Extending the Potential Unemployment Benefit Duration for Marginally Attached Workers*

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Abstract

How does unemployment insurance (UI) affect marginally attached workers? We study a Norwegian rule that doubles potential benefit duration from 12 to 24 months at a low earnings threshold. Using a sharp regression discontinuity design and longitudinal employer-employee data, we find that the extension increases long-term unemployment among men by 5 percentage points, but also raises subsequent employment and labor earnings by 15 to 20 percent. Much of the initial employment effect operates through return to the previous employer, although workers gradually transition to new firms over time. We find no corresponding effects for women. The results show that UI can strengthen long-run labor market attachment for a subset of marginal workers and highlight the role of firms in shaping the effects of social insurance.

1 Introduction

A large literature studies the effects of unemployment insurance (UI) on workers (see Schmieder et al., 2016 for a review). Yet most of this evidence concerns workers with relatively strong labor force attachment, leaving the effects of UI on marginally attached workers largely unknown. This is an important omission. Marginally attached workers are likely to face different labor-market frictions, may have access to other income-support programs, and

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may therefore respond to UI incentives differently from the broader workforce (Autor and Duggan, 2003). Understanding how UI affects this group is therefore important for both economic incidence and policy design.

We address this gap by studying how extending potential unemployment benefit duration affects marginally attached workers, which we proxy for using low pre-displacement earnings.[1] We combine quasi-experimental variation in benefit eligibility with rich Norwegian administrative data. In Norway, potential benefit duration is 12 months by default, but rises to 24 months if prior earnings exceed an earnings threshold. Because this threshold is relatively low, the policy discontinuity primarily affects low-income workers. Relative to the broader unemployed population, workers near this cutoff are younger, earlier in their careers, and more likely to experience subsequent unemployment.[2]

We estimate the causal effects of UI generosity using a sharp regression discontinuity (RD) design embedded in the earnings-based eligibility rule. A natural concern is that workers may anticipate future unemployment and manipulate earnings to qualify for the longer benefit duration. In our setting, however, such manipulation is unlikely. The threshold is revised annually with aggregate wage growth, which limits scope for *ex ante* earnings responses. Consistent with this, we show that the distribution of UI claims and observable covariates is smooth through the cutoff, suggesting that assignment of the 12-month extension is as good as random at the threshold.

We find that the UI extension increases long-term unemployment but also strengthens long-run labor market attachment. In the short run, the extension raises the probability that a spell lasts longer than 12 months by 5 percentage points among men, while leaving the employment rate unchanged. We find no corresponding effect on unemployment duration among women. Over the longer run, the 12-month extension substantially improves male employment and earnings, raising employment persistently by nearly 20 percent and labor earnings by about 20 percent over the five years following initial job loss. In the first year after benefit exhaustion, return to the previous employer accounts for 90 percent of the employment effect, although this margin becomes less important over time as workers transition to new employers.

Why do UI extensions leave short-run labor supply largely unchanged, yet increase em-

[1] Survey data and studies that rely on them typically define marginally attached workers as individuals who are not in the labor force but want and are available for work, and who have looked for a job in the past year but not in the last four weeks (U.S. Bureau of Labor Statistics, 2025; Jones and Riddell, 1999, 2019). Empirically, this behavioral definition is strongly correlated with low earnings (see, e.g., Kodrzycki, 2000; Gray et al., 2005). This motivates our income-based proxy, as direct measures of job search behavior are not available in our administrative data.

[2] However, our heterogeneity analysis reveals that the positive employment effects are concentrated among the relatively better-attached subset of this low-income group.

ployment in the long run? Although our data do not allow us to isolate a single mechanism, we explore several possible explanations. First, we examine how the effects vary with long-term unemployment (LTU) risk, distinguishing between worker- and firm-level sources of that risk. On the worker side, we predict LTU risk using observable worker and market characteristics, following Mueller and Spinnewijn (2024). We find that employment effects at benefit exhaustion are larger among workers with low predicted LTU risk, suggesting that the relevant margin may be stronger prior attachment or better re-employment prospects rather than weak aggregate demand alone. On the firm side, following Lachowska et al. (2025), we estimate a variant of the two-way fixed-effects AKM model (Abowd et al., 1999) using unemployment duration as the outcome. The results also show larger employment effects for workers separating from high-risk firms, a pattern consistent with a greater role for employer attachment or return to the previous employer among more cyclical firms.

A second possibility is that, as UI benefits are exhausted, workers transition to other sources of income support. Such alternatives may provide partial insurance but involve fixed costs, including the stigma of applying for welfare benefits (Moffitt, 1983) or moving back in with parents (Kaplan, 2012). We find no evidence of benefit substitution, including into disability insurance. Workers may nonetheless rely on informal insurance through spousal labor supply, precautionary savings, or short-term credit, but these channels appear limited in our setting. Only 9 percent of workers in our sample are married, suggesting little scope for intra-household insurance, and we find no evidence that savings or borrowing respond to eligibility for a longer benefit duration. These results suggest that labor supply among workers in our sample is less sensitive to UI design than is typically observed among more strongly attached workers. We also examine local labor market conditions. Splitting the sample by unemployment rates within narrowly defined occupation-region cells, we find that the extension generates lasting employment gains in tight labor markets, but primarily extends UI receipt in slack markets. We return to these patterns in Section 5.

Much of the existing research on UI duration focuses on workers with relatively strong labor force attachment and on outcomes such as unemployment duration, reemployment wages, and job-finding rates. For example, Nekoei and Weber (2015a) show that extending PBD raises reemployment wages in Austria, whereas Caliendo et al. (2013) and Schmieder et al. (2016) find that extensions later in the spell among 40-year-olds in Germany reduce

wages.[3] Evidence on lower-earning and more weakly attached workers remains much thinner. A notable exception is Jackson et al. (2025), who study the sudden expansion of UI in the US during the COVID-19 pandemic among previously uninsured workers such as gig workers and the self-employed, a setting in which voluntary quits and layoffs are difficult to disentangle.

Our contribution is therefore narrower than a general claim about UI and all marginally attached workers. We provide evidence on a specific margin: low-earning workers near an earnings threshold for longer benefit duration in Norway, with economically important effects concentrated among men. The paper also speaks to the literature on temporary layoffs and recall, beginning with Feldstein (1976) and Katz and Meyer (1990). Once recall becomes central, firms matter alongside workers' own search decisions. In that respect, our evidence is closest to work emphasizing recall and firm-side adjustment in unemployment dynamics (Nekoei and Weber, 2015b; Fujita and Moscarini, 2017; Gallant et al., 2020). Our results are consistent with the idea that longer UI can preserve attachment long enough for some workers to return to their previous employer, although the mechanism evidence remains suggestive rather than definitive.[4]

Finally, our paper relates to the literature on interactions across safety-net programs. Pioneering work by Black et al. (2002) and Autor and Duggan (2003) shows that worsening local economic conditions increase exits from the labor force through the Disability Insurance (DI) system. By contrast, using time and across-state variation in potential UI duration, Mueller et al. (2016) find no effects of UI extensions on DI applications.[5] Other related studies document interactions between private and public insurance, including credit-market adjustment around job loss (Keys, 2018), housing-market stabilization through UI extensions (Hsu et al., 2018), and private credit as a substitute for public insurance (Herkenhoff et al., 2023). In our setting, the evidence suggests limited substitution from longer UI into disability insurance,

[3] Lalive (2007) finds no effects of UI expansions, small or large, on reemployment and wages among 50-year-old workers in Austria, but does find that women stay unemployed for longer following the extension. Card et al. (2007a) and Le Barbanchon (2016) find no effects on wages or labor force attachment from tenure-based UI extensions in Austria and France, respectively. Van Ours and Vodopivec (2006) use a difference-in-differences approach to study the impact of lowering the PBD and find no impact on wages or the stability of new employment relationships in Slovenia. Marinescu and Skandalis (2021) show that, in France, beneficiaries tend to target lower wages upon benefit exhaustion, suggesting that dynamic selection effects may underlie the negative wage impacts of UI benefit expansions.

[4] For example, Lachowska et al., 2025 show that firms affect inequality through accommodating unemployment insurance claims. Aizawa et al. (2023) and Aizawa et al. (2025) study how firms' policies shape labor market outcomes for individuals with disabilities and the effectiveness of DI programs.

[5] Maestas et al. (2015) and Charles et al., 2018 also document a positive association between DI applications and economic conditions in the US while Rege et al. (2009) show that job displacement increases entry into DI in Norway. Using random assignment of appeal judges and case examiners in the US to instrument for DI allowance, French and Song (2013), Maestas et al. (2013), and Autor et al. (2019) confirm previous findings that DI benefits reduce labor earnings and labor force participation.

savings, or credit, but these estimates are better viewed as descriptive margins than as a full welfare accounting.[6]

Taken together, the results point to a more specific policy lesson than a broad reading of the introduction would suggest. In this Norwegian setting, longer UI increases long unemployment spells for men near the eligibility cutoff, but it also appears to preserve attachment for a subset of workers with meaningful scope to return to the previous employer. That interpretation is less persuasive for women, for whom the estimates are close to zero and measurement issues around part-time work and family-related interruptions are more salient. We therefore view the paper's main contribution as documenting a local but informative case in which extended UI may strengthen later employment through reattachment to the previous firm, rather than as establishing a broad result for all marginally attached workers.

The remainder of the paper is organized as follows. Section 2 provides an overview of the Norwegian setting and describes the data. Section 3 outlines our empirical strategy. Section 4 presents the main findings, and Section 5 interprets the results. Finally, Section 6 concludes.

2 Background and Data

Below we describe the key features of the Norwegian safety net and the sources of our data.

2.1 The Norwegian Safety Net

The Norwegian safety net was established by law in 1967 and has expanded its coverage over time. Today, it encompasses the health system, various income support programs such as disability and unemployment insurance, and old-age retirement. The system covers all residents, is financed through payroll taxes, and has no employer experience rating.

Employment Protection. Employment protection in Norway is relatively strong, ranking at the median among OECD countries. European labor laws regulate employment protection and mass layoffs. Importantly, a firm that plans to downsize must notify the public employment agency and the affected workers; the notice period varies from 1 to 6 months, depending on the worker's age and tenure. During the 12 months following a mass layoff, a firm must consider its former employees when hiring for a new position. After this period, it is free to select any candidate.

[6] Our findings also relate to research on targeting efficiency of programs, including early theoretical work by Parsons (1991) and Diamond and Sheshinski (1995) and more recent studies by Autor et al. (2019), Deshpande and Li (2019), and Deshpande and Lockwood (2022).

