Unemployment Insurance as a Subsidy to Risky Firms*

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Abstract

We document that a more generous unemployment insurance (UI) system shifts labor supply from safer to riskier firms and reduces the compensating wage differential that firms exposing workers to higher unemployment risk need to pay. Reallocation of labor supply towards riskier firms has real implications for firm performance and risk-taking. We find that a more generous UI system increases risky firms' value and fosters entrepreneurship by reducing labor costs for new firms. Exploiting a UI reform in Brazil that affects only part of the workforce allows us to compare labor supply for workers with different degrees of UI protection within the same firm, sharpening the identification of the results. Altogether, our results suggest that UI provides a transfer system from safe to risky firms.

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1 Introduction

Risk-taking in pursuit of profitable investment opportunities is a fundamental driver of economic growth (Acemoglu and Zilibotti, 1997; Levine and Zervos, 1998). It is often argued that individuals' risk-aversion reduces corporate risk-taking below its optimal level. For example, risk-aversion of managers and other employees may restrict firms' ability to engage in profitable but risky financing and investment strategies (Jensen and Meckling, 1976; Amihud and Lev, 1981; Smith and Stulz, 1985; Holmström, 1999; Murphy, 1999; Berk et al., 2010; Gormley and Matsa, 2016).

Firms can devise mechanisms to mitigate the impact of employees' risk preferences on firm value. For example, they can design contracts to insure employees against income risk. However, such mechanisms are limited in scope to insure employees against all business risks. In particular, firms cannot fully insure workers against unemployment risk, since layoffs often occur in states in which firms lack resources to compensate employees (Lamadon, 2014; Fagereng et al., 2017). Consistent with this view, workers and executives demand a wage premium for exposure to unemployment risk (Abowd and Ashenfelter, 1981; Topel, 1984; Li, 1986; Rosen, 1986; Hamermesh and Wolfe, 1990; Peters and Wagner, 2014), which is akin to a tax on corporate activities that increase unemployment risk.

In this paper, we assess whether a policy externality of unemployment insurance (UI) is to increases workers' tolerance to corporate risk-taking. We show that a more generous UI system reallocates labor supply towards riskier firms and reduces the compensating wage differential risky firms need to pay. Additionally, a more generous UI system encourages entrepreneurship by reducing new firms' labor costs. Combined with evidence that a reduction in UI generosity reduces risky firms' value, these results suggest that UI acts as a subsidy to risky firms by lowering their labor costs.

Detecting how changes in UI reallocate labor supply in the economy is beset by challenges. Most notably, firm characteristics and the design of UI systems are endogenous and may correlate with confounding factors that affect cross-sectional and time-series variation in employment and wages. For example, UI duration is often extended during recessions. Additionally, differences in employment and wages are jointly determined by the supply of and demand for labor, which are difficult to disentangle in the data.

To overcome these challenges, we exploit a sudden and unexpected UI reform in Brazil in 2015 that tightened eligibility criteria for about 60 percent of workers. Prior to the reform, all laid-off workers were eligible for UI benefits if they had been employed for at least six consecutive months prior to layoff. The reform tightened eligibility criteria for workers with less than two previous UI benefits spells. Specifically, after the reform, first (second) time

applicants for UI benefits require formal employment for at least 18 (12) months during the previous 24 (16) months to qualify (see Figure 1). Eligibility criteria remained unchanged for workers with at least two previous UI benefits spells.

In our empirical analysis, we compare changes in labor supply to risky and safe firms in response to a tightening of UI eligibility criteria. Since firms employ workers affected and workers unaffected by the reform, we can compare changes in labor supply around the reform for both groups of workers within the same firm. This triple-difference estimation with unaffected workers as a control group allows us to control for firm-time fixed effects, which rules out confounding factors related to firm-level changes in labor supply and demand.

We observe a reduction in labor supply for workers whose eligibility criteria for UI are tightened by the reform. In our strictest specification comparing workers around the threshold at which the reform applies (one vs. two prior UI benefits spells), we find that employment drops by 2.18 percent and wages increase by 0.52 percent for workers whose eligibility criteria are tightened relative to workers unaffected by the reform. Similarly, hiring rates of workers affected by the reform decline by 0.34 percentage points per month and hiring wages increase by 1.34 percent relative to workers unaffected by the reform.

Comparing changes in labor supply for risk and safe firms, we observe a relative decline in employment of workers affected by the reform by 0.24 to 0.48 percentage points and a relative increase in wages by 0.08 to 0.18 percentage points per risk decile.[1] We find the same patterns for hiring rates and wages. Further consistent with a reallocation of labor supply from riskier to safer firms when UI is less generous, we observe that workers affected by the reform become more likely to transition from riskier to safer firms after the reform.

The surprise announcement of the UI reform allows us to quantify the value of UI for risky relative to safe firms in an event study. In the five days around the announcement of the reform, firms with above median levels of risk experience about 1.2-2.3 percent lower returns than safe firms in line with the estimated increase in labor costs for risky firms. This suggests that a more generous UI system benefits risky relative to safe firms.

We complement our analysis with a set of robustness tests that strengthen the interpretation of our findings. Our triple-difference estimation implies that any confounding factor would not only have to differentially affect labor supply of workers affected and workers unaffected by the reform, but this difference would also need to vary with firm risk. Additionally, any alternative explanation would need to account for the sharp response of employment and wages after the announcement of the reform.

First, to mitigate concerns that workers with one additional UI spell differ in terms of

[1] We compute firm-level unemployment risk through layoff intensities and measures of credit risk.

characteristics that affect their labor supply around the reform, we compare changes in labor supply for workers around the one vs. two previous UI benefits spells threshold to changes in labor supply for workers around adjacent thresholds (two vs. three and three vs. four spells). We find that labor supply of worker with one additional UI benefits spell does not systematically decline for risky relative to safe firms around the reform except at the threshold at which the reform applies.[2]

Second, we assess whether the macro-economic environment affects our results. During our sample period Brazil experienced a recession that deepened after the reform. To explain our results, employment and wages of workers affected and unaffected by the reform would need to change differentially in response to macro-economic shocks. Additionally, these different would need to vary with firm risk. Differences in labor demand for affected and unaffected workers during recessions cannot explain the results since employment relatively declines but wages increase for workers affected by the reform. Instead, labor *supply* of workers affected and workers unaffected by the reform would need to respond differently during an economic downturn and for safe and risky firms. To assess this possibility, we examine changes in employment and wages for both groups of workers around the period during which the Brazilian economy was affected by the global financial crisis in 2008. We find that neither employment nor wages respond differentially for workers affected and workers unaffected by the reform in safe and risky firms. This suggests that labor supply of workers affected and workers unaffected by the reform does not differentially change for safe and risky firms during recessions.

Third, we ensure that our results are not affected by changes in the composition of workers around the reform. Replicating our wage analysis at the individual worker level allows us to control for worker fixed effects. We find qualitatively and quantitatively identical results, which suggests that our results are not affected by changes in the composition of workers for riskier and safer firms.

