The Labor Market Outcomes of (Quasi) Unemployment Insurance[1]

Evidence from the *Retiro Parcial por Desempleo* in Mexico

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July 18, 2025

[1]I am deeply grateful to Adrian Rubli for his guidance throughout the research process that culminated in this work. His generous time and thoughtful discussions undoubtedly improved the quality of this project. I also thank Horacio Larreguy, whose characteristic rigor and persistence were instrumental in strengthening the research. I further benefited from the insightful comments and support of peers in Horacio's research seminar, whose feedback shaped this project at every stage.

Abstract

Unemployment insurance (UI) programs aim to protect workers from income loss while they search for new jobs, but they often involve trade-offs: benefits can help workers hold out for better opportunities, yet may also discourage job search and delay re-employment. These dynamics are especially complex in developing countries, where formal UI is rare and informality is widespread. This paper studies Mexico's *Retiro Parcial por Desempleo* (RPD), which allows unemployed workers to withdraw savings from their individual pension accounts. Using rich administrative data and a fuzzy regression discontinuity design exploiting a sharp eligibility threshold, I find that eligibility for RPD increases program take-up by approximately four percentage points. RPD take-up significantly prolongs unemployment duration—by up to 36 weeks over a three-year horizon—but does not lead to improvements in reemployment wages, job stability, or cumulative formal earnings. Heterogeneity analyses show that these adverse effects are concentrated among younger, lower-income, and female workers, while some better-off groups experience modest earnings gains. These findings suggest that while the RPD provides short-term liquidity, it does not enhance long-term labor market outcomes and may reinforce existing inequalities. Nonetheless, the program's self-financed structure and minimal take-up may help limit moral hazard, offering a pragmatic—but limited—approach to unemployment protection in settings with high informality and constrained state capacity.

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Introduction

Job loss can cause severe income disruptions, yet in the absence of public intervention, workers must self-insure by borrowing, drawing down savings, or relying on informal support networks. Economists have long argued that such self-insurance is inefficient: when credit markets are incomplete and workers are risk-averse, precautionary strategies often fall short, and the welfare losses from income volatility can be large. Unemployment insurance (UI) programs address this market failure by pooling risk across individuals and time, thereby enabling workers to smooth consumption during periods of joblessness (Baily 1978; Gruber 1997). Beyond their insurance function, UI programs may also improve job match quality by reducing the urgency to accept the first available job, leading to higher earnings and long-run productivity (Acemoglu and Shimer 1999, 2000).

These benefits, however, must be balanced against potential moral hazard; generous benefits may reduce the urgency to search for work, prolonging unemployment. In developing countries, where fiscal capacity is limited and informality widespread, traditional UI systems are often unviable. As a result, many governments rely on self-financed alternatives designed to provide limited liquidity while minimizing work disincentives. One such program is Mexico's *Retiro Parcial por Desempleo* (RPD), which allows unemployed formal-sector workers to withdraw a portion of their pension savings upon job loss.

This paper addresses the question: What are the causal labor market effects of access to the RPD program in Mexico? I study the impact of RPD eligibility and take-up on three key outcomes: (i) job search behavior, proxied by unemployment duration and employment survival; (ii) job quality, measured by reemployment wages and job duration; and (iii) medium-term labor market performance, defined as formal earnings and months worked in the three years following displacement. Understanding these effects is critical for assessing whether quasi-UI schemes like RPD enable better job matches or merely delay reemployment, and whether they reinforce or mitigate longer-term income risks.

A growing body of research has examined the effects of UI on labor market outcomes in both high- and low-income contexts. In advanced economies, natural experiments and regression discontinuity designs show that more generous UI consistently prolongs unemployment spells (e.g., Katz and Meyer 1990; Schmieder, Wachter, and Bender 2012). Evidence on job quality is more mixed: while some studies report gains in reemployment wages and job stability (e.g., Nekoei and Weber 2017; Centeno and Novo 2006), others find negligible or even adverse effects on earnings (e.g., Schmieder and Wachter 2016; Le Barbanchon 2016). In middle-income countries, UI interacts with informality in complex ways. For instance, Britto (2022) finds that in Brazil, over half of the additional unemployment induced by extended UI is offset by increased informal employment, raising concerns about behavioral leakage and program effectiveness.

This study contributes to the literature by being the first to estimate the causal impact of Mexico's RPD program using administrative data and a fuzzy regression discontinuity design. While earlier causal studies (e.g., Velázquez Guadarrama 2023; Carreño Godínez 2025) relied on survey data, I leverage high-frequency longitudinal records from the Mexican social security system to construct a clean analytic sample and directly observe eligibility, take-up, and formal labor market trajectories. The RPD's eligibility threshold—requiring at least two years of social security contributions—offers a compelling quasi-experimental setting. By exploiting this

discontinuity and tracking administrative take-up, I estimate local average treatment effects (LATEs) for compliers across a rich set of labor market outcomes. While internal validity is strong, generalizability is limited to younger formal-sector workers near the eligibility threshold.

The results show that RPD take-up has large and significant effects on unemployment duration: users remain unemployed for approximately 36 additional weeks over a three-year period, from a baseline of 51 weeks for the ineligible, with effects evident within the first three months after displacement. However, these extended unemployment spells do not translate into higher job quality. On average, RPD users do not experience gains in wages, job duration, or cumulative earnings. In fact, most subgroups—particularly lower-income, younger, and female workers—experience substantial earnings losses over the three-year horizon. Although some subgroups, such as higher-income or male workers, see small gains in reemployment earnings, these are not sufficient to offset longer unemployment spells. These findings challenge optimistic models of UI that predict gains in productivity and matching quality (Acemoglu and Shimer 2000), and instead suggest that self-financed programs like RPD may distort job search without delivering meaningful returns in terms of labor market outcomes.

Importantly, heterogeneity analyses reveal that the effects of RPD vary markedly across demographic and macroeconomic conditions. Younger and lower-wage workers face much larger increases in unemployment and sharper earnings losses—suggesting that RPD may exacerbate, rather than alleviate, pre-existing vulnerabilities, and thus renforce inequality. In contrast, workers displaced during the COVID-19 crisis exhibit smaller increases in unemployment and even experience modest gains in cumulative earnings. This points to a potentially countercyclical role for liquidity provision, where temporary support during downturns may cushion income losses and facilitate labor market reintegration.

These findings offer several policy-relevant insights. First, the structure of the RPD program; being self-financed and actuarially neutral—appears to reduce concerns

about fraud and fiscal burden but also limits its redistributive potential. Second, the regressive nature of its benefits—favoring those with more savings and stronger labor market histories—raises concerns about equity and adequacy. Lastly, while the program may be appropriate as a minimal safety net, it does not substitute for the functions of traditional unemployment insurance. The results contribute empirical grounding to policy debates in Mexico and other developing economies over whether to expand self-financed schemes, replace them with publicly funded UI, or design hybrid systems tailored to institutional capacity.

Nonetheless, the study has limitations. The fuzzy RD design identifies local treatment effects for a narrow group of workers—those displaced in their third year of formal labor market participation—limiting external validity. Moreover, the administrative data do not capture transitions into informal employment or broader welfare outcomes, such as psychological stress or household income dynamics. Finally, the modest first stage—only a 4 percentage point increase in take-up—constrains precision in second-stage estimates and necessitates cautious interpretation.

The remainder of the paper proceeds as follows. Section 1 reviews the literature on UI and quasi-UI programs in both developed and developing countries. Section 2 provides institutional background on the RPD program and Mexico's broader unemployment protection framework. Section 3 describes the data and sample construction. Section 4 outlines the empirical strategy. Section 5 presents the main results, including heterogeneity analysis. I finish with a discussion of the findings and their implications for policy.

Chapter 1

Literature Review

This section reviews key strands of the unemployment protection literature relevant to the Retiro Parcial por Desempleo (RPD). I begin by outlining the evolution of economic thought on unemployment insurance (UI), followed by empirical evidence on its labor market effects. I then examine how UI functions in developing economies, where informality and weak enforcement complicate its impact. Finally, I turn to pension-linked and quasi-UI schemes, focusing on RPD as a self-financed alternative in a context with limited access to formal unemployment support.

1.1 History of Economic Thought on Unemployment Insurance

Unemployment insurance (UI) has its roots in early 20th-century social insurance schemes, introduced to buffer workers against income loss during joblessness. Economists and policymakers initially debated its merits: while UI promised to stabilize incomes and aggregate demand (especially during downturns like the

Great Depression), critics warned even early on that providing benefits to the unemployed could weaken work incentives (Price 1985).

Over time, UI became a standard component of the welfare state – by 1935 the U.S. had a federal-state UI system, following earlier European models – and was seen as both a social safety net and an automatic stabilizer for the economy. Classic economic thought began formalizing this trade-off in the 1960s and 1970s. For example, job search theory highlighted that more generous UI would raise workers' reservation wages and reduce search intensity, potentially lengthening unemployment spells (Stigler 1961, 1962).

By the late 1970s, seminal theoretical contributions articulated the balance between consumption smoothing benefits and moral hazard costs of UI. Baily (1978) derived the optimal benefit replacement rate under this trade-off, showing that while risk-averse workers gain welfare from smoothing consumption during unemployment, too generous a benefit can induce longer spells of joblessness. Subsequent models, such as Shavell and Weiss (1979) explored optimal time profiles of benefits, suggesting payments should ideally decline over the spell to encourage re-employment. These early theories formalized the notion that UI's insurance value must be weighed against incentive effects.

Empirically, evidence soon confirmed UI's consumption-smoothing role: Gruber (1997) found that a 10 percentage-point increase in the UI replacement rate reduces the drop in household consumption on job loss by about 2.7%, significantly cushioning unemployed families. This finding provided quantitative support for the idea that UI mitigates welfare losses during unemployment. At the same time, researchers noted that UI could raise unemployment rates. Katz and Meyer (1990) used administrative data to show that a one week increase of UI benefit duration increases the average unemployment duration by 0.16 to 0.20 weeks. Thus, by the 1990s, the economics literature broadly accepted that UI entails a trade-off: it protects income and consumption for the unemployed, but creates moral hazard that can aggravate unemployment.

Importantly, later theoretical work added nuance to this view. General equilibrium and job-matching models indicated that UI might improve the quality of job matches. For instance, Acemoglu and Shimer (2000) argued that more generous UI could lead workers to seek higher-productivity jobs, potentially raising long-run output. Others pointed out that UI's effect on job-finding could be offset by better matches or higher post-unemployment wages. Over the decades, UI became a focal example of optimal policy design under uncertainty: Hopenhayn and Nicolini (1997) derived formulas for an ideal UI system that gradually decreases benefits to incent re-employment, and later "sufficient statistics" approaches (e.g. Chetty (2008)) quantified the welfare trade-off using risk aversion and behavioral elasticities.

By the 2000s, surveys of the literature (Atkinson and Micklewright (1991); Krueger and Meyer (2002)) concluded that while UI undoubtedly prolongs unemployment spells, it also substantially raises recipients' welfare by smoothing consumption and can even yield positive externalities (like better job matches or macroeconomic stabilization). A recent meta-analysis of 55 studies reinforces this historical perspective: it finds that despite some publication bias inflating earlier estimates, UI benefits do have a tangible "disemployment" effect (elasticity of unemployment duration with respect to benefit generosity on the order of 0.2–0.5) – yet an optimal policy is not zero UI, rather a moderate replacement rate around 30% is welfare-maximizing once both insurance and behavioral responses are accounted for (Cohen and Ganong 2024). In summary, the evolution of economic thought on UI has progressed from simple cautionary tales about work disincentives to a sophisticated understanding of how UI can be designed to balance equity and efficiency over the business cycle.

1.2 Empirical Evidence

A large empirical literature, especially from the 1980s onward, has quantified the labor market effects of unemployment insurance. Early empirical studies used aggregate time-series or cross-sectional variation and found suggestive correlations

between generous UI and higher unemployment. However, more rigorous evidence emerged as researchers exploited policy changes and micro-data. The landmark study by Katz and Meyer (1990) demonstrated a sharp spike in the job-finding rate as unemployed workers' benefits were about to expire, indicating that many workers remained unemployed until their UI ran out and then quickly found jobs. This provided clear evidence that UI availability lengthens unemployment spells. Around the same time, Meyer (1990) estimated that higher benefit levels or longer entitlement durations significantly reduce the hazard rate of leaving unemployment, with implied elasticities often in the range of 0.4–0.6. In practical terms, a 10% increase in the UI benefit level was found to increase average unemployment durations by roughly 4–8% in the U.S. context (Krueger and Meyer (2002)). Such findings, replicated in Canada and Europe, solidified the consensus that more generous UI leads to longer spells of insured unemployment, all else equal.