Unemployment Insurance. Employees who have been laid off or have had their working hours reduced by at least 50% are eligible for UI benefits, provided their past earnings exceed 1.5 times the National Insurance basic amount (Grunnbeløpet in Norwegian, G hereafter),[7] a fairly low threshold (Røed and Zhang, 2003). Past earnings are calculated as the greater of earnings in the previous calendar year or the average over the last three calendar years. Unemployment benefits are paid after a three-day waiting period and replace 62% of past earnings. The potential duration of unemployment benefits, initially set at 52 weeks, can be extended to 104 weeks for workers whose earnings over the previous three years are above 2G. Program administrators take into account income from regular employment, paid leave, and self-employment.

To remain eligible for benefits, recipients must actively seek employment and be willing to accept any job (whether full-time or part-time) at any geographic location (whether within or across commuting zones). Non-compliance with these requirements can result in sanctions by the Unemployment Insurance agency, such as cancellation of benefit payments for up to 26 weeks. Benefits may also be extended to partially unemployed workers who experience a reduction in hours. The potential duration of benefits may thus exceed the statutory limit as individuals move between part-time and full-time employment.

Alternative Income Support. After exhausting their unemployment benefits, workers can apply for other means-tested social safety net programs. Social assistance typically replaces around 30% of prior earnings, with eligibility requirements varying across geographic administrative units. Both early retirement programs and vocational rehabilitation offer cash transfers of roughly 60% of past earnings. Eligibility for early retirement requires workers to be over 62 years old and affiliated with industries and firms covered by national collective bargaining agreements. Eligibility for vocational rehabilitation depends on a worker's health status, educational attainment, and the relevance of their skills to other occupations. Upon completion of a vocational rehabilitation program, workers who cannot engage in significantly gainful activity due to documented physical or mental impairments may apply for DI benefits. Individuals granted DI benefits rarely return to the workforce in full and are automatically transferred to the old-age retirement program at age 67 (see, e.g., Kostol and Mogstad, 2014).

[7] As of May 2024, the Grunnbeløpet (G) is set at NOK 118,620 or approximately USD 12,000. Benefit payouts are capped at 6G. Unless otherwise stated, all monetary amounts in this paper are deflated using a consumer price index with 2015 as the base year and expressed in NOK 1,000.

2.2 Data

Using anonymized worker and firm identifiers, our study combines four administrative data registers from Statistics Norway:

1. *Income and Wealth:* The first dataset contains individual and household income records, as well as firm income statements and balance sheets, for 1993 to 2017. Collected by the tax authority, this dataset covers various income sources, including labor income, self-employment income, capital income, and public transfers.[8]

2. *Employer-Employee Matched Data*: The second dataset is linked employer-employee data, available from 2000 to 2018. These data provide detailed information on employment contract dates, wage, occupational code, geographic location, and tenure at both the firm and establishment levels.[9]

3. *Unemployment Spells:* The third dataset records all unemployment spells at the individual level from 2003 to 2018. We link these data to newly collected data on UI claims. This supplementary dataset contains complete records for all individuals entering the UI program between 2003 and 2018. Among the key variables are the start date of each spell, prior earnings, and the number of dependent children. Program administrators use this information to determine UI eligibility (i.e., whether the applicant's previous income exceeds the 1.5G minimum) and PBD (i.e., whether their previous income exceeds the 2G threshold for two rather than one year of benefits).

4. *Population:* Finally, we merge the above three datasets with the population register to obtain individual-level demographic characteristics, such as sex, age, municipality of residence, and level of schooling.

An advantage of the Norwegian administrative registers is that they are public records, so there is no attrition due to non-response or lack of consent. In particular, the income and wealth data cover all Norwegian residents, not just those employed in jobs covered by social security, respondents to wealth surveys, or households filing estate tax returns. Most of these data are reported by third parties (e.g., employers, banks, and financial intermediaries), with coverage and reliability rated as exceptional by international standards (see, e.g., Atkinson et al. 1995).

[8]The balance sheet data are measured as of December 31 each year and include assets and debts such as credit card debt, mortgages, bank deposits, and—starting in 2010—housing value.

[9]Before 2015, employers only reported to the authorities when an employment contract was modified. However, since 2015, employers have updated the records of all employment contracts every month.

3 Identifying the Effects of UI Extensions

This section describes the empirical samples and the research design we use to isolate the causal effects of expanding the potential duration of unemployment benefits.

3.1 Empirical Samples

From the universe of all unemployment spells between 2002 and 2018, we first construct a balanced sample of ever-unemployed workers, keeping only the first spell for each individual. To follow workers for up to 6 years after the initial UI claim and at least 6 years before regular retirement, we exclude UI claims filed after 2012 and restrict the sample to individuals aged 20 to 61 at job loss. Panel A of Table 1 presents summary statistics for this baseline sample. Our main empirical sample of marginally attached workers is a subset of the baseline sample, including only those with prior earnings between 1.5G and 2.5G (i.e., within 0.5G of the 2G threshold for two years of UI benefits). Panel B presents summary statistics for this group of workers, while Panels C and D further break it down by gender.

Because our employer-employee matched data are only available from 1997 to 2018, individual career histories are only partially observed. Nonetheless, for each worker, the total time employed during this period can be divided into years before and after the first recorded UI claim. We use the share of years employed before the claim as a proxy for career stage. The last panel of Table 1 shows that, at the time of their first UI application, workers in the RDD sample are, on average, at an earlier stage in their careers than typical workers in the full sample of unemployed individuals. This subset of workers also experiences more unemployment spells in the future. These patterns are consistent with weaker labor market attachment. However—as we show in Section 4—the workers whose employment outcomes respond the most to the extension are the better-attached individuals within this marginal group.

The gender composition is similar in both the full sample of unemployed workers (Column A1) and the subsample of marginally attached workers (Column B1). An important gender difference, however, is that sampled men are much less likely to be married—a feature we will briefly return to later. Sampled women are more likely to have children and are slightly older than sampled men. Regarding sector of employment, men are more likely to be employed in mining and construction, and transportation and sales, while women are more concentrated in education and health. These characteristics may influence the underlying need for insurance and the overall likelihood of long-term unemployment differently across genders.

Table 1: Summary Statistics of Workers

	A. UI		B. RDD		C. RDD Men		D. RDD Women	
	(1) Mean	(2) SD	(1) Mean	(2) SD	(1) Mean	(2) SD	(1) Mean	(2) SD
Demographics								
Female	0.43		0.44					
Immigrant	0.15		0.23		0.27		0.19	
Married	0.39		0.17		0.09		0.27	
Any children	0.37		0.21		0.12		0.32	
Age	34.71	11.62	27.17	9.44	26.40	9.35	28.14	9.46
Education								
High school graduate	0.35		0.24		0.23		0.25	
Bachelor's degree	0.16		0.13		0.09		0.17	
Master's degree	0.09		0.11		0.13		0.09	
Income & wealth								
Labor income	303.81	156.99	152.31	42.94	152.81	47.40	151.68	36.57
Bank deposits	72.29	154.67	34.98	85.43	31.54	83.45	39.28	87.66
Disposable income	252.10	134.13	154.26	61.83	145.81	57.31	164.85	65.56
Homeowner	0.32		0.08		0.03		0.15	
Sector of employment								
Agriculture	0.01		0.02		0.02		0.01	
Mining, construction, & utilities	0.06		0.07		0.12		0.02	
Manufacturing	0.14		0.15		0.18		0.12	
Transportation & sales	0.21		0.23		0.29		0.16	
Information & finance	0.21		0.24		0.22		0.27	
Education & health	0.10		0.12		0.05		0.21	
Arts & recreation	0.07		0.12		0.08		0.16	
Others	0.02		0.04		0.04		0.04	
Career history								
Cumul. % years employed	0.40	0.20	0.27	0.16	0.26	0.16	0.29	0.17
Cumul. % years employed full-time	0.46	0.28	0.32	0.23	0.31	0.20	0.34	0.25
No. future unemployment spells	1.20	1.71	1.42	1.84	1.51	1.89	1.30	1.77
Observations	621757		18715		10408		8307	

Notes: This table reports summary statistics for the overall sample of unemployed and marginally attached workers. The eligibility cutoff for two years of unemployment benefits—2G, or twice the basic amount—is set to zero. Marginally attached workers are those whose previous earnings fall within 0.5G of this cutoff (i.e., 1.5G-2.5G). All samples are aged 20 to 61 at the time of initial UI application and exclude the partially self-employed. All time-varying variables are measured as of the year before the first UI year. Monetary values are expressed in thousands of 2015 NOK. The average NOK/$ exchange rate in 2023 was 10. Career history is truncated, as employer-employee matched data are only available from 1997 to 2018. The cumulative share of years employed (full-time) is an indicator of career stage and is calculated as the number of years between 1997 and 2018 that the worker was employed (full-time) prior to the first UI claim, divided by the total number of years in the same period that the worker was employed (full-time).

3.2 Regression Discontinuity Design

Our empirical strategy leverages the sharp discontinuity in eligibility for longer PBD to identify the causal effects of UI generosity on labor market outcomes. For each outcome of interest Y, we estimate the following econometric model:

$$Y_i = \beta \left\{ \mathbb{1} \left(\text{Prior earnings}_i \geq 2G \right) \right\} + \mathbf{X}_i' \gamma + \epsilon_i, \tag{1}$$

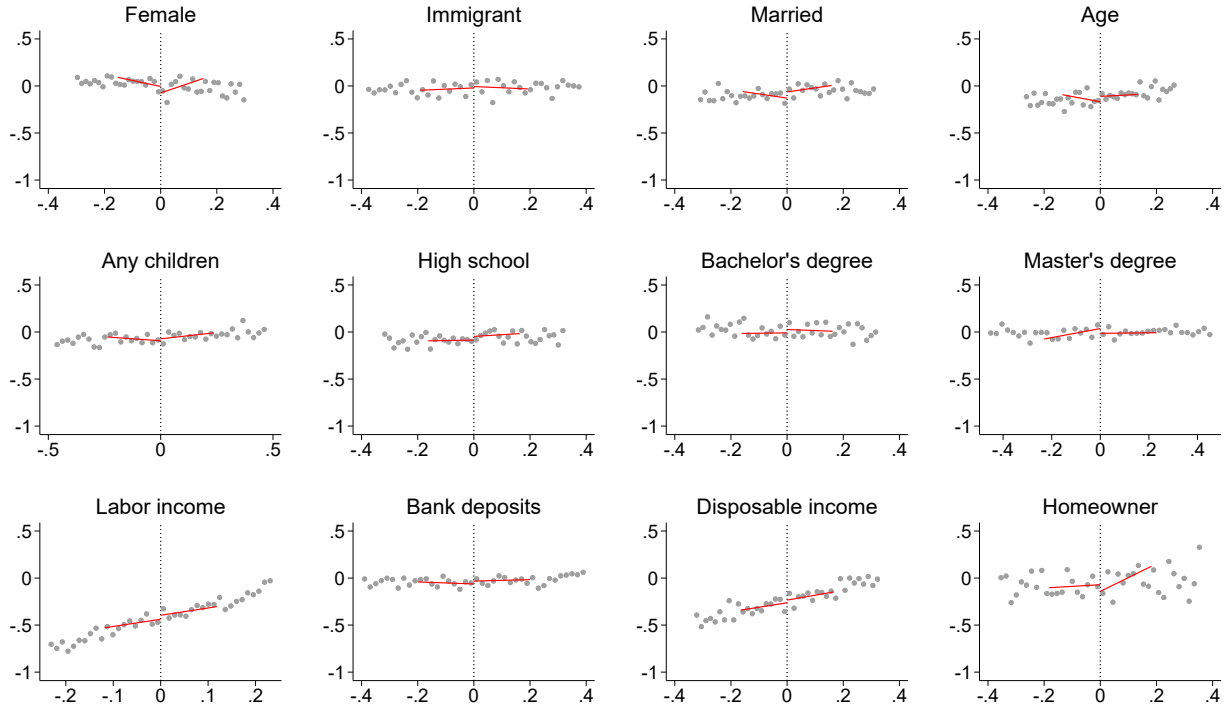
where i indexes workers, $\mathbb{1}(\cdot)$ is the indicator function, $\mathbf{X}_i$ is a vector of individual-level covariates, and ϵ_i is the error term. Our baseline estimation does not include control variables; the covariates $\mathbf{X}_i$ are introduced in robustness checks. Following Calonico et al. (2014), we estimate Equation 1 using outcome-specific optimal bandwidths and the triangular kernel, which assigns greater weight to workers closer to the threshold.

