Evaluating Unemployment Insurance in a Developing Economy: Evidence from Colombia*

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Abstract

This paper examines the impact of unemployment insurance (UI) on labor market outcomes in Colombia, a developing economy with high informality but a uniquely enforced UI system in which only formal worker are eligible for the benefits according to their contributions to the social security system. Using administrative matched employer–employee records, we exploit a regression discontinuity design around the eligibility threshold. To address potential endogeneity, we focus on workers who lost jobs due to firm closures. Our results show that UI eligibility increases the duration of unemployment spells, but does not significantly affect reemployment wages. However, eligible individuals tend to secure longer-lasting jobs and are more likely to be reemployed in higher-paying firms, suggesting improved job match quality in non-wage dimensions.

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1 Introduction

Developing economies often face significant credit and insurance market failures, limiting individuals' ability to manage financial risks effectively. Unemployment insurance (UI) can play a crucial role in enhancing the welfare of workers by smoothing consumption during periods of joblessness and addressing credit constraints. By providing financial support, UI enables unemployed individuals to take the time needed to search for better job opportunities, potentially improving job match quality and overall productivity (Acemoglu and Shimer, 1999; Tatsiramos, 2009).

However, the design of unemployment benefits must carefully balance providing income security with preserving incentives to seek employment actively. In developing economies, this challenge is heightened by a greater risk of moral hazard, as weak monitoring and enforcement systems, combined with the prevalence of informal job opportunities, can reduce the motivation to pursue formal employment. Designing effective UI policies in these contexts requires strategies that minimize disincentives while offering adequate protection for the unemployed.

This study examines the impact of UI on labor market outcomes in Colombia, using employer–employee matched administrative records from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA). A key feature of Colombia's UI system is its strict enforcement of employment status. UI contributions are deducted directly from the wages of formal workers, which reduces the possibility that informal workers could make fraudulent claims. Workers are eligible for UI if they have contributed to the system for at least 12 of the 36 months preceding job loss.

To estimate the causal effect of UI in Colombia, we exploit the eligibility rule that grants benefits only to workers who have contributed for at least 12 of the 36 months preceding job loss. This criterion creates a sharp cutoff in access to UI, which we analyze using a regression discontinuity design (RDD). The identifying assumption is that both observed and unobserved determinants of labor market outcomes vary smoothly around the threshold, so the discontinuous change in benefit eligibility can be attributed to the policy. To address potential endogeneity concerns—since unemployment may depend on individual characteristics—we restrict attention to workers who lost their jobs due to firm closures, which are plausibly unrelated to worker attributes.

We use PILA data to identify these firm closures, which we treat as external shocks to employment. For each affected worker, we calculate total contributions over the prior 36 months to determine UI eligibility. Our strategy therefore combines the exogeneity of firm closures with the quasi-random assignment generated by the 12-month contribution cutoff. We further show that results are robust to alternative settings involving plausibly exogenous separations, such as mass layoffs. The findings indicate that UI eligibility prolongs unemployment spells but does not increase reemployment wages. However, eligible workers remain employed longer in their subsequent jobs, suggesting that benefits improve job stability even if they do not raise initial post-unemployment wages.

To further examine the extent to which unemployment benefits improve the quality of subsequent employment, we analyze whether eligible workers are more likely to reenter the labor market in firms typically regarded as higher quality. Specifically, we test whether workers above the eligibility threshold exhibit a greater probability of reemployment in firms characterized by higher average wages and larger workforce size. The results suggest that, in the Colombian context, access to UI may attenuate job search intensity, while enhancing job match quality primarily along non-wage dimensions.

The remainder of this paper is organized as follows. Section 2 describes the contribution of our paper to the empirical literature on the effects of UI on labor market outcomes. Section 3 provides contextual background by outlining the functioning of UI in Colombia. Section 4 describes the data sources, with particular attention to the characteristics and relevance of the PILA dataset. Section 5 presents the empirical strategy, and Section 6 discusses the main results. Finally, Section 7 concludes and offers policy recommendations.

2 Literature Review

The literature on UI has long emphasized the tension between two opposing effects on the short-term unemployed: a moral hazard effect, whereby benefits reduce job search effort, and an income effect, whereby benefits relax liquidity constraints and thereby allow greater search effort. The net effect on unemployment duration is thus theoretically ambiguous (Bardey et al., 2015). Evidence of the moral hazard effect was first documented in seminal studies such as Moffitt (1985), which showed that unemployment exit rates spike immediately before benefits expire. While both the moral hazard and liquidity-constraint mechanisms imply longer unemployment spells, the latter suggests an improvement in match quality, as workers face less pressure to accept low-quality jobs. From a policy perspective, alleviating liquidity constraints through UI can therefore be desirable (Card et al., 2007). This mechanism implies that UI may lead to higher entry wages and longer job tenure, among other indicators of improved match quality. Recent work further suggests that easing liquidity constraints may generate positive feedback effects on job creation: by encouraging workers to search for higher-productivity jobs, UI increases firms' incentives to create such jobs (Acemoglu and Shimer, 2000). A key empirical contribution of this paper is to show that eligibility for UI benefits significantly increases the likelihood of reemployment in higher-productivity firms.

This paper contributes to the extensive literature estimating the effects of UI on the job-finding rate of unemployed workers. Examples include Katz and Meyer (1990b), Katz and Meyer (1990a), Dickens et al. (1999), and Bover et al. (2002), among others. The consensus in this literature is that UI exerts a significant negative effect on job-finding rates, thereby increasing the duration of unemployment spells. Consistent with these findings, our results indicate that UI eligibility significantly lengthens the unemployment duration of eligible workers. By contrast, the evidence on the quality of reemployment—measured in terms of job characteristics or wages—is far more limited and generally does not support positive effects of UI (Card et al., 2007; Lalive, 2007; Schmieder et al., 2016). A small number of studies do find positive effects of UI generosity or extensions on wages (Centeno and Novo, 2009; Nekoei and Weber, 2017). Our findings show no significant impact on entry wages or average wages following unemployment for workers above the eligibility threshold. However, we extend the literature by considering alternative labor market outcomes that capture aspects of job-match quality, such as tenure in the first post-unemployment job and the probability of reemployment in higher-productivity firms. Along these dimensions, we document positive and significant effects of UI.

Our findings also speak to the literature on UI in developing economies characterized by high rates of informality. A central concern in these contexts is that the coexistence of formal and informal labor markets may alter both the incentives and the general equilibrium effects of UI. For instance, Bosch and Esteban-Pretel (2015) show, using a search and matching model calibrated to Mexico, that the introduction of UI in an economy with pervasive informality can either increase or decrease formality, depending on the generosity of benefits and the design of contribution requirements. Similarly, Van Doornik et al. (2018) exploit an unanticipated reform in Brazil to document collusion between firms and workers, who use the informal sector strategically to extract rents from the UI system. These dynamics highlight how informality can amplify unintended behavioral responses to UI. Other studies emphasize the potential benefits of adapting UI schemes to informal labor markets. Cirelli et al. (2021) demonstrate that introducing UI savings accounts (UISA) in middle-income economies can reduce unemployment, increase formality, and generate welfare gains. Likewise, Gerard and Gonzaga (2021) challenge the view that informality necessarily raises the efficiency costs of UI, showing instead that such costs may be attenuated in economies where formal reemployment rates are already low.

Although our paper does not directly test the interaction between UI and informality, our results contribute to this literature in two ways. First, by focusing on Colombia—a country with one of the highest informality rates in Latin America—we provide new evidence on the effects of UI in precisely the type of environment where these concerns are most salient. Second, our findings show that UI not only lengthens unemployment spells but also improves job-match quality along non-wage dimensions, such as tenure and reemployment in higher-productivity firms. This suggests that even in labor markets with widespread informality, UI can generate efficiency gains by fostering more stable and productive employment relationships. In this sense, our contribution complements the existing literature by highlighting a channel through which UI may improve labor market outcomes in developing economies, despite the challenges posed by informality.

3 Unemployment Insurance in Colombia

3.1 Context on Colombian Labor Market

The Colombian labor market exhibits distinctive features even relative to other economies in the Caribbean and Latin America. Notably, Colombia consistently records one of the highest unemployment rates in the region. According to Banco de la República, Grupo de Análisis del Mercado Laboral (2025), during the period 2023–2025 Colombia's unemployment rate was approximately 3 percentage points above the average of the largest Latin American economies (Mexico, Argentina, Brazil, Peru, and Chile) highlighting its comparatively weak labor market performance within this group.

Beyond persistently high unemployment, the Colombian labor market is also characterized by a high prevalence of informality and self-employment, both exceeding regional averages (Banco de la República, Grupo de Análisis del Mercado Laboral, 2025; Otero-Cortés, ed, 2025). In 2025, the national informality rate stood at 56 percent, and during the pandemic in 2021 it reached 62 percent (Grupo de Análisis del Mercado Laboral, 2025).

High unemployment and informality in Colombia have been widely linked in the literature to structural rigidities in the formal labor market and to relatively high labor costs. A central element in this dynamic is the minimum wage, which exceeds 85% of the national average wage and is strongly binding: approximately 40% of formal workers earn at or near the minimum wage (Becerra-Camargo and Morales-Zurita, 2025; Flórez et al., 2021). On the demand side, the structure of firms further shapes these labor market outcomes. The formal sector is predominantly composed of small and medium-sized enterprises (SMEs), with 90% of firms employing fewer than 50 workers. Despite their numerical dominance, however, these firms account for only 30% of formal employment (Flórez et al., 2021).

To address these structural constraints, a series of labor market reforms were implemented over the past decade, many of which overlapped temporally with the unemployment insurance (UI) policies examined in this study. Importantly, however, the eligibility criteria for these reforms differed substantially from those governing access to UI.

For instance, between 2010 and 2013, a policy targeted youth and first-time job seekers through payroll tax reductions (Becerra-Camargo and Morales-Zurita, 2025). Between 2012 and 2014, broader tax reforms reduced effective payroll tax rates across firms and facilitated contributions for part-time workers (Samaniego de la Parra et al., 2024; Fernández and Villar, 2017; Kugler et al., 2017; Morales and Medina, 2017). More recently, between 2021 and 2024, a wage subsidy program was introduced to support payroll costs, with a particular focus on SMEs and on young and female workers (Bonilla-Mejía et al., 2025).

Crucially, none of these initiatives determined eligibility on the basis of accumulated social security contributions—the key assignment variable in our empirical design. For this reason, we argue that these concurrent reforms are unlikely to confound or bias the estimates presented in this paper.

3.2 Establishment of the Unemployed Protection Mechanism (MPC) in 2013

In 2013, Colombia introduced Law 1636, which created the Unemployed Protection Mechanism (*Mecanismo de Protección al Cesante*, MPC) alongside the Public Employment Service (*Servicio Público de Empleo*, PES), with the aim of coordinating passive and active labor market policies. The MPC incorporates an economic benefits component designed to mitigate the adverse effects of unemployment on household income. Eligible workers may receive benefits for up to six months, including contributions to the health and pension systems, as well as the family subsidy (a monthly cash transfer provided for each dependent). Access to these benefits is conditional on compliance with active labor market policies, such as participation in training programs and the requirement to apply for job vacancies offering at least 80% of the worker's previous salary.

To qualify for the MPC, employees must have contributed to a Family Compensation Fund (*Caja de Compensación Familiar*, CCF) for at least twelve months—either continuously or intermittently—within the past three years. Independent workers must meet a higher threshold, requiring at least twenty-four months of contributions within the same period, and must have formally terminated their employment contract. In both cases, eligibility is contingent upon the worker having ended their employment relationship, regardless of the reason, and not having any alternative source of income.