Notably, this effect was not merely observational – it was confirmed by natural experiments. For example, when the U.S. extended benefit durations during recessions, researchers observed proportional increases in unemployment length (Moffitt 1985). Similarly, Lalive (2008) examined an Austrian program that dramatically extended benefits for older workers (from 1 year to up to 4 years) and found the job-finding rate plummeted for the treated group. In essence, as benefits became available for longer, workers – especially those near retirement age – delayed re-entry into employment. These seminal contributions established a causal link between UI and labor supply behavior, leveraging difference-in-differences (policy reforms across regions or time) and regression discontinuity designs for credible identification.

Over time, researchers applied increasingly creative strategies to pin down causality. Regression discontinuity (RD) designs have been particularly influential. In some countries, UI benefit schedules or durations change discontinuously with age or tenure. Schmieder, Wachter, and Bender (2012) exploited age thresholds in Germany's UI system and confirmed that extending UI entitlement by one month causally increases unemployment duration (by roughly 0.1–0.2 months on average),

remarkably similar to estimates from the U.S. and elsewhere. Likewise, Lalive (2007) and Card, Chetty, and Weber (2007) used RD and related methods in Austria and found sizable effects on spell length when benefits were extended. The consistency of findings across settings (U.S., Canada, Europe) lent credibility to the external validity of the result: UI dampens the urgency of job search.

In addition, randomized experiments have been conducted to study UI incentives. One famous trial was the Illinois Reemployment Bonus Experiment in the 1980s, which offered UI recipients a lump-sum bonus for quick reemployment. The experiment showed that modest financial incentives ($500 bonuses) induced earlier exits from unemployment, albeit the effects were not large (e.g. reducing average duration by about a week) and the program's cost-effectiveness was debated. Nonetheless, it confirmed that unemployed workers respond to financial incentives, reinforcing the evidence that the structure of UI benefits influences behavior.

Beyond unemployment duration, seminal studies have examined how UI affects other labor market outcomes like re-employment wages, job match quality, and long-run earnings. Theoretically, longer search enabled by UI could lead to better matches (higher wages or productivity), but longer non-employment could also erode skills or signal lower productivity to employers. The empirical evidence is mixed. On the one hand, several studies find positive effects on job quality: for example, a recent study in Austria found that a moderate extension of benefits led to a 0.5% increase in re-employment wages on average (Nekoei and Weber 2017), suggesting that extra search time helped workers find better matches. And in the United States, longer potential UI durations have been linked to unemployed individuals holding out for higher productivity jobs, which can even spur job creation by improving matching efficiency (Centeno and Novo 2006). These findings echo the insight of Acemoglu and Shimer (2000) that UI can raise overall productivity.

On the other hand, some research finds little gain in match quality: a study in Germany found a slight reduction in re-employment wages (~0.8%) when benefits were extended (Schmieder and Wachter 2016) and a study in France detected no dis-

cernible impact on subsequent job match quality or wages (Le Barbanchon 2016). Thus, while UI unambiguously lengthens unemployment spells, its impact on post-unemployment outcomes can vary – with some contexts showing improved job matches and others showing flat or slightly negative effects. Part of this difference may arise from whether UI mainly delays acceptance of *sub-par* jobs (thereby improving matches) or whether it causes skill atrophy. Recent meta-analyses and cross-country comparisons indicate that in aggregate, the micro-elasticity of unemployment with respect to UI is similar to the macro-level impact, implying that general equilibrium effects (like wage adjustments or vacancy creation) do not dramatically alter the basic conclusion (Cohen and Ganong 2024). In summary, the seminal empirical literature – through a combination of natural experiments, policy discontinuities, and even randomized trials – has firmly established that UI benefits *cause* longer unemployment spells, while also highlighting important nuances in terms of search intensity, job quality, and the optimal calibration of UI in different contexts.

1.3 Developing Countries and Informal Labor Markets

While most classic UI studies focus on advanced economies, a growing body of research examines how unemployment benefits operate in developing countries, where labor markets are characterized by high informality and weaker administrative capacity (Ulku and Georgieva, n.d.). In many developing economies, formal UI systems either did not exist historically or covered only a small fraction of workers. Instead, governments often relied on protective labor regulations – such as stringent job security rules and mandated severance pay – as a way to shield workers from income loss. This "protecting jobs" approach (e.g. requiring large severance payouts or restricting layoffs) was seen as more feasible in environments without the bureaucratic means to implement a contributory UI system. However, it came at the cost of labor market rigidity (Garibaldi and Violante 2005). By the 2000s, there was increas-

ing recognition that overly strict labor regulations impeded job creation, leading to reforms embracing "flexicurity" – flexibility in hiring/firing combined with security via unemployment benefits. Chile's introduction of a national UI system in 2002 was a pioneering example, and other middle-income countries (Brazil, Colombia, etc.) gradually expanded or created UI programs. Yet, implementing UI in developing contexts raises unique challenges: most notably, the presence of a large informal sector where workers and firms operate outside the regulated, tax-paying economy.

A key question is how informal labor markets interact with unemployment insurance. Conventional wisdom suggested that UI could be especially distortionary in such settings – for example, workers might continue working informally while claiming UI, effectively turning the benefit into a hidden subsidy for off-the-books employment. Indeed, early analyses posited that a large informal sector would magnify UI's moral hazard problem, as beneficiaries could earn untaxed income without forfeiting benefits (Hartley, Ours, and Vodopivec 2011). Recent research has sought to quantify these effects. In an important study on Brazil, Gerard and Gonzaga (2021) combined an optimal UI framework with Brazilian data to evaluate whether informality increases the efficiency costs of UI. Using a quasi-experimental variation in benefit duration (an age-based eligibility cutoff for extended benefits), they found the usual result that UI prolongs formal unemployment spells (moral hazard), but intriguingly the efficiency cost was *lower* in Brazil than in the United States. In areas or demographic groups with higher informality, the incremental effect of UI on formal re-employment was smaller, essentially because formal job-finding rates were low to begin with in those high-informality segments. Many workers would remain jobless or informal with or without UI, so the relative "excess" duration caused by benefits was modest. Their conclusion was that moral hazard may actually become a more salient concern as an economy formalizes – a counterintuitive insight that in very informal economies, UI might do less harm than expected (since the alternative to formal unemployment is often informal work or inactivity anyway).

Other evidence from developing countries, however, shows that UI can induce shifts

to informal employment during the insured spell. In Brazil, formal employees are legally barred from concurrently holding a registered job while on UI, but they can and often do find unregistered work. Britto (2022) provides causal evidence on this behavior. In his study of Brazil's UI system and severance pay scheme (*Fundo de Garantia do Tempo de Serviço*, FGTS), Britto first uses a regression discontinuity design to show that receiving a lump-sum severance payment (on formal job) lengthens the time until a worker returns to a formal job by about 1.9 weeks after three years since displacement. He then examines a one-month extension of UI benefits (using another discontinuity in eligibility) and finds that the extended UI causes an even larger initial increase in formal unemployment duration compared to the lump-sum. Crucially, by matching administrative data with household surveys, Britto finds that 57% of the "lost" formal employment time under extended UI is offset by increased informal employment. In other words, more than half of those extra weeks of formal joblessness are weeks in which the individuals are working informally while collecting UI. This reveals a strategic response: workers exploit the benefit by substituting formal jobs (which would end UI receipt) with informal work until benefits are exhausted. Such findings underscore the importance of informality as an auxiliary unemployment insurance mechanism in developing economies.

That said, the medium-term impacts of UI in these contexts may be more benign than the short-term behavior suggests. Britto's analysis shows that although an extra month of UI induces a significant temporary drop in formal employment, those on extended UI partially catch up in employment after benefits expire, so that over a three-year horizon their total time in formal work is similar to those who only got the lump-sum. Moreover, the UI recipients found jobs with higher wages than those who only got the lump-sum severance. This aligns with evidence from other developing settings that UI can improve match quality or give workers more bargaining power. Indeed, Britto finds the one-month UI extension was less harmful to reemployment wages than the lump-sum assistance, possibly because having ongoing benefits allowed workers to hold out for better offers. Similarly, a study in

Argentina found that unemployment benefits recipients tended to find formal jobs with slightly higher wages than non-recipients at only a moderate cost of increased unemployment duration (González-Rozada and Ruffo 2023).

In summary, global evidence from middle- and low-income countries suggests that the fundamental effects of UI – consumption smoothing and extended job-search time – appear universal, but high informality profoundly shapes outcomes. UI in developing economies often has "leakage" into the informal sector: some share of beneficiaries will work outside the formal system while receiving benefits (Britto 2022). This cushions their income (a form of hidden employment) but undermines the intent of UI and raises questions about monitoring. Yet, the presence of informality can also mean the net social cost of UI is not as high as in a fully formal setting, since many would not have formal jobs regardless (Gerard and Gonzaga 2021). Policymakers thus face a dilemma: how to extend unemployment protection to workers in economies where many do not work in the formal sector. Experiments are ongoing – for example, some countries have tested unemployment assistance for self-employed or informal workers (often via cash transfer programs rather than true "insurance"). Overall, the literature underscores that context matters – the optimal design and expected effects of UI in a developing country (with limited enforcement and many informal jobs) will differ from those in a developed economy. Research on Brazil, Mexico, Chile, and others is gradually filling in these gaps, pointing to the importance of complementary policies (like cracking down on informal work by UI claimants, integrating informal workers into the system through incentives or through active labor market policies) to make unemployment protection effective in the developing world.

1.4 Pension-Integrated and Quasi-UI Schemes

Beyond traditional unemployment insurance, many countries have implemented or experimented with alternative mechanisms to protect workers against job loss.

These *pension-adjacent* or *quasi-UI* schemes include programs like lump-sum severance pay, unemployment savings accounts, and allowing early withdrawals from pension funds during unemployment. Such schemes are especially common in countries that lack a comprehensive UI system or seek to reduce the moral hazard associated with standard UI benefits.

Severance pay is the oldest and most widespread alternative to UI. Under severance mandates, employers must provide a one-time payout to workers upon involuntary separation, typically based on tenure. This lump-sum acts as a form of unemployment compensation, but it is financed at the firm level and paid only at layoff. Many Latin American and Asian countries historically relied on severance pay in lieu of UI. However, severance has well-documented shortcomings: it usually covers only formal sector layoffs, provides no help to those who quit or were informally employed, and may deter employers from hiring, or encourage firing workers before they accumulate tenure.

Recognizing the limitations of pure severance pay, some countries have adopted Unemployment Insurance Savings Accounts (UISAs) or hybrid systems that combine self-insurance with social insurance. The idea of UISAs, posited by academics like Feldstein and Altman (2007), Orszag and Snower (2002) and Stiglitz and Yun (2005), is to have each worker accumulate savings in an individual account during employment, which can be drawn down during periods of unemployment. If the worker's account balance is insufficient, a public solidarity fund or government backstop can cover the difference, ensuring a minimum benefit, but importantly, any unused balance remains the worker's property, often convertible to retirement funds. This mechanism aims to *internalize the cost* of unemployment benefits – since workers essentially pay for their own UI (at least up to a point), they have less incentive to abuse the system. In theory, UISAs should reduce the moral hazard problem inherent in pooled UI, because any withdrawal today will deplete one's own savings (or future pension), thereby discouraging unnecessary use.

Chile's UI system, introduced in 2002, is a flagship example of UISA. It established

individual unemployment accounts financed by payroll contributions, alongside a common *Fondo Solidario* for those who exhaust their savings or have very low balances (Hartley, Ours, and Vodopivec 2011). Workers first draw benefits from their personal account; only if that is empty (and if they meet eligibility criteria) do they receive benefits from the solidarity pool. This two-tier design was intended to preserve work incentives while still providing a safety net for the most vulnerable. By 2008, about 80% of Chilean private-sector workers were contributing to these accounts, and the system had expanded coverage dramatically compared to the old severance pay regime.

