

Strategic Formal Layoffs: Unemployment Insurance and Informal Labor Markets ^{*}

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Abstract

Exploiting an unemployment insurance (UI) reform in Brazil, we study incentive effects of UI in the presence of informal labor markets. We find that eligibility for UI benefits increases formal layoffs by eleven percent. Most of the additional layoffs are related to workers transitioning to informal employment. We further document formal layoff and recall patterns consistent with rent extraction from the UI system. Workers are laid off as they become eligible for UI benefits and recalled when benefits cease. These patterns are stronger for industries and municipalities with a high degree of labor market informality.

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1 Introduction

Experiencing shocks to labor income is one of the most salient risks faced by households (Rothstein and Valletta 2017). To allay adverse effects of job loss on household income, government-mandated unemployment insurance (UI) programs have been in place in developed countries for decades. However, adverse labor supply effects of UI generate a trade-off between providing insurance against income shocks and distorting labor supply.¹

More recently, the implementation of government-mandated UI programs has been spreading to an increasing number of mid-income and developing countries (Holzmann et al. 2011; ILO 2017), for which we understand little about how their specific labor market characteristics interact with incentive effects of UI. A prominent feature of labor markets in mid-income and developing countries is a high degree of informality, which may interact with UI in unique ways. For example, informal labor markets provide workers with the opportunity to receive UI benefits while being employed informally.

To shed more light on the incentive effects of UI in the presence of large informal labor markets, we exploit a UI reform in Brazil that had a discontinuous effect depending on workers' tenure. As a heterogeneous mid-income country with ample variation in labor market informality, Brazil constitutes an ideal setting for our analysis. We find that eligibility for UI benefits increases formal layoffs by 11.11 percent. Most of the additional formal layoffs are related to workers flowing into informal employment. These patterns occur primarily in industries and municipalities with a high degree of labor market informality.

The UI reform was unexpectedly announced on December 29, 2014 and implemented on March 1, 2015. Prior to the reform, all workers with tenure of 6 months are eligible for UI benefits. After the reform, workers applying for benefits for the first (second) time require 18 (12) months of formal employment during the previous 24 (16) months to be eligible for benefits. Thus, a subset of workers with tenure between 6 and 18 (12) months loses eligibility for UI benefits after the reform. The design of the reform motivates our main identification strategy: a difference-in-difference methodology, in which we compare changes in employment and unemployment patterns around the reform for workers above and below the six-month threshold. The fact that workers with two or more prior UI benefits spells are unaffected by the reform allows us to augment our estimation to a triple-difference methodology, comparing workers around the six-month threshold who are affected by the reform to workers around the threshold who are unaffected by the reform. This rules out confounding factors that

¹E.g., Solon (1979), Moffitt (1985), Katz and Meyer (1990a), Meyer (1990, 1995), Card and Levine (2000), Meyer and Mok (2007), Card et al. (2015a), Farber, Rothstein, and Valletta (2015), Johnston and Mas (2018), and Landais (2015) for the U.S., and Card, Chetty, and Weber (2007), Lalive (2008), Schmieder, von Wachter, and Bender (2012, 2016), and Card et al. (2015b) for Western Europe.

could differentially affect workers around the threshold, for example macro-economic shocks, seasonal effects, or changes in incentives for workers below the threshold.

We start our analysis by examining how eligibility for UI benefits affects layoff rates. Throughout the paper, we define layoffs as formal layoffs upon which workers become unemployed or work informally. We find that layoffs drop by 0.43 percentage points (11.11 percent) for affected workers above the six-month threshold, who lose eligibility for UI benefits after the reform, compared with workers below the six-month threshold, who were never eligible for UI benefits, relative to the same difference for unaffected workers.² Based on a difference-in-bunching estimation, we further find that layoff rates of workers below the six-month threshold are unaffected by the reform. This suggests that the additional layoffs above the threshold constitute additional layoffs due to eligibility for UI benefits rather than simply a change in layoff timing. The results are virtually unaffected by controlling for age-month and education-month fixed effects, and for a reduced sample of workers with tenure of four to seven months in the four months around the reform. Moreover, the results are unaffected by including expired temporary contracts in the definition of layoffs.

Informal labor markets provide a unique alternative to formal employment in the light of UI. Workers can receive benefits while being employed informally. We find that higher layoff rates for workers eligible for UI benefits correlate with labor market informality. A ten percentage point higher share of informal employment in a given industry or municipality is associated with an about 0.1 percentage points (2.6 percent) higher layoff rate for workers eligible for UI benefits. The link between higher layoff rates and informal labor markets is corroborated by survey evidence showing that higher layoff rates of workers eligible for UI benefits are mostly related to workers transitioning from formal to informal employment. When workers with more than six months' tenure lose eligibility for UI benefits after the reform, they become less likely to transition to informal employment upon formal layoff. Our results suggest that 94 percent of the additional layoffs due to eligibility for UI benefits are related to formal-to-informal employment transitions (see Section 3.2 for details).

Higher layoff rates may be driven by various mechanisms. Workers may be more likely to shirk when their outside option is to receive UI benefits. Firms facing labor demand fluctuations may (temporarily) lay off workers who qualify for UI benefits, since they are less likely to search for alternative employment (Katz 1986; Jurajda 2002). Alternatively, firms and workers may time formal unemployment spells to coincide with eligibility for UI benefits, to extract rents from the UI system (Feldstein 1976; Baily 1977), either through direct collusion, or firms may establish a reputation for laying off workers when they are

²Carvalho, Corbi, and Narita (2018) document similar patterns for an extended period around the reform.

eligible for UI benefits to signal their intentions in a more subtle way (Christofides and McKenna 1996).

We provide evidence consistent with firm-worker coordination to extract rents from the UI system explaining at least part of the additional layoffs. Specifically, we show that layoffs and recalls are timed to coincide with eligibility for UI benefits. Workers who are laid off as they become eligible for UI benefits are more likely to be recalled by their previous employer after they exhaust their UI benefits. While workers' incentives to shirk are also stronger when they are eligible for UI benefits, it is hard to argue that firms are more likely to recall shirking workers.

Finally, we provide (indirect) evidence that firms continue to informally employ workers who qualify for UI benefits after laying them off formally. When firms lay off a worker, they often hire a new worker as a replacement. However, if firms continue to employ a laid off worker informally, they do not need to hire a replacement. Consistent with this conjecture, we find that firms are 0.72 percentage points (1.23 percent) more likely to hire a replacement worker after laying off a worker who is eligible for UI benefits, compared with after laying off a worker who is ineligible for UI benefits.

On examining changes in salaries around the reform, we find that salaries increase 0.80 percent more for workers affected by the reform. Within the group of affected workers salaries increase 0.30 percent more for workers who were previously laid off just as they qualified for UI benefits. This suggests that workers who engage in strategic timing of unemployment spells to coincide with eligibility for UI benefits before the reform accept lower equilibrium salaries, consistent with workers passing on rents extracted from the UI system to firms through lower salaries (see Section 3.3 for details).

The main contribution of our paper is to shed light on incentive effects of UI in the presence of informal labor markets. Our findings suggest that UI leads to higher layoff rates in the presence of informal labor markets mostly due to transitions to informal employment. We estimate that about 11 percent of UI payments for workers laid off with a tenure of 6 to 17 months are paid to strategically unemployed workers, whose layoffs are determined by their eligibility for UI benefits rather than demand shocks.

Evidence on the interaction of UI and labor market informality is scant. The closest paper to ours is Gerard and Gonzaga (2021), who examine the effects of UI on formal reemployment rates for varying levels of labor market informality by exploiting variation in UI benefits duration. They find that the marginal effect of an additional month of benefits duration on search intensities is weak leading them to conclude that efficiency costs of social programs are lower for more informal labor markets. Our findings that UI has a stronger

effect on layoff rates in the presence of informal labor markets challenge this view.³ In addition, a potential reason for why Gerard and Gonzaga (2021) find lower moral hazard effects at the reemployment margin could be that their research design requires them to focus on a subset of workers that does not exhibit moral hazard at the layoff margin. Specifically, Gerard and Gonzaga (2021) focus on workers laid off with tenure around 24 months where, different from the six-month tenure threshold, the distribution of layoff rates is smooth. Thus, their analysis applies to a selected subset of workers for whom moral hazard effects may be weaker.⁴

Our results also relate to the literature on layoffs more broadly. Most evidence on higher employment outflow for workers eligible for UI benefits is from Canada where individuals who quit their job can qualify for UI benefits (Christofides and McKenna 1995, 1996; Green and Riddell 1997; Baker and Rea 1998; Green and Sargent 1998). Existing studies provide mixed insights regarding the effects of different aspects of UI design on layoff rates. Anderson and Meyer (1997b) find that benefit levels but not duration impact UI take-up. Winter-Ebmer (2003) documents that layoffs are higher when benefits duration is extended. Jurajda (2002) finds that higher benefits levels have no effect on layoff rates. Recent studies from Austria and Germany find no or weak evidence of layoff timing with respect to UI eligibility (Card, Chetty, and Weber 2007; Schmieder, von Wachter, and Bender 2012). This is consistent with our findings that informal labor markets play an important role in generating higher layoff rates for workers eligible for UI benefits. Our cross-sectional estimates imply that for levels of labor market informality observed in Austria and Germany (1.4 - 2.9 percent (European Social Survey, 2014)), less than 3 percent of layoffs occur as a consequence of workers' eligibility for UI benefits. Nevertheless, our findings may be relevant for sectors with high levels of informality even in developed countries.

Finally, our results relate to the literature on temporary layoffs and recalls. Temporary layoffs account for a high fraction of employment outflow (Feldstein 1978; Katz and Meyer 1990b) with high recall rates for laid off workers (Nekoei and Weber 2015; Fujita and Moscarini 2017), in particular with imperfect experience rating (Feldstein 1978; Topel 1983, 1985; Anderson and Meyer 1994; Card and Levine 1994). We complement these studies by showing that firms and workers extract rents from the UI system through temporary layoffs when workers can be employed informally. This makes temporary layoffs an important concern in designing UI systems in the presence of large informal labor markets.

³Other recent studies that analyze UI programs in middle-income and developing countries (Gasparini, Haimovich, and Olivieri 2009; Gonzalez-Rozada, Ronconi, and Ruffo 2011; Amarante, Arim, and Dean 2013) do not directly examine how incentive effects of UI interact with informal labor markets.

⁴Moreover, Gerard and Gonzaga (2021) calculate the insurance value of UI from consumption patterns, which has been shown to be challenging (Landais and Spinnewijn, forthcoming).