CCFs are private, non-profit entities that administer a component of Colombia's social security system. Employers are required to contribute a payroll tax equivalent to 4% of workers' wages, which finances the benefits managed by the CCFs. These benefits include monthly family subsidies for dependents, access to health, education, recreation, and housing programs, as well as the administration of unemployment protection schemes such as the MPC. CCFs are part of the Employment Service Network (*Red de Prestadores del Servicio Público de Empleo*), which brings together both public and private providers. All providers in the network are prohibited from charging fees to job seekers and operate under a licensing system designed to ensure compliance with operational standards and legal requirements. Oversight is carried out by the Special Administrative Unit of the Public Employment Service (*Unidad Administrativa Especial del Servicio Público de Empleo*, UAESPE), which develops standardized methodologies, tools, and benchmarks, and provides technical support to providers to promote uniform service quality and close implementation gaps. Within this institutional framework, active labor market policies administered through the PES include supply-side services such as résumé registration, job orientation, candidate pre-selection, and referrals, as well as demand-side services such as vacancy registration and employer guidance on candidate profiling and job position management. In this system, the CCFs serve as the exclusive providers responsible for implementing the MPC.

3.3 Expansion of MPC Benefits in Response to the COVID-19 Pandemic (2020-2022)

On March 12, 2020, in response to the economic emergency triggered by the COVID-19 pandemic, the Colombian government expanded the scope of the MPC. Workers who had contributed to a CCF for at least one year—either continuously or intermittently—within the previous five years became eligible, in addition to the benefits established under Law 1636 of 2013, to receive a direct cash transfer intended to help cover basic expenses according to individual needs and consumption priorities. The transfer was equivalent to twice the prevailing legal monthly minimum wage (SMMLV), distributed in three equal monthly installments, and was available for a maximum of three months while the state of emergency was in effect. The state of emergency remained effective until June 30, 2022.

To ensure continuity of support measures and to promote employment recovery in the aftermath of the pandemic, the Ministry of Labor enacted Law 2225 of 2022, which amended Law 1636 of 2013 with respect to the economic benefits of the MPC. The reform introduced a new direct cash transfer equivalent to 1.5 SMMLV, provided over a four-month period on a declining scale: 40% of 1.5 SMMLV in the first month, 30% in the second, 20% in the third, and 10% in the fourth. Eligibility was restricted to workers earning less than four SMMLV. In addition, contributions to the social security system for health and pensions—calculated on the basis of one SMMLV—were maintained for up to six months for all eligible workers.

4 Data and Summary Statistics

Our estimations of the effects of the UI protection program are based on employer–employee-linked administrative records from the Colombian Social Security System.

In Colombia, both self-employed workers and formal-sector firms are required to report their contributions through a centralized system known as the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA). The PILA provides monthly information on wages, employment status, and detailed characteristics of both workers and firms. This allows us to track workers' movements across employers, their entries into and exits from the formal labor market, as well as firms' dynamics, including entry, size, and exit.

The dataset is extensive, covering more than 450,000 firms and approximately 13.5 million workers in an average year during our study period. For the purposes of identification, however, we restrict the sample to reduce potential self-selection into treatment. A concern is that workers might voluntarily leave their jobs after securing eligibility for UI benefits, possibly as a strategy to climb the job ladder. To address this issue, we construct our estimation sample exclusively from workers whose separation results from firm closures, ensuring that job loss is involuntary. We define a firm closure as its disappearance from the PILA records for at least 24 consecutive months, following a presence of at least 12 consecutive months. This definition minimizes the risk of misclassification and strengthens the credibility of our identification strategy by reducing bias from endogenous worker exits.

In this study, we examine two institutional regimes of the layoff protection mechanism implemented in Colombia. The first corresponds to the framework established by Law 1636 of 2013, which remained in force without substantive modification until March 2020, when temporary measures were introduced in response to the COVID-19 crisis. The second regime reflects the modified design implemented between 2020 and 2022, incorporating the adjustments enacted to address the economic emergency triggered by the pandemic. Accordingly, our empirical analysis distinguishes between two policy periods. The first spans April 2013 to December 2019 and captures the original design of the program (Policy 1). The second covers March 2020 to June 2022 and corresponds to the pandemic-adjusted version of the scheme (Policy 2).

Table 1 reports summary statistics for the estimation samples corresponding to the first version (2013–2019) and the second version (2020–2022) of the employment protection policy. The samples consist of workers in PILA who lost their jobs due to firm closures, as defined above. Overall, the descriptive statistics display remarkably similar patterns across the two policy periods.

Columns (1) and (2) present statistics for the 2013–2019 period. During this period, eligible workers experienced shorter unemployment spells after the shock (2.4 months on average) compared with 4.1 months for non-eligible workers. Average log wages are similar across the two groups (13.7 vs. 13.6). However, eligible workers remained in their first post-shock job longer (13.6 months on average) relative to non-eligible workers (8.9 months). At the time of separation, most workers in both groups were employed in the service sector (82%). Eligible workers also accumulated, on average, 25.5 months of contributions prior to job loss.

Eligible workers are older, as expected given the accumulation requirement. There is a similar proportion of male workers among eligible and non-eligible workers. Since workers above the eligibility threshold experience shorter unemployment spells, they remain employed for a larger proportion of the post-unemployment period relative to non-eligible workers. After unemployment, eligible workers are less likely to change industry or city of residence compared to non-eligible workers. They are equally likely to have been employed in a large firm prior to the shock.

Turning to the period following the implementation of the second policy, columns (3) and (4) of Table 1 show a very similar pattern. Eligible workers again display shorter unemployment spells than non-eligible workers (2.1 months vs. 3.3 months) and comparable log wages (14.0 vs. 13.9). In addition, they remain in their first post-shock job for an average of 9.9 months, compared to 6.6 months for non-eligible workers. Differences in age, gender composition, sectoral distribution, mobility, and firm size closely mirror those observed in the earlier period.

5 Empirical Strategy

After restricting the sample to workers who lost their jobs due to firm closures, as described in the previous section, we compute each worker's total Social Security contributions over the 36 months preceding the closure. This contribution history is central to our empirical strategy, as it determines eligibility for UI benefits. In Colombia, workers qualify for UI only if they have contributed for at least 12 months within the 36 months prior to job loss.

Our findings should therefore not be interpreted as externally valid for all types of job separations, whether voluntary or involuntary. Workers displaced by firm closures may differ systematically from the broader population of individuals who separate from employment.[1] Nevertheless, we can argue in favor of the internal validity in the cases we study in this paper: firm closures and massive layoffs.

We employ a standard regression discontinuity design (RDD) in which treatment assignment is determined by a running variable with a clearly defined cutoff. In our case, the running variable is the cumulative number of months of social security contributions at the time of layoff, and the treatment is eligibility for UI. Workers with at least 12 months of contributions within the relevant period qualify as eligible workers, making the 12-month threshold the cutoff in our estimation. The key identifying assumption is that workers cannot precisely manipulate their contribution histories to sort around the threshold. Under this assumption, workers just below and just above the 12-month cutoff are statistically comparable. Consequently, the discontinuous change in the probability of treatment at the threshold can be interpreted as quasi-random, allowing for credible causal inference on the impact of UI eligibility on subsequent labor market outcomes (Cattaneo et al., 2020; Lee and Lemieux, 2010). Appendix Figure 1 illustrates our identification strategy and the distinction between eligible and non-eligible workers.

It is important to note that the 12 months of contributions can be accumulated in fractions of a month over a worker's employment history. In practice, this means that employees must demonstrate contributions equivalent to 360 days within the three years preceding job loss to qualify as eligible workers. As a result, the running variable (the total number of months of contributions) is continuous rather than discrete, which increases its variability around the cutoff. To estimate the causal effect of UI eligibility, we therefore use the total contributions accumulated during the three years prior to the unemployment shock, as recorded in the PILA, and compare workers just below the 12-month threshold with those just above it.

Let $x_{it} = contributions_{it} - c$ denote the running variable centered at the eligibility threshold c. Define the treatment indicator as $T_{it} = \mathbf{1}(x_{it} \geq 0)$, which equals 1 if individual i, who experiences a layoff at time t, has accumulated at least $c = 12$ months of contributions and is therefore eligible for UI.

[1] Appendix Table 1 reports summary statistics for a random sample of individuals who experience job separation during our study period. A comparison of Table 1 and Appendix Table 1 reveals differences across several dimensions. For example, unemployment spells following firm closures are shorter than those observed after a random separation in both post-policy periods. However, employment duration in the first job after separation is longer among workers displaced by firm closures than among those experiencing general separations.

Following the standard approach in the regression discontinuity literature, we estimate the local treatment effect using a weighted least squares regression of the form:

$$Y_{it} = \alpha + \delta_{RD}\, T_{it} + \beta\, x_{it} + \tau\,(x_{it} \cdot T_{it}) + \gamma Z_i' + \varepsilon_{it}. \tag{1}$$

where Y_{it} denotes the labor market outcome of interest. In our application, these include the duration of unemployment spells and the characteristics of the first job obtained after the layoff shock, such as the entry wage, job tenure, and the economic sector of reemployment, among others. The vector Z_i contains predetermined covariates, specifically the worker's age and sex, as well as characteristics of the pre-layoff firm, including firm age and industry fixed effects. Standard errors are clustered at the individual age level, as age is a key determinant of job search outcomes and may induce correlation in the error term across observations within the same age group.

Consistent with standard practice in the regression discontinuity literature, equation (1) is estimated locally around the cutoff. Specifically, we restrict the sample to observations within a bandwidth h of the eligibility threshold, that is, $|x_{it}| \leq h$, and estimate the model using weighted least squares with a triangular kernel that assigns greater weight to observations closer to the cutoff. Bandwidth selection follows a mean squared error–optimal procedure following Cattaneo et al. (2024), and inference is based on robust bias-corrected standard errors.

Within the chosen bandwidth, this specification provides a local linear approximation to the conditional expectation function on either side of the cutoff and permits the slope of the outcome with respect to contributions to differ above and below the eligibility threshold. For individuals with $x_{it} < 0$, the conditional expectation of Y_{it} is locally linear in the running variable with slope β, whereas for individuals with $x_{it} \geq 0$, the local slope is $\beta + \tau$. Under the standard continuity assumption of the regression discontinuity design, the causal effect of UI eligibility at the cutoff is given directly by δ_{RD}, which measures the discontinuity in the conditional expectation of Y_{it} at $x_{it} = 0$.

Importantly, our empirical strategy is designed to identify the effect of the institutional eligibility rule, rather than the effect of individual benefit receipt. The object of interest in this paper is the causal impact of crossing the statutory contribution threshold that defines access to UI. This distinction is central. Eligibility is determined by a formal institutional rule set by policymakers, whereas benefit take-up is an endogenous behavioral response that depends on individual preferences, information, and transaction costs, among others. Because policymakers directly control eligibility criteria but cannot directly mandate take-up, the relevant policy lever in our setting is the eligibility threshold itself. The sharp RD therefore recovers the intention-to-treat (ITT) effect of institutional design on labor market outcomes.

By contrast, a fuzzy RD design would identify a local average treatment effect (LATE) for compliers, those individuals whose benefit receipt status is altered by crossing the eligibility cutoff. While informative in other contexts, this parameter would shift the focus from institutional rules to behavioral compliance. Interpreting such a LATE would require modeling heterogeneity in take-up decisions and understanding why some eligible workers claim benefits while others do not. This is a different question from the one we address. Our objective is to quantify how the eligibility rule itself shapes post-layoff outcomes among workers at the margin of qualification, regardless of the specific behavioral channel through which the effect operates.

Accordingly, the discontinuity we estimate should be interpreted as the reduced-form impact of institutional access to UI. This parameter captures the total equilibrium response to the policy rule (including both benefit receipt and any anticipatory or search-related behavioral adjustments induced by eligibility) and therefore corresponds directly to the effect of the policy instrument available to regulators. In the sections that follow, we explore heterogeneity in these effects and examine post-layoff behavior in order to shed light on the mechanisms through which eligibility shapes labor market outcomes. While our design does not separately identify the causal effect of benefit receipt, analyzing differential responses across groups and outcomes allows us to provide evidence on the behavioral channels underlying the estimated eligibility effects.

