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Marcelo Bergolo
Mery Ferrando
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Joan Vilá

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Marcelo Bergolo*
Mery Ferrando**
Matias Giaccobasso***
Joan Vilá****

* Instituto de Economía at Universidad de la República and IZA Institute of Labor Economics
** Tilburg University
*** VATT Institute for Economic Research
**** Instituto de Economía at Universidad de la República

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Abstract[1]

Job displacement generates large and persistent income losses. This paper examines how alternative social protection mechanisms mitigate the consequences of job loss in Uruguay, a middle-income country with a relatively well-developed social safety net. Using matched employer-employee administrative data, we combine variation from mass layoffs with difference-in-differences and regression-discontinuity designs to estimate the effects of job displacement and the insurance value of two policies: (i) a traditional contributory unemployment insurance program and (ii) a noncontributory conditional cash transfer program targeting low-income households. We document that after employment falls it gradually rebounds, while earnings decline sharply, with labor income falling by about 50% in the short term, and remain below pre-displacement levels even five years later. The persistent earnings losses are primarily driven by lower post-displacement wages. Comparing the insurance value of alternative policy responses, we find that the cash transfer program provides substantial protection for vulnerable and middle-income workers facing layoffs, while unemployment insurance does not generate comparable medium-term gains.

JEL Classification: J64, J65, I38, H53

Keywords: job displacement, unemployment insurance, social protection, mass layoffs

[1]We thank Craig McIntosh for his thoughtful comments and insightful discussion, and seminar participants at the IDB Research Network Workshop for helpful suggestions.

1 Introduction

Low- and middle-income countries are subject to frequent adverse macroeconomic shocks (Koren and Tenreyro, 2007) that reduce labor demand and directly affect workers and households that rely on their labor income. Many households lack the resources to buffer these shocks and are therefore highly vulnerable to poverty (Torche and Lopez-Calva, 2013; Ferreira et al., 1999). To protect against such risks, governments have implemented social safety nets. In developing countries, these typically include unemployment insurance (UI) for formal workers and conditional cash transfers (CCTs) for poor households with children (World Bank, 2015). The COVID-19 pandemic exposed large protection gaps in the face of the sudden and widespread loss of employment and income, leading many governments around the world—especially in developing countries—to expand or adapt existing social protection systems, scale up cash transfer programs, and, in some cases, temporarily broaden UI coverage (Gentilini, 2022).

In developing countries, UI primarily covers displaced workers strongly attached to the formal labor market, but high informality and weak enforcement severely restrict its reach, leaving many exposed to job loss without protection, often with regressive consequences (Sehnbruch, 2020; Vodopivec, 2013; Gerard and Naritomi, 2021; Gerard and Gonzaga, 2021). Where UI coverage is limited, CCTs, unlike UI, offer noncontributory income support to low-income households and can stabilize income in times of hardship, even if they are not explicitly designed as shock-response instruments (Fiszbein et al., 2011; Banerjee et al., 2024; De Janvry et al., 2006). While both types of programs can help stabilize income, they target distinct populations. Many middle-income but vulnerable households—who are not poor enough to qualify for CCTs and may lack access to stable formal employment—often fall outside the reach of safety nets (Fiszbein and Schady, 2009; Levy and Schady, 2013; Gentilini, 2022; Vera-Cossio et al., 2023). Yet little is known about the extent to which social protection systems actually insure households against income losses following job displacement, which groups benefit from existing programs, and who remains unprotected.

We use rich administrative data from Uruguay to provide novel evidence on how the social safety net mitigates the consequences of job displacement for different targeted groups. We exploit mass layoff events to examine post-displacement employment and earnings trajectories. We then estimate the extent to which Uruguay's two main social protection programs—UI and the conditional cash transfer program (Asignaciones Familiares–Plan de Equidad, or AFAM-PE)—buffer income declines. Uruguay offers an ideal setting for this analysis, as it combines a long-standing contributory UI system with a large CCT program and provides high-quality administrative data that allows us to link workers' labor market histories with benefit records.

We exploit mass-layoff events to implement a difference-in-differences design that identifies the causal effect of job displacement on workers' employment and earnings. To examine the mitigating role of social protection, we extend this analysis in two ways. First, we exploit program eligibility discontinuities—based on UI tenure and the CCT program's poverty-score thresholds—which generate plausibly exogenous variation near the thresholds, in a regression discontinuity (RD) design. Second, we combine program eligibility discontinuities with the mass-layoff shocks in a regression-discontinuity difference-in-differences (RD-DiD) framework (Grembi et al., 2016; Eggers et al., 2018; Takahashi, 2024). Even without concurrent policy changes at the cutoffs, the RD-DiD strategy lets us estimate, within the same setting, both the direct effects of job displacement and the dynamic, event-time impacts of UI and CCTs on post-layoff outcomes. This unified approach provides new evidence on the insurance value of each program and identifies which groups of displaced workers are effectively protected and which remain outside the reach of existing safety nets.

The first set of results shows patterns consistent with prior evidence (Jacobson et al., 1993; Schmieder et al., 2023; Bertheau et al., 2023; Britto, 2022): Individual labor earnings fall by roughly 50% in the first two quarters after displacement and, despite a partial recovery, do not return to pre-layoff levels even five years later. While employment gradually rebounds, the persistence of income losses is driven primarily by the intensive margin, with wages in post-displacement jobs remaining below pre-shock levels. We then show that these dynamics exhibit substantial heterogeneity across workers, depending on their position in the income distribution and their access to social protection. In particular, workers covered by CCT programs experience faster recovery of earnings, whereas those without coverage face more persistent losses. This distinction is especially relevant in the context of developing countries, where many workers are employed in the informal sector. In such settings, the presence of CCT programs plays a central role in shaping post-displacement outcomes. While UI does not appear to generate significant medium-term gains, low- and middle-income workers benefit from the sustained income support provided by AFAM-PE, which helps improve their labor market outcomes following adverse displacement shocks.

Our study contributes to the literature on the design and efficiency of social insurance systems in developing economies (Gerard and Gonzaga, 2021; Gerard and Naritomi, 2021; Britto, 2022). Most existing work focuses on UI as the primary policy instrument to smooth income losses after job displacement, but little is known about the role of CCTs in this context. We extend this literature by jointly analyzing contributory (UI) and noncontributory programs (CCTs)—two pillars of social protection that target distinct populations but coexist within the same institutional framework. In doing so, we provide the first evidence on how these programs interact to buffer income shocks following job loss, identifying which groups of workers are effectively insured and which remain outside the reach of existing safety nets.

Our paper closely relates to that of Britto (2022), who analyzes the employment effects of a lump-sum job-displacement transfer and a one-month extension of UI in Brazil. However, the one-time cash bonus examined by Britto operates outside the formal safety net and thus resembles a liquidity shock rather than an insurance mechanism. In contrast, we study programs that form the core of the social protection system.

The rest of the paper is organized as follows. Section 2 provides institutional background. Section 3 describes our data sources. Section 4 outlines our empirical strategies, namely the event-study, RD, and RD-DiD designs. Finally, Sections 5 and 6 present preliminary results.

2 Institutional Background

Asignaciones Familiares–Plan de Equidad. AFAM-PE was established in 2008 to reform the cash transfer schemes that had been in place since 2005. Like most CCT programs in Latin America, it provides monetary support to low-income households with children. In our study period, the program covered approximately 400,000 children—about half of all children in the country—at a fiscal cost of roughly 0.4% of GDP, placing it among the region's largest programs, alongside PROGRESA-Oportunidades for Mexico and Bolsa Familia in Brazil (Giaccobasso, 2026).

The program applies two eligibility criteria: (i) a proxy means test and (ii) an income threshold. The test includes a set of sociodemographic variables and housing characteristics to approximate the household's probability of belonging to the first income quintile.[2] Program eligibility is determined by comparing each household's poverty score with an eligibility threshold. The score is calculated at the time of application and is updated only after an in-person verification visit by program officers. The second eligibility condition requires household per capita income to fall below a specified threshold. The income used for this test is computed at application and then periodically verified against administrative records as long as the household remains enrolled in the program. Household income comprises formal sources for all members: wages and other employment earnings reported to Social Security, as well as pensions. After enrollment, households are expected to meet school-attendance and periodic health-check requirements; In practice, however, monitoring and enforcement of these conditionalities were limited during the period covered by our data. While AFAM-PE eligibility is determined at the household level, the main outcomes analyzed in this paper are measured at the worker level. Household characteristics are used exclusively for policy assignment and sample characterization.

Program beneficiaries receive a cash transfer according to the number of children (using equivalence scales) and with an additional supplement for children enrolled in secondary

[2] The baseline proxy means test was estimated on Uruguay's national household survey using a fully saturated specification of household characteristics. The variables used to compute the score included building quality, household size, number of rooms, presence of children under age 18, mean years of schooling, and employment type, among others. The resulting coefficients were used out of sample on AFAM-PE application records to generate a score for each applicant household. See Bergolo and Cruces (2021); Amarante et al. (2011).

school. In 2008, the base transfer was approximately US$33 and the secondary-school top-up was US$14. Over 2008–12, transfers were sizable, averaging about 68% of the minimum wage for the typical AFAM household and roughly 48% of the poverty line (Bergolo and Cruces, 2021). Conditional on eligibility, the AFAM-PE benefit level is invariant to household members' earnings; it depends only on household composition (and schooling supplements).[3] There is no statutory time limit on participation. Families exit if they no longer meet some of the eligibility conditions or when the beneficiary turns 18.

Unemployment insurance. Uruguay's UI relies on policy on policy set by the Ministry of Labor and is administered by the social security agency (Banco de Prevision Social, or BPS). To qualify, workers must have experienced either (i) a permanent separation, (ii) a temporary layoff due to a complete suspension of employer activities, or (iii) a reduction in working days or hours of at least 25% (see Amarante et al., 2013). UI covers all formal private sector workers, with eligibility conditioned on contract type. For dependent employees, eligibility requires at least 180 days of work in the 12 months preceding the involuntary separation (270 days within the past 30 months for rural workers). For day laborers or piece-rate workers, the thresholds are 150 days in the prior 12 months (225 days within the past 30 months for rural workers). Workers cannot re-enroll until a full year has passed since they last received the benefit.

The benefit has a statutory maximum of six months; however, the executive branch may, at its discretion, authorize extensions. As a general rule, if a worker has not been recalled after six months, the separation is treated as a dismissal, entitling the worker to severance pay. UI benefits are terminated if the beneficiary obtains another formal job (i.e., one with social security contributions), rejects a job offer, or begins receiving a pension. The system neither monitors job search activity nor applies punitive sanctions. In 2009, Uruguay reformed its UI by altering the benefit schedule. Prior to the reform, beneficiaries received a flat 50% of their average wage for up to six months. Starting in 2009, a declining schedule was adopted—with 66% in month 1 lowering to 40% by month 6—in a way that kept average transfer amounts unchanged.[4] Another change was to make UI and engagement in other economic activity

[3]In other words, as long as the family income is below the eligibility threshold, their members can increase (formal) earnings without any reduction in benefits.

[4]The declining schedule is: 66%, 57%, 50%, 45%, 42%, and 40% for months 1–6, respectively.

compatible. More specifically, beneficiaries may now temporarily suspend benefits when they take a short-term job and subsequently resume receipt upon returning to unemployment.

3 Data Sources

This section describes our data sources. To estimate the effects of UI and CCTs, we link social security records for individuals and firms to AFAM-PE using a unique individual identifier. This linkage yields a longitudinal panel of individuals spanning more than 20 years, enabling analysis of labor market dynamics around mass layoff events.

Labor histories from social security records. We use employer-employee matched administrative records from the Ministry of Labor and Social Security covering the universe of formal employment in Uruguay from 1998 to 2022. The dataset covers employment and contribution histories for all registered (formal) workers in the private and public sectors, including those registered as self-employed, with at least one month of contributions to the social security system (BPS). The dataset provides monthly job-level information—start date, contract type, industry/sector, region, days worked, wages, and non-wage benefits—and records the dates of and reasons for separations (e.g., layoff, contract expiration, retirement). While the data capture only formal employment, Uruguay's informal sector is relatively small (about 17%–20% of total employment). Additionally, we link these records to other Social Secuirty Administration (SSA) databases containing information related to UI and sick-pay benefits.

These administrative records allow us to trace earnings trajectories before and after dismissal and to construct our treatment indicator for separations due to mass layoffs. From these data, we also define our main (individual level) outcomes. Total labor earnings are defined as the sum of all labor-related income received by a worker in a given year. Labor earnings are constructed exclusively from earnings reported in social security records and therefore exclude transfer payments, including AFAM-PE benefits and other non-labor-income sources. Therefore, all earnings measures throughout the paper refer to individual labor income rather than household income. Formal employment is a binary indicator equal to 1 if a worker has positive registered labor earnings in that period, and 0 otherwise. An indicator value of 0 encompasses states without registered earnings, including unemployment, inactivity, retirement, or informal employment. We also reconstruct eligibility for UI and UI take-up in the months following separation.

AFAM-PE records. We use administrative records from the Ministry of Social Development (MIDES) covering the universe of program applications from the start of the program (2008) through 2019. Household-level information is used only to determine AFAM-PE eligibility and to characterize workers' socioeconomic conditions. For each application, the file reports household- and individual-level characteristics collected via interviews conducted by program officials. Available variables include the application date; the eligibility (poverty) score used for means testing; program status; housing characteristics; household size and composition; and individual sociodemographic characteristics (sex, age, schooling, labor market participation). Monthly data following the application date track program status, payments to the household, and, when applicable, updates to housing information. The raw application data comprise 747,204 forms linked to 1,476,696 unique individuals.

4 Empirical Strategy

4.1 Sample Selection

The main challenge in identifying the effects of layoffs on workers' employment and income trajectories is the potential for selection of displaced workers. Following standard practice (Gathmann et al., 2020; Bertheau et al., 2023; Britto et al., 2022), we focus on mass-layoff events, defined as contractions of at least 33% of a firm's total employment over the preceding 12 months. Because mass layoffs are usually triggered by firm-level shocks exogenous to individual workers, this restriction mitigates selection on both observable and unobservable characteristics at the time of separation. We exploit variation in the timing of mass layoffs—arguably unrelated to workers' decisions—and estimate a difference-in-differences model of job loss.