2 Institutional Background and Data

This section provides information about Brazil's UI system, the UI reform, and the data used for the empirical analysis in the paper.

2.1 Unemployment Insurance in Brazil

In Brazil, every formal worker holds a working card, which employers are required to sign whenever a worker is hired, promoted, or dismissed. This information is reported to the Ministry of Economics every year. Formal employees are entitled to a minimum wage. Payroll taxes amount to 20 percent of the formal wage to finance the public pension system, plus 8.5 percent for the worker's seniority account (FGTS). All formal employees in the private sector participate in the UI system. Funding for the UI system comes from the social integration program (PIS), mainly financed through a 0.65 percent tax on annual firm sales.

To be eligible for UI benefits, workers cannot simply quit, but need to be laid off by their employer. UI benefits are paid for 3 to 5 months, depending on the worker's time in formal employment. Three payments are made if a worker was employed for between 6 and 11 months in the last 36 months, four payments are made if a worker was employed for between 12 and 23 months in the last 36 months, and five payments are made if a worker was employed for at least 24 months in the last 36 months. In 2015, the monthly payment ranges from 1 to 1.76 minimum salaries, depending on the average pre-layoff salary. The UI system does not feature a direct experience rating mechanism. If a firm lays off a worker, it must pay an additional 50 percent of the total contribution that has accumulated in the employee's FGTS. This cost of laying off a worker amounts to 8-19 percent of the expected benefits payments to the worker, depending on the pre-layoff salary. 80 percent of the penalty is directly paid to the worker, and 20 percent goes towards funding the UI system. Thus, implied experience rating, after accounting for payments that remain within the firm-worker relationship, is about 1.6-3.8 percent of the UI benefits paid to the worker through the UI system. Firing workers requires a valid legal justification such as absence from work, negligence or other proven misconduct. Firing does not lead to penalties, and fired workers do not qualify for UI. However, firings are rare (3.5 percent of all dismissals), since the burden of providing evidence is high and judges tend to rule in favor of employees.

2.2 Unemployment Insurance Reform

To be eligible for UI benefits prior to March 1, 2015, a worker had to be employed over a consecutive period of at least 6 months prior to layoff, not be earning other labor income, and

not have successfully applied for UI benefits during the previous 16 months. On December 29, 2014, the parliament announced a provisional measure that tightened eligibility criteria for UI benefits.⁵ This provisional measure was formally enacted the next day. The new criteria were set to be enforced from March 1, 2015. Both the sudden implementation and the content of the new law were unexpected.⁶ The main reason for the quick implementation was attempts on the government's part to reduce the growing budget deficit. The size and duration of UI benefits were not altered and employers' UI contributions were unaffected. Thus, the reform had no direct effect on employers' demand for formal labor.

The reform affected about 60 percent of workers. These were workers with less than two successful prior applications for UI benefits. To be eligible for UI benefits after the reform, a longer employment history was required. Specifically, workers who were applying for the first time required documented employment of at least 18 months during the 24 months prior to layoff. Workers who were applying for the second time required 12 months of formal employment during the last 16 months (see Figure 1). The provisional measure was applied from March 2015 and became law in July 2015 with some adjustments.⁷

2.3 Data

We use data from RAIS (Relação Anual de Informações Sociais), a large restricted-access matched employee-employer administrative dataset from Brazil. The RAIS database records information on all formally employed workers in a given year and is maintained by the Ministry of Economics. All formally-registered firms in Brazil are legally required to report annual information on each worker that the firm employs. RAIS includes detailed information on the employer (tax number, sector of activity, establishment size, geographical location), the employee (social security number, age, gender, education), and the employment relationship (salary, tenure, type of employment, hiring date, layoff date, etc.). We use data from RAIS for the 2013–2016 period. By the end of 2014, the database covers about 50 million formal employees. We exclude all public sector employees, since they do not participate in the UI program.

We combine data from RAIS with information on the number of previous unemployment

⁵We provide a detailed summary of the parts of the law governing eligibility criteria before and after the reform in Appendix A.

⁶Estadao Politica, December 29, 2014, “Força Sindical nega ter sido consultada sobre ajuste em benefícios.” Doornik et al. (2020) show that firms whose workers benefit more from a more generous UI system experience a greater drop in their stock price following the announcement on December 29.

⁷The adjusted requirements from July 2015 required a first-time applicant to have at least 12 months of employment in the last 18 months. A second-time applicant had to have at least 9 months of employment in the last 12 months.

spells maintained by the Ministry of Economics. This information is crucial, as the reform only applies to workers with fewer than two past UI benefits spells. In addition, we use information on the location of firms (municipality), firms' two-digit industry classification (National Classification of Economic Activities), and information on workers' occupations (Classificação Brasileira de Ocupações).

We also use data from the monthly employment survey Pesquisa Mensal de Emprego (PME).⁸ The survey interviews 44,189 individuals in six metropolitan areas (Recife, Salvador, Belo Horizonte, Rio de Janeiro, Sao Paulo, and Porto Alegre). Households are interviewed over two separate four-month periods that are eight months apart. The main advantage of the PME is that based on an algorithm developed in Ribas and Soares (2008) individuals can be assigned a unique identifier giving the survey a panel structure. The survey provides information on whether a worker is formally employed, informally employed, or unemployed.

To exploit cross-sectional variation in labor market informality, we link RAIS with information on labor market informality from the 2010 Brazilian census. The census asks whether or not an individual has a job and whether or not the job is formal, and reports labor market informality shares for twenty different industry classifications (see Table 1). While 66 percent of domestic services employees work informally, in the most formal industry, electricity and gas, only 5.6 percent employees work informally. In terms of geographic variation, most municipalities fall within the range of 20 to 70 percent labor market informality, with somewhat higher average informality in the north (Figure 2).

3 Empirical Analysis

This section describes our empirical analysis. Our empirical design exploits two sources of variation. First, we exploit that a discontinuity in eligibility for UI benefits at six months' tenure is eliminated by the UI reform. The post-reform period provides us with a counterfactual for when workers above and below six months' tenure are ineligible for UI benefits. Second, the reform does not apply to workers with at least two previous UI benefits spells. This provides a counterfactual of workers for whom eligibility rules do not change around the reform, which we can exploit in a triple-difference estimation.

⁸We are thankful to Data Zoom, developed by the Department of Economics at PUC-Rio, for providing the codes for accessing IBGE microdata.

3.1 Descriptive Statistics

For our main analysis, we focus on workers with consecutive formal employment up to eleven months in the two years around the UI reform (March 2014 to February 2016). Observable characteristics in Table 2 suggest that workers with tenure above and below six months are of similar age, earn a similar salary, have a similar share of male and university educated individuals, and work for similarly sized firms and in similar industries.

3.2 Layoffs

Difference-in-Difference Estimation We start our analysis by examining layoff patterns around the reform in Figure 3. For each tenure level, we generate a difference-in-difference estimate. Specifically, we estimate

$$Layoff_{it} = \alpha_{t'} + \alpha_{aff,cal} + \delta \cdot Affected_i * Reform_t + \epsilon_{it}, \quad (1)$$

where $Layoff_{it}$ is a dummy variable that takes the value of one if worker i is laid off in month t and zero otherwise. $Affected_i$ takes the value of one for workers with zero or one prior UI benefits spell at the time of the reform, and zero otherwise. The dummy variable $Reform_t$ takes the value of zero from March 2014 to February 2015 and one from March 2015 to February 2016. The estimation controls for municipality-industry-occupation-month fixed effects ($\alpha_{t'}$) to absorb shocks at the local industry-occupation level and affected-calendar month fixed effects ($\alpha_{aff,cal}$) to control for seasonal patterns in layoff rates for affected and unaffected workers.

For the tenure ranges from 6 to 11 months and 12 to 17 months, we observe a drop in layoff rates for affected workers by 0.4 and 0.3 percentage points, respectively, when they lose eligibility for UI benefits after the reform. For workers below the six-month threshold, we do not observe systematic changes in layoff rates. Affected workers' post-reform layoff rates are marginally lower for some tenures beyond 17 months. One explanation for lower layoff rates beyond 17 months is that requalifying for UI benefits becomes harder for affected workers after the reform, which increases their incentive to remain in their current job.

To ensure that the patterns in Figure 3 are not driven by different time trends for affected and unaffected workers, in Figure 4 we estimate the difference in layoff rates for workers with 6 to 11 months' tenure compared to workers with 0 to 5 months' tenure for affected relative

to unaffected workers each month. Specifically, we estimate

$$\begin{aligned} Layoff_{it} = & \alpha_{t'} + \alpha_{6m,t} + \alpha_{ten,cal} + \beta_1 \cdot Affected_i + \beta_2 \cdot 6Months_{it} * Affected_i \quad (2) \\ & + \sum_{s=-12, s \neq -1}^{12} \iota_s \cdot Month_s * 6Months_{it} * Affected_i + \epsilon_{it}, \end{aligned}$$

where the dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to eleven months and zero for workers with tenure of zero to five months. The dummy variables $Month_s$ take the value of one s months after the implementation of the reform in March 2015 and zero otherwise. Eligibility-month fixed effects ($\alpha_{6m,t}$) control for differences in layoff rates for workers on either side of the threshold that are unrelated to the reform, and tenure-calendar month fixed effects ($\alpha_{ten,cal}$) to control for tenure-specific seasonal patterns in layoff rates. All other variables are defined as above.

We observe parallel trends before the reform. In contrast, after the reform layoff rates for affected workers with tenure of 6 to 11 months drop sharply and remain consistently lower. Together, this suggests that changes in layoff rates for affected workers with 6 to 11 months' tenure are driven by the reform rather than different time trends.

We complement the insights from the graphical analysis by estimating equation (2) replacing individual $Month_s$ dummies with the $Reform_t$ dummy from equation (1). The results are gathered in Table 3. We find that layoff rates for affected workers above the six-month threshold drop by 0.43 percentage points (11.11 percent) when they lose eligibility for UI benefits after the reform (column I). In columns II and III, we replace month-municipality-industry-occupation fixed effects with month-municipality and month-municipality-industry fixed effects, respectively, which does not affect the results. In column IV, we add age-month and education-month fixed effects, and find similar results. In column V, we limit the sample to workers with 4 to 7 months' tenure in the four months around the reform. This ensures that the results are not affected by affected workers selecting into jobs with longer expected tenure after the reform (Green and Riddell, 1997). In column VI, we expand the sample to include workers with 0 to 17 months' tenure and observe similar results with smaller magnitudes, which reflects the fact that workers with one prior UI spell do not lose eligibility for UI benefits between 12 to 17 months. In column VII, we include expired temporary contracts in the definition of layoffs, which does not affect the results. Finally, in column VIII, we show that the results are not driven by a relabeling of layoffs as other forms of separations such as quits and firings, which behave similarly around the reform.