Since the eligibility dummy, $\mathbb{1} \left(\text{Prior earnings}_i \geq 2G \right)$, is the main explanatory variable, Equation 1 specifies the reduced-form model, and the coefficient of interest, β, captures the intention-to-treat (ITT) effect. It admits a causal interpretation under two key identifying assumptions. The first is akin to an exclusion restriction: the potential outcomes are smoothly distributed across the cutoff. This assumption is untestable, but is generally supported by the absence of jumps in the means of predetermined characteristics at the cutoff. The second assumption is that workers could not manipulate the assignment variable in order to gain additional insurance against future income shocks. One concern is that knowledge of future risks of unemployment may induce workers to move earnings above the threshold. However, most occupations in our context offer little flexibility to adjust labor supply, with the exception of the self-employed. To alleviate manipulation concerns, we exclude UI applicants with self-employment income before a spell.

The RD design allows us to be transparent about the above identifying assumptions. Local randomization at the threshold implies that the aggregate distribution of the assignment variable, prior earnings, should be continuous around the threshold. Any bunching of individuals on the right side of the cutoff would indicate sorting, invalidating the assumptions behind the research design. Reassuringly, Appendix Figure A1 shows that the density of the assignment variable is flat around the cutoff both for the full sample of marginally attached workers (Panel A) and separately by gender (Panels B and C), providing graphical evidence that manipulation is unlikely.

Another implication of local randomization is that predetermined characteristics should evolve smoothly on either side of the cutoff. Figure 1 illustrates the distribution of these variables, where each is normalized to mean zero and standard deviation 1 to facilitate comparison across characteristics. In each scatter plot, we use 20 bins on each side of the thresh-

Figure 1: Balancing Checks - Z-score



Notes: Predetermined variables are normalized to have a mean of 0 and a standard deviation of 1. The eligibility cutoff for two years of unemployment benefits—2G, or twice the basic amount—is normalized to zero. Twenty bins and local linear regressions are used on both sides of the cutoff. The sample includes workers aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

old and fit local linear regression lines separately on each side.[10] The plots corroborate the assumption of local randomization, showing no jump in the predetermined variables at the cutoff. We report the corresponding RD estimates in Appendix Table A1, where the null hypothesis is never rejected at conventional significance levels. In Appendix Table A2, we perform the same tests separately by gender. Out of 22 tests, we reject the null hypothesis in one at the 5% level and one at the 10% level, both in the subsample of women.

[10]Following the recommendations in Korting et al. (2023), we confirm the robustness of this graphical evidence in unreported analyses by using the equal-spacing-mimicking-variance estimator to determine the bin size and number of bins. The resulting supplementary binned scatter plots are available upon request. The simplified figures, with a pre-selected number of bins (20 on each side of the cutoff), offer better visual clarity and are therefore presented in the main text.

4 Results

This section presents our main findings. We begin with evidence on UI utilization, then examine long-run labor market outcomes, followed by robustness checks.

4.1 Incidence of Long-Term Unemployment

What is the effect of the expansion of unemployment benefits on the duration of unemployment spells? Following conventions in the UI literature (see, e.g., Lalive, 2007; Card et al., 2007b), we measure unemployment duration as the number of days a person receives unemployment benefits. Long-term unemployment is defined as spells lasting more than 100, 200, ..., 600, 700 days. We estimate a series of RD regressions where the dependent variables are indicator variables for unemployment duration defined at 10-day increments. Specifically, each dummy is equal to one if the spell exceeds 10, 20, ..., up to 720 days, and zero otherwise. Our baseline specification uses local linear regressions with separate slopes on each side of the cutoff, the triangular kernel, and the MSE-optimal bandwidth. We allow the optimal bandwidth to vary by outcome and apply bias-corrected standard errors as in Calonico et al. (2014). Estimation is performed separately by gender.

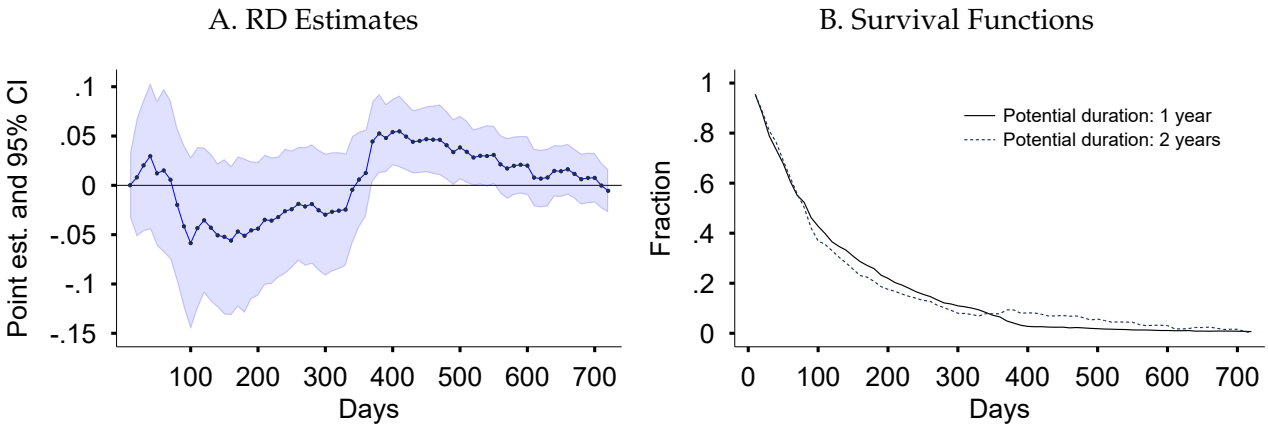
Figure 2 presents the estimates for male unemployment duration, with the x-axis marking the key long-term unemployment thresholds. Panel A plots the point estimates and 95% confidence intervals from the RD regressions. The results indicate that the UI extension increases the benefit utilization rate by about five percentage points, but only for spells lasting longer than one year. This effect begins to decay after 400 days. The estimates are statistically significant between 350 and 500 days; outside this range, they are indistinguishable from zero.

Panel B illustrates the corresponding survival functions for untreated and treated job seekers. The untreated survival function is calculated as the average unemployment rate at each duration among workers with past earnings just below the threshold, as identified by the RD optimal bandwidth selection. The treated survival function is obtained by adding the point estimates from Panel A to the untreated function.[11] As shown in Panel B, the survival functions for the two groups cross at approximately one year.

Table 2 summarizes the regression results for a subset of outcomes. In Panel A, the first row reports the estimates of the effects of the UI extension on the probability of having a spell lasting more than 400, 500, and 600 days. The second row reports the standard er-

[11] These survival functions are generally monotonically decreasing, but they need not decrease everywhere because the effective estimation sample varies with the optimal bandwidth across regressions.

Figure 2: Effects on Unemployment Duration Among Men



Notes: Panel A plots the point estimates and 95% confidence intervals of the effects of the UI extension on unemployment duration among men. The outcomes are indicator variables equal to 1 if a worker remains unemployed for more than 10 days, 20 days, and so on, up to 720 days. Panel B plots the corresponding survival functions of unemployment duration, distinguishing between the untreated (*Potential duration: 1 year*) and the treated (*Potential duration: 2 years*). The gaps between the two curves represent the treatment effects. In both panels, the key long-term unemployment thresholds (100, 200, . . . , 700 days) are marked on the x-axis. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

rors, and the third row presents the means of the dependent variables among the untreated. The number of observations in each regression varies according to the optimal bandwidth selected for each outcome. The point estimate is statistically significant at the 5% level for spells exceeding 400 and 500 days. However, we fail to reject the null hypothesis of no treatment effect for spells exceeding 600 days.

In stark contrast, the UI extension has no detectable effect on unemployment duration among women. Using the female subsample, Panel B of Table 2 reports the point estimates for spells longer than 400, 500, and 600 days, while Appendix Figure A2 illustrates the effects across the full distribution of durations and the corresponding survival functions. In our Norwegian context, men and women appear to respond differently to changes in PBD. This pattern is consistent with evidence from other settings (see, e.g., Ferey et al., 2025 for France). Theoretically, this gender gap could be driven by marriage or motherhood, as women with children are found to be more selective in their job search, often applying for fewer positions than women without children (Philippe and Skandalis, 2023).

In our sample, women are more likely than men to have children and to be married at the time of their first UI claim (see Table 1). To explore the role of motherhood and secondary-earner status, we restrict attention to child-free and single individuals in Appendix Table A3. The extension of unemployment benefits continues to have positive effects on male un-

employment duration in these subgroups, but only for spells exceeding 400 days. Notably, for women, the lack of response persists even among those without children or unmarried. Taken together, our results suggest that marginally attached male and female workers respond differently to financial incentives. However, we caution against over-interpreting these exploratory findings as evidence of no child penalty. Our data include only the number of dependent children a UI claimant has *at the time of application*. As we do not observe birth parity over time, we are unable to further investigate the importance of motherhood or how the benefit extension may have affected childbearing decisions.[12]

Another complication is the interaction between UI receipt and part-time employment. Norwegian regulations allow claimants to work reduced hours and continue receiving partial benefits; a benefit extension that induces women to remain in part-time employment longer would be invisible to our employment measure, which requires at least four hours per week and annual earnings of at least G (see Section 4.2). Relatedly, our administrative data do not distinguish between maternity leave and other interruptions to the unemployment spell, nor do they allow us to track births that may occur during the benefit period. These limitations prevent us from ruling out that the gender difference partly reflects differential transitions into part-time work or into household-related inactivity rather than differential search responses to financial incentives. For this reason, the remainder of our analysis will focus on men.

