estimates could be biased. We follow the work of Cattaneo et al. (2018) to test for manipulation. Figure 3(b) shows the density (relative to the closest discontinuity) overlapped by the point estimators and confidence intervals on both sides of the discontinuity. The manipulation test with optimal bandwidth shows a t-statistic of 0.86 (p-value=0.39) for the robust estimate. Therefore, we cannot reject that there is no manipulation.[26]

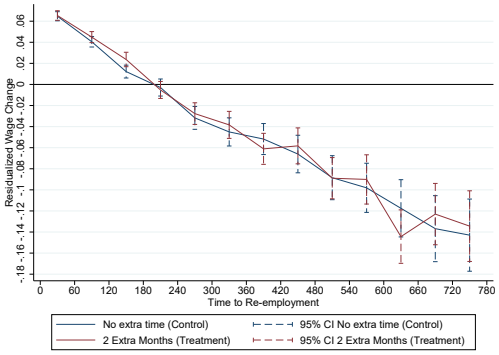
3.2 The Causal Effect of the Exhaustion of UI Benefits on Re-employment Wages

Our previous results indicate that a two-month extension of the potential duration of UI has a small, negative, and insignificant impact on the wage losses between the pre-displacement and re-employment jobs. However, the fact that an extension does not have a strong effect on wage losses on average does not necessarily imply that its effects throughout the entire distribution of time to re-employment are zero.

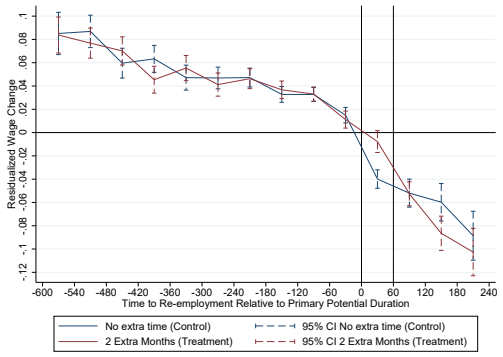
To understand these dynamics, we plot the evolution of the average residualized change in wage for our sample of workers in the treatment and control groups, separately, throughout the distribution of time to re-employment. This is shown in Figure 4 (a). The residualized wage change is generated from regressing the wage change on a vast array of observed characteristics of the worker and the economy. As in the previous section, we define a worker as part of the treatment (control) group if her previous working experience is at most 90 days above (below) one of the policy thresholds. The evolution of wage losses is almost identical in the treatment and control groups, and we do not find significant differences at any point of the distribution. However, Figure 4 (a) combines workers with multiple potential durations in both the treatment and the control groups. This hides important differences in the evolution of wage losses across groups, specifically around the point at which workers exhaust UI benefits.

[26] Figure A.6 shows the manipulation test for each discontinuity separately. While most discontinuities show no manipulation, we observe some evidence of positive manipulation in the discontinuities at 720 and 1440 days and some evidence of negative manipulation at the discontinuity at 1800 days.

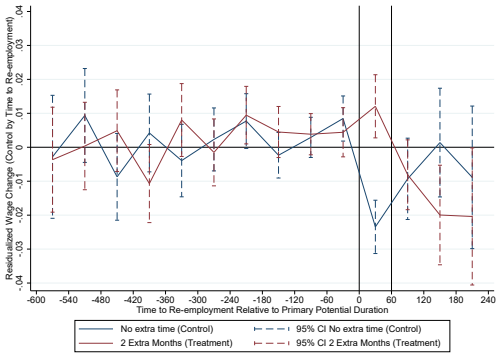
Figure 4: Wage Change Evolution over Time to Re-employment



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



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



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

Note: Figure 4 presents the evolution of the average residualized wage change for workers in the treatment and control groups. The residualized wage change is generated by regressing the wage change on a vast array of observed characteristics of the worker and the economy and then averaging the residuals separately for treatment and control groups within each bin (no reference bin). In panel (a), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment. In panel (b), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment relative to primary potential duration. In panel (c), we present the evolution of the average residualized wage change throughout the distribution of time to re-employment relative to primary potential duration. Here, we additionally include time to re-employment as a control variable when residualizing the wage change. In all the panels, the average is calculated using a 60-day window. We analyze the workers whose tenure is close to a cutoff, such that whether they belong to the treatment group (receiving an additional two months of potential duration of UI) or to the control group is close to randomization. The window we choose to classify a worker into either treatment or control group is 90 days from or past the discontinuity. The red (blue) line shows the residualized wage change path for workers with (without) two extra months of potential duration of UI.

To highlight this, we define the variable "time to re-employment relative to primary potential duration." For workers in the control group, we define it as the difference between the time to re-employment and the potential duration of UI. For workers in the treatment group, we define it as the difference between time to re-employment and the potential duration of UI the worker would have had if she had not crossed the discontinuity, obtaining

two extra months of potential duration of UI (i.e., her actual potential duration minus 60 days). Defining time to re-employment relative to the primary potential duration allows us to compare the evolution of wage losses in the treatment and control groups for workers with a very similar time to re-employment. Further, it helps us highlight how the wage losses evolve before, during, and after the point of expiration of UI benefits in each group.

Figure 4 (b) shows the evolution of the average residualized wage change against time to re-employment relative to the primary potential duration for workers in the treatment and control groups. We do not find significant differences in wage losses between workers in the treatment and control groups anywhere in the distribution except at one point: when the group with shorter potential duration has just exhausted their UI benefits while the treatment group still can collect UI benefits (i.e., from 0 to 60 on the x-axis). Within this range, the control group sees significantly larger wage losses (3 percentage points) that do not equalize across groups until those in the treatment group exhaust their UI benefits. This result is even clearer if we additionally control for time to re-employment when we generate the residualized wage change, as shown in Figure 4 (c). This evidence suggests that an extension of the potential duration of UI not only increases the time to re-employment but, conditional on time to re-employment, also impacts changes in re-employment wages. Specifically, re-employment wages change in the time range in which workers would have already exhausted the UI benefits had they not received a two-month extension.

To estimate the effect of the exhaustion of UI benefits on wage changes, we propose an empirical strategy that combines our RD designs with a difference in differences empirical strategy based on the figure above:

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

In equation (2), y_i refers to the wage change between the pre-displacement and re-employment job of a worker i. T_i takes the value 1 for workers in the treatment group and zero for those in the control group. As before, workers will belong to the treatment (control) group if they are located within 90 days to the right (left) of one of the discontinuities. B_i controls for discontinuity fixed effects, and X_i is a matrix that contains worker characteristics

and economy-wide variables (age, gender, education, wealth, previous tenure, previous experience, type of contract, previous wage, part-time coefficient, location, discontinuity, and month-year fixed effects).

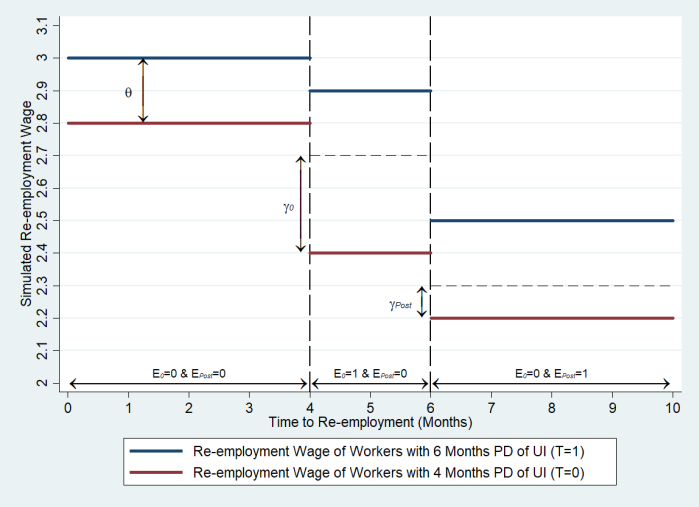
E_0 and E_{Post} are defined based on the variable time to re-employment relative to primary potential duration. E_0 takes the value 1 if the time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60. If the time to re-employment relative to primary potential duration is smaller or equal to zero or larger than 60, E_0 is zero. For workers in the control group, it denotes whether the worker found a job within 60 days after the expiration of UI benefits. For workers in the treatment group, it denotes whether the worker found a job within 60 days of the expiration of her UI benefits. E_{Post} takes the value 1 if the time to re-employment relative to the primary potential duration is above 60 and zero if it is smaller or equal to 60. For workers in the control group, it denotes whether a worker's time to re-employment is at least 60 days after the expiration of UI benefits. In contrast, for workers in the treatment group, it denotes whether the worker's time to re-employment is after the expiration of UI benefits.

To facilitate the understanding of this empirical strategy, let us first introduce an example in which we consider only one discontinuity. Focus on the discontinuity around 540 days of prior tenure. Workers close to it but with shorter previous experience can collect UI benefits for 4 months, while those close to it but with more than 540 days of prior experience can collect UI benefits for 6 months. The definition of treatment and control groups in the difference in differences is based on the RD design. In this example, workers with working experience from 540 to 629 days constitute the treatment group (T=1), while those with working experience from 450 to 539 days form the control group (T=0).

The difference in differences originates from considering 3 different periods, depending on when workers were re-employed, as defined by E_0 and E_{Post}. We illustrate how we construct it for this example in Figure 5. The first period comprises workers finding a job from the moment they start the unemployment spell until they reach 4 months of non-employment duration. During this period, workers in both groups were collecting UI when they found a job. Workers re-employed in the first period have E_0=0 and E_{Post}=0. The second period

comprises workers re-employed starting at 4 months and 1 day of non-employment duration and finishing exactly at 6 months of non-employment duration. In the absence of treatment, workers who were re-employed during the second period would not have been receiving UI benefits just before re-employment. However, because the treatment group was exogenously granted 2 extra months of potential duration, treated workers were collecting UI benefits just before re-employment while control workers were not. For workers re-employed in the second period, E_0=1 and E_{Post}=0. The third period comprises workers re-employed after 6 months and 1 day of non-employment duration. Here, workers in both groups could not collect UI benefits just before they found a job. Workers re-employed in the third period have E_0=0 and E_{Post}=1.

Figure 5: Graphical Explanation of the Difference in Differences Empirical Strategy with Only Two Potential Duration Groups (Simulated Data)



Note: Figure 5 presents a graphical explanation of the differences in differences empirical strategy described in the main text when applied to two groups. For this example, we use the groups with a potential duration of UI that is 4 and 6 months. The values in the y-axis are simulated for explanatory purposes and do not represent the real data.

The difference in differences compares the wages (or wage changes) of the treatment and control groups between the first and second periods. Thus, it calculates the difference in wages between workers re-employed before 4 months and between 4 and 6 months of non-employment duration in the treatment group and compares it to the same difference in wages for the control group. Including the third period allows us to estimate two differences in differences, one between the first and the second periods and one between the first and

the third periods. Figure 5 shows the simulated difference in differences for this example.

Suppose there is no differential dynamic selection across treated and control groups, conditional on time to re-employment. Then, if the exhaustion of UI benefits affects re-employment wages when it takes place, the difference in differences estimate between the first and second periods, γ_0, should be positive. Additionally, if exhausting the UI benefits earlier had persistent effects, thus affecting re-employment wages even after both groups had already exhausted their UI benefits, the difference in differences between the first and third periods, γ_{Post}, should also be positive.[27] Finally, suppose that exhausting UI benefits earlier affects re-employment wages even before the exhaustion takes place because workers anticipate it and react in advance to it. Then, the difference in re-employment wages between the treatment and control groups in the first period, θ, should also be positive.

In the general case, the idea is identical, but we combine workers with all potential durations together. We do this by defining the first, second, and third periods based on time to re-employment relative to primary potential duration instead of time to re-employment. γ_0, associated with $E_0 \times T_i$, is our estimate of the causal impact of the exhaustion of UI benefits. More precisely, γ_0 captures the average difference in wage changes from exiting unemployment in the second period (starting the day after the control group exhausts their UI benefits and finishing 60 days after it, once the treatment group expires their benefits) vs. the first for treated workers, relative to the same difference in the control group.[28] γ_{Post} captures the average difference in wage changes from exiting unemployment in the third period (starting 60 days after the control group expires their benefits, or the day the treatment group expires theirs) vs. the first for treated workers, relative to the same difference in the control group. Note that γ_{Post} is not an estimate of the dynamic treatment effect of the expiration of UI benefits because, when E_{Post} is 1, both groups have already exhausted their benefits.[29]

[27] Note that this interpretation requires the treatment effect of the exhaustion of UI benefits to be the same in the group with 4 months of UI potential duration than in the group with 6 months. As we discuss later, FigureE.1 suggests that is the case.

[28] Table A.6 presents additional robustness results where we allow for some anticipation in the reaction to the exhaustion of UI benefits. Slight variations in the start and end points that determined whether $E_0 = 1$ do not affect our estimates.

[29] If we interpret γ_{Post} as a dynamic treatment effect, it would be the dynamic treatment effect on wages

The key assumption of our empirical strategy is that there is no selection process that is specific to the exhaustion of UI benefits. This assumption rules out the case where some workers are re-employed right before (or after) the exhaustion of UI based on characteristics that also affect re-employment wages. This empirical specification allows for the dynamic selection of workers, in both observed and unobserved characteristics, that determines when they exit non-employment, as long as this selection is the same throughout the distribution of time to re-employment in the treatment and control groups. A different way to understand this assumption is that, conditional on the duration of non-employment, selection into re-employment is not a function of whether the worker reaches the exhaustion of UI benefits. Further in this section, we extensively test whether this assumption holds.

Table 2: Effect of the exhaustion of UI benefit. All discontinuities

	Daily Wage change			Hourly Wage change		
θ	0.000	0.001	0.001	-0.002	0.001	0.000
	[0.003]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
γ_0	0.027***	0.032***	0.035***	0.006	0.012**	0.014***
	[0.008]	[0.007]	[0.007]	[0.006]	[0.005]	[0.005]
γ_{Post}	-0.009*	-0.011**	-0.012***	-0.009**	-0.008**	-0.009**
	[0.005]	[0.005]	[0.004]	[0.004]	[0.004]	[0.004]
Controls	D	All	All	D	All	All
Time to Re-emp Ctrl	No	No	Yes	No	No	Yes
N	122387	122020	122020	122387	122020	122020

Note: Table 2 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) to (3) is the change in daily wages, while in columns (4) to (6), the outcome is the change in hourly wages. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table 2 presents different estimates of γ_0. In column (1), we do not include any additional covariates as controls, and the results suggest that passing the point of exhaustion of UI benefits exacerbates the daily wage losses by 2.7 percent. Our identification assumption is that there is no self-selection of workers specific to the exhaustion of UI benefits. To account for the possible differences in observed characteristics, columns (2) and (5) of Table 2 include

of exhausting UI benefits 2 months earlier (not just of the exhaustion of UI benefits) for workers finding a job after the expiration of UI benefits (conditional on no heterogeneity in the treatment effect of the exhaustion of UI benefits on wages.).

a vast array of controls in the estimation.[30] Even after accounting for differences in observed characteristics, we estimate that exhausting UI benefits deepens the daily wage losses by 3.2 percent and the hourly losses by 1.2 percent. For additional robustness, columns (3) and (6) additionally control non-parametrically for time to re-employment. Our estimates remain very similar, at 3.5 percent for daily wages and 1.4 percent for hourly wages.

These results are consistent with previous findings in the literature. Nekoei and Weber (2017) find that daily re-employment wages declined by an additional 4 to 5 percent for workers with 30 weeks of potential duration relative to workers with 39 weeks during the 9-week period after the former group had exhausted their UI benefits. Schmieder and von Wachter (2016) also find that workers' re-employment wages decline an additional by around 6 or 7 percent right when their benefits expire relative to the wages of workers that could collect UI benefits for an extra 6 months. However, their estimated effect is very short-lived (1 month) and gets offset once the group with longer UI potential duration exhausts their UI benefits. On the other hand, Lalive et al. (2015) find no effect of the expiration of UI benefits on re-employment wages. However, in their sample, comprised of older workers in Austria, only 3 percent of workers see their benefits expire (vs. 22 percent of workers in our sample). This makes their sample of workers exhausting the UI benefits potentially a heavily selected sample of all the unemployed. Furthermore, the Austrian equivalent of unemployment assistance is significantly more generous (up to 92 percent of the previous UI benefits) than its Spanish counterpart.

Our results indicate that the reduction of daily re-employment wages at the point of exhaustion of UI benefits is primarily caused by a reduction in hours worked. But, is this effect temporary, driven by "stepping-stone" jobs, or does it prevail in the long run? To answer this question, Table A.7 shows the causal impact of the exhaustion of UI benefits on the average re-employment wage during the first year after unemployment and the average re-employment wage during the first five years after unemployment. The negative effect on daily re-employment wages of the expiration of UI benefits remains unchanged when we

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

consider the average wage during the first re-employment year. However, after five years, the negative effect softens by around 25 percent. On the other hand, the effect on hourly wages still remains mostly unchanged after 5 years. These results suggest that the negative consequences of the exhaustion of UI benefits on wages are neither short-lived nor necessarily caused by workers taking "stepping-stone" jobs that allow them to climb up the ladder faster.

A limitation of the above difference in differences specification is that it does not consider the distance to the RD threshold of different workers. Despite the short bandwidth we impose to classify workers as part of the treatment or the control group, it is possible that the differences across groups in wage losses around the point of exhaustion of UI benefits are driven by workers located further away from the discontinuity. If that is the case, there may be no differences between groups exactly at the threshold.

To test that our above specification does not provide biased estimates of the effect of the exhaustion of UI benefits on re-employment wage losses, we provide two robustness tests. First, we reduce the window around the discontinuities used to classify workers as part of the treatment and control group in our difference in differences empirical strategy. Thus, instead of using 90 days, we shorten the window to just 37 days, matching the MSE optimal RD bandwidth. Table A.8 shows the results of this exercise, in which our estimates remain unchanged compared to those in Table 2.