Studies evaluating Chile's reform find encouraging results. Hartley, Ours, and Vodopivec (2011) reported that the introduction of UISAs improved re-employment incentives: workers with ample savings in their accounts did not prolong their unemployment spells as much as they would have under a pure UI system, since they were drawing on their own funds. In line with this, the authors found that an extension of benefits financed by the solidarity fund led to longer unemployment durations (a moral hazard effect), whereas those relying solely on their individual accounts showed no increase in unemployment duration. This absence of behavioral response in the self-funded tier suggests that when workers use their own money (forced-savings) for unemployment, they treat it differently than a no-strings-attached benefit (Ulku and Georgieva, n.d.). Essentially, Chile's experience showed that moral hazard may be sharply reduced by the UISA design – a finding consistent with theoretical expectations.

Following Chile, countries like Colombia and Mauritius implemented similar savings-account-based unemployment benefit schemes (Ulku and Georgieva, n.d.). Colombia, for instance, has a program of *cesantías* (unemployment savings) where employers deposit a month's pay per year into an account that workers can withdraw upon separation or other contingencies. These systems blur the line between pensions and unemployment insurance, effectively using a pre-funded individual buffer to handle job-loss income shocks.

Mexico provides a salient case of a quasi-UI pension withdrawal scheme with the *Retiro Parcial por Desempleo* (RPD). Lacking a formal UI program for most workers[1], Mexico allows individuals to make a withdrawal from their Afore (individual retirement savings account) when they become unemployed. This policy, in effect since the early 2000s, treats the retirement account as a self-insurance vehicle for unemployment.[2] Studies by Mexico's pension regulator have noted a sharp increase in such withdrawals, especially during the COVID-19 Pandemic, and have continued to raise since (CONSAR 2024), likely due to the increased salience that the program achieved during the employment crisis that followed the COVID-19 pandemic.

Empirical evidence of the RPD scheme is still emerging. One concern is that allowing easy access to pension funds in mid-career could undermine long-term savings adequacy. Another is that, similar to severance, a one-off withdrawal may be spent quickly. Evidence from Brazil's severance suggests people exhibit *present bias*: Gerard and Naritomi (2021) showed that upon layoff, Brazilian workers who received their FGTS lump-sum increased their consumption by 35% in the initial weeks, even though they subsequently faced a 14% drop in consumption in the longer term. This indicates that lump-sum payments tend to be consumed rather than smoothed, potentially leaving individuals unprotected later in their unemployment spell. We might expect a similar pattern for pension withdrawals – an upfront infusion that could be exhausted before re-employment, thereby failing to fully smooth income over the unemployment duration.

Velázquez Guadarrama (2023) used public data from the *Encuesta Nacional de Ocupación y Empleo* (ENOE) and a regression discontinuity design to show that the RPD program has several significant causal effects on labor market outcomes. Specifically, the study estimates that being eligible for the RPD increases salary income by 2.47%, suggesting that having access to these funds allows individuals to pur-

[1] A state-level unemployment insurance was implemented in Mexico City, albeit coverage is low, eligibility rules are obscure, and its benefits are small since it is funded through the state budget (Loa Aguirre and Rubio Campos 2019).

[2] I present the features of the RPD program in detail in Section 2.

sue potentially riskier, better-paying jobs or engage in longer hiring processes. He also finds that eligibility increases the probability of being employed by 2.67%, a result noted as potentially counterintuitive, but which might be explained by individuals taking jobs perceived as having a higher risk of future unemployment or, importantly, demonstrating that the moral hazard often associated with traditional unemployment insurance is mitigated since the funds are drawn from the individual's own savings. Finally, eligibility leads to an estimated increase in the duration of unemployment by 83 days, implying that individuals eligible for the program utilize the financial support to extend their job search and find a better match for their skills and needs. These results collectively suggest that integrating unemployment benefits with a pension system, can be an effective mechanism for mitigating moral hazard while potentially improving resource allocation in the labor market through higher wages and employment probability, despite increasing the duration of unemployment.

Moreover, Carreño Godínez (2025) also used ENOE data and a Difference-in-Differences design to show that eligibility for the RPD is estimated to increase the probability of transitioning into entrepreneurship. Depending on the model specification used, this increase is estimated to be between 5.01% and 21.4% greater for eligible individuals compared to those who do not meet the eligibility criteria. Carreño interprets these results as evidence that, in a context like Mexico where access to credit is limited and a traditional state-funded unemployment insurance does not exist, the early withdrawal mechanism from individual retirement accounts (Afore) functions as a financial safety net. This access to financial resources appears to facilitate the creation of new businesses by providing individuals with the necessary support to undertake the risky decision of starting an enterprise. These findings suggest that mechanisms allowing access to individual savings can indirectly promote entrepreneurship by providing a lump-sum payment to the unemployed.

Chapter 2

Institutional Background

This section provides an overview of the institutional framework surrounding unemployment protection in Mexico, with a focus on the *Retiro Parcial por Desempleo* (RPD) program. I begin by describing the eligibility conditions and usage rules of the RPD, a mechanism that allows unemployed workers to make partial withdrawals from their individual pension accounts. I then document the evolution of the program's adoption over time, highlighting its increased relevance during periods of economic crisis. Finally, I situate RPD within the broader context of income substitution in Mexico, where the failure to enforce legally mandated severance pay has rendered employer-based protections largely ineffective. In this setting, RPD emerges as a self-financed, readily accessible—but ultimately limited—alternative for workers facing job loss.

2.1 The Partial Withdrawal

The RPD program works as follows. The pension system in Mexico is a standard defined contributions scheme with individual accounts. While working in formality, workers accumulate resources financed in part by themselves, by their employer and by the government.

2.1.1 Eligibility Criteria

If a worker faces an unemployment spell of at least 46 days, she may withdraw a fraction of her individual account balance through the RPD. The amount of money a worker can have access to depends on the number of years since her account was opened and the number of weeks she has contributed to social security. The *Ley del Seguro Social* considers two eligibility schemes, each allowing a worker to withdraw an amount up to:

A. Ten months of the minimum wage or one month of her most recent wage, whichever is lower, for workers satisfying:

   i. having entered the formal labor market at least three years ago, and

   ii. having contributed to social security for at least two years.[1]

B. Three times her monthly wage or 11.5% of her account balance, whichever is lower, for workers who entered the formal labor market at least three years ago, regardless of contributions to social security.

Figure 2.1 provides a visual representation of the eligibility criteria. Note that a worker who became dismissed after four years since she entered the labor market, and with only one year of contributions to social security would not be immediately eligible to withdraw funds from her account. However, if she just waited in unemployment for a year, she would become eligible through Scheme B, without having

[1] Contributions to social security need not be continuous to reach eligibility to RPD.

to make further contributions. Hence, ineligible workers may become eligible by the mere passage of time. This feature of the eligibility structure is important to the construction of the analysis sample, as I show in Section 3.2.

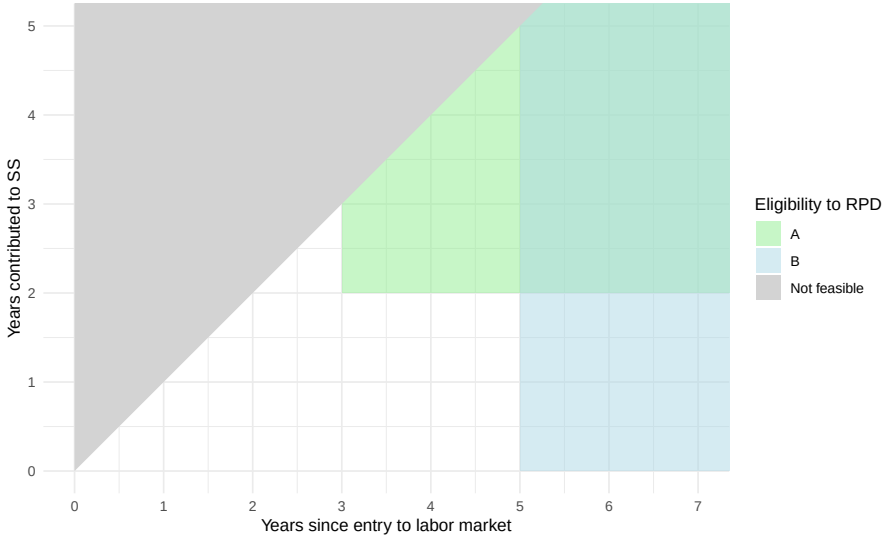
Moreover, workers who are eligible through both schemes may choose the one that allows them to withdraw the largest amount. Having used the RPD once, a worker may use it again, only after waiting for five years since her last withdrawal. Importantly, using the RPD not only depletes the worker's individual pension savings account, but it also reduces proportionally the amount of contributions to social security, which are used to determine and finance the pension at retirement age. This is a key feature of the RPD, as it allows workers to access their savings during unemployment, but it also reduces their future pension benefits.

2.2 Usage and Adoption

Figure 2.2 shows the cumulative number of partial withdraws from the individualized pension accounts since it was implemented in 1997. The data comes from a confidential administrative data set from the *Instituto Mexicano del Seguro Social* (IMSS). The RPD program was first implement in 2009 to give unemployed workers a source of income substitution in the context of the Great Recession. This event can be seen as a sharp increase in the usage of RPD, but also as a change in the trend of the number of withdrawals from the pension system. Accordingly, in the next big employment crisis, the COVID-19 pandemic, another trend increase followed. This shifts in the slope of the cumulative usage of RPD shows that employment crises have a persistent effect on the usage of the program, likely due to the increased salience of the program.

To put figures into perspective, by the end of 2024, 16,966,851 workers had made 24,598,838 withdrawals from the pension system. By that same moment, IMSS reported a total of 22,238,379 active workers in the formal labor market. Indeed, not all RPD users were active workers by the end of 2024, but this number gives a sense

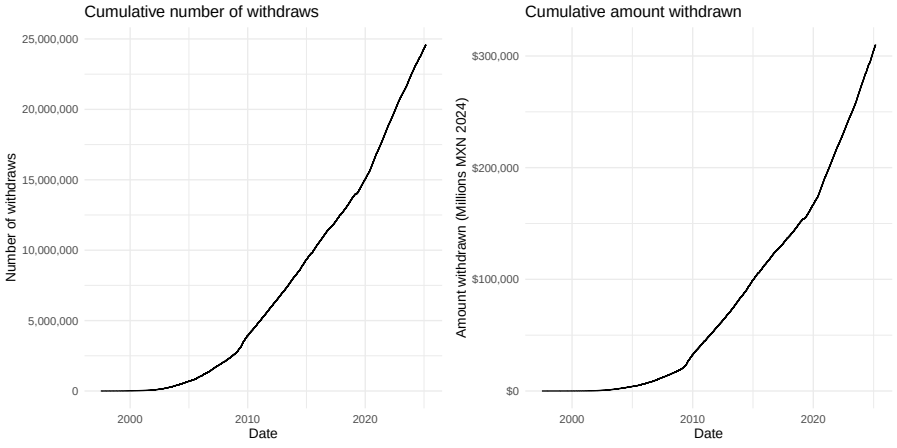
Figure 2.1: Eligibility Criteria



Note: This figure conceptualizes eligibility to RPD. The x-axis shows the number of years since entry to the labor market, while the y-axis shows the number of years contributed to social security. The green area represents the eligibility criteria for Scheme A: having entered the formal labor market at least three years before and having contributed to social security for at lest two years. The blue area represents the eligibility criteria for Scheme B: having entered the formal labor market at least three years before, regardless of contributions to social security. The gray area represents the not feasible zone: workers cannot contribute more days to social security than they have been in the formal labor market.

of the magnitude of the usage program.

Figure 2.2: Usage and Adoption



Note: This figure shows the cumulative number of partial withdrawals (left) and the cumulative amount withdrawn (right) out of the Mexican individual pension account system (Afore). Source: IMSS.

2.3 Income Substitution for the Unemployed in Mexico

In theory, Mexican labor law provides mandatory severance pay (indemnización constitucional) as the main form of income substitution for workers who lose their jobs. Upon unjustified dismissal, employers are required to provide affected workers with three months of salary, plus seniority bonuses and accrued benefits. This legal severance framework reflects the broader Latin American tradition of employer liability systems as a substitute for unemployment insurance. However, in practice, enforcement of these entitlements is weak, uneven, and often illusory—rendering severance rights largely ineffective as a tool for income smoothing during unem-

ployment.