Table 2: Effects on Unemployment Duration

	A. Men			B. Women		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.054***	0.038**	0.020	0.011	0.029	0.022
	(0.017)	(0.016)	(0.015)	(0.025)	(0.020)	(0.014)
Dep. mean	0.028	0.018	0.011	0.047	0.021	0.009
Observations	5182	3971	3372	3157	3490	3641

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. The sample includes workers aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

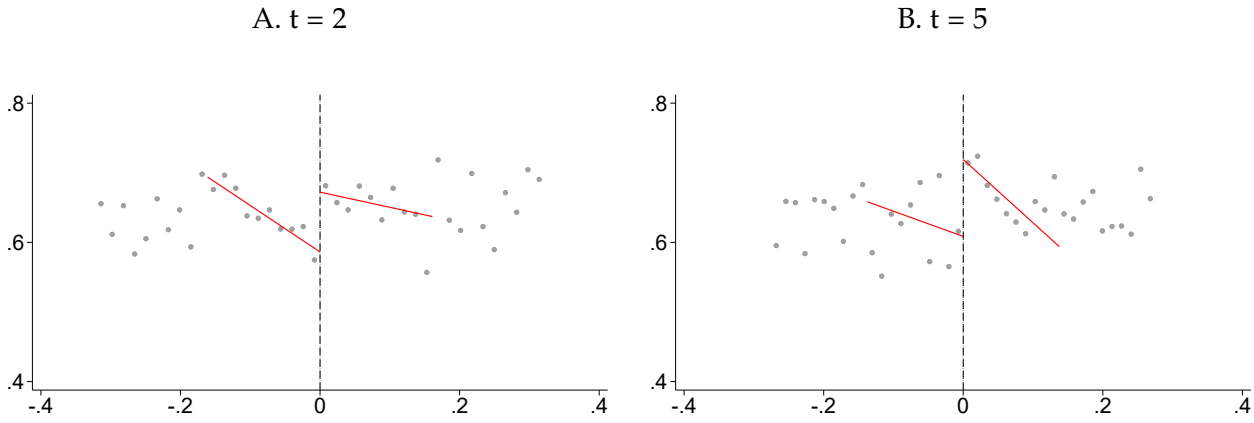
[12] In unreported analyses, we find that the benefit extension increases the likelihood that men are separated from their pre-UI spouse or partner two years after their UI application. However, it has no effect on the civil or marital status of women. These results are available upon request.

4.2 Labor Market Outcomes

We now examine the net impact of the UI extension on employment and related labor market outcomes. We begin by examining the impact on employment two years after the start of the unemployment spell, by which time both treated and untreated workers have exhausted their potential benefit duration. To examine longer-term effects, we also measure employment five years after the initial spell. For both horizons, being employed is defined as having an employment contract at the end of the calendar year with at least four hours of work per week and having annual earnings of at least the basic amount G.

The RD results for employment are visualized in Figure 3, with binned scatter plots constructed in a similar fashion to those in Figure 1. For each bin, we compute the average employment rate and extend the horizontal axis to twice the optimal bandwidth on either side of the cutoff. There is a clear jump in employment rates at the threshold corresponding to two years of benefits. In Panel A, the increase two years after the start of the spell is roughly 10 percentage points, or a 16% rise relative to the untreated mean. Panel B shows the bins and fitted lines for employment measured five years after the start of the spell. The jump remains positive and of similar magnitude, indicating that the UI extension has a significant positive effect on employment in both the short and long run.

Figure 3: Employment Rates After Jobless Spell



Notes: The plots show the discontinuities around the UI extension cutoff in the average employment rate among men two years (Panel A) and five years (Panel B) after the start of the unemployment spell. Twenty bins and local linear regressions are used on both sides of the cutoff. A worker is considered employed if his annual labor earnings equal or exceed the basic amount, and he works at least four hours per week at the end of the year. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Table 3 reports the corresponding regression coefficients and bias-corrected standard errors. Column A1 shows that the estimated effect is statistically significant at two years, with

the ratio of the coefficient to its standard error indicating that the probability of incorrectly rejecting a true null effect is below 5%. Column B1 presents the corresponding estimate five years after the start of the spell, showing a statistically significant positive effect with the probability of incorrectly rejecting a true null below 1%.

Labor Force Participation. To examine whether the UI extension affects the likelihood of being unemployed in the future, we create an indicator equal to one if a person receives any UI benefits in the second or fifth year. Using these dummies as the dependent variables, Columns A2 and B2 show no evidence of differential impacts by the length of potential benefits. More than one third of the sampled men experience a spell of unemployment by the end of the second year, decreasing to 18% by the fifth year. Turning to labor force participation (LFP), defined as being either employed or unemployed, we find that the effects are similar in magnitude to those on employment but are estimated with even greater precision (Columns A3 and B3).

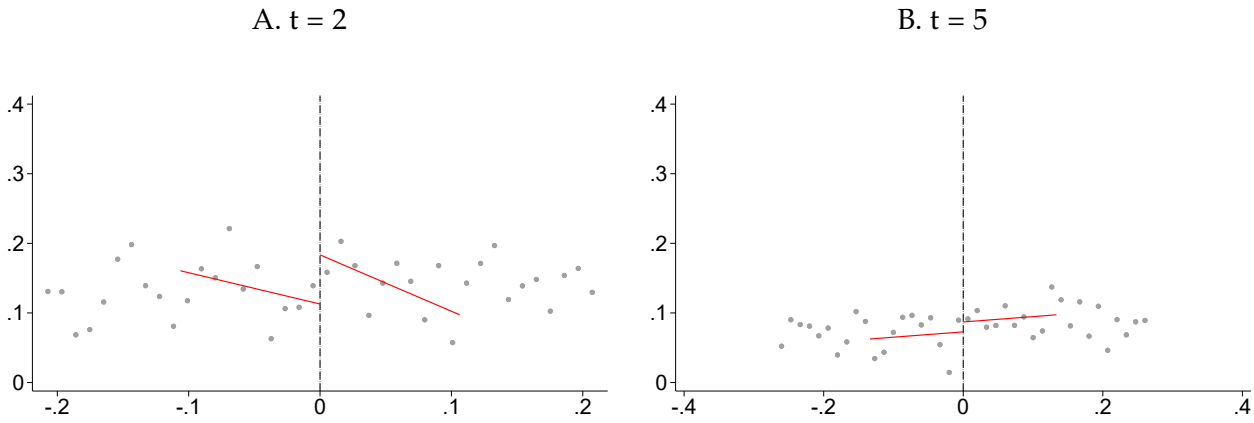
Table 3: Effects on Employment and Labor Force Participation

	A. t = 2			B. t = 5		
	(1) Employed	(2) UI	(3) LFP	(1) Employed	(2) UI	(3) LFP
Estimated effect	0.099**	0.022	0.101***	0.124***	−0.025	0.115***
	(0.040)	(0.043)	(0.035)	(0.044)	(0.031)	(0.042)
Dep. mean	0.640	0.376	0.792	0.620	0.180	0.708
Observations	3396	3278	3129	2936	3418	2930

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Return to the Previous Employer. A substantial fraction of unemployment spells end in return to the previous employer, a margin that has long been central to theories of temporary layoffs and recall (Feldstein, 1976; Katz and Meyer, 1990). Motivated by this literature, we estimate the effects of the UI extension on return to the previous employer, which we define as workers re-employed by their previous firm by the end of the calendar year. Figure 4 presents the graphical evidence. Panel A shows a substantial increase in return to the previous employer at the cutoff, suggesting that this margin accounts for a large share of the positive employment effect. Panel B reports the same outcome three years later, showing that the effect remains positive but is considerably smaller.

Figure 4: Recall Hires After Jobless Spell



Notes: The plots show the discontinuities around the UI extension cutoff in the average rate of return to the previous employer among men two years (Panel A) and five years (Panel B) after the start of the unemployment spell. Twenty bins and local linear regressions are used on both sides of the cutoff. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Table 4 summarizes the regression results and provides the average outcomes in the control group for comparison. The averages show that return to the previous employer is not uncommon: in the second year, nearly 14% of untreated workers return to their previous employer, compared to an overall employment rate of 64% (Column A1 of Table 3). By the fifth year, this rate falls to 7%, while the overall employment rate remains at 62% (Column B1 of Table 3).

The RD estimates are sizable and statistically significant. Comparing Column 2 of Table 4 with Column A1 of Table 3, return to the previous employer accounts for about 90 percent of the employment effect at two years. By the fifth year, the effect on this margin has declined

Table 4: Effects on Recall Hires

	Recalled				
	t = 1	t = 2	t = 3	t = 4	t = 5
Estimated effect	0.051	0.088**	0.082***	0.055**	0.041*
	(0.032)	(0.036)	(0.030)	(0.025)	(0.025)
Dep. mean	0.185	0.136	0.105	0.077	0.068
Observations	3750	2293	2381	2834	2843

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

and is no longer statistically significant, while the overall employment effect persists. This pattern is consistent with the previous employer serving as an initial re-entry point to the labor market, after which workers gradually transition to other firms. At the same time, we interpret this evidence cautiously, since return to the previous employer is only an indirect measure of recall and match preservation.

Labor Income, Transfers, and Taxes. Next, we assess the impacts on labor income. To improve precision, we compute differences relative to the year before the job loss spell ($t = -1$) and average outcomes over six years, from the start of the spell ($t = 0$) to five years later ($t = 5$). Column 1 of Table 5 reports the RD estimates. The effect on male labor earnings is both statistically significant and economically large, amounting to roughly 20% of average prior earnings among the untreated. The magnitude is comparable to the employment effect on the extensive margin, consistent with the earnings gains reflecting additional workers entering employment rather than higher wages conditional on working.

Table 5: Effects on Average Income Changes

	Average change over $t = \overline{0,5}$			
	(1) Labor income	(2) UI benefit	(3) Taxes	(4) Disp. income
Estimated effect	29.602**	−0.683	5.912*	12.200
	(12.363)	(1.457)	(3.474)	(8.191)
Dep. mean	0.624	20.327	1.174	0.558
Pre-UI level mean	150.650	0.762	28.907	143.924
Observations	3046	4481	3087	3544

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. *Pre-UI level mean* is the average across both control and treatment groups of the relevant outcome in level in the year before the first UI claim. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Our data on hours are too coarse to decompose labor income into hours worked and hourly wages. Moreover, the effect on employment prevents isolating impacts on hours and wages without conditioning on an endogenous outcome. With this caveat in mind, we examine the effect of the UI extension on the quality of employment by estimating firm fixed effects from a log-wage regression on person and firm indicators, as in Abowd et al. (1999). Table 6 provides suggestive evidence that extending UI improves the quality of the employer in the short and medium term.