Second, we follow the methods in Schmieder et al. (2016) and Nekoei and Weber (2017). We estimate our original RD specification for the sample of workers whose time to re-employment falls in the period after the control group has exhausted their UI benefits but before the treatment group does. This allows us to focus on workers just around the potential duration discontinuity and isolate their response from the response coming from workers further away from the threshold. We present this result in column (2) of Table 3.[31] Our RD

[31]This RD design can also be implemented within the difference in differences described above by estimating the following regression: $y_i = \gamma_0 E_0 \times T_i + \gamma_{Post} E_{Post} \times T_i + \theta T_i + \beta_0^E\ E_0 + \beta_{Post}^E\ E_{Post} + \kappa r D_i + \gamma_0^+ E_0 \times T_i \times r D_i + \gamma_{Post}^+ E_{Post} \times T_i \times r D_i + \theta^+ T_i \times r D_i + \beta_0^{E-}\ E_0 \times r D_i + \beta_{Post}^{E-}\ E_{Post} \times r D_i + \beta_b B_i + X_i \beta_X + \epsilon_i$, where rD_i captures the distance of worker i to the discontinuity. There is only one main difference between this specification and the specification whose results are shown in Table 3. In this specification, the effects of the covariates on the outcome are forced to be identical for all observations, regardless of their time to re-employment. Doing this provides a slight increase in the precision of the estimates. The results of running the RD design within the differences in differences are shown in Table A.9. The estimates are nearly identical to those presented in Table 3.

estimate indicates that, at the discontinuity, wage losses are a significant 2.7 percent larger for workers who just exhausted their UI benefits compared to workers who, having an almost identical time to re-employment, still have 60 more days of UI benefits. The conclusion is similar for hourly wage changes, shown in column (5) of Table 3. At the discontinuity, hourly wage losses are 1.5 percent larger for workers who have just exhausted their UI benefits compared to workers who, having an almost identical time to re-employment, still can collect UI benefits for 60 more days.[32]

Table 3: Effect of a Two-Month Extension of the Potential Duration of UI. All Discontinuities

	Time to Re-employment relative to Primary PD					
	≤ 0	> 0 and ≤ 60	> 60	≤ 0	> 0 and ≤ 60	> 60
	Daily Wage Change			Hourly Wage Change		
RD Estimate	0.001	0.027**	-0.005	0.002	0.015*	-0.007
	[0.005]	[0.013]	[0.011]	[0.004]	[0.009]	[0.007]
Controls	All	All	All	All	All	All
N	81961	11486	28573	81961	11486	28573

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

So far, we have focused our attention on the evolution of wage losses in the treatment and control groups around the point of exhaustion of UI benefits. However, panels (b) and (c) of Figure 4 also suggest that wage losses in the treatment and control groups at any other point (before the exhaustion of UI benefits in the control group and after the exhaustion of UI benefits in the treatment group) appear very similar. The remaining estimates displayed in Table 2, for both daily and hourly wage changes, confirm this intuition. The coefficient θ

[32]Table A.10 shows an equivalent RD estimation where the outcomes are long-run measures of wage changes. Wage losses, measured using the average daily wage during the first year (first five years) of re-employment, are 2.7 percent (1.8 percent) larger for workers who just exhausted their UI benefits compared to workers who have an almost identical time to re-employment, still have 60 more days of UI benefits. Similarly, we estimate that hourly wage losses (first five years average) are 1.2 percent for this same group. Thus, 33 percent of the negative effects of the exhaustion of UI benefits on daily wages dissipate over time, but almost none of its effect on hourly wages disappears. Both conclusions are identical to those provided by the difference in differences estimates.

captures the differences in wage changes between treatment and control groups before the expiration of UI benefits in the control group. All specifications show an estimated θ that is very close to zero and insignificant. Moving to the period after the exhaustion of UI benefits for both groups, the coefficient γ_{Post} suggests that workers in the control group see slightly less negative changes in wages, although the magnitude is small. However, we do not find this result to be compelling evidence of differential re-employment wages across groups in the period after both groups have exhausted their UI benefits. As shown below, the coefficient of γ_{Post} becomes insignificant and close to zero in every single one of our robustness exercises. Likewise, once we control by individual fixed effects in Section 3.3, the estimate of γ_{Post} becomes positive and insignificant.

Our difference in differences' results pre- and post-benefit exhaustion do not appear to be driven by workers further away from the discontinuity. The difference in differences strategy using a 37-day window to classify workers as treated or control provides estimates of θ and γ_{Post} that are very close to zero. This is shown in Table A.8. Similarly, the RD results where we focus on workers whose time to re-employment occurs before the expiration of benefits in the control group, and on workers whose time to re-employment occurs after both groups have exhausted their UI benefits are also insignificantly different from zero. This is shown in columns (1) and (3) of Table 3. Furthermore, both robustness exercises also indicate that the difference in wages losses between treatment and control for workers re-employed after the treatment group exhaust their UI benefits is not just insignificant but also very close to zero. The coefficient of γ_{Post} in column (2) of Table A.8 is just -0.006, and the RD estimate in column (3) of Table 3 is only -0.005.[33]

For additional robustness, we decompose the period before the exhaustion of benefits for workers in the control group and after the expiration of UI benefits in the treatment group in smaller time intervals. This allows us to fix the time to re-employment more accurately. The results are shown in Figure 6(a), (c), and (d), respectively. In no period other than at the exhaustion of UI benefits in the control group (but before the exhaustion of UI benefits for the treatment group), we consistently find significant differences in wage losses across groups.

[33] In Table A.9, where we embed the RD design within the difference in differences, this estimate, γ_{Post}, is even closer to zero (-0.001).

In Figure 6(b), (d), and (f), we change the outcome to be the predicted wage change based on observed worker and economy-wide pre-displacement characteristics. The results show that workers in the treatment and control groups showed no differences in their predicted wage losses at any point of the distribution.

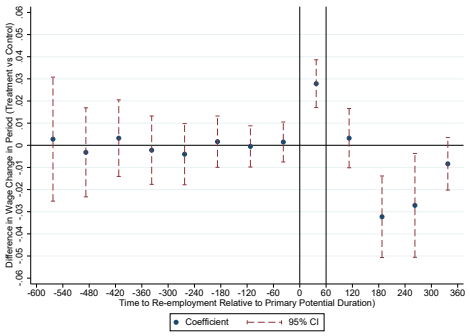
3.3 Robustness: Dynamic Selection on Permanent Unobserved Heterogeneity

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

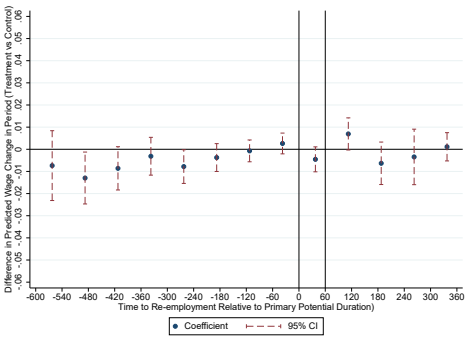
However, even after the different validity tests we display above, it is possible that workers are manipulating their previous experience in ways that remain unobserved in the data, which would invalidate our RD design. Furthermore, our analysis of the wage losses of workers with exogenously shorter and longer potential durations of UI along the distribution of time to re-employment assumes that dynamic selection is identical in both groups. This closes the door to patterns of selection that depend on the duration of UI benefits. It is possible that the workers leaving non-employment in the treatment and control groups after the former reaches the exhaustion of benefits (but before the latter does) differ in unobserved characteristics that could affect their re-employment wages. If that is the case, dynamic selection would be different between treatment and control groups around this point.

To address these concerns, we re-estimate our previous exercises for a sample of workers for whom we observe two or more UI claims. This allows us to control for permanent unobserved heterogeneity in all our estimates. Table A.11 shows the summary statistics of our sample of workers with two or more UI claims and compares them to the summary statistics of the complete sample. While the samples are relatively similar, they are not

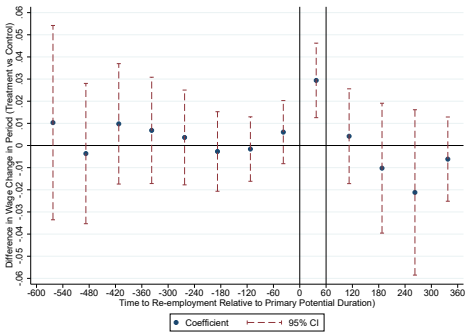
Figure 6: Period by Period Differences in the Evolution of Wage Changes



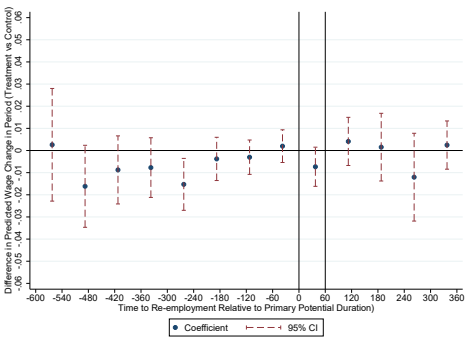
(a) Wage Change (DiD)



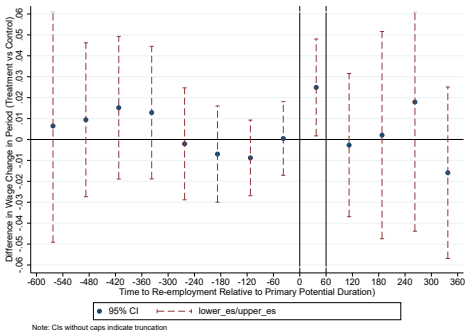
(b) Predicted Wage Change (DiD)



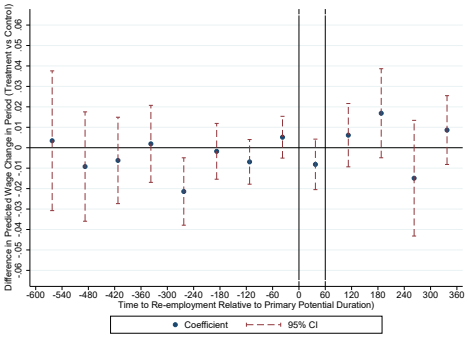
(c) Wage Change (DiD, bw=37)



(d) Predicted Wage Change (DiD, bw=37)



(e) Wage Change (RD)



(f) Predicted Wage Change (RD)

Note: Figure 6(a) presents the estimated coefficients of an extension of equation (2) where we divide the period prior to the exhaustion of UI benefits in the control group, and the period after the exhaustion of UI benefits in the treatment group in multiple 60-day periods. Each coefficient represents the difference in wage changes between treatment and control groups in the corresponding 60-day period of the time to re-employment relative to primary potential duration distribution. We analyze the workers whose tenure is close to a cutoff, such that whether they belong to the treatment group (receiving an additional two months potential duration of UI) or the control group is close to randomization. The window we choose to classify a worker into either treatment or control group is 90 days from or past the discontinuity. Figure 6(b) is equivalent to Figure 6 (a) but replaces the outcome variable with the predicted wage change from a regression on pre-displacement worker and economy-wide characteristics. Panels (c) and (d) are identical to (a) and (b), respectively, but change the window to classify a worker into either treatment or control group to 37 days from or past the discontinuity (MSE optimal bandwidth). Figure 6 (e) presents the estimated coefficients of separate RD regressions in the form of equation (1) that include all control variables and discontinuity fixed effects (see text). In each regression, the sample comprises workers whose time to re-employment relative to primary potential duration is within the corresponding 60-day window. The RD estimates are conventional estimates as shown in Calonico et al. (2014) using a polynomial of degree one, triangular kernel, and a bandwidth of 90 days. Figure 6(f) is equivalent to Figure 6 (e) but replaces the outcome variable in each RD regression with the predicted wage change from a regression on pre-displacement worker and economy-wide characteristics. The 95 percent confidence intervals are constructed from robust standard errors.

perfectly identical.

The fact that both samples are not identical will make the estimates in this section informative of the role of permanent unobserved heterogeneity in our estimates for workers with two or more UI claims. However, using the results controlling for worker fixed effects to assess the importance of permanent unobserved heterogeneity in the complete sample will require additional assumptions. Specifically, we will need to assume that any differential patterns of dynamic selection between treatment and control group based on unobserved permanent characteristics are uncorrelated with the number of UI claims. This implies that any differential patterns of dynamic selection between treatment and control groups based on unobserved permanent heterogeneity must also be uncorrelated with any observed characteristics that differ between the complete sample and the sample of workers with two or more UI claims.

We will maintain this assumption in our interpretation of the results below and in Section 5.4. We did not find any evidence of differential dynamic selection between the treatment and control groups based on observed characteristics in our results in Section 3.2. Therefore, it seems unlikely that there exist patterns of dynamic selection based on unobserved permanent heterogeneity that vary across groups and correlate with observed characteristics or the number of UI claims.

With these caveats in mind, we start by replicating our RD results for this sample of workers in Table A.12. Column (1) restricts our sample to workers with two or more UI claims but does not include individual fixed effects, while column (2) includes them. Our results in column (1) strongly indicate that the response of workers with two or more UI claims to a two-month extension of the potential duration of UI is not significantly different from that of our complete sample. In our restricted sample, workers entitled to collect UI benefits for two additional months increase their non-employment duration by 37 days. This estimate is significantly different from zero at the 1 percent confidence level. Controlling for unobserved permanent heterogeneity in column (2) (i.e., adding individual fixed effects to our estimation) does not change our conclusions.[34] These results suggest that there is no

[34]We show the balance test for this sample, with and without individual fixed effects, in columns (3) and (4) of Table A.5. We do not find evidence of an imbalance in observed characteristics across workers close to

manipulation of the running variable based on unobserved permanent characteristics, a fact that strengthens the credibility of our main estimates.

We next move on to analyzing the evolution of wage losses across the distribution of time to re-employment. The results of our difference in differences empirical strategy in equation (2) for this sub-sample are shown in Table A.13. Compared to our main results, our conclusions remain unchanged. Columns (1) and (2) (respectively (5) and (6) for hourly wage losses) do not use individual fixed effects, while columns (3) and (4) (respectively (7) and (8) for hourly wage changes) do. The results show that compared to our complete sample, our sample of workers with two or more UI claims sees an extremely similar increase in wage losses around the point of exhaustion of UI benefits (2.9 percent in the restricted sample vs. 3.5 percent in the complete sample). Furthermore, even after we control for unobserved individual heterogeneity, our results indicate that exhausting the UI benefits significantly exacerbates the wage losses by 2.4 to 2.5 percent. This result strongly suggests that differential dynamic selection in the treatment and control groups around the point of exhaustion of UI benefits based on permanent unobserved worker characteristics does not appear to be a concern in our sample. We see this as further evidence that the recovered estimates in the previous section represent the causal impact of the exhaustion of UI benefits on re-employment wages.

Similarly, when we compare the wage losses in the treatment and control groups at any other point in time other than when the control group exhausts their UI benefits but before the treatment group does, we find no significant differences. The remaining estimates (θ and γ_{Post}) displayed in Table A.13 for both daily and hourly wage changes show this. The difference in wage losses between treatment and control groups prior to the expiration of UI benefits in the control group and after both groups have exhausted their benefits remains insignificant even after controlling for individual fixed effects. Furthermore, the coefficient of γ_{Post} is not only insignificant but also turns slightly positive. These results suggest that dynamic selection based on permanent unobserved characteristics appears unimportant in explaining the differential evolution of wage losses over time across groups, including at the

but on different sides of the discontinuities.

point of exhaustion of UI benefits.

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

4 A Conceptual Model

This section presents an illustrative framework that helps us connect our empirical findings with the channels behind the negative relationship between re-employment wages and the duration of non-employment. Here, we lay out an outline of the main theoretical results of interest. The detailed model and derivations are shown in Appendix D.

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

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

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

In $g(\cdot, \theta)$, workers only care about the future available benefits, $\mathcal{B}(t) = \{b_t, b_{t+1}, \cdots\}$, representing the future available unemployment insurance or unemployment assistance benefits. This is especially the feature of hand-to-mouth unemployed workers in that their past income history does not affect their future search decisions. θ denotes the structural parameters, including preference parameters (discount factor β, utility function, etc), search technology parameters, and the state of the economy. We specify $b_t = \bar{b}$ for the period during which the worker collects unemployment insurance benefits ($t < B$) and $b_t = \underline{b}$ later on, once the worker runs out of UI benefits and starts collecting unemployment assistance.[35]

[35] Unemployment assistance benefits start once the standard unemployment insurance benefits expire. The duration and amount of unemployment assistance benefits will depend on different household and individual

Given this structure, g can be essentially collapsed into a function of limited dimensions in (B, t, θ).[36] Furthermore, when $t \geq B$, $g(B, t, \theta)$ is a constant and denoted by g^*.

To clarify our exposition, let us consider a prototype parametric example of g that uses the search technology function specified by Nekoei and Weber (2017) (see Appendix D.1 for details):

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

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

The shortcut representation of the optimal wage path in equation (3) is an extended counterpart of the expression proposed by Schmieder et al. (2016). As shown in Appendix D.1, it takes its micro-model foundation from general directed job search models under two important assumptions. First, it assumes that duration dependence is a continuous function of time in unemployment. This implies that the only reason why re-employment wages can decline discretely at the point of exhaustion of UI benefits is that reservation wages discretely decline at that point by an equivalent amount. Second, it assumes the separability between the value of the non-employment and the duration dependence. This assumption is weaker than our first one, and we use it to facilitate our exposition. Moreover, even if this assumption were not satisfied, our shortcut representation of the optimal wage path would still represent a first-order approximation to the real optimal wage path as long as the complementarity between the value of non-employment and the duration dependence is not too strong.

With this optimal wage path set-up, we can now think about how the expected wage of

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

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

a worker changes in response to an extension of UI:

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

Equation (5) holds by the definition of the total derivative and does not rely on a specific model. It illustrates the decomposition of the impact of an extension of the potential duration of UI on expected wages into the sum of two offsetting channels. First, in response to an extension of the potential duration of UI, workers increase their optimal wage choice (and that translates into re-employment wage changes) at each point in time, because they now enjoy a higher value of unemployment. This effect, when positive, refers to what we also call a "binding reservation wage". Even though there is no reservation wage in this framework, we refer to it as such to remain consistent with Schmieder et al. (2016) and other wage-posting models (Burdett and Mortensen (1998)). On the other hand, in models in which workers bargain over wages, this term features how the workers' wage choice responds to the value of their outside option (Jäger et al. (2020)). Second, an extension of the potential duration of UI lowers the relative price of unemployment, increasing time in unemployment. This reduces future wages due to either negative duration dependence or by potentially pushing these workers to find a job past the point at which UI benefits have expired. The two channels work in an offsetting way, such that the final effect on wages of an extension of the potential duration of UI can take a positive or a negative sign.

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

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

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

However, two empirical findings make the assumption of a non-binding reservation wage unreasonable in our data: First, in Section 3.1, we find a negative but insignificant effect of

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

Second, our empirical evidence indicates that wages respond to the expiration of UI benefits only when workers reach it but not in advance of it. As discussed in Krueger and Mueller (2016), the decline in reservation wages up to UI exhaustion is approximately equal to the response of reservation wages to the permanent elimination of UI benefits. In our case, because the workers' wage choice only responds to the elimination of UI benefits around the exhaustion of benefits, this implies that the decline of the reservation wage when UI expires is also equivalent to the response to wage choice to an extension of UI. This suggests that, in our case, the assumption of a non-binding reservation wage is inadequate.