We restrict the analysis sample to private sector employees aged 20–60 with pre-layoff earnings at or above the minimum wage and employed at firms with at least five workers.Because CCT benefits are only available to households with an eligible child, we further restrict the sample to workers with at least one potentially eligible child when examining the program's mitigating effects. Our treatment group consists of workers displaced in mass-layoff events between 2007 and 2012. This window provides sufficient pre-treatment periods to assess parallel trends and multiple post-layoff periods to allow us to estimate dynamic effects. It also falls entirely within the implementation period of the CCT program. Appendix Table A.1 reports descriptive characteristics for the universe of workers observed in the administrative records and illustrates the effects of applying successive sample restrictions to the data until arriving at our final analysis sample. Relative to the universe of workers, the final sample appears to overrepresent young men residing in the capital city and workers with strong attachment to the formal labor market.

We select the control group from the universe of formal workers without a mass layoff via an exact-matching procedure. Matching covariates include sex, age (five-year bins), contract type, region (capital vs. rest of the country), industry, pre-layoff yearly earnings (UYU 10,000 bins), and months employed in the previous year. When a treated worker has multiple controls, one control is randomly selected. Using one-to-one exact matching, we successfully pair 75% of treated workers with a control, resulting in a sample of 23,500 workers. Using the matched treated-control pairs, we build a balanced panel spanning from five years before to five years after the layoff event. For control units, we assign placebo event dates equal to the layoff period of their matched treated worker.

4.2 Event-Study Model

To estimate the effects of mass layoffs on workers' earnings and employment, we implement an event study for the sample of treated and control workers constructed via the exact-matching procedure described above. Following Bertheau et al. (2023), our baseline specification is

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{k=-5}^{k=5} \gamma_k \mathbf{1}(t = t_i^* + k) + \sum_{k=-5}^{k=5} \theta_k \mathbf{1}(t = t_i^* + k) \times T_i \times 1\{EventTime_{it} = k\} + \beta X_{i,t} + \epsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ denotes yearly earnings or employment and T_i is an indicator equal to 1 for workers who experience a mass-layoff event during the study period and 0 otherwise. The interaction between T_i and event-time indicators identifies dynamic treatment effects before and after displacement. We also include individual (α_i) and year (λ_t) fixed effects. In all specifications, coefficients are normalized to the year prior to the layoff by omitting period $k = -1$. Standard errors are clustered at the worker level.[5]

The coefficients θ_k capture the dynamic effects of displacement in event time, comparing treated workers to their matched controls five years before and after the event date t_i^*. Post-event coefficients ($k > 0$) constitute our primary objects of interest, summarizing the impact of mass layoffs on outcomes. In contrast, pre-event coefficients ($k < 0$) serve as diagnostics for parallel trends and potential anticipatory responses to the shock. Our matching design ensures that controls are never treated, mitigating concerns arising in the recent literature on staggered difference-in-differences about two-way fixed-effects estimators that average heterogeneous treatment effects (De Chaisemartin and dâHaultfoeuille, 2023).

Finally, we examine heterogeneity across vulnerable populations. First, we estimate effects for low-income individuals, defined as those eligible for the CCT program at baseline (measured prior to the mass-layoff event). Second, we assess impacts for the vulnerable but non-poor, as classified based on their pre-shock position in the income distribution.

4.3 RD Strategy

To estimate the effects of AFAM-PE and UI, we exploit the discontinuities induced by each program's eligibility rules. Specifically, both programs include arbitrary eligibility criteria based on administrative cutoffs—prior-year days worked for UI and a poverty-score threshold for AFAM-PE—which generate plausibly exogenous variation in treatment near the thresholds.

We implement an RD design to identify causal effects locally at each cutoff. Under standard RD assumptions—continuity of potential outcomes at the cutoff and perfect compliance with eligibility rules— the discontinuity in outcomes at the threshold identifies the average treatment effect at the cutoff for each program (Hahn et al., 2001). Because of imperfect compliance with the rules of both programs, we implemented a fuzzy RDD strategy based

[5]We also implement a quarterly version of this specification.

on the following two-equation system:[6]

$$Y_i = \alpha + \tau \hat{T}_i + \beta_1 f(Z_i^s) + \beta_2 f(Z_i^s) T_i + \mathbf{X}_i + \upsilon_i \tag{2}$$

$$T_i = \mu + \delta D_i^s + \lambda_1 f(Z_i^s) + \lambda_2 f(Z_i^s) D_i^s + \mathbf{X}_i + \varepsilon_i \tag{3}$$

Here, $s \in \{\text{CCT, UI}\}$ indexes the policy; Y_i denotes the outcome of interest (namely, earnings or employment in the years following the layoff); $\hat{T}_i$ represents the predicted treatment status obtained from the first-stage eligibility discontinuity (beneficiary of the CCT program or UI); Z_i^s is the running variable that determines eligibility (the poverty-score index for the CCT or prior-year days worked for UI); and $\mathbf{X}_i$ is a vector of control covariates.

Because treatment T_i may be endogenous with respect to the outcome Y_i, we instrument T_i with an eligibility indicator D_i^s using the first-stage equation. The indicator D_i^s equals 1 when the observation lies on the eligible side of the cutoff—that is, when the poverty-score index meets the CCT threshold or when prior-year days worked meet the UI threshold—and 0 otherwise. The 2SLS estimator delivers $\hat{\tau}$, the causal effect of receiving the program on the outcome Y_i.

In addition, we examine whether quasi-random variation in program eligibility status influences individual outcomes by estimating the following reduced-form equation:

$$Y_i = \alpha_0 + \tau_{RF} D_i^s + \beta_1 f(Z_i^s) + \beta_2 f(Z_i^s) D_i^s + \varepsilon_i \tag{4}$$

The reduced-form coefficient τ_{RF} is interpreted as the intention-to-treat effect of program eligibility on outcomes.

We estimate the RD models using local linear regressions fitted separately on each side of the cutoff (Imbens and Lemieux, 2008; Calonico et al., 2019a). Following Calonico et al. (2014), we report mean-squared-error-optimal (MSERD) point estimates and standard errors and conduct inference using bias-corrected estimates for p-values. Standard errors are clustered at the individual level.

[6] Britto et al. (2022) and Gerard and Gonzaga (2021) use a similar strategy for the case of UI in Brazil, and Bergolo and Cruces (2021) implemented a RD strategy based on the CCT poverty score.

We next detail the RD design for each program, defining the endogenous and exogenous variables in each case. We document first-stage relevance by showing that D_i^s strongly predicts program take-up in the first-stage regression. We also provide evidence there is no manipulation of the running variable at the cutoff, a necessary condition for the fuzzy RD design to be valid. Under these assumptions, the local variation in treatment assignment induced by the threshold is as good as random.

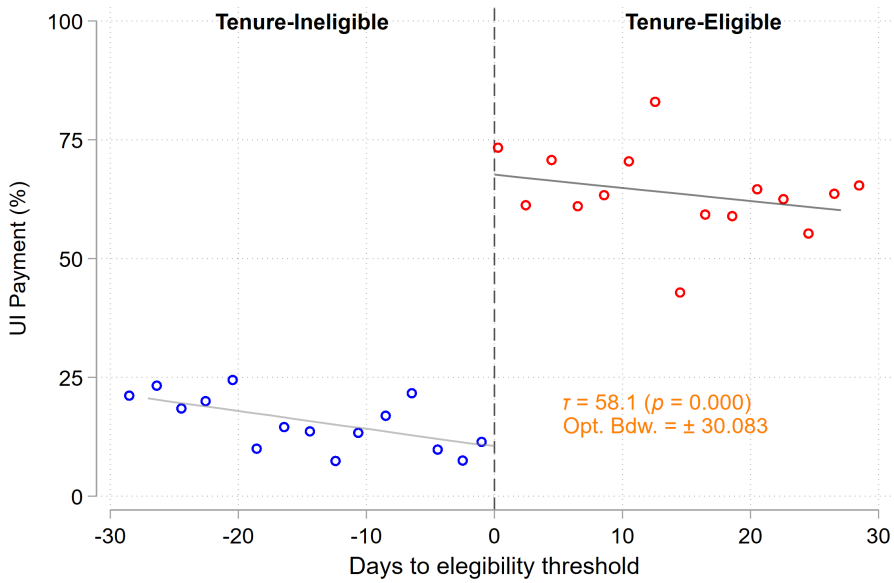
4.3.1 Unemployment Insurance

To determine eligibility for UI, we assess compliance with the tenure rule using each worker's contract type and sector of activity at the time of the mass layoff. The running variable is normalized to the applicable days-worked threshold for UI, Z_i^{UI}. Our endogenous treatment indicator, T_i^{UI}, equals 1 if the worker receives at least one month of UI benefits in the layoff month or the month immediately after.

First stage. We first establish instrument relevance for the UI case by estimating the first-stage specification in Equation (3). Figure 1 plots the probability of receiving UI benefits in the mass-layoff month, $\Pr(T_i^{\text{UI}} = 1)$, against the running variable—the distance (in days) from the UI eligibility threshold, Z_i^{UI}. In the figure, we restrict the running variable (tenure, in days) to the data-driven optimal bandwidth of Calonico et al. (2020), approximately ± 60 days around the cutoff. Each point plots a within-bin mean of UI receipt, computed over 15 quantile-spaced bins on each side of the threshold.

Figure 1 shows a pronounced first-stage discontinuity: The probability of receiving UI rises by about 50 percentage points at the eligibility cutoff, and the jump is statistically significant at the 1% level (p-value ≤ 0.001).

Figure 1: Effect of Eligibility Rule on Probability of Receiving UI Benefits



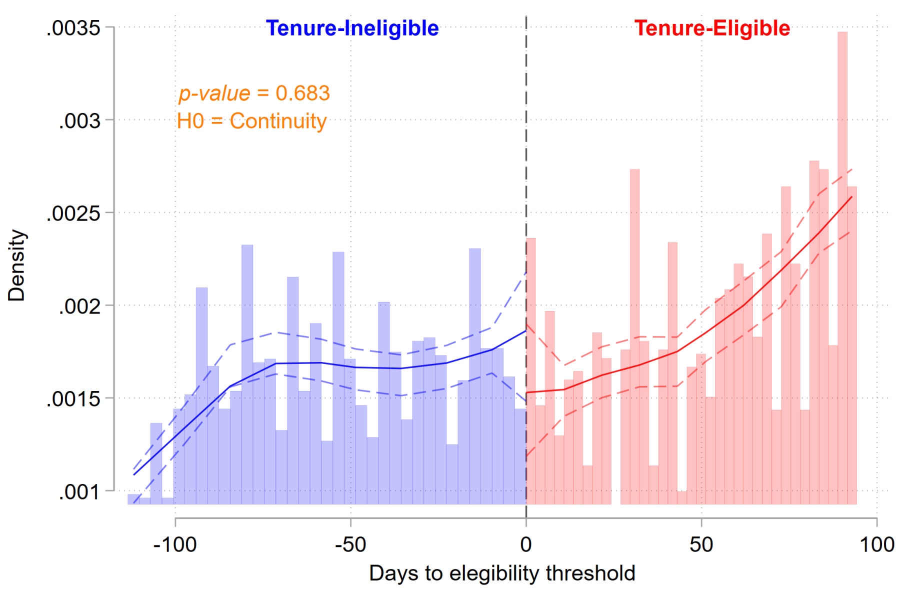
Notes: The figure presents the identification strategy and the empirical first stage of the RD design. It plots the share of workers receiving UI benefits as a function of the distance (in days) from the eligibility cutoff. The bandwidth is chosen by minimizing the mean squared error (MSERD) using the default settings of the `rdrobust` Stata command, which implements local linear regressions with triangular kernel weights (Calonico et al., 2019b, 2020). Robust standard errors are clustered at the worker level. The sample used corresponds to the baseline sample. Source: Authors' calculations using administrative records from BPS and MIDES.

Compliance, however, is imperfect: Figure 1 shows that roughly 20% of ineligible workers receive the benefit, while more than 25% of eligible workers do not. Two factors could account for most of the noncompliance among eligible workers: (i) A subset of laid-off workers secure a new job before (or during) the benefit month, so no UI payment is recorded even though they satisfy the eligibility cutoff. (ii) Beyond the tenure/work-history requirement, UI rules require that the entitlement has not been previously exhausted; workers who have already exhausted their benefit do not receive UI despite meeting the tenure threshold. For the ineligible workers who receive UI, two mechanisms likely explain the discrepancy. First, some workers combine employment across sectors with heterogeneous eligibility rules in the pre-layoff period, so applying a single tenure threshold can misclassify their eligibility. Second, under the rotating UI regimeâin which workers alternate between working and non-working days or hours rather than experiencing a full layoffâconditions are verified only at initial activation; thereafter, benefits can continue even if the worker returns to work intermittently and fails to meet the ongoing eligibility rules.

Manipulation. For the RD design to be valid, the running variable must be free of manipulation. In our setting, the running variable is the number of days of tenure used to determine UI eligibility. Manipulation could arise if workers or employers time the dismissal date to satisfy the UI threshold. We mitigate this concern by focusing on mass layoffs as plausibly exogenous shocks to workers.

Figure 2 implements density-continuity tests following **?** and **?**. We plot a histogram of (normalized) days relative to the eligibility cutoff and report the associated continuity p-value. Consistent with no manipulation, the test yields a p-value=0.56, so we fail to reject density continuity at the threshold.

Figure 2: Manipulation Test: Days Worked Prior to Layoff



Notes: The figure reports the density-discontinuity (continuity) test for the running variable—days worked in the previous year—at the unemployment insurance eligibility threshold, following **?**. Bandwidths and polynomial order are selected using the default options of the `rddensity` Stata command. The displayed p-value corresponds to the null of no discontinuity in the density at the cutoff. Source: Authors' calculations using administrative records from BPS and MIDES.