Fourth, to ensure that our results are not affected by characteristics correlated with firm risk, we instrument for layoff risk by links to suppliers exposed to adverse weather shocks. Fazio, Silva and Skrastins (2019) document that customers linked to suppliers exposed to a weather shock experience a drop in cash flows and employment of thirteen and six percent, respectively. We confirm that these shocks affect layoff risk in the long term. We find qualitatively identical results using this instrument variable strategy.

Finally, our results are robust to defining layoff risk at the firm-occupation level, controlling for workers' wage growth profiles, and are present in areas with above and below

[2]Our results are also robust to controlling for differences in observable characteristics, such as age, tenure, and occupation.

median levels of labor market informality.

In the second part of the paper, we examine whether UI generosity has real implications for risk-taking. We focus on changes in entrepreneurship, which constitutes an inherently risky endeavor and is easier to measure than changes in risk-taking at the firm level.

We find that making it harder for workers to qualify for UI benefits reduces new business creation. First, a simple entitlement effect suggests that individuals for whom it becomes harder to qualify for UI benefits are more likely to start a new business when formal employment becomes less attractive (Hamermesh 1979). Consistent with this conjecture, we find that workers affected by the reform become 3.38 percent more likely to start a new business relative to workers unaffected by the reform. Second, since the reform increases risky firms' labor costs, starting a new business becomes less profitable. Accordingly, we find that individuals are 1.21 percent less likely to start a new business per ten percentage point increase in the share of workers in a given municipality experiencing a reduction in UI benefits. Lower rates of business creation in municipalities with a larger share of affected workers suggest that the labor cost channel dominates the entitlement effect. Finally, we find that the average quality of new businesses increases after the reform, suggesting that tightening access to UI benefits prevents the creation of marginal firms.

Together, our results suggest that a less generous UI system reallocates labor supply from risky to safe firms, forcing risky firms to pay higher wages to attract workers. Higher labor costs reduce risky firms' value and discourage new business formation. Thus, the UI system acts as a subsidy to risky firms' labor costs and encourages risk-taking in the economy. Since UI systems are typically financed through taxes on labor, they constitute a transfer system from safer to riskier firms if UI is not perfectly experience-rated.

While recent empirical studies provide evidence consistent with a link between firm-level unemployment risk and labor supply (Brown and Matsa, 2016; Baghai et al., 2019), evidence on equilibrium employment and wage effects is scant. Brown and Matsa (2016) exploit data on job applications, which allows them to isolate labor supply responses to perceived unemployment risk. Comparing applications to firms with different levels of unemployment risk, they show that higher riskiness of firms is associated with fewer job applications. Similarly, Baghai et al. (2019) document that a reduction in labor supply manifests through skilled workers departing distressed firms at a higher rate.

Our setting allows us to examine the equilibrium effects of changes in labor supply in response to firm risk on real outcomes: employment, wages, firm value, and entrepreneurship. Faced with a reduction in labor supply, it is unclear to what extent risky firms adjust their employment and wages, since this depends on their labor demand elasticity.[3] Our results

[3]It is also possible that fewer job application rates have no detectable effect if labor supply is abundant.

suggest that firms adjust both margins. They pay higher wages when workers demand higher compensation for unemployment risk, but are unwilling to fully compensate all workers and rather accept a drop in their workforce. Additionally, we document that a contraction in labor supply has a negative effect on risky firms' value. Moreover, we show that workers' sensitivity to unemployment risk is relevant not only to existing firms but also in the decision to start new firms. When workers are more concerned about unemployment risk, fewer new businesses are created.[4]

Changes in workers' sensitivity to risk have direct implications for corporate risk-taking. Prior literature shows that risk-averse CEOs alter firms' financing and investment policies to reduce risk (Jensen and Meckling, 1976; Amihud and Lev, 1981; Tufano, 1996; Gormley and Matsa, 2016; Schoenherr and Starmans, 2019). More recent work argues that workers affect corporate risk-taking by altering their labor supply in response to changes in firm risk. For example, Agrawal and Matsa (2013) show that firms pursue safer financing strategies when workers are more sensitive to unemployment risk due to weaker UI protection. Our analysis shows that UI programs play an important role in reducing workers' sensitivity to unemployment risk, allowing firms to engage in riskier financing and investment strategies.

We document that one channel through which workers' sensitivity to unemployment risk affects risk-taking in the economy is entrepreneurship. Hombert et al. (Forthcoming) show that providing UI to business owners affects their propensity to start a new business. We document two novel channels through which providing UI to a firm's employees rather than the owner of the business affects entrepreneurship. First, the provision of UI crowds out entrepreneurship through an entitlement effect that makes formal employment relatively more attractive. Second, UI reduces the labor costs of new businesses, which in turn makes starting a new business more profitable and spurs entrepreneurship. We find that the labor cost effect dominates leading to a positive relationship between UI provision and entrepreneurship.

Our findings also provide novel insights into the literature on the optimal UI design. The standard Baily-Chetty framework characterizes the optimal policy choice as a trade-off between adverse effects of UI on labor supply and welfare gains from protecting individuals against income shocks. Some recent studies show that UI has additional important implications. For instance, Hsu, Matsa and Melzer (2018) document that UI acts as a housing market stabilizer, Kuka (forthcoming) argues that access to UI improves health outcomes, and Barr and Turner (2015) argue that UI programs affect post-secondary enrollment choices. Our findings suggest that UI also creates a transfer system in which safe firms subsidize risky firms' labor costs and therefore subsidizes corporate risk-taking.

[4]Overall, our results suggest that the labor supply channel documented in Brown and Matsa (2016) and Baghai et al. (2019) has important real implications.

The rest of the paper is organized as follows. Section 2 describes the institutional background and data used for the empirical analysis in the paper. In Section 3 we develop testable hypotheses. The empirical analysis is presented in Section 4. Section 5 provides results from additional tests that assess the robustness of our findings. Section 6 concludes.

2 Institutional Background and Data

This section provides information about the UI system in Brazil, and describes the UI reform implemented in March 2015 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 twenty percent of the formal wage to finance the public pension system, plus eight and a half percent for the workers' 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), which is mainly financed through a 0.65 percent tax on annual firm sales.

UI benefits are paid for three to five months, depending on workers' time in formal employment. Three payments are made if a worker was employed between six and eleven months in the last 36 months, four payments are made if a worker was employed 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 wage. The UI system does not feature a direct experience rating mechanism. If a firm dismisses a worker without a justified reason, it must pay an additional fifty 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 wage. 80 percent of the penalty is directly paid to the worker, and 20 percent goes towards funding the UI system. Firing workers with a valid legal justification does not involve 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 six months prior to layoff, may not earn other labor income, and may not have successfully applied for UI benefits during the previous 16 months. On December 30, 2014, the parliament passed a provisional measure that tightened eligibility criteria for UI benefits. 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.[5] The main driver for the quick implementation and the tightening in eligibility criteria was attempts on the government's part to reduce the growing budget deficit. The size and duration of UI benefits were not altered. Importantly, UI contributions of employers were unaffected by the reform. Thus, the reform had no direct effect on employers' demand for formal labor.

The reform affected the about 60 percent of workers with less than two successful past applications for UI benefits. For these workers, eligibility criteria were tightened. 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 turned into law in July 2015 with some adjustments. The 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 nine months of employment in the last 12 months.