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

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

The first component captures duration dependence, $\rho_h \frac{dED^*}{dB} < 0$. The second component is the binding reservation wage effect $\frac{\partial g(B,t,\theta)}{\partial B}\text{pr}(B)$. It captures the effect on the workers' wage choice of an extension of the potential duration of UI just at the point at which the original benefits would have ended, weighted by the probability of reaching that point. The final component, the anticipation effect, is the sum of two pieces.

The first piece, $\sum_{t<B} \frac{\partial g(B,t,\theta)}{\partial B} \text{pr}(t)$ represents the response of the worker's wage choice in each period before the exhaustion of UI benefits to an extension of the potential duration of UI, weighted by the period's probability of re-employment. The second piece, $\sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta) - g^*)$ arises because an extension of the potential duration of UI decreases the hazard rate earlier in the unemployment spell. This makes workers more likely to reach the original point of exhaustion of UI benefits (i.e., the point at which workers would have exhausted the UI benefits had they not been granted a two-month extension of the potential duration of UI). Because exhausting the UI benefits negatively impacts the worker's re-employment wage, if workers are more likely to reach it under a UI extension, not accounting for this term would overestimate duration dependence.[37] Note that the channel through which the second piece affects re-employment wages is the same as the channel through which the duration dependence does: by extending the time to re-employment. However, this second piece is not part of duration dependence because the root cause of each mechanism is different. While duration dependence exists independently from the UI benefits, this effect only exists because the worker's wage choice responds to the existence of UI benefits. If re-employment wages did not respond to UI benefits (or if UI did not exist), this effect would disappear.

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

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

Compared with the duration dependence IV estimator in Schmieder et al. (2016), which applies when reservation wages do not bind, the LMOS duration dependence estimator will

[37] Note that an extension of the potential duration of UI can make workers more likely to reach the original point of exhaustion of UI benefits while at the same time making workers less likely to exhaust their UI benefits (since their point of exhaustion of UI benefits is now later). However, the key here is whether workers are more likely to reach the point where their benefits would have ended without the extension. The effects for the period starting at the original UI exhaustion point and finishing at the new UI exhaustion point are captured by the binding reservation wage effect. This is because the binding reservation wage effect is just one more term within $\sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta) - g^*)$, specifically that for $t = B$. We give them different names and express them separately for two reasons. First, it helps simplify their interpretation (the terms within the anticipation effects are different than zero if workers react in anticipation to the exhaustion of UI benefits. In contrast, the binding reservation wage effect is a contemporaneous response to it). Second, it makes the mapping between the moments recovered from the data and the objects in the model perfectly clear.

diverge drastically from IV if the binding reservation wage effect and the anticipation effect are important. Moreover, when the anticipation effect is close to zero (which could reflect myopic or over-optimistic[38] job seekers), but the binding reservation wage effect is sizable, the IV estimator will not represent a lower bound of the duration dependence effect because it will be biased toward zero. This will be our case, as we show below.

One potential concern of the LMOS estimator in equation (8) is that it combines the local responses to policy variations of potentially very different workers. However, as we show in Section D.2 of the Appendix, the LMOS estimator is robust to linear aggregation of heterogeneous workers, where the workers could differ in their human capital, risk aversion, leisure preferences, etc. As long as ρ_h does not vary across types of workers, the LMOS estimator will recover duration dependence. If ρ_h varies with the worker's type, the LMOS estimator will still recover a weighted average of the type's duration dependence. The weights will be in proportion to the initial type's density and to the response of the type's non-employment duration to a change in potential duration.

5 Estimating Duration Dependence

Estimating duration dependence using the LMOS estimator requires us to estimate the binding reservation wage effect and the anticipation effect.

5.1 Binding Reservation Wage Effect

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

[38] Over-optimistic job seekers are unemployed workers who heavily overestimate their probability of finding a job. In a standard job search model in which unemployed workers heavily overestimate the job-finding rate, the reservation wage will be constant over the non-employment duration until the worker is close to reaching the exhaustion of UI benefits. Just before the exhaustion of UI benefits, the reservation wage will sharply decline and remain constant afterward.

From Table 2, this is 3.5 percent for daily wages (or 2.7 percent in daily wages after controlling for permanent unobserved heterogeneity) and 1.4 percent for hourly wages.

These estimates are consistent with findings in previous literature. For instance, Krueger and Mueller (2016) find that the within-worker weekly reservation wage declines by 1.8 percent after the exhaustion of UI benefits. However, their limited sample size combined with the exceptionally long potential duration of UI benefits in NJ at the time rendered this effect insignificant. Similarly, Marinescu and Skandalis (2021) find that the decrease in monthly target wage that can be attributed to the reaction to the depletion of unemployment insurance amounts to 2.4 to 3.5 percent. As in our case, their decline is mostly driven by a decline in the targeted hours rather than by a decline in targeted hourly wages.[39]

5.2 Anticipation Effect

The main takeaway of the estimation of the anticipation effect is that, empirically, it is close to zero. To see this, note that the anticipation effect is the sum of two pieces.

The first piece is $\sum_{t<B} \frac{\partial g(B,t,\theta)}{\partial B} \text{pr}(t)$. As discussed above, it represents the response of the worker's wage choice in each period prior to the exhaustion of UI benefits to an extension of the potential duration of UI. Our previous empirical results strongly suggest that this term is zero. To see this, note that our estimates in panels (a), (c), and (e) of Figure 6 indicate that wage losses evolve identically for workers in the treatment and control groups for all the periods prior to the exhaustion of UI benefits. This is a consistent result of both the difference in differences empirical strategy and the RD strategy. Furthermore, both empirical strategies also indicate that predicted re-employment wages at each point of the distribution are the same in the treatment and control groups (panels (b), (d), and (f) of Figure 6). In combination, both pieces of evidence strongly suggest that equality in wage choice, and not differential dynamic selection in the treatment and control groups before the exhaustion of UI benefits, is the reason why wage losses are identical between both groups. This makes this first piece in our estimation zero.

[39]The comparison of our results to those in Marinescu and Skandalis (2021) is not perfectly straightforward. While we find that this decline occurs sharply around the exhaustion of UI benefits, their estimated effects are for the entire year before the exhaustion of UI benefits. However, as the results in their Appendix show, most of their effect occurs within the last 3 months before the expiration of UI benefits.

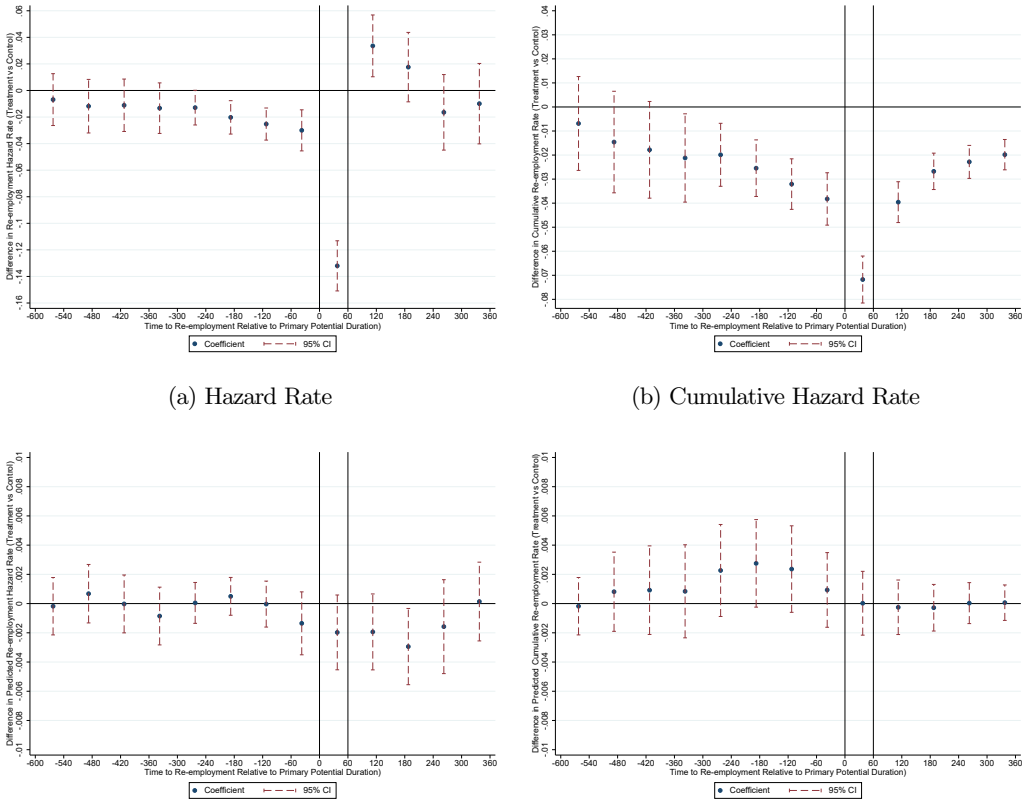
The second piece is $\sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta) - g^*)$. It captures the response of the hazard rate early in the spell to an extension of the potential duration of UI. The relevance of this channel depends on the difference in the hazard rates between treated and control units in all periods prior to the exhaustion of UI benefits. If they are identical, this channel will be irrelevant. On the other hand, if the hazard rate responds to an extension of the potential duration of UI from the start of the non-employment spell, its relevance will be larger.

Figure 7 (a) plots the estimated difference in the hazard rate between the treatment and control groups at each point of the time to re-employment relative to the primary potential duration distribution. The period-by-period difference in hazard rates is zero at the start, but over the last few periods before the original point of exhaustion of UI benefits, this difference becomes negative and significantly different from zero. The conclusions are similar when looking at the cumulative unconditional probability of re-employment, shown in Figure 7 (b). By the time the control group reaches the expiration of UI benefits, the cumulative re-employment probability of workers in the treatment group is approximately 4 percentage points lower than for workers in the control group. Differential dynamic selection across groups appears to play no role in explaining these differences in re-employment probabilities. This is evidenced in panels (c) and (d) of Figure 7. The difference between the predicted hazard rates (and the predicted cumulative hazard rates) across groups is estimated to be insignificantly different from zero in all periods of interest.

Note that under the assumption that $g(B,t,\theta) - g^*$ is constant for all t smaller than B (more on this below), we can express our term of interest as: $(g(B,t,\theta) - g^*) \cdot \frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$. The RD estimation indicates that the cumulative probability of existing non-employment before the original point of expiration of UI benefits is 4.3 percentage points lower for the treated group. Therefore, $\frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$=-0.043.

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

Figure 7: Differences in Re-employment Rates



(a) Hazard Rate

(b) Cumulative Hazard Rate

(c) Predicted Hazard Rate

(d) Predicted Cumulative Hazard Rate

Note: Figure 7 (a) presents the estimated coefficients of separate RD regressions in the form of equation (1), including all control variables and discontinuity fixed effects (see text). The outcome in each regression is a dummy variable taking the value of one if the worker is re-employed within that period and the value of zero if she is not (conditional on not being re-employed before that period). Figure 7 (b) is equivalent to panel (a) but replaces the outcome variable with a dummy variable taking the value of one if the worker is re-employed within or before that period and the value of zero if she is not. Figure 7 (c) and (d) are equivalent to Figure 7 (a) and (b), respectively, but replace the outcome variable with the predicted probability of re-employment from a regression on pre-displacement worker and economy-wide characteristics. The RD estimates are conventional estimates as shown in Calonico et al. (2014) using a polynomial of degree one, triangular kernel, and a bandwidth of 90 days. The 95 percent confidence intervals are constructed from robust standard errors.

previous literature.

Our preferred approximation of $g(B,t,\theta) - g^*$ comes from the causal effect of the exhaustion of the UI benefits on wage losses. From Table 2, this estimate is 3.5 percent. The assumption here is that the counterfactual wage change we would observe in all periods $t < B$ in the absence of UI benefits would decrease by the same amount as we estimate it does at the point of exhaustion of UI benefits.

Appendix E discusses in detail the quality of our approximation, but, in short, our

approximation is consistent with the findings in the previous literature and those from additional sources of exogenous variation in our data. For instance, Kim et al. (2024) report that the absence of UI benefits is associated with a 3.9 percent lower reservation wage for US workers, with a range between 2.5 and 4.3 percent, depending on whether the unemployment rate is higher or lower. This finding is similar to the point estimate reported in Krueger and Mueller (2016). They find that the absence of UI benefits is associated with a decline in reservation wages of 4.7 percent, even if their limited sample size makes this estimate insignificant.

Our data also enables us to estimate the response of re-employment wages to the absence of UI benefits, using exogenous variation in the replacement rate created by a policy change in 2012 (see Appendix E.1). Following the methodology in Rebollo-Sanz and Rodríguez-Planas (2020), we find that in the absence of UI benefits, workers' re-employment wage losses would increase by an insignificant additional 0.5 percent. Our approximation, 3.5 percent, lies in the middle of the range of the previously discussed estimates.

Our initial assumption, that $g(B,t,\theta)-g^*$ is constant for all t smaller than B, also appears to hold in our data and the findings from previous work. We analyze whether the effects of the exhaustion of UI benefits on wage losses vary for workers reaching the point of exhaustion with different non-employment durations and find no significant differences (see Appendix E.2). Looking at the previous literature, the results in Marinescu and Skandalis (2021) suggest that $g(B,t,\theta)-g^*$ could be at most 4 percent at the beginning of the unemployment spell. From there, it should decrease to a level of around 2.5 percent before the effects of the exhaustion of UI benefits kick in. Since they estimate the effect of the exhaustion of UI benefits on target wages to be 3.5 percent, this indicates that our approximation could be, if anything, a slight overestimation of the actual $g(B,t,\theta)-g^*$. This is because we find that the period-by-period difference in hazard rates between treatment and control groups is zero at the start of the unemployment spell but becomes negative and significant over the last few periods before the exhaustion of UI benefits.

Overestimating $g(B,t,\theta)-g^*$ would imply that our estimate of the anticipation effect captures an upper bound of its relevance. If the true $g(B,t,\theta)-g^*$ was smaller than 3.5

percent, the real anticipation effect would be even smaller and less relevant, making the true LMOS estimate more negative.

In summary, of the two different terms that form the anticipation effect, one is zero, and the second one is at most 0.0015 (0.043*0.035). Therefore, accounting for this effect in our LMOS estimator of duration dependence will have a limited impact on our conclusions. In our results later in this section, we present the estimates of duration dependence including and excluding this term, to make clear its limited relevance and its interpretation from a theoretical perspective.

5.3 Results on Duration Dependence

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

The estimated duration dependence using the LMOS estimator is shown in the first row of Table 4. Our findings suggest that one additional month of non-employment increases the daily wage losses by 0.75 percent per month. This result is significantly different from zero at the 90 percent confidence level. This effect is equivalent to an additional wage loss of 9 percent for one additional year of unemployment duration.

Table 4: Duration Dependence (LMOS) vs
IV Estimates from Schmieder and von Wachter (2016)

	Daily Wage Change	Hourly Wage Change
LMOS	-0.0075*	-0.0023
90% CI	[-0.0147,-0.0001]	[-0.0081, 0.0041]
LMOS (Ignoring Anticipation Effect)	-0.0091*	-0.0029
90% CI	[-0.0163,-0.0015]	[-0.0089, 0.0034]
IV (Schmieder and von Wachter (2016))	-0.0064	-0.0019
90% CI	[-0.0135, 0.0013]	[-0.0075, 0.0046]
Ratio LMOS vs. IV	117%	121%
Controls	All	All
N	122027	122027

Note: Table 4 presents in row (1) the estimated (monthly) duration dependence (LMOS estimator) in daily re-employment wage (column (1)) and in hourly re-employment wage (column (2)). In row (2), we show an estimate of duration dependence (LMOS estimator without anticipation effects) that ignores the anticipation effects. Row (3) shows the IV estimate proposed in Schmieder and von Wachter (2016). Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Bootstrapped 90 percent confidence intervals (500 bootstraps) in brackets. The sample is bootstrapped at the unemployment spell level. p-value: * 0.10 ** 0.05, *** 0.01.

As discussed above, the anticipation effects are of limited relevance. This can be observed by comparing the results in the first and second rows of Table 4. In the latter, we ignore these anticipation effects and find estimates of duration dependence that are only 0.16 percentage points more negative (0.91 percent per month of non-employment). Our main estimate of the LMOS assumes that the response of the wage choice in the periods before the exhaustion of UI benefits is the same as the one we observe at the point of exhaustion of benefits. As discussed above, we understand this approximation as an upper bound of how much the choice of wage could respond in these periods. If the response during these periods were smaller,[40] the real LMOS estimate that includes the anticipation effects would be more negative. Based on our model, the small anticipation effect suggests that workers behave as if their β is significantly lower than one, showing a strong degree of impatience, myopia, or over-optimism. This is not an uncommon result in this literature, something previously highlighted in Ganong and Noel (2019), Mueller et al. (2021), or DellaVigna et al. (2022).

How do these estimates compare to those in the prior literature? The closest point of comparison comes from Schmieder et al. (2016). Their proposed IV estimator, using German data, suggests that one additional month of non-employment decreases re-employment wages by 1 percent. However, their work and our paper have a few major differences. First, the IV estimator is only suited to cases where the reservation wage is not binding. As discussed above, this is not the case in our data. Using the IV estimator in our case would result in an underestimation of the degree of duration dependence. This is shown in Table 4, where we compare the LMOS estimates (row 1) versus those using the IV estimator (row 3). We find that the IV estimator underestimates the deterioration rate of labor market opportunities by 17 percent.

Second, Schmieder et al. (2016) argue that their IV estimate is a lower bound for the causal effect of non-employment duration on re-employment wages when reservation wages are slightly binding. While this is true under their assumptions, in our case, these assumptions do not hold. Given how reservation wages bind in our data (for the two months after the original point of exhaustion of UI benefits, workers enjoy reservation wages that are around

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

3 percent higher under an extension of UI), the IV estimate captures not only the causal effect of the duration of non-employment on re-employment wages but also the effect of a higher wage choice. This can be seen comparing rows (2) and (3) in Table 4. While the anticipation effect works to offset this difference (because the re-employment probability before the expiration of UI benefits decreases driven by the extension of the UI), its effect is not strong enough. Therefore, if we are interested in estimating duration dependence net of the effect of the exhaustion of UI benefits on re-employment wages (our LMOS estimator), the IV estimate no longer acts as a lower bound.

An important part of the degree of duration dependence in daily wages appears to be driven by workers' inability to find full-time jobs as the duration of non-employment increases. As shown in column (2) of Table 4, when we ignore hours worked and focus only on hourly wages, the estimated degree of duration dependence becomes smaller (0.23 percent per month of unemployment) and insignificantly different from zero.

Our analysis of hourly wages is conceptually much closer to that in Schmieder et al. (2016), in that reservation hourly wages are just barely binding. The main reason why reservation wages are barely binding when analyzing hourly wage losses can be seen in Table 2. Hourly wage losses between the pre-displacement and re-employment jobs see a much smaller increase at the point of exhaustion of UI benefits compared to daily wage losses. This results in their IV estimate for hourly wages being almost identical to our LMOS estimate of duration dependence in hourly wages.