4.3.2 CCT Program

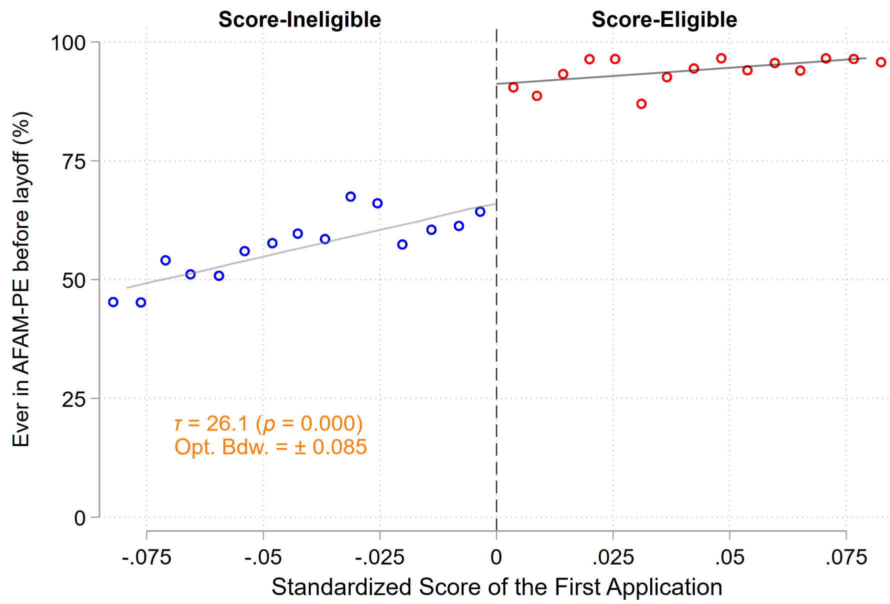
To estimate the effect of the CCT program, we define individual exposure based on whether the worker's household was a beneficiary in the quarter preceding the mass layoff. This timing is intended to capture the program's insurance-like role for subsequent employment trajectories. To avoid endogenous take-up, we exclude households that apply after the shock.

Two challenges arise when implementing the RD design to analyze the CCT program. First, because the program has operated since 2008, households may reapply multiple times, potentially manipulating the poverty-score running variable to regain eligibility. Second, repeated applications introduce ambiguity into the definition of treatment status over time. Following Jepsen et al. (2016) and Giaccobasso (2026), we select the running variable at the first application and define the endogenous variable as ever-treated. This approach mitigates concerns about score manipulation at reapplication and aligns identification with pre-shock program status.

First stage. Figure 3 plots the RD first stage for the CCT program. It depicts the probability of being a beneficiary as a function of a household's standardized poverty score at its first application. The sample is restricted to the MSE-optimal bandwidth following Calonico et al. (2020).

Compliance with the eligibility rule is not perfect, but most of the apparent targeting error reflects households classified as ineligible. This stems from our design choice based on eligibility at the first application to limit the scope for learning and score manipulation; it implies that some later beneficiaries are coded as ineligible in our baseline. However, we document a discrete jump of 30 percentage points in program participation at the cutoff (p-value $\leq$ 0.001). In duration terms, this first stage corresponds to about 12 additional months of program receipt for households just above the threshold relative to otherwise similar households just below it (see Appendix Figures A1 and A2).

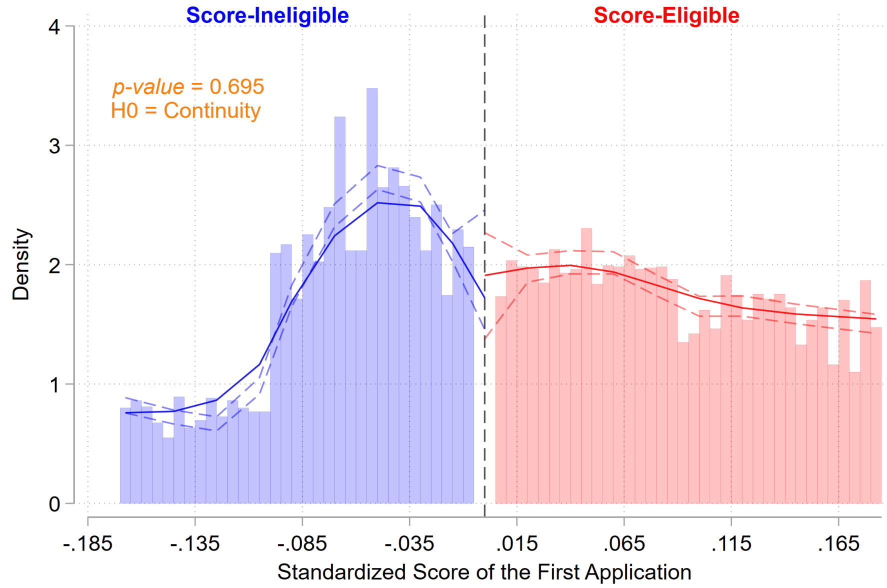
Figure 3: Effect of Eligibility Rule on Probability of Receiving CCT Benefits



Notes: The figure presents the identification strategy and the empirical first stage of the RD design of the AFAM-PE program. It plots the share of workers receiving AFAM-PE benefits as a function of the poverty score. The bandwidth is chosen by minimizing the mean squared error using the default settings of the `rdrobust` Stata command, which implements local linear regressions with triangular kernel weights (Calonico et al., 2019b). Robust standard errors are clustered at the worker level. The sample used corresponds to the baseline sample. Source: Authors' calculations using administrative records from BPS and MIDES.

Manipulation. Figure 4 reports the manipulation test for the CCT program (**??**). The running variable is the poverty score from the proxy means test that determines eligibility. In principle, applicants could try to misreport their characteristics to qualify; however, the score aggregates many household characteristics, limiting the scope for targeted manipulation. Restricting the analysis to first applications further reduces strategic adjustment after learning the rule. Consistent with this, the density shows no excess mass at the cutoff, and the local-polynomial density test fails to reject continuity (p-value $=$ 0.806).

Figure 4: Manipulation Test: CCT Program Eligibility Poverty Score



Notes: The figure reports the density-discontinuity (continuity) test for the running variable—poverty score—at the AFAM-PE eligibility threshold, following **?**. Bandwidths and polynomial order are selected using the default options of the `rddensity` Stata command. The displayed p-value corresponds to the null of no discontinuity in the density at the cutoff. Source: Authors' calculations using administrative records from BPS and MIDES.

4.4 RDD-DiD

To estimate post-layoff policy effects, we implement an RD-DiD design (Grembi et al., 2016; Eggers et al., 2018 and Galindo-Silva et al., 2021 for fuzzy RD-DiD, as in our case). The approach combines (i) an RD in program eligibility at a cutoff with (ii) a difference-in-differences (DiD) comparison across pre- and post-layoff periods for units around that cutoff. RD-DiD is commonly used when multiple interventions coincide at the same threshold, causing other confounding events to coincide with the treatment of interest at the discontinuity.

In our setting, even absent concurrent policy changes at the cutoff, RD-DiD allows us to derive the effect of policies over event time and attenuates concerns about endogenous shifts in the composition of eligible and ineligible units around the threshold (for example, optimal behavioral responses). Formally, we restrict the sample to workers within the optimal bandwidth around each program's eligibility threshold and estimate the following econometric specification:

$$
\begin{aligned}
Y_{it} = \gamma_0 &+ \sum_{k=-K}^{K} \gamma_{1,k}\, T_{it} \times D_{it}^{p} + \sum_{k=-K}^{K} \gamma_{2,k}\, T_{it} + \sum_{k=-K}^{K} \gamma_{3,k}\, D_{it}^{p} + \sum_{k=-K}^{K} \gamma_{4,k}\, (Z_{it} - c) \\
&+ \sum_{k=-K}^{K} \gamma_{5,k}\, T_{it} \times D_{it}^{p}\, (Z_{it} - c) + \sum_{k=-K}^{K} \gamma_{6,k}\, T_{it}\, (Z_{it} - c) + \sum_{k=-K}^{K} \gamma_{7,k}\, D_{it}^{p}\, (Z_{it} - c) \qquad (5) \\
&+ \sum_{k=-K}^{K} \gamma_{8,k}\, \mathbf{X}_{it} + \delta + \alpha_i + \lambda_t + \varepsilon_{it}
\end{aligned}
$$

Each coefficient γ_k captures the effect k periods after the mass-layoff event (with the omitted pre-event period as the reference). T_i is an indicator for displaced workers, and D_i^p denotes eligibility for program p (AFAM-PE or UI). We include worker and calendar-year fixed effects (α_i and λ_t). The running variable Z_i^p measures distance to the policy-specific cutoff c_p (prior-year days worked for UI; poverty score for AFAM-PE) and is modeled flexibly on either side of the threshold. Our parameters of interest are the interaction terms $T_i \times D_i^p \times \mathbf{1}\{k\}$, which identify the difference-in-discontinuity effects for each event time k around the layoff.

Identification rests on the standard assumptions of the DiD and RD strategies. On the DiD side, units just above and below each cutoff would have followed parallel pre-trends absent the layoff, and these trends must be stable over the event window. On the RD side, potential outcomes (and predetermined covariates) must be continuous in the running variable at the eligibility threshold, with no manipulation around the cutoff. In addition, no other policy or discontinuity should differentially affect workers on either side of the threshold in a neighborhood of the cutoff, so that the only break at the threshold is program eligibility.

5 Results

We now present the main results regarding how mass layoffs affect workers' trajectories in the labor market.

5.1 Descriptive Evidence

We begin by documenting the monthly incidence of mass layoffs among firms over the study period. We then summarize the quality of the exact matching between displaced (treated) and non-displaced (control) workers. Finally, we describe program coverage, documenting how enrollment in UI and AFAM-PE varies across displaced workers and how our sample is positioned along the income distribution.

Mass layoffs. Figure A3 plots the monthly share of firms experiencing a mass layoff. In Panel (a), the series peaks during Uruguay's 2001–2 economic crisis at roughly 6% of firms. Over our analysis window (2007–12), the incidence averages about 4% per month. Panel (b) disaggregates by firm size: Smaller firms exhibit a higher incidence—above 6% on average—whereas medium and large firms report 3%–4%. By sector, firms experiencing mass layoffs are concentrated in trade and transportation services and in business support services, with a nontrivial share in primary activities as well (see Appendix Figure A4).

Matching procedure. Table 1 reports descriptive statistics for the socioeconomic characteristics of the matched sample. The dataset includes nearly 54,000 workers split evenly between treated and control groups (26,875 each). By construction, all matching variables are perfectly balanced between treated and control workers, as we employ exact matching on gender, region, firm size, months employed, and tenure. The only exceptions are age and income, which are matched on bins rather than exact values, allowing for small within-bin variation. Treated workers are on average 32.6 years old compared to 35.5 for controls ($p < 0.01$), and average income differs slightly across groups (UYU 342,164 vs. 336,232), though the latter difference is not statistically significant ($p = 0.27$).

Program coverage. Among workers in our sample who experience a mass layoff, roughly 75% meet the tenure requirement to qualify for UI. Satisfying the formal-tenure threshold does not imply that they are not employed in the informal sector. Many workers combine formal and informal jobs or report only part of their earnings in the formal sector. Because UI benefits are tied to formal earnings, this can materially reduce the benefit amount received after displacement. Additionally, eligibility is not synonymous with receipt, as take-up can be incomplete and some workers may be ineligible at the time of the claim because they exhausted prior benefits, limiting effective insurance at layoff.

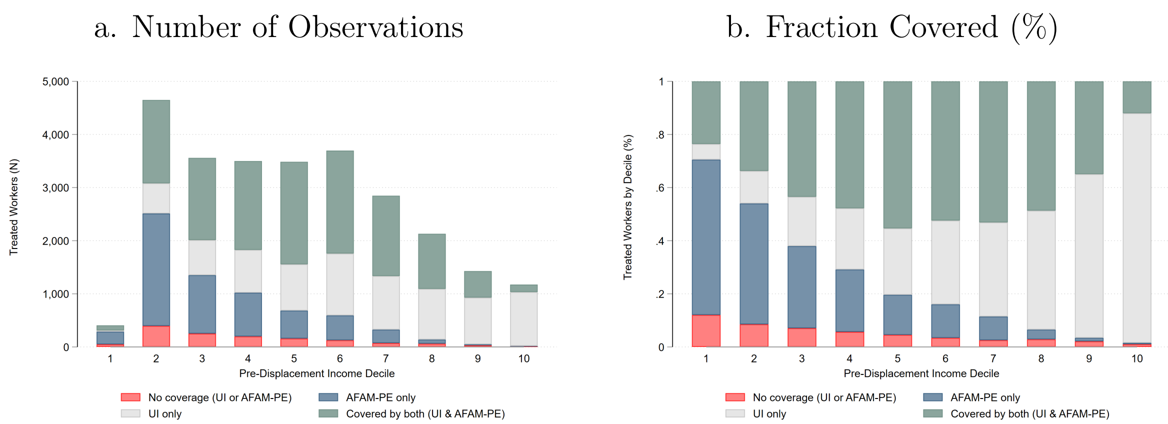
Table 1: Baseline Balance: Matched Treated and Control Workers

	Treated (1)	Control (2)	Diff. P-Value (3)
Matching Variables			
Age	32.55	35.48	0.00***
Female	42.71	42.71	1.00
Region (Capital)	70.10	70.10	1.00
Average Income (UY)	342,164.28	336,232.38	0.27
Months Employed	10.09	10.09	1.00
6-10 workers (%)	23.66	23.66	1.00
11-50 workers (%)	52.27	52.27	1.00
+50 workers (%)	24.07	24.07	1.00
Tenure (in months)	10.09	10.09	1.00
Observations	26,875	26,875	

Notes: Columns (1)–(2) report sample means for displaced (Treated) and matched non-displaced (Control) workers. Column (3) reports two-sided p-values from tests of equality of means. Region (Capital) equals 1 for Montevideo. Average Income (UY) is measured in Uruguayan pesos. Months Employed refers to months worked in the year prior to displacement; Tenure (in days) is total days with the employer. Firm-size rows report the share of workers in firms with 6–10, 11–50, and >50 employees. Eligible UI indicates meeting the tenure requirement for unemployment insurance; Applied to AFAM-PE indicates any prior application; Eligible to AFAM-PE is defined by the poverty-score cutoff. Asterisks denote significance levels: p<0.10 (*), p<0.05 (**), p<0.01 (***).