The core issue lies in the functioning of Mexico's labor courts. In particular, in Mexico City, workers who wish to claim their legally mandated severance must navigate a judicial process that is slow, opaque, and vulnerable to both corruption and administrative incompetence. Degetau (2023) provides experimental evidence from the Mexico City Local Labor Courts showing that the mere act of serving notification—a basic procedural step that triggers the legal process—is often compromised. The study reveals that notification orders may fail not due to complexity or distance but because of strategic delays enabled by collusion between defendants (employers) and court notifiers.

Further compounding these problems is the widespread information asymmetry among litigants. Sadka, Seira, and Woodruff (2024) demonstrate that plaintiffs in labor disputes often have unrealistic expectations about the outcomes of their cases, which leads to protracted litigation and missed opportunities for settlement. Their randomized controlled trial shows that providing plaintiffs with predictive information about expected case duration, probability of success, and likely monetary outcomes significantly increases same-day settlement rates and improves short-term financial well-being. However, this intervention also reveals the deep dysfunction of the system: only when plaintiffs are given external tools to navigate uncertainty can they extract meaningful value from the legal process.

The statistical odds of a dismissed worker successfully enforcing their severance rights are vanishingly low. As Sadka, Seira, and Woodruff (2024) document, more than half of all dismissed workers in Mexico report never receiving severance pay, yet only 13% pursue legal action—a striking indication of the widespread perception that litigation is futile . And with good reason: nearly 30% of lawsuits remain unresolved four years after being filed, leaving workers in legal limbo with no income and no resolution. Even among the rare plaintiffs who persist through the full litigation process and win a favorable judgment, only 20% ever collect the full amount awarded. The practical message is devastatingly clear: in Mexico's current labor

justice system, even winning your case is no guarantee of receiving what the law promises. For the average worker facing dismissal, the path to severance is not just slow and bureaucratic—it is overwhelmingly stacked against them.

Taken together, these findings paint a troubling picture: while Mexico's legal framework nominally guarantees severance pay as income support for the unemployed, the institutions tasked with enforcing this right often fail to deliver. Procedural inefficiencies, the potential for corruption, and severe information gaps all contribute to a de facto collapse of enforcement. As a result, many dismissed workers are left without effective legal recourse, and severance pay—despite being codified in law—cannot be relied upon as a real source of income during unemployment.

In this vacuum, workers may increasingly turn to alternative, self-financed mechanisms such as the RPD program—a partial withdrawal from their individual pension accounts—as their only accessible form of income substitution. Unlike severance pay, RPD is automatic and does not require litigation. However, it also lacks redistributive features, may deplete retirement savings, and provides only modest relief. This shift from legal entitlements to self-funded liquidity illustrates a broader institutional failure: in the absence of credible enforcement, even well-designed labor protections may become functionally irrelevant.

Chapter 3

Data

3.1 Data Sources

To study the effect of the Retiro Parcial por Desempleo (RPD) on labor market outcomes, I use two confidential administrative data sources. The first is a matched employer-employee panel from the Instituto Mexicano del Seguro Social (IMSS), which tracks the entirety of the formal labor market. From this data set, I construct eligibility criteria and labor market outcomes. I restrict the analysis to workers who first entered the formal labor market between January 2010 and December 2017.[1] This yields a universe of 14,819,690 workers across 406,813,240 labor contracts.

Although the IMSS employment data does not directly record unemployment episodes (i.e., the cause, onset, or duration of unemployment), unemployment spells can be inferred from gaps between labor contracts. I define a gap as a period during which an individual is not formally employed by any firm. While such gaps

[1] This restriction ensures that all workers are subject to the same pension reform (in effect since July 1997) and thus face uniform incentives regarding the accumulation of weeks of contribution and the use of their individual pension account balance for income substitution during unemployment.

do not necessarily imply unemployment[2], they are sufficient for the RPD authority to consider a worker eligible for benefits.

The second data source is the administrative registry of partial withdrawals from individual pension accounts. This registry includes every instance of RPD usage and records the date of withdrawal, amount withdrawn, account balance, weeks contributed to social security at that point, the last employer's ID, and the worker's unique identifier (Número de Seguridad Social, NSS). Crucially, I use the NSS to match the two data sets, enabling me to track the labor market trajectories of RPD users before and after its use. See Section 2.2 for a detailed overview of the RPD program and its adoption.

3.2 Sample Selection

To estimate the effect of RPD on labor market outcomes, I leverage the eligibility criterion of having at least two years of contributions to social security at the moment of unemployment as s source of quasi-exogenous variation, (See Section 2 for context on the eligibility criteria.). Hence, I restrict the analysis sample to workers who (i) faced exactly one unemployment spell of at least 46 days during the third year since they joined the formal labor marked, and who (ii) had accumulated between one and three years worth of contributions to the social security system.

Additionally, I restricted the analysis sample to workers with (a) ages between 18 and 65 years old, (b) who were registered in the *Régimen obligatorio* as permanent or eventual workers, (c) were not self-employed, (d) were not registered to an agrarian firm, and (e) were not in the top 1% of the unemployment duration distribution.

These restrictions yield an analysis sample of of 877,749 workers. Figure 3.1 shows the sample selection of workers in the eligibility criteria space at the time their

[2]A gap could result from voluntary separation, a transition into informal work, or involuntary displacement.

unique unemployment spell began. Although the restrictions I impose on the analysis sample render my results non-generalizable to the entire population of workers, they allow me to leverage the institutional characteristics for identification of the effect of eligibility to RPD on labor market outcomes. Table 5.8 shows additional summary statistics of the analysis sample.

Within the analysis sample, only those workers who had accumulated at least two years of contributions to the social security system by the time they become displaced are eligible to withdraw funds from their pension account. Table 3.1 shows workers' features across eligibility status at the moment of displacement. Indeed, there is a systematic positive selection of workers to eligibility, as workers who are eligible have contributed more to social security, had higher wages and longer tenures before displacement. Groups show imbalance across other characteristics mainly because the sample size is large, while the magnitude of the difference is not economically significant. Critically for my identification strategy, I show in Section 4.1 that these differences across eligibility status are smooth around the eligibility threshold of 2 years of contributions to social security.

Figure 3.1: Sample Selection

Note: This figure provides a conceptual representation of eligibility for the RPD program. The x-axis denotes years since entry into the formal labor market, and the y-axis indicates years of contributions to social security. The green area corresponds to Scheme A eligibility, which requires at least three years since labor market entry and a minimum of two years of social security contributions. The blue area corresponds to Scheme B eligibility, which requires at least three years since labor market entry, irrespective of contribution history. The red square marks the subsample selected for empirical analysis. The gray area represents an infeasible region, as individuals cannot accumulate more years of social security contributions than years since formal labor market entry.

Table 3.1: Balance of Outcomes and Covariates by Eligibility Status

Characteristic	Eligible N = 427,927	Ineligible N = 449,822	p-value
Displacement date	2,018.07 (2.12)	2,018.02 (2.10)	<0.001
Outcomes			
Take up - 12 months	37,400 (8.7%)	685 (0.2%)	<0.001
Survival - 3 months	322,651 (75%)	366,208 (81%)	<0.001
Unemp. Dur. - Cens. 6 months	20.0 (7.2)	21.2 (6.7)	<0.001
Unemp. Dur. - Cens. 36 months	50 (48)	56 (50)	<0.001
Eligibility criteria			
Days since entry to formal labor market	1,264 (104)	1,265 (103)	<0.001
Days contributed to SS	914 (106)	545 (106)	<0.001
Previous job			
Tenure (weeks)	35 (35)	21 (21)	<0.001
Daily wage (MXN 2024)	283 (199)	248 (151)	<0.001
Demographics			
Female	163,078 (38%)	167,778 (37%)	<0.001
Age at displacement date (years)	24.9 (5.6)	24.5 (5.3)	<0.001
No CURP	13,929 (3.3%)	16,106 (3.6%)	<0.001

Note:
This table reports means and standard deviations (in parentheses) for key characteristics in the analysis sample, disaggregated by eligibility status at the date of displacement. For binary variables—such as *Take-up*, *Survival*, Female, and *No CURP*—the table reports the count and the corresponding percentage (in parentheses). *No CURP* indicates that the worker does not have a unique population identification number (CURP) registered with the IMSS. The p-value column reports the results from hypothesis tests of equality of means across eligible and ineligible groups.

Chapter 4

Empirical Strategy

To estimate the effect of the *Retiro Parcial por Desempleo* (RPD) on labor market outcomes I use a regression discontinuity design (RDD) exploiting the eligibility condition of having contributed to social security for at least two years at the moment of displacement. I leverage data on RPD take up to estimate the local average treatment effect (LATE) on compliers using a fuzzy discontinuity design. The fuzzy RDD can be expressed as a two-stage least squares regression, where the first stage estimates the effect of eligibility on take up of the RPD program as in

$$T_i = \alpha + \beta D_i + f(C_i - 2) + u_i \quad \text{where} \quad |C_i - 2| \leq h \tag{4.1}$$

where T_i is a dummy indicating worker i's take up to RPD, D_i indicates whether i is eligible to withdraw funds from her pension account, i.e. $C_i \geq 2$, where C_i represents i's contributions to SS measured in years. $f(\cdot)$ is a polynomial function of C_i and h is the bandwidth around the threshold. β identifies the causal effect of eligibility on take up of the RPD program.

Hence, the second stage estimates the effect of taking up RPD on labor market outcomes as in

$$Y_i = \gamma + \tau \hat{T}_i + f(C_i - 2) + v_i \quad \text{where} \quad |C_i - 2| \leq h \tag{4.2}$$

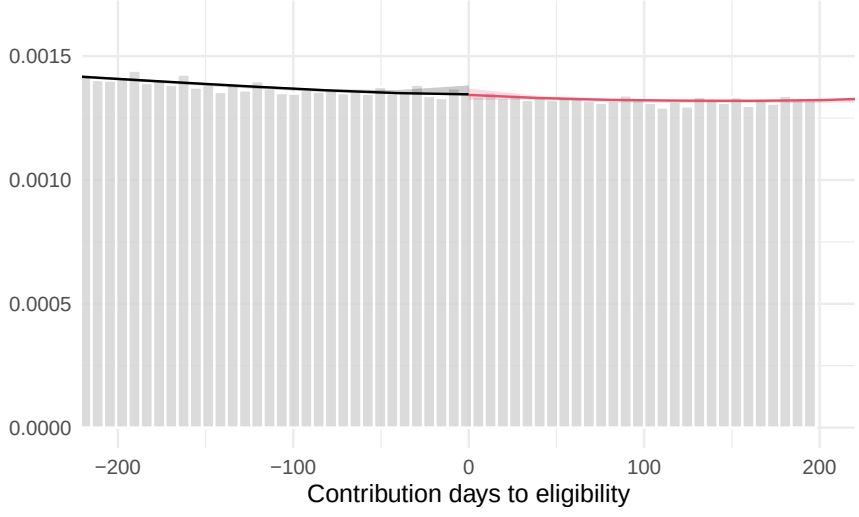
where Y_i is an outcome of interest, $\hat{T}_i$ is the predicted value of the first stage. The main parameter of interest is τ, which identifies the LATE around the eligibility threshold. I use a local linear polynomial ($p = 1$) with a triangular kernel and the bandwidth selected using the method proposed by Calonico, Cattaneo, and Farrell (2020). Specifically, I'm interested in the hypothesis test given by $H_0 : \tau = 0$ vs $H_1 : \tau \neq 0$. For inference, I use heteroskedasticity-robust standard errors with nearest-neighbor variance estimator, as proposed by Calonico, Cattaneo, and Titiunik (2014).

4.1 Evidence on Design Validity

I provide two main tests on the validity of the sharp design in Equation 4.1 plus an additional test on the validity of the fuzzy design in Equation 4.2. First, I show that there is no evidence of manipulation around the eligibility threshold. Second, I show that the covariates are balanced around the eligibility threshold. Finally, I show that the first stage is strong.