Marinescu and Skandalis (2021) estimates duration dependence in monthly wages to be 1.5 percent per year in France, primarily driven by hourly wages and not hours worked. Their estimate for monthly wages is significantly smaller than our estimate for daily wages, and the role of hours worked in explaining it significantly differs between both papers. However, a few aspects separate our work from theirs. First, Marinescu and Skandalis (2021) focus on a sample of low-wage workers. These workers earn close to minimum wage and thus should see lower duration dependence because the minimum wage sets a lower bound for their target wages. Second, we use exogenous variation in potential duration to estimate duration dependence, which allows us to relax the assumptions we need to impose for its

identification.[41] Third, Marinescu and Skandalis (2021) estimate duration dependence on target wages, using the posted wages of the jobs the workers applied to as their measure target wages. The mapping from the average target wage in the re-employment period to the re-employment wage could change over the duration of non-employment, making the comparison between their estimates of duration dependence on target wages and estimates of duration dependence in re-employment challenging. For instance, if workers are equally likely to apply to part-time jobs at different unemployment durations but are more likely to accept part-time jobs as the duration of unemployment increases, duration dependence in re-employment wages and target wages could be different.[42]

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

Our results above indicate that duration dependence is 0.75 percent in daily wages and 0.23 percent in hourly wages. However, the moments used in the estimation do not take into account the potential role of permanent unobserved heterogeneity. To consider this, we repeat our estimation in a sample of workers with two or more UI claims, both with and without including individual fixed effects. The results are shown in Table A.15. Three main points are worth highlighting.

First, we cannot rule out that the estimated LMOS (in both daily and hourly wages) is identical to our main estimates using the complete sample. Second, there is no strong evidence that dynamic selection based on permanent unobserved heterogeneity plays an important role in biasing our main results, especially those for hourly wages. The estimated LMOS in hourly wages is identical in our complete sample, in our sample of workers with two or more UI claims when we do not include individual fixed effects, and in this same sample after controlling for permanent unobserved heterogeneity. Third, we find some evidence (even if statistically insignificant) that dynamic selection based on unobserved permanent heterogeneity affects our main results regarding daily wages. Our estimate of the LMOS

[41] The identification in Marinescu and Skandalis (2021) requires the assumption that the evolution of search effort and target wages during the unemployment spell is uncorrelated with the worker's potential duration or UI eligibility.

[42] An additional example of this problem would arise if workers apply to "long-shot" jobs (i.e., jobs paying high wages for which the workers may not be good matches) at differential rates over the non-employment spell. In this case, duration dependence in target and re-employment wages could also be different.

for the sample with two or more UI claims after controlling for individual fixed effects is 0.43 percent. While this estimate is not statistically significantly different from our main result (0.75 percent), the difference between both is worth mentioning. This result suggests that a non-trivial part of the increase in wage losses that we observe as the duration of non-employment accumulates could be driven by selection in hours worked (workers with a higher propensity to choose fewer hours of work exit non-employment later).

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

6 Conclusion

This paper estimates the causal effect of the duration of non-employment on re-employment wages. Using exogenous variation in the potential duration of UI, we estimate that monthly duration dependence is 0.75 percent in daily wages and 0.23 percent in hourly wages.

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

regarding the prospects of labor market opportunities over time.

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

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

Third, while we assume that duration dependence is linear in the duration of non-employment, this does not necessarily need to be the case. Duration dependence may be stronger at the beginning of the unemployment spell if, for instance, workers' specific human capital depreciates faster than general human capital. Cohen et al. (2023) document that skill depreciation in general human capital is unlikely to be a major explanation for duration dependence in re-employment wages or hazard rates. However, the evidence regarding specific human capital is scarce. Whether duration dependence is linear in the duration of non-employment has important consequences for the distributional effects of UI, especially for long-term unemployed workers.

Last, our paper does not consider the extensive margin of finding a job versus leaving the labor force and how it evolves over time. Does the probability of finding any job decline over time? Previous work has found it difficult to provide an answer to this question, even when data on the search behavior of unemployed workers are available (see DellaVigna et al. (2022)). However, this is an important question to explore from the perspective of understanding duration dependence in re-employment wages, since the relevance of the

extensive margin may differ across workers. For instance, in low-wage jobs, workers have less space for bargaining over wages, making this extensive margin more relevant. Therefore, a crucial piece to understanding the job market opportunities of long-term unemployed workers relies on understanding the dynamics of the job-finding rate over time.

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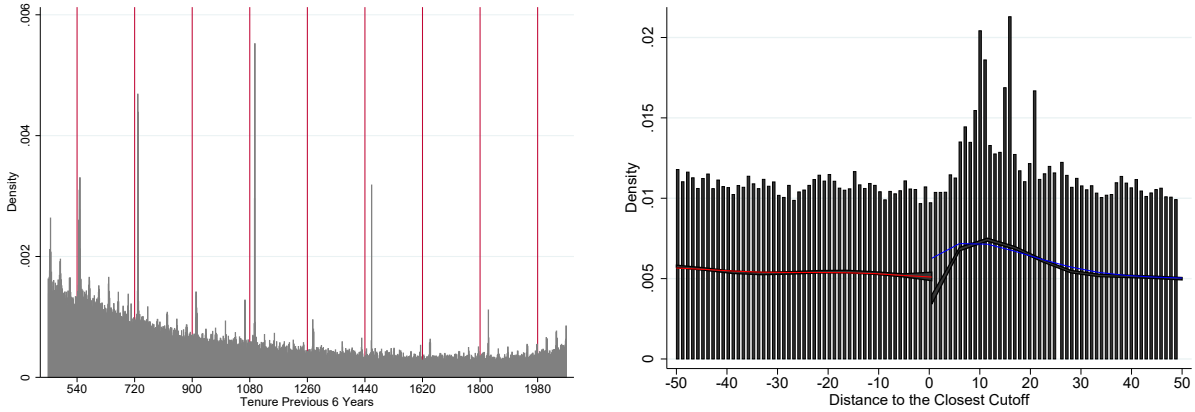
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Appendix For Online Publication

A Additional Figures

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

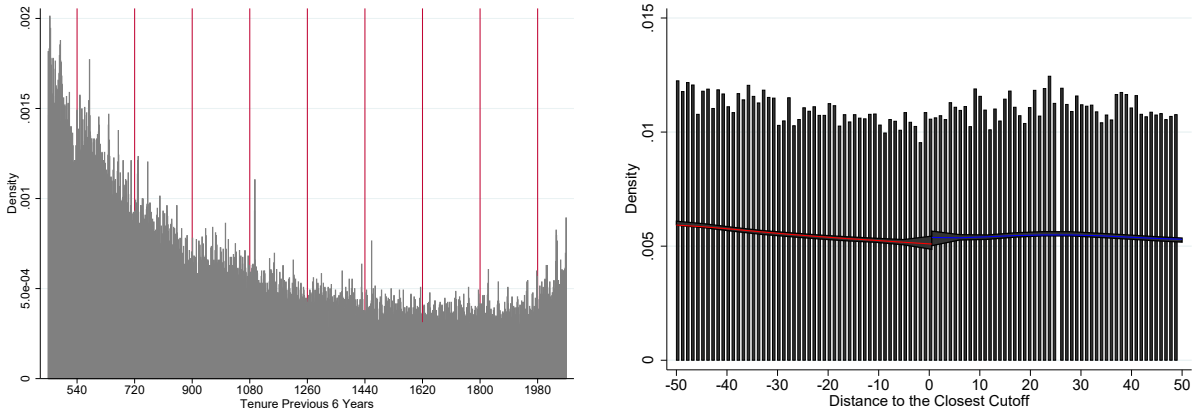


(a) Previous Tenure

(b) Manipulation Test Previous Tenure (Relative to Policy Discontinuities) t = -7.078, p-value = 0.000

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

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

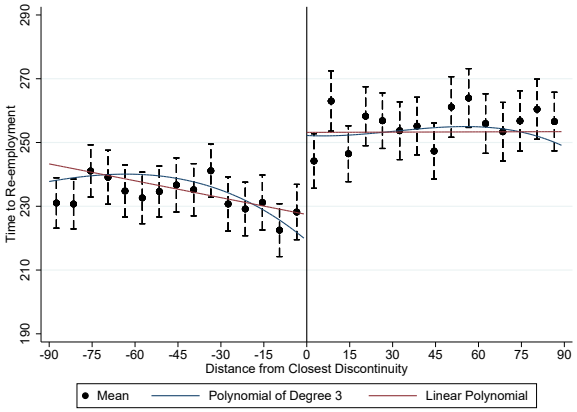


(a) Previous Tenure

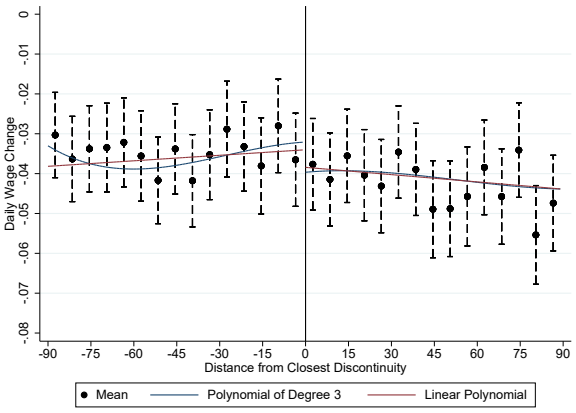
(b) Manipulation Test Previous Tenure (Relative to Policy Discontinuities): t = 0.860, p.value = 0.390

Note: These figures plot the distribution of working experience in the previous 6 years (the running variable) for our final sample. Panel (a) presents tenure separately for each discontinuity, with red bars marking each of the policy thresholds. Panel (b) presents the distribution around the closest policy discontinuity for all thresholds combined and adds the point estimates and confidence intervals of the manipulation test as in Cattaneo et al. (2018). The estimate does not reject no manipulation.

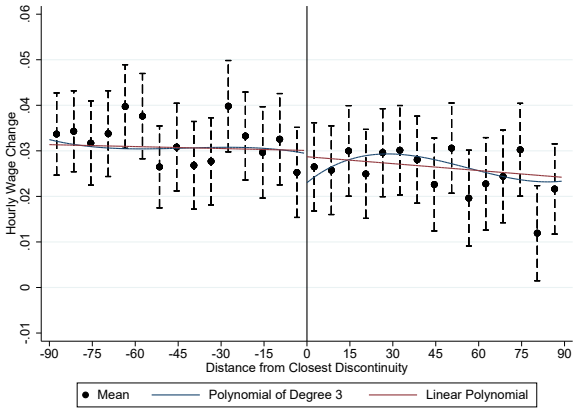
Figure A.3: RD Plots with Different Degree Polynomials



(a) Time to Re-employment



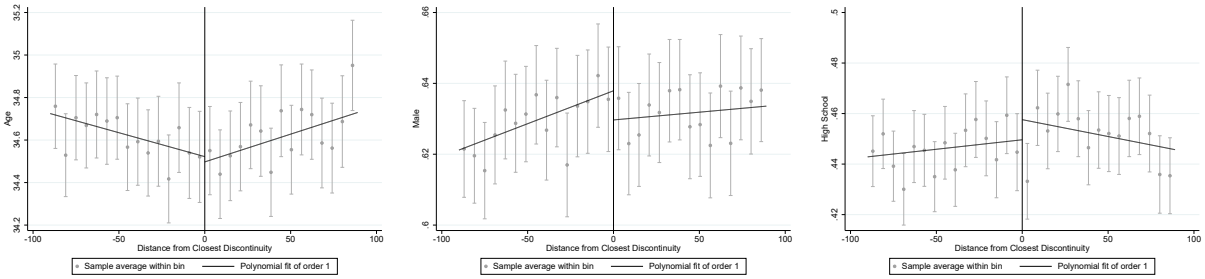
(b) Daily Wage Change



(c) Hourly Wage Change

Note: These figures plot the RD estimates from Calonico et al. (2014) along with the mean of the outcome and confidence intervals of the mean in each bin using different polynomial degrees. The outcome for each panel is shown in the panel's caption. The specification includes discontinuity fixed effects (see text). All RD estimates in all figures use a bandwidth of 90 days.

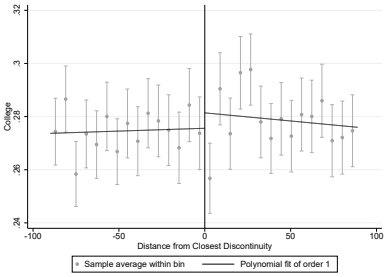
Figure A.4: Balance Tests for Different Covariates



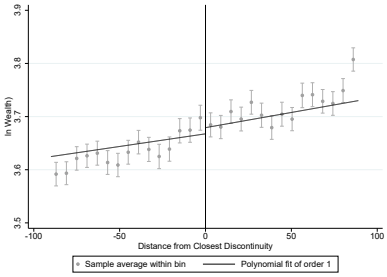
(a) Age

(b) Male

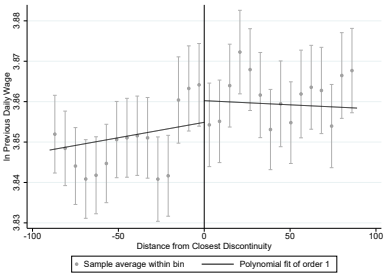
(c) High School



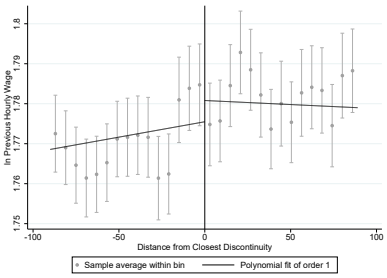
(d) College



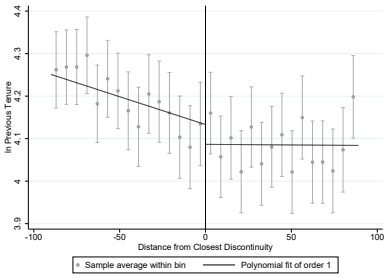
(e) *ln* Wealth



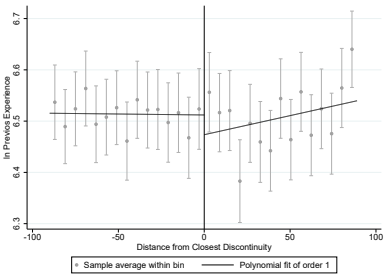
(f) Previous Daily Wage



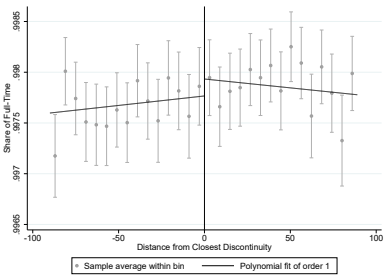
(g) Previous Hourly Wage



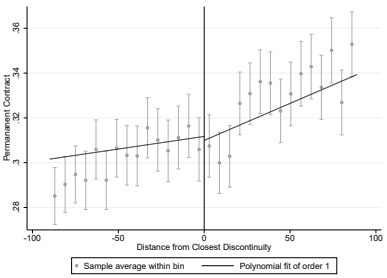
(h) Previous Firm Tenure



(i) Previous Experience



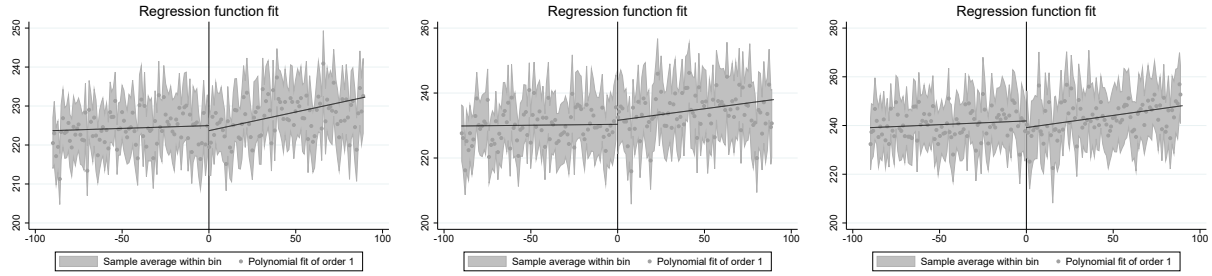
(j) Share of Full-Time



(k) Permanent Contract

Note: Figure A.4 shows the balance test for a series of observed covariates around the discontinuity. We pool all discontinuities together. These figures plot the RD estimates from Calonico et al. (2014) along with the mean of the outcome and confidence intervals of the mean in each bin using a polynomial of degree 1. The specification includes discontinuity fixed effects (see text). All RD estimates in all figures use a bandwidth of 90 days.