To evaluate the fiscal costs of the program, we examine government cash transfers and taxes owed. Column 2 of Table 5 reports the effect of the benefit extension on average UI

Table 6: Effects on Match Quality

	Firm FE - AKM wage				
	t = 1	t = 2	t = 3	t = 4	t = 5
Estimated effect	0.026	0.057*	0.046*	0.032	−0.026
	(0.032)	(0.032)	(0.028)	(0.031)	(0.024)
Dep. mean	−0.155	−0.128	−0.107	−0.094	−0.074
Observations	2028	2166	2492	2218	3045

Notes: *p < 0.1 **p < 0.05 ***p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

cash payouts. While the estimates are imprecise, we can reject that the effect exceeds 2% of the average pre-UI labor income. Column 3 suggests that the UI extension may have increased tax revenue by nearly 20%; however, we do not have enough statistical power to draw a firm conclusion. Column 4 reports the estimated effect on the change in disposable income. Although statistically insignificant, this estimate indicates that disposable income may have increased by 8% relative to the pre-UI average.

Finally, we explore other forms of insurance. Appendix Table A4 shows that the benefit extension does not affect participation in other social insurance programs, such as disability insurance or social assistance. Likewise, we find no evidence that UI substitutes for private insurance through savings or credit markets, as measured by bank deposits and private debt growth. This is unsurprising given the low levels of homeownership and liquid assets in the year prior to the unemployment spell. As the marriage rate in our sample is only 9%, we do not consider intra-household insurance, but examine whether the UI extension affects the likelihood of living with parents. We find a positive point estimate, but the effect is too imprecise to be considered conclusive.[13]

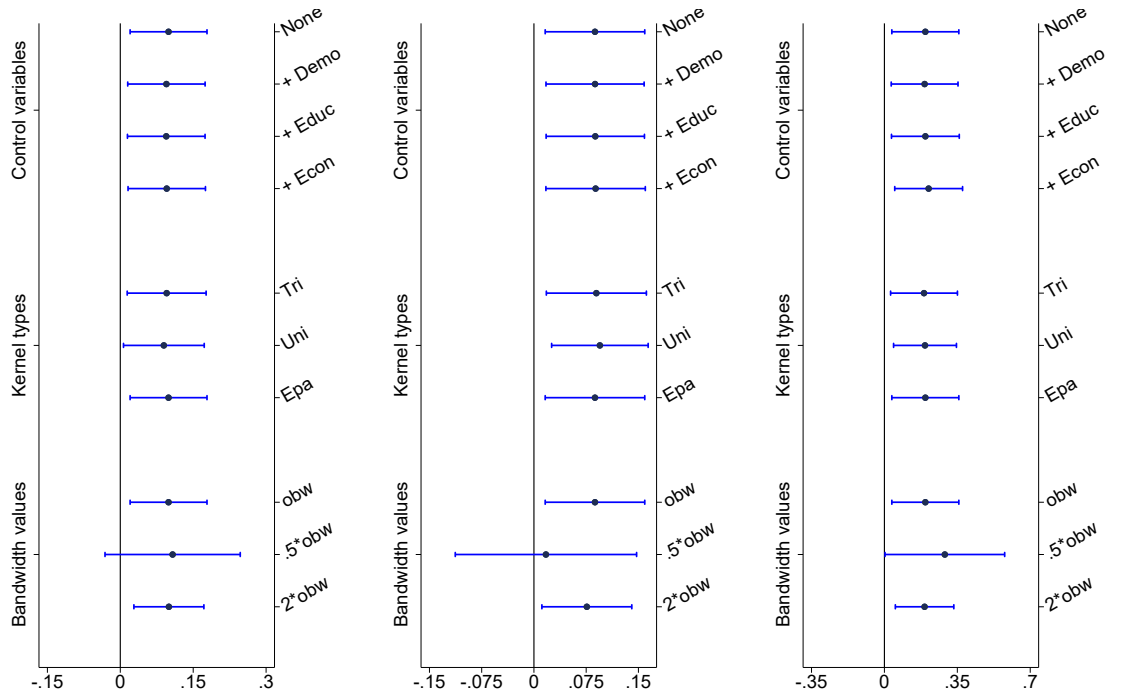
4.3 Robustness

Our baseline RD specification employs the triangular kernel, assigning greater weight to observations closer to the threshold. Following recent advances in nonparametric RD estimation, we also use optimally selected bandwidths, bias-corrected point estimates, and bias-corrected standard errors. This section demonstrates that our results are robust to these specification choices.

[13]The average probability of living with parents two years after job loss is 0.360. The estimated effect is 0.066, with a standard error of 0.045.

We perform three specification checks and summarize the results in Figure 5. First, we vary the bandwidth by multiples of the optimal value. Second, we alternate between different weighting schemes. Finally, we introduce different sets of covariates. Our analysis focuses on three primary outcomes: employment (Panel A), return to the previous employer (Panel B), and change in labor income (Panel C). The first two variables are measured in the short term (two years out), while the last outcome is averaged over six years following the initial UI application.

Figure 5: Robustness Checks



A. Employed at t = 2 B. Recalled at t = 2 C. Δ Labor income over t = $\overline{0,5}$

Notes: This figure reports bias-corrected local-linear RD estimates for three main outcomes: employment at year t = 2 in Panel A, return to the previous employer at year t = 2 in Panel B, and six-year average change in labor income in Panel C. The blue spikes represent 95% confidence intervals. Outcomes are measured two years (Panels A and B) or over five years (Panel C) after the initial UI claim, while control variables are predetermined and measured one year prior to this claim. Estimates in Panel C are scaled by the sample average of the relevant outcome (in level) in this reference year across both control and treated individuals. In each panel, the bottom pane varies the bandwidth value: MSE-optimal bandwidth (*obw*, baseline), half this bandwidth (*.5*obw*), and twice this bandwidth (*2*obw*). The middle and top panes report estimates with MSE-optimal bandwidths. The middle pane alternates between three kernel types: Triangular (*Tri*), Uniform (*Uni*), and Epanechnikov (*Epa*) while the top pane sequentially adds three sets of controls to the baseline model of no covariates: demographics (gender, immigrant status, marital status, age, and number of children with child benefits), education (dummies for high school, bachelor's degree, and master's degree), and economic (labor income, disposable income, and bank deposits). The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Across the three panels, the bottom cells show estimates from varying the bandwidth. Doubling the bandwidth leaves both point estimates and standard errors virtually unchanged, while shrinking it reduces precision, as expected. The middle cells show robustness to the choice of kernel: using uniform weights, as suggested by Dahl et al. (2014), or Epanechnikov weights produces estimates identical in sign and similar in magnitude to those obtained with triangular weights. Finally, the top cells show that adding demographic, educational, and economic covariates has no material impact on our conclusions. This invariance is unsurprising, as it provides further evidence for the exogeneity of the eligibility threshold, previously validated in Section 3.2. All in all, these specification checks confirm that our findings are not driven by specific choices of bandwidth, weighting scheme, or control variables.

4.4 Heterogeneity

Why do UI extensions leave labor supply unaffected in the short run, but increase employment in the long run? To shed light on these questions, we first conduct subsample analyses based on worker characteristics and, second, on firm and market characteristics. Table 7 reports the RD estimates of treatment effect heterogeneity by worker characteristics, focusing on employment, return to the previous employer, and five-year average labor income.

We begin by exploring heterogeneity by education. As high school completion has been shown to be a strong predictor of labor force attachment (Abraham and Kearney, 2020), we split the sample by the status of high school completion. Indeed, we find that both the larger effects on benefit utilization (Panel I of Appendix Table A5) and the positive effects on labor market outcomes (Panel I of Table 7) are concentrated among workers who have completed high school.

Next, we split the sample at the median age at job loss (31 years) to examine heterogeneity by age. Younger workers drive the positive effects on labor market outcomes (Panel II of Table 7), while older workers exhibit higher benefit utilization rates (Panel II of Appendix Table A5). These findings indicate that the group of workers whose outcomes depend on the expansion is not primarily young workers, as we see the greatest response on long-term receipt of UI benefits among the older.

To assess whether variation in treatment effect can be predicted, we follow recent advances in predicting LTU (Mueller and Spinnewijn, 2023) and employ a Random Forest (RF) algorithm on multiple-spell data, using an indicator for spells lasting 12 months or more as the outcome. The RF model is trained separately for each year, with parameter tuning performed via three-fold cross-validation. Additional details of the estimation procedure are provided in Appendix C.

Table 7: Heterogeneous Effects on Labor Market Outcomes by Individual Characteristics

I. By Skill

	A. High skill			B. Low skill		
	I. t = 2		II. t = $\overline{0,5}$	I. t = 2		II. t = $\overline{0,5}$
	(1) Employed	(2) Recalled	(1) Δ Labor income	(1) Employed	(2) Recalled	(1) Δ Labor income
Estimated effect	0.172***	0.115**	73.803***	0.041	0.070*	2.918
	(0.065)	(0.058)	(20.680)	(0.052)	(0.039)	(12.295)
Dep. mean	0.596	0.161	0.569	0.672	0.129	0.641
Pre-UI level mean			145.792			154.917
Observations	1365	943	1152	2045	2031	2758

II. By Age

	A. Young			B. Old		
	I. t = 2		II. t = $\overline{0,5}$	I. t = 2		II. t = $\overline{0,5}$
	(1) Employed	(2) Recalled	(1) Δ Labor income	(1) Employed	(2) Recalled	(1) Δ Labor income
Estimated effect	0.114**	0.073*	29.114**	0.027	0.114	19.540
	(0.046)	(0.038)	(13.535)	(0.098)	(0.076)	(26.863)
Dep. mean	0.648	0.119	0.686	0.588	0.180	0.346
Pre-UI level mean			150.917			149.333
Observations	2615	2116	2314	654	586	805

III. By Individual LTU Risk

	A. High risk LTU12			B. Low risk LTU12		
	I. t = 2		II. t = $\overline{0,5}$	I. t = 2		II. t = $\overline{0,5}$
	(1) Employed	(2) Recalled	(1) Δ Labor income	(1) Employed	(2) Recalled	(1) Δ Labor income
Estimated effect	0.056	0.050	36.027**	0.161***	0.118**	22.686
	(0.055)	(0.045)	(17.894)	(0.052)	(0.052)	(14.959)
Dep. mean	0.638	0.149	0.565	0.654	0.113	0.692
Pre-UI level mean			148.122			153.563
Observations	1968	1364	1694	1739	1357	1779

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. *Pre-UI level mean* is the average across both control and treatment groups of the relevant outcome in level in the year prior to the first UI claim. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the partially self-employed.