5.1 Validity of Regression Discontinuity Assumptions

A key requirement for the validity of the regression discontinuity design is that predetermined characteristics—both observable and unobservable—evolve smoothly at the cutoff. In this context, workers just below and just above the 12-month contribution threshold should be comparable in expectation. Any discontinuous change in covariates at the threshold would cast doubt on the identifying continuity assumption.

To assess this condition empirically, we re-estimate equation (1) using each predetermined covariate as the dependent variable. The results are reported in Table 2. For both

Policy 1 and Policy 2, we find no statistically significant discontinuities at the 5% level in any of the observable characteristics within the optimal bandwidth.

These findings provide support for the identifying assumption that workers marginally below and above the eligibility threshold are comparable. In particular, we find no evidence that eligible and non-eligible workers differ systematically in the observable covariates included as controls in our main specification.

Another key identifying assumption of the local regression discontinuity design is that individuals cannot precisely manipulate their position relative to the cutoff. In our setting, this requires that workers are unable to strategically adjust their accumulated contributions in order to sort just above the 12-month eligibility threshold. If such manipulation were present, the comparability of observations around the cutoff would be compromised.

A standard diagnostic in the regression discontinuity literature is to examine whether the density of the running variable exhibits a discontinuity at the threshold. In the absence of sorting or manipulation, the distribution of the running variable should evolve smoothly across the cutoff.

We conduct two validation exercises. First, Figure 1 presents a graphical analysis of the density of the running variable (total contributions during the 36 months prior to layoff) for the two policies under analysis. Visual inspection reveals no evidence of excess mass at the 12-month cutoff relative to adjacent values for either policy. Second, we formally test for manipulation using the density discontinuity test proposed by Cattaneo et al. (2024). This procedure evaluates whether there is a statistically significant change in the density of the running variable at the cutoff. In both Policy 1 (2013–2019) and Policy 2 (2020–2022), we fail to reject the null hypothesis of continuity at conventional significance levels, providing no evidence of sorting around the threshold.[2]

[2] For Policy 1 (2013–2019), the test statistic is 0.4548 with a *p*-value of 0.6492. For Policy 2 (2020–2022), the test statistic is 0.6275 with a *p*-value of 0.5304.

6 Results

6.1 Results of UI Eligibility: 2013–2019

We begin with graphical evidence from the regression discontinuity design. Figure 2 presents the corresponding RD plots for the main post-displacement outcomes. The figure shows a clear discontinuity in unemployment duration at the cutoff, indicating that eligible individuals remain unemployed for longer. At the same time, there is a positive jump in the duration of the first post-unemployment job, suggesting that eligible workers subsequently experience more stable employment relationships. By contrast, there is no visible discontinuity in either entry or average wages. Taken together, these patterns are consistent with a job search mechanism in which UI eligibility relaxes liquidity constraints, allowing workers to search longer and form more stable matches, even in the absence of wage gains. We next turn to formal estimates of equation (1) to quantify these effects.

Table 3 reports the effects of UI eligibility on key post-closure outcomes for the 2013–2019 period. We examine four outcomes: (i) unemployment duration, (ii) log entry wages, (iii) log average wages following re-employment, and (iv) the duration of the first formal job after the shock. For each outcome, we report estimates with and without additional covariates. Across specifications, UI eligibility significantly increases unemployment duration: eligible workers remain unemployed about 1.3 months longer than non-eligible workers, on average. We find no statistically significant effects on either entry or average real wages, indicating that UI does not improve matches along the wage dimension. However, eligibility is associated with a significant increase in the duration of the first post-unemployment job. Bias-corrected estimates indicate that eligible workers remain in their initial job for approximately 0.7 additional months. This pattern is consistent with UI extending job search and improving match quality along non-wage dimensions, such as job stability.

Table 4 examines the characteristics of firms in which workers are reemployed following unemployment, providing further insight into match quality. We analyze the probability of working in firms that pay above the median wage and in large firms (more than 50 employees), as well as the likelihood of switching industries and relocating across cities.[3]

[3] Appendix Table 2 presents summary statistics at the firm level for the census of formal firms from

We find that UI eligibility increases the probability of reemployment in firms with above-median wages by 1.4 percentage points, although it has no significant effect on the likelihood of entering large firms. In addition, eligibility raises the probability of switching industries, suggesting improved sorting into more suitable jobs. By contrast, UI reduces geographic mobility: eligible workers are less likely to transition to a job in a different city, although this effect is relatively modest in this period. Anecdotal evidence suggests that enforcement of program requirements was relatively weak during the early stages of the program, which may help explain the limited impact on mobility.

6.2 Results of UI Eligibility after the Expansion of Benefits: 2020–2022

Figure 3 presents the corresponding RD plots for the 2020–2022 period. The graphical evidence closely mirrors the earlier period: we observe a positive discontinuity in both unemployment duration and post-unemployment job duration, and no visible effects on wages.

We next present formal estimates for Policy 2, implemented starting in 2020. Table 5 reports the effects of UI eligibility following the expansion of benefits, under which eligible workers could receive transfers equivalent to 1.5 minimum wages over a four-month period. Despite the increase in generosity, the effect on unemployment duration remains remarkably similar to that in the earlier period, at approximately one additional month (0.97 months with controls and 1 month without controls).

Consistent with the previous period, we find no statistically significant effects on real wages. We do observe a positive effect on the duration of the first post-unemployment job, although the magnitude is smaller than under Policy 1, ranging from approximately 0.44 to 0.47 additional months depending on the specification.

Table 6 examines the characteristics of post-unemployment jobs under Policy 2. As before, UI eligibility increases the probability of reemployment in firms paying above the median wage, with a larger effect of 4.1 percentage points. We again find no significant effect on the probability of entering a large firm.

In contrast to Policy 1, the negative effect on geographic mobility is more pronounced in this period: eligible workers are approximately 3.6 percentage points less likely to

PILA. As the table reveals, most firms (66%) are mature (more than 6 years old), 86% are small (fewer than 20 employees), and only 3% are large (more than 100 employees). The private services sector accounts for 30% of firms, while the primary sector represents 8%.

change cities. One possible explanation is that stronger economic incentives under the reform were accompanied by stricter enforcement of program requirements, particularly the obligation to attend in-person training sessions.

Finally, as in the earlier period, UI eligibility significantly increases the probability of switching industries. This pattern suggests that extended search facilitates reallocation toward more suitable matches, even across sectors.[4]

Taken together, the results indicate that UI eligibility consistently increases unemployment duration in both periods, with effects of approximately one additional month despite the expansion in benefit generosity. We find no evidence of improvements along the wage dimension. However, UI eligibility appears to enhance match quality along non-wage margins: eligible workers experience longer job tenure, are more likely to switch industries, and are more likely to transition into firms paying above the median wage. At the same time, UI reduces geographic mobility, particularly under Policy 2. Overall, the evidence suggests that UI primarily operates by extending job search, thereby improving match quality along non-wage dimensions while influencing workers' mobility and reallocation decisions.

7 Heterogeneity of the results and possible mechanisms

In this section, we examine heterogeneity in the effects of UI eligibility to shed light on the mechanisms underlying the longer unemployment spells observed at the eligibility margin in both policy periods. Specifically, we re-estimate equation (1) separately for workers with pre-layoff wages below the 33rd percentile and for those above the 66th percentile of the annual wage distribution. Workers in the lower-wage group are more likely to face binding liquidity constraints, whereas such constraints are expected to be less relevant for higher-wage workers. Comparing the estimated eligibility effects across these groups allows us to assess whether the extension of unemployment duration operates primarily through the relaxation of liquidity constraints.

While our design does not isolate the effect of benefit receipt per se, examining heterogeneity in responses around the eligibility threshold provides evidence on the behavioral channels through which institutional access to UI affects post-layoff labor market

[4] One possible mechanism operates through training opportunities associated with UI eligibility, which may help workers acquire transferable skills and facilitate sectoral transitions.

outcomes. The results, reported in Appendix Tables 3 and 4 for Policy 1 and Policy 2, respectively, provide suggestive evidence on the role of liquidity constraints in shaping the impact of UI eligibility.

For Policy 1, we find that the effect of UI eligibility on unemployment duration is 1.38 months for the bottom-wage sample, compared to 0.78 months for the top-wage sample. This pattern supports the interpretation that relaxing liquidity constraints is an essential mechanism behind the increase in unemployment spells, since the effect is stronger among lower-wage workers, who are more likely to face binding financial restrictions. Moreover, for non-wage measures of match quality (Appendix Tables 3 and 5), the effects are consistently larger and statistically significant in the bottom-wage group. Specifically, job tenure in the first post-unemployment position increases by 1.1 months. In addition, UI eligibility raises the probability of reemployment in a more productive firm by 3.4 percentage points and increases the likelihood of switching industries. In the top-wage group, we find no effects on job tenure in the first post-unemployment position or on the probability of reemployment in a more productive firm.

Similarly, for Policy 2 (Appendix Tables 4 and 6), we find that the effects of UI eligibility are substantially stronger for workers in the bottom wage tercile than for those in the top tercile. In particular, eligible low-wage workers experience longer unemployment spells and greater tenure in their first post-unemployment job relative to their higher-wage counterparts. Moreover, UI eligibility increases the probability of reemployment in a more productive firm by approximately 9 percentage points for the bottom-wage group, while yielding no statistically significant effect for the top-wage group.

Overall, the heterogeneity results indicate that the effects of UI eligibility are concentrated among lower-wage workers. In both policy periods, the extension of unemployment duration is substantially larger for individuals in the bottom wage tercile, and the improvements in non-wage dimensions of match quality (such as job tenure, transitions to more productive firms, and industry switching) are primarily driven by this group. By contrast, for higher-wage workers, the estimated effects are smaller and generally statistically insignificant.

This pattern is consistent with the relaxation of liquidity constraints as a central mechanism underlying the behavioral responses to UI eligibility. If comparable effects were observed among higher-wage workers (who are less likely to face binding financial constraints) the evidence would be more indicative of a moral hazard channel operating through reduced search effort. Instead, the concentration of effects among lower-wage workers suggests that the extension of unemployment spells primarily reflects improved search facilitated by the easing of liquidity constraints rather than a generalized reduction in search intensity.

We next examine heterogeneity in the effects of UI eligibility by gender. Appendix Tables 7, 8, 9 and 10 report separate estimates for men and women, beginning with Policy 1. We find that the impact of UI eligibility on unemployment duration is substantially larger for men than for women: eligible men remain unemployed approximately 1.5 additional months, compared to 0.74 months for eligible women. This pattern suggests that, if UI benefits relax liquidity constraints, men may be better positioned to translate longer search into improved matches upon reemployment.

Consistent with this interpretation, the results for Policy 1 indicate that male eligible workers experience improvements along several dimensions of match quality. UI eligibility increases their tenure in the first post-unemployment job by roughly 1.1 months, raises the probability of reemployment in a more productive firm by 2.8 percentage points, and increases the likelihood of working in a large firm by 2.3 percentage points, an outcome often associated with higher productivity. In addition, we detect a positive and statistically significant effect on entry wages for men in the specification without controls.

By contrast, we find no evidence of improved matches for female eligible workers under Policy 1. UI eligibility has no significant effect on the duration of their first job after unemployment and is associated with negative and statistically significant effects on the probability of reemployment in both more productive and larger firms. A similar pattern emerges under Policy 2: the increase in unemployment duration remains larger for men than for women, and UI eligibility raises the probability of reemployment in a more productive firm for male workers but not for female workers.