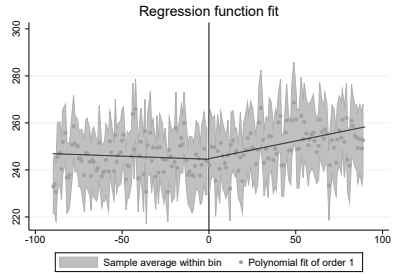
Figure A.5: Covariate Index Balance Test by Discontinuity



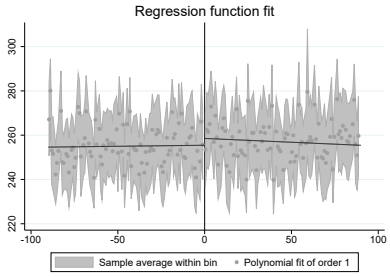
(a) 540 Days (E=-1.30, SE=1.33)

(b) 720 Days (E=1.24, SE=1.67)

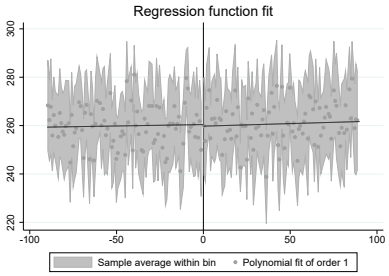
(c) 900 Days (E=-2.74, SE=2.07)



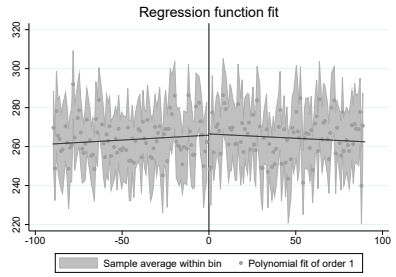
(d) 1080 Days (E=0.27, SE=2.27)



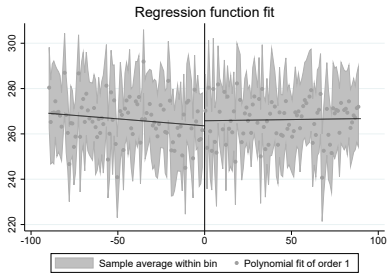
(e) 1260 Days (E=3.07, SE=2.65)



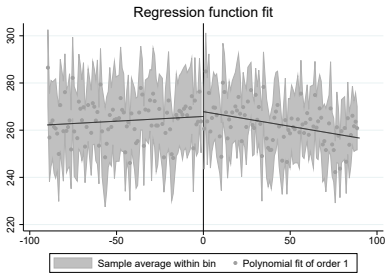
(f) 1440 Days (E=-0.65, SE=2.72)



(g) 1620 Days (E=0.65 SE=2.88)



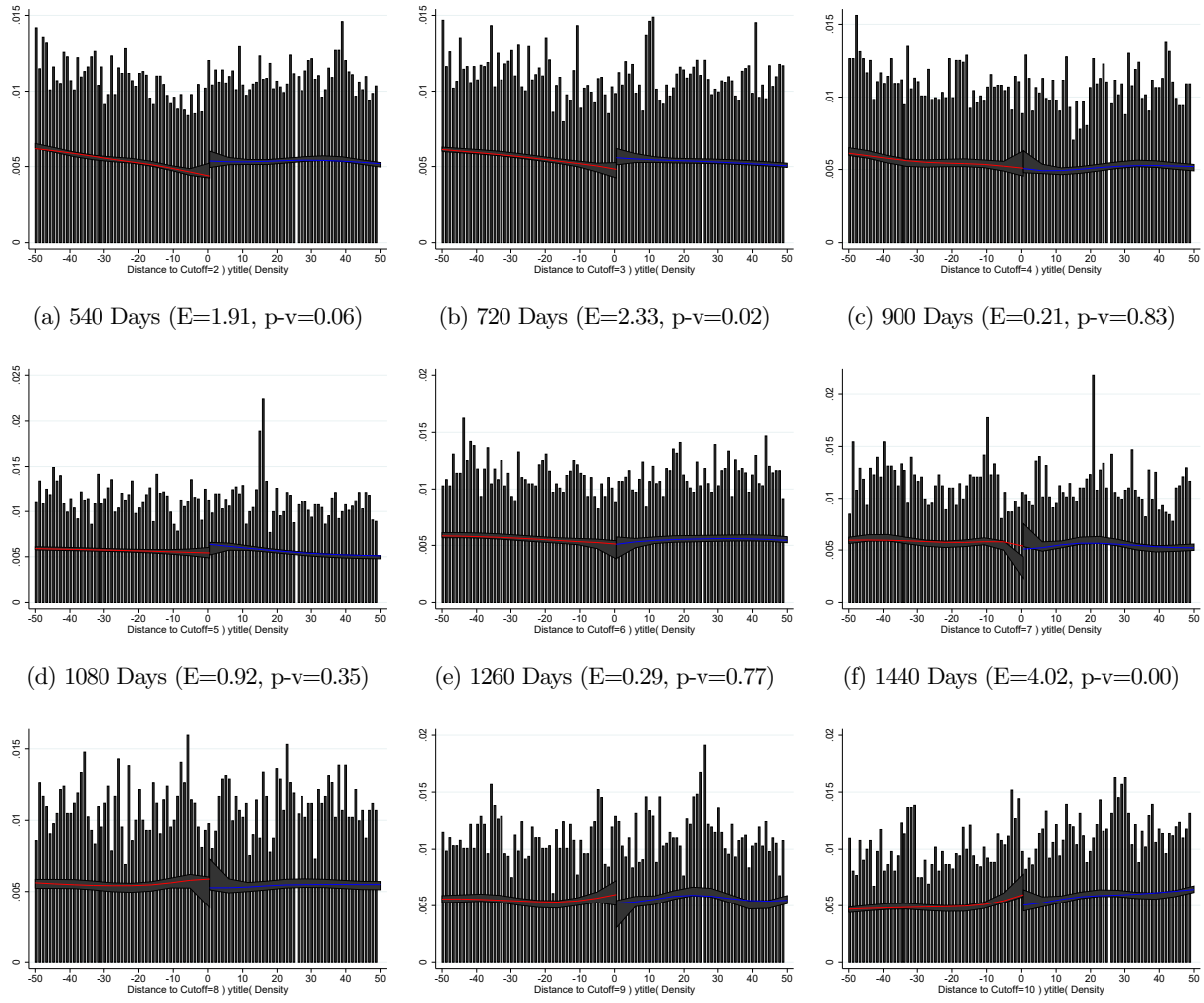
(h) 1800 Days (E=2.24, SE=2.86)



(i) 1980 Days (E=2.02, SE=2.57)

Note: Figure A.5 shows the balance test of the covariate index, separately for each discontinuity. The index is a linear combination of a vector of characteristics correlating with the duration of unemployment, including wealth, previous wage (hourly and daily), experience, tenure, gender, education (high school and college), age, part-time coefficient, and type of contract. All RD estimates in all figures use a bandwidth of 90 days, a polynomial of degree 1, and include discontinuity fixed effects (see text).

Figure A.6: Manipulation Tests by Discontinuity



(a) 540 Days (E=1.91, p-v=0.06) (b) 720 Days (E=2.33, p-v=0.02) (c) 900 Days (E=0.21, p-v=0.83)

(d) 1080 Days (E=0.92, p-v=0.35) (e) 1260 Days (E=0.29, p-v=0.77) (f) 1440 Days (E=4.02, p-v=0.00)

(g) 1620 Days (E=1.60 p-v=0.11) (h) 1800 Days (E=-2.26, p-v=0.02) (i) 1980 Days (E=-1.85, p-v=0.06)

Note: Figure A.6 shows the manipulation test for each discontinuity in our sample. Each panel presents the distribution around the specific discontinuity and adds the point estimates and confidence intervals of the manipulation test from Cattaneo et al. (2018). The bias-corrected robust estimates are shown in the caption below each sub-figure.

Additional Tables

Table A.1: Summary Statistics

Potential Duration	120	180	240	300	360	420	480	540	600	660	All
Days Collecting UI	92.01	109.9	136.3	157.3	183.6	200.9	219.4	240.5	254.7	263.5	161.9
	(49.66)	(69.13)	(90.21)	(112.2)	(134.4)	(154.0)	(174.5)	(196.6)	(214.2)	(229.6)	(142.2)
Time to Re-employment (Days)	184.5	190.5	219.9	235.5	266.2	279.9	294.8	313.6	320.6	329.3	241.8
	(234.4)	(246.3)	(269.2)	(278.5)	(304.4)	(313.8)	(324.6)	(340.2)	(342.3)	(356.3)	(291.1)
Re-employment in 6 months	0.721	0.670	0.608	0.583	0.538	0.521	0.504	0.490	0.483	0.480	0.592
Re-employment in 12 months	0.888	0.878	0.846	0.811	0.753	0.731	0.711	0.682	0.676	0.674	0.799
Share Exhausting UI	0.382	0.262	0.225	0.198	0.186	0.164	0.146	0.143	0.124	0.109	0.220
Real Previous Daily Wage	47.23	48.10	48.41	49.81	50.39	51.41	52.57	52.60	53.13	54.08	49.85
	(16.15)	(17.04)	(16.94)	(18.28)	(19.15)	(19.87)	(20.46)	(20.43)	(20.62)	(21.47)	(18.52)
Real Previous Hourly Wage	5.904	6.013	6.051	6.227	6.299	6.426	6.571	6.575	6.642	6.760	6.231
	(2.019)	(2.130)	(2.117)	(2.285)	(2.394)	(2.483)	(2.557)	(2.554)	(2.577)	(2.684)	(2.315)
Previous (6 Years) Tenure	491.5	622.6	804.1	987.4	1165.2	1348.1	1527.6	1709.7	1891.0	2027.4	1049.3
	(25.60)	(51.11)	(51.53)	(51.33)	(51.97)	(51.84)	(51.94)	(51.37)	(52.72)	(25.97)	(480.2)
Age	35.11	34.69	34.33	34.32	34.35	34.32	34.36	34.65	34.98	35.65	34.63
	(7.236)	(7.104)	(7.017)	(6.993)	(6.958)	(6.911)	(6.908)	(6.920)	(6.847)	(6.804)	(7.024)
Share Male	0.623	0.634	0.641	0.633	0.627	0.627	0.624	0.633	0.633	0.666	0.633
Share College	0.245	0.260	0.269	0.286	0.282	0.294	0.301	0.285	0.274	0.251	0.272
Share High School	0.402	0.423	0.439	0.456	0.456	0.472	0.492	0.468	0.459	0.436	0.443
Wealth*	42.34	43.89	44.68	48.91	51.01	55.20	58.63	64.15	69.98	77.27	51.30
	(36.38)	(36.25)	(35.52)	(35.70)	(35.51)	(36.13)	(36.58)	(37.65)	(38.74)	(39.37)	(37.76)
Change Log Real Daily Wage	-0.012	-0.008	-0.025	-0.040	-0.046	-0.051	-0.068	-0.076	-0.085	-0.095	-0.038
	(0.357)	(0.362)	(0.378)	(0.374)	(0.387)	(0.387)	(0.384)	(0.389)	(0.390)	(0.395)	(0.377)
Change Log Real Hourly Wage	0.052	0.053	0.045	0.031	0.023	0.020	0.0036	-0.004	-0.015	-0.028	0.029
	(0.298)	(0.306)	(0.313)	(0.312)	(0.323)	(0.327)	(0.325)	(0.329)	(0.332)	(0.337)	(0.317)
N	17519	27293	18910	14616	11826	10093	8756	8395	8782	5939	132129

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

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

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate l	31.6***	29.6***	-0.009	-0.014*	-0.005	-0.010*
	[5.6]	[5.4]	[0.009]	[0.008]	[0.008]	[0.006]
Controls	Disc	All	Disc	All	Disc	All
Bandwidth	35.2	36.2	25.1	27.5	23.9	26.0
N	132129	130118	122387	122020	122387	122020

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

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

	Daily Wage Change Avg Wage 1st Year after Re-employment		Daily Wage Change Avg Wage First 5 Years after Re-employment		Hourly Wage Change Avg Wage First 5 Years After Re-employment	
RD Estimate	-0.008*	-0.008**	-0.008*	-0.008**	-0.002	-0.001
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]
Controls	Disc	All	Disc	All	Disc	All
N	128864	128480	129285	128896	129285	128896

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

Table A.4: Effect of a two-month extension of the potential duration of UI. All discontinuities

	Discontinuity	Time to Re-employment	Daily Wage Change	Hourly Wage Change
RD Estimate	540	27.1***	-0.001	0.004
		[5.6]	[0.007]	[0.005]
	720	22.9***	-0.003	-0.000
		[6.6]	[0.008]	[0.006]
	900	26.5***	-0.008	-0.006
		[7.8]	[0.010]	[0.007]
	1080	25.5***	-0.019*	-0.016**
		[8.4]	[0.010]	[0.008]
	1260	36.3***	-0.007	-0.004
		[9.6]	[0.012]	[0.009]
	1440	31.3***	0.018	-0.007
		[10.1]	[0.012]	[0.010]
	1620	50.1***	-0.021	0.003
		[10.8]	[0.013]	[0.010]
	1800	14.4	-0.012	-0.005
		[10.6]	[0.013]	[0.010]
	1960	27.7***	-0.008	0.008
		[9.7]	[0.012]	[0.009]
Controls		All	All	All
Bandwidth		90	90	90
N		129301	121248	121248

Note: Table A.4 presents the estimated causal effect of a two-month extension of the potential duration of UI on the time to re-employment (column (1)), on the wage change (column (3)), and on the hourly wage change (column (5)), separately for each discontinuity. We estimate as one RD specification, allowing the effect of crossing the discontinuities to vary for each discontinuity. Controls "All": All controls included (including discontinuity fixed effects; see text). All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.5: Balance Test. All discontinuities

Panel A: Age				
RD Estimate	-0.005	0.002	0.078	0.116
	[0.136]	[0.085]	[0.136]	[0.133]
Bandwidth	35 (O)	90	90	90
Panel B: Male				
RD Estimate	-0.006	-0.007	-0.002	-
	[0.012]	[0.006]	[0.010]	[.]
Bandwidth	25 (O)	90	90	-
Panel C: High School				
RD Estimate	-0.013	0.002	0.004	-
	[0.012]	[0.006]	[0.010]	[.]
Bandwidth	22 (O)	90	90	-
Panel D: College				
RD Estimate	-0.022*	0.002	0.002	-
	[0.012]	[0.005]	[0.009]	[.]
Bandwidth	19 (O)	90	90	-
Panel E: *ln* Wealth				
RD Estimate	0.006	0.016	-0.000	0.003
	[0.016]	[0.009]	[0.014]	[0.015]
Bandwidth	28 (O)	90	90	90
Panel F: *ln* Previous Daily Wage				
RD Estimate	-0.012	0.003	0.004	0.010
	[0.008]	[0.004]	[0.007]	[0.007]
Bandwidth	25 (O)	90	90	90
Panel G: *ln* Previous Hourly Wage				
RD Estimate	-0.012	0.003	0.004	0.010
	[0.008]	[0.004]	[0.007]	[0.007]
Bandwidth	25 (O)	90	90	90
Panel H: *ln* Previous Firm Tenure				
RD Estimate	0.009	-0.021	-0.042	0.016
	[0.069]	[0.038]	[0.055]	[0.055]
Bandwidth	27 (O)	90	90	90
Panel I: *ln* Previous Experience				
RD Estimate	0.069	-0.007	0.009	0.000
	[0.056]	[0.031]	[0.013]	[0.003]
Bandwidth	29 (O)	90	90	90
Panel J: Share of Full Time				
RD Estimate	0.000	0.000	-0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	23 (O)	90	90	90
Panel K: Permanent Contract				
RD Estimate	0.016	-0.003	-0.001	-0.009
	[0.012]	[0.005]	[0.009]	[0.011]
Bandwidth	19 (O)	90	90	90
Panel L: Cov. Index - Predicted Time to Re-employment				
RD Estimate	1.118	0.173	-0.444	-1.800
	[1.337]	[0.707]	[1.123]	[1.291]
Bandwidth	25 (O)	90	90	90
Controls	Disc	Disc	Disc	Disc
Worker FE	No	No	No	Yes
Sample	Complete	Complete	2+ Obs	2+ Obs
N	130118	130118	45864	45348

Note: Table A.5 presents the balance test of a two-month extension of the potential duration of UI on different observed worker characteristics. Controls "Disc": Discontinuity fixed effects. Bandwidth: Indicates the length of the bandwidth. The symbol (O) indicates optimal bandwidth following Calonico et al. (2020). Sample "Complete": The complete sample is used in the estimation. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

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

	Panel A: Daily Wage change		
θ	0.000	0.001	0.001
	[0.003]	[0.002]	[0.002]
γ_0	0.025***	0.029***	0.031***
	[0.007]	[0.007]	[0.007]
γ_{Post}	-0.010*	-0.011**	-0.011**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
Start	-8	-8	-8
End	52	52	52
N	122387	122020	122020
	Panel B: Daily Wage change		
θ	-0.000	0.001	0.001
	[0.003]	[0.002]	[0.002]
γ_0	0.029***	0.030***	0.031***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.010**	-0.011***	-0.011**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
Start	-15	-15	-15
End	45	45	45
N	122387	122020	122020

Note: Table A.6 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. In both panels, workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. "Start" specifies the lowest value of time to re-employment relative to primary potential duration in which the variable E_0 takes the value one. "End" specifies the highest value of time to re-employment relative to primary potential duration in which the variable E_0 takes the value one. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.7: Effect of the exhaustion of UI benefit. All discontinuities

Panel A: Daily Wage change (First-year average re-employment wage)			
θ	0.000	0.001	0.000
	[0.002]	[0.002]	[0.002]
γ_0	0.031***	0.035***	0.038***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.002	-0.006	-0.008**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
N	128864	128480	128480
Panel B: Daily Wage change (First 5 years average re-employment wage)			
θ	0.001	0.002	0.001
	[0.002]	[0.002]	[0.002]
γ_0	0.022***	0.026***	0.029***
	[0.007]	[0.006]	[0.006]
γ_{Post}	-0.003	-0.007*	-0.009**
	[0.005]	[0.004]	[0.004]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
N	129285	128896	128896
Panel C: Hourly Wage change (First 5 years average re-employment wage)			
θ	-0.001	0.001	0.001
	[0.002]	[0.002]	[0.002]
γ_0	0.007	0.012**	0.013***
	[0.006]	[0.005]	[0.005]
γ_{Post}	-0.006	-0.008**	-0.009***
	[0.004]	[0.003]	[0.003]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
N	129285	128896	128896

Note: Table A.7 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs using longer-term definitions of re-employment wages. Panel (A) defines the re-employment job wage as the average daily wage during the first year after re-employment, while panel (B) defines the re-employment job wage as the average daily wage during the first five years after re-employment, and panel (C) uses hourly wages. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

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

Panel A: Daily Wage change			
θ	0.000	0.003	0.003
	[0.004]	[0.003]	[0.003]
γ_0	0.025**	0.037***	0.038***
	[0.012]	[0.010]	[0.010]
γ_{Post}	-0.002	-0.006	-0.008
	[0.009]	[0.008]	[0.008]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
RD Bandwidth	37	37	37
N	48923	48779	48779
Panel B: Hourly Wage change			
θ	0.000	0.003	0.003
	[0.003]	[0.003]	[0.003]
γ_0	0.014	0.018**	0.019**
	[0.010]	[0.008]	[0.008]
γ_{Post}	-0.010	-0.007	-0.007
	[0.007]	[0.006]	[0.006]
Controls	D	All	All
Time to Re-emp Ctrl	No	No	Yes
RD Bandwidth	37	37	37
N	48923	48779	48779

Note: Table A.8 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in Panel (A) is the change in daily wages, while in Panel (B), the outcome is the change in hourly wages. Workers are included in the treatment or control group if they are located within 37 days of one of the discontinuities that extend the potential duration of UI by two months. We chose 37 days because it represents the optimal bandwidth of the RD in equation (1) when using all control variables. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. RD Bandwidth: Indicates the maximum distance from the RD discontinuity to classify a worker as part of the treatment or control group. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.9: Effect of the exhaustion of UI benefit. All discontinuities

	Daily Wage change		Hourly Wage change	
θ	0.000	0.001	0.002	0.002
	[0.004]	[0.004]	[0.003]	[0.003]
γ_0	0.028**	0.026**	0.014*	0.013
	[0.012]	[0.011]	[0.009]	[0.009]
γ_{Post}	-0.001	-0.004	-0.009	-0.009
	[0.008]	[0.008]	[0.006]	[0.006]
Controls	All	All	All	All
Time to Re-emp Ctrl	No	Yes	No	Yes
N	121248	121248	121248	121248