We split the sample at the median predicted LTU risk of 22.5%. The employment and return-to-the-previous-employer effects are larger in the low-LTU-risk group. Panel III of Appendix Table A5 shows that the rate of benefit utilization is similar across the two groups. The income effect is slightly larger and statistically significant for the high-LTU-risk group, while it is insignificant for the low-LTU-risk group. These patterns suggest that the workers with the strongest later employment response are not simply those at greatest observable risk of prolonged non-employment.

Firm and Market Differences. Having established that employment effects are not predictable from worker-level LTU risk, we turn to differences across firm types. Following Lachowska et al. (2025), we estimate a two-way fixed effects model of unemployment duration to isolate factors attributable to firms while holding the composition of workers constant. Our main sample consists of workers with relatively short employment histories whereas identification of the firm effects relies on observing workers who experience multiple unemployment spells and separate from at least two employers. We therefore use the full panel of unemployment spells to estimate firm fixed effects; additional details are provided in Appendix B. To preserve statistical power, we split the sample at the median of the estimated ex-employer fixed effect.

The results are presented in Panel I of Table 8. The employment effects are larger among workers who separate from firms with high predicted LTU risk. In addition, the effect on return to the previous employer is more pronounced for workers from these firms than for those separating from low-risk firms.[14] Taken together, these patterns are consistent with a greater role for employer attachment and return to the previous employer among workers leaving more cyclical firms, although they do not separately identify that channel.

Lastly, we examine whether the effects of the extension vary with local labor market conditions. To do so, we split the sample by unemployment levels within occupation-region cells. Panel II of Table 8 reports the results. In high-unemployment markets, the extension yields little improvement in employment two years after job loss, despite increasing unemployment duration (Panel II of Table A6). By contrast, the employment gains are concentrated in tighter labor markets. This pattern is consistent with, though far from conclusive proof of, an interpretation in which longer UI is more effective when recall or re-employment opportunities remain viable.

[14] Workers from high-risk firms account for the majority of long-term UI benefit use; estimates for both subsamples are reported in Panel II of Appendix Table A5.

Table 8: Heterogeneous Effects by Firm and Market Characteristics

I. By Firm UI Duration

	A. High duration			B. Low duration		
	I. t = 2		II. t = $\overline{0,5}$	I. t = 2		II. t = $\overline{0,5}$
	(1)	(2)	(1)	(1)	(2)	(1)
	Employed	Recalled	Δ Labor income	Employed	Recalled	Δ Labor income
Estimated effect	0.142**	0.137***	43.094***	0.068	0.029	24.570
	(0.058)	(0.052)	(16.030)	(0.053)	(0.043)	(17.705)
Dep. mean	0.649	0.147	0.609	0.633	0.126	0.649
Pre-UI level mean			150.544			151.382
Observations	1732	1086	1799	2065	1650	1445

II. By Labor Market Condition

	A. High unemployment market			B. Low unemployment market		
	I. t = 2		II. t = $\overline{0,5}$	I. t = 2		II. t = $\overline{0,5}$
	(1)	(2)	(1)	(1)	(2)	(1)
	Employed	Recalled	Δ Labor income	Employed	Recalled	Δ Labor income
Estimated effect	0.083	0.050	38.876**	0.180**	0.123	44.025
	(0.052)	(0.046)	(16.139)	(0.088)	(0.081)	(27.271)
Dep. mean	0.650	0.109	0.546	0.610	0.184	0.588
Pre-UI level mean			159.963			141.929
Observations	2079	1391	1742	774	538	805

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. *Pre-UI level mean* is the average across both control and treatment groups of the relevant outcome in level in the year prior to the first UI claim. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the partially self-employed.

5 Discussion

The heterogeneous effects of the two-year extension suggest that the policy does not operate through a single mechanism. The evidence is most consistent with an interpretation that combines recall, employer attachment, and labor market tightness, but it does not by itself isolate these channels. We therefore use a simple framework to organize the findings, rather than to claim that the mechanism is fully identified.

A Simple Framework for Interpreting the Evidence

To organize the empirical patterns, consider a simple search environment in which an unemployed worker chooses search effort while facing both recall opportunities and new job offers. The worker receives benefit b per period for up to T periods, after which benefits lapse and the worker receives a lower continuation value $\bar{V}^{post}$ (capturing the value under social assistance or non-participation). While unemployed with $\tau \geq 1$ periods of benefits remaining, the worker may (i) be recalled by the previous employer with exogenous probability $p \in [0, 1]$, or (ii) find a new job with probability $\lambda(s)$, where $s \geq 0$ is search effort and $\lambda(\cdot)$ is increasing and concave ($\lambda' > 0$ and $\lambda'' < 0$). Search effort costs c(s), with $c(\cdot)$ increasing and convex ($c' > 0$ and $c'' > 0$). Recall offers arrive independently of the worker's search decisions, capturing the idea that recall is a firm-side decision. Let $V^R > V^N > \bar{V}^{post}$: recall ($V^R$) is preferred because it preserves firm-specific capital, new matches (V^N) are preferred to benefit exhaustion, and exhaustion ($\bar{V}^{post}$) is the least desirable outcome.

The value of unemployment with τ periods of benefits remaining satisfies:

$$V^U(\tau) = \max_{s \geq 0} \left\{ b - c(s) + \delta \left[p\, V^R + \lambda(s)\, V^N + \left(1 - p - \lambda(s)\right) V^U(\tau - 1) \right] \right\}, \tag{2}$$

for $\tau = 1, \ldots, T$, with terminal condition $V^U(0) = \bar{V}^{post}$. The optimal search effort $s^*(\tau)$ satisfies the first-order condition:

$$c'(s^*) = \delta\, \lambda'(s^*) \left[V^N - V^U(\tau - 1) \right]. \tag{3}$$

The worker searches harder when the gain from a new job relative to continued unemployment, $V^N - V^U(\tau - 1)$, is large.

This framework nests both moral hazard and option value. Extending T raises $V^U(\tau)$ at every τ by delaying the point at which the worker faces $\bar{V}^{post}$. Through the first-order condition (3), this *reduces* search effort: the higher continuation value $V^U(\tau - 1)$ shrinks the gap $V^N - V^U(\tau - 1)$ that motivates search. This is the standard moral-hazard channel—

longer benefits make unemployment more attractive, reducing the urgency of finding a new job.

However, the extension can increase the likelihood that a worker remains attached long enough for recall to occur. By remaining unemployed longer, the worker survives to periods in which recall offers may still arrive. Since recall is modeled as a firm-side process that is independent of the worker's search effort, this channel can operate even as search effort falls. The net effect on later employment then depends on which force dominates: the reduction in new-job finding or the increased chance of remaining available for recall.

The relative strength of these forces depends on p. For workers with high recall probability, the option-value channel may dominate: the benefit extension preserves their attachment until the firm recalls them, and the decline in search effort is less costly when return to the previous employer is especially valuable. For workers with $p \approx 0$, the moral-hazard channel is more likely to dominate: the extension reduces search without any offsetting return-to-employer margin. Formally, the total probability of exiting unemployment in the current period is $p + \lambda(s^*(\tau))$, and the marginal effect of extending T on this flow exit rate is:

$$\frac{d\left[p + \lambda(s^*)\right]}{dT} = \underbrace{0}_{\text{conditional recall rate unchanged}} + \underbrace{\lambda'(s^*)\frac{ds^*}{dT}}_{<0 \text{ (moral hazard)}} . \tag{4}$$

This expression describes the contemporaneous exit margin, not eventual employment over a longer horizon. The latter may still increase if longer benefit duration keeps workers in the risk set for future return to the previous employer. In that sense, the net employment effect can be positive when the value of preserved attachment is large enough relative to the search-effort reduction.

The framework also suggests one reason employment effects could be larger in tight labor markets. When $\lambda(s)$ is high for any given s—reflecting abundant job opportunities—the continuation value $V^U(\tau - 1)$ is already closer to V^N, so the moral-hazard distortion from extending T may be smaller. In slack markets, the opposite may hold: the extension may produce a larger search-effort reduction with less scope for preserved attachment to translate into later employment. This pattern is consistent with the evidence, though it is not unique to this interpretation.

Reconciling the Findings

The framework provides one way to organize the heterogeneity results when the recall probability p is allowed to vary by firm type, though those patterns are not unique to this

interpretation. Let p_j denote the recall probability for workers separating from firm type $j \in \{\text{high-risk, low-risk}\}$. High-risk firms—those with high estimated LTU fixed effects—tend to be cyclically sensitive establishments that rely on temporary layoffs and recall hiring to manage workforce fluctuations. For their workers, unemployment is more often a waiting state than a search state, so $p_{high} > p_{low}$. Extending T then benefits precisely these workers by prolonging the period over which recall remains feasible.[15]

The medium-run evidence is consistent with, though it does not prove, the view that these returning matches may be especially valuable for the workers affected by the extension. Return to the previous employer accounts for a large share of the employment effect at benefit exhaustion, while this margin becomes less important over time as workers transition to new firms. That pattern fits naturally with an interpretation based on employer attachment and re-entry through the previous firm, although other explanations remain possible.

The larger employment effects among workers with low predicted LTU risk are also consistent with this interpretation. One possibility is that these workers have stronger underlying attachment to their previous employers or better prospects for re-entry, making additional benefit duration more likely to preserve an existing match rather than simply delay search. By contrast, for higher-risk workers, the extension may lengthen benefit use without generating the same later employment gains.

The results by labor market slackness also point in this direction. If the extension primarily operated through moral hazard, employment losses would be largest in tight markets, where reduced search effort is most costly. Instead, employment gains are concentrated in tight markets, a pattern more consistent with the recall-and-option-value channel being relatively more important when job arrival rates are high.

Our framework, however, does not address the gender gap in responses, which likely reflects differences in labor supply margins (part-time work, maternity leave) that are outside the model. We discuss the gender gap separately below.

The Gender Gap in Benefit Utilization and Outcomes

The absence of employment and earnings effects for women warrants particular caution in interpretation. Appendix Figure A2 shows that treated and untreated women have nearly identical survival functions throughout the unemployment spell, suggesting little effect of the extension on measured UI utilization. At the same time, the labor market outcomes

[15]This interpretation is consistent with the institutional regulation described in Section 2: during the 12 months following a mass layoff, a firm is legally required to give priority to its former employees when hiring. Workers from high-risk firms are disproportionately likely to be displaced in mass layoffs.

are not a clean null: women just above the threshold display a positive but imprecisely estimated increase in return to the previous employer two years after spell onset, without a corresponding increase in labor income over five years. This combination makes the female results difficult to interpret rather than straightforwardly zero.