Our findings of differential job market outcomes between men and women are consistent with a growing literature emphasizing gender differences in reservation wages, liquidity constraints, and search behavior. Several studies document that women set lower reservation wages than men (Brown et al., 2011; Caliendo et al., 2017), and experimental evidence shows that this gap persists even when offer distributions and outside options are held constant (McGee and McGee, 2025). Lower reservation wages mechanically reduce search duration and increase the likelihood of accepting earlier — and potentially lower-paying — offers. In addition, women display stronger preferences or constraints regarding job flexibility and commuting distance (Le Barbanchon et al., 2021), which further narrow their effective opportunity set during search. Together, these mechanisms provide a coherent explanation for why women in our setting may transition more quickly into employment but into positions with different wage or job-quality characteristics.

These differences may be amplified by financial and family-related constraints. Casarico et al. (2023) argues that childcare-related liquidity constraints disproportionately affect mothers, generating inefficiencies and widening gender gaps in participation and wages. Empirically, women exhibit lower short-term financial resilience in different countries (Matemane et al., 2025), which may increase the urgency of accepting job offers during unemployment spells. Moreover, the motherhood penalty literature documents that childbirth induces persistent declines in employment, hours, and earnings, alongside shifts toward informal, part-time, and more flexible jobs (Berniell et al., 2021, 2023). These adjustments reflect the higher demand for flexibility following parenthood but often come at the cost of weaker long-term career trajectories. In light of this evidence, our findings can be interpreted as consistent with a framework in which tighter liquidity constraints, lower reservation wages, and stronger flexibility needs interact to produce systematically different labor market outcomes for men and women.

8 Effects of the UI after Massive Layoffs

As described above, our baseline estimation strategy relies on firm closures as plausibly exogenous shocks, independent of individual worker characteristics. However, one potential concern is that some workers may anticipate impending closures and selectively exit declining firms before the formal shutdown occurs. If forward-looking workers with better outside options leave in advance, the set of workers observed at the time of closure may not be fully representative of the original workforce, raising concerns about selection and the external validity of the estimates.

To address this issue, we extend the analysis to examine the effects of UI eligibility in the case of massive layoffs, which, similar to closures, can be viewed as largely exogenous shocks from the worker's perspective but may be less subject to anticipatory sorting. We define a massive layoff as the first period in which a firm's 12th-order moving average of employment falls by at least 30%, provided that the firm does not close within the subsequent 12 months. For workers separated due to such layoffs, we compute their total social security contributions over the 36 months preceding the event. As in the baseline specification, workers are considered eligible for UI benefits if they contributed for at least 12 of those 36 months (see Appendix Figure 1).

For this exercise, we also validate the assumption that eligible and non-eligible workers are statistically similar in the vicinity of the threshold. As in the case of the baseline specification, to assess this assumption empirically, we re-estimate equation (1) using each observable covariate as the dependent variable. The results are presented in Appendix Table 11. While most predetermined characteristics do not differ statistically between eligible and non-eligible workers, we detect discontinuities in a small subset of controls. Under Policy 1, we find differences in age and an indicator for employment in older firms; under Policy 2, we find differences in indicators for employment in older firms and for employment in primary-sector firms. In all specifications, we therefore report results both with and without covariate adjustment, and show that the estimated discontinuity in outcomes is robust to including these controls. Finally, as in the case of the baseline specification, we formally assess continuity at the threshold using the manipulation test proposed by Cattaneo et al. (2024). The test results fail to reject the null of no manipulation, supporting the validity of the design.[5]

We then re-estimate equation (1) following the approach outlined in Section 3. Results for Policy 1 are presented in Panel A of Appendix Table 12. Overall, the findings are broadly consistent with the baseline estimates. Following a massive layoff, UI eligibility significantly increases the duration of unemployment spells by about 1.5 months. Additionally, it lengthens the duration of the first post-unemployment job by 0.6 months, decreases the likelihood of reemployment in a different city, and increases the probability of switching industries. Some differences emerge relative to the baseline: there is no significant effect on the probability of reemployment in more productive firms, but we do find a positive and significant effect (1.4 percentage points) on the probability of being hired by a large firm (more than 50 employees). Moreover, unlike in the baseline, we detect a small but statistically significant effect on entry wages (0.88 percentage points).

The results for Policy 2, presented in the Panel B of Appendix Table 12, likewise resemble those from the baseline. After a massive layoff, UI eligibility lengthens unem-

[5]For Policy 1 (2013-2019), the test statistic is -1.009 with a p-value of 0.3126. For Policy 2 (2020-2022), the test statistic is 0.8327 with a p-value of 0.4050.

ployment spells by about 0.54 months. We find no significant effects on entry or average real wages; however, eligibility is associated with improvements in job-match quality along other dimensions. Specifically, it increases the probability of reemployment in firms with above-median wages (2.2 percentage points) and in firms from a different industry (2.3 percentage points). However, it does not affect the likelihood of relocating to a different city.

While workers separated due to closures and those affected by massive layoffs may differ in unobservable ways, the results across both settings point to a consistent pattern: UI eligibility increases unemployment duration but also improves matching quality. These improvements are reflected in longer tenure in the first post-unemployment job, a higher probability of reemployment in larger or better-paying firms, and, in some cases, modest gains in entry wages, as observed under Policy 1 following massive layoffs.

9 Conclusions

Our analysis shows that unemployment insurance (UI) subtly but meaningfully shapes the labor market outcomes of workers affected by firm closures in Colombia. We find that UI eligibility prolongs unemployment spells, particularly among workers in the lower end of the wage distribution at the time of job loss and among men. The stronger effects for low-wage workers support the hypothesis that relaxing liquidity constraints extends search time by enabling workers to wait for more suitable matches.

We find no evidence that UI systematically improves entry wages or average post-unemployment wages. However, eligibility does improve job-match quality along non-wage dimensions. Eligible workers exhibit longer tenure in their first post-unemployment jobs and are more likely to be reemployed in more productive firms, defined as those with above-median average wages. These results suggest that the benefits of UI extend beyond short-term income support, contributing to more stable and productive employment relationships.

The populations for whom UI most prolongs unemployment—low-wage workers and men—are also those who experience the largest gains in match quality. Among these groups, UI eligibility significantly increases both the probability of reemployment in higher-paying firms and tenure in the first post-shock job. For men, we additionally find a positive effect on reemployment in larger firms, as well as a modest increase in entry wages. These patterns are consistent with the interpretation that relaxing liquidity constraints enables workers to reject poor job offers, endure longer unemployment spells, and ultimately secure better matches.

We extend the analysis to episodes of massive layoffs and find broadly consistent results. Following mass separations, UI eligibility again lengthens unemployment spells while facilitating improved matches, including higher probabilities of reemployment in more productive and larger firms, longer job tenure, and, under Policy 1, small but significant increases in entry wages.

Taken together, these findings highlight the role of UI in enhancing labor market efficiency, particularly in contexts where firm closures or mass layoffs generate abrupt employment disruptions. While UI does not appear to raise wages directly, it contributes to more effective job transitions by enabling workers to take the time necessary to secure more stable and productive positions. In this way, our study underscores the importance of UI design in Colombia and across Latin America, suggesting that policies which combine immediate financial relief with improved long-term labor market outcomes can be especially valuable.

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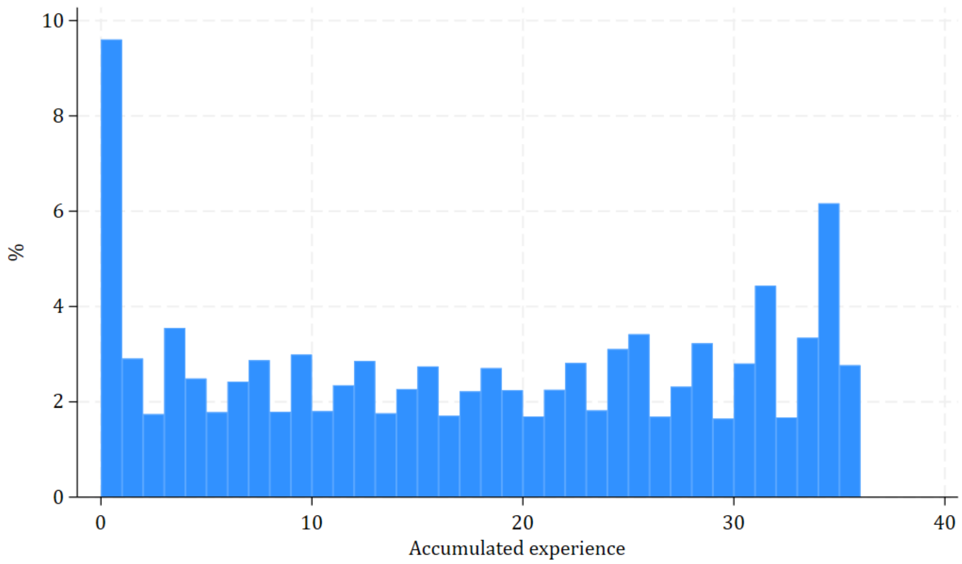
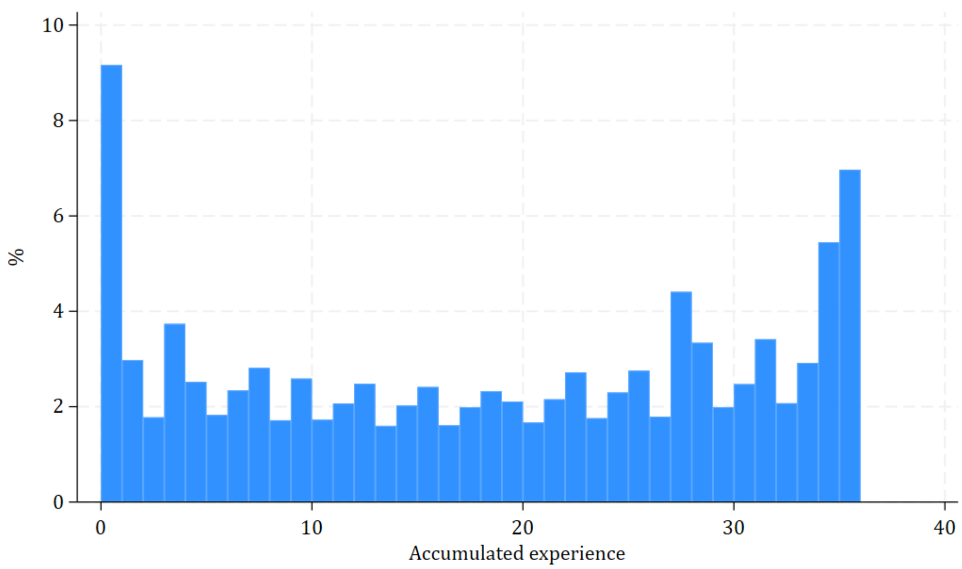
Figure 1. Density of Accumulated Months of Contributions in the 36 Months Prior to Layoff



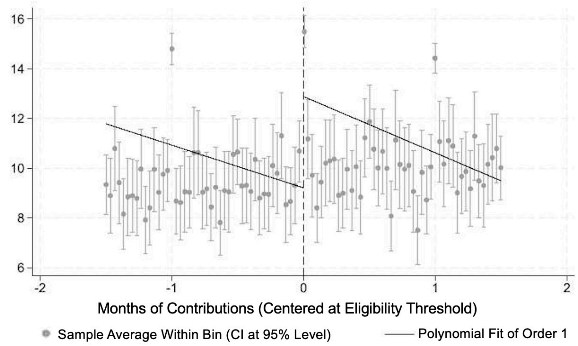
(a) 2013–2019 (Policy 1)



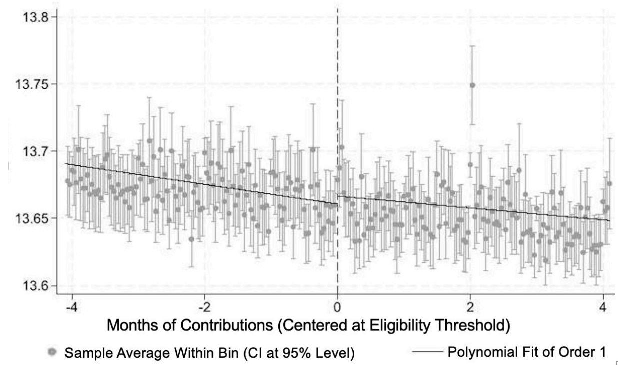
(b) 2020–2022 (Policy 2)

Notes: The data are drawn from the Unified Social Security Contributions Form (Planilla Integrada de Liquidación de Aportes, PILA). Panel (a) covers the period 2013–2019, while Panel (b) covers 2020–2022. The sample is restricted to workers displaced due to firm closures. The figures display the empirical density of the running variable, defined as accumulated months of contributions in the 36 months prior to layoff. The UI eligibility threshold is 12 months.