To estimate the effect of the reform for the average worker, we compute the reduction in expected UI benefits. Before the reform, the probability of being laid off with a tenure of 6 to 17 months (which no longer qualifies workers for UI benefits after the reform) is 0.1549. Given that the average worker qualifies for 4.51 months of UI benefits with a replacement rate of 0.6661, this means that the drop in expected UI benefits amounts to $0.1549 * 4.51 * 0.6661 = 0.4653$ monthly wages. For workers whose eligibility criteria change from 6 to 12 months, the equivalent value is 0.2863 monthly wages based on a 0.0953 probability of being laid off with a tenure between 6 to 11 months. For the adjusted eligibility criteria from July 2015 that required workers with one previous UI spell to be employed for nine months, the expected drop in UI benefits is 0.1595 monthly wages based on an expected layoff probability of 0.0531 with a tenure of six to eight months. In our empirical analysis, we relate these values to observed changes in wages and firm value.

[5] Estadao Politica, December 29, 2014, "Forca Sindical nega ter sido consultada sobre ajuste em beneficios".

2.3 Data

Our main data on employment and wage measures stems from RAIS (Relação Anual de Informações Sociais), a large restricted-access matched employee-employer administrative data set from Brazil. The RAIS database records information on all formally employed workers and is maintained by the Ministry of Economics in Brazil. All 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 (wage, tenure, type of employment, hiring date, layoff date, reason for layoff, etc.). We use data from RAIS for the period from 2013–2016. In a given year, RAIS covers about 50 million formal employees.

The median firm in RAIS has three employees. For these firms it is not possible to compute the risk measures that are central to our analysis. Additionally, in our sharpest specification, we require firms to employ or hire workers with exactly one and exactly two prior UI benefits spells within the same occupation or age group. To satisfy these requirements, we restrict our sample to firms with 100 or more workers, which are classified as large firms in Brazil. For these firms, we are able to compute the firm risk measures and explore the cross-sectional variation in employees required for our analysis (see Section 4 for details).

We combine data from RAIS with information on the number of previous unemployment spells with UI benefits payments maintained by the Ministry of Labor. This information is crucial, as the reform only applies to workers with less than two past UI benefits spells. We exclude public sector employees, since they do not participate in the UI program. Additionally, we use information on the location of firms (municipality), firms' two-digit industry classification (National Classification of Economic Activities), and information on workers' occupations (Classi cação Brasileira de Ocupaçõ).

For our analysis, we need to classify firms according to the unemployment risk to which their workers are exposed. We compute three proxies of firm risk. The first proxy directly captures layoff risk. For each firm, we compute the probability of a newly hired worker being laid off within 12 months during the pre-reform period from 2013 to 2014. For our other proxies, we obtain information on all outstanding corporate loans recorded by SCR (Sistema de Informações de Crédito do Banco Central), a restricted-access credit registry managed by the Central Bank of Brazil. This provides us with market-based proxies of firm risk through credit spreads and provisions on loans that banks have to hold with the Central Bank based on the assessed risk of a given borrower.

For our instrumental variable strategy, we rely on information about firms' suppliers,

which we identify using transaction-level data from the Brazilian Payment Systems (the Sistema de Transferência de Reservas (STR) and Sistema de Transferência de Fundos (CIP-Sitraf). We classify firms as suppliers based on the direction of observed money transfers.

We also use two data sets from the Brazilian IRS (Receita Federal). One data set covers information about all formal businesses in Brazil (Cadastro Nacional de Pessoas Jurídicas), from which we extract information on ownership, and opening and closing dates.[6] The other data set, the population registry of Brazil (Cadastro de Pessoas Físicas), provides information about individuals' residence.

3 Hypothesis Development

This section provides a simple model to illustrate the main effects of the UI reform in Brazil on labor supply, wages, firm value, and entrepreneurship. We use the insights from the model to develop testable hypotheses.

3.1 Employment and Wages

Suppose that in a given period workers receive a formal job offer and are endowed with an outside option that they can resort to if they reject the job offer, for example an informal job or self-employment. Job offers consist of two parameters: layoff risk λ associated with the firm making the offer and a wage w. If workers accept the job offer, they earn the wage w with probability (1-λ) and are laid off with probability λ, in which case they receive an UI benefit B. The level of UI benefits B is a fraction of the previous wage and thus $w > B$. If workers reject the job offer, they enjoy a utility of v from their outside option. Thus, the condition for workers to accept a job offer is $(1-\lambda)w + \lambda B \geq v$, which implies that the reservation wage w_R, the lowest wage a worker is willing to accept, is given by $w_R = \frac{v-\lambda B}{(1-\lambda)}$.

The reservation wage for a worker is higher for firms with a higher level of layoff risk λ. Intuitively, an increase in λ shifts probability from the higher income wage state to the lower income unemployment state, reducing the value of formal employment by $\lambda(w-B)$. This drop in the value of employment needs to be compensated for by a higher wage.[7] A higher level of UI benefits reduces the worker's reservation wage by increasing the value of formal employment.

[6] The Recita Federal lists the legal representative of a company, typically its CEO. For newly formed firms, which we examine in our analysis, the founder is the legal representative in virtually all cases.

[7] We assume workers to be risk-neutral. If workers were risk-averse, the wage differential for unemployment risk would be larger.

The main effect of the UI reform is to reduce the probability of qualifying for UI for a subset of workers. Thus, for these workers the expected value of UI benefits is lower: βB, with $\beta < 1$. This changes workers' reservation wage to $w'_R = \frac{v-\lambda\beta B}{(1-\lambda)} > \frac{v-\lambda B}{(1-\lambda)} = w_R$.

The difference in the new and old reservation wages for workers affected by the reform is $\frac{(1-\beta)\lambda B}{(1-\lambda)}$. From this, it follows that the reservation wage increases more, the lower the probability of qualifying for UI benefits (β) is. Additionally, the difference is increasing in λ. Thus, the reservation wage for workers affected by the UI reform increases with layoff risk. Intuitively, for firms with higher layoff risk workers are more likely to experience unemployment states and are therefore more sensitive to the reduction in expected UI benefits payments.

These insights imply two testable implications:

Hypothesis 1 (H1): *Workers are less likely to accept formal employment and demand higher wages after the reform when it is harder to qualify for UI benefits.*

Hypothesis 2 (H2): *Firms with higher layoff risk are more affected by lower labor supply and higher wage demands after the reform when it is harder to qualify for UI benefits.*

3.2 Firm Value

Our model can be extended to illustrate the effect of reduced access to UI on firm value and entrepreneurship. A firm's profits π can be described as the difference between the revenues generated from its output and the cost of its inputs: $\pi = f(K, L)P - rK - wL$, where $f(K, L)$ denotes the firm's output, P is the price of its output, K and L are the input capital and labor, respectively. The variables r and w denote the cost of capital and labor. As discussed above, the cost of labor $w = \frac{v-\lambda B}{(1-\lambda)}$ is a negative function of expected UI benefits payments conditional on layoff. Thus, a drop in expected UI benefits that leads to an increase in wages reduces firms' profitability. The importance of this effect is positively correlated with the riskiness of the firm (λ), which implies that the UI reform which reduces access to UI for some workers has a more negative effect on profits and hence on firm value for riskier firms:

Hypothesis 3 (H3): *The value of risky firms relatively declines following the announcement of the reform that tightens UI eligibility criteria.*

3.3 Entrepreneurship

Marginal individuals that decide to either start their own business or seek formal employment trade off the expected returns from entrepreneurship and formal employment. Thus, the

conditions for starting a new business before and after the UI reform can be described as:

$$pre-reform: f(K,L)P - rK - \frac{v-\lambda B}{(1-\lambda)}L \geq (1-\lambda)w + \lambda B, \tag{1}$$

$$post-reform_{affected}: f(K,L)P - rK - \frac{v-\lambda\beta B}{(1-\lambda)}L \geq (1-\lambda)w + \lambda\beta B, \tag{2}$$

$$post-reform_{unaffected}: f(K,L)P - rK - \frac{v-\lambda\beta B}{(1-\lambda)}L \geq (1-\lambda)w + \lambda B, \tag{3}$$

where (1) describes the inequality for all workers before the reform, and (2) and (3) describe the inequality for workers affected and unaffected by the reform, respectively.