Several explanations are consistent with this pattern. First, recall in female-dominated sectors may disproportionately take the form of part-time positions. Norway has one of the highest rates of part-time employment in the OECD, particularly among women in health and education, the sectors most represented in our female sample (see Table 1). A return to the previous employer in a part-time position could preserve the employment relationship without generating large earnings gains. Second, maternity leave and other family-related interruptions are not separately identified in our data. If some treated women exit UI earlier because of childbearing or related transitions, measured utilization and subsequent earnings may not fully capture their behavioral response to the extension. Third, women in our sample who have children may be more likely to occupy a secondary-earner role within the household, which could make labor supply less sensitive to own UI generosity.

Our data do not allow us to adjudicate among these explanations. For that reason, we do not interpret the female estimates as showing that UI generosity is irrelevant for women with weak attachment. Instead, we view them as evidence that the relevant adjustment margins for women may be measured less well in our setting, or may differ from those operating for men. The corroborating evidence from Ferey et al. (2025), who document larger male duration elasticities in France using a different design and institutional setting, suggests that the gender contrast is not unique to Norway. Understanding that contrast remains an important question for future research.

6 Conclusion

A large literature studies how unemployment insurance affects job search and unemployment duration, but most of that evidence concerns workers with relatively strong labor market attachment. Much less is known about lower-earning workers with weaker attachment, whose responses may also depend on other parts of the safety net and on firm-side reemployment margins.

This paper studies one specific case: a Norwegian rule that extends potential UI duration from one year to two years for workers near a low earnings threshold. Using a sharp regression discontinuity design and linked administrative employer-employee data, we document three main patterns. First, the extension increases the incidence of long unemployment spells among men. Second, it also raises subsequent male employment and earnings over

the medium run. Third, much of the initial employment effect operates through return to the previous employer, although that margin becomes less important over time as workers transition to new firms.

We interpret these findings as informative but local. Relative to the standard UI-duration literature, the paper contributes evidence on a lower-earning population that has received comparatively little attention. Relative to the recall literature, the results are consistent with the importance of temporary layoffs, recall expectations, and employer reattachment emphasized by Feldstein (1976) and Katz and Meyer (1990). At the same time, the mechanism evidence remains indirect. Return to the previous employer is suggestive of recall and match preservation, but does not by itself establish why those matches are preserved, or whether the same pattern would arise outside a recall-friendly institutional setting.

The policy implications should therefore be interpreted cautiously. In this setting, longer UI appears capable of increasing later employment for a subset of male workers with stronger reattachment prospects, especially in tighter labor markets. But that result does not imply that longer UI will generally improve employment among all marginally attached workers, or that the welfare effects are unambiguously positive. The paper is on firmer ground documenting a specific empirical pattern than making broad claims about optimal UI design.

Finally, the gender contrast is central rather than incidental. We find no comparable effects for women, and the available data do not allow us to determine whether this reflects genuinely different behavioral responses or limitations in measuring part-time work, maternity-related interruptions, and related margins. For that reason, the most credible interpretation of the paper is not that UI extensions broadly strengthen attachment among marginally attached workers, but that they may do so for a specific group of men in a setting where reattachment to the previous employer appears empirically important. Explaining why this pattern is absent for women remains an important question for future research.

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Online Appendix

A Additional Tables and Figures

A.1 Additional Tables

Table A1: Summary Statistics and Balancing Checks

	(1) N	(2) Mean	(3) SD	(4) Point est.	(5) SE	(6) t-stat
Demographics						
Female	5811	0.44		−0.04	0.03	−1.13
Immigrant	7267	0.24		0.01	0.03	0.26
Married	6007	0.18		0.03	0.02	1.12
Age	5191	28.03	9.75	0.71	0.66	1.07
Any children	9005	0.23	0.42	0.01	0.02	0.45
Education						
High school	6167	0.26		0.01	0.03	0.42
Bachelor's degree	6139	0.12		0.01	0.02	0.51
Master's degree	8808	0.11		−0.02	0.02	−0.97
Income & wealth						
Labor income	4577	173.04	51.69	2.89	2.85	1.01
Bank deposits	7541	36.38	78.75	2.06	4.76	0.43
Disposable income	6261	167.61	61.14	2.44	3.72	0.66
Homeowner	1061	0.09	0.29	−0.03	0.04	−0.74

Notes: This table reports summary statistics and results of balancing checks for the sample of marginally attached workers used in the RD estimation. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed. All time-varying variables are measured as of the year prior to the first UI year. Monetary values are expressed in thousands of 2015 NOK. The average NOK/$ exchange rate in 2023 was 10.

Table A2: Balancing Checks by Gender

A. Men

	(1) N	(2) Mean	(3) SD	(4) Point est.	(5) SE	(6) t-stat
Demographics						
Immigrant	3674	0.27		−0.03	0.04	−0.84
Married	3901	0.11		−0.02	0.02	−0.84
Age	3098	26.93	9.48	−0.14	0.85	−0.17
Any children	3814	0.13	0.34	−0.01	0.03	−0.24
Education						
High school	3824	0.25		0.03	0.03	0.84
Bachelor's degree	2684	0.09		−0.01	0.03	−0.36
Master's degree	2987	0.13		−0.01	0.03	−0.24
Income & wealth						
Labor income	3734	173.33	55.84	−0.94	3.14	−0.30
Bank deposits	4578	32.02	73.75	−1.64	5.76	−0.28
Disposable income	3411	159.46	58.29	1.86	4.39	0.42
Homeowner	574	0.04	0.20	−0.03	0.04	−0.70

B. Women

	(1) N	(2) Mean	(3) SD	(4) Point est.	(5) SE	(6) t-stat
Demographics						
Immigrant	2634	0.19		0.05	0.04	1.34
Married	2486	0.26		0.10	0.04	2.37
Age	2713	29.44	9.91	1.63	0.91	1.79
Any children	3635	0.36	0.48	0.04	0.04	0.90
Education						
High school	2186	0.28		−0.01	0.05	−0.28
Bachelor's degree	3144	0.17		0.05	0.04	1.38
Master's degree	2266	0.09		−0.04	0.04	−1.03
Income & wealth						
Labor income	2169	172.68	45.84	6.20	4.22	1.47
Bank deposits	3200	41.94	84.37	8.76	8.09	1.08
Disposable income	2779	178.02	63.09	4.94	6.27	0.79
Homeowner	422	0.17	0.37	−0.07	0.10	−0.72

Notes: This table reports summary statistics and results of balancing checks for the sample of marginally attached workers used in the RD estimation, separately by gender. All samples are aged 20 to 61 at the time of initial UI application and exclude the self-employed. All time-varying variables are measured as of the year prior to the first UI year. Monetary values are expressed in thousands of 2015 NOK. The average NOK/$ exchange rate in 2023 was 10.

Table A3: Effects on Unemployment Duration Among Child-Free or Single Individuals

	A. Men			B. Women		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
a. No children						
Estimated effect	0.046**	0.026	0.007	0.001	0.022	0.006
	(0.018)	(0.017)	(0.015)	(0.030)	(0.025)	(0.017)
Dep. mean	0.029	0.019	0.010	0.038	0.017	0.008
Observations	4331	3277	2612	1776	2035	2111
b. Unmarried						
Estimated effect	0.053***	0.031*	0.014	−0.013	0.017	0.007
	(0.018)	(0.016)	(0.015)	(0.025)	(0.020)	(0.016)
Dep. mean	0.025	0.016	0.009	0.042	0.018	0.010
Observations	4120	3625	2893	2411	2794	2275

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. The sample includes men and women aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Table A4: Effects on Other Insurance Utilization in the Short Run

	t = 2			
	(1) DI	(2) Δ Social assistance	(3) Δ Bank deposits	(4) Δ Debt
Estimated effect	0.002	−1.567	11.187	−44.117
	(0.003)	(1.515)	(8.201)	(48.514)
Dep. mean		−0.356	0.715	1.822
Pre-UI level mean	0.000	3.439	28.817	121.196
Observations	3179	3066	3730	2753

Notes: * p < 0.1 ** p < 0.05 *** p < 0.01. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. *Pre-UI level mean* is the average across both control and treatment groups of the relevant outcome in level in the year prior to the first UI claim. The sample includes men aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

Table A5: Heterogeneous Effects on Unemployment Duration by Individual Characteristics

I. By Skill

	A. High skill			B. Low skill		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.085***	0.041*	0.012	0.040*	0.038*	0.027
	(0.031)	(0.024)	(0.020)	(0.023)	(0.021)	(0.020)
Dep. mean	0.027	0.018	0.010	0.030	0.019	0.012
Observations	1593	1744	1763	2565	2533	1979

II. By Age

	A. Young			B. Old		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.030*	0.022	0.014	0.151***	0.107**	0.053
	(0.018)	(0.016)	(0.014)	(0.046)	(0.046)	(0.045)
Dep. mean	0.026	0.016	0.010	0.033	0.028	0.014
Observations	4161	3509	3025	1082	844	749

III. By Individual LTU Risk

	A. High risk LTU12			B. Low risk LTU12		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.062**	0.040*	0.025	0.058**	0.022	0.006
	(0.030)	(0.022)	(0.020)	(0.026)	(0.024)	(0.022)
Dep. mean	0.035	0.020	0.013	0.023	0.017	0.010
Observations	1916	2483	2193	1943	1554	1483

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. All samples are aged 20 to 61 at the time of initial UI application and exclude the partially self-employed.

Table A6: Heterogeneous Effects on Unemployment Duration by Firm and Market Characteristics

I. By Firm UI Duration

	A. High duration			B. Low duration		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.090***	0.056***	0.029*	0.039	0.019	0.008
	(0.031)	(0.021)	(0.016)	(0.027)	(0.027)	(0.024)
Dep. mean	0.034	0.017	0.008	0.023	0.022	0.015
Observations	1572	1745	2606	2070	1723	1660