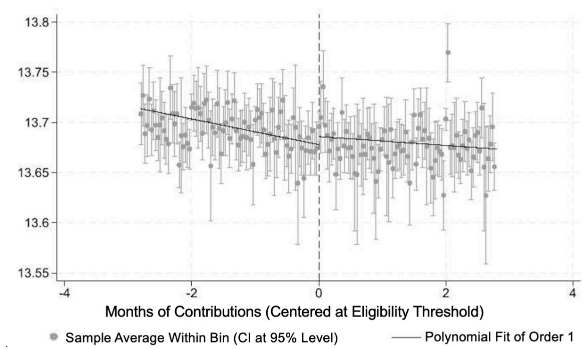
Figure 2. Effects of UI Eligibility on Unemployment Duration, Wages, and Job Stability, 2013-2019 (Policy 1)



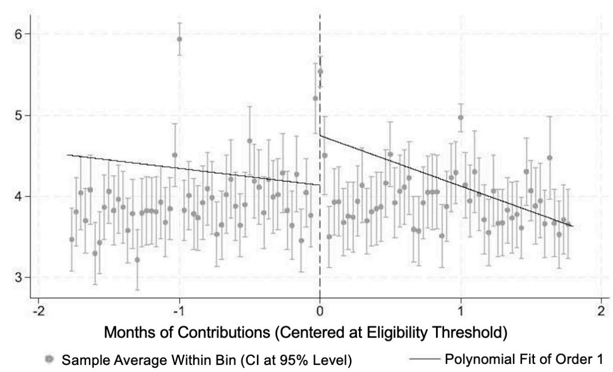
(a) Unemployment Duration



(b) Log Real Entry Wage



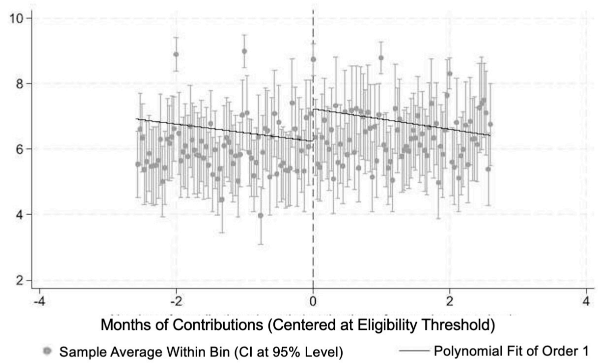
(c) Log Real Average Wage



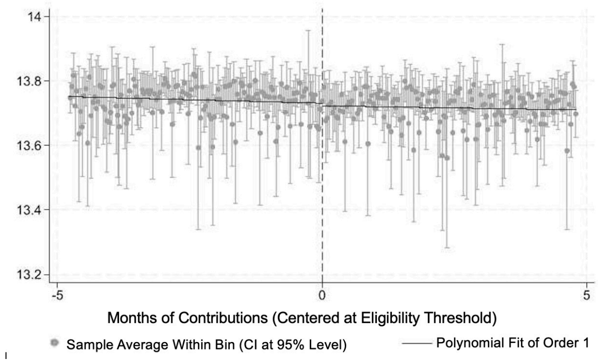
(d) Post-Unemployment Job Duration

Notes: The data are drawn from the Unified Social Security Contributions Form (Planilla Integrada de Liquidación de Aportes, PILA), covering the period 2013–2019. Figures present regression discontinuity plots for post-displacement outcomes around the UI eligibility threshold (normalized to zero). The running variable is the number of months of contributions in the 36 months prior to job loss, centered at the eligibility cutoff. Each panel plots average outcomes within bins of the running variable (dots), along with 95 percent confidence intervals (vertical lines). The solid lines correspond to local linear fits estimated separately on each side of the cutoff using a triangular kernel. Panel (a) shows unemployment duration (in months); panel (b) reports the log of the real entry wage; panel (c) reports the log of average real wages following re-employment; and panel (d) shows the duration of the first formal job after unemployment (in months). The vertical dashed line indicates the eligibility threshold for UI benefits.

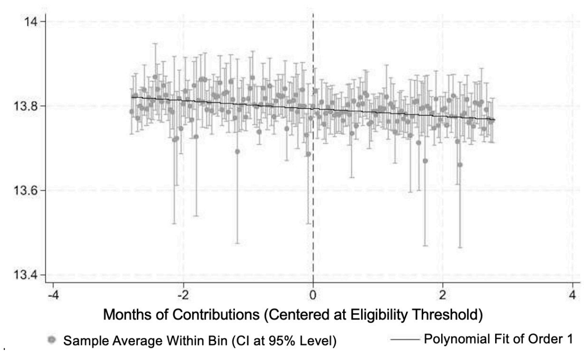
Figure 3. Effects of UI Eligibility on Unemployment Duration, Wages, and Job Stability, 2020-2022 (Policy 2)



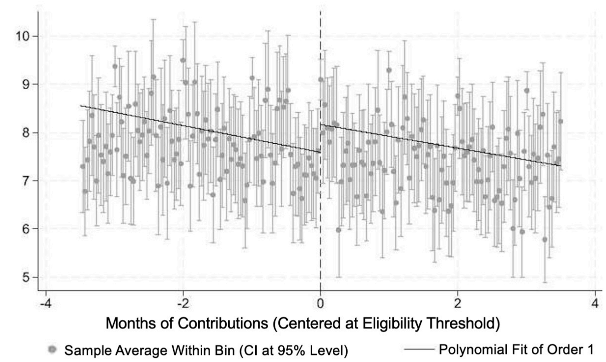
(a) Unemployment Duration



(b) Log Real Entry Wage



(c) Log Real Average Wage



(d) Post-Unemployment Job Duration

Notes: The data are drawn from the Unified Social Security Contributions Form (Planilla Integrada de Liquidación de Aportes, PILA), covering the period 2020-2022. Figures present regression discontinuity plots for post-displacement outcomes around the UI eligibility threshold (normalized to zero). The running variable is the number of months of contributions in the 36 months prior to job loss, centered at the eligibility cutoff. Each panel plots average outcomes within bins of the running variable (dots), along with 95 percent confidence intervals (vertical lines). The solid lines correspond to local linear fits estimated separately on each side of the cutoff using a triangular kernel. Panel (a) shows unemployment duration (in months); panel (b) reports the log of the real entry wage; panel (c) reports the log of average real wages following re-employment; and panel (d) shows the duration of the first formal job after unemployment (in months). The vertical dashed line indicates the eligibility threshold for UI benefits.

Table 1. Descriptive Statistics at the Worker Level

	2013-2019 (Policy 1)		2022-2024 (Policy 2)	
Accumulated Experience	$\geq$ 360 days	$<$ 360 days	$\geq$ 360 days	$<$ 360 days
	(1)	(2)	(3)	(4)
Unemployment Spell (months)	2.392	4.166	2.062	3.371
	(3.354)	(5.468)	(2.511)	(3.926)
Log Wage Growth (log points)	0.013	0.043	0.035	0.062
	(0.524)	(0.653)	(0.736)	(0.934)
Log Nominal Wage (log COP)	13.733	13.511	14.045	13.928
	(0.647)	(0.566)	(0.752)	(0.718)
Post-Unemployment Job Durantion (months)	13.631	8.937	9.982	6.601
	(8.483)	(7.972)	(5.939)	(5.565)
Proportion of Time Employed After Unemployment	0.356	0.234	0.794	0.594
	(0.198)	(0.174)	(0.245)	(0.301)
Change Industry (indicator = 1)	0.498	0.585	0.484	0.549
	(0.500)	(0.493)	(0.500)	(0.498)
Change City (indicator = 1)	0.252	0.347	0.303	0.383
	(0.434)	(0.476)	(0.460)	(0.486)
Large Firm (indicator = 1)	0.483	0.479	0.500	0.454
	(0.500)	(0.500)	(0.500)	(0.498)
Primary Sector (indicator = 1)	0.082	0.108	0.080	0.062
	(0.274)	(0.310)	(0.271)	(0.241)
Secondary Sector (indicator = 1)	0.102	0.074	0.100	0.088
	(0.303)	(0.262)	(0.300)	(0.284)
Tertiary Sector (indicator = 1)	0.816	0.818	0.821	0.85
	(0.387)	(0.386)	(0.384)	(0.357)
Accumulated Months of Contributions	25.519	5.018	26.362	5.179
	(7.114)	(3.845)	(7.156)	(3.678)
Age	39.274	34.875	41.868	35.759
	(11.439)	(12.116)	(12.283)	(12.534)
Male (indicator = 1)	0.602	0.644	0.549	0.613
	(0.489)	(0.479)	(0.498)	(0.487)
Firm Age 1 (indicator = 1)	0.191	0.325	0.194	0.295
	(0.393)	(0.468)	(0.395)	(0.456)
Firm Age 3 (indicator = 1)	0.346	0.176	0.431	0.29
	(0.476)	(0.381)	(0.495)	(0.454)
Observations	753,942	337,095	95,279	38,675

Notes: The data are from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA), covering the period April 2013 to March 2022. The sample is restricted to workers who lost their jobs due to firm closures. Unemployment spell denotes the total number of months between job loss and re-employment. Log wage growth is the change in the natural logarithm of monthly wages between the pre- and post-unemployment job. Post-Unemployment Job Duration is the total number of months spent employed after the unemployment spell within the observation window. Proportion of Time Employed After Unemployment is the fraction of months employed over the post-unemployment observation period. Accumulated Months of Contributions measures total months of social security contributions during the 36 months prior to job loss. Firm Age 1 and Firm Age 3 are indicators for firms in the youngest and oldest age terciles, respectively. Sector indicators refer to the industry classification of the first post-unemployment job. Reported figures are means, with standard deviations in parentheses.

Table 2. Covariate Balance Check

	2013-2019 (Policy 1)			2020-2022 (Policy 2)		
Covariate	Robust Coefficient	Obs.	Bandwidth	Robust Coefficient	Obs.	Bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)
Age	1.27511	106,890	2.7	0.64351	90,493	8.8
	(0.76882)			(0.96842)		
Log wage at shock	0.00619	167,020	4.1	0.01196	60,056	5.9
	(0.01211)			(0.01306)		
Less than 2 years firm	0.00407	143,622	3.6	-0.02642	46,763	4.5
	(0.00891)			(0.01522)		
More than 6 years firm	-0.00438	172,710	4.6	0.00681	39,838	5.6
	(0.00948)			(0.01569)		
Primary Sector	0.00226	100,639	2.5	-0.00724	39,838	3.9
	(0.00557)			(0.00790)		
Tertiary Sector	-0.00609	103,955	2.6	-0.00640	38,198	3.7
	(0.00744)			(0.01282)		

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from April 2013 to March 2022. The sample is restricted to workers displaced due to firm closures, as described in the main text. The table presents covariate balance tests around the regression discontinuity cutoff. The running variable is the number of months of Social Security contributions accumulated in the 36 months prior to job loss. The cutoff is set at 12 months, which determines eligibility for UI benefits. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth.