Difference-in-Bunching Estimation One concern with the estimation strategy in equation 2 is that changes in eligibility rules for the tenure range above 6 months may have dynamic effects on layoff incentives for shorter tenures. For example, layoffs at shorter tenures may be delayed when eligibility for UI benefits can be achieved with a shorter period of continued employment. This would lead to missing mass in layoff rates below the six-month threshold before the reform (Khoury, 2021) and therefore bias our estimates. While the evidence in Figure 3 suggests that layoff rates below the six-month tenure threshold are not systematically affected by the reform, a more direct methodology to assess the presence of dynamic incentive effects below the eligibility threshold is a bunching design (Chetty et al., 2011; Kleven and Waseem, 2013). We provide a detailed description of the implementation of the bunching estimation in Appendix B.

Figure 5 provides the observed and counterfactual layoff distributions. The counterfactual distribution is based on the post-reform layoff patterns when the eligibility threshold is at 12 or 18 months for workers with one or zero prior UI benefits spells, respectively. In addition, the counterfactual distribution adjusts for differences in layoffs around the reform for unaffected workers to account for general time-series changes in layoffs. We find that the observed distribution is almost identical to the counterfactual distribution for the tenure range from 0 to 5 months. This suggests that layoff intensities below the six-month threshold are not affected by their adjacency to the eligibility threshold. This further implies that the excess layoffs we observe when workers are eligible for UI benefits with six months' tenure are additional layoffs that do not occur in the absence of UI benefits rather than a shift in layoff timing.

We complement the graphical evidence in Figure 5 with estimates of excess layoffs (\hat{B}) in the tenure range from 6 to 11 or 6 to 17 months, and the missing mass (\hat{M}) below the threshold in Table 4. The estimates are scaled by the number of affected workers with tenure range from 6 to 11 or 6 to 17 months before the reform (N_{6+}^{pre}). Consistent with the graphical evidence, we find that layoff rates are 0.62 percentage points higher for affected workers with tenure from 6 to 11 months when they qualify for UI benefits (column I). The estimate is similar with 0.59 percentage points for the tenure range from 6 to 17 months (column II). The slightly higher magnitudes compared to the triple-difference methodology in Table 3 are mainly due to more stringent fixed effects in the triple-difference estimation. In contrast, layoff rates below the 6-month threshold are not affected by changes in the eligibility threshold. If anything, we observe marginally higher layoff rates (i.e., the missing mass is negative) for 0 to 5 months' tenure before the reform when all workers with tenure of six months are eligible for UI benefits. This implies that the additional layoffs when workers qualify for UI benefits are generated at the extensive margin, i.e., constitute additional

layoffs, rather than a shift in layoff timing.

Labor Market Informality Next, we examine the role of informal labor markets in explaining the layoff patterns we observe. Informal labor markets are unique in enabling workers to receive UI benefits while being (informally) employed. We exploit two sources of variation in labor market informality: cross-sectional variation in informality across industries and municipalities.

To formally assess how strategic formal layoff rates interact with informal labor markets, we add a continuous variable *Informal*, the share of informal employment in a given industry or municipality, and its interaction with the other independent variables in equation (2). The results are displayed in Table 5, columns I and II. We find that a ten percentage point increase in labor market informality at the industry level leads to a 0.13 percentage point greater decrease in layoff rates for affected workers with tenure of 6 to 11 months when they lose eligibility for UI benefits after the reform (column I). The effect is similar with a 0.09 percentage point greater decrease per ten percentage point increase in labor market informality at the municipality level (column II).

In columns III to V, we follow the methodology in Chetty, Friedman, and Saez (2013) by estimating excess layoff rates as the triple-difference in layoff rates for affected and unaffected workers around the six months eligibility threshold before and after the reform for each industry (column III) and municipality (columns IV and V). We then correlate the resulting excess layoff rates with labor market informality. We find similar point estimates with a 0.12 and 0.11 percentage point greater decrease in layoff rates for affected workers with tenure of 6 to 11 months when they lose eligibility for UI benefits after the reform per ten percentage point increase in labor market informality at the industry and municipality levels, respectively (columns III and IV). In column V, we predict labor market informality at the municipality level based on a municipality's industry composition and include the predicted informality level as a control variable. The estimate remains almost identical after including the predicted level of labor market informality, which suggests that geographical variation provides a distinct source of informality.⁹ The consistency of the estimates across different sources of variation in labor market informality and estimation methods validates the interpretation of the results.

Informal Employment Next, we assess the extent to which higher layoff rates of workers who qualify for UI benefits are related to transitions to informal employment. Formal-

⁹In Table A.3, we replicate the Chetty, Friedman, and Saez (2013) methodology for our bunching estimates and find qualitatively identical results.

to-formal transitions can be identified from administrative data. However, since informal employment is not recorded, administrative data does not allow us to distinguish between formal-to-informal employment transitions and formal-to-unemployment transitions.

The PME survey helps us overcome this obstacle. It includes information on formal employment, informal employment, and unemployment and can be transformed into a panel using the algorithm provided by Ribas and Soares (2008). This allows us to track individuals' employment status over time. Specifically, we observe whether workers transition to unemployment or informal employment upon formal layoff.¹⁰

To formally assess whether higher layoff rates of workers who qualify for UI benefits are related to informal employment inflow, we estimate

$$Informal_{it} = \alpha_{it'} + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot 6Months_{it} * Reform_t + \epsilon_{it}, \quad (3)$$

where $Informal_{it}$ is a dummy variable that takes the value of one if worker i reports being informally employed following a month in which the worker reports being formally employed and zero if the worker reports to be unemployed following a month in which the worker reports to be formally employed. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of 6 to 11 months in the last month of formal employment, and zero for workers with tenure of 4 to 5 months in the last month of formal employment. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March to November 2015 and zero for the pre-reform period from May 2014 to January 2015.¹¹

The results are displayed in Table 6, columns I to III. We find similar results when we include month fixed effects (column I), month and municipality fixed effects (column II), or month-municipality fixed effects (column III). Laid off workers with tenure of 6 to 11 months become about 6 to 7 percentage points less likely to transition to informal employment following a formal layoff when they become ineligible for UI benefits after the reform. Since the PME survey does not allow us to differentiate between workers affected and workers unaffected by the reform, the magnitude of the effect on treated workers is about 1.5 times the estimated effect (about one third of workers are unaffected by the reform).

To examine whether the rate of formal-to-formal transitions changes after the reform, we return to administrative data, which allows us identify cases in which a worker is laid

¹⁰The PME does not differentiate between layoffs and other forms of separations. However, our previous results suggest that the reform does not affect other forms of separations.

¹¹Since the PME is discontinued at the beginning of 2016, the last cohort of laid-off workers we include is November 2015. For individuals that are formally employed in February 2015 and no longer formally employed in March 2015, we do not know whether the layoff occurred in the pre-reform or post-reform period. Thus, we do not assign February 2015 to either period.

off by one firm and immediately takes on a formal job at a different firm. Specifically, we estimate equation (3) for the time-period from March 2014 to February 2016 with $Formal_{it}$ as the dependent variable, which is a dummy variable that takes the value of one if worker i is formally laid off in month t and formally employed at a different firm the next month, and zero if worker i is laid off in month t and not formally employed in the next month. The results are displayed in Table 6, column IV. After the reform, the share of workers that transition to another formal job upon being laid off increases by 3.26 percentage points.

Together, the results from Table 6 allow us to compute the share of the additional layoffs due to eligibility for UI benefits that can be attributed to formal-to-formal employment transitions, transitions to informal employment, and transitions to unemployment. The results from Table 3, column I suggest that formal layoff rates are 11.11 percent higher when workers are eligible for UI benefits. In the data, 20.5 percent of laid off workers with a tenure of 6 to 11 months at layoff transition to formal employment after the reform. Before the reform, a 3.26 percentage point lower rate of workers transition formal-to-formal. This suggests that the share of formal-to-formal transitions is similar before and after the reform $((20.50-3.26)*1.1111=19.16$ vs. 20.50). This in turn implies that the additional workers laid off due to eligibility for UI benefits transition to either informal employment or unemployment.

Since the magnitude of the effect on treated workers is about 1.5 times the estimated effect, the average estimate across columns I to III implies that workers are $(6.28*1.5)=9.42$ percentage points less likely to transition from formal to informal employment when they are no longer eligible for UI benefits. Thus, most $(9.42*1.1111/11.11=94.16$ percent) of the 11.11 percent of strategic layoffs estimated in Section 3.2 are related to informal employment, consistent with the fact that most of the additional layoffs occur in industries and municipalities with higher degrees of labor market informality.

3.3 Recalls and Informal Employment at the Same Firm

The evidence on layoff rates suggests that eligibility for UI benefits leads to additional layoffs that would not have occurred otherwise in the presence of informal labor markets. This is inconsistent with mechanisms related to shocks to the joint surplus of formal employment relationships, which would predict that discontinuity in eligibility rules leads to layoff delays rather than additional layoffs (Khoury, 2021). In this section, we assess the presence of recall patterns, hiring of workers to replace laid off workers, and salary patterns consistent with coordination or collusion between workers and firms to extract rents from the UI system. It is important to note that this analysis does not preclude the existence of other mechanisms

to extract rents from the UI system through strategic layoffs, e.g. shirking.

Firms and workers may devise various strategies to extract rents from the UI system while workers are employed informally. We focus on two possible strategies that provide empirical predictions which we can test in the data. First, firms may temporarily lay off workers to allow them to receive UI benefits while they work informally and recall them once their benefits payments cease. In this case, we should see that firms are more likely to recall workers who are laid off when they become eligible for UI benefits. Second, a firm may keep a formally laid off worker as an informal employee. While informal employment is not directly observable, we can indirectly proxy for keeping workers employed informally by examining differences in the rate at which firms replace a laid off worker. Some layoffs are motivated by firms' desire to replace a given worker with a more productive one. In the data, we observe that for more than half of layoffs firms hire a different worker within a month of laying off a worker. However, if firms continue to employ a formally laid off worker informally, the firm does not need to replace the worker.