Note: Table A.9 presents the RD estimates within the difference in differences empirical strategy that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) and (32) is the change in daily wages, while in columns (3) and (4), the outcome is the change in hourly wages. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.10: Effect of a Two-Month Extension of the Potential Duration of UI. All Discontinuities

	Panel A: Wage Change (First-year average re-employment wage)					
	Time to Re-employment relative to Primary PD					
	≤ 0	> 0 and ≤ 60	> 60	≤ 0	> 0 and ≤ 60	> 60
	Daily Wage Change					
RD Estimate	0.001	0.027**	-0.005			
	[0.005]	[0.013]	[0.011]			
Controls	All	All	All			
N	81961	11486	28573			

	Panel B: Wage Change (First 5 years average re-employment wage)					
	Time to Re-employment relative to Primary PD					
	≤ 0	> 0 and ≤ 60	> 60	≤ 0	> 0 and ≤ 60	> 60
	Daily Wage Change			Hourly Wage Change		
RD Estimate	0.002	0.018*	-0.013	0.003	0.012	-0.012
	[0.004]	[0.011]	[0.010]	[0.004]	[0.011]	[0.009]
Controls	All	All	All	All	All	All
N	86024	12045	30411	86166	12083	30647

Note: Table A.10 presents the estimation of the causal effect of a two-month extension of the potential duration of UI on the wage change for three different groups of workers. The outcome in columns (1) to (3) is the change in daily wages, while in columns (4) to (6), it is the change in hourly wages. In column (1), we restrict the sample to workers whose time to re-employment is shorter than their potential duration of UI if they are part of the control group or, if they are part of the treatment group, their time to re-employment is shorter than what would have been their potential duration of UI had they not crossed the discontinuity (i.e., time to re-employment relative to primary potential duration shorter than zero). In column (2), we restrict the sample to workers whose time to re-employment is between 1 day and 60 days longer than their potential duration of UI if they are part of the control group or, if they are part of the treatment group, their time to re-employment is shorter than their potential duration of UI (i.e., time to re-employment relative to primary potential duration is larger than zero but smaller or equal to 60). In column (3), we restrict the sample to workers whose time to re-employment is more than 60 days longer than their potential duration of UI if they are part of the control group or, if they are part of the treatment group, their time to re-employment is longer than their potential duration of UI (i.e., time to re-employment relative to primary potential duration larger than 60). Panel (A) defines the re-employment job wage as the average daily wage during the first year after re-employment, while panel (B) defines the re-employment job wage as the average daily/hourly wage during the first five years after re-employment. Controls "All": All controls included (including discontinuity fixed effects; see text). All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.11: Summary Statistics for Final Sample and Sample of Workers with 2 or more UI Claims

	Complete Sample	Sample with 2 or More UI Claims
Potential Benefit Duration	319.7	291.3
	(162.3)	(151.1)
Days Collecting UI	161.9	148.8
	(142.2)	(129.2)
Time to Re-employment (Days)	241.8	212.4
	(291.1)	(254.8)
Re-employment in 6 months	0.592	0.629
Re-employment in 12 months	0.799	0.837
Share Exhausting UI	0.220	0.216
Real Previous Daily Wage	49.85	49.78
	(18.52)	(17.91)
Real Previous Hourly Wage	6.231	6.223
	(2.315)	(2.239)
Previous (6 Years) Tenure	1049.3	964.9
	(480.2)	(446.6)
Age	34.63	35.65
	(7.024)	(6.643)
Share Male	0.633	0.707
Share College	0.272	0.222
Share High School	0.443	0.379
Wealth*	51.30	61.83
	(37.76)	(38.57)
Change Log Real Daily Wage	-0.038	-0.016
	(0.377)	(0.350)
Change Log Real Hourly Wage	0.029	0.029
	(0.317)	(0.306)
N	132129	43660

Note: Table A.11 presents the summary statistics of the final sample used in the paper (column (1)) and of the sample of workers with two or more UI claims (column (2)). Means and standard deviations (in parentheses) are shown. *Wealth is the discounted sum of real wages throughout the worker's working history.

Table A.12: Effect of a two-month extension of the potential duration of UI. All discontinuities

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	36.6***	40.3***	-0.007	-0.004	-0.003	-0.003
	[4.0]	[5.3]	[0.005]	[0.007]	[0.004]	[0.005]
Controls	All	All	All	All	All	All
Worker FE	No	Yes	No	Yes	No	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	45864	45348	43366	41283	43366	41283

Note: Table A.12 presents the estimation of the causal effect of a two-month extension of the potential duration of UI on the time to re-employment (columns (1) and (2)), on the wage change (columns (3) and (4)), and on the hourly wage change (columns (5) and (6)). The results in the table use a sample of workers for whom we observe two or more UI claims. Controls "All": All controls included (including discontinuity fixed effects; see text). Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. All specifications use a bandwidth of 90 days. All estimates are conventional estimates as shown in Calonico et al. (2014) using a triangular kernel. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01

Table A.13: Effect of the exhaustion of UI benefit. All discontinuities

	Daily Wage change				Hourly Wage change			
θ	-0.003	-0.003	-0.002	-0.002	-0.003	-0.003	-0.002	-0.002
	[0.004]	[0.003]	[0.004]	[0.004]	[0.003]	[0.003]	[0.004]	[0.004]
γ_0	0.025***	0.029***	0.024**	0.025**	0.010	0.013*	0.010	0.012
	[0.010]	[0.009]	[0.012]	[0.012]	[0.008]	[0.008]	[0.009]	[0.009]
γ_{Post}	-0.003	-0.004	0.009	0.008	-0.007	-0.007	0.003	0.003
	[0.007]	[0.007]	[0.009]	[0.009]	[0.006]	[0.006]	[0.007]	[0.007]
Controls	All	All	All	All	All	All	All	All
Time to Re-emp Ctrl	No	Yes	No	Yes	No	Yes	No	Yes
Worker FE	No	No	Yes	Yes	No	No	Yes	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	43659	43659	41775	41775	43659	43659	41775	41775

Note: Table A.13 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) to (4) is the change in daily wages, while in columns (5) to (8) the outcome is the change in hourly wages. The results in the table use a sample of workers for whom we observe two or more UI claims. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control group for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Control "All": All controls included (including discontinuity fixed effects; see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.14: Effect of the exhaustion of UI benefit. All discontinuities

Panel A: Daily Wage change		
θ	-0.001	-0.001
	[0.010]	[0.010]
γ_0	0.051*	0.051*
	[0.027]	[0.027]
γ_{Post}	0.007	0.002
	[0.024]	[0.024]
Controls	All	All
Time to Re-emp Ctrl	No	Yes
Worker FE	Yes	Yes
Sample	2+ Obs	2+ Obs
RD Bandwidth	37	37
N	7843	7798
Panel B: Hourly Wage change		
θ	-0.003	-0.004
	[0.008]	[0.009]
γ_0	0.039*	0.040*
	[0.024]	[0.024]
γ_{Post}	-0.015	-0.016
	[0.019]	[0.019]
Controls	All	All
Time to Re-emp Ctrl	No	Yes
Worker FE	Yes	Yes
Sample	2+ Obs	2+ Obs
RD Bandwidth	37	37
N	7843	7798

Note: Table A.14 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in Panel (A) is the change in daily wages, while in Panel (B), the outcome is the change in hourly wages. Workers are included in the treatment or control group if they are located within 37 days of one of the discontinuities that extend the potential duration of UI by two months. We chose 37 days because it represents the optimal bandwidth of the RD in equation (1) when using all control variables. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. RD Bandwidth: Indicates the maximum distance from the RD discontinuity to classify a worker as part of the treatment or control group. p-value: * 0.10 ** 0.05, *** 0.01.

Table A.15: Deterioration rate of labor market opportunities estimates (LMOS) vs IV estimates from Schmieder and von Wachter (2016)

	Daily Wage Change		Hourly Wage Change	
LMOS	-0.0066	-0.0043	-0.0030	-0.0026
90% CI	[-0.0148, 0.0024]	[-0.0142, 0.0057]	[-0.0095, 0.0045]	[-0.0100, 0.0053]
LMOS (Ignoring $\mathbf{o}(\beta)$)	-0.0080	-0.0054	-0.0036	-0.0030
90% CI	[-0.0161, 0.0009]	[-0.0151, 0.0044]	[-0.0101, 0.0040]	[-0.0108, 0.0052]
IV (Schmieder et al. (2016))	-0.0061	-0.0039	-0.0028	-0.0025
90% CI	[-0.0142, 0.0032]	[-0.0140, 0.0062]	[-0.0095, 0.0047]	[-0.0099, 0.0055]
Ratio LMOS vs. IV	108%	110%	107%	104%
Controls	All	All	All	All
Worker FE	No	Yes	No	Yes
Sample	2+ Obs	2+ Obs	2+ Obs	2+ Obs
N	43366	41283	43366	41283

Note: Table A.15 presents the estimated (monthly) deterioration rate of labor market opportunities in daily re-employment wage (columns (1) and (2)) and in hourly re-employment wage (columns (3) and (4)) (row 1) and compares it with an estimate of the deterioration rate of labor market opportunities that ignores the anticipation effects (row 2) and to the IV estimate suggested in Schmieder and von Wachter (2016) (row 3). The sample is that of workers with two or more UI claims. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Worker FE: Whether the estimation of all moments used to calculate the LMOS includes individual fixed effects or not. All estimates are constructed from conventional RD estimates as shown in Calonico et al. (2014)) using a triangular kernel. Bootstrapped 90 percent confidence intervals (500 bootstraps) in brackets. The sample is bootstrapped at the unemployment spell level. p-value: * 0.10 ** 0.05, *** 0.01.

B Data-Cleaning Process & Sample Restrictions

Using the historical records of the MCVL, we build a sample of displacements (i.e., unemployment insurance claims preceded by a working spell) for workers ages 25 to 50 between 1994 and 2016. We impose several restrictions when constructing our sample. We limit our sample to workers who are re-employed within 5 years of losing their job. Additionally, we eliminate individuals who have been self-employed at some point in the 6 years prior to unemployment or who exit unemployment into self-employment. Furthermore, we discard any individual who presents negative wages.[43] We also discard those who simultaneously work and collect unemployment benefits, something that was possible at certain points due to very specific programs implemented by Social Security.

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

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

We exclude all the unemployment spells of workers whose tenure in the previous 6 years is less than 450 days or more than 2,070 days. By doing this, we do not take advantage of two of the policy discontinuities (359-360 days and 2,159-2,160 days) available in our data. We avoid the former discontinuity because, during our sample period, there were several changes to the rules governing the subsidies for those without enough tenure to qualify for unemployment benefits, which would complicate the analysis even further. In the case of the latter, the policy schedule dictates that only tenure in the previous 6 years should be considered, which creates a mechanical bunching of workers to the right of the discontinuity. Extending the window where we count the previous tenure as seven years would solve the mechanical bunching but at the cost of misclassification of workers across the discontinuities.

We remove workers entering unemployment after exhausting the predetermined length of certain temporary contracts.[44] We impose this restriction for two reasons. First, these workers are aware of the expiration date of their contracts and are more likely to start searching for new jobs before their previous employment spell finishes. Second, workers exhausting temporary contracts have a much higher probability of having a previous tenure of exactly half-year, one year, etc. While this is not a problem in itself, when combined with the UI schedule in Spain, this results in these workers usually being located just to the right of our discontinuities of interest and receiving an additional two months of potential duration of UI.[45] As shown in Figure A.1 (b), keeping these workers in our sample would create manipulation in the running variable. Removing these workers results in a much smoother distribution of the running variable, as shown in Figure A.2 (b).

To reach our final sample, we make one additional sample restriction. We eliminate

[44]We remove unemployment spells of temporary workers whose latest contract lasted exactly one of the following durations: 15 days, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 11 months, 1 year, 1 year and 3, 6, 9, or 11 months, 2 years, 2 years and 3, 6, or 11 months, 3 years, 3 years and 6 months, 4 years, 4 years and 6 months, 5 years, and 5 years and 6 months.

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

workers working 35 hours per week or more in the pre-displacement job during the period used to determine the potential duration of UI. Since we do not observe hours worked per week but instead a part-time coefficient for each job, we focus on workers whose previous job had a part-time coefficient larger than 0.88 during the period used to determine the potential duration of UI. We implement this restriction because 40 percent of unemployment spells of workers with a part-time coefficient below 0.88 do not have an associated previous wage. This is drastically different in the case of full-time workers, where less than 1 percent of unemployment spells do not have information on previous wages. We additionally discard UI claims from workers who, upon re-employment, have a job with a part-time coefficient smaller than 0.25 (equivalent to 10 or fewer hours per week on average). Note that this restriction allows workers to work less than 10 hours per week, but conditional on working on a given day, the worker needs to work for at least 2 hours during that day. This last restriction only affects 800 unemployment spells in our sample. Note that the restrictions on part-time workers do not affect our main results, as shown in Appendix C.

Tables B.1 and B.2 summarize the effects of these restrictions on the size and representativeness of our sample. Carryover claims from workers without enough previous experience (between UI claims) to generate a new UI claim represent the bulk of our original sample. 51 percent of our original sample corresponds to these types of claims. Within the set of spells that could generate a new UI claim (i.e., spells with more than 360 days of previous working experience in the last 6 years), only 7 percent of unemployment spells use a carryover claim. All in all, removing both types of carry-over claims reduces our sample size by 54 percent.

Focusing on new UI claims, the restriction on previous experience to workers above 450 days and below 2070 days removes an additional 36 percent of unemployment spells. The vast majority of these spells correspond to workers whose previous experience is well above 6 years, which qualifies them for the maximum UI benefit duration. Compared to our final sample, this group has a significantly larger maximum UI

potential duration and, on average, collects UI benefits for 60 more days and remains non-employed for 70 more days. They were older, richer, and had better-paying jobs before the unemployment spell.

Our two final restrictions, the removal of workers exhausting the length of a temporary contract and part-time workers, eliminate from the sample workers who were more likely to be younger, female, and in worse-paying jobs prior to entering unemployment. Compared to our final sample, they tend to have a shorter potential duration of UI benefits but collect those benefits for a similar number of days and stay non-employed for longer. The former restriction reduces the sample by an additional 22 percent, while the latter further decreases it by an extra 15 percent.

Our final sample contains over 132,000 unemployment spells corresponding to 106,000 different workers.

Table B.1: Summary Statistics
From Original Sample to Final Sample

	Original Sample	Minus Carryovers Unqualified for New Claim	Minus All Carryovers	Minus Tenure Restrictions	Minus Spells Exhausting Predet. Length Contracts	Minus Part-Time (Final Sample)
Potential Benefit Duration	303.4*	353.3**	364.8	313.1	317.0	319.7
	(242.8)	(225.8)	(227.3)	(159.2)	(161.7)	(162.3)
Days Collecting UI	164.4	179.9	181.5	161.2	161.7	161.9
	(168.6)	(174.9)	(176.4)	(139.2)	(141.8)	(142.2)
Time to Re-employment (Days)	243.0	266.7	270.3	247.5	244.8	241.8
	(302.6)	(314.3)	(316.8)	(295.5)	(295.1)	(291.1)
Re-employment in 6 months	0.612	0.546	0.559	0.582	0.590	0.592
Re-employment in 12 months	0.796	0.769	0.765	0.793	0.796	0.799
Share Exhausting UI	-	-	0.254	0.232	0.225	0.220
Real Previous Daily Wage	49.74	50.12	49.98	48.22	48.61	49.85
	(19.40)	(19.72)	(19.71)	(18.27)	(18.52)	(18.52)
Real Previous Hourly Wage	6.218	6.265	6.247	6.027	6.077	6.231
	(2.424)	(2.465)	(2.464)	(2.284)	(2.315)	(2.315)
Previous (6 Years) Tenure	1014.74	1131.37	1166.0	1027.2	1041.5	1049.3
	(686.34)	(672.5)	(676.2)	(471.5)	(478.4)	(480.2)
Age	35.58	35.31	35.20	34.34	34.53	34.63
	(7.13)	(7.101)	(7.100)	(7.012)	(7.016)	(7.024)
Share Male	0.591	0.598	0.591	0.586	0.602	0.633
Share College	0.267	0.272	0.274	0.283	0.281	0.272
Share High School	0.443	0.452	0.455	0.460	0.456	0.443
Wealth***	62.84	61.72	59.78	48.56	49.60	51.30
	(48.39)	(48.98)	(48.18)	(36.80)	(37.51)	(37.76)
Change Log Real Daily Wage	-0.050	-0.066	-0.067	-0.042	-0.044	-0.038
	(0.420)	(0.428)	(0.429)	(0.421)	(0.424)	(0.377)
Change Log Real Hourly Wage	0.045	0.033	0.034	0.058	0.057	0.029
	(0.336)	(0.339)	(0.339)	(0.331)	(0.333)	(0.317)
N	676594	334604	311139	198811	155253	132129

Note: Table B.1 presents the summary statistics of the original sample and each of the sub-samples after each main restriction imposed during the construction of our final sample. Each column displays the summary statistics for all the unemployment spells satisfying the condition described at the top. Means and standard deviations (in parentheses) are shown.* Workers without enough experience to create a new claim using a carryover claim are assigned a potential benefit duration of zero. ** Workers with enough experience to create a new UI claim but instead using a carryover claim are assigned the potential duration they would have received if they had created a new claim. *** Wealth is the discounted sum of real wages throughout the worker's working history.