Figure 5 plots policy coverage by pre-layoff income decile (year $t = -1$) for workers displaced in mass layoffs. Most displaced workers lie in the lower half of the distribution. Coverage patterns diverge sharply across programs: UI coverage is nearly universal in the top deciles, whereas CCT-only coverage is concentrated in deciles 2–3. Uncovered workers are disproportionately in the bottom deciles; in the middle, a sizable share is covered by both programs. This overlap indicates that AFAM-PE can complement UI by providing incremental support at displacement, strengthening the buffer against job-loss shocks.

Figure 5: Program Coverage by Pre-Displacement Income Decile



Notes: The figure shows, for displaced (treated) workers, the number covered by UI only, AFAM-PE only, both UI and AFAM-PE, or neither in the year prior to job loss. Deciles are computed from pre-layoff formal labor income ($t = -1$). Coverage is defined at the time of displacement using administrative eligibility/participation records. The figure highlights near-universal UI coverage at the top of the distribution, AFAM-PE-only coverage is concentrated in lower deciles (notably 2–3), with meaningful overlap in the middle deciles. Source: Authors' calculations using administrative records from BPS and MIDES.

Vulnerable and middle-income groups. We next examine how workers in our treatment and control samples are positioned along the income distribution, including their potential classification into vulnerable and middle-income groups. To do so, we rely on two alternative approaches to classify workers into income-based subgroups of the population. First, following standard definitions based on daily per capita income in purchasing power parity (PPP) terms (for example, Banerjee and Duflo (2008)), we define four income groups: poor (USD 0–5.5), vulnerable (USD 5.5–13), middle income (USD 13–70), and high income (above USD 70 per day). This classification allows us to compare our results with a broad set of international studies and corresponds to the standard income group definitions commonly used for Latin American countries (World Bank, 2021; Vera-Cossio et al., 2023; Azevedo et al., 2015; Cruces et al., 2011). Because our administrative records do not contain complete information on household composition and total household income, we impose a simplifying assumption to construct per capita income measures. Namely, we assume that the household consists of the sampled adult—who is also assumed to be the sole income earner—and the number of dependent children observed in the data. While restrictive, this assumption allows us to consistently locate workers within the income distribution using the available information.[7]

As alternative approaches, we rely on two complementary income classifications. The first is based on absolute income thresholds, using individual labor income and the official poverty line for a single-adult household. Under this approach, we define four groups in relative terms: poor (income below one poverty line), vulnerable (between one and two poverty lines), middle income (between one and three poverty lines), and high income (above three poverty lines). The second approach is based on relative income positions within the distribution, classifying workers according to their income relative to the median. Specifically, we define four groups: workers earning less than 60% of the median, between 60% and 100% of the median, between 100% and 200% of the median, and more than 200% of the median.

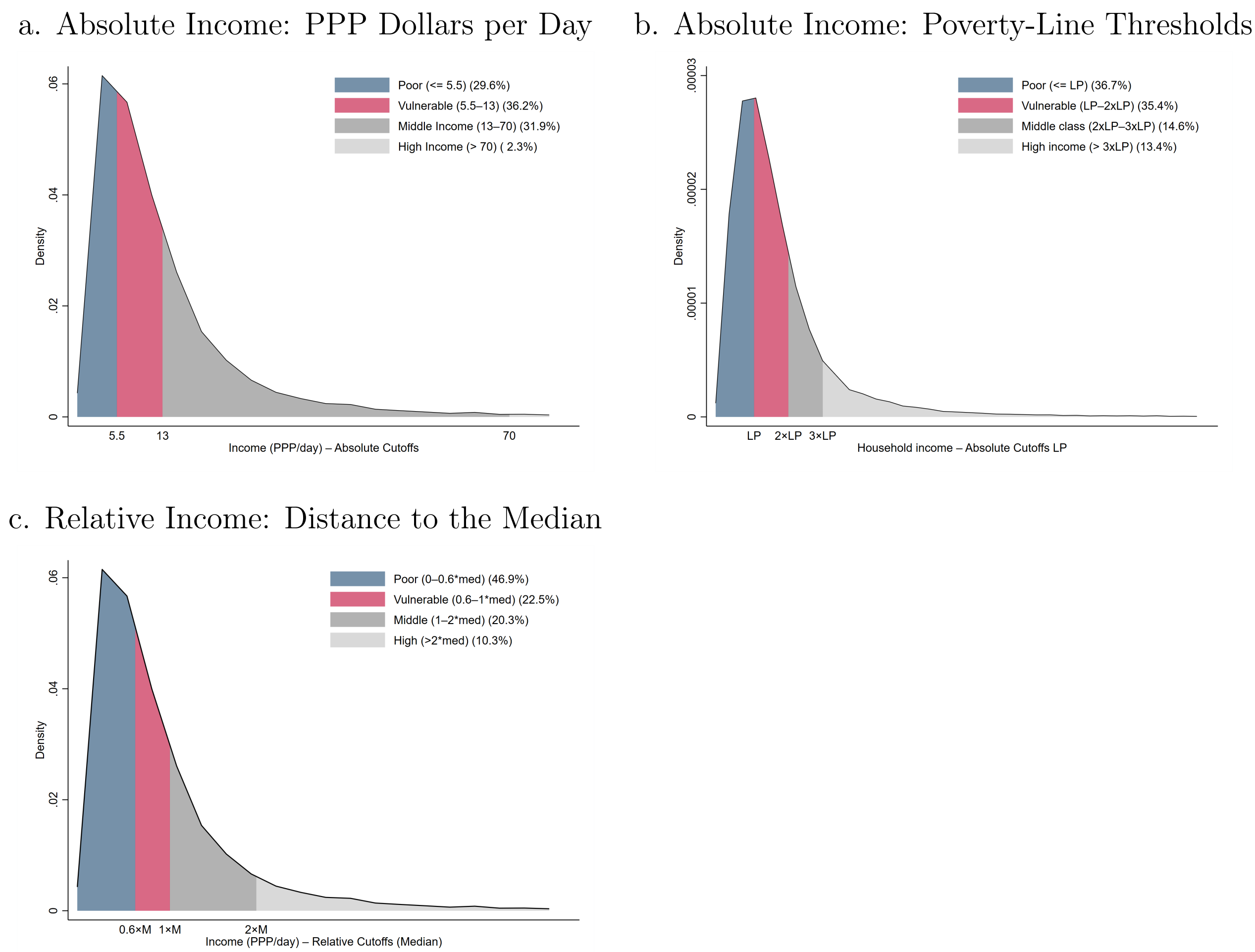
Figure 6 depicts the distribution of treated workers in our sample under these complementary classification schemes. Under the absolute PPP-based definition (Panel (a)), a large share of treated workers fall into the poor and vulnerable categories, with only a small fraction classified as high income.[8] The alternative approaches yield similar patterns, confirming that a substantial proportion of treated workers are located close to the official poverty threshold (Panel (b)) and below the median income level (Panel (c)) in Uruguay. These patterns indicate that the population affected by mass layoffs in our analysis is economically vulnerable and concentrated in the lower half of the income distribution, even when alternative income classifications are applied.

Appendix Figure A7 plots policy coverage across income groups defined using absolute income thresholds. Similarly to the patterns observed when using income deciles, coverage differs sharply across programs. CCT-only coverage is highly concentrated among poor households and, to a lesser extent, vulnerable workers. In contrast, UI coverage is nearly universal within the middle-income group. In addition, nearly half of treated workers in the vulnerable group are covered by both programs.

[7]As an alternative specification, we also construct household income measures assuming the presence of an additional adult in the household with earnings identical to those of the sampled individual.

[8]Appendix Figure A6 replicates this figure under an alternative assumption used to construct household income.

Figure 6: Income Distribution of Treated Workers Under Alternative Classification Schemes



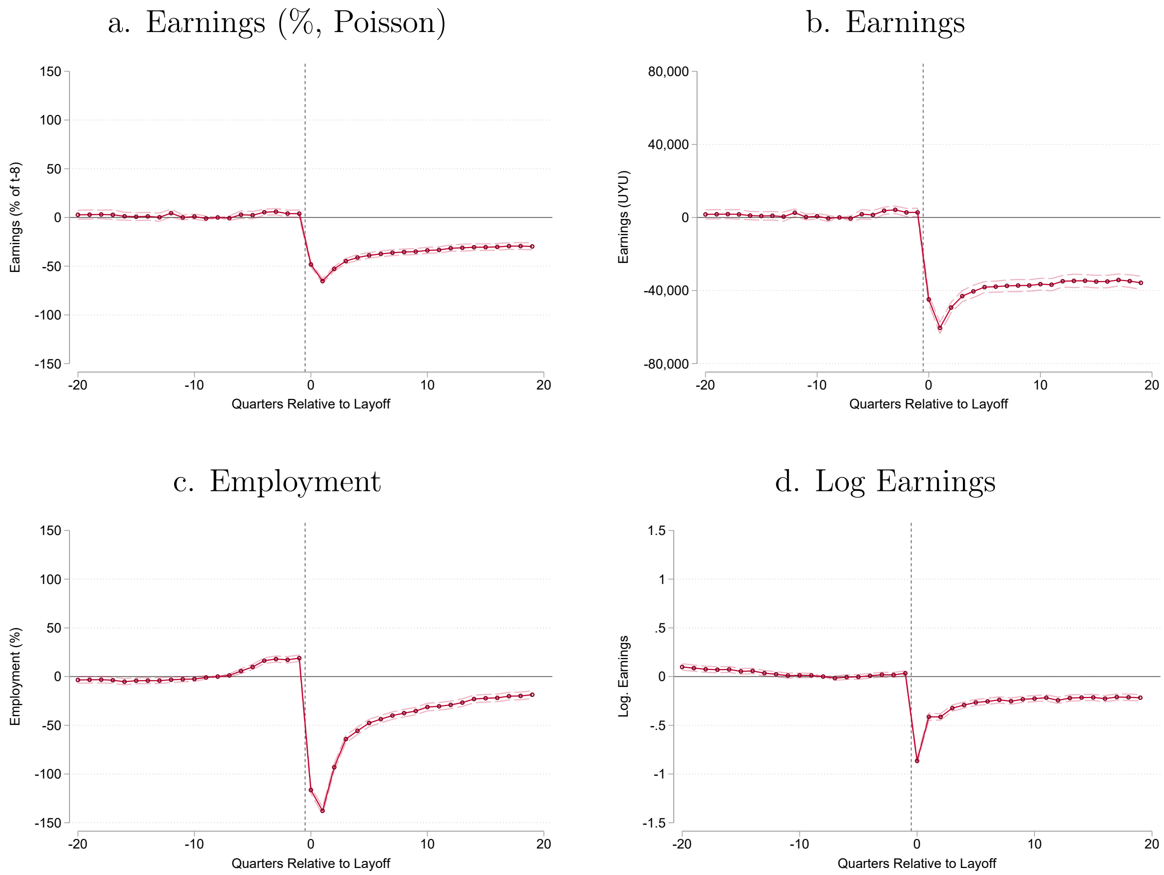
Notes: The figure shows, for displaced (treated) workers, the income distribution under alternative income classification schemes. Panel (a) classifies individuals using absolute income thresholds based on daily income measured in purchasing power parity (PPP) dollars, distinguishing poor, vulnerable, middle-income, and high-income groups following standard international definitions (USD 5.5, 13, and 70 per day). Panel (b) applies absolute income cutoffs defined relative to the official poverty line for a single-adult household in Uruguay. Panel (c) presents a relative classification based on individual income relative to the median, with thresholds set at 60%, 100%, and 200% of the median income. Income for treated workers is computed from pre-layoff formal labor income ($t = -1$). For household income we assume that the household consists of the sampled adult—assumed to be the sole income earner—and the number of dependent children observed in the data. Source: Authors' calculations using administrative records from BPS and MIDES.

5.2 Effect of Job Loss on Labor Market Outcomes

Figure 7 presents event-study estimates of the effects of mass-layoff displacement on labor income and employment. Specifically, we plot the point estimates θ_k from Equation 1 for up to 20 quarters before and after the shock, omitting $k = -8$ as the reference period. All specifications include individual and calendar-year fixed effects and use exactly matched workers who did not experience a mass layoff as the control group. Panels (a) and (b) report effects on total labor income: Panel (a) presents estimates from a Poisson specification, while Panel (b) expresses changes relative to average income in the omitted period. Panel (c) shows effects on the extensive margin, defined as employment in the formal labor market, and Panel (d) depicts responses along the intensive margin.

Consistent with the displacement literature (Jacobson et al., 1993; Schmieder et al., 2023; Bertheau et al., 2023), the figure documents large and persistent earnings losses following mass layoffs. Earnings decline by roughly 50% in the first year after displacement and remain about 15% below pre-layoff levels even five years after the event. This pattern is consistent with the evidence documented for Spain, Italy, and Portugal (Bertheau et al., 2023), as well as for Brazil (Britto et al., 2022), where earnings decline by approximately 40%–50% in the first year and do not fully recover to pre-event levels even five years after the event. Panel (c) shows that the initial earnings loss is primarily driven by the extensive margin, reflecting sharp employment declines in the quarters immediately following displacement. At the same time, Panel (d) indicates that displacement is also associated with reductions along the intensive margin of labor supply. Five years after the shock, employment has largely recovered to its pre-displacement level, in line with prior evidence. In contrast, the intensive margin rebounds more quickly—within the first year—but remains approximately 15% below pre-shock levels in the medium term. This persistent reduction in earnings conditional on employment accounts for the medium-term gap in total earnings.

Figure 7: Effects of Mass Layoffs on Labor Market Outcomes: Intensive and Extensive Margins, Full Matched Sample



Notes: The figure presents event-study estimates of the effects of mass layoffs on workers' labor market outcomes using the full matched sample. The horizontal axis measures quarters relative to the layoff event ($t = 0$). Estimates are obtained from Equation 1, controlling for individual and calendar-year fixed effects. The omitted reference period is $k = -8$. Panel (a) reports effects on labor earnings estimated using a Poisson specification and expressed in percentage terms. Panel (b) shows earnings effects in levels, relative to average earnings in the omitted period. Panel (c) displays impacts on employment in the formal labor market (extensive margin), while Panel (d) reports effects on log earnings conditional on employment (intensive margin). The sample includes displaced workers and exactly matched non-displaced workers as controls. Dashed lines indicate 95% confidence intervals. Source: Authors' calculations using administrative records from BPS and MIDES.