Recalls We start by exploring whether firms that lay off workers when they become eligible for UI benefits are more likely to recall them when their benefits cease. Specifically, for workers laid off with a tenure of 11 months or less, we estimate

$$\begin{aligned} Recall_{it}^{4-10} = & \alpha_{t'} + \alpha_{6m,t} + \alpha_{ten,cal} + \beta_1 \cdot Affected_i + \beta_2 \cdot 6Months_{it} * Affected_i \quad (4) \\ & + \beta_3 \cdot 6Months_{it} * Reform_t \cdot Affected_i + \epsilon_{it}, \end{aligned}$$

where $Recall_{it}^{4-10}$ is a dummy variable that takes the value of one if worker i returns to the same firm four to ten months after being laid off and zero otherwise. Since workers are eligible for at least three months of UI benefits, we start the recall window four months after layoff. We end the recall window ten months after layoff, since workers may not apply for UI benefits within 16 months of their last successful application. Thus, firms that repeatedly lay off and recall the same worker have an incentive to recall workers up to ten months after layoff in order for them to be eligible for benefits after another six months of formal employment. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of 6 to 11 months at layoff and zero for workers with tenure of up to 5 months at layoff. The dummy variable $Affected_i$ takes the value of one for workers with zero or one prior UI benefits spell at the time of the reform and zero otherwise. The dummy variable $Reform_t$ takes the value of one for workers laid off between March 2015 and February 2016 and zero for workers laid off between March 2014 and February 2015. We control for municipality-industry-occupation-month fixed effects ($\alpha_{t'}$) to absorb shocks at the local industry-occupation level, eligibility-

month fixed effects ($\alpha_{6m,t}$) to control for differences in recalls for workers on either side of the threshold unrelated to the reform, and tenure-calendar month fixed effects ($\alpha_{ten,cal}$) to control for tenure-specific seasonal patterns.

The results are displayed in Table 7, column I. We find that strategically laid off workers who are eligible for UI benefits are more likely to be recalled by their former employer when their benefits cease. After the reform, when workers with 6 to 11 months' tenure at layoff are no longer eligible for UI benefits, they become 0.35 percentage points less likely to be recalled by their former employer, which is equivalent to 9.56 percent greater recall rates for workers who are laid off after they become eligible for UI benefits. Thus, firms are not only more likely to lay off workers when they qualify for UI benefits, but also more likely to recall these workers, consistent with formal unemployment spells being timed to coincide with workers' eligibility for UI benefits.

Worker Replacement Next, we assess whether some workers who are laid off as they qualify for UI benefits remain informally employed by their previous employer. Since informal employment is not recorded in administrative data, we resort to indirect evidence. When firms lay off a worker, they often hire another worker as a replacement. If firms continue to employ a worker informally, a replacement is not required. Specifically, we estimate

$$\begin{aligned} Replace_{i,t+1} = & \alpha_{it'} + \alpha_{6m,t} + \alpha_{ten,cal} + \beta_1 \cdot Affected_i + \beta_2 \cdot 6Months_{it} * Affected_i \quad (5) \\ & + \beta_3 \cdot 6Months_{it} * Reform_t \cdot Affected_i + \epsilon_{it}, \end{aligned}$$

where $Replace_{i,t+1}$ is a dummy variable that takes the value of one if a firm hires a new worker within one month of laying off worker i in month t and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of 6 to 11 months at layoff and zero for workers with tenure of up to 5 months at layoff. We exclude months in which a firm lays off multiple workers. The dummy variable $Affected_i$ takes the value of one for workers with zero or one prior UI benefits spell at the time of the reform and zero otherwise. The dummy variable $Reform_t$ takes the value of one for workers laid off between March 2015 and February 2016 and zero for workers laid off between March 2014 and February 2015. We control for municipality-industry-occupation-month fixed effects ($\alpha_{it'}$) to absorb shocks at the local industry-occupation level, eligibility-month fixed effects ($\alpha_{6m,t}$) to control for differences in replacement of workers on either side of the threshold unrelated to the reform, and tenure-calendar month fixed effects ($\alpha_{ten,cal}$) to control for tenure-specific seasonal patterns in worker replacement.

The results are displayed in Table 7, column II. When workers with 6 to 11 months'

tenure lose eligibility for UI benefits after the reform, the difference in the rate of hiring a replacement worker increases by 0.72. This suggests that firms are less likely to replace workers laid off when they become eligible for UI benefits consistent with some of them continuing to be informally employed by the firm.

Salary Finally, to assess whether workers and firms share rents from the UI system through salaries, we examine changes in salaries for affected and unaffected workers around the reform. Figure 6 depicts the time-series evolution of salaries for affected relative to unaffected workers around the reform. While we observe parallel trends before the reform, salaries increase for affected workers following the announcement of the reform in December 2014, and continue to increase throughout 2015.

In Table 8, we report the results from estimating

$$\log(\text{salary})_{it} = \alpha_i + \alpha_{jt} + \alpha_{t'} + \beta_2 \cdot \text{Affected}_i * \text{Reform}_t + \epsilon_{it}, \quad (6)$$

where $\log(\text{salary})_{it}$ is the log of worker i 's salary in month t . Since workers became aware of the implications of the reform once it was announced on December 29, 2014, we define the Reform_t dummy as one from January to December 2015 and as zero from January 2013 to December 2014. The dummy variable Affected_i takes the value of one for workers affected by the reform and zero for workers unaffected by the reform. Worker fixed effects (α_i) ensure that we track salaries for the same worker over time. As before, month-municipality-industry-occupation fixed effects ($\alpha_{t'}$) ensure that we compare salaries of affected and unaffected workers in the same municipality, industry, and occupation within a given month. Finally, we control for firm-month fixed effects (α_{jt}) to ensure that changes in salaries are not driven by affected and unaffected workers being employed by different types of firms.

The results in column I show that salaries of workers affected by the reform increase by 0.80 percent relative to workers unaffected by the reform. In addition, salaries increase by 0.30 percent more for affected workers who were laid off with a tenure of 6 to 17 months in the year before the reform in column II, which suggests that salary increases are greater for workers that engage in strategic layoffs to extract rents from the UI system. Given that our previous results imply that 11.11 percent of layoffs in the tenure range from 6 to 11 months are strategic with respect to eligibility for UI benefits, the 0.30 percent increase in salaries is consistent with a $(0.3/0.1111)=2.7$ percent increase in salaries for strategic workers. Together, the results suggest that workers whose layoff timing coincides with eligibility for UI benefits before the reform accept a lower salary, potentially as a means to share rents extracted from the UI system with their employer.

4 Costs of Strategic Unemployment to the UI System

In this section, we quantify the costs of strategic unemployment for the UI system. Our findings in Section 3.2 suggest that 11.11 percent of layoffs are driven by eligibility for UI benefits and 94.16 percent of the additional layoffs are related to workers transitioning to informal employment upon layoff. Rent extraction from the UI system generates a scheme of transfers to firms for whom it is easier to game the system (Anderson and Meyer 1997a), and reduces the funds available for insurance purposes. The back-of-the-envelope estimates we provide should be interpreted as a rough approximation, since we make several assumptions in their derivation.

We document that about 11.11 percent of layoffs above the six-month threshold are due to workers' eligibility for UI benefits (Table 3, columns I and VI). For tenure beyond 17 months, we need to make an assumption to compute the full costs of strategic formal unemployment to the system. We consider a conservative scenario, where we assume that strategic layoffs cease immediately above 17 months' tenure, and an alternative scenario, where we assume that strategic layoffs persist at the same rate above 17 months' tenure. This provides us with a range in which the actual costs are likely to fall. From the Ministry of Economics, we obtain data on the fraction of UI payments made to workers with a given level of tenure at layoff. In 2014, 19.4 percent of UI benefits were paid to workers with tenure of no more than 17 months at layoff. Thus, under the conservative scenario, 2.16 percent of all UI payments go to strategically formally unemployed workers ($0.194 \times 0.1111 = 2.16$ percent). Under the alternative scenario, 11.11 percent of unemployment payments go to strategically formally unemployed workers.

In 2014, total UI benefits payments were 32.8bn BRL, or 0.6 percent of GDP. For the conservative scenario, 2.16 percent of the total UI benefits payments of 32.8bn BRL equals 0.71bn BRL, or 0.013 percent of GDP. The respective value for the alternative scenario is 0.067 percent of GDP. The annual costs and distortions from strategic formal unemployment increase with the size of informal labor markets. The estimates from Table 5 imply that strategic layoffs increase by about 0.11 percentage points per ten percentage point increase in labor market informality at the industry or municipality level. We depict the degree of strategic formal layoffs in Figure 7 (solid line). Additionally, we add a plot of the range of total costs as a fraction of GDP for different levels of labor market informality for the conservative and alternative scenarios (dotted lines). For markets with very low levels of informality, costs to the UI system due to strategic formal unemployment are negligible. However, they can reach more than 0.03 percent of GDP for high levels of labor market informality even under the most conservative assumption, and more than 0.15 percent of

GDP under the least conservative assumption.

Our cross-sectional estimates also allow us to relate our results to recent studies that examine differences in layoff rates around tenure and age thresholds that discontinuously alter workers' eligibility for severance payments in Austria (Card, Chetty, and Weber, 2007) or UI benefits duration in Germany (Schmieder, von Wachter, and Bender, 2012). While labor market informality reaches 28.5 percent in Brazil, according to the European Social Survey (2014) informality rates are 1.4 percent in Austria and 2.9 percent in Germany. At these levels of labor market informality, our estimates imply that the share of strategic layoffs due to eligibility for UI benefits is below 3 percent.¹²

Additional costs induced by strategic formal unemployment are harder to quantify. For example, it may lead workers to switch between the formal and the informal sector in a manner they would not deem optimal in the absence of UI-benefits induced unemployment (Haanwinckel and Soares, forthcoming). Additionally, employing workers informally or having to replace workers temporarily may induce costs to firms and reduce investment in firm-specific human capital. Moreover, if strategic unemployment reduces the number of months for which workers are employed formally, the government's tax revenues are lower.

5 Conclusion

In recent years, several mid-income and developing countries have begun implementing government-mandated unemployment insurance programs (Holzmann et al. 2011; ILO 2017). However, most of the empirical evidence on incentive effects of UI in the literature is from developed countries in North America and Europe. Understanding how labor market characteristics that are more prevalent in mid-income and developing countries interact with UI is important for the purpose of adapting the design of UI programs to these countries.