Table B.2: Summary Statistics for each Subsample Exclude and Final Sample

	Carryovers Unqualified for New Claim	Carryovers Qualified for New Claim	Experience Above 2070 or Below 450	Exhausting Predetermined Length Contracts	Part-Time	Final Sample
Potential Benefit Duration	-	183.7*	454.7	299.0	292.0	319.7
	(-)	(101.8)	(291.0)	(149.0)	(154.0)	(162.3)
Days Collecting UI	70.25	156.8	217.0	159.1	160.6	161.9
	(70.54)	(149.5)	(222.6)	(129.2)	(138.5)	(142.2)
Time to Re-employment (Days)	99.03	214.7	309.9	257.1	273.2	241.8
	(153.5)	(271.1)	(347.1)	(296.7)	(329.5)	(291.1)
Re-employment in 6 months	0.901	0.639	0.519	0.555	0.568	0.592
Re-employment in 12 months	0.957	0.824	0.717	0.783	0.764	0.799
Share Exhausting UI	-	-	0.292	0.255	0.271	0.220
Real Previous Daily Wage	47.41	52.23	53.04	46.79	36.89	49.85
	(17.09)	(19.77)	(21.65)	(17.27)	(13.87)	(18.52)
Real Previous Hourly Wage	5.926	6.529	6.629	5.849	4.612	6.231
	(2.136)	(2.472)	(2.706)	(2.158)	(1.734)	(2.315)
Previous (6 Years) Tenure	306.0	621.0	1407.3	976.1	967.4	1049.3
	(25.15)	(315.2)	(879.7)	(442.3)	(454.7)	(480.2)
Age	37.20	37.04	36.69	33.66	33.58	34.63
	(7.099)	(6.884)	(7.005)	(6.954)	(6.869)	(7.024)
Share Male	0.543	0.703	0.601	0.528	0.309	0.633
Share College	0.239	0.243	0.258	0.289	0.372	0.272
Share High School	0.392	0.410	0.445	0.476	0.575	0.443
Wealth**	69.60	90.20	79.26	44.87	33.48	51.30
	(44.04)	(51.83)	(58.34)	(33.90)	(30.65)	(37.76)
Change Log Real Daily Wage	0.047	-0.062	-0.110	-0.036	-0.092	-0.038
	(0.348)	(0.423)	(0.439)	(0.411)	(0.731)	(0.377)
Change Log Real Hourly Wage	0.122	0.0152	-0.009	0.064	0.320	0.029
	(0.306)	(0.341)	(0.350)	(0.322)	(0.369)	(0.317)
N	341990	23799	112328	43558	23124	132129

Note: Table B.2 presents the summary statistics of each subsample excluded in our data cleaning process (columns (1) to (5)) and for the final sample used in the paper (column (6)). Each column displays the summary statistics for all the unemployment spells satisfying the condition described at the top. Means and standard deviations (in parentheses) are shown. * The potential benefit duration that workers in these unemployment spells would have had if they had created a new claim instead of using a carryover claim. **Wealth is the discounted sum of real wages throughout the worker's working history.

C Main Results: Full Sample + Part-Time Workers

As described in Section 2, we restrict our sample to unemployment spells of workers working 35 or more hours per week. We impose this restriction because of the significant number of unemployment spells of part-time workers with missing information on pre-displacement daily wages. Additionally, as we show below, we find some mild evidence of potential selection within the sample of part-time workers, which could put at risk our empirical strategy. Nevertheless, this section replicates the main results of the paper in a sample that makes no restrictions on workers entering unemployment from part-time jobs. While we describe the results in detail below, the main takeaway is that our choice of removing the unemployment spells of part-time workers does not affect the results in the paper.

Using this sample, Table C.1 shows that the response of time to re-employment to a 2-month extension of the potential duration is approximately 26 days. This estimate is extremely similar to the estimate in our final sample, which is 28 days. Similarly, the response of daily and hourly wages to an extension of the potential duration of UI is estimated to be slightly negative and insignificant, at -0.5 percent and -0.2 percent, respectively. Both estimates are almost identical to our original ones.

Table C.2 shows the balance test for this sample. While the vast majority of our estimates align with those in the main text, we observe some mild evidence of selection in our results including worker fixed effects. As shown in column (3), unemployment spells to the right of the discontinuities, and thus granted an additional 2 months of potential duration, are more likely to be full-time jobs before entering unemployment. Consequently, spells to the right of the discontinuity have a slightly lower predicted time to re-employment.

Table C.3 shows the effect of an extension of the potential duration of UI on re-employment wages throughout the distribution of time to re-employment relative to the primary potential duration. As in our main results, we find that an extension of the UI potential duration increases re-employment wages just during the two months in which, in its absence, the workers would have exhausted the UI benefits. Specifically,

in this time frame, re-employment wages in the treatment group are 4.3 percent higher than in the control group. This estimate is very similar to the one in the main text (3.5 percent). In this sample, we still find that the effects of the exhaustion of UI benefits on daily wages are primarily driven by changes in hours worked. Treated workers finding a job in the time frame in which they would have already exhausted the benefits had they not been granted an additional 2-month potential duration see hourly wages that are just 1.2 percent higher than workers from the control group finding a job in the same time frame (i.e., in the first two months after the expiration of their UI benefits). This result is almost identical to the one in the main text (1.4 percent).

Given the previous results, it is no surprise that we find almost identical estimates of duration dependence in this sample compared to our main sample. As in the main sample, we observe important differences in our estimates of duration dependence in re-employment wages depending on whether we consider daily or hourly wages. This is shown in Table C.4. Within this sample, the LMOS estimate in daily (hourly) wages is -0.76 (-0.23) percent per month of non-employment. Both estimates are identical to the ones we recover in the main text (-0.75 and -0.23 percent, respectively). Finally, Table C.5 presents equivalent estimates to those in Table C.4 but includes individual fixed effects in the estimation. Controlling by permanent unobserved heterogeneity reduces the LMOS estimate in daily wages from 0.76 percent to 0.52 percent per month. This is almost identical to the decline we observe in our main sample (from 0.75 to 0.43 percent per month). Finally, controlling by permanent unobserved heterogeneity does not affect the results for hourly wages. This is an identical conclusion to that from the main sample.

Table C.1: Effect of a two-month extension of the potential duration of UI. All discontinuities
Final sample + part-time workers

	Time to Re-employment		Daily Wage Change		Hourly Wage Change	
RD Estimate	26.010***	37.693***	-0.005	-0.006	-0.002	-0.004
	[3.282]	[5.082]	[0.005]	[0.007]	[0.003]	[0.005]
Controls	All	All	All	All	All	All
Sample	All	2+ Obs	All	2+ Obs	All	2+ Obs
Worker FE	No	Yes	No	Yes	No	Yes
N	143988	51380	134882	46681	134882	46681

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

Table C.2: Balance Test. All discontinuities
Final Sample + part-time workers

Panel A: Age			
RD Estimate	-0.059	-0.005	0.054
	[0.126]	[0.079]	[0.121]
Bandwidth	(O)	90	90
Panel B: Male			
RD Estimate	-0.001	-0.008	-
	[0.012]	[0.006]	[.]
Bandwidth	(O)	90	-
Panel C: High School			
RD Estimate	-0.011	0.001	-
	[0.012]	[0.006]	[.]
Bandwidth	(O)	90	-
Panel D: College			
RD Estimate	-0.015	0.001	-
	[0.010]	[0.005]	[.]
Bandwidth	19 (O)	90	-
Panel E: *ln* Wealth			
RD Estimate	0.017	0.020**	-0.001
	[0.016]	[0.009]	[0.014]
Bandwidth	(O)	90	90
Panel F: *ln* Previous Daily Wage			
RD Estimate	-0.004	0.004	0.013*
	[0.008]	[0.004]	[0.007]
Bandwidth	(O)	90	90
Panel G: *ln* Previous Hourly Wage			
RD Estimate	-0.004	0.004	0.013*
	[0.008]	[0.004]	[0.007]
Bandwidth	(O)	90	90
Panel H: *ln* Previous Firm Tenure			
RD Estimate	-0.022	-0.026	0.019
	[0.065]	[0.035]	[0.050]
Bandwidth	(O)	90	90
Panel I: *ln* Previous Experience			
RD Estimate	0.046	-0.010	-0.001
	[0.056]	[0.030]	[0.003]
Bandwidth	(O)	90	90
Panel J: Share of Full Time			
RD Estimate	0.003	0.002	0.007**
	[0.004]	[0.002]	[0.003]
Bandwidth	(O)	90	90
Panel K: Permanent Contract			
RD Estimate	0.009	-0.002	-0.014
	[0.011]	[0.005]	[0.010]
Bandwidth	(O)	90	90
Panel L: Cov. Index - Predicted Time to Re-employment			
RD Estimate	0.077	0.338	-3.029**
	[1.156]	[0.689]	[1.251]
Bandwidth	25 (O)	90	90
Controls	Disc	Disc	Disc
Worker FE	No	No	Yes
Sample	Complete	Complete	2+ Obs
N	143988	143988	51400

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

Table C.3: Effect of the exhaustion of UI benefit. All discontinuities
Final sample + part-time workers

	Daily Wage change				Hourly Wage change			
θ	0.001	-0.000	-0.003	-0.003	0.001	0.000	-0.001	-0.001
	[0.003]	[0.003]	[0.005]	[0.005]	[0.002]	[0.002]	[0.003]	[0.003]
γ_0	0.040***	0.043***	0.025**	0.028**	0.011**	0.012**	0.009	0.010
	[0.007]	[0.007]	[0.013]	[0.013]	[0.005]	[0.005]	[0.009]	[0.009]
γ_{Post}	-0.010*	-0.011**	0.021**	0.019**	-0.008**	-0.009***	0.009	0.009
	[0.005]	[0.005]	[0.010]	[0.010]				
	[0.003]	[0.003]	[0.007]	[0.007]				
Controls	All	All	All	All	All	All	All	All
Time to Re-emp Ctrl	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	2+ Obs	2+ Obs	All	All	2+ Obs	2+ Obs
Worker FE	No	No	Yes	Yes	No	No	Yes	Yes
N	134880	134880	47208	47208	134880	134880	47208	47208

Note: Table C.3 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs. The outcome variable in columns (1) to (4) is the change in daily wages, while in columns (5) to (8), the outcome is the change in hourly wages. The sample includes all unemployment spells in our final sample plus all unemployment spells of workers displaced from part-time jobs. Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. θ captures the difference in wage changes between the treatment and control groups for all workers whose time to re-employment relative to primary potential duration is smaller than or equal to zero (see text). γ_0 captures the difference in wage changes between the treatment and control group for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text). γ_{Post} captures the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than 60 (see text). Controls "Disc": Discontinuity fixed effects. Control "All": All controls included (including discontinuity fixed effects, see text). Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Sample "2 + obs": The sub-sample of workers with two or more UI claims is used in the estimation. Worker FE indicates whether the estimation includes individual fixed effects. Robust standard errors in brackets. p-value: * 0.10 ** 0.05, *** 0.01.

Table C.4: Duration Dependence (LMOS) vs
IV Estimates from Schmieder and von Wachter (2016)
Final Sample + part-time workers

	Daily Wage Change	Hourly Wage Change
LMOS	-0.0076	-0.0023
90% CI	[-0.0167,0.0017]	[-0.0082,0.0036]
LMOS (Ignoring Anticipation Effect)	-0.0096*	-0.0028
90% CI	[-0.0188,-0.0002]	[-0.0089,0.0031]
IV (Schmieder and von Wachter (2016))	-0.0061	-0.0018
90% CI	[-0.0147,0.0033]	[-0.0076,0.0039]
Ratio LMOS vs. IV	125%	128%
Controls	All	All
N	134882	134882

Note: Table C.4 presents in row (1) the estimated (monthly) duration dependence (LMOS estimator) in daily re-employment wage (column (1)) and in hourly re-employment wage (column (2)). In row (2), we show an estimate of duration dependence (LMOS estimator without anticipation effects) that ignores the anticipation effects. Row (3) shows the IV estimate proposed in Schmieder and von Wachter (2016). The sample includes all unemployment spells in our final sample plus all unemployment spells of workers displaced from part-time jobs. Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Bootstrapped 90 percent confidence intervals in brackets. The sample is bootstrapped at the spell level. p-value: * 0.10 ** 0.05, *** 0.01.

Table C.5: Deterioration rate of labor market opportunities estimates (LMOS) vs
IV estimates from Schmieder and von Wachter (2016)
Final Sample + part-time workers

	Daily Wage Change	Hourly Wage Change
LMOS	-0.0052	-0.0034
90% CI	[-0.0171,0.0062]	[-0.0136,0.0043]
LMOS (Ignoring $\mathbf{o}(\beta)$)	-0.0067	-0.0040
90% CI	[-0.0187,0.0047]	[-0.0140,0.0037]
IV (Schmieder et al. (2016))	-0.0048	-0.0033
90% CI	[[-0.0168,0.0067]	[-0.0133,0.0045]
Ratio LMOS vs. IV	108%	104%
Controls	All	All
Worker FE	Yes	Yes
Sample	2+ Obs	2+ Obs
N	46681	46681

Note: Table C.5 presents the estimated (monthly) deterioration rate of labor market opportunities in daily re-employment wage (column (1)) and in hourly re-employment wage (column (2)) (row 1) and compares it with an estimate of the deterioration rate of labor market opportunities that ignores the anticipation effects (row 2) and to the IV estimate suggested in Schmieder and von Wachter (2016) (row 3). The sample includes all unemployment spells in our final sample plus all unemployment spells of workers displaced from part-time jobs. The sample is that of workers with two or more UI claims. Controls "Disc": Discontinuity fixed effects. Controls "All": All controls are included in both RD and DD specifications (including discontinuity fixed effects; see text). Worker FE: Whether the estimation of all moments used to calculate the LMOS includes individual fixed effects or not. All estimates are constructed from conventional RD estimates as shown in Calonico et al. (2014)) using a triangular kernel. Bootstrapped 90 percent confidence intervals in brackets. The sample is bootstrapped at the spell level. p-value: * 0.10 ** 0.05, *** 0.01.

D Theoretical Framework

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

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

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

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

$$\mathcal{V}_t(w) = 1/(1 - \beta)u(w) \tag{D.2}$$

subject to:

$$\mathcal{F}(w, \lambda, e) = y(t) \text{ or } \lambda = \Lambda(w, e, y(t)) \tag{D.3}$$

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

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

This expression of the search technology function in equation (D.3) is very general and includes those derived from directed search models[46] with matching technology (e.g., telephone matching, urn-ball matching, and Cobb-Douglas matching) or from

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

a random search model.[47]. Notice that productivity is decreasing over the duration of non-employment, generating a source for duration dependence. Moreover, we also derive an implicit function Λ from $\mathcal{F}$. This Λ specifies a corresponding job-finding rate, given the wage choice, search effort, and worker's productivity. We use Λ and $\mathcal{F}$ interchangeably to facilitate our exposition.

Taking the first-order condition with respect to w:

$$V(w^*) - U_t = \frac{\lambda}{-\frac{\partial \lambda}{\partial w}} \frac{\partial V}{\partial w} \tag{D.4}$$

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

Using equation (D.4), it is possible to express the optimal wage iteratively:

$$u(w^*) = \underbrace{(1-\beta)\sum_{\tau \geq t} \beta^{\tau - t} \left(u(b_\tau) + \ell\right)}_{\text{Value of UI and leisure}} + \underbrace{(1-\beta)\left(\rho_t \frac{\partial V_t}{\partial w} - \beta \mathcal{C}_t + \sum_{\tau \geq t+1} \beta^{\tau - t + 1}(\lambda_\tau \rho_\tau \frac{\partial V_\tau}{\partial w} - \beta \mathcal{C}_\tau)\right)}_{\text{Option value of searching}} \tag{D.5}$$

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

Let us now discuss two special cases. First, when β goes to zero, workers become

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

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

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

$$ln(w^*(t)) = (1-\beta)\left(u(b_t) + \sum_{j=1}^{\infty} \beta^j \left(u(b_{t+j}) - e_{t+j}\right)\right) + \rho(t) + (1-\beta)\cdot\beta \sum_{j=1}^{\infty} \delta^j \lambda_{t+j}\rho(t+j) \tag{D.6}$$

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

D.2 LMOS estimator with Heterogeneous Job Seekers

This section discusses the applicability of our duration dependence estimator in a setting in which unemployed workers are heterogeneous. We use a simple two-type model to show that our method applies to a set-up in which workers are heterogeneous, despite local interpretation of our estimate of the decline in wages at the point of exhaustion of UI benefits.

Define $i = H$ and $i = L$ to be indices for two types of workers, with probability p_H and p_L. These types of workers could differ across multiple dimensions (human capital, risk aversion, leisure preferences, etc.), but, for simplicity, suppose they share the same duration dependence ρ_h. By definition:

$$\frac{\partial Eln(w)}{\partial B} = p_H \cdot \frac{\partial Eln(w|H)}{\partial B} + p_L \cdot \frac{\partial Eln(w|L)}{\partial B} \tag{D.7}$$

Applying equation (7) respectively to type H and L, and plugging-in equation (D.7), we reach:

$$\frac{\partial Eln(w)}{\partial B} = p_H \cdot [\rho_h \frac{dED^*_{|H}}{dB} + \sum_{t \leq B} \frac{\partial g(B,t,\theta_H)}{\partial B} \text{pr}(t|H)] + p_L \cdot [\rho_h \frac{dED^*_{|L}}{dB} + \sum_{t \leq B} \frac{\partial g(B,t,\theta_L)}{\partial B} \text{pr}(t|L)] \tag{D.8}$$
$$+ p_H [\sum_{t<B} \frac{\partial \text{pr}(t|H)}{\partial B} (g(B,t,\theta_H) - g^*_H)] + p_L [\sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B} (g(B,t,\theta_L) - g^*_L)]$$

Similarly, by definition of D^*, $E(g(B,t,\theta))$ and $\text{pr}(t)$:

$$\frac{\partial ED^*}{\partial B} \equiv p_H \frac{\partial ED^*_{|H}}{\partial B} + p_L \frac{\partial ED^*_{|L}}{\partial B} \tag{D.9}$$

$$\frac{\partial E(g(B,t,\theta))}{\partial B} \equiv [p_H \text{pr}(t|H) \frac{\partial E(g(B,t,\theta_H))}{\partial B} + p_L \text{pr}(t|L) \frac{\partial E(g(B,t,\theta_L))}{\partial B}] / (p_H \text{pr}(t|H) + p_L \text{pr}(t|L)) \tag{D.10}$$

$$\text{pr}(t) \equiv p_H \text{pr}(t|H) + p_L \text{pr}(t|L) \tag{D.11}$$

When $t = B$, $\frac{\partial E(g(B,t,\theta))}{\partial B}$ represents the response of the wage to an extension of the UI potential duration. It is a local effect for workers finding a job at the point of

exhaustion of UI benefits. Therefore, workers who are more likely to find a job around this point will dominate the estimate of the response of wages to the exhaustion of UI.

Redefine the term $E_{\frac{\partial \text{pr}(t|i)}{\partial B}}(g((B,t,\theta_i)-g_i^*)$, which is the decline in wages weighted by the job searching elasticities, as:

$$E_{\frac{\partial \text{pr}(t|i)}{\partial B}}(g((B,t,\theta_i)-g_i^*) \equiv \frac{p_H \frac{\partial \text{pr}(t|H)}{\partial B}(g(B,t,\theta_H)-g_H^*)+p_L \frac{\partial \text{pr}(t|L)}{\partial B}(g(B,t,\theta_L)-g_L^*)}{p_H \frac{\partial \text{pr}(t|H)}{\partial B}+p_L \frac{\partial \text{pr}(t|L)}{\partial B}} \tag{D.12}$$

Finally, combining the definitions above with equation (D.8):

$$\frac{\partial Eln(w)}{\partial B} = \rho_h \frac{\partial ED^*}{\partial B} + \sum_{t \leq B} \text{pr}(t) \cdot \frac{\partial E(g(B,t,\theta))}{\partial B} + \sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B} \cdot E_{\frac{\partial \text{pr}(t|i)}{\partial B}}(g((B,t,\theta_i)-g_i^*) \tag{D.13}$$

Therefore, our identification of the LMOS applies to a case with heterogeneous unemployed workers.