Table 3. Main Effects of UI Eligibility: 2013-2019 (Policy 1)

	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.90375***	0.94174***	0.00199	0.00414	-0.00064	0.00508	0.88719***	0.87743***
	(0.10512)	(0.12671)	(0.00287)	(0.00650)	(0.00497)	(0.00718)	(0.16424)	(0.16507)
Bias-corrected	1.27282***	1.32449***	0.00212	0.00996	-0.00910*	-0.00542	0.69771***	0.64401***
	(0.10512)	(0.12671)	(0.00287)	(0.00650)	(0.00497)	(0.00718)	(0.16424)	(0.16507)
Robust	1.27282***	1.32449***	0.00212	0.00996	-0.00910	-0.00542	0.69771***	0.64401***
	(0.15406)	(0.16547)	(0.00409)	(0.00738)	(0.00626)	(0.00880)	(0.21107)	(0.20715)
Observations	75172	75172	179229	179229	118890	118890	51942	51942
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.8	1.8	4.1	4.1	2.8	2.8	1.1	1.1

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2019. The sample is restricted to workers displaced due to firm closures, as described in the main text. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 4. Effects of UI Eligibility: Potential Mechanisms, 2013-2019 (Policy 1)

	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.01206***	0.01199**	0.00278	0.00256	0.01943***	0.02043***	-0.00655	-0.00697
	(0.00446)	(0.00491)	(0.00695)	(0.00766)	(0.00590)	(0.00586)	(0.00554)	(0.00562)
Bias-corrected	0.01403***	0.01608***	-0.00493	-0.00685	0.03025***	0.03107***	-0.01340**	-0.01337**
	(0.00446)	(0.00491)	(0.00695)	(0.00766)	(0.00590)	(0.00586)	(0.00554)	(0.00562)
Robust	0.01403**	0.01608**	-0.00493	-0.00685	0.03025***	0.03107***	-0.01340*	-0.01337*
	(0.00677)	(0.00675)	(0.01025)	(0.01094)	(0.00900)	(0.00880)	(0.00791)	(0.00800)
Observations	249647	249647	109495	109495	133536	133536	148973	148973
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	5.1	5.1	2.8	2.8	3.3	3.3	3.8	3.8

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2019. The sample is restricted to workers displaced due to firm closures, as described in the main text. Dependent variable: in Columns (1) and (2), *High wage firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. In Columns (3) and (4), *Large firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. In Columns (5) and (6), *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. In Columns (7) and (8), *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 5. Main Effects of UI Eligibility: 2020-2022 (Policy 2)

	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.75677***	0.79883***	0.00898	0.01334	0.00185	0.00336	0.20498	0.22687
	(0.15415)	(0.17539)	(0.01313)	(0.01484)	(0.00984)	(0.01202)	(0.15704)	(0.15891)
Bias-corrected	0.97050***	1.02202***	0.00369	0.00653	-0.00912	-0.00659	0.44163***	0.47163***
	(0.15415)	(0.17539)	(0.01313)	(0.01484)	(0.00984)	(0.01202)	(0.15704)	(0.15891)
Robust	0.97050***	1.02202***	0.00369	0.00653	-0.00912	-0.00659	0.44163**	0.47163**
	(0.18594)	(0.20429)	(0.01730)	(0.01983)	(0.01375)	(0.01546)	(0.21568)	(0.22064)
Observations	18325	18325	54338	54338	31608	31608	40402	40402
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.6	1.6	4.8	4.8	2.8	2.8	3.5	3.5

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. The sample is restricted to workers displaced due to firm closures, as described in the main text. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

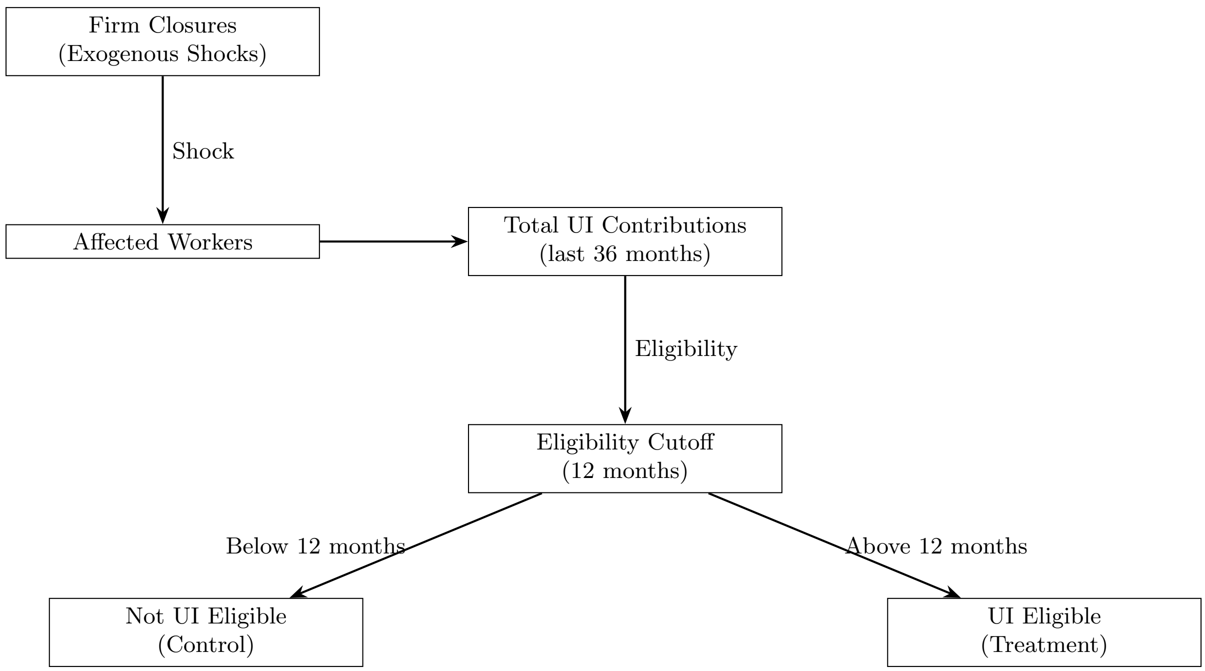
Table 6. Effects of UI Eligibility: Potential Mechanisms, 2020-2022 (Policy 2)

	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.02313***	0.02485***	-0.00801	-0.01004	0.03389**	0.03589**	-0.01861	-0.02057*
	(0.00790)	(0.00833)	(0.01194)	(0.01239)	(0.01409)	(0.01413)	(0.01136)	(0.01154)
Bias-corrected	0.04107***	0.04419***	-0.01284	-0.01672	0.03569**	0.03832***	-0.03625***	-0.03934***
	(0.00790)	(0.00833)	(0.01194)	(0.01239)	(0.01409)	(0.01413)	(0.01136)	(0.01154)
Robust	0.04107***	0.04419***	-0.01284	-0.01672	0.03569*	0.03832*	-0.03625**	-0.03934**
	(0.01195)	(0.01245)	(0.01760)	(0.01763)	(0.02096)	(0.02096)	(0.01610)	(0.01632)
Observations	51745	51745	26906	26906	25949	25949	39026	39026
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.9	3.9	2.6	2.6	2.5	2.5	3.8	3.8

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. The sample is restricted to workers displaced due to firm closures, as described in the main text. Dependent variable: in Columns (1) and (2), *High wage firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. In Columns (3) and (4), *Large firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. In Columns (5) and (6), *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. In Columns (7) and (8), *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Figures and Tables

Appendix Figure 1. Selection of the sample



Appendix Table 1. Descriptive Statistics at the Worker Level - Random Sample of Separated Workers

	2013-2019 (policy 1)		2022-2024 (policy 2)	
Accumulated Experience	$\geq$ 360 days	$<$ 360 days	$\geq$ 360 days	$<$ 360 days
	(1)	(2)	(3)	(4)
Unemployment Spell (months)	6.800	7.622	5.082	5.675
	(6.171)	(6.408)	(5.123)	(5.311)
Log Wage Growth (log points)	-0.041	0.015	-0.077	-0.036
	(0.382)	(0.241)	(0.718)	(0.722)
Log Nominal Wage (log COP)	13.745	13.560	14.248	14.053
	(0.562)	(0.329)	(0.810)	(0.755)
Post-Unemployment Job Durantion (months)	8.545	5.999	6.299	4.760
	(7.364)	(5.951)	(6.221)	(5.021)
Proportion of Time Employed After Unemployment	0.215	0.177	0.568	0.479
	(0.166)	(0.148)	(0.292)	(0.281)
Change Industry (indicator = 1)	0.531	0.559	0.491	0.518
	(0.499)	(0.497)	(0.500)	(0.500)
Change city (indicator = 1)	0.399	0.452	0.432	0.478
	(0.490)	(0.498)	(0.495)	(0.500)
Large firm (indicator = 1)	0.709	0.692	0.706	0.686
	(0.454)	(0.462)	(0.455)	(0.464)
Primary Sector (indicator = 1)	0.067	0.068	0.058	0.063
	(0.250)	(0.251)	(0.233)	(0.243)
Secondary Sector (indicator = 1)	0.081	0.069	0.082	0.077
	(0.273)	(0.253)	(0.275)	(0.266)
Tertiary Sector (indicator = 1)	0.852	0.863	0.860	0.860
	(0.355)	(0.344)	(0.347)	(0.347)
Accumulated Months of Contributions	25.367	5.124	30.649	6.105
	(7.905)	(3.051)	(6.994)	(2.934)
Age	36.785	29.108	41.961	31.727
	(12.682)	(10.919)	(12.287)	(11.698)
Male (indicator = 1)	0.537	0.576	0.540	0.546
	(0.499)	(0.494)	(0.498)	(0.498)
Firm Age 1 (indicator = 1)	0.006	0.041	0.011	0.091
	(0.079)	(0.198)	(0.105)	(0.287)
Firm Age 3 (indicator = 1)	0.918	0.828	0.886	0.683
	(0.275)	(0.377)	(0.318)	(0.465)
Observations	532,163	751,554	1,371,280	494,435

Notes: The data are from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA), covering the period April 2013 to March 2024. The population analyzed consists of a random sample of individuals who experienced job separation during this period. Unemployment spell denotes the total number of months between job loss and re-employment. Log wage growth is the change in the natural logarithm of monthly wages between the pre- and post-unemployment job. Post-Unemployment Job Duration is the total number of months spent employed after the unemployment spell within the observation window. Proportion of Time Employed After Unemployment is the fraction of months employed over the post-unemployment observation period. Accumulated Months of Contributions measures total months of social security contributions during the 36 months prior to job loss. Firm Age 1 and Firm Age 3 are indicators for firms in the youngest and oldest age terciles, respectively. Sector indicators refer to the industry classification of the first post-unemployment job. Reported figures are means, with standard deviations in parentheses.

Appendix Table 2. Descriptive Statistics at Firm Level

	Mean	Standard Deviation
Log nominal wage	13.845	0.556
$\leq$2 years	0.105	0.306
3–4 years	0.116	0.320
5–6 years	0.119	0.324
$>$6 years	0.660	0.474
$\leq$20 employees	0.863	0.344
21–50 employees	0.078	0.268
51–100 employees	0.029	0.167
$>$100 employees	0.030	0.172
Agriculture and Mining	0.083	0.276
Manufacture	0.090	0.286
Construction	0.088	0.283
Transportation and Trade	0.237	0.425
Private Services	0.301	0.459
Goverment and social services	0.200	0.400

Notes: The data are from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA), covering the period April 2013 to March 2022. Reported figures are means and standard deviations.