To assess incentive effects of UI in the presence of large informal labor markets, we exploit a reform of UI benefits eligibility criteria and cross-sectional variation in labor market informality in Brazil. We document that workers are 11.11 percent more likely to be laid off when they qualify for UI benefits. Formal layoffs and recalls are timed to coincide with eligibility for UI benefits. Specifically, firms lay off workers when they become eligible for UI benefits, and recall them when their benefits cease. Changes in salaries around the reform indicate that workers pass on part of the rents they extract from the UI system to their

¹²In Card, Chetty, and Weber (2007) the incentives of workers and firms are also misaligned since workers would prefer to stay employed beyond the 36 month threshold to receive severance payments, whereas firms may prefer workers to be laid off before reaching the threshold to avoid severance payments. This misalignment of incentives may also contribute to the absence of strategic layoffs around the threshold.

employer, consistent with models of implicit contracting in the presence of UI (Feldstein 1976; Baily 1977; Christofides and McKenna 1996). Other mechanisms, such as shirking by workers or firms laying off workers who are less likely to find alternative employment in response to demand fluctuations may also be relevant in contributing to layoffs due to eligibility for UI benefits.

Survey evidence suggests that the majority (94 percent) of the additional layoffs due to eligibility for UI benefits are related to transitions into informal employment and higher layoff rates of workers who qualify for UI are strongly correlated with labor market informality. This suggests that informal labor markets play an important role in strategic layoffs in response to UI eligibility. Moreover, firms are less likely to hire a replacement worker after laying off a worker who qualifies for UI benefits, compared with after laying off a worker who is ineligible for UI benefits. This suggests that some firms continue to employ workers informally while they are on UI benefits.

Our findings have implications for UI design. The timing of layoffs and recalls to match workers' UI eligibility suggests that part of the UI system transfers rents towards firms and workers who exploit the system. While we are cautious about general welfare statements, our findings suggest that reducing potential rents could reduce the adverse incentive effects of UI in the presence of informal labor markets. Rents can be reduced by lowering replacement rates and the duration of benefits, or by increasing experience rating to increase the cost of layoffs. More nuanced policy implications include tweaks to the UI system that prevent repeated temporary layoffs of the same worker by the same firm. The strong correlation with labor market informality suggests that these considerations are particularly important for mid-income and developing countries with large informal labor markets, and that reducing labor market informality, or better monitoring in combination with higher fines, may also reduce adverse incentive effects.

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Table 1: **Informality by Industry**

Industry	Informal Employment Share	Employment Share
Domestic Services	0.6617	0.0975
Agriculture, Livestock, Forestry, Fisheries, Aquaculture	0.5693	0.0691
Other Services	0.4788	0.0173
Arts, Culture, Sports, Recreation	0.4314	0.0074
Construction	0.4074	0.0636
Accommodation, Food	0.3155	0.0336
Real Estate	0.2850	0.0038
Trade, Repair of Motor Vehicles and Motorcycles	0.2562	0.1569
Water, Sewerage, Waste Management, Decontamination	0.2211	0.0074
Other Activities	0.2157	0.0586
Professional, Scientific, and Technical Activities	0.2144	0.0257
Transport, Storage, Postal Services	0.2012	0.0430
Education	0.1827	0.0754
Manufacturing	0.1547	0.1348
Human Health, Social Services	0.1542	0.0461
Information, Communication	0.1441	0.0149
Public Administration, Defense, Social Security	0.1422	0.0752
Extractive Industries	0.1408	0.0060
Administrative Activities and Complementary Services	0.1389	0.0437
Financial Activities and Related Insurance and Services	0.0903	0.0167
Electricity and Gas	0.0556	0.0034

This table lists the informal employment share for a given industry and the share of workers employed in the respective industry based on the 2010 Census.

Table 2: Summary Statistics for Workers Around the Threshold

	0-5 Months' Tenure	6-11 Months' Tenure	Difference
Age (Years)	32	31	1
Salary (Brazilian Real)	1,335	1,224	11
Male	0.604	0.602	0.002
University Education	0.111	0.094	0.017
Firm Size (Employees)	94	94	0
Fraction in Construction	0.028	0.025	0.003
Fraction in Manufacturing	0.120	0.096	0.024
Fraction in Agriculture	0.017	0.016	0.002

This table reports pre-reform descriptive statistics for workers with tenure of zero to five months in the first column, and workers with tenure of six to eleven months in the second column. The third column depicts the difference.

Table 3: Formal Layoffs

Dep. Var.:	I	II	III	IV	V	VI	VII	VIII
	<i>Layoff_{it}</i> (mean: 0.0292)				<i>Quit_{it}</i> (mean: 0.0365)			
<i>Affected_i</i>	0.0015*** (0.0001)	-0.0026*** (0.0002)	0.0004*** (0.0001)	-0.0008*** (0.0001)	0.0015** (0.0007)	0.0010*** (0.0001)	0.0062*** (0.0003)	0.0077*** (0.0003)
<i>6Months_{it} * Affected_i</i>	-0.0017*** (0.0003)	-0.0023*** (0.0003)	-0.0019*** (0.0003)	-0.0018*** (0.0003)	0.0020** (0.0009)	-0.0005* (0.0003)	-0.0056*** (0.0003)	-0.0020*** (0.0003)
<i>6Months_{it} * Reform_t * Affected_i</i>	-0.0043*** (0.0003)	-0.0042*** (0.0003)	-0.0042*** (0.0003)	-0.0040*** (0.0003)	-0.0045*** (0.0009)	-0.0039*** (0.0002)	-0.0041*** (0.0003)	-0.0002 (0.0002)
Month*Municipality FE	-	yes	-	-	-	-	-	-
Month*Municipality*Industry FE	-	no	yes	-	-	-	-	-
Month*Municipality*Industry*Occupation FE	yes	no	no	yes	yes	yes	yes	yes
Month*Eligibility FE	yes	yes	yes	yes	yes	yes	yes	yes
Tenure*Calendar-Month FE	yes	yes	yes	yes	no	yes	yes	yes
Month*Age FE	no	no	no	yes	no	no	no	no
Month*Education FE	no	no	no	yes	no	no	no	no
Clustered SE	muni	muni	muni	muni	muni	muni	muni	muni
Observations	33,273,434	33,273,434	33,273,434	33,269,285	1,771,376	43,723,702	33,273,434	33,273,434
<i>R</i> ²	0.131	0.017	0.049	0.131	0.216	0.116	0.131	0.126

The dependent variable *Layoff_{it}* is a dummy variable that takes the value of one if worker *i* is formally laid off in month *t* and zero otherwise. In column VII, it also takes the value of one for expired temporary contracts. In column VIII, the dependent variable *Quit_{it}* is a dummy variable that takes the value of one if worker *i* separates from their employer, but is not laid off, in month *t* and zero otherwise. The dummy variable *6Months_{it}* takes the value of one for workers with tenure of six to eleven months (six to seven months and six to seventeen in columns V and VI, respectively) and zero for workers with tenure up to five months (four to five months in column V). The dummy variable *Reform_t* takes the value of one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015 (March to April 2015 and January to February 2015 in column V). The dummy variable *Affected_i* takes the value of one for workers with zero or one prior UI benefits spell and zero for workers with two or more prior UI benefits spells. Due to computational constraints, the estimates are based on a 10% random sample. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: **Bunching Estimation**

	I	II
Estimation Window:	[0; 11]	[0; 17]
\hat{B}/N_{6+}^{pre}	0.0062*** (0.0001)	0.0059*** (0.0001)
\hat{M}/N_{6+}^{pre}	-0.0005*** (0.0001)	-0.0003*** (0.0001)
$(\hat{B} - \hat{M})/N_{6+}^{pre}$	0.0067*** (0.0001)	0.0062*** (0.0001)
Observations	16,507,139	16,507,139
Bootstrap replications	100	100

This table reports bunching (\hat{B}/N_{6+}^{pre}) and missing mass (\hat{M}/N_{6+}^{pre}) of layoff rates above and below the six month tenure threshold, respectively. The empirical distribution is estimated based on affected workers from March 2014 to February 2015. The counterfactual distribution is based on affected workers from March 2015 to February 2016, and adjusted based on the difference in pre-reform and post-reform layoff rates of unaffected workers. Standard errors from bootstrapping 100 random samples with replacement are reported in parentheses. *** denotes statistical significance at the 1% level.

Table 5: **Formal Layoffs and Labor Market Informality**

	I	II	III	IV	V
Dep. Var.:	<i>Layoff_{it}</i> (mean: 0.0292)		<i>Excess Layoff</i>		
Informality:	Industry	Municipality	Industry	Municipality	
<i>6Months_{it} * Informal</i>	0.0506*** (0.0044)	0.0116*** (0.0038)			
<i>6Months_{it} * Reform_t * Informal</i>	0.0066 (0.0048)	-0.0062 (0.0049)			
<i>Affected_i * Informal</i>	0.0111*** (0.0013)	0.0023 (0.0015)			
<i>6Months_{it} * Affected_i * Informal</i>	0.0029 (0.0025)	0.0093*** (0.0027)			
<i>6Months_{it} * Reform_t * Affected_i * Informal</i>	-0.0129*** (0.0033)	-0.0090*** (0.0035)			
<i>Informal</i>			-0.0120*** (0.0039)	-0.0114*** (0.0017)	-0.0103*** (0.0028)
$\widehat{Informal}$					-0.0047 (0.0015)
Month*Municipality*Industry*Occupation FE	yes	yes	-	-	-
Tenure*Calendar-Month FE	yes	yes	-	-	-
Month*Municipality*Eligibility FE	yes	no	-	-	-
Month*Industry*Eligibility FE	no	yes	-	-	-
Clustered SE	muni	muni	-	-	-
Observations	33,273,434	33,270,682	20	5,559	5,559
<i>R</i> ²	0.139	0.131	0.582	0.031	0.031

The dependent variable *Layoff_{it}* is a dummy variable that takes the value of one if worker *i* is formally laid off in month *t* and zero otherwise. The dependent variable *ExcessLayoff* is the triple-difference between changes in layoff rates for workers in the 6 to 11 months tenure range compared with the 0 to 5 months tenure range around the reform for affected relative to unaffected workers in a given industry (column III) or municipality (columns IV and V). The dummy variable *6Months_{it}* takes the value of one for workers with tenure of 6 to 11 months and zero for workers with tenure of up to 5 months. The dummy variable *Reform_t* takes the value of one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015. The dummy variable *Affected_i* takes the value of one for workers with zero or one prior UI benefits spell and zero for workers with two or more prior UI spells. The variable *Informal* is the share of informal employment in a given industry (columns I and III) or municipality (columns II, IV, and V). The variable $\widehat{Informal}$ is the predicted level of informality at the municipality level based on its industry composition. Due to computation constraints, the estimates in columns I and II are based on a 10% random sample. Standard errors are reported in parentheses. The bottom part of the table provides information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table 6: Informal Employment