6 The Buffering Effects of Public Policies

In this section, we examine the extent to which public policies mitigate the shock of job displacement. We assess whether existing policy instruments are able to partially offset its adverse effects and smooth post-displacement income trajectories. We structure the analysis around two key components of the social protection system. In Section 6.1, we analyze the mitigating role of AFAM-PE, focusing on its capacity to cushion income losses following job displacement. In Section 6.2, we turn to UI and evaluate its effectiveness in stabilizing earnings during the period of non-employment after a mass layoff. For each policy, we proceed in two steps. First, we estimate its causal effect using an RD design. Second, we focus on the joint effect of job displacement and policy exposure by implementing a combined RD-DiD framework.

6.1 The Effect of AFAM-PE

We begin by examining the effects of the CCT program within the sample of workers who experienced a mass layoff and applied for AFAM-PE. Figure 8 presents the effect of program eligibility on quarterly earnings measured one and three years after the displacement event. These estimates correspond to Equation 2 and report reduced-form RD effects, identifying the impact of AFAM-PE solely through discontinuities in program eligibility.

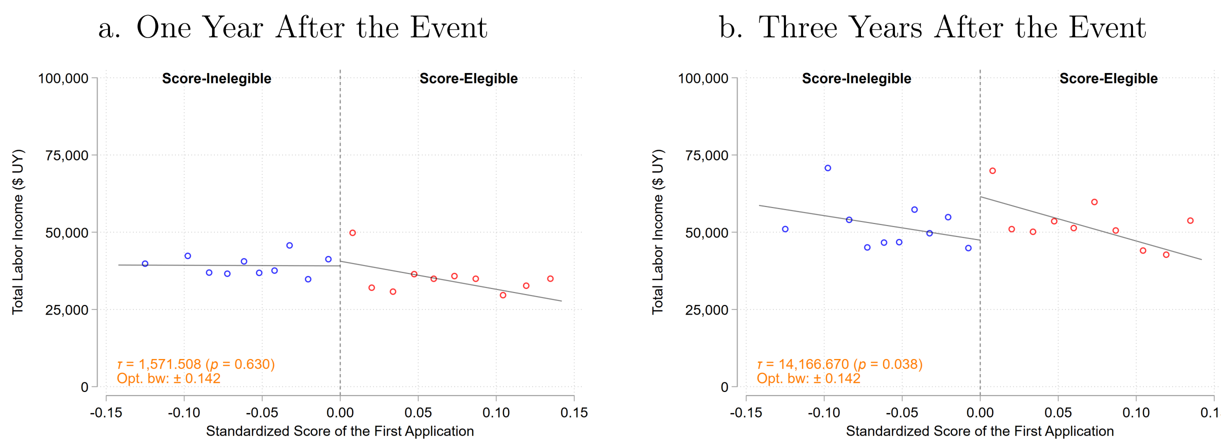
Three years after the displacement shock, we observe positive and statistically significant effects of AFAM-PE eligibility on earnings. The estimated effect amounts to approximately UYU 14,000 and is statistically significant at the 5% level. This finding suggests that the program plays a meaningful role in mitigating the medium-term income losses associated with mass layoffs.

Appendix Figure A5 extends the RD analysis by reporting the effects of AFAM-PE eligibility for each quarter from one year before the displacement event through five years afterward. In addition to presenting estimates for total earnings (Panel (a)), the figure also reports effects on the extensive margin of labor income (Panel (b)), capturing changes in employment participation following displacement.

Panel (a) shows positive and statistically significant effects of AFAM-PE eligibility on total labor earnings starting in the third year after displacement, which remain stable over the subsequent two years. This pattern is consistent with a setting in which the income support provided by the transfer relaxes short-term liquidity constraints after job loss, potentially affecting job search behavior and the timing of reemployment. These effects do not mechanically reflect the inclusion of transfer payments in our outcome measures. Labor earnings are constructed exclusively from social security records and therefore exclude AFAM-PE benefits and other non-labor income sources. The positive medium-term estimates therefore reflect stronger labor market recovery rather than a direct accounting effect of transfer receipt.

Panel (b) is consistent with this interpretation. In the quarters immediately following the shock, AFAM-PE eligibility is associated with lower employment rates, suggesting a temporary reduction along the extensive margin. Such patterns align with job search models in which income support increases reservation wages or allows workers to extend search duration. These negative employment effects are concentrated in the first year after displacement and dissipate thereafter.

Figure 8: Effects of CCT Program Eligibility Around Mass Layoffs



Notes: This figure shows regression discontinuity (RD) estimates of the effect of AFAM-PE eligibility on quarterly earnings among workers who experienced a mass layoff and applied for the program. The panels report estimated effects one year (Panel (a)) and three years after displacement (Panel (b)). Estimates correspond to Equation 2 and represent reduced-form effects identified through the program eligibility threshold. The optimal bandwidth used in each figure is computed in the fuzzy RD estimation. For the remaining elements in the figure, the default options of the `rdrobust` Stata command are selected: local linear regressions and triangular weights. The blue and red circles represent average outcomes computed in 10-quantile-spaced bins at each side of the threshold. Robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

We next explore the joint effect of mass layoffs and CCTs using an RD-DiD framework (see Section 4). Relative to the RD approach, this strategy offers two additional advantages. First, it allows us to account for potential heterogeneous responses to job displacement between eligible and non-eligible workers. Second, it provides a framework to assess the extent to which the transfer mitigates the negative effects of the displacement shock on post-layoff outcomes.

Figure 9 presents the RD-DiD estimates from Equation 5 without restricting the sample to the optimal RD bandwidth around the CCT eligibility threshold. The figure depicts the evolution of labor market outcomes around the displacement event for AFAM-PE-ineligible workers (control units) and the differential effect for AFAM-PE-eligible workers.

Panels (a) and (b) show, first, a sharp decline in labor earnings in the quarters immediately following the displacement event for ineligible workers, followed by a relatively rapid recovery that extends through the second year after the layoff. Eligible workers do not exhibit statistically significant differences relative to ineligible workers during this initial post-displacement period. In the medium term, however, the program implies positive and statistically significant effects, diverging from those of non-eligible workers. Panels (c) and (d) document the corresponding dynamics along the extensive (employment) and intensive (log of labor incomes) margins. The positive effects observed for total labor income for AFAM-PE-eligible workers appear to be driven by both margins, with higher employment levels and higher labor earnings emerging from the third year after the displacement shock onward.

Figure 9: RD-DiD: Dynamic Joint Effects of Mass Layoffs and CCT Eligibility on Labor Market Outcomes

Notes: This figure presents regression-discontinuity difference-in-differences (RD-DiD) estimates of the joint effects of mass layoffs and AFAM-PE-eligibility on labor market outcomes. Red triangles report estimated outcomes for AFAM-PE-ineligible workers (controls), while blue circles show the differential effect for eligible workers (effect of the program). Panel (a) displays effects on total labor income in real Uruguayan pesos, Panel (b) reports effects on labor income using a Poisson transformation, Panel (c) shows effects on months employed, and Panel (d) presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers who applied for AFAM-PE. Unlike the RD specifications, the sample is not restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

Appendix Figure A8 reports results from the same RD-DiD specification, incorporating the income and employment trajectories of CCT-eligible and -ineligible workers. The figure illustrates that the differential emerging from the third year after displacement translates into a gradual recovery for program beneficiaries. In particular, eligible workers recover pre-displacement levels of total income (Panel (b)) and regain pre-shock levels of employment and labor earnings by approximately the fifth year after the shock (Panels (c) and (d)). In contrast, ineligible workers do not exhibit a comparable recovery over the same horizon, especially with respect to labor earnings among those who remain employed (Panel (d)). Finally, Appendix Figure A9 replicates the specification shown in Figure 9, restricting the sample to an optimal bandwidth selected following Calonico et al. (2020). The results are consistent with those obtained using the unrestricted sample, showing positive and statistically significant effects of CCT eligibility emerging from the third year after the displacement event.

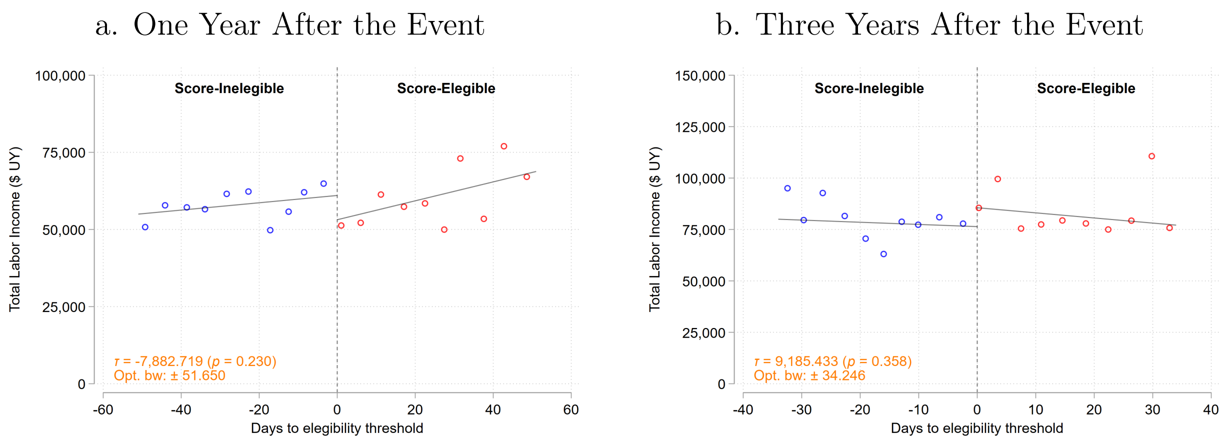
Overall, the results provide evidence that CCT eligibility is associated with improved labor market outcomes following mass layoffs. RD estimates indicate positive and statistically significant effects on labor earnings emerging in the medium term, while RD-DiD specifications show that eligible workers experience a faster recovery compared to non-eligible workers. Dynamic analyses suggest that these gains emerge from the third year after displacement onward and are driven by improvements along both the extensive and intensive margins.

6.2 The Effect of UI

In this section, we examine whether UI plays a role in mitigating the effects of mass layoffs. As in the CCT analysis, we proceed in two steps. First, we analyze reduced-form effects using an RD design based on tenure thresholds that determine eligibility for UI (see Section 4). We then assess the joint effect of job displacement and program eligibility by implementing an RD-DiD framework (Equation 5).

Figure 10 shows the effect of UI eligibility on quarterly total labor income one and three years after the displacement event. The figure reports reduced-form RD estimates. Unlike the case of AFAM-PE, we do not observe statistically significant effects at these horizons, although point estimates suggest slightly lower earnings one year after displacement. Appendix Figure A10 examines the probability of remaining out of the labor market three and six months after the shock. Consistent with standard job search models, UI coverage may reduce search intensity or delay job acceptance while benefits are received. In our setting, this pattern is reflected in a higher probability of remaining out of the labor market at three and six months after displacement, which coincide with the period of benefit coverage.

Figure 10: Effects of Unemployment Insurance Eligibility Around Mass Layoffs



Notes: This figure shows regression discontinuity (RD) estimates of the effect of UI eligibility on quarterly earnings among workers who experienced a mass layoff. The panels report estimated effects one year (Panel (a)) and three years after displacement (Panel (b)). Estimates correspond to Equation 2 and represent reduced-form effects identified through the program eligibility threshold. The optimal bandwidth used in each figure is computed in the fuzzy RD estimation. For the remaining elements in the figure, the default options of the `rdrobust` Stata command are selected: local linear regressions and triangular weights. The blue and red circles represent average outcomes computed in 10-quantile-spaced bins at each side of the threshold. Robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

Appendix Figure A11 reports reduced-form estimates of the effect of UI eligibility on earnings and employment around the displacement event, using quarterly outcomes (Panels (a) and (b)) and monthly outcomes (Panels (c) and (d)). As shown in the figure, there are no statistically significant differences between eligible and ineligible workers prior to the layoff, supporting the validity of the RD design. In the post-displacement period, we observe statistically significant negative effects on both labor earnings and the extensive margin in the months immediately following the shock. These patterns are consistent with standard predictions from job search models in which UI temporarily reduces search intensity. This mechanism is most clearly reflected in Panel (d), which shows negative effects on employment rates persisting until the exhaustion of benefit coverage, six months after displacement.

We next implement the RD-DiD model to jointly capture the effects of mass layoffs and UI, allowing us to assess the potential mitigating role of UI in the post-displacement period. Figure 11 presents the results of this exercise for our main outcomes: total income (Panels (a) and (b)), employment (Panel (c)), and labor earnings among employed workers (Panel (d)). The figure illustrates the negative impact generated by the displacement shock among control workers who are ineligible for UI, as well as a negative and statistically significant differential effect for UI-eligible workers in the months immediately following the layoff. Appendix Figures A8 and A13 replicate Figure 11, showing outcome trajectories for UI-eligible and -ineligible workers and the differential effect for eligible workers, split by AFAM eligibility and when restricting the sample to an optimal bandwidth around the UI eligibility threshold, respectively. The resulting patterns are qualitatively similar to those observed in the unrestricted sample, supporting the robustness of the RD-DiD estimates.

In contrast to the CCT program, the lower search intensity and reemployment rates observed among UI recipients in the months following displacement do not appear to translate into improved employment trajectories in subsequent years. Beyond the first year after the layoff, eligible and ineligible workers exhibit similar patterns in both employment and earnings. Accordingly, UI does not appear to generate positive matching effects between firms and workers that ultimately result in higher labor earnings for eligible workers.