As evident from comparing equations (1) and (2), the UI reform has two effects on individuals' decision to start a new business. Reducing the expected value of UI benefits by a factor $\beta < 1$ makes formal employment less valuable for individuals affected by the reform, which implies an increase in entrepreneurship. On the other hand, running a new business with employees is less profitable due to the higher wages that workers demand after the reform, in particular for high-risk firms, such as new businesses.[8] This implies that fewer businesses are created after the reform. For individuals unaffected by the reform, only the profitability effect is relevant (equation (3)), which suggests that these individuals become less likely to start a new business after the reform. The insights from equations (1) to (3) provide us with two testable implications:

Hypothesis 4 (H4): *Workers for whom it is harder to qualify for UI benefits through formal employment after the reform are relatively more likely to start a new business.*

Hypothesis 5 (H5): *Higher labor costs discourage the creation of new businesses after the reform.*

Our framework allows us to consider which type of new businesses are generated or disappear after the reform. Examining equation (3) suggests that the firms that are not created due to higher labor costs are those that would barely have been profitable at pre-reform levels of labor costs and are no longer profitable under higher labor costs after the reform. However, at the same time labor costs increase for all new businesses that hire workers reducing their profitability. Due to these opposing channels - positive selection and negative performance effects - the overall change in profitability of new businesses is ambiguous and depends on the relative strength of the two channels.

[8]The average wage that firms would be required to pay after the reform is a weighted average of the wage of affected and unaffected workers. For notational convenience, we set the post-reform wage equal to the wage of affected workers. This does not affect the qualitative insights.

Individuals for whom formal employment becomes relatively less attractive due to the reform would have started a very profitable business even before the reform when formal employment was more valuable. Thus, the additional businesses that are created by workers for whom formal employment becomes less attractive are those for which expected profits are relatively lower. This suggests that

Hypothesis 6 (H6): *The quality of businesses created by workers for whom it becomes harder to qualify for UI benefits relatively declines after the reform.*

4 Empirical Analysis

This section presents our empirical analysis to test the hypotheses developed in the previous section, assessing the impact of UI on employment, wages, firm value, and entrepreneurship.

4.1 Descriptive Statistics

Before discussing our empirical strategy and results, we present descriptive statistics. In Table 1, Panel A, we report descriptive statistics separately for workers affected and workers unaffected by the reform. The last column shows the difference between both groups. All values are based on December 2014, the month in which the UI reform was announced.

Workers affected by the reform account for 69.36 percent of workers in the sample. The difference in wages between workers affected and unaffected by the reform is BRL 27 and not statistically significant. Similarly, wages for newly hired workers are almost identical. We do not observe significant differences in workers' education or the industries in which they are employed. Workers affected by the reform are 4.8 years younger, have 3.6 months longer tenure at their current job, and are 3 percent more likely to work in skilled occupations. We ensure that our analysis is not affected by these differences in our empirical design and additional robustness tests.

Panel B reports descriptive statistics for the firm-level measures that proxy for unemployment risk: the layoff rate within twelve months of hiring, the spread on debt obligations, and default provisions that banks are required to hold at the Central Bank on a firms' debt. On average, firms lay off 18 percent of workers within twelve months of hiring. The highest spread a firm pays on its debt is 120 percent on average, and banks on average hold 8 percent of the outstanding debt with the Central Bank in default provisions.

4.2 Employment and Wages

We begin our analysis by examining the relationship between access to UI and labor supply, and its differential effect on safe and risky firms by testing Hypotheses (1) and (2).

4.2.1 Basic Reform Effect

To examine whether workers reduce their formal labor supply when it becomes harder to qualify for UI benefits, we compare changes in employment and wages for workers who face tighter eligibility criteria after the reform and workers who are unaffected by the reform.

Hypothesis (1) predicts that employment declines and wages increase for workers affected by the reform. We start by depicting the time series of employment (top left panel) and wages (top right panel) in Figure 2 for workers affected (solid lines) and for workers unaffected by the reform (dashed lines).[9] While employment for both groups of workers follows parallel trends before the reform, employment of workers affected by the reform sharpely drops from the first month after the announcement of the reform. Similarly, wages of affected workers relatively increase from the month after the announcement of the reform.[10] The same trends are present for hiring rates and hiring wages (bottom panels). Together, the trends in employment and wages are in line with a reduction in formal labor supply for workers affected by the reform, consistent with Hypothesis (1).

We confirm the insights from the graphical analysis statistically by estimating

$$ERate_{ij,age,t} = \alpha_{it} + \alpha_{ij} + \alpha_{age,t} + \beta \cdot Affected_j * Reform_t + \epsilon_{ij,age,t}, \quad (4)$$

where subscript i denotes firms, j denotes worker groups, and t denotes time in months. Since we are interested in firm-level outcomes, we aggregate outcomes for both affected and unaffected workers at the firm level. This captures net changes in firm-level employment taking into account employment separations and hirings. Additionally, as workers affected by the reform tend to be younger, we group workers in age groups and saturate equation (4) with age group-month fixed effects $\alpha_{age,t}$.[11] Thus, in a given month we compute the average outcome for each firm and worker-age group, separately for workers affected and workers unaffected by the reform. $ERate_{ij,age,t}$ measures employment normalized by firm i's employment in the month before the announcement of the reform. The dummy variable $Affected_j$ takes the value of one for the group of workers affected by the reform and zero

[9] All plots are adjusted for calendar month-worker group fixed effects to control for seasonal effects.

[10] In Brazil, firms are free to engage in bilateral negotiations with workers, provided that wages remain above the level agreed on in collective bargaining. This allows for variation in wages at the individual level.

[11] We split workers into six age groups: below 20, 20-29, 30-39, 40-49, 50-59, 60 and above.

for workers unaffected by the reform. The dummy variable $Reform_t$ takes the value of one for the post-reform period in 2015 and 2016 and zero for the pre-reform period in 2013 and 2014.

Our empirical setting allows us to include firm-time fixed effects (α_{it}). This eliminates concerns about time-varying confounding factors that may affect labor supply, labor demand, or wages at the firm level (e.g., macroeconomic shocks, productivity shocks). Thus, the variation we exploit stems from differential changes in employment and wages for workers affected and workers unaffected by the reform within the same firm and month and in the same age group, captured by β. We additionally include firm-worker group fixed effects (α_{ij}) to ensure that we track changes in employment and wages for the same firm-worker group pair over time.