	I	II	III	IV
Dep. Var.:	<i>Informal_{it}</i> (mean:0.2488)			<i>Formal_{it}</i> (mean:0.1907)
<i>6Months_{it}</i>	0.0161 (0.0172)	0.0167 (0.0161)	0.0220 (0.0166)	-0.1040*** (0.0020)
<i>6Months_{it} * Reform_t</i>	-0.0589** (0.0267)	-0.0591** (0.0254)	-0.0703** (0.0263)	0.0326*** (0.0013)
Month FE	yes	yes	-	-
Municipality FE	no	yes	-	-
Month*Municipality FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	-	yes
Clustered SE	month	month	month	muni
Observations	5,172	5,172	5,172	7,035,133
<i>R</i> ²	0.005	0.010	0.026	0.249

The results in columns I to III are based on data from the PME survey, the results in column IV are based on administrative data from RAIS. The dependent variable *Informal_{it}* takes the value of one if worker *i* reports to be informally employed following formal employment in month *t* and zero if worker *i* reports to be unemployed following formal employment in month *t*. The dependent variable *Formal_{it}* is a dummy variable that takes the value of one if worker *i* transitions to a different formal job upon layoff in month *t* and zero if worker *i* transitions to formal unemployment. The dummy variable *6Months_{it}* takes the value of one for workers with tenure of six to eleven months and zero for workers with tenure of up to five months (four to five months in columns I to III). The dummy variable *Reform_t* takes the value of one for the post-reform period from March to November 2015 and zero for the pre-reform period from May 2014 to January 2015 in columns I to III, and one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015 in column IV. Standard errors are reported in parentheses. The bottom part of the table provides information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table 7: Recalls and Worker Replacement

	I	II
Dep. Var.:	$Recall_{it}^{4-10}$	$Replace_{i,t+1}$
Mean:	(0.0366)	(0.5831)
$Affected_i$	0.0013*** (0.0003)	-0.0073*** (0.0012)
$6Months_{it} * Affected_i$	-0.0035*** (0.0007)	0.0003 (0.0013)
$6Months_{it} * Reform_t * Affected_i$	-0.0035*** (0.0006)	0.0072*** (0.0013)
Month*Municipality*Industry*Occupation FE	yes	yes
Month*Eligibility FE	yes	yes
Tenure*Calendar-Month FE	yes	yes
Clustered SE	muni	muni
Observations	7,035,103	6,297,271
R^2	0.305	0.292

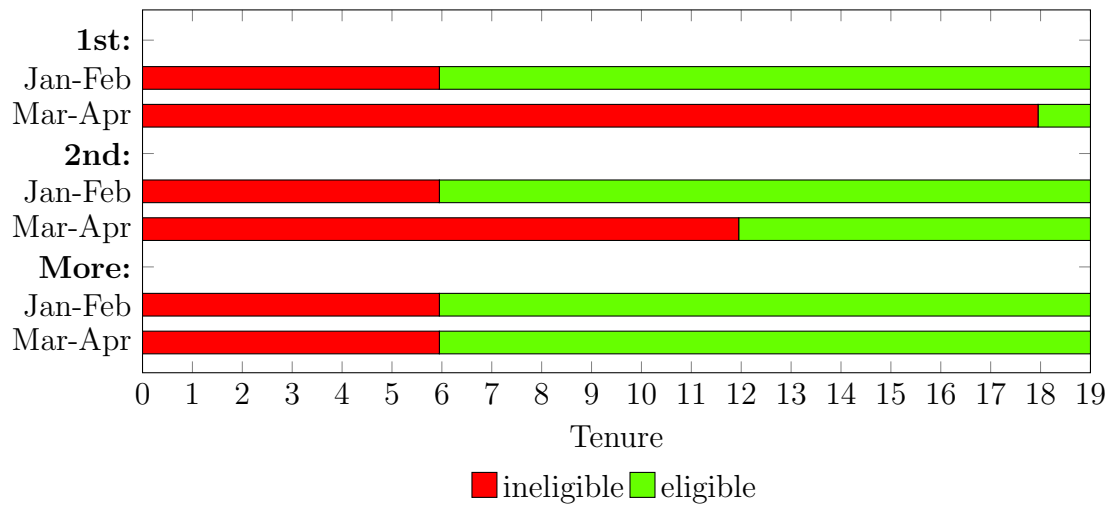
The dependent variable $Recall_{it}^{4-10}$ is a dummy variable that takes the value of one if worker i is recalled four to ten months after being laid off and zero otherwise. The dependent variable $Replace_{i,t+1}$ is a dummy variable that takes the value of one if a firm hires a new worker within one month of laying off worker i . The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of 6 to 11 months and zero for workers with tenure of up to 5 months. The dummy variable $Reform_t$ takes the value of one from March 2015 to February 2016 and zero from March 2014 to February 2015. The dummy variable $Affected_i$ takes the value of one for workers with zero or one prior UI benefits spell and zero for workers with two or more prior UI spells. Standard errors are reported in parentheses. The bottom part of the table provides information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table 8: **Salaries**

Dep. Var.: $\log(\text{salary})_{it}$	I	II
$Affected_i * Reform_t$	0.0080*** (0.0004)	0.0073*** (0.0004)
$Affected_i * Reform_t * Strategic_i$		0.0030*** (0.0011)
Month*Muni*Ind*Occupation FE	yes	yes
Worker FE	yes	yes
Firm*Month FE	yes	yes
Strategic*Month FE	no	yes
Observations	396,579,042	396,579,042
R^2	0.979	0.979

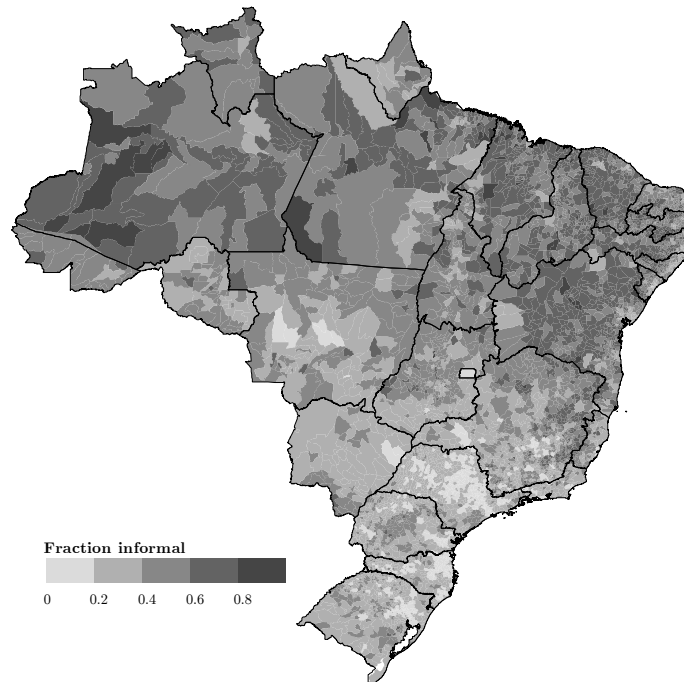
The dependent variable $\log(\text{salary})_{it}$ is the log of worker i 's salary in month t . The dummy variable $Affected_i$ takes the value of one for workers with zero or one prior UI benefits spell and zero for workers with two or more prior UI benefits spells. The dummy variable $Reform_t$ takes the value of one from January to December 2015 and zero from January 2013 to December 2014. The dummy variable $Strategic_i$ takes the value of one if worker i was laid off with a tenure of six to seventeen months during the pre-reform period and zero otherwise. Due to computational constraints, the estimates are based on a 40 percent random sample. Standard errors are reported in parantheses. The bottom part of the table provides information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Figure 1: UI Eligibility Around the Reform



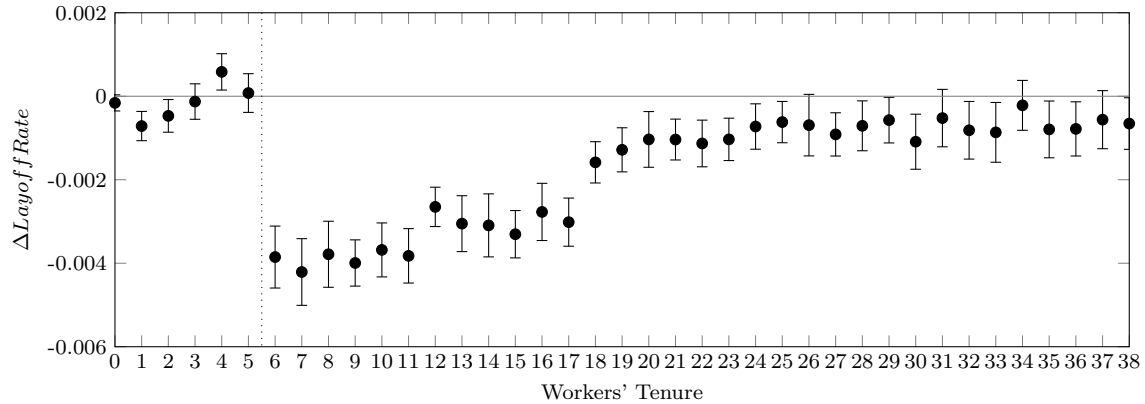
This figure shows eligibility criteria for UI benefits before and after the reform based on workers' tenure for workers who apply for UI benefits for the first time, the second time, or the third time or more. Red bars indicate tenure not satisfying eligibility criteria, green bars indicate tenure satisfying eligibility criteria.

Figure 2: Labor Market Informality by Municipality



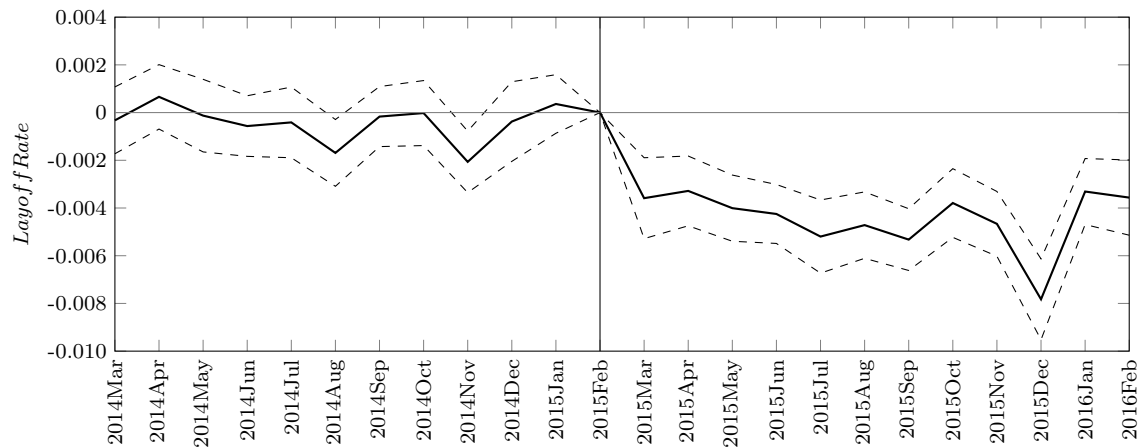
This figure depicts informal labor market shares for all municipalities in Brazil based on the 2010 Census.