Moreover, even if ρ_h varies across types of workers, our LMOS estimator still recovers duration dependence but with a different interpretation. Similar to Schmieder et al. (2016), we interpret it as a weighted average of the different type's duration dependence, where the weight is in proportion to the initial type density and the duration effect $\frac{\partial D^*_{|i}}{\partial B}$.

D.3 Equivalence between the Decline in Wages at the Exhaustion of UI Benefits and the Wage Response to an Extension of the UI Potential Duration

Our goal is to show that the decline in wages at the point of exhaustion of UI benefits is equivalent to the elasticity of the wage choice to an extension of the UI potential duration. This equivalence is similar to that in Krueger et al. (2014), but instead of considering a permanent removal of UI benefits, we consider an extension of the UI potential duration.[48]

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

$$\frac{\partial g(w_{B+1})}{\partial b_{B+1}} \equiv ln(w^*_{B+1})|(B+1) - ln(w^*_{B+1})|(B) \tag{D.14}$$

Based on equation (D.6), we can express the above equation as:

$$\frac{\partial g(w_{B+1})}{\partial b_{B+1}} = (1-\beta)(u(\bar{b}) - u(\underline{b})) \tag{D.15}$$

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

$$ln(w^*_B)|(B) - ln(w^*_{B+1})|(B) = (1-\beta)(u(\bar{b}) - u(\underline{b})) \tag{D.16}$$

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

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

The first piece in equation (D.16) is of first-order importance because it captures

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

the abrupt change in the value of the outside option. On the other hand, the relevance of the second piece ("Duration Dependence net of Changes in UI Benefits") will be significantly more limited. This occurs because the time frame of the decline can be very small (think about the wage right before and right after the UI exhaustion), pushing the effect coming from duration dependence toward zero.[49]

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

$$\underbrace{\frac{\partial g(w_{B+1})}{\partial b_{B+1}}}_{\text{Binding wage}} \underset{\Delta t \to 0}{\to} \underbrace{ln(w_B^*)|(B) - ln(w_{B+1}^*)|(B)}_{\text{Wage drop at UI exhaustion}} \tag{D.17}$$

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

E Approximating $g(B,t,\theta) - g^*$ and assessing whether it is constant for all t smaller than B.

Estimating duration dependence ($LMOS$ estimator) requires us to estimate $\sum_{t<B} \frac{\partial \text{pr}(t)}{\partial B}(g(B,t,\theta) - g^*)$, the second piece of the anticipation effect. Assuming that $g(B,t,\theta) - g^*$ is constant for all t smaller than B, our premise in the body of the paper, allows us to express our term of interest as: $(g(B,t,\theta) - g^*) \cdot \frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$. In the main text, we provide a causal estimate for $\frac{\partial \text{pr}(D^* < B\tau)}{\partial B}$. After this, all that's left is to provide an estimate for $g(B,t,\theta) - g^*$ and to assess whether assuming that $g(B,t,\theta) - g^*$ is constant for all t smaller than B is reasonable. In this appendix, we start by discussing the quality of our estimate for $g(B,t,\theta) - g^*$, based on previous findings in the literature and

[49] Even if the time frame is longer, it is possible to extract the effect coming from duration dependence from the left-hand side of the equation (D.16), provided that duration dependence is a smooth process throughout time in non-employment

additional sources of exogenous variation in our data. From there, we move to test and validate the assumption that $g(B,t,\theta) - g^*$ is constant for all t smaller than B.

E.1 The quality of our approximation for $g(B,t,\theta) - g^*$

As mentioned in the main text, our preferred approximation of $g(B,t,\theta) - g^*$ comes from the causal effect of the exhaustion of the UI benefits on wage losses. From Table 2, this estimate is 3.5 percent. The assumption behind this approximation is that, in the absence of UI benefits, the increase in wage losses that we would observe in all periods $t < B$ would be identical to the one we estimate at the point of exhaustion of benefits. Our approximation is consistent with the findings of Kim et al. (2024) for the US for a similar period of time. Kim et al. (2024) report that the absence of UI benefits reduces reservation wages by an average of 3.9 percent, with a range between 2.5 and 4.3 percent depending on whether the unemployment rate is higher or lower. Similarly, this approximation is very similar to the point estimate reported in Krueger and Mueller (2016) for New Jersey for 2009. They find that the absence of UI benefits is associated with a decline in reservation wages of 4.7 percent, even if their limited sample size makes this estimate insignificant.

Our data also allows us to provide an additional estimate of $g(B,t,\theta) - g^*$. To do this, we use exogenous variation in the replacement rate after 6 months of collecting UI benefits generated by a policy change in 2012. Rebollo-Sanz and Rodríguez-Planas (2020) provide a detailed explanation of the details of the policy change and the context in Spain at the time. In short, in July 2012, the government unexpectedly lowered the replacement rate after 6 months of collecting UI benefits to 50 percent for workers unemployed after July 15th, 2012. Those unemployed before that date maintained a replacement rate after 6 months of collecting UI benefits of 60 percent.

Rebollo-Sanz and Rodríguez-Planas (2020) use a difference in difference empirical strategy to estimate the effects of this policy on different outcomes. Their treatment group comprises individuals potentially eligible to be affected by the policy (i.e., workers entering unemployment in 2012 with potential benefit durations of more than 6 months). Their control group is formed by individuals entering unemployment

in 2012 with potential benefit durations of 6 or fewer months. Their difference in differences estimate compares the outcomes of treated and control individuals before and after the reform.

The treatment and control groups in Rebollo-Sanz and Rodríguez-Planas (2020) are not the only possible options to study the effects of the policy. The monthly benefit level in Spain is bounded by a maximum and a minimum. The existence of these bounds made the policy change heterogeneous, even for workers with potential benefit durations above 6 months. Workers with previous wages below or above certain levels were not affected by the policy change because the caps in the monthly benefit level left their monthly benefits after 6 months of collecting UI unchanged. This fact enables the creation of different treatment and control groups. The second treatment group comprises workers entering unemployment in 2012 with potential benefit durations of more than 6 months and previous wages in the range affected by the policy. The second control group is formed by individuals whose potential duration of UI is identical to that of the treatment group, but whose previous wages were too low or too high to be affected by the policy change due to the maximum and minimum benefit levels.

Following the methodology in Rebollo-Sanz and Rodríguez-Planas (2020), we construct a difference in differences estimator in which the treatment and control groups are defined based on the two possible treatment and control groups described above. Our treatment group is formed by individuals entering unemployment in 2012 with potential durations of UI of more than 6 months and previous wages in the range affected by the policy change. The control group is formed by two sub-groups of individuals also entering unemployment in 2012. First, individuals with similar previous wages to those in the treatment group but with a potential duration of UI of 6 or fewer months. Second, individuals with potential durations of UI benefits of more than 6 months but whose previous wages were high or low enough that they were unaffected by the policy change. The "pre" period starts on the first day of 2012 and ends on July 15th, 2012. The post period starts on July 16th, 2012, and ends on the last day of 2012.

The results are shown in Table E.1. Similar to the findings in Rebollo-Sanz and Rodríguez-Planas (2020) using the same data, we find that the decline in the replacement rate significantly reduced the average number of non-employment days by around 12 percent (or 25 days). However, conditional on time to re-employment, the policy change did not affect re-employment wage changes (point estimate= -0.00022, SE = 0.0171). While this point estimate is insignificant, we can still use it to approximate the wage losses we would observe in the complete absence of UI benefits. A back-of-the-envelope calculation indicates that a 10 percentage point decline in the replacement rate after 6 months of collecting UI benefits corresponds to a decline in the total amount of UI benefits collected by the average treated worker in this period of time of 4.1 percent. This would imply that, in the complete absence of UI benefits, workers' re-employment wage losses would increase by an additional 0.54 percent. While this estimate is imprecise, it suggests that a large increase in re-employment wage losses in response to the complete absence of UI benefits seems very unlikely.

In summary, the discussion above strongly indicates that approximating $g(B,t,\theta) - g^*$ with the estimate of the causal effect of the exhaustion of UI benefits on re-employment wages appears reasonable. Our approximation, 3.5 percent, lies in the middle of the range of the findings of the previous literature and those from alternative sources of exogenous variation in our data (0.5 to 4.7 percent).

E.2 Validity of the assumption that $g(B,t,\theta) - g^*$ is constant for all t smaller than B

To test whether $g(B,t,\theta) - g^*$ is constant for all t smaller than B, we take advantage of the fact that our sample combines workers with multiple potential durations. This allows us to estimate the causal impact of the exhaustion of UI benefits for different groups of workers, using our difference in differences strategy. The key is that by estimating the effect for each discontinuity, we take advantage of the variation in our sample on how long workers have been unemployed once they face the point of exhaustion of UI benefits. For the first discontinuity, the time is only 4 months, while for the last, it is 22 months. Finding that the estimated effect of the exhaustion of UI

benefits on wage losses is not significantly different across discontinuities would provide support for our assumption that the counterfactual wage loss we would observe in period t in the absence of UI benefits would be the same as the one we estimate at the point of exhaustion of UI benefits.

The results are shown in Figures E.1 and E.2. We do not find significant differences in the causal impact of the exhaustion of UI benefits on wage losses across discontinuities, even if the point estimates move toward somewhat higher values after the third discontinuity. Therefore, we do not find any significant evidence that the absence of UI benefits would affect the wage losses differently depending on the duration of non-employment.[50]

In addition to the evidence in our own data, the estimates from the previous literature discussed above, also support the assumption that $g(B,t,\theta) - g^*$ is constant for all t smaller than B. These estimates are derived from samples of unemployed workers with a wide variety of non-employment durations at the time of the interview. The fact that we do not observe any clear pattern of variation in the decline in the reservation wage in the absence of UI benefits amongst them provides an additional piece of evidence supporting our main assumption. Additionally, the results in Marinescu and Skandalis (2021) suggest that $g(B,t,\theta) - g^*$ could be at most 4 percent at the beginning of the unemployment spell. From there, it should decrease to around 2.5 percent before the effects of the exhaustion of UI benefits kick in. Since they estimate the effect of the exhaustion of UI benefits on target wages to be 3.5 percent for their sample, this indicates that our approximation is, if anything, a slight overestimation of the actual $g(B,t,\theta) - g^*$. This is because we find that the period-by-period difference in hazard rates between treatment and control groups is zero at the start of the unemployment spell but becomes negative and significant over the last few periods before the exhaustion of UI benefits.

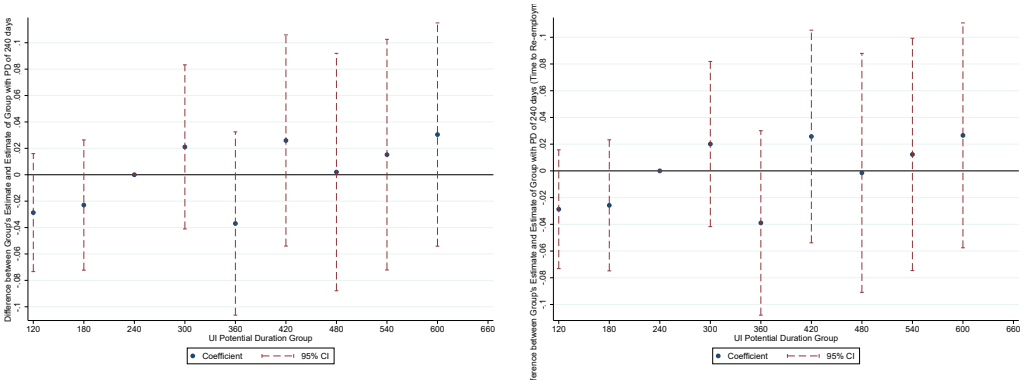
[50] Suppose we were to take the increase of the point estimates over the distribution of discontinuities in Figures E.1 and E.2 as evidence that the effect of the exhaustion of UI benefits gets worse the longer the non-employment duration (even if the difference between estimates is insignificant). Then, using the aggregated causal impact of the exhaustion of UI benefits as our approximation would result in an upwardly biased estimate of the real $g(B,t,\theta) - g^*$. This would imply that the anticipation effect is even less relevant for determining duration dependence.

Table E.1: Effect of a Decline in the Replacement Rate after 6 Months of Collecting UI Benefits from 60 to 50 percent

	Time to Re-employment	*ln* Time to Re-employment	Daily Wage Change
Treated × Post Policy	-25.1*	-0.131**	-0.00022
	[15.1]	[0.052]	[0.01715]
Controls	All	All	All
Time to Re-emp Ctrl	No	No	Yes
N	8735	8735	8103

Note: Table E.1 presents the DiD estimates that identify the causal effect of a decrease in the replacement rate from 60 to 50 percent after 6 months of collecting UI benefits on time to re-employment (column (1)), on the log of time to re-employment (column (2)), and on the daily wage change between the pre-displacement and re-employment jobs (column (3)). Control "All": Includes controls by log wealth, log experience, log previous tenure, previous hourly and daily wage, and fixed effects by age, gender, education (high school and college dummies), potential duration, starting month of the unemployment spell, type of contract pre-displacement, and share of full-time. Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Only unemployment spells started during 2012 are used in the estimation. See sections 5.2and E.1 for additional details. p-value: * 0.10 ** 0.05, *** 0.01.

Figure E.1: Effect of the Exhaustion of UI Benefits on Wage Losses
(Relative to the Effect for Workers with 240 Days of Potential Duration of UI)

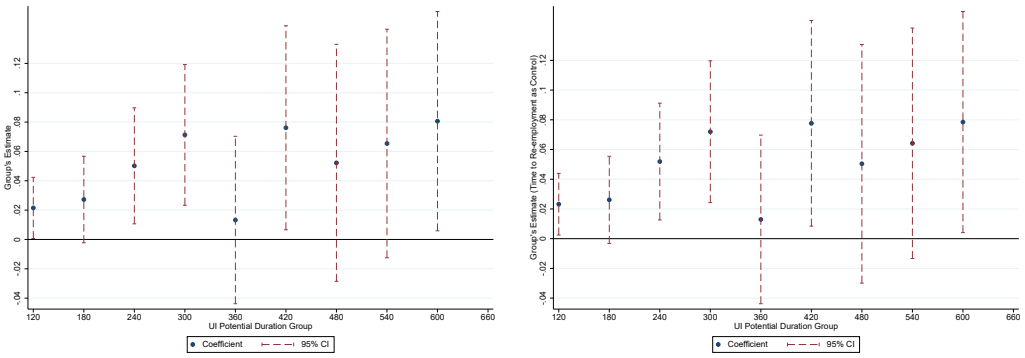


(a) by Discontinuity

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

Note: Figure E.1 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs, separately for workers around each one of the policy discontinuities (each value on the x-axis represents one discontinuity). Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. In panel (a), the point estimates displayed show the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text) for workers around the discontinuity shown in the x-axis, relative to workers around the discontinuity generated between 899 and 900 days. The specification includes all controls (including discontinuity fixed effects; see text). Panel (b) is identical to panel (a) but additionally controls for time to re-employment. The 95 percent confidence intervals shown are generated from robust standard errors.

Figure E.2: Effect of the Exhaustion of UI Benefits on Wage Losses



(a) by Discontinuity

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

Note: Figure E.2 presents the DiD estimates that identify the causal effect of benefit exhaustion on the change in wage between the pre-displacement and re-employment jobs, separately for workers around each one of the policy discontinuities (each value on the x-axis represents one discontinuity). Workers are included in the treatment or control group if they are located within 90 days of one of the discontinuities that extend the potential duration of UI by two months. In panel (a), the point estimates displayed show the difference in wage changes between the treatment and control groups for workers whose time to re-employment relative to primary potential duration is larger than zero but smaller than or equal to 60 (see text) for workers around the discontinuity shown in the x-axis. The specification includes all controls (including discontinuity fixed effects; see text). Panel (b) is identical to panel (a) but additionally controls for time to re-employment. The 95 percent confidence intervals shown are generated from robust standard errors.

E.3 Extension of the UI Potential Duration from 6 to 8 Months

In the main text, we combine all discontinuities in our RD design. However, the discontinuity for workers with prior work experience at 720 days presents a difference relative to the others. It not only extends the potential duration of UI benefits by two months but also reduces the replacement rate during those two months by 10 or 20 percentage points, depending on the time period.

This section presents evidence supporting our decision to combine all discontinuities and thus ignore the fact that one is slightly different from the others. To test whether the response to a two-month extension of the UI potential duration is different when this extension also reduces the re-employment rate, we use the policy change affecting the replacement rate described in Section E.1 of this Appendix. We use the exact same difference in differences empirical strategy as described above, but narrow our analysis to workers whose prior experience qualifies them to collect UI benefits for 6 or 8 months.

If the response to an extension of the UI duration is different when that extension implies a reduction in the replacement rate, we would expect our difference in difference estimates for this group of workers to be significantly negative. The results are shown in Table E.2. We find no evidence suggesting that reducing the replacement rate from 60 to 50 percent after 6 months of collecting UI benefits changes the outcomes of workers with 8 months of UI potential duration affected by the change.

Table E.2: Effect of a Decline in the Replacement Rate after 6 Months of Collecting UI Benefits from 60 to 50 percent

Panel A: Control Workers: Similar Workers with 6 Months of PD of UI			
	Time to Re-employment	*ln* Time to Re-employment	Daily Wage Change
Treated × Post Policy	1.0	0.014	-0.030
	[31.3]	[0.118]	[0.038]
Controls	All	All	All
Time to Re-emp Ctrl	No	No	Yes
N	1697	1697	1579

Note: Table E.2 presents the DiD estimates that identify the causal effect of a decrease in the replacement rate from 60 to 50 percent after 6 months of collecting UI benefits on time to re-employment (column (1)), on the log of time to re-employment (column (2)), and on the daily wage change between the pre-displacement and re-employment jobs (column (3)). Treated workers have 8 months of potential duration of UI, started an unemployment spell in 2012, and have wages in a range affected by the change in the replacement rate. Control workers come from two sub-groups. First, workers with similar previous wages to those in the treatment group who also started an unemployment spell in 2012 but only have 6 months of potential duration of UI. Second, workers who started an unemployment spell in 2012 with 8 months of potential duration of UI but whose replacement rate was unaffected by the policy change due to the maximum and minimum levels in the monthly UI benefit. Control "All": Includes controls by log wealth, log experience, log previous tenure, previous hourly and daily wage, and fixed effects by age, gender, education (high school and college dummies), potential duration, starting month of the unemployment spell, type of contract pre-displacement, and share of full-time. Time to Re-emp Ctrol "Yes": Adds as additional control the time to re-employment. Only unemployment spells started during 2012 are used in the estimation. See section 5.2 for additional details. p-value: * 0.10 ** 0.05, *** 0.01.