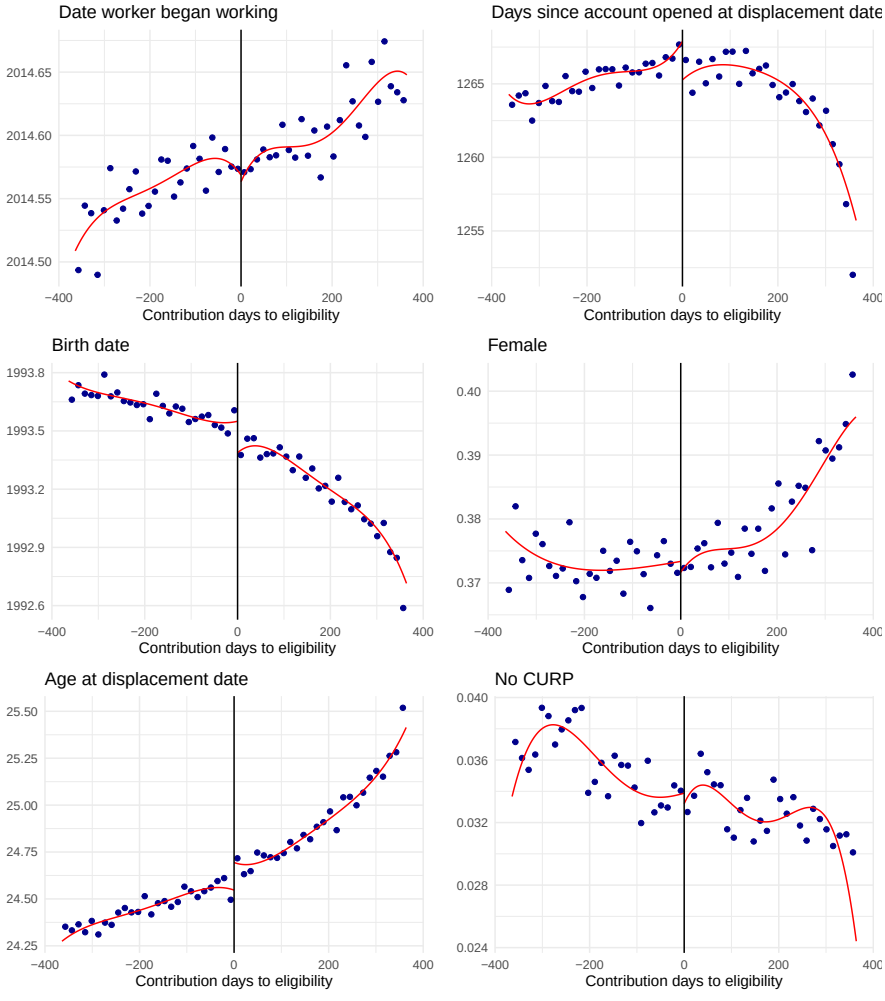
First, Figure 4.1 shows the result of the Cattaneo, Jansson, and Ma (2020) test for manipulation of the running variable. The figure shows no significant bunching around the eligibility threshold, consistent with the absence of manipulation. Second, Figure 4.2 and Figure 4.3 show the continuity of covariates of workers around the eligibility threshold. Crucially, Table 4.1 shows the estimation and inference of Equation 4.1, showing there is no evidence of discontinuity at the threshold for any of the covariates (Cattaneo, Idrobo, and Titiunik 2019). Finally, I present evidence on the strength of the first stage in Section 5.1.

Figure 4.1: Density of the Running Variable



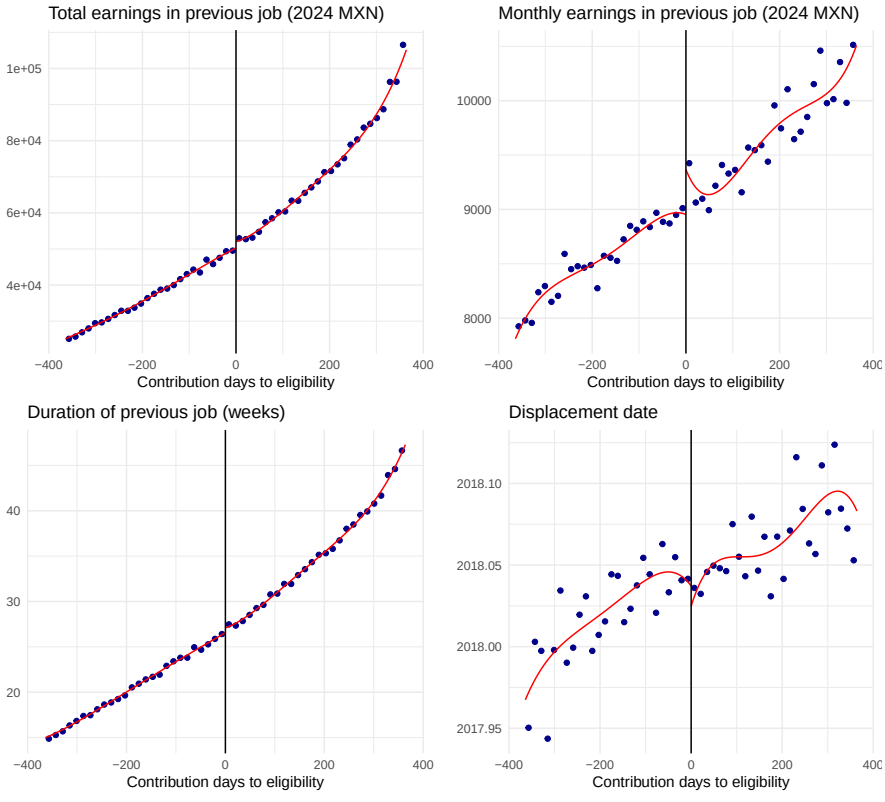
Note: This figure shows there was no manipulation into eligibility by altering the days of contributions to social security at the moment of displacement (Cattaneo, Jansson, and Ma 2020).

Figure 4.2: Covariates around Eligibility Threshold



Note: This figure shows (*i*) sample means for evenly-spaced 14-day bins relative to the eligibility threshold, and (*ii*) the underlying regression function estimated using a triangular kernel, optimal bandwidth and a linear local polynomial using Calonico, Cattaneo, and Titiunik (2015). The figure shows workers' covariates do not systematically jump at the eligibility threshold.

Figure 4.3: Previous Job Outcomes around Eligibility Threshold



Note: his figure shows (*i*) sample means for evenly-spaced 14-day bins relative to the eligibility threshold, and (*ii*) the underlying regression function estimated using a triangular kernel, optimal bandwidth and a linear local polynomial using Calonico, Cattaneo, and Titiunik (2015). The figure shows workers' previous job outcomes do not systematically jump at the eligibility threshold.

Table 4.1: Eligibility Effect: Balance of Covariates

	Female	Birth date	Began working	Unemployment date	Age
RPD	−0.001	−0.143	−0.005	−0.011	0.125
	(0.005)	(0.059)	(0.022)	(0.022)	(0.051)
Mean at cutoff - ineligible	0.374	1,994	2,015	2,018	24.6
Observations	877,749	847,714	877,749	877,749	847,714
	Days since account opened	No CURP	Prev job duration (weeks)	Prev job total earnings	Prev job av monthly earnings
RPD	−2.124	−0.002	0.424	1634.627	311.151
	(1.005)	(0.002)	(0.315)	(843.632)	(214.297)
Mean at cutoff - ineligible	1,268	0.0343	26.7	50,504	8,997
Observations	877,749	877,749	877,749	877,749	877,749

Note: This table reports the estimated coefficient of interest from Equation 4.1, obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the procedure of Calonico, Cattaneo, and Titiunik (2014).

Chapter 5

Results

This section presents the empirical results of the fuzzy regression discontinuity design (RDD) used to estimate the causal effect of the *Retiro Parcial por Desempleo* (RPD) program on labor market outcomes. I begin by documenting the first-stage effects of program eligibility on take-up, which justifies the fuzzy RDD strategy. I then estimate the local average treatment effect of RPD take-up on job search behavior and medium-term labor market outcomes. The results are complemented by a heterogeneity analysis that explores how these effects vary by gender, income level, age, and exposure to the COVID-19 pandemic.

5.1 First Stage

This section presents evidence that workers eligible to the RPD program (with at least two years of social security contributions at the time of job displacement) are significantly more likely to take up than ineligible workers. These first-stage results confirm three key points: (i) the running variable has been correctly constructed,

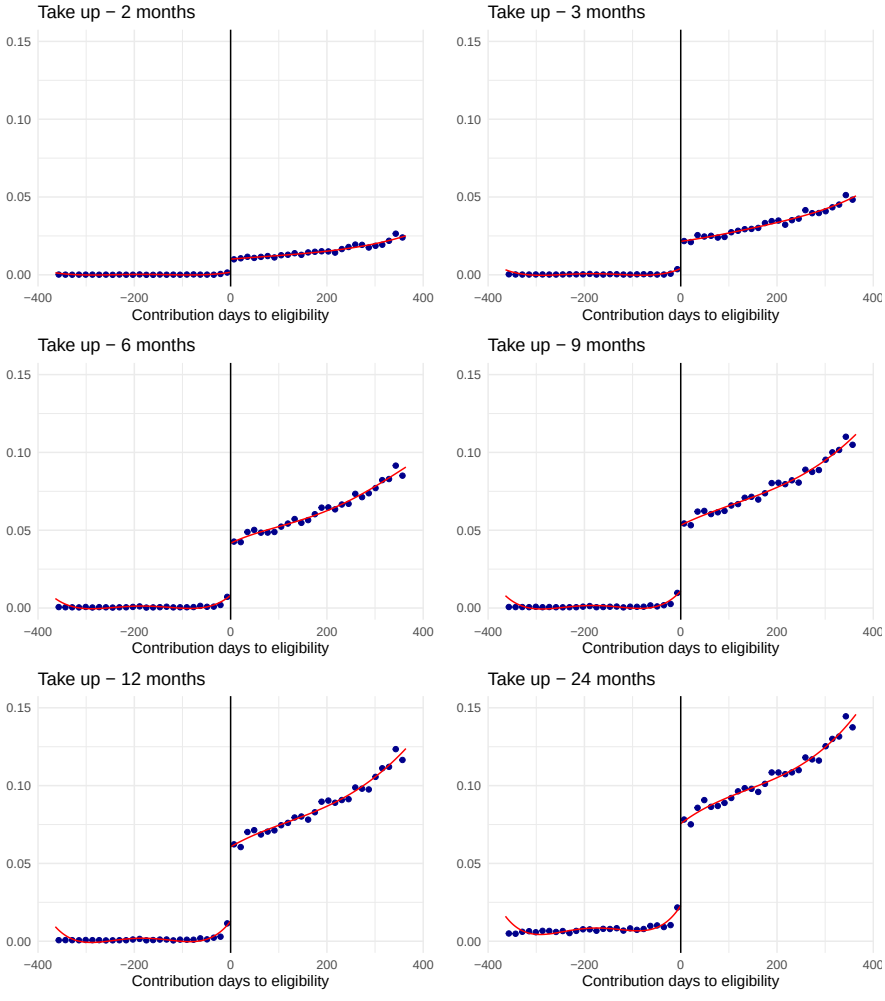
(ii) the first stage of the fuzzy regression discontinuity design is strong, and (iii) eligibility has a meaningful, albeit modest, effect on program take-up.

Figure 5.1 shows that displaced workers just above the eligibility threshold are more likely to access the RPD program to withdraw funds from their pension account. Inference from Figure 5.2 and Table 5.1 confirms that the take-up discontinuity is statistically significant and stabilizes at around 4 percentage points after twelve months since displacement.

Interestingly, Figure 5.1 shows that take-up increases among initially ineligible workers after the twelfth month following displacement. This pattern should not be interpreted as conventional non-compliance. Rather, it reflects a change in eligibility status over time. Under Eligibility Mode B, workers become eligible for the RPD program once they accumulate at least five years of participation in the formal labor market, regardless of their contribution history (see Section 2 and Figure 2.1). Given that the analytic sample is restricted to individuals displaced between their third and fourth year in the formal sector, these workers only reach the five-year threshold approximately one year after displacement. Once eligible, they take up the program at rates comparable to the original compliers, which accounts for the observed stabilization in the intent-to-treat (ITT) effect after month 12 (see Figure 5.2).