Figure 11: RD-DiD: Dynamic Joint Effects of Mass Layoffs and AFAM-PE Eligibility on Labor Market Outcomes

Notes: This figure presents regression-discontinuity difference-in-differences (RD-DiD) estimates of the joint effects of mass layoffs and AFAM-PE eligibility on labor market outcomes. Red triangles report estimated outcomes for AFAM-PE-ineligible workers (controls), while blue circles show the differential effect for eligible workers (effect of the program). Panel (a) displays effects on total labor income in real Uruguayan pesos, Panel (b) reports effects on labor income using a Poisson transformation, Panel (c) shows effects on months employed, and Panel (d) presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers who applied for AFAM-PE. Unlike the RD specifications, the sample is not restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

7 Program Overlap and Population Composition

We next examine whether the differences between UI and CCTs reflect differences in the policies themselves or differences in the populations targeted by each program. Since the two programs overlap substantially among vulnerable workers, comparisons based on the full sample may combine policy effects with composition effects. To address this issue, we conduct two complementary exercises. First, we reestimate the effect of UI within a comparable subsample of workers eligible for CCTs. This allows us to assess whether the baseline differences between the two programs are driven by differences in beneficiary characteristics. Second, we estimate the effects of CCTs separately according to workers' exposure to UI, distinguishing between individuals receiving only CCT and those simultaneously covered by both programs.

7.1 UI Effects Within the CCT Program Population

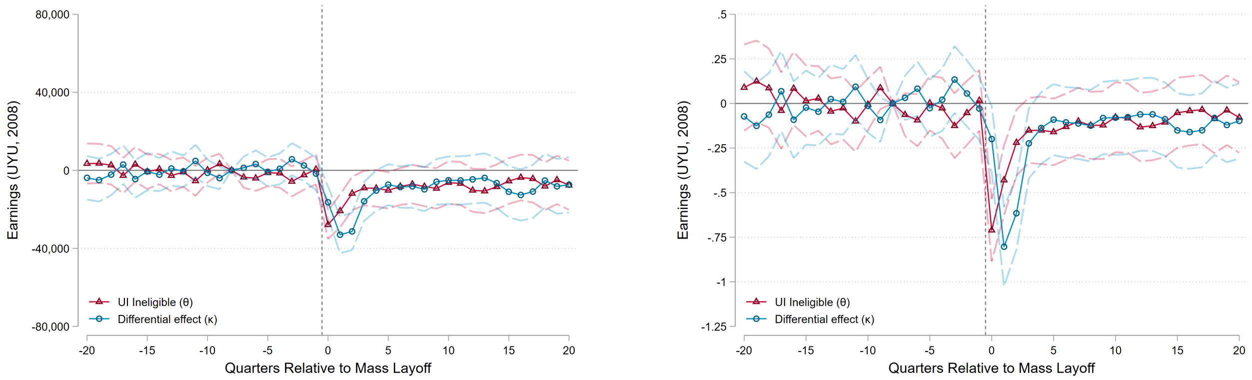
Our first exercise restricts the analysis to workers who are eligible for CCT and estimates the effects of UI within this more homogeneous population. UI and CCT target populations with different labor market attachment and socioeconomic characteristics. Therefore, differences in estimated policy effects could arise because UI beneficiaries are systematically different from CCT beneficiaries. Restricting the analysis to the CCT population allows us to compare the effects of UI among individuals with more similar socioeconomic conditions.

Figure 12 reports dynamic RD-DiD estimates of UI's effects when restricting the analysis to workers within the AFAM-PE eligibility window. The results remain broadly consistent with our baseline findings. The red series reports the effects of displacement among UI non-eligible workers, while the blue series captures the differential effect associated with UI eligibility. Across both panels, we find limited evidence that UI generates persistent improvements in post-displacement labor outcomes. Although point estimates fluctuate over time, the differential effects remain relatively close to zero and are generally imprecisely estimated. Overall, these findings suggest that the differences observed between AFAM-PE and UI are unlikely to be driven solely by differences in the populations targeted by each program. Even within a more comparable population of vulnerable workers, we continue to find limited evidence that UI improves medium-term labor market outcomes.

Figure 12 reports the dynamic effects of UI when restricting the analysis to workers within the AFAM-PE eligibility window. The results remain broadly consistent with our baseline findings. Panel (a) shows no evidence of persistent positive effects of UI eligibility on total labor income following displacement. Similarly, Panel (b) shows limited evidence of sustained employment gains. If anything, employment appears temporarily lower during the months immediately following the shock, which is consistent with the idea that UI may increase time spent outside formal employment while benefits are available.

Figure 12: Dynamic Effects of Unemployment Insurance Eligibility on Formal Labor Income Around Mass Layoffs, CCT Score Window

a. Total Labor Income b. Total Labor Income (Poisson Transformation)



Notes: This figure reports dynamic RD-DiD estimates of unemployment insurance's effects when restricting the sample to workers within the AFAM-PE eligibility window. The sample includes workers experiencing mass layoffs and follows them from one year before to five years after displacement. The red series reports the estimated effects of displacement among UI non-eligible workers, while the blue series reports the differential effects associated with UI eligibility. Panel (a) presents estimates for total labor income in levels, and Panel (b) presents estimates using the Poisson transformation. Local linear regressions with triangular weights are used throughout, and robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

7.2 Heterogeneity by Joint Program Participation

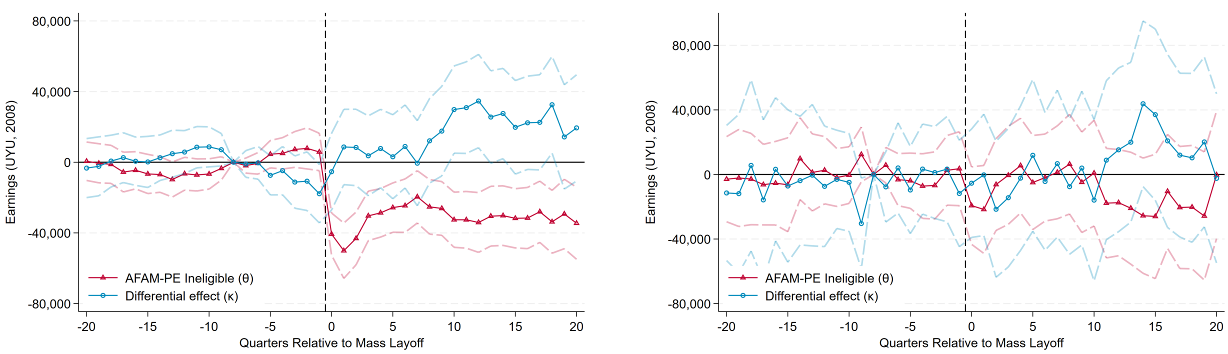
Our second exercise examines whether CCTs' effects differ according to exposure to UI. Specifically, we estimate CCT program effects separately for workers in two groups: (i) workers receiving CCTs only and (ii) workers receiving both CCTs and UI simultaneously. This exercise allows us to assess whether the insurance value of AFAM-PE depends on access to contributory protection mechanisms.

Figure 13 reports CCTs' effects separately for workers receiving only CCTs and workers simultaneously covered by both CCTs and UI. The estimates suggest some heterogeneity across groups, although the results should be interpreted cautiously given the smaller sample sizes resulting from splitting the RD sample according to joint program participation. Panel (a) shows that among workers covered by both programs, estimated effects become gradually more positive in the medium term, suggesting stronger earnings recovery after displacement. In contrast, Panel (b) presents more volatile estimates among workers receiving only CCTs, with wider confidence intervals and less stable dynamics over time.

Taken together, these results provide preliminary evidence that the baseline CCT effects are not driven exclusively by workers lacking access to UI. However, given the reduced statistical power in these subsamples, we view these estimates primarily as suggestive evidence regarding the interaction between contributory and noncontributory components of the social protection system.

Figure 13: Dynamic Effects of CCT Eligibility Around Mass Layoffs: Heterogeneity by Joint Program Participation

a. Both Programs   b. Only CCT



Notes: This figure reports dynamic regression-discontinuity estimates of AFAM-PE eligibility's effects for workers experiencing mass layoffs. The sample is split according to exposure to unemployment insurance: Panel (a) restricts the sample to workers simultaneously receiving AFAM-PE and unemployment insurance, while Panel (b) restricts the sample to workers receiving AFAM-PE only. The red series reports effects of mass layoffs for non-eligible workers, while the blue series reports differential effects associated with AFAM-PE eligibility. Local linear regressions with triangular weights are used throughout, and robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

8 Conclusion

Social safety nets typically include a UI component to mitigate the adverse effects of job loss for workers with relatively strong attachment to the formal labor market and sufficient contribution histories. In low- and middle-income countries, informality limits the effectiveness of this type of policy, as low-income families that are more exposed to adverse macroeconomic conditions often lack access to contributory benefits because their formal labor market attachment is more unstable. We showed that in such contexts, noncontributory policies, such as CCTs, can play an important buffering role.

We documented that mass layoff events are associated with large and persistent negative effects on labor market outcomes. However, individuals who experience additional income support from the cash transfer prior to displacement exhibit faster recovery than those who do not. By contrast, we find no clear evidence of similar effects for the traditional insurance policy, UI, at least in our preliminary results.

One possible interpretation is that, rather than insurance that activates only at the time of the shock, persistent income support in the form of large and predictable transfers to low-income families helps shape the conditions under which households navigate negative income shocks. These results are preliminary, however, and further work is needed to better understand the dynamic responses, how they evolve over time, and the mechanisms underlying the differences across policy instruments.

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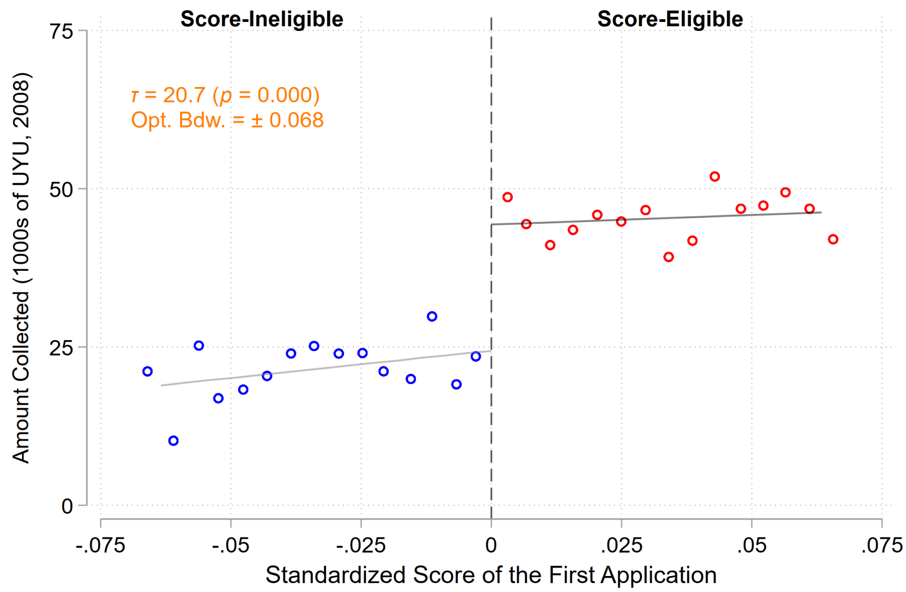
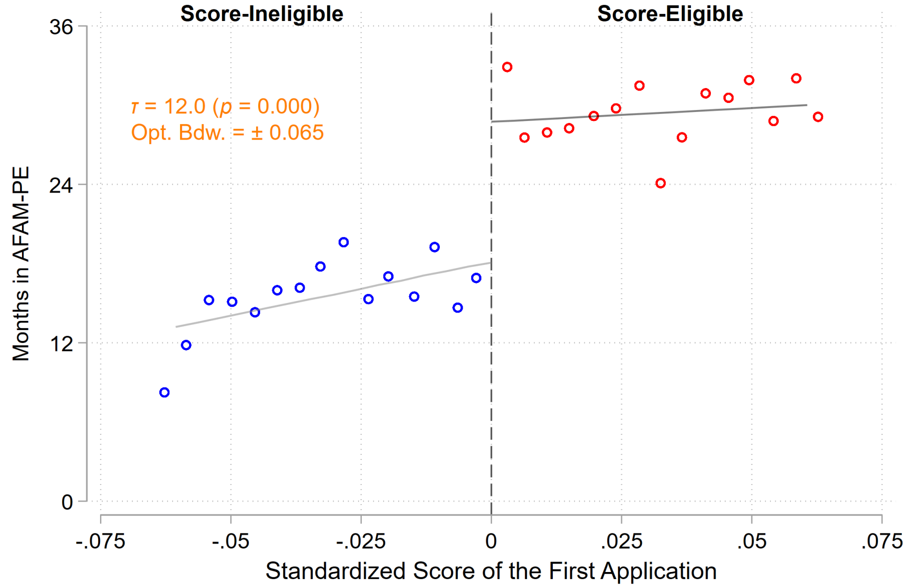
Figure A1: Effect of Eligibility Rule on Amount of AFAM-PE Benefits



Notes: The figure presents the identification strategy and the empirical first stage of the RD design of AFAM-PE program. It plots the amount collected by AFAM-PE benefits as a function of the poverty score. The bandwidth is chosen by minimizing the mean squared error (MSERD) using the default settings of the *rdrobust* Stata command, which implements local linear regressions with triangular kernel weights Calonico et al. (2019b). Robust standard errors are clustered at the worker level. The sample used corresponds to the baseline sample. Source: Authors' calculations using administrative records from BPS and MIDES.

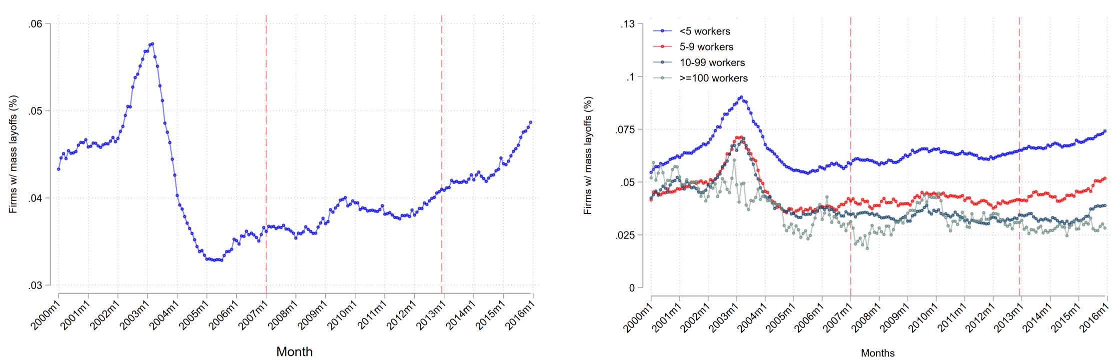
Figure A2: Effect of Eligibility Rule on Months of AFAM-PE Benefits



Notes: The figure presents the identification strategy and the empirical first stage of the RD design of AFAM-PE program. It plots the months receiving AFAM-PE benefits as a function of the poverty score. The bandwidth is chosen by minimizing the mean squared error (MSERD) using the default settings of the *rdrobust* Stata command, which implements local linear regressions with triangular kernel weights Calonico et al. (2019b). Robust standard errors are clustered at the worker level. The sample used corresponds to the baseline sample. Source: Authors' calculations using administrative records from BPS and MIDES.