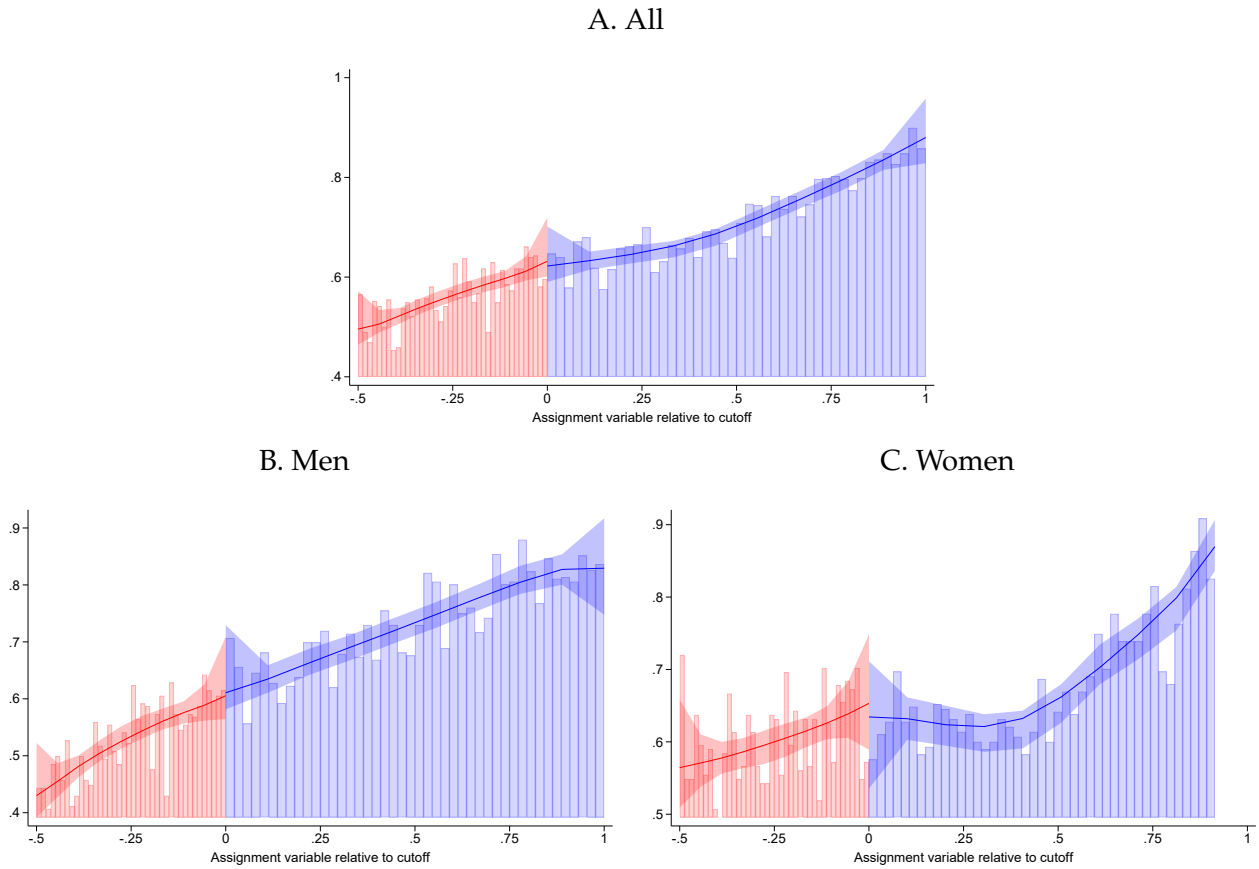
II. By Labor Market Condition

	A. High unemployment market			B. Low unemployment market		
	(1) > 400 days	(2) > 500 days	(3) > 600 days	(1) > 400 days	(2) > 500 days	(3) > 600 days
Estimated effect	0.067***	0.061***	0.037*	0.067**	−0.002	0.002
	(0.025)	(0.021)	(0.019)	(0.034)	(0.023)	(0.019)
Dep. mean	0.019	0.010	0.005	0.007	0.009	0.004
Observations	2252	2517	2032	851	707	974

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Bias-corrected and heteroskedasticity-robust standard errors in parentheses. *Dep. mean* refers to the mean below the cutoff, i.e., the average of the dependent variable among the untreated. All samples are aged 20 to 61 at the time of initial UI application and exclude the partially self-employed.

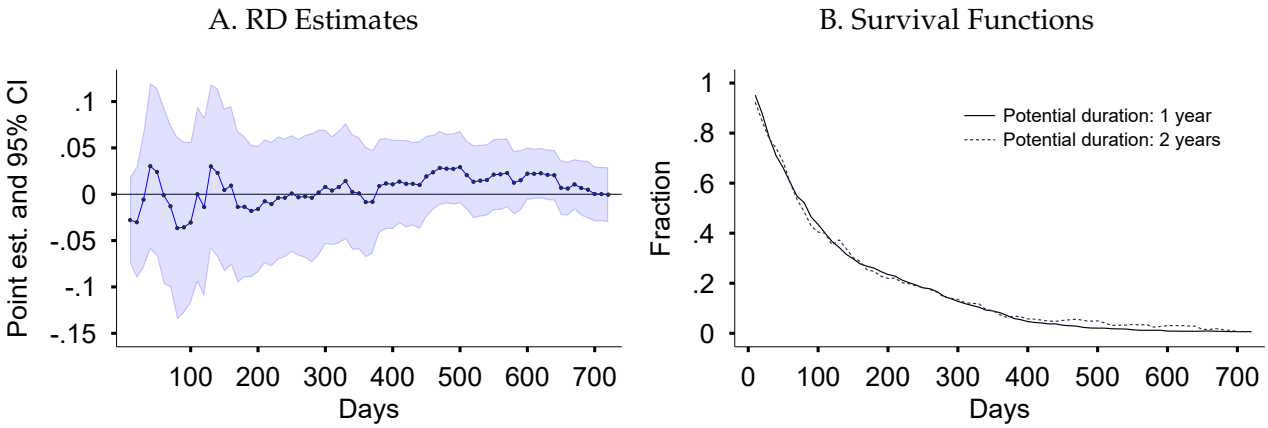
A.2 Additional Figures

Figure A1: Density of Assignment Variable



Notes: This figure presents the density of the assignment variable for all workers eligible for unemployment benefits (Panel A) and separately by gender (Panels B and C). The eligibility cutoff for two years of benefits—2G, or twice the basic amount—is normalized to zero. At the time of their initial UI application, the sampled workers have previous earnings between 1.5G and 3G, are between 20 and 61 years old, and are not self-employed. All estimation is conducted nonparametrically, with optimal bandwidths selected separately on each side of the cutoff, following Cattaneo et al. (2020).

Figure A2: Effects on Unemployment Duration Among Women



Notes: Panel A plots the point estimates and 95% confidence intervals of the effects of the UI extension on unemployment duration among women. The outcomes are indicator variables equal to 1 if a worker remains unemployed for more than 10 days, 20 days, and so on, up to 720 days. Panel B plots the corresponding survival functions of unemployment duration, distinguishing between the untreated (*Potential duration: 1 year*) and the treated (*Potential duration: 2 years*). The gaps between the two curves represent the treatment effects. In both panels, the key long-term unemployment thresholds (100, 200, …, 700 days) are marked on the x-axis. The sample includes women aged 20 to 61 at the time of initial UI application and excludes the fully and partially self-employed.

B A Two-Way Fixed Effect Model of Unemployment

We decompose the duration of unemployment into additive fixed effects that represent the heterogeneity of workers and firms, in the spirit of the wage decomposition of Abowd et al. (1999, henceforth AKM), but focusing on a two-way model for unemployment duration. Following Lachowska et al. (2025), we include a zero for a worker who does not experience a spell of unemployment and calculate the number of days a person has received UI benefits in a given year as d_{ijt}. To estimate both an individual i's and a firm j's contribution, we require a sample of workers who experience job loss from at least two employers (i.e., switchers). The sample of switchers identifies the firm effect and allows us to control for the average quality of employees across different firms. We estimate the following AKM duration model

$$d_{it} = \alpha_i + \phi_{j(i,t)} + r_{it}, \tag{B1}$$

using where α_i is a time-invariant person effect for worker i, and $\phi_{j(i,t)}$ is the permanent firm fixed effect of firm j that employs i in year t. The time-varying residual component r_{it} is assumed to be uncorrelated with worker mobility.

We estimate the model based on the full sample of workers, their employers, and all unemployment spells from 2001 to 2017. We interpret worker effects as a proxy for the unobserved quality of workers and firm effects as a proxy for the firm's contribution to long-term spells arising from differences in the use of temporary layoffs, the use of specific human capital, and overall riskiness from other factors, such as competition or corporate financing decisions.

C Machine Learning for Individual Heterogeneity

Our sample of spells includes all UI claims between 2002 and 2012. To determine the maximum depth of the tree, the maximum number of splitting features, and the number of bootstrapped trees, we used half of the sample. We use predetermined variables as predictors, i.e., variables measured prior to the unemployment spell in question, summarized in Table C1 below.

Table C1: Variables Included in Baseline Random Forest Model of LTU

Socio-demographics	Income (last 3 years)	Employment (last 3 years)	Industry and region
Educational attainment	Labor income	Number of unemployment spells	2-digit industry code
Age	Non-labor income	Days on UI	Region
Gender	Household income	Employment status	
Marital status		Number of employers	
Number of kids with child benefits		Pre-unemployment job tenure	
Years since immigration		Pre-unemployment firm size	
Immigration status			

Notes: Income and employment histories include variables capturing annual income and employment characteristics during the three years before the unemployment spell. Firm size is measured by workforce and regions, referring to Norwegian counties.

These variables include background characteristics, sector of employment and area of residence prior to a given spell (two-digit industry code and region), income history (annual individual income, both wage, and non-wage, as well as annual total household income), and employment history (number of unemployment spells, days on UI, employment status, number of employers, job tenure at the pre-unemployment firm, and the size of this firm). We measure income and employment histories for up to three years before the spell.

The predictions are based on the 2006 sample. There is a strong correlation in the yearly estimates of LTU risk, with pairwise Pearson correlation coefficients in the vicinity of 0.6-0.7. We report the explanatory power of the model and the relative importance of the different types of predictors in the Appendix Table C2. Panel A tabulates R^2 from regressing observed LTU on predicted LTU in the hold-out sample when the other groups of predictors are added to the model one after another (sequential contribution). At the same time, Panel B displays the same statistics when these groups are paired with socio-demographics one at a time (marginal contribution).

Consistent with Mueller and Spinnewijn (2023), we find that employment history has the strongest explanatory power on LTU risk. Panel A shows that married immigrants, fathers, and highly educated men are more likely to be in the high-risk group. Panel B shows a similar but less pronounced relationship for women, where there is barely any difference in the number of children in the high and low-risk groups. Perhaps surprisingly, we find no clear evidence that the high- and low-risk groups differ by income and wealth.

Table C2: R^2 for Random Forest Models of LTU (12 Months) - Training Year: 2006

	A. Sub-models of baseline - Sequential			
	(1)	(2)	(3)	(4)
R^2 $(LTU12_{Predicted}, LTU12_{Observed})$	0.00000	0.00006	0.00467	0.00485
Socio-demographics	Y	Y	Y	Y
Income history		Y	Y	Y
Employment history			Y	Y
Industry & region				Y
	B. Sub-models of baseline - Marginal			
	(1)	(2)	(3)	(4)
R^2 $(LTU12_{Predicted}, LTU12_{Observed})$	0.00000	0.00006	0.00180	0.00138
Socio-demographics	Y	Y	Y	Y
Income history		Y		
Employment history			Y	
Industry & region				Y

Notes: This table shows the R^2 from regressing a dummy for actual unemployment on a dummy for predicted unemployment in the hold-out sample for the year 2006 for various models. Panel A starts from the basic model with only socio-demographic predictors in Column 1. Moving rightwards, each column sequentially incorporates an additional variable group into the model. Panel B adds the same variable groups one at a time.