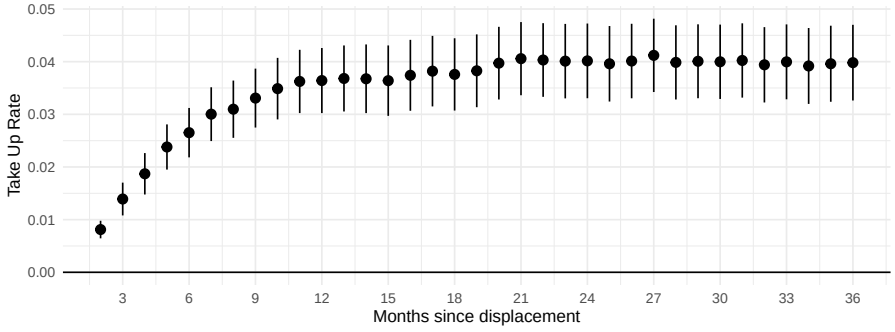
Table 5.1 confirms that the ITT effect on program take-up is highly statistically significant, albeit small in magnitude. This justifies the use of a fuzzy RDD strategy to estimate the local average treatment effect of RPD on labor market outcomes among compliers. The modest size of the first-stage effect is consistent with the institutional design of the RPD program: unlike traditional unemployment insurance schemes, RPD allows only a partial withdrawal from workers' own pension accounts. It is, by design, a self-financed benefit that internalizes the cost of unemployment support.

Figure 5.1: Eligibility Effect on Take Up



Note: This figure displays (i) sample means calculated within evenly spaced 14-day bins relative to the eligibility threshold, and (ii) the estimated regression function obtained using a local linear polynomial with a triangular kernel and optimal bandwidth selection following the method of Calonico, Cattaneo, and Titiunik (2015). The results show a discrete increase in program take-up at the eligibility threshold.

Figure 5.2: Dynamic Effect of Eligibility on Take Up



Note: This figure presents the estimated coefficients of interest from Equation 4.1. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Calonico, Cattaneo, and Titiunik (2014). Vertical lines across point estimates represent 95% confidence intervals.

Table 5.1: Eligibility Effect: First Stage

	Take up - 2 months	Take up - 3 months	Take up - 6 months	Take up - 9 months	Take up - 12 months
RPD	0.008	0.014	0.027	0.033	0.036
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Mean at cutoff - ineligible	0.00174	0.00602	0.0133	0.0187	0.0231
Observations	877,749	877,749	877,749	877,749	877,749

Note: This table reports the estimated coefficient of interest from Equation 4.1, obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the procedure of Calonico, Cattaneo, and Titiunik (2014).

5.2 Job Search Outcomes

I now turn to the effects of RPD take-up on labor market outcomes, estimated using the fuzzy regression discontinuity design described in Equation 4.2. Figure 5.3 and Table 5.2a present the dynamic treatment effects on unemployment duration and employment survival. By three months after displacement, taking up RPD increases the probability of remaining unemployed (i.e., survival in unemployment) by 31 percentage points. After 36 months, the cumulative effect on unemployment duration amounts to 36 additional weeks. These effects are statistically significant at the 5% level and align with the broader empirical literature: income support during unemployment typically prolongs joblessness. At the eligibility cutoff, RPD users experience a 70% increase in unemployment duration relative to non-eligible workers after three years. These findings underscore the potential social costs of income-substitution programs such as RPD, particularly through the lens of forgone contributions to the social security system due to extended non-employment spells.

For completeness, Appendix Figure 5.4, Figure 5.5, and Figure 5.6, along with Table 5.9, present intention-to-treat (ITT) estimates for the same outcomes, as described in Equation 4.1, as well as the corresponding RD plots. The ITT estimates show smaller effects than those reported in comparable international settings. For instance, Britto (2022) finds that a lump-sum unemployment benefit program in Brazil increased employment survival by 1.9 percentage points after three months, whereas the estimated effect of RPD is just 0.7 percentage points over the same period. Similarly, Velázquez Guadarrama (2023), using ENOE survey data, reports that eligibility to RPD increases unemployment duration by 11.8 weeks (or 82.85 days), while the ITT estimate from this study points to a much smaller increase of just 0.65 weeks over a three-year horizon. Importantly, none of the ITT estimates reported here are statistically significant at the 5% level.

Turning to employment quality, Table Table 5.2b reports the estimated effects of RPD take-up on post-displacement outcomes such as earnings and job duration. The

composition of re-employed workers appears similar around the eligibility threshold in terms of prior earnings (see column 4), suggesting no substantial selection effects. However, there is limited evidence that RPD take-up improves subsequent job quality. While the point estimate for monthly earnings in the next job suggests a sizeable increase—more than a 100% gain relative to pre-displacement wages, or approximately $9,500 MXN—this estimate is only significant at the 10% level. Moreover, the effects on next job cumulative earnings and job duration are statistically indistinguishable from zero.

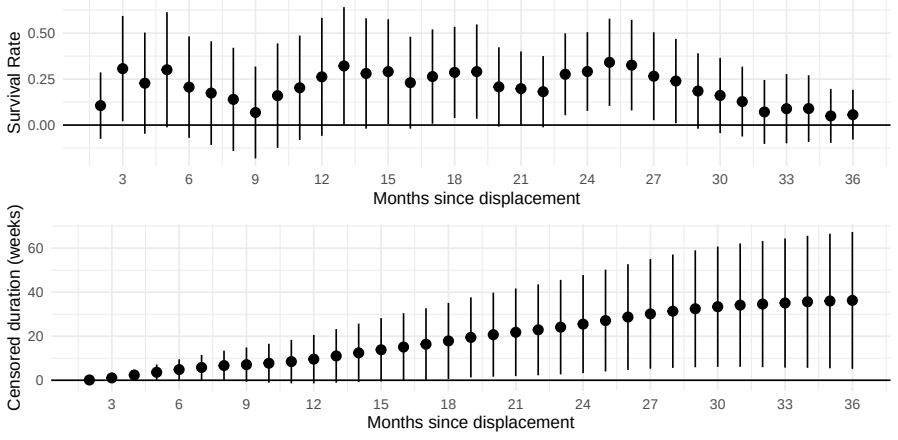
When placed in context with the existing literature, these ITT results point to a mixed picture. (?) finds a statistically significant 1.7% decline in annual earnings associated with Brazil's program, while eligibility for RPD is associated with a non-significant 4.6% increase. Velázquez Guadarrama (2023), using ENOE data, finds a smaller yet statistically significant increase of 2.47% in wage income among eligible workers. Overall, while some point estimates suggest potentially positive effects of RPD on earnings, the lack of statistical significance and the inconsistency with prior studies call for cautious interpretation.

5.3 Medium Term Effects

This section examines the medium-term labor market outcomes associated with participation in the RPD program. Table 5.3 reports the estimated effects for RPD users, computed using the fuzzy regression discontinuity design described in Equation 4.2. Although none of the estimates reach statistical significance at the 5% level, the results suggest a persistent decline in formal labor earnings over the three-year post-displacement period. This decline appears to be driven entirely by the extensive margin, as RPD recipients were employed for fewer months and, on average, reported no wage earnings.

While most effects are not statistically distinguishable from zero, they may reflect longer-lasting disruptions in formal labor market attachment. Taken together, these

Figure 5.3: Dynamic Effect of RPD on Survival and Duration of Unemployment



Note: This figure presents the estimated coefficients of interest from Equation 4.2. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Calonico, Cattaneo, and Titiunik (2014). Vertical lines across point estimates represent 95% confidence intervals.

Table 5.2: RPD Effect: Job Search Outcomes

(a) Survival and Duration of Unemployment

	Survival - 3 months	Survival - 36 months	Duration - 6 months (wks)	Duration - 36 months (wks)
RPD	0.31	0.06	4.85	36.27
	(0.15)	(0.07)	(2.37)	(15.86)
Mean at cutoff - ineligible	0.778	0.106	20.5	51.5
Observations	877,749	877,749	877,749	877,749

(b) Next Job Quality

	Monthly earnings	Total earnings	Job duration (weeks)	Prev. job earnings
RPD	9428.61	67 095.54	16.49	9626.59
	(4882.68)	(61 137.52)	(14.54)	(6055.86)
Mean at cutoff - ineligible	8,327	87,306	35.7	9,005
Observations	873,434	873,434	873,434	873,434

Note: This table reports the estimated coefficient of interest from Equation 4.2, where treatment is defined as program take-up 12 months after displacement. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Cattaneo, Idrobo, and Titiunik (2024). Currencies are expressed in 2024 Mexican Pesos.

Table 5.3: RPD Effect: Earnings, Employment and Wages over Medium Term

(a) Earnings

	Total 3 years	Year 1	Year 2	Year 3
RPD	−20 378.63	−13 116.15	−16 418.20	−3121.18
	(44 061.00)	(12 983.56)	(17 775.58)	(24 304.58)
Mean at cutoff - ineligible	148,762	31,965	54,471	62,345
Observations	877,749	877,749	877,749	877,749

(b) Months Worked

	Total 3 years	Year 1	Year 2	Year 3
RPD	−1.41	−1.95	−0.57	−0.15
	(2.65)	(1.26)	(1.12)	(1.42)
Mean at cutoff - ineligible	15.3	3.8	5.67	5.86
Observations	877,749	877,749	877,749	877,749

(c) Average Monthly Earnings

	Total 3 years	Year 1	Year 2	Year 3
RPD	903.31	634.12	−1806.90	−259.16
	(1808.21)	(1814.44)	(1954.97)	(2146.99)
Mean at cutoff - ineligible	8,903	8,012	8,959	9,902
Observations	794,688	578,415	650,874	633,586

Note: This table reports the estimated coefficient of interest from Equation 4.2, where treatment is defined as program take-up 12 months after displacement. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Cattaneo, Idrobo, and Titiunik (2024). Currencies are expressed in 2024 Mexican Pesos.

findings indicate that the RPD program not only extends the duration of unemployment but also fails to enhance post-displacement job quality or promote higher long-term earnings in the formal sector—contrary to the predictions of the more optimistic unemployment insurance literature (Acemoglu and Shimer 1999, 2000).

5.4 Additional Results: Heterogeneous Treatment Effects

In this section, I present additional results on the heterogeneity of the effect of the RPD program on labor market outcomes. I explore the heterogeneity of the treatment effect by age, wage, and exposure to COVID-19 in Table 5.4, Table 5.5, and Table 5.7, respectively.

Income. Table 5.4 explores heterogeneous effects of the RPD program by pre-displacement income, dividing the sample at the median wage in the previous job ($6,669.2 MXN). Take-up rates do not differ significantly between the above- and below-median wage groups, suggesting comparable levels of program engagement across income levels.

However, the impact on labor market outcomes varies substantially. Among workers earning below the median, the RPD significantly increases the probability of remaining unemployed within the first three months following displacement. Over a 36-month horizon, the effect on unemployment duration for this group is nearly ten times larger than for their higher-income counterparts, indicating a much more pronounced and persistent detachment from formal employment among lower-wage workers.

In terms of reemployment outcomes, RPD recipients with above-median pre-displacement wages secure jobs that pay approximately 67% more than those obtained by ineligible workers. In contrast, no statistically significant effect is found on the wages of reemployed workers in the below-median group. Over the full three-year horizon, higher-income workers exhibit a positive, albeit imprecise, impact on total earnings. Meanwhile, lower-income workers experience an average earnings loss of approximately $40,000 MXN—driven by prolonged unemployment and the absence of wage gains upon reemployment—although this estimate is not statistically distinguishable from zero.

These findings highlight the importance of accounting for income heterogeneity in program design. In the case of the RPD, the structure of benefits may inadvertently reinforce existing inequalities—offering greater long-term advantages to relatively better-off workers, while leaving lower-wage workers worse off in both employment duration and earnings trajectories.

Age. Table 5.5 examines heterogeneous effects of the RPD program by age at the time of displacement, dividing the sample at the median age of 22.9 years. As with income, take-up rates do not differ significantly between younger and older workers, indicating comparable engagement with the program across age groups.

The effects on unemployment duration are not statistically significant for either group over the short (3-month) or medium (36-month) horizons. However, point estimates suggest meaningful differences in magnitude. By three months, younger workers exhibit a 23 percentage point increase in the probability of remaining unemployed, compared to only a 10 percentage point increase for older workers—more than twice the effect. This disparity widens substantially over time: after 36 months, the cumulative increase in unemployment duration is 35 weeks for younger workers, compared to just 3 weeks for older workers—an effect nearly ten times larger.

In contrast, there is no clear evidence of differential effects on post-displacement job quality. All estimates related to wages and job duration in the next job are statistically indistinguishable from zero for both age groups.