The results are reported in Table 2, Panel A. We find that employment relatively decreases by about 6 percent for workers affected by the reform (column I). On examining changes in wages, we find that wages of workers affected by the reform relatively increase by 0.90 percent (column II). We observe similar effects for hired workers with a 0.52 percent relative decrease in monthly hiring rates scaled by total firm employment for workers affected by the reform (column III), and a 2.52 percent relative increase in hiring wages (column IV).

To mitigate concerns about differenes in characteristics of workers affected and unaffected by the reform, we restrict our sample to workers around the threshold at which the reform applies in Panel B. Specifically, we compare workers with exactly one previous unemployment spell who are affected by the reform and workers with exactly two previous UI benefits spells who are not affected by the reform. We find qualitatively identical effects as for the full sample. The magnitudes around the threshold are about half as large as for the full sample. The difference in magnitudes is driven by the fact that the drop in expected UI benefits is about half as large for workers with one prior UI spell compared with workers with no prior UI spell, which implies that the effect of the reform is only half as strong for workers with exactly one prior UI benefits spell (see Section 2.2).

4.2.2 Firm Risk

We next turn to testing Hypothesis (2), which predicts that labor supply of affected workers declines more for riskier firms after the reform.[12] Figure 3 depicts the time-series of employment (top left panel), wages (top right panel), hiring (bottom left panel), and hiring wages (bottom right panel) separately for firms in the highest risk quartile (solid lines) and firms in

[12] A necessary condition for workers to be sensitive to firm risk is that they perceive it. Brown and Matsa (2016) provide survey evidence that workers' perception of the riskiness of firms is strongly correlated with various measures of financial health.

the lower risk quartile (dashed lines).[13] The plots depict the difference in outcomes for the group of workers affected by the reform and workers unaffected by the reform. For example, the top left plot shows that while employment of workers affected by the reform decreases relative to employment of workers unaffected by the reform for all firms, the relative decline is stronger for risky firms. Altogether, the plots provide evidence of a stronger decline in employment and increase in wages for workers affected by the reform in risky firms.

We confirm the patterns from the graphical analysis by estimating

$$\begin{aligned} ERate_{ij,age,t} &= +\alpha_{it} + \alpha_{ij} + \alpha_{age,t} + \beta \cdot Affected_j * Reform_t \\ &\quad +\delta \cdot Risk_i * Affected_j * Reform_t + \epsilon_{ij,age,t}. \end{aligned} \tag{5}$$

Since workers are unlikely to track the exact value of risk measures, but rather have a broad sense of a firm's conditions, $Risk_i$ splits firms into deciles according to each of the proxies of firm risk (see Section 4.1).

The results are displayed in Table 3. We observe that employment of workers affected by the reform decreases by 27 to 67 basis points per risk decile relative to workers unaffected by the reform (odd columns). Wages for workers affected by the reform relatively increase by 6 to 29 basis points per risk decile after the reform (even columns). The results are robust to focusing on workers around the threshold at which the reform applies in Panel B.

Risky firms are not substituting the drop in employment of workers affected by the reform by employing more workers unaffected by the reform. In Table A.1, we show that total employment at risky firms drops in accordance with the reduction in labor supply of workers affected by the reform. For example, the 0.31 percent relative decline in employment for high credit spread firms after the reform is consistent with the drop in employment of workers affected by the reform. Employment of workers affected by the reform declines 0.27 percent more compared with workers affected by the reform for high credit spread firms (Table 3, column I). Thus, the relative change in employment of workers unaffected by the reform x can be computed from (x+(x-0.0027))=-0.0031, which yields x=0.02. Thus, employment of workers unaffected by the reform in risky firms remains stable after the reform with a 0.02 percentage point decline per risk decile compared to a 0.27 percentage point decline for affected workers.[14]

In Table 4 we examine relative changes in hiring and hiring wages for safe and risky firms around the reform. After the reform, monthly hiring of workers affected by the reform declines by 0.01 to 0.02 percentage points (about 1 percent) relative to the existing workforce

[13] While these graphs are based on the credit risk measure, we observe the same patterns for all risk proxies.

[14] Relative employment changes of affected and unaffected workers in safe and risky firms is similar around the reform for all risk measures.

per risk decile (odd columns). Hiring wages of workers affected by the reform relatively increase by 0.09 to 0.16 percent per risk decile after the reform (even columns). The results are robust to focusing on workers around the threshold at which the reform applies in Panel B.

To provide additional insights into the reallocation of labor supply, we examine transitions of workers across firms. Specifically, we examine whether workers affected by the reform become more likely to transition from risker to safer firms by estimating

$$\begin{aligned} \Delta Risk_{ij,age,t} &= \alpha_{it} + \alpha_{ij} + \alpha_{age,t} + \beta \cdot Affected_j * Reform_t \\ &\quad + \rho \cdot Affected_j * Reform_t * Risk_i + \epsilon_{ij,age,t}, \end{aligned} \tag{6}$$

where $\Delta Risk_{ij,age,t}$ is the fraction of workers that transitions to a safer firm after parting with firm i in month t. The sample comprises all workers who experience separation from their employer during our sample period. α_{it} denotes firm-month fixed effects, which ensures that the variation we exploit stems from workers separating from the same firm in the same month. All other variables are defined as before.

The results are shown in Table 5. We find that workers who become less likely to qualify for UI benefits after the reform become 0.21 to 0.26 percentage points more likely to transition to safer firms (odd columns). Interacting the independent variable in equation (6) with $Risk_i$, we find that workers become particularly more likely to transition to safer firms if they were previously employed at risky firms (even columns). The evidence on transitions at the worker level strengthens our interpretation of the results that a reduction in access to UI benefits leads to a reallocation of labor supply from riskier to safer firms.

Overall, the results in this section support Hypothesis (2) that reducing access to UI benefits reallocates formal labor supply from riskier to safer firms.[15]

4.2.3 Pass-Through

Next, we relate the observed changes in wages for workers affected by the reform to the expected drop in UI benefits. In Section 2.2, we calculate the expected reduction in UI benefits to be 0.2863 monthly wages for workers whose eligibility criteria change from six to twelve months of employment after the reform and 0.1595 monthly wages for workers whose eligibility criteria change from six to nine months of employment. In the data, the expected employment duration is 13.24 months. Thus, if workers seek to be fully compensated for the expected loss in UI benefits over their expected employment period, they need to demand

[15] Our wage results also imply that differential access to social security systems may lead to wage dispersion within the same firm for reasons other than productivity (Mueller et al. 2017).

(0.2863/13.24=)2.16 percent or (0.1595/13.24=)1.20 percent higher wages, respectively.