Figure 3: **Formal Layoffs by Tenure**



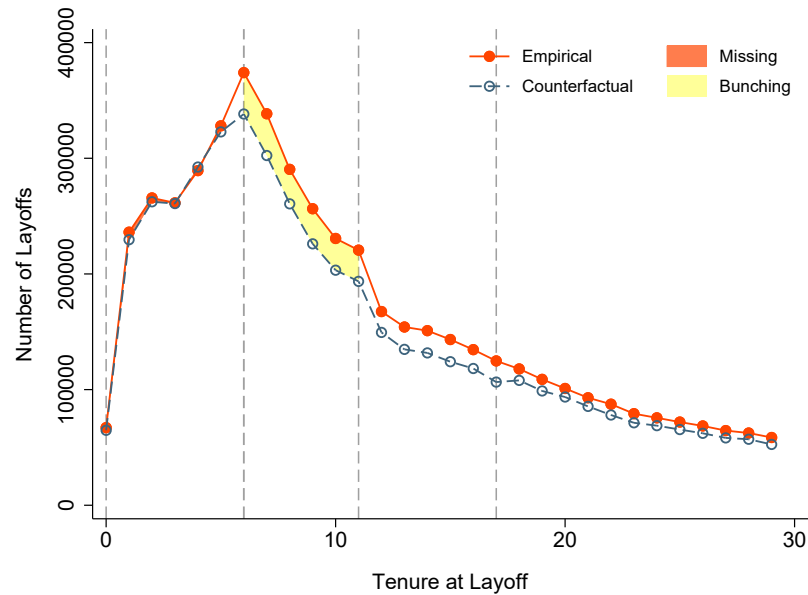
This figure estimates changes in layoff rates for affected and unaffected workers around the reform (March 2015 to February 2016 relative to March 2014 to February 2015) for each tenure from 0 to 38 months with occupation-municipality-industry-month and affected-calendar month fixed effects. The vertical bars represent 99 percent confidence intervals.

Figure 4: **Formal Layoffs – Dynamics**



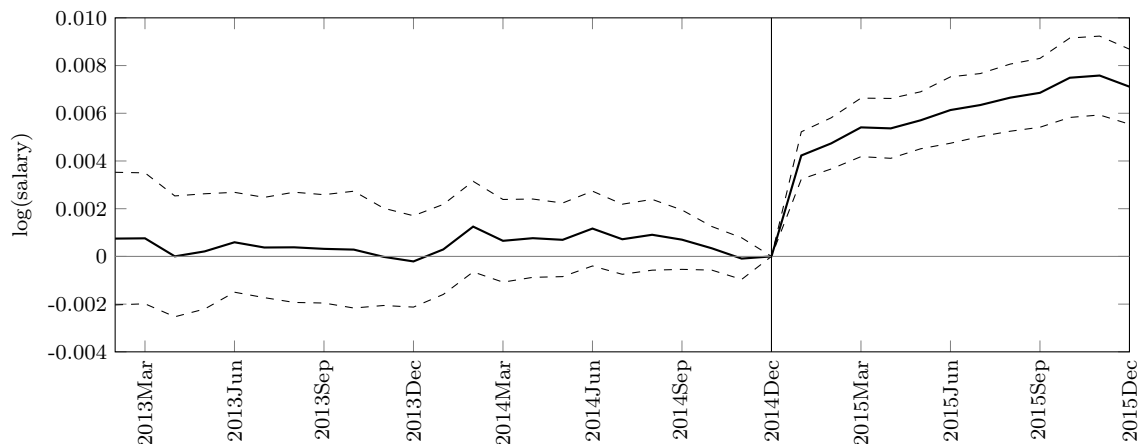
This figure depicts the results from estimating equation (2) comparing layoff rates for workers with 6 to 11 months' tenure relative to workers with up to 5 months' tenure for affected compared to unaffected workers for every month from March 2014 to February 2016, normalized to zero in February 2015. The dashed lines represent 99 percent confidence intervals.

Figure 5: **Formal Layoffs by Tenure - Bunching Estimation**



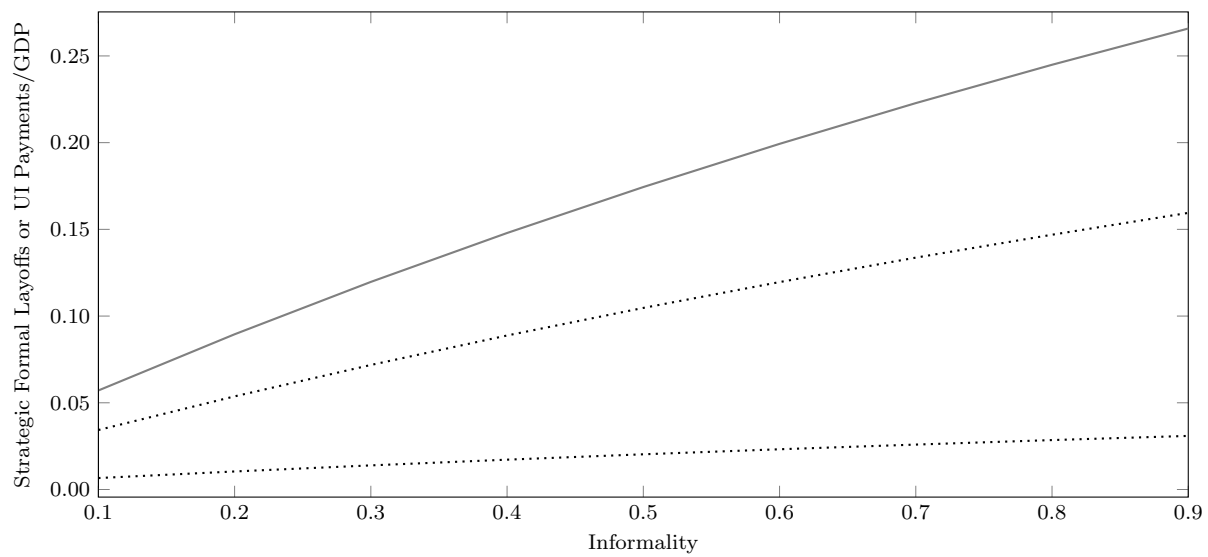
This figure plots the empirical (orange) and counterfactual (black) layoff distributions. The empirical distribution is estimated based on affected workers from March 2014 to February 2015. The counterfactual distribution is based on affected workers from March 2015 to February 2016, and adjusted based on the difference in pre-reform and post-reform layoffs of unaffected workers. The vertically lines denote the pre-reform UI eligibility threshold of 6 months for affected workers, and the post-reform eligibility thresholds for affected workers of 11 or 17 months. Each dot denotes the (estimated) number of layoffs for each tenure bin, where a bin is generated by rounding down to the nearest integer.

Figure 6: **Salaries**



This figure plots the log difference in residualized salaries for affected and unaffected workers from January 2013 to December 2015 normalized to zero in December 2014, residualized by controlling for individual, industry-municipality-occupation-month, and firm-month fixed effects. Due to computational constraints, the estimates are based on a 40 percent random sample. The dashed lines represent 99 percent confidence intervals.

Figure 7: Strategic Formal Unemployment Share by Informality



This figure depicts the fraction of formal layoffs due to eligibility for UI benefits (solid line) and the range of annual UI benefits payments due to strategic formal unemployment as a fraction of GDP (dotted lines) for different levels of labor market informality.

Appendix A. Additional Tables and Figures

Table A.1: Variable Definitions - Dependent Variables

	Variable Definition	Main Sample	Sample Period
Formal Layoffs			
$Layoff_{it}$	Dummy variable: 1 if worker i is laid off in month t , 0 otherwise	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
$Quit_{it}$	Dummy variable: 1 if worker i separates from her employer in month t other than through layoff, 0 otherwise	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
\hat{B}/N_{6+}^{pre}	Excess layoffs (bunching) for workers with 6 to 11 (17) month tenure scaled by the number of affected workers with tenure from 6 to 11 (17) between March 2014 and February 2015.	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
\hat{M}/N_{6+}^{pre}	Missing mass of layoffs for workers with up to five months of tenure scaled by the number of affected workers with tenure from 6 to 11 (17) between March 2014 and February 2015.	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
$(\hat{B} - \hat{M})/N_{6+}^{pre}$	Extensive margin of excess layoffs for workers with 6 to 11 (17) month tenure scaled by the number of affected workers with tenure from 6 to 11 (17) between March 2014 and February 2015.	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
$ExcessLayoff$	The triple-difference between changes in layoff rates for workers in the six to eleven months tenure range compared with the zero to five tenure range around the reform for affected relative to unaffected workers in a given industry or municipality.	Formally employed workers with tenure of 0 to 11 months (RAIS)	March 2014 to February 2016
Employment Status upon Formal Layoff			
$Informal_{it}$	Dummy variable: 1 if worker i reports to be informally employed in month $t + 1$ after being formally employed in month t , 0 if worker i reports to be unemployed in month $t + 1$ after being formally employed in month t	Workers with tenure of 4 to 11 months in last month of formal employment (PME)	May 2014 to November 2015
$Formal_{it}$	Dummy variable: 1 if worker i is formally employed by a different firm upon formal layoff in month t , 0 if worker i is not formally employed elsewhere upon formal layoff in month t	Workers with tenure of 0 to 11 months at layoff (RAIS)	March 2014 to February 2016
Recalls and Replacement Hiring			
$Recall_{it}^{4-10}$	Dummy variable: 1 if worker i is recalled by the same firm 4 to 10 months after layoff in month t , 0 otherwise	Workers with tenure of 0 to 11 months at layoff (RAIS)	March 2014 to February 2016
$Replace_{i,t+1}$	Dummy variable: 1 if a firm hires a worker within one month of laying off worker i in month t , 0 otherwise	Workers with tenure of 0 to 11 months at layoff (RAIS)	March 2014 to February 2016
Salaries			
$\log(salary)_{it}$	Log of worker i 's salary in month t	Formally employed workers (RAIS)	January 2013 to December 2015

This table lists all dependent variables and their definitions, and describes the main sample, data source, and sample period for each variable.