Over the full three-year horizon, younger workers appear to experience greater earnings losses. On average, they earned approximately $20,000 MXN less than their counterparts who were not eligible for RPD, compared to a $9,000 MXN loss among older workers. These estimates, however, are imprecise and should be interpreted with caution.

Taken together, these findings suggest that age is an important dimension of heterogeneity in the effects of unemployment assistance programs. In the case of the RPD, younger workers may face more persistent labor market disruptions, with weaker

labor market attachement, potentially compounding early-career vulnerabilities.

Gender. Table 5.6 examines the heterogeneous effects of the RPD program by gender. Panel A presents results for males, while Panel B reports estimates for females. Take-up rates are similar across groups, with no statistically significant gender differences in program participation 12 months after displacement.

However, the effects on labor market outcomes diverge markedly by gender. Among males, RPD take-up increases the probability of remaining unemployed three months after displacement by 40 percentage points. This effect persists over time, resulting in a cumulative increase in unemployment duration of 49 weeks over the subsequent three years—an effect that is both large and statistically significant. In contrast, female recipients exhibit no significant change in short-term survival or long-term unemployment duration. The point estimate for unemployment duration is actually negative (−8.7 weeks), but imprecisely estimated.

Differences in earnings outcomes are similarly pronounced. Male RPD recipients experience large, positive effects on monthly earnings in their next job—approximately $11,252 MXN higher than ineligible males—and a cumulative three-year earnings gain of $1,066 MXN. These estimates, while imprecise, suggest a potential improvement in job quality among men. In contrast, female RPD recipients see no significant gain in monthly earnings and suffer an average three-year total earnings loss of $16,126 MXN. This reduction in earnings is likely driven by delayed labor market reentry and the absence of wage gains upon reemployment.

Interestingly, the margin of adjustment differs across genders. Male recipients work 2.7 fewer months over the three-year period, while female recipients actually work 1.5 months more. Yet, the increase in work months among women does not translate into earnings gains, possibly reflecting reemployment in lower-wage or more precarious jobs.

Taken together, these findings highlight significant gender disparities in the labor market effects of the RPD program. While male recipients face longer unemploy-

ment durations, they appear to recover through access to better-paying jobs. Female recipients, in contrast, do not experience improvements in job quality and incur significant earnings losses. These results underscore the need for gender-sensitive design in unemployment support policies, as uniform benefit structures may yield unequal labor market outcomes.

Exposure to COVID-19. Table 5.7 explores heterogeneous effects of the RPD program based on whether individuals were displaced before or after the onset of the COVID-19 pandemic. Panel A includes workers who were displaced before March 2019, and thus had at least a full year of unemployment before the pandemic hit in March 2020. These individuals are considered to have no exposure to pandemic-related labor market disruptions during their initial post-displacement period. Panel B, by contrast, focuses on individuals displaced after March 2020, who faced full exposure to the labor market conditions created by the pandemic.

First-stage estimates show that take-up rates are slightly lower among post-pandemic displacees (3%) than among pre-pandemic ones (4%), although the difference is modest. However, labor market outcomes diverge sharply across cohorts.

Among workers with no exposure to the pandemic (Panel A), RPD take-up leads to substantial and significant increases in unemployment duration. The probability of remaining unemployed at 3 months rises by 37 percentage points, and cumulative unemployment duration increases by over 52 weeks—essentially one additional year of joblessness. Despite these adverse effects on employment continuity, there is a modest increase in monthly earnings in the next job (12,416 MXN), although total earnings over the three-year horizon fall by 8,539 MXN, suggesting that the gains in job quality do not fully offset the costs of prolonged unemployment.

In contrast, among workers fully exposed to the COVID-19 crisis (Panel B), RPD take-up is associated with much weaker effects on labor market behavior. The effect on 3-month survival is negligible (1 pp), and the cumulative increase in unemployment

Table 5.4: RPD Effect: Heterogeneity over Wage

(a) Below Median Wage

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	0.38	46.16	15.80	4737.20	−39 836.31	−3.64	−60.70
	(0.00)	(0.14)	(17.22)	(21.26)	(5196.56)	(38 497.50)	(3.41)	(1467.20)
Mean at cutoff - ineligible	0.0121	0.797	53.8	36.2	7,269	119,239	14.6	7,617
Observations	438,875	438,875	438,875	436,925	436,925	438,875	438,875	393,137

(b) Above Median Wage

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	−0.05	−4.67	17.05	6416.72	17 516.18	0.66	2081.56
	(0.00)	(0.14)	(15.71)	(21.47)	(3044.42)	(73 028.49)	(4.21)	(2808.53)
Mean at cutoff - ineligible	0.027	0.763	49.7	35.2	9,437	178,004	16	10,180
Observations	438,874	438,874	438,874	436,509	436,509	438,874	438,874	401,551

Note: This table reports the estimated coefficient of interest from Equation 4.1, obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the procedure of Calonico, Cattaneo, and Titiunik (2014). Panel A reports results for individuals whose earnings in their previous job were below the sample median wage ($6,669.2), while Panel B reports results for those above the median. All monetary values are expressed in 2024 Mexican Pesos.

duration is only 22 weeks. Strikingly, reemployment outcomes for this group appear worse: RPD recipients experience a decline in job duration (−9.4 weeks) and a relatively small increase in next-job earnings (5,011 MXN). However, despite weaker job quality indicators, total earnings over the three-year period rise by 37,277 MXN—likely due to higher reemployment rates and a labor market rebound post-pandemic.

These results suggest that the macroeconomic context at the time of displacement plays a crucial role in shaping the effects of unemployment assistance. While RPD recipients displaced before COVID-19 faced steep penalties in terms of extended non-employment and modest earnings gains, those displaced during the pandemic experienced smaller disruptions and more favorable cumulative earnings trajectories. Importantly, this contrast highlights the interaction between individual-level support programs and broader labor market conditions: a well-timed benefit may facilitate recovery in a tightening labor market, while the same benefit in a more stable environment may prolong detachment without improving long-term outcomes.

Table 5.5: RPD Effect: Heterogeneity over Age

(a) Below Median Age

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	0.23	35.16	−4.43	60.02	−20 965.39	−2.11	357.61
	(0.00)	(0.17)	(17.98)	(18.77)	(1805.32)	(42 939.90)	(3.82)	(1261.62)
Mean at cutoff - ineligible	0.0162	0.767	47.7	33.4	7,747	141,633	15.9	8,327
Observations	423,857	423,857	423,857	422,155	422,155	423,857	423,857	388,767

(b) Above Median Age

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	0.10	3.10	28.18	9759.34	−9224.03	−1.35	114.08
	(0.00)	(0.12)	(16.58)	(19.68)	(6752.33)	(59 252.06)	(3.75)	(2246.94)
Mean at cutoff - ineligible	0.0214	0.793	56.1	38.4	9,032	157,792	14.9	9,601
Observations	423,857	423,857	423,857	421,318	421,318	423,857	423,857	378,703

Note: This table reports the estimated coefficient of interest from Equation 4.2, where treatment is defined as program take-up 12 months after displacement. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Cattaneo, Idrobo, and Titiunik (2024). Panel A shows the results for the below median age group (22.3 years), while panel B shows the results for the above median age group. Currencies are expressed in 2024 Mexican Pesos.

Table 5.6: RPD Effect: Heterogeneity over Gender

(a) Males

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	0.40	49.08	14.55	11 252.23	3074.41	−2.73	1066.12
	(0.00)	(0.14)	(18.27)	(16.88)	(6816.09)	(52 415.38)	(3.08)	(1827.85)
Mean at cutoff - ineligible	0.0136	0.753	47	33.3	8,288	155,075	16	8,965
Observations	546,893	546,893	546,893	544,534	544,534	546,893	546,893	502,724

(b) Females

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	−0.17	−8.71	15.66	1994.00	−16 126.79	1.51	1194.77
	(0.01)	(0.14)	(17.36)	(22.98)	(2160.69)	(59 431.83)	(3.68)	(2252.93)
Mean at cutoff - ineligible	0.0281	0.823	59.3	39.7	8,403	137,174	14.1	8,794
Observations	330,856	330,856	330,856	328,900	328,900	330,856	330,856	291,964

Note: This table reports the estimated coefficient of interest from Equation 4.2, where treatment is defined as program take-up 12 months after displacement. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Cattaneo, Idrobo, and Titiunik (2024). Panel A shows the results for the male group, while panel B shows the results for the female group. Currencies are expressed in 2024 Mexican Pesos.

Table 5.7: RPD Effect: Heterogeneity over Exposure to COVID-19

(a) No Exposure (Displaced before March 2019)

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.04	0.37	52.76	18.01	12 416.29	−8539.55	−2.92	1241.50
	(0.00)	(0.19)	(21.75)	(26.88)	(8273.95)	(55 380.27)	(4.25)	(2301.33)
Mean at cutoff - ineligible	0.0113	0.775	50.7	38.3	8,003	143,580	15.5	8,497
Observations	561,937	561,937	561,937	560,491	560,491	561,937	561,937	503,428

(b) Full Exposure (Displaced after March 2020)

	Take up (First stage)	Survival - 3 months	Duration - 36 months (wks)	Job duration (wks)	Monthly Earnings	Total Earnings - 3 years	Months Worked - 3 years	Monthly Earnings - 3 years
RPD	0.03	0.01	21.99	−9.39	5010.11	37 277.66	−2.31	4689.68
	(0.01)	(0.20)	(25.68)	(19.50)	(2769.82)	(69 127.25)	(4.50)	(3634.36)
Mean at cutoff - ineligible	0.0491	0.78	48.4	30.1	9,004	169,405	16.1	9,879
Observations	156,826	156,826	156,826	155,207	155,207	156,826	156,826	148,663

Note: This table reports the estimated coefficient of interest from Equation 4.2, where treatment is defined as program take-up 12 months after displacement. Estimates are obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the method of Cattaneo, Idrobo, and Titiunik (2024). Currencies are expressed in 2024 Mexican Pesos.

Conclusions

This thesis evaluates the labor market effects of Mexico's *Retiro Parcial por Desempleo* (RPD), a policy that allows displaced formal sector workers to make early withdrawals from their pension savings. Leveraging a discontinuity at two years of social security contributions, I implement a fuzzy regression discontinuity design to estimate the local average treatment effect of program take-up on labor market outcomes.

The results show that RPD take-up substantially increases unemployment duration, with significant effects observable as early as three months after displacement and persisting over a three-year horizon. The increase in non-employment is large and robust, suggesting that liquidity provision through the RPD distorts job search incentives. However, there is no consistent evidence that RPD take-up improves job quality or medium-term formal labor income. While some groups—particularly higher-income or male workers—experience modest earnings gains upon reemployment, these are offset by longer unemployment spells. For lower-income, younger, and female workers, RPD take-up is associated with longer unemployment and cumulative earnings losses, although some of these estimates are imprecise.

Take-up rates remain low, and the first-stage estimates are small but precise, indicating that the program successfully targets a narrow set of workers. Notably, heterogeneity analysis reveals that labor market context plays a crucial role: among

workers displaced during the COVID-19 pandemic, RPD take-up did not extend unemployment duration and is even associated with higher total earnings. These findings suggest that the RPD may serve a countercyclical role, particularly in times of systemic labor market disruption.

Several limitations of the study should be acknowledged. First, the fuzzy RD design identifies local effects for compliers—those induced to take up the program by marginal eligibility—limiting external validity. Second, the administrative data do not capture informal employment or non-wage welfare outcomes, potentially understating total adjustment margins. Third, despite large point estimates, wide confidence intervals in the second stage warrant cautious interpretation.

My results underscore the institutional tensions in Mexico's fragmented income protection system. Severance pay, the de jure main income substitute, is often inaccessible due to weak legal enforcement (Sadka, Seira, and Woodruff 2024; Degetau 2023). In this context, the RPD emerges as a second-best option: it is self-financed, administratively simple, and unlikely to induce fraud. Yet this model shifts the burden of income protection onto workers themselves—especially those with fewer savings—thereby reinforcing labor market inequalities.

A more ambitious policy agenda would involve developing a publicly funded unemployment insurance system that ensures risk-sharing beyond individual savings. However, such a system would require overcoming institutional barriers: effective delivery depends not only on eligibility design but also on credible enforcement and administrative capacity.