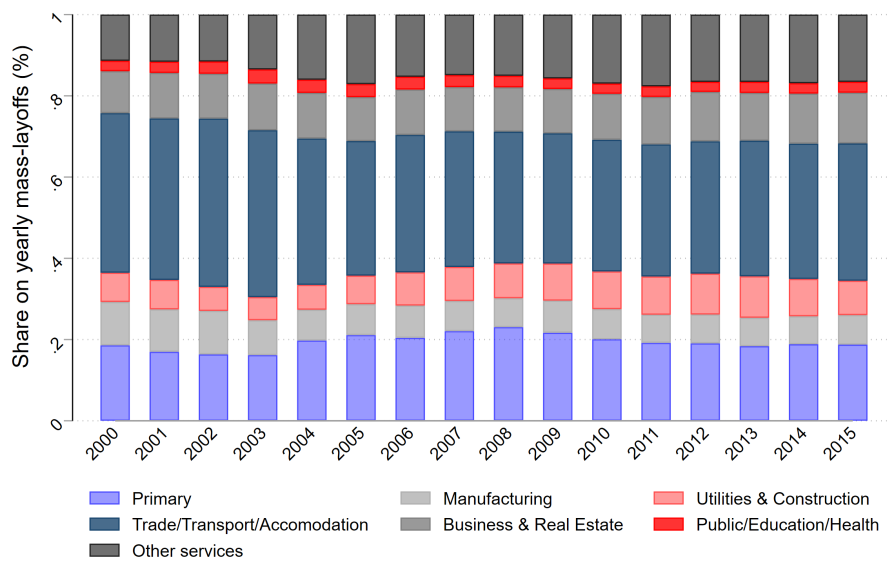
Figure A3: Monthly Share of Firm's Mass Layoff.

(a) *Total* (b) *By Firm Size*



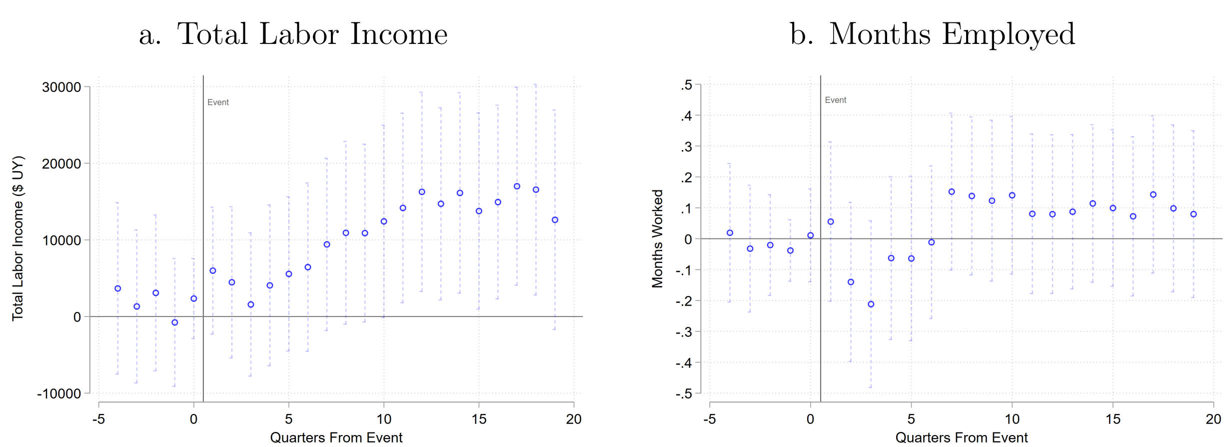
Notes: Panel (a) plots the monthly share of firms experiencing a mass layoff in the universe of registered firms. Panel (b) shows the same series by firm size (<5, 5-9, 10-99, ≥100 workers). Vertical dashed lines mark our select period of analysis. The incidence spikes during the 2001-02 crisis and declines thereafter, with a 4% of firms systematically exhibiting higher mass-layoff rates. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A4: Sectoral Distribution of Displaced Workers (Treated), by Year



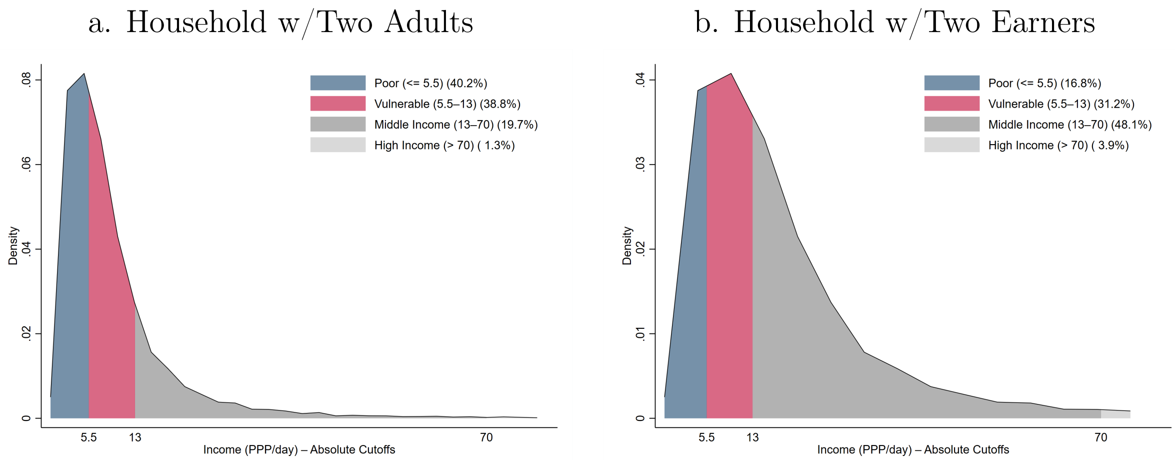
Notes: The figure shows the distribution of yearly mass-layoff firms by sector. Sectors follow administrative classifications: Primary, Manufacturing, Utilities & Construction, Trade/Transport/Accommodation, Business & Real Estate, Public/Education/Health, and Other services. The figure shows a persistent concentration of mass layoffs in Trade/Transport/Accommodation and Other services. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A5: Dynamic Effects of AFAM-PE Eligibility Around Mass Layoffs



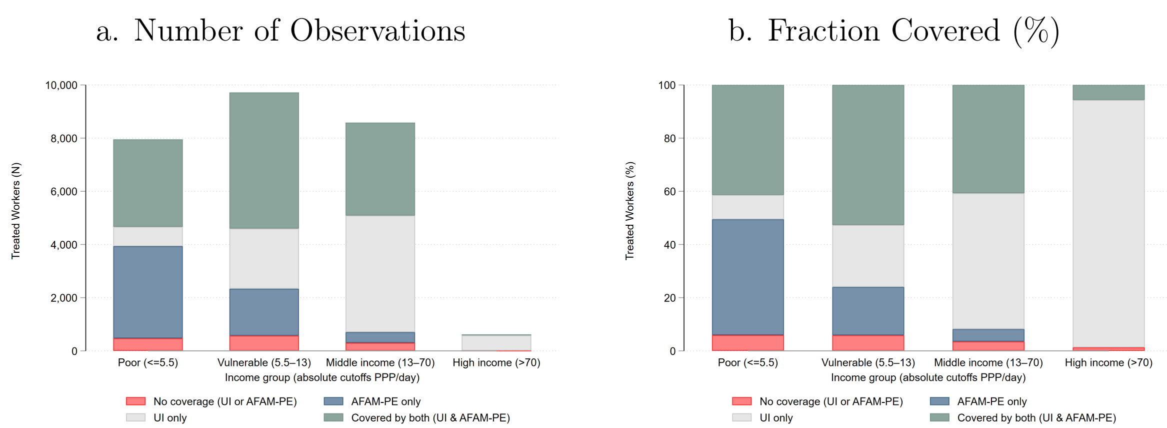
Notes: This figure reports regression discontinuity (RD) estimates of the effect of AFAM-PE eligibility for each quarter from one year prior to the mass layoff through five years after displacement. Panel a presents effects on total quarterly earnings, while Panel b reports months employed by quarter. Estimates correspond to Equation 2 and represent reduced-form effects identified through the program eligibility threshold. The optimal bandwidth used in each figure is computed in the Fuzzy RD estimation for each quarter. For the remaining elements in the Figure, the default options of the *rdrobust* Stata command are selected: local linear regressions and triangular weights. Robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A6: Income Distribution of Treated Workers under Absolute PPP-Based Classification and Alternative Household Income Definitions



Notes: The figure shows, for displaced (treated) workers, the income distribution under an absolute income classification based on daily income measured in purchasing power parity (PPP) dollars. Income groups follow standard international thresholds: poor (USD 0–5.5), vulnerable (USD 5.5–13), middle-income (USD 13–70), and high-income (above USD 70 per day). Both panels use the same absolute PPP-based classification but differ in the construction of household income. Panel a assumes that the household consists of two adults and the number of dependent children observed in the data, with labor income equal to the observed earnings of the sampled worker. Panel b maintains the same household size assumption but constructs household income by doubling observed labor income, effectively assuming two earners. Income for treated workers are computed from pre-layoff formal labor income ($t = -1$). Source: Authors' calculations using administrative records from BPS and MIDES.

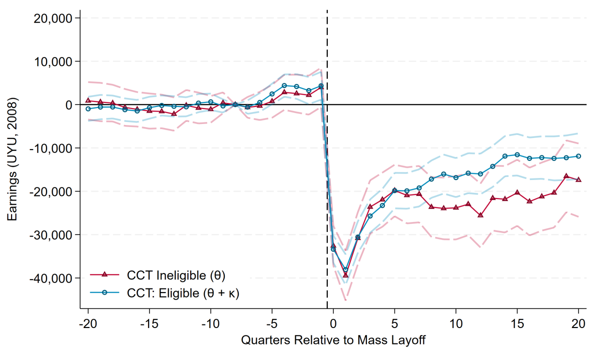
Figure A7: Program Coverage across Income Groups Defined by PPP-Based Thresholds



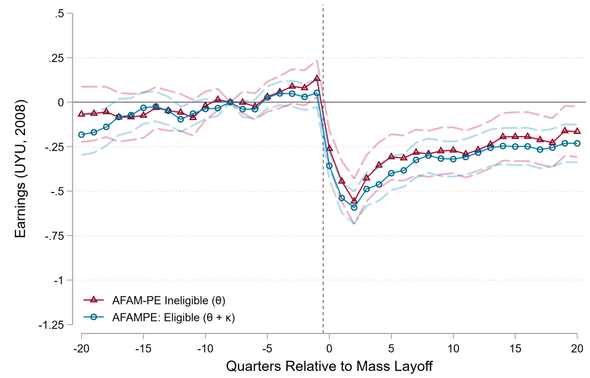
Notes: The figure shows, for displaced (treated) workers, program coverage across income groups defined using absolute income thresholds based on daily income measured in purchasing power parity (PPP) dollars. Income groups are classified as poor (USD 0–5.5), vulnerable (USD 5.5–13), middle-income (USD 13–70), and high-income (above USD 70 per day), computed using pre-layoff labor income ($t = -1$). Panel a reports the number of treated workers in each income group by program coverage status (no coverage, unemployment insurance only, AFAM-PE only, and coverage by both programs). Panel b reports the corresponding shares within each income group. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A8: RD-DiD: Dynamic Joint Effects of Mass Layoffs and AFAM-PE Eligibility on Labor Market Outcomes (Elegible vs Inelegible).

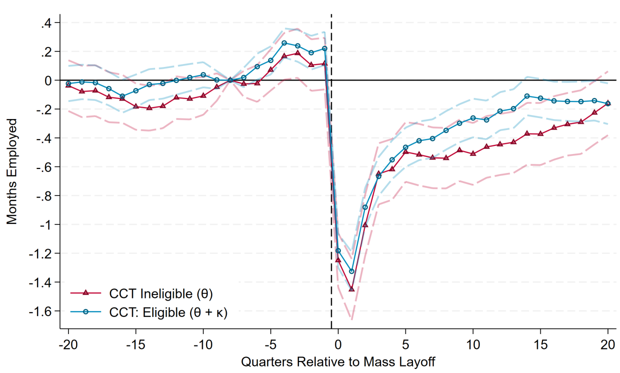
a. Total Labor Income



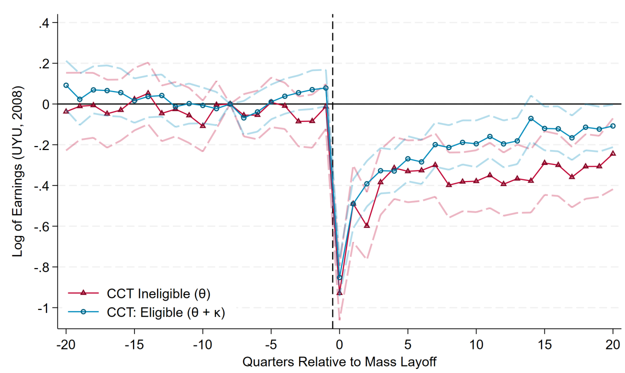
b. Labor Income (Poisson Transformation)



c. Months Employed



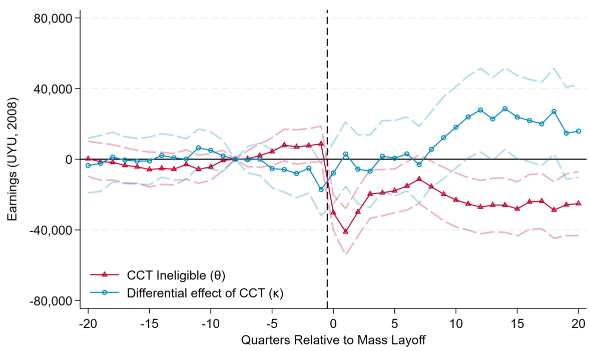
d. Log of Labor Income



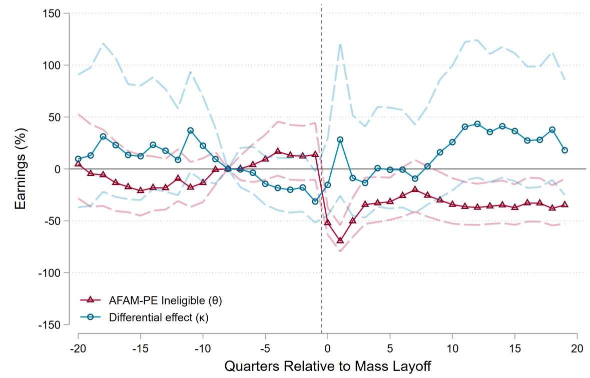
Notes: This figure presents regression discontinuity difference-in-differences (RD–DiD) estimates of the joint effects of mass layoffs and AFAM-PE eligibility on labor market outcomes. Red triangles report estimated outcomes for AFAM-PE ineligible workers (controls), while blue circles show the effect for eligible workers. Panel a displays effects on total labor income in real Uruguayan pesos, Panel b reports effects on labor income using a Poisson transformation, Panel c shows effects on months employed, and Panel d presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers who applied for AFAM-PE. Unlike the RD specifications, the sample is not restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A9: RD-DiD: Dynamic Joint Effects of Mass Layoffs and AFAM-PE Eligibility on Labor Market Outcomes (Optimal Bandwith).