Appendix Table 3. Income Heterogeneity: Main Effects of UI Eligibility Under Policy 1

Panel A. *Workers With Pre-Layoff Wages Below The 33rd Percentile*

	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	1.29530***	1.32972***	0.00033	-0.00021	-0.01010	-0.01096	0.73717***	0.74896***
	(0.19689)	(0.20823)	(0.00426)	(0.00452)	(0.00834)	(0.00831)	(0.19638)	(0.19767)
Bias-corrected	1.38617***	1.42114***	-0.00097	-0.00170	-0.02155***	-0.02290***	1.08842***	1.09507***
	(0.19689)	(0.20823)	(0.00426)	(0.00452)	(0.00834)	(0.00831)	(0.19638)	(0.19767)
Robust	1.38617***	1.42114***	-0.00097	-0.00170	-0.02155*	-0.02290*	1.08842***	1.09507***
	(0.26664)	(0.27171)	(0.00609)	(0.00641)	(0.01218)	(0.01215)	(0.29348)	(0.29438)
Observations	17684	17684	52859	52859	34439	34439	24046	24046
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.3	1.3	4.0	4.0	2.6	2.6	1.9	1.9

Panel B. *Workers With Pre-Layoff Wages Above The 66th Percentile*

	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.75644***	0.80336***	-0.00490	0.00808	0.00555	0.01942	-0.11086	-0.18852
	(0.12007)	(0.17350)	(0.00714)	(0.01623)	(0.00979)	(0.01679)	(0.23781)	(0.25401)
Bias-corrected	0.78314***	0.88943***	-0.01356*	-0.00060	0.00400	0.01647	-0.15585	-0.28374
	(0.12007)	(0.17350)	(0.00714)	(0.01623)	(0.00979)	(0.01679)	(0.23781)	(0.25401)
Robust	0.78314***	0.88943***	-0.01356	-0.00060	0.00400	0.01647	-0.15585	-0.28374
	(0.15754)	(0.19203)	(0.01122)	(0.02128)	(0.01309)	(0.02079)	(0.28958)	(0.30486)
Observations	51676	51676	44382	44382	44776	44776	21519	21519
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	4.3	4.3	3.8	3.8	3.8	3.8	1.9	1.9

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2019. Panel A restricts the sample to workers displaced by firm closures with pre-layoff wages below the 33rd percentile, while Panel B restricts it to those above the 66th percentile. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 4. Income Heterogeneity: Main Effects of UI Eligibility Under Policy 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. *Workers With Pre-Layoff Wages Below The 33rd Percentile*							
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.65859***	0.70671***	-0.00021	0.00137	0.00108	0.00028	0.10655	0.17155
	(0.25005)	(0.25891)	(0.02685)	(0.02698)	(0.01847)	(0.01847)	(0.28696)	(0.28764)
Bias-corrected	1.09933***	1.16176***	0.00122	0.00365	-0.01912	-0.02147	0.47724*	0.56571**
	(0.25005)	(0.25891)	(0.02685)	(0.02698)	(0.01847)	(0.01847)	(0.28696)	(0.28764)
Robust	1.09933***	1.16176***	0.00122	0.00365	-0.01912	-0.02147	0.47724	0.56571
	(0.33784)	(0.35152)	(0.04054)	(0.04061)	(0.02460)	(0.02459)	(0.37390)	(0.38006)
Observations	9414	9414	13292	13292	9615	9615	11898	11898
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.1	3.1	4.5	4.5	3.2	3.2	4.0	4.0
	Panel B. *Workers With Pre-Layoff Wages Above The 66th Percentile*							
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.15270	0.20116	0.00251	0.02302	0.01176	0.03212	-0.12783	-0.09688
	(0.16928)	(0.20103)	(0.02043)	(0.02573)	(0.01654)	(0.02335)	(0.26355)	(0.26684)
Bias-corrected	0.40196**	0.48917**	-0.00417	0.01630	-0.00270	0.01654	0.01385	0.04334
	(0.16928)	(0.20103)	(0.02043)	(0.02573)	(0.01654)	(0.02335)	(0.26355)	(0.26684)
Robust	0.40196*	0.48917*	-0.00417	0.01630	-0.00270	0.01654	0.01385	0.04334
	(0.24372)	(0.26477)	(0.03074)	(0.03525)	(0.02531)	(0.03144)	(0.35934)	(0.36493)
Observations	13666	13666	11412	11412	11414	11414	16453	16453
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	4.0	4.0	3.3	3.3	3.3	3.3	4.9	4.9

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. Panel A restricts the sample to workers displaced by firm closures with pre-layoff wages below the 33rd percentile, while Panel B restricts it to those above the 66th percentile. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 5. Income Heterogeneity: Firm Effects of UI Eligibility Under Policy 1

	Panel A. *Workers With Pre-Layoff Wages Below The 33rd Percentile*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.02376**	0.01652	-0.00111	-0.00215	0.02744**	0.02606**	-0.01659*	-0.01764*
	(0.01075)	(0.01143)	(0.01097)	(0.01120)	(0.01174)	(0.01143)	(0.00976)	(0.00977)
Bias-corrected	0.03448***	0.02376**	-0.01229	-0.01347	0.03470***	0.03488***	-0.01689*	-0.01797*
	(0.01075)	(0.01143)	(0.01097)	(0.01120)	(0.01174)	(0.01143)	(0.00976)	(0.00977)
Robust	0.03448**	0.02376	-0.01229	-0.01347	0.03470**	0.03488**	-0.01689	-0.01797
	(0.01589)	(0.01631)	(0.01474)	(0.01506)	(0.01593)	(0.01535)	(0.01411)	(0.01400)
Observations	43032	43032	39407	39407	45287	45287	45279	45279
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	2.9	2.9	3.1	3.1	3.7	3.7	3.7	3.7
	Panel B. *Workers With Pre-Layoff Wages Above The 66th Percentile*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.01127	0.01165	0.00082	-0.00324	0.01788*	0.01914*	-0.00269	-0.00261
	(0.00971)	(0.01098)	(0.01506)	(0.01634)	(0.01014)	(0.01013)	(0.00940)	(0.00948)
Bias-corrected	0.00703	0.00228	-0.01938	-0.02802*	0.03405***	0.03319***	-0.00526	-0.00580
	(0.00971)	(0.01098)	(0.01506)	(0.01634)	(0.01014)	(0.01013)	(0.00940)	(0.00948)
Robust	0.00703	0.00228	-0.01938	-0.02802	0.03405**	0.03319**	-0.00526	-0.00580
	(0.01335)	(0.01575)	(0.01993)	(0.02110)	(0.01587)	(0.01609)	(0.01372)	(0.01370)
Observations	46093	46093	23909	23909	38410	38410	45860	45860
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.5	3.5	2.1	2.1	3.6	3.6	4.3	4.3

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2020. Panel A restricts the sample to workers displaced by firm closures with pre-layoff wages below the 33rd percentile, while Panel B restricts it to those above the 66th percentile. Dependent variable: *High-Wage Firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. *Large Firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 6. Income Heterogeneity: Firm Effects of UI Eligibility Under Policy 2

	Panel A. *Workers With Pre-Layoff Wages Below The 33rd Percentile*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.05758***	0.04421**	-0.01498	-0.01537	0.00522	0.00761	-0.01942	-0.01730
	(0.01833)	(0.01947)	(0.01738)	(0.01816)	(0.02171)	(0.02174)	(0.02217)	(0.02234)
Bias-corrected	0.08911***	0.07508***	-0.01141	-0.01079	0.01831	0.02156	-0.03830*	-0.03716*
	(0.01833)	(0.01947)	(0.01738)	(0.01816)	(0.02171)	(0.02174)	(0.02217)	(0.02234)
Robust	0.08911***	0.07508***	-0.01141	-0.01079	0.01831	0.02156	-0.03830	-0.03716
	(0.02455)	(0.02494)	(0.02670)	(0.02709)	(0.03137)	(0.03123)	(0.03159)	(0.03232)
Observations	12072	12072	11747	11747	12803	12803	11543	11543
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.4	3.4	4.3	4.3	4.8	4.8	4.2	4.2
	Panel B. *Workers With Pre-Layoff Wages Above The 66th Percentile*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	-0.01380	-0.00293	-0.03982*	-0.04423*	0.04536**	0.04651**	-0.01528	-0.01356
	(0.01643)	(0.01676)	(0.02355)	(0.02434)	(0.02154)	(0.02191)	(0.02230)	(0.02305)
Bias-corrected	-0.01571	-0.00312	-0.08961***	-0.09542***	0.05380**	0.05627**	-0.03564	-0.03083
	(0.01643)	(0.01676)	(0.02355)	(0.02434)	(0.02154)	(0.02191)	(0.02230)	(0.02305)
Robust	-0.01571	-0.00312	-0.08961***	-0.09542***	0.05380	0.05627*	-0.03564	-0.03083
	(0.02311)	(0.02435)	(0.03112)	(0.03173)	(0.03294)	(0.03345)	(0.03304)	(0.03402)
Observations	14834	14834	6451	6451	9682	9682	8363	8363
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.9	3.9	2.1	2.1	3.2	3.2	2.9	2.9

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. Panel A restricts the sample to workers displaced by firm closures with pre-layoff wages below the 33rd percentile, while Panel B restricts it to those above the 66th percentile. Dependent variable: *High-Wage Firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. *Large Firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 7. Gender Heterogeneity: Main Effects of UI Eligibility Under Policy 1

	Panel A. *Men*							
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	1.03402***	1.09371***	0.00230	0.01054	-0.00124	0.00589	1.12731***	1.15180***
	(0.13244)	(0.16159)	(0.00375)	(0.00718)	(0.00611)	(0.00795)	(0.18539)	(0.18636)
Bias-corrected	1.48282***	1.55547***	0.00405	0.01826**	-0.00462	0.00915	1.10022***	1.09334***
	(0.13244)	(0.16159)	(0.00375)	(0.00718)	(0.00611)	(0.00795)	(0.18539)	(0.18636)
Robust	1.48282***	1.55547***	0.00405	0.01826**	-0.00462	0.00915	1.10022***	1.09334***
	(0.18991)	(0.20816)	(0.00514)	(0.00825)	(0.00784)	(0.00948)	(0.24710)	(0.23879)
Observations	43773	43773	97942	97942	110401	110401	36587	36587
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.5	1.5	3.4	3.4	3.9	3.9	1.2	1.2
	Panel B. *Women*							
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.73915***	0.74466***	0.00228	-0.00539	0.00494	-0.00308	0.02822	0.04634
	(0.11656)	(0.12259)	(0.00628)	(0.01051)	(0.00847)	(0.01229)	(0.19733)	(0.18977)
Bias-corrected	0.67623***	0.68994***	-0.00770	-0.02038*	-0.01489*	-0.03713***	-0.25818	-0.24572
	(0.11656)	(0.12259)	(0.00628)	(0.01051)	(0.00847)	(0.01229)	(0.19733)	(0.18977)
Robust	0.67623***	0.68994***	-0.00770	-0.02038	-0.01489	-0.03713**	-0.25818	-0.24572
	(0.14780)	(0.15126)	(0.00905)	(0.01370)	(0.01153)	(0.01552)	(0.30593)	(0.28895)
Observations	89918	89918	48032	48032	43515	43515	36435	36435
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	6.3	6.3	3.3	3.3	3.0	3.0	2.5	2.5

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2019. Panel A restricts the sample to male workers displaced by firm closures, while Panel B restricts it to female workers. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 8. Gender Heterogeneity: Main Effects of UI Eligibility Under Policy 2

	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
Panel A. *Men*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.85881***	0.89327***	0.00399	0.00694	0.00211	0.00244	0.10230	0.12763
	(0.19039)	(0.20742)	(0.01523)	(0.01634)	(0.01202)	(0.01393)	(0.18863)	(0.19257)
Bias-corrected	1.00114***	1.06170***	0.00231	0.00332	-0.00506	-0.00846	0.29532	0.33422*
	(0.19039)	(0.20742)	(0.01523)	(0.01634)	(0.01202)	(0.01393)	(0.18863)	(0.19257)
Robust	1.00114***	1.06170***	0.00231	0.00332	-0.00506	-0.00846	0.29532	0.33422
	(0.25432)	(0.26595)	(0.02069)	(0.02277)	(0.01714)	(0.01905)	(0.23985)	(0.24464)
Observations	12538	12538	38621	38621	23301	23301	32196	32196
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.8	1.8	5.4	5.4	3.2	3.2	4.5	4.5
Panel B. *Women*								
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wages		Post-Unemployment Job Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.09336	0.10664	0.01659	0.02221	0.00776	0.01128	0.18745	0.21633
	(0.20003)	(0.20901)	(0.02146)	(0.02391)	(0.01289)	(0.01608)	(0.22370)	(0.22348)
Bias-corrected	0.38983*	0.42897**	0.01030	0.01557	-0.01003	-0.00213	0.23299	0.28657
	(0.20003)	(0.20901)	(0.02146)	(0.02391)	(0.01289)	(0.01608)	(0.22370)	(0.22348)
Robust	0.38983	0.42897	0.01030	0.01557	-0.01003	-0.00213	0.23299	0.28657
	(0.27146)	(0.27987)	(0.03209)	(0.03462)	(0.01789)	(0.02005)	(0.31397)	(0.30784)
Observations	15868	15868	19049	19049	13981	13981	18172	18172
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.7	3.7	4.4	4.4	3.1	3.1	4.1	4.1