The findings point to several promising directions for future research. First, aggregate behavioral effects—such as changes in employer hiring or firing practices—remain an open question. Second, the observed heterogeneity by income, age, gender, and macroeconomic context calls for richer models of household and labor market behavior under liquidity constraints. Finally, the intersection between legal institutions and social protection policy in low-capacity states remains an underexplored

but vital area for understanding the design and implementation of unemployment assistance in developing countries.

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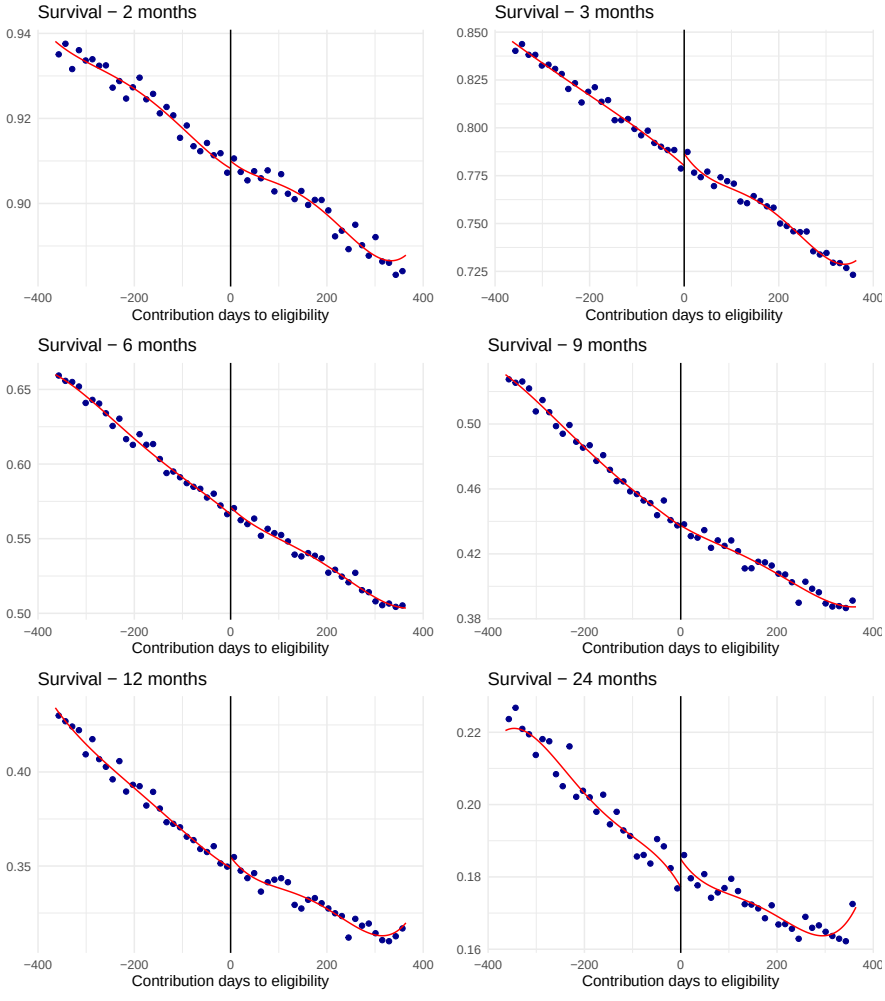
Appendix

Table 5.8: Summary Statistics

Variable	n	Mean	Median	Std.Dev.	Min	Max
Covariates						
Age at displacement date	847,714	24.7	22.9	5.5	18.0	65.0
Birth date	847,714	1,993.4	1,994.6	5.7	1,948.5	2,003.5
Date worker began working	877,749	2,014.6	2,014.9	2.1	2,010.0	2,018.0
Days since account opened at displacement date	877,749	1,264.5	1,259.0	103.1	1,095.0	1,460.0
Displacement date	877,749	2,018.0	2,018.3	2.1	2,013.0	2,022.0
Female	877,749	0.4	0.0	0.5	0.0	1.0
No CURP	877,749	0.0	0.0	0.2	0.0	1.0
Previous Job						
Duration of previous job (weeks)	877,749	27.7	16.4	29.7	0.1	156.3
Monthly earnings in previous job (2024 MXN)	877,749	9,095.4	6,669.2	19,424.5	1,631.2	3,634,843.3
Total earnings in previous job (2024 MXN)	877,749	54,235.8	25,769.4	82,490.9	107.6	2,894,843.6
Partial Withdrawal (RPD)						
Amount withdrawn (2024 MXN)	62,632	7,690.6	6,637.5	4,365.9	324.2	134,480.5
Contributed weeks at take up date	62,632	125.9	130.0	29.1	0.0	1,273.0
Days to take up	62,632	440.8	228.0	477.2	46.0	2,892.0
Days withdrawn	62,632	318.4	301.0	126.8	0.0	4,075.0
Job Search Outcomes						
Survival out of formal employment after 3 months	877,749	0.8	1.0	0.4	0.0	1.0
Survival out of formal employment after 36 months	877,749	0.1	0.0	0.3	0.0	1.0
Weeks out of formal employment censored at 3 months	877,749	12.1	12.9	1.8	6.3	12.9
Weeks out of formal employment censored at 36 months	877,749	53.1	32.6	49.1	6.3	154.3
Next Job Quality						
Duration of next job (weeks)	873,434	36.8	14.7	59.3	0.1	611.3
Monthly earnings in next job (2024 MXN)	873,434	8,461.7	6,948.8	6,643.8	1,599.3	2,171,267.3
Total earnings in next job (2024 MXN)	873,434	91,888.4	23,412.3	245,995.0	105.4	10,500,054.5
Medium Term Outcomes (Over 3 Years)						
Average monthly earnings after displacement (2024 MXN)	794,688	9,007.6	7,515.1	5,841.1	3,163.5	84,996.0
Months worked after displacement	877,749	15.2	14.1	11.5	0.0	89.5
Total earnings after displacement (2024 MXN)	877,749	149,523.5	102,486.9	180,264.6	0.0	4,035,597.8

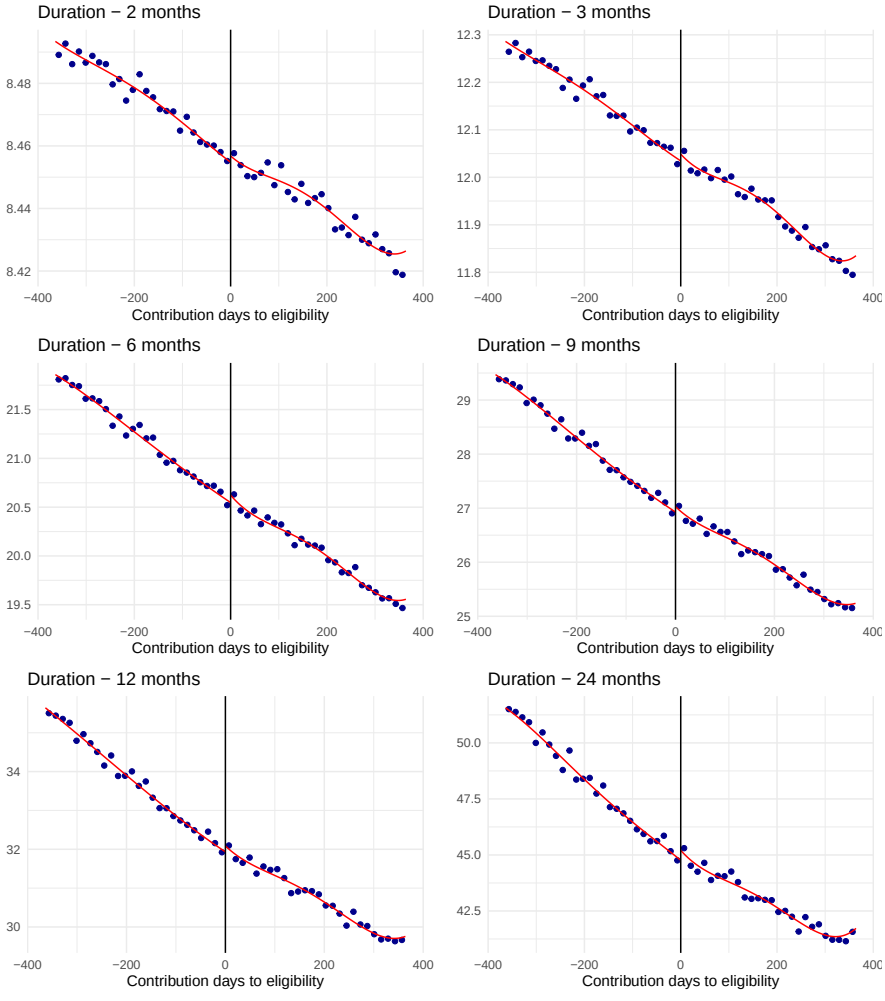
Note: This table reports summary statistics for the variables used in the analysis. The statistics are based on the final sample used for estimation.

Figure 5.4: Eligibility Effect: Survival in Unemployment



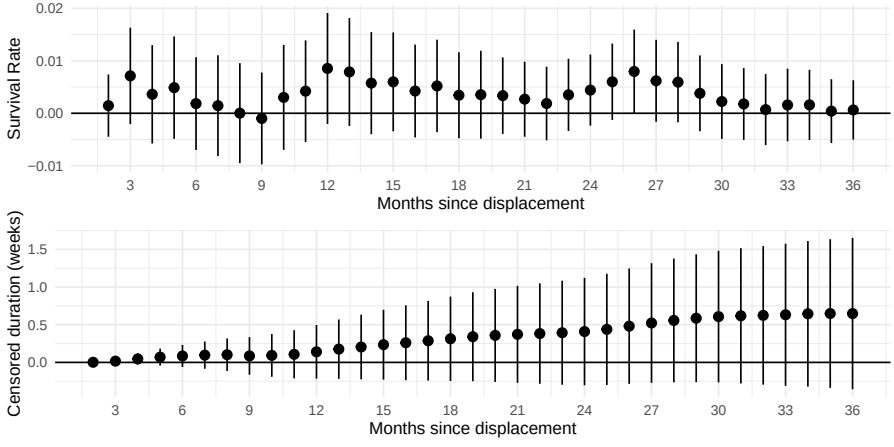
Note: his figure shows (*i*) sample means for evenly-spaced 14-day bins relative to the eligibility threshold, and (*ii*) the underlying regression function estimated using a triangular kernel, optimal bandwidth and a linear local polynomial using Calonico, Cattaneo, and Titiunik (2015).

Figure 5.5: Eligibility Effect: Duration of Unemployment



Note: his figure shows (*i*) sample means for evenly-spaced 14-day bins relative to the eligibility threshold, and (*ii*) the underlying regression function estimated using a triangular kernel, optimal bandwidth and a linear local polynomial using Calonico, Cattaneo, and Titiunik (2015).

Figure 5.6: Dynamic Effect of Eligibility on Survival and Duration of Unemployment



Note: This figure shows the estimated coefficient of interest in Equation 4.1 . The estimates are obtained using a triangular kernel, optimal bandwidth and a linear local polynomial. Bias corrected estimates and robust standard errors computed using Calonico, Cattaneo, and Titiunik (2014). 95 percent confidence intervals.

Table 5.9: Eligibility Effect: Job Search Outcomes

(a) Survival and Duration of Unemployment

	Survival - 3 months	Survival - 36 months	Duration (wks) - 6 months	Duration (wks) - 36 months
RPD	0.007	0.001	0.085	0.648
	(0.005)	(0.003)	(0.075)	(0.513)
Mean at cutoff - ineligible	0.78	0.108	20.5	51.9
Observations	877,749	877,749	877,749	877,749

(b) Next Job Quality

	Monthly earnings (2024 MXN)	Total earnings (2024 MXN)	Job duration (weeks)	Prev. job earnings
RPD	283.48	1469.40	0.56	276.07
	(163.04)	(2178.74)	(0.62)	(215.71)
Mean at cutoff - ineligible	8,333	87,703	35.7	8,978
Observations	873,434	873,434	873,434	873,434

Note: This table reports the estimated coefficient of interest from Equation 4.1, obtained using a local linear regression with a triangular kernel and optimal bandwidth selection. Bias-corrected point estimates and robust standard errors are computed following the procedure of Calonico, Cattaneo, and Titiunik (2014).