We compare these values to the observed changes in wages estimated in Table 2. Since 69.36 percent of the workers affected by the reform in our sample require 12 months of employment to qualify for UI benefits after the reform and 30.64 percent require nine months of employment, the required increase in wages to offset the expected loss in UI benefits amounts to (0.6936*0.0216+0.3064*0.0120)=0.0187. The observed change in wages for newly hired workers is about somewhat higher with 0.0252 (column IV, Panel A). Similarly, the change in hiring wages for workers with one previous UI spell, who require nine months of employment to qualify for UI after the reform, is slightly higher with 1.34 percent (column IV, Panel B) than the required 1.20 percent to recover the expected loss in UI benefits. This suggests that fluctuations in UI protection fully pass through into equilibrium wages. If workers are risk-averse, the pass-through can exceed one to one.

The probability of being laid off during the tenure range that no longer qualifies workers for UI benefits after the reform varies with firm risk. For example, the probability of being laid off with a tenure of six to eleven months is 0.0139 for the safest decile of firms, but 0.2128 for the riskiest decile of firms. When we update our estimates, adjusting for observed layoff probabilities, UI benefits duration, and replacement rates for workers in firms in different risk deciles, we get that workers with no previous UI benefits spell would require a 0.58 percent higher increase in wages per risk decile to be compensated for the additional expected loss in UI benefits in riskier firms after the reform (0.35 percent for workers with one previous UI benefits spell).

Our estimates in Table 4 allow us to assess whether these cross-sectional differences in unemployment risk at the firm level are reflected in differential wage responses to changes in eligibility rules for UI benefits. For the full sample, the required wage increase per risk decile after the reform to offset the additional unemployment risk at riskier firms is (0.6936*0.58+0.3064*0.35)=0.51 percent. Our estimate is somewhat lower at 0.29 percent (column VI, Panel A). Similarly, for workers with one previous unemployment spell, the estimate of 0.18 percent (column VI, Panel B) is lower than the 0.35 percent that would be required to offset the additional expected loss in UI benefits per risk decile. This suggests that UI reduces compensating wage differentials that firms are required to pay to workers to compensate them for higher layoff risk by about 51-57 cents per dollar in expected UI benefits. A possible explanation for the lower pass-through with respect to unemployment risk could be that workers underestimate differences in unemployment risk across firms.

4.2.4 Robustness

We next turn to strengthening the interpretation of our main results by assessing their robustness against potential alternative explanations.

Prior UI Benefits Spells In our tightest specification, we compare workers with exactly one or two prior UI benefits spells. A potential concern with this analysis is that workers that experienced one more UI benefits spells differ from workers with one viewer UI benefits spell. To dispel such concerns, we compare changes in labor supply for workers with one additional UI benefits spell around the one-vs-two-spells threshold at which the reform applies and workers with one additional UI benefits spell around adjacent thresholds that are not affected by the reform (two vs. three and three vs. four spells). Specifically, we control for changes in labor supply for workers with one additional previous UI benefits spell by estimating

$$\begin{aligned} ERate_{ij,age,t} &= \alpha_{it} + \alpha_{ij} + \beta_1 \cdot UPeriods_j * Reform_t + \beta_2 \cdot Affected_j * Reform_t \\ &\quad + \beta_3 \cdot UPeriods_j * Reform_t * Risk_i + \beta_4 \cdot Affected_j * Reform_t * Risk_i \\ &\quad + \epsilon_{ij,age,t}, \end{aligned} \tag{7}$$

for the sample of workers with one to four previous unemployment spells, where $UPeriods_j$ measures the number of prior UI benefits spells, all other variables are defined as before.

The results are collected in Table 6, Panel A. Across all risk proxies, we find no evidence that an additional UI benefits spell is associated with lower labor supply to risky firms other than at the one-vs-two-prior-UI-spells threshold. If anything, an additional UI benefits spell at other thresholds is associated with *higher* employment of workers affected by the reform in riskier firms. Hence, controlling for effects of an additional UI benefits spell on employment and wages does not affect the estimates for workers around the reform threshold.

Macro-Economic Conditions Next, we assess whether our results are affected by macro-economic conditions. Brazil was in recession for most of our sample period. To affect the results, labor *supply* of workers affected and workers unaffected by the reform would have to react differentially to macro-economic shocks. Differential labor demand effects cannot explain our results, since demand effects would imply employment and wages to change in the same direction. For example, if workers affected by the reform are more adversely affected by a recession, both their employment and wages should decline. In contrast, we observe that employment and wages change in opposite directions compared with workers unaffected by the reform.

From the outset, there is no clear reason for why workers who received UI benefits one

more time change their labor *supply* differentially in response to macro-economic shocks and do so only for risky firms. To directly assess changes in labor supply of workers affected and workers unaffected by the reform in response to macro-economic conditions, we turn to the last recession when Brazil's economy was affected by the global financial crisis.

The results are gathered in Table 6, Panel B. We define the crisis period as the nine month of negative growth from October 2008 to June 2009, and use a symmetric nine months window from January 2008 to Septmber 2008 as the pre-crisis period. We do not observe a differential response of employment or wages for workers affected and unaffected by the UI reform for risky and safe firms. This suggests that labor supply to risky and safe firms does not differentially respond to macro-economic shocks for workers affected and unaffected by the UI reform.

Worker Composition The composition of workers employed by risky and safe firms may change after the reform. In this case, changes in wages could be partially driven by changes in the composition of workers. To dispel this concern, we reestimate the wage and hiring wage tests in Tables 2 to 4 at the individual worker level, which allows us to control for worker fixed effects.

The results are shown in Table 7. We find that the result are almost identical after controlling for worker fixed effects (Panel A),[16] and for hiring wages after controlling for a hired workers' wage at their previous job, which is akin to controlling for worker fixed effects in specifications with hired workers (Panel B).

Exogenous Variation in Firm Risk Risk is an endogenous firm characteristic that may be correlated with other characteristics that could differentially affect labor supply for workers who are exposed to different levels of UI protection.

To mitigate this concern, we instrument for layoff risk is equation (5) using exogenous weather shocks.[17] Specifically, we instrument for layoff risk by links to suppliers that are exposed to adverse weather shocks during the pre-reform period by estimating

$$\widehat{Risk_i} = \gamma_1 \cdot shock_i + \gamma_2 \cdot exposure_i + \gamma_3 \cdot shock\ size_i + \gamma_4 \cdot log(employees)_i + \epsilon_i, \quad (8)$$

where $shock_i$ is a dummy variable that takes the value of one if a supplier of firm i experiences

[16] For computational constraints, we extract a 10 percent random sample of workers.

[17] Severe weather shocks are defined as natural disasters that are declared as an emergency by the federal government and cause at least BRL 100 million in damage. The data is provided by the Brazilian Integration Ministry. Fazio, Silva and Skrastins (2019) document that being connected to suppliers hit by unexpected weather shocks reduces firms' cash flows by 11 percent and their employment by six percent.

an adverse weather shock and zero otherwise,[18] $exposure_i$ is the share of firm i's total payments going to shocked suppliers, $shock\ size_i$ is the log of total damages caused by the natural disaster that affects firm i's suppliers, and $log(employees)_i$ is the log of firm i's employment.

The results are reported in Table A.2. The results in column I indicate that being connected to a supplier who experiences a natural disaster shifts firms up by 2.23 deciles in the distribution of layoff risk and by an additional 0.05 deciles per ten percentage point increase in the fraction of a firm's cash flows going to an affected supplier. After accounting for firm-specific exposure, the overall size of the damages in the area where the supplier is located has no positive effect on layoff risk. Finally, firm size has no effect on how layoff risk changes through connections to affected suppliers. On the one hand, larger firms are more likely to be affected because they rely on more suppliers. On the other hand, being connected to a larger number of suppliers makes them more diversified than smaller firms.