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. Panel A restricts the sample to male workers displaced by firm closures, while Panel B restricts it to female workers. Dependent variable: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage at the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total months spent at the first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 9. Gender Heterogeneity: Firm Effects of UI Eligibility Under Policy 1

	Panel A. *Men*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.01500**	0.01975***	0.02080***	0.02106**	0.01552***	0.01634***	-0.00818	-0.00905
	(0.00681)	(0.00757)	(0.00766)	(0.00823)	(0.00599)	(0.00619)	(0.00735)	(0.00747)
Bias-corrected	0.02833***	0.03674***	0.02329***	0.02295***	0.02299***	0.02350***	-0.02670***	-0.02665***
	(0.00681)	(0.00757)	(0.00766)	(0.00823)	(0.00599)	(0.00619)	(0.00735)	(0.00747)
Robust	0.02833***	0.03674***	0.02329**	0.02295**	0.02299**	0.02350***	-0.02670***	-0.02665***
	(0.00989)	(0.01018)	(0.01118)	(0.01159)	(0.00907)	(0.00909)	(0.00956)	(0.00973)
Observations	115122	115122	70264	70264	101360	101360	85997	85997
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.6	3.6	2.6	2.6	3.8	3.8	3.1	3.1
	Panel B. *Women*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	-0.02950**	-0.04282***	-0.04038***	-0.04217***	0.02040*	0.02570**	0.00865	0.00866
	(0.01383)	(0.01447)	(0.01085)	(0.01085)	(0.01109)	(0.01095)	(0.01010)	(0.01052)
Bias-corrected	-0.05013***	-0.07189***	-0.06068***	-0.06334***	0.04092***	0.04859***	0.01116	0.01155
	(0.01383)	(0.01447)	(0.01085)	(0.01085)	(0.01109)	(0.01095)	(0.01010)	(0.01052)
Robust	-0.05013***	-0.07189***	-0.06068***	-0.06334***	0.04092**	0.04859***	0.01116	0.01155
	(0.01671)	(0.01768)	(0.01741)	(0.01765)	(0.01606)	(0.01603)	(0.01373)	(0.01435)
Observations	28120	28120	43251	43251	38785	38785	33369	33369
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	1.8	1.8	3.3	3.3	3.0	3.0	2.6	2.6

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2013 to 2019. Panel A restricts the sample to male workers displaced by firm closures, while Panel B restricts it to female workers. Dependent variable: *High-Wage Firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. *Large Firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 10. Gender Heterogeneity: Firm Effects of UI Eligibility Under Policy 2

	Panel A. *Men*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.03287***	0.03510***	-0.00664	-0.00753	0.01664	0.01907	-0.01251	-0.01425
	(0.01020)	(0.01053)	(0.01396)	(0.01499)	(0.01377)	(0.01355)	(0.01612)	(0.01617)
Bias-corrected	0.05152***	0.05508***	-0.00903	-0.01185	0.02235	0.02661**	-0.03313**	-0.03589**
	(0.01020)	(0.01053)	(0.01396)	(0.01499)	(0.01377)	(0.01355)	(0.01612)	(0.01617)
Robust	0.05152***	0.05508***	-0.00903	-0.01185	0.02235	0.02661	-0.03313	-0.03589
	(0.01457)	(0.01464)	(0.01872)	(0.01968)	(0.02191)	(0.02171)	(0.02221)	(0.02207)
Observations	36148	36148	18653	18653	23625	23625	22454	22454
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	4.3	4.3	2.9	2.9	3.6	3.6	3.4	3.4
	Panel A. *Women*							
	High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	0.00234	0.00267	-0.02760	-0.02902	0.05328***	0.05614***	-0.03101*	-0.03265*
	(0.01446)	(0.01529)	(0.01885)	(0.01885)	(0.02028)	(0.02114)	(0.01758)	(0.01805)
Bias-corrected	0.01012	0.00908	-0.02694	-0.02811	0.04847**	0.05311**	-0.04203**	-0.04403**
	(0.01446)	(0.01529)	(0.01885)	(0.01885)	(0.02028)	(0.02114)	(0.01758)	(0.01805)
Robust	0.01012	0.00908	-0.02694	-0.02811	0.04847*	0.05311**	-0.04203	-0.04403
	(0.01905)	(0.01991)	(0.02884)	(0.02881)	(0.02583)	(0.02624)	(0.02759)	(0.02860)
Observations	22574	22574	13105	13105	10226	10226	14557	14557
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	4.3	4.3	3.4	3.4	2.7	2.7	3.9	3.9

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from 2020 to 2022. Panel A restricts the sample to male workers displaced by firm closures, while Panel B restricts it to female workers. Dependent variable: *High-Wage Firm* is a dummy that takes a value of 1 if the worker ended up in a firm with average salaries above the median in the PILA sample. *Large Firm* is a dummy that takes a value of 1 if the worker ended up in a firm that has more than 50 employees. *Change Industry* is a dummy that takes a value of 1 if the worker changed industry for their first job after unemployment. *Change City* is a dummy that takes a value of 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix Table 11. Covariate Balance Check - Massive Layoffs

Covariate	Policy 1 (2013-2019) Robust Coefficient	Obs.	Bandwidth	Policy 2 (2020-2022) Robust Coefficient	Obs.	Bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)
Age	2.48589***	109,551	2.2	0.67212	110,568	6.1
	(0.77213)			(0.84869)		
Log wage at shock	-0.02897	135,381	2.9	-0.02169	59,235	3.3
	(0.01675)			(0.01576)		
Less than 2 years firm	-0.00059	171,655	3.6	-0.00056	66,574	3.8
	(0.00065)			(0.00152)		
More than 6 years firm	-0.05564***	95,739	3.1	-0.11979***	29,112	3.4
	(0.01476)			(0.02158)		
Primary Sector	0.00517	113,121	2.3	-0.01576***	65,068	3.7
	(0.00485)			(0.00513)		
Tertiary Sector	-0.00469	135,381	2.9	0.01543	65,068	3.7
	(0.00623)			(0.00796)		

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA) and cover the period from April 2013 to March 2022. The sample is restricted to workers displaced due to massive layoffs, as described in the main text. The table presents covariate balance tests around the regression discontinuity cutoff. The running variable is the number of months of Social Security contributions accumulated in the 36 months prior to job loss. The cutoff is set at 12 months, which determines eligibility for UI benefits. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth.

Appendix Table 12. Massive Layoff: Main and Firm Effects of UI Eligibility Under Policies 1 and 2

	Panel A. *Policy 1 (2013-2019)*															
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wage		Post-Unemployment Job Duration		High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Conventional	1.59***	1.59***	0.008**	0.008**	-0.002	-0.002	0.527***	0.527***	0.004	0.004	0.011	0.011	0.015***	0.015***	-0.018**	-0.018**
	(0.157)	(0.157)	(0.003)	(0.003)	(0.004)	(0.004)	(0.108)	(0.108)	(0.005)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)	(0.008)	(0.008)
Bias-corrected	1.501***	1.501***	0.009***	0.009***	-0.001	-0.001	0.61***	0.61***	0.002	0.002	0.014*	0.014*	0.016***	0.016***	-0.021***	-0.021***
	(0.157)	(0.157)	(0.003)	(0.003)	(0.004)	(0.004)	(0.108)	(0.108)	(0.005)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)	(0.008)	(0.008)
Robust	1.501***	1.501***	0.009**	0.009**	-0.001	-0.001	0.61***	0.61***	0.002	0.002	0.014*	0.014*	0.016***	0.016***	-0.021**	-0.021**
	(0.164)	(0.164)	(0.004)	(0.004)	(0.005)	(0.005)	(0.113)	(0.113)	(0.006)	(0.006)	(0.008)	(0.008)	(0.005)	(0.005)	(0.008)	(0.008)
Observations	112231	112231	209131	209131	259069	259069	59944	59944	154762	154762	119778	119778	171054	171054	89959	89959
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	3.7	3.7	6.2	6.2	7.8	7.8	2.7	2.7	4.3	4.3	5.1	5.1	5.6	5.6	3.7	3.7
	Panel B. *Policy 2 (2020-2022)*															
	Unemployment Duration		Log Real Entry Wage		Log Real Average Wage		Post-Unemployment Job Duration		High-Wage Firm		Large Firm		Change Industry		Change City	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Conventional	0.701***	0.701***	0.002	0.002	-0.007	-0.007	-0.265**	-0.265**	0.01	0.01	0.005	0.005	0.013	0.013	0.008	0.008
	(0.136)	(0.136)	(0.015)	(0.015)	(0.011)	(0.011)	(0.124)	(0.124)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Bias-corrected	0.544***	0.544***	0.013	0.013	-0.003	-0.003	-0.051	-0.051	0.022***	0.022***	0.012	0.012	0.023***	0.023***	0.012	0.012
	(0.136)	(0.136)	(0.015)	(0.015)	(0.011)	(0.011)	(0.124)	(0.124)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Robust	0.544***	0.544***	0.013	0.013	-0.003	-0.003	-0.051	-0.051	0.022**	0.022**	0.012	0.012	0.023*	0.023*	0.012	0.012
	(0.177)	(0.177)	(0.021)	(0.021)	(0.013)	(0.013)	(0.154)	(0.154)	(0.01)	(0.01)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Observations	50285	50285	71395	71395	61080	61080	59654	59654	90620	90620	62195	62195	70964	70964	62195	62195
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Bandwidth	2.9	2.9	4.0	4	3.4	3.4	3.3	3.3	3.6	3.6	3.5	3.5	4.0	4	3.5	3.5

Notes: The data are drawn from the Unified Social Security Contributions Form (*Planilla Integrada de Liquidación de Aportes*, PILA). Panel A reports the results for Policy 1 (2013–2019), whereas Panel B reports the results for Policy 2 (2020–2022). The sample is restricted to workers displaced by massive layoffs. Dependent variables: Columns (1) and (2) report the number of months unemployed after the shock, conditional on being hired. Columns (3) and (4) report the logarithm of the real wage in the first job after unemployment. Columns (5) and (6) report the logarithm of the real average wage after the shock. Columns (7) and (8) report the total number of months spent in the first job after unemployment. In columns (9) and (10), the dependent variable is a dummy equal to 1 if the worker's first post-unemployment job is in a firm with average salaries above the median in the PILA sample. In columns (11) and (12), the dependent variable is a dummy equal to 1 if the firm has more than 50 employees. In columns (13) and (14), the dependent variable is a dummy equal to 1 if the worker changed industry for their first job after unemployment. In columns (15) and (16), the dependent variable is a dummy equal to 1 if the worker changed cities for their first job after unemployment. Controls include age, average wage at the shock firm, log individual wage at the time of the shock, firm age, and sector. Reported coefficients correspond to the estimated discontinuity at the cutoff obtained from local linear regressions with a triangular kernel. Bandwidth selection follows the mean squared error–optimal procedure proposed by Cattaneo et al. (2024). Inference is based on robust bias-corrected standard errors. The number of observations corresponds to individuals within the optimal bandwidth. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.