Table A.2: Variable Definitions - Independent Variables

	Variable Definition	Notes
$6Months_{it}$	Dummy variable: 1 if worker i 's current tenure is 6 months or higher, 0 if worker i 's current tenure is below 6 months	For tests that are conditional on formal layoff, the variable is defined based on worker i 's tenure at layoff
$Reform_t$	Dummy variable: 1 for the post-reform period between March 2015 and February 2016, 0 for the pre-reform period between March 2014 and February 2015	For salary tests, the post-reform period starts after the announcement of the reform in January 2015, and the pre-reform period ends in the month of the announcement of the reform in December 2014
$Affected_i$	Dummy variable: 1 for workers affected by the reform (less than two prior UI benefits spells), 0 for workers unaffected by the reform (two or more prior UI benefits spells)	
$Informal$	Share of informally employed workers in total employment in a given industry or municipality	Based on the 2010 Brazilian Census
$\widehat{Informal}$	The predicted level of informality at the municipality level based on its industry composition	Based on the 2010 Brazilian Census
$Strategic_i$	Dummy variable: 1 if worker i is laid off with a tenure of 6 to 17 months in 2013 or 2014, 0 otherwise	

This table lists all independent variables and their definitions.

Table A.3: Excess Layoff Bunching Estimates and Informality

	I	II	III
Dep. Var.:	Excess Layoff in Municipality		
	Bunching	Missing	Total Effect
	\hat{B}/N_{6+}^{pre}	\hat{M}/N_{6+}^{pre}	$(\hat{B} - \hat{M})/N_{6+}^{pre}$
$Informal$	0.0267*** (0.0038)	-0.0008 (0.0034)	0.0285*** (0.0056)
$\widehat{Informal}$	0.0896*** (0.0090)	-0.0229** (0.0092)	0.1137*** (0.0137)
Observations	5,286	5,134	5,134
R^2	0.191	0.006	0.129

This table reports bunching (\hat{B}/N_{6+}^{pre}) and missing mass (\hat{M}/N_{6+}^{pre}) of layoff rates above and below the six month threshold, respectively. The empirical distribution is estimated based on affected workers from March 2014 to February 2015. The counterfactual distribution is based on affected workers from March 2015 to February 2016, and adjusted based on the difference in pre-reform and post-reform layoffs of unaffected workers. The variable $Informal$ is the share of informal employment in a given municipality. The variable $\widehat{Informal}$ is the predicted level of informality at the municipality level based on its industry composition. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level.

Appendix A. Summary of Labor Laws

In this section, we summarize the changes in the labor law that define a worker's eligibility for unemployment benefits. Eligibility is defined in Article 3 of the original labor law 7998, which was enacted on January 11, 1990. It was updated by the Provisional Measure 665, which was first announced on December 29, 2014 and came into effect on March 1, 2015.¹³ Finally, the provisional measure was transformed into Law 13 135 on June 16, 2015 and has been in effect since July 1, 2015. In what follows next, we provide the relevant part of each law defining a worker's eligibility for UI benefits, the source of the law, the wording of the law in Portuguese, and the English translation.

Law 7998, in effect from January 11, 1990 until March 1, 2015

Source: http://www.planalto.gov.br/ccivil_03/LEIS/L7998.htm

Portuguese [definition of eligibility]:

Art. 3º Terá direito à percepção do seguro-desemprego o trabalhador dispensado sem justa causa que comprove:

I - ter recebido salários de pessoa jurídica ou pessoa física a ela equiparada, relativos a cada um dos 6 (seis) meses imediatamente anteriores à data da dispensa;

English [definition of eligibility]:

Art. 3 A worker dismissed without just cause shall have the right to claim unemployment insurance if the following is satisfied:

I – The worker has received salaries from a firm or an individual equivalent to it for each of the six (6) months immediately preceding the date of the dismissal;

Provisional Measure MPV 665; Announced December 29, 2014. In effect between March 1, 2015 and July 1, 2015

Source: http://www.planalto.gov.br/ccivil_03/_Ato2011-2014/2014/Mpv/mpv665.htm

Portuguese [definition of eligibility]:

¹³The reform was officially published by the Federal Government on December 30, 2014, while the newspapers started to discuss it on December 29, 2014.

Art. 3º Terá direito à percepção do seguro-desemprego o trabalhador dispensado sem justa causa que comprove:

I - ter recebido salários de pessoa jurídica ou pessoa física a ela equiparada, relativos:

- a) a pelo menos dezoito meses nos últimos vinte e quatro meses imediatamente anteriores à data da dispensa, quando da primeira solicitação;*
- b) a pelo menos doze meses nos últimos dezesseis meses imediatamente anteriores à data da dispensa, quando da segunda solicitação; e*
- c) a cada um dos seis meses imediatamente anteriores à data da dispensa quando das demais solicitações;*

English [definition of eligibility]:

Art. 3 A worker dismissed without just cause shall have the right to claim unemployment insurance if the following is satisfied:

I – The worker has received salaries from a firm or an individual equivalent to it:

- a) for at least eighteen months in the last twenty-four months immediately preceding the date of dismissal at the time of the first request;*
- b) for at least twelve months in the last sixteen months immediately preceding the date of dismissal at the time of the second request; and*
- c) for each one of the six months immediately preceding the date of the dismissal at the time of the third or higher request;*

Law 13 134; Enacted June 16, 2015. In effect from July 1, 2015

Source: http://www.planalto.gov.br/ccivil_03/_Ato2015-2018/2015/Lei/L13134.htm

Portuguese [definition of eligibility]:

Art. 3º Terá direito à percepção do seguro-desemprego o trabalhador dispensado sem justa causa que comprove:

I - ter recebido salários de pessoa jurídica ou de pessoa física a ela equiparada, relativos a:

- a) pelo menos 12 (doze) meses nos últimos 18 (dezoito) meses imediatamente anteriores à data de dispensa, quando da primeira solicitação;*
- b) pelo menos 9 (nove) meses nos últimos 12 (doze) meses imediatamente anteriores à data de dispensa, quando da segunda solicitação; e*
- c) cada um dos 6 (seis) meses imediatamente anteriores à data de dispensa, quando das demais solicitações;*

English [definition of eligibility]:

Art. 3 A worker dismissed without just cause shall have the right to claim unemployment insurance if the following is satisfied:

I – The worker has received salaries from a firm or an individual equivalent to it:

- a) for at least 12 (twelve) months in the last 18 (eighteen) months immediately preceding the date of the dismissal at the time of the first request;*
- b) for at least 9 (nine) months in the last 12 (twelve) months immediately preceding the date of the dismissal at the time of the second request; and*
- c) for each of the six (6) months immediately preceding the date of the dismissal at the time of the third or higher request;*

Appendix B. Bunching Estimates

Measuring excess layoff rate and missing mass around the eligibility threshold requires an accurate estimate of a counterfactual layoff distribution. While the existing literature has developed standard approaches for estimating this type of counterfactual from a single cross-section of data, those approaches require the assumption that the counterfactual distribution is smooth around the threshold (Chetty et al., 2011; Kleven and Waseem, 2013). Since we have a group of affected and unaffected workers before and after the reform, we do not need to make this assumption. Our counterfactual distribution leverages both the time-series dimension and the fact that some workers were unaffected by the reform. We construct

the pre-reform counterfactual layoff distribution for affected workers by adjusting the post-period layoff distribution of affected workers based on observed changes to the distribution in the layoff distribution of unaffected workers. Below we describe in detail how we obtain the counterfactual distribution.

Counterfactual Layoff Distribution We first estimate the counterfactual number of layoffs that would have occurred in each tenure bin d in the pre-reform period. We construct the counterfactual pre-reform layoff distribution from the observed post-reform layoff distribution of affected workers plus an adjustment that is based on the observed changes in the layoff distribution of unaffected workers around the reform. We scale the observed number of layoffs by the respective total employment (N) in each tenure bin. This provides us with layoff rates (e.g. $n_{ad}^{pre}/N_{ad}^{pre}$ and $n_{ad}^{post}/N_{ad}^{post}$), which are directly comparable across tenure levels for affected and unaffected workers both before and after the reform. Thus, our counterfactual distribution is given by:

$$\hat{n}_{ad}^{pre} = \left(\frac{n_{ad}^{post}}{N_{ad}^{post}} + \left(\frac{n_{ud}^{pre}}{N_{ud}^{pre}} - \frac{n_{ud}^{post}}{N_{ud}^{post}} \right) \right) \cdot N_{ad}^{pre}, \quad (\text{B1})$$

where $n_{ad}^{post}/N_{ad}^{post}$ denotes the post-reform period layoff rate for the affected workers for each tenure-bin, $(n_{ud}^{pre}/N_{ud}^{pre} - n_{ud}^{post}/N_{ud}^{post})$ estimates the change in the layoff rate of unaffected workers before and after the reform for each tenure-bin, and N_{ad}^{pre} denotes the number of affected workers within each tenure bin before the reform.

Bunching, Missing Mass, and the Effect of UI on Layoff Rates Based on this counterfactual, we measure the effects of eligibility for UI benefits on excess layoff rates above the eligibility threshold (bunching) as well as the missing mass below the eligibility threshold by comparing the empirical pre-reform layoff distribution of affected workers to the counterfactual distribution. We define the bunching mass as the sum of the difference between the empirical and counterfactual distributions over the region $[6, \bar{d}]$ for which eligibility for UI benefits varies around the reform for affected workers:

$$\hat{B} = \left| \sum_{d=6}^{\bar{d}} (\hat{n}_{ad}^{pre} - n_{ad}^{pre}) \right|. \quad (\text{B2})$$

Similarly, we define the missing mass below the threshold as

$$\hat{M} = \sum_{d=0}^5 (\hat{n}_{ad}^{pre} - n_{ad}^{pre}). \quad (\text{B3})$$

To facilitate the interpretation of the results, we report them as layoff rates, i.e. relative to the total number of affected workers in the respective tenure range. Specifically, we report the bunching estimate as \hat{B}/N_{6+}^{pre} , and the missing mass as \hat{M}/N_{6+}^{pre} , where N_{6+}^{pre} is the sum of all affected workers in the pre-reform affected tenure range $[6, \bar{d}]$. This allows us to directly compare the estimates to our benchmark estimates based on equation 2. We calculate standard errors for all estimated parameters by bootstrapping from the observed sample of layoffs, drawing 100 random samples with replacement and re-estimating the parameters at each iteration.

Finally, for our main analysis, we set $\bar{d} = 11$. This is motivated by the fact that this is the highest tenure threshold at which all affected workers are affected by the UI reform. In addition, we replicate the estimation results for $\bar{d} = 17$, which is the highest tenure range for which some of the affected workers are affected by the reform.