While we rely on the pre-reform period to predict layoff risk to steer clear of endogenous effects of the reform on firm risk, it is important for our strategy to be valid that shocks predict layoff risk in the long-run. The results in column II confirm that this is the case. Being linked to a supplier that is subject to a natural disaster during the pre-reform period predicts higher layoff risk even during the post-reform period.

The results from the second stage are displayed in Table 8. We find a 2.18 percent higher decline in employment (column I) and a 0.08 percent higher decline in hiring rates per risk decile for workers affected by the reform (column III). Wages and hiring wages of workers less likely to qualify for UI after the reform increase by 1.13 and 1.08 percent more per risk decile (columns II and IV). Observing qualitatively identical results relying on exogenous shocks to firms to proxy for firm risk mitigates concerns that the reduction in labor supply to risky firms after the reform may be driven by unobservable firm characteristics correlated with risk.

The estimates from the instrument variable strategy are about three to four times larger for employment and wages and about six to seven times larger for hiring rates and wages. Various factors could explain the difference in magnitudes. First, endogenous measures of firm risk based on layoff intensities and credit risk may be correlated with other firm characteristics that reduce workers' sensitivity to UI protection. Second, shocks to a supplier linked to a firm are salient events that may be easier to gauge than other dimensions of firms' risk, especially for outside workers. Third, workers with higher risk tolerance may sort into riskier firm making them less sensitive to UI protection. Since exogenous shocks to suppliers

[18]To identify firms connected to suppliers in the affected areas, we use inter-bank transfer data (STR), which records inter-bank payments between firms.

are not foreseen, sorting of workers with higher risk tolerance into affected firms is unlikely to have occurred. Thus, workers of firms that experience an exogenous shock to risk on might be more risk-averse on average than workers of firms whose riskiness builts up over time.

4.3 Firm Value

Hypothesis (3) predicts that higher labor costs associated with a reduction in access to UI benefits for some workers after the reform lead to a relative drop in value for risky firms.

The surprise announcement of the reform allows us to estimate the relative effect of tightening eligibility criteria for UI on the value of safe and risky firms. We conduct a standard event study comparing cumulative abnormal returns of safe and risky listed firms in a five trading day window around December 29, 2014 by estimating[19]

$$CAR_i \quad = \quad \alpha + \gamma \cdot Risk_i + \epsilon_i, \tag{9}$$

where CAR_i is the cumulative abnormal stock return of firm i estimated using a market model (MacKinlay 1997) with the Brazilian stock market index (BOVESPA) as the benchmark. The variable $Risk_i$ takes the value of one for firms with above median levels of risk and zero for firms with below median levels of risk.

Figure 4 depicts the distribution of cumulative abnormal returns of firms with above (solid lines) and below (dashed lines) median levels of credit spreads (left panel), default provisions (middle panel), and layoff risk (right panel) for the five day window around the announcement of the reform. There is a clear leftward shift in the return distribution for risky firms around the announcement of the UI reform for all risk proxies.

Measuring the magnitude of the effect in Table 9, we find that firms with above median levels of risk experience a 1.19 to 2.38 percent higher decline in firm value than safe firms following the announcement of the reform.[20] This suggests that the provision of UI has a differential impact on risky and safe firms. The generosity of the UI system acts as a subsidy to risky firms by reducing their labor costs as documented in the previous section, consistent with the predictions of Hypothesis (3).

Next, we relate changes in firm value to changes in wages following the announcement of the UI reform. To calculate implied changes in firm value from changes in wages, we compute firm value as a multiple of the earnings that the firm generates. In this case,

[19]The results are qualitatively identical for a three day window around December 29.

[20]Since many stocks are not frequently traded in Brazil, we depict the results for different thresholds of days that a stock is traded in 2014 (50 days in columns I, IV, and VII, 100 days in columns II, V, and VIII, and 200 days in columns III, VI, and IX.)

changes in earnings are proportionally reflected in the value of the firm, i.e., a permanent drop in earnings by one percent reduces firm value by one percent.

Our estimates suggest that wages increase by 0.90 percent for workers affected by the UI reform. Since for listed firms 74.03 percent of workers are affected by the reform, their wage bill increases by (0.0090*0.7403=) 0.67 percent. Wages account for 59 percent of listed firms' earnings in 2014 for the firms in our sample. This implies that a 0.67 percent increase in wages reduces earnings and therefore firm value by (0.59*0.0067=) 0.40 percent.

Since we estimate the relative change in firm value for high-risk and low-risk firms, we need to compute the implied change in firm value separately for firms with above and below median levels of risk. The average high-risk firm is five risk deciles above the average low-risk firm, which implies that wages for workers affected by the reform increase by (5*0.0029=) 1.45 percent more for risky firms where risk is measured as layoff risk (Table 3, column VI). Multiplied by the fraction of workers affected by the reform, this means that risky firms' wage bill increases by (0.0145*0.7403=) 1.07 percentage points more than for safe firms. Risky firms also feature lower earnings with wages accounting for 123 percent of their earnings, compared to 55 percent for safe firms.

Taking into account the differences in earnings and the higher increase in wages, high-risk firms' value drops by 1.53 percent more than for safe firms ([-0.0032+7.5*0.0029]*0.7403*1.23 = 0.0169 vs. [-0.0032+2.5*0.0029]*0.7403*0.55 = 0.0016). This estimate falls in the range of estimated changes in firm value for risky firms documented in Table 9.[21]

4.4 Entrepreneurship

Finally, we examine the effect of changes in the generosity of UI on entrepreneurship by testing Hypotheses (4) to (6).

We start by depicting changes in business formation rates around the reform in Figure 6. In the top Panel, we plot monthly business formation rates for worker affected (solid line) and workers unaffected (dashed line) by the reform. While we observe parallel trends in business formation rates before the reform, workers affected by the reform become more likely to start a new business from about three months after the reform consistent with Hypothesis (4). In Panel B, we show business formation rates for municipalities with above-median levels (solid line) and below-median levels (dashed line) of workers affected by the reform (see Figure 5).[22] While business formation rates evolve similarly before the reform, from about three

[21] For non-listed firms we compute a relative drop in implied firm value for risky over safe firms of 1.99 percent.

[22] We measure the share of affected workers in the entire working age population to capture all potential employees of new firms.

months after the reform business formation rates relatively decline in municipalities with above median levels of workers affected by the reform consistent with Hypothesis (5).

To test Hypothesis (4) formally, we compare changes in the rate at which individuals start a new business for workers affected and unaffected by the reform by estimating

$$log(business\ formation)_{jmt} = \alpha_{jm} + \alpha_{mt} + \mu \cdot Affected_j \cdot Reform_t + \epsilon_{jmt}, \qquad (10)$$

where $log(business\ formation)_{jmt}$ is the number of businesses established by affected or unaffected individuals j in municipality m in month t. Individual group-municipality fixed effects α_{jm} ensure that we track changes in entrepreneurship for the same group of individuals in the same municipality over time, municipality-month fixed effects α_{mt} control for local shocks that may affect new business formations. All other variables are defined as before.

The results are collected in Table 10, Panel A, column I. We find that individuals affected by the reform start 3.38 percent more new business after the reform compared to individuals unaffected by the reform, consistent with Hypothesis (4).

Hypothesis (5) predicts that higher labor costs after the reform reduce the expected profits from starting a new business and reduce new business formation. To test this hypothesis, we cannot compare individuals affected and unaffected by the reform, since higher labor costs of potential employees affect them equally. Instead, we replace the variable $Affected_j$ in equation (10) with a variable $\%Affected_m$ representing the share of workers in a given municipality who are affected by the reform. To sharpen the identification we control for mesoregion-time fixed effects, which ensures that we compare adjacent municipalities.[23]

The results are displayed in Panel B, column I. We find that business formation rates decline by 1.21 percentage points per ten percentage points increase in the share of individuals in a given municipality being affected by the reform as predicted by Hypothesis (5).

Under Hypothesis (6), new businesses established by workers affected by the reform are of lower quality, reducing the average quality of businesses ($\mu < 0$). To measure the quality of newly established businesses, we use default rates and access to credit markets and replace the dependent variable in equation (10) with these measures.

The results are gathered in columns II to IV in Table 10, Panel A. Wages are similar for workers hired by firms started by workers affected and workers unaffected by the reform. We find evidence consistent with new businesses established by workers affected by the reform being less profitable than the average firm. The probability for a new business closing within twelve months relatively increases by 0.43 percentage points for businesses started

[23] A mesoregion consists of about eight neighboring municipalities on average that are defined as being similar by the central statistics bureau (IBGE).

by individuals affected by the reform (column III). Additionally, these businesses are 0.29 percentage points less likely to obtain credit financing (column IV).

Next, we examine whether higher labor costs affect the average quality of new businesses. On the one hand higher labor costs may lead marginal and less profitable businesses to disappear, on the other hand higher labor costs turn new businesses less profitable. To test which effect dominates in the data, we compare the quality of businesses established in municipalities in which more or less workers are affected by the reform. The results are shown in Table 10, Panel B. Consistent with labor costs increasing more in municipalities in which more workers are affected by the reform, we find that newly established firms have to pay 0.43 percent higher wages per ten percentage point increase in the fraction of workers affected by the reform relative to their workers' previous wage (column II). Additionally, we observe that a ten percentage point increase in workers affected by the reform in a given municipality is associated with a 0.38 percentage point decline in closures (column III), and a 0.42 percentage point increase in access to credit (column IV). This suggests that the positive selection effect of higher expected labor costs driving out the establishment of marginal firms dominates the negative performance effect of higher wages.

Overall, the results in this section suggest that a more generous UI system encourages entrepreneurship, leading to more businesses being created. This effect is particularly strong for marginal businesses that become unprofitable when labor costs increase.

5 Additional Robustness Tests

This section presents additional robustness tests to strengthen the interpretation of the empirical results. Concerns about confounding factors correlated with firm risk are mitigated by the analysis in Section 4.2.4. Thus, the focus in this section is on potential differences between workers affected and workers unaffected by the reform and labor market characteristics that may interact with our findings.

Occupations In our main tests we measure unemployment risk at the firm level. Since unemployment risk may vary within the same firm, we replicate our main tests computing layoff risk separately for high-skilled and low-skilled workers in the same firm and saturate equation (5) with firm-skill-time, age group-skill-time, and firm-worker group-skill fixed effects. Thus, we compare affected and unaffected workers within the same firm, skill level, and month, additionally controlling for time trends among people in the same age group and occupation.

The results are shown in Table A.3. Employment declines by 0.41 percent (column I)

and hiring rates decline by 0.02 percent (column III) per risk decile for workers affected by the reform. Wages and hiring wages for workers affected by the reform increase by 0.20 and 0.09 percent more per risk decile after the reform (columns II and IV). This suggests that our results are robust to measuring layoff risk at different levels of granularity and are not driven by differences in workers' skill level.

Tenure-Wage Growth Profile Workers affected and workers unaffected by the reform have different average tenures. As a consequence, they may be on a different wage growth trajectory. To predict a worker's wage based on their tenure-wage profile, we compute the average wage growth of workers with the same tenure and define the difference in the observed and the predicted wage as excess wage growth ($log(wage)^{e}_{jt} = log(wage)_{jt} - log(wage)^{predicted}_{jt}$) and replace the dependent variable in equations (4) and (5).

We find that adjusting for wage growth based on workers' tenure-wage trajectory does not affect the results (Table A.4). Excess wage growth is 0.69 percent higher for workers affected by the the reform compared to workers unaffected by the reform (column I). As before, we find that this effect is stronger for riskier firms, increasing by 16 to 28 basis points per increase in risk decile (columns II to IV). We find the same patterns for newly hired workers.[24]

To account for the possibility that the wage-tenure profile is not linear, we control for workers' tenure and all its interaction with the independent variables in equation (5). The results reported in Table A.5 are very similar to the benchmark specification from Tables 3 and 4.

Altogether, these results suggest that differential changes in wages for workers affected and unaffected by the reform are not explained by differences in their tenure-wage profile.

Labor Market Informality Informality is a pervasive feature of labor markets in Brazil. To examine to what extent the presence of informal labor markets affects our results, we split the sample into above and below median levels of labor market informality across municipalities and industries in Brazil.[25]

The results for above and below median levels of labor market informality are depicted in Tables A.6 (municipality level) and A.7 (industry level). We find that the effects are present in high- and low-informality labor markets.

[24]For newly hired workers, we use the tenure at their previous job.
[25]Information on labor market informality is from the Brazilian Census.

6 Conclusion

In this paper, we examine how unemployment insurance affects the allocation of labor supply across firms that expose workers to different levels of unemployment risk. Exploiting a reform in Brazil that alters eligibility criteria for part of the workforce allows us to control for firms' labor demand by examining changes in employment and wages for different workers within the same firm. We find that a reduction in the generosity of UI reduces labor supply to riskier firms, forcing them to pay higher compensating wage differentials. Lower labor supply and higher labor costs are consistent with a relative decline in risky firms' value following the announcement of the reform.

These findings imply that a more generous UI system subsidizes risky firms' labor costs and establishes a transfer system from safe to risky firms. We find that as a consequence in areas in which a larger fraction of the workforce is affected by a reduction in UI generosity, entrepreneurship declines after the reform. This suggests that encouraging corporate risk-taking is an important policy externality of UI. An additional implication of our findings is that reducing compensating differentials through the provision of UI may affect the composition of firms' production factors.

The theory of second-best (Lipsey and Lancaster 1956) cautions us against general welfare statements. Whether encouraging risk-taking in the economy has benign or malign effects on growth depends on whether workers are risk-averse to the extent that profitable risky activities become unprofitable after taking into account compensating wage differential. Typically, workers are thought to be risk-averse and have been shown to demand compensating wage differentials for exposure to unemployment risk. This may constitute an implicit tax on corporate risk-taking and drive it below optimal levels. In this case, UI reduces the implicit tax on corporate risk-taking and brings it closer to its optimal level.

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