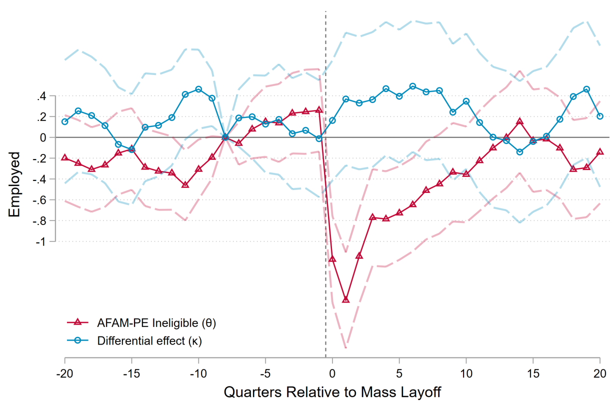
a. Total Labor Income



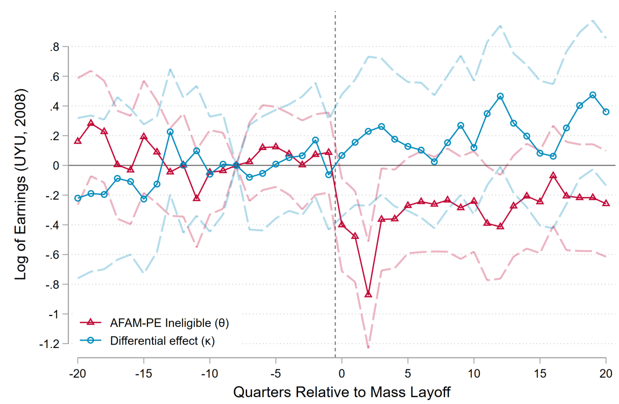
b. Labor Income (Poisson Transformation)



c. Months Employed



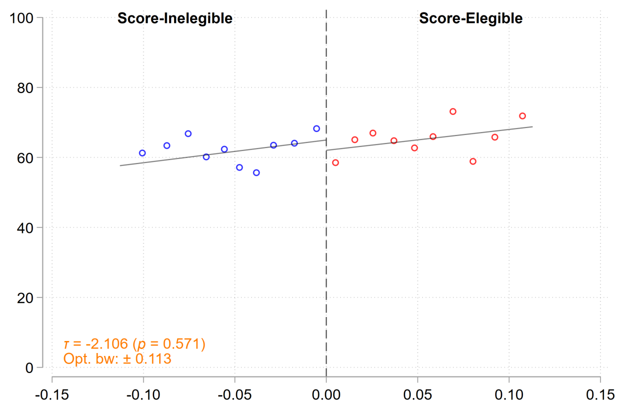
d. Log of Labor Income



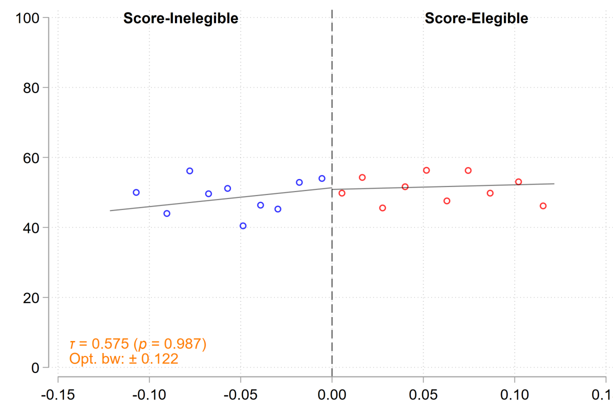
Notes: This figure presents regression discontinuity difference-in-differences (RD–DiD) estimates of the joint effects of mass layoffs and AFAM-PE eligibility on labor market outcomes. Red triangles report estimated outcomes for AFAM-PE ineligible workers (controls), while blue circles show the differential effect for eligible workers (effect of the program). Panel a displays effects on total labor income in real Uruguayan pesos, Panel b reports effects on labor income using a Poisson transformation, Panel c shows effects on months employed, and Panel d presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers who applied for AFAM-PE. The sample is restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A10: Reduced-Form RD Effects of Unemployment Insurance Eligibility on Non-Employment After a Mass Layoff

a. 3 months



b. 6 months



Notes: Figure presents reduced-form regression discontinuity (RD) estimates of the effect of unemployment insurance (UI) eligibility on the probability of remaining out of the formal labor market after a mass layoff. Panel a reports effects three months after displacement, while Panel b reports effects six months after displacement. The running variable is job tenure. Dots represent mean outcomes within tenure bins, and solid lines correspond to local linear fits estimated separately on each side of the threshold. Estimates correspond to Equation 2 and represent reduced-form effects identified through the program eligibility threshold. The optimal bandwidth used in each figure is computed in the Fuzzy RD estimation for each quarter. For the remaining elements in the Figure, the default options of the *rdrobust* Stata command are selected: local linear regressions and triangular weights. Robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

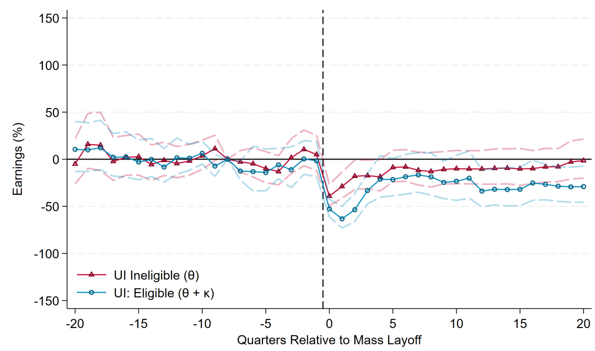
Figure A11: Dynamic Effects of Unemployment Insurance Eligibility Around Mass Layoffs

Notes: This figure reports regression discontinuity (RD) estimates of the effect of UI eligibility for each quarter (panel a and b) and months (panel c and d) from one year prior to the mass layoff through five years after displacement. Panel a presents effects on total quarterly earnings, while Panel b reports months employed by quarter. Panel c and report shows the equivalent results in monthly basis. Estimates correspond to Equation 2 and represent reduced-form effects identified through the program eligibility threshold. The optimal bandwidth used in each figure is computed in the Fuzzy RD estimation for each quarter. For the remaining elements in the Figure, the default options of the *rdrobust* Stata command are selected: local linear regressions and triangular weights. Robust standard errors are clustered at the worker level. Source: Authors' calculations using administrative records from BPS and MIDES.

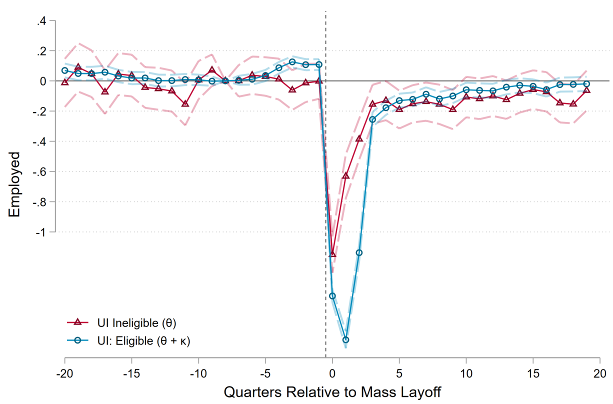
Figure A12: RD-DiD: Dynamic Joint Effects of Mass Layoffs and UI Eligibility on Labor Market Outcomes (Elegible vs Inelegible).

a. Total Labor Income

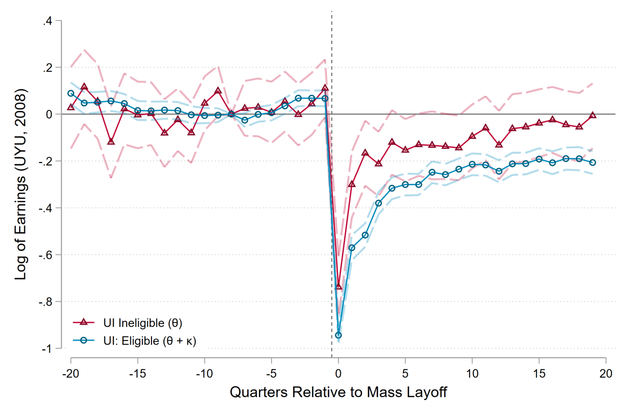
b. Labor Income (Poisson Transformation)



c. Months Employed



d. Log of Labor Income



Notes: This figure presents regression discontinuity difference-in-differences (RD–DiD) estimates of the joint effects of mass layoffs and UI eligibility on labor market outcomes. Red triangles report estimated outcomes for UI ineligible workers (controls), while blue circles show the effect for eligible workers. Panel a displays effects on total labor income in real Uruguayan pesos, Panel b reports effects on labor income using a Poisson transformation, Panel c shows effects on months employed, and Panel d presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers. Unlike the RD specifications, the sample is not restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

Figure A13: RD-DiD: Dynamic Joint Effects of Mass Layoffs and UI Eligibility on Labor Market Outcomes (Optimal Bandwith).

Notes: This figure presents regression discontinuity difference-in-differences (RD–DiD) estimates of the joint effects of mass layoffs and UI eligibility on labor market outcomes. Red triangles report estimated outcomes for UI ineligible workers (controls), while blue circles show the differential effect for eligible workers (effect of the program). Panel a displays effects on total labor income in real Uruguayan pesos, Panel b reports effects on labor income using a Poisson transformation, Panel c shows effects on months employed, and Panel d presents effects on the log of labor income. Dashed lines indicate 95% confidence intervals. Estimates are based on Equation 5. The sample consists of workers who experienced a mass layoff and matched control workers. The sample is restricted to an optimal bandwidth around the eligibility threshold. Source: Authors' calculations using administrative records from BPS and MIDES.

Table A.1: Sample Definition and Descriptive Statistics of Workers (2007-2012).

	Universe of workers		With Children		Firm > 5 wrokers		Sample	
	Mean (1)	SD (2)	Mean (1)	SD (2)	Mean (1)	SD (2)	Mean (1)	SD (2)
Panel A. Individual characteristics								
Female (%)	46.03	(49.84)	50.00	(50.00)	49.05	(49.99)	42.71	(49.47)
Age	36.99	(14.59)	34.32	(9.70)	33.62	(9.70)	32.79	(8.09)
Montevideo (capital city) (%)	54.48	(49.80)	53.96	(49.84)	59.15	(49.16)	68.36	(46.51)
Panel B. Employment characteristics								
Total HH Income (U)	265540.72	(518951.86)	271991.68	(516953.01)	332852.23	(579314.42)	255498.45	(570094.75)
Months Employed	7.55	(4.43)	7.88	(4.26)	8.21	(4.12)	8.97	(3.65)
Hours Employed	1173.41	(798.99)	1181.17	(790.37)	1222.69	(782.58)	1289.88	(723.26)
Days Employed	196.90	(135.03)	197.14	(132.77)	202.50	(131.92)	208.72	(116.36)
Monthly workers (%)	61.81	(48.59)	58.25	(49.32)	56.17	(49.62)	51.80	(49.97)
Per-day workers (%)	34.28	(47.46)	38.70	(48.71)	40.50	(49.09)	45.86	(49.83)
Others (%)	3.92	(19.41)	3.06	(17.21)	3.33	(17.95)	2.33	(15.10)
Panel C. Firm Size								
1 worker (%)	15.60	(36.29)	13.93	(34.62)	0.00	(0.00)	0.00	(0.00)
1-5 workers (%)	16.35	(36.99)	15.24	(35.94)	0.00	(0.00)	0.00	(0.00)
6-10 workers (%)	11.01	(31.30)	10.88	(31.14)	15.36	(36.06)	30.20	(45.91)
11-100 workers (%)	29.57	(45.64)	30.47	(46.03)	43.02	(49.51)	45.16	(49.77)
> 100 workers (%)	27.47	(44.64)	29.48	(45.60)	41.62	(49.29)	24.64	(43.09)
Observations	1893318		853410		627393		53750	

Notes: The table reports descriptive statistics for workers observed in the administrative records between 2007 and 2012. Columns progressively apply sample restrictions, starting from the universe of workers in the records (columns 1-2) and sequentially restricting the sample to workers with children (columns 3-4), workers employed in firms with more than five employees (columns 5-6), and the final analysis sample (columns 7-8). Panel A reports individual characteristics, Panel B employment characteristics, and Panel C firm size composition. For each variable, the table reports the mean and standard deviation (in parentheses). Monetary values are expressed in real UYU. Source: Authors' calculations using administrative records from BPS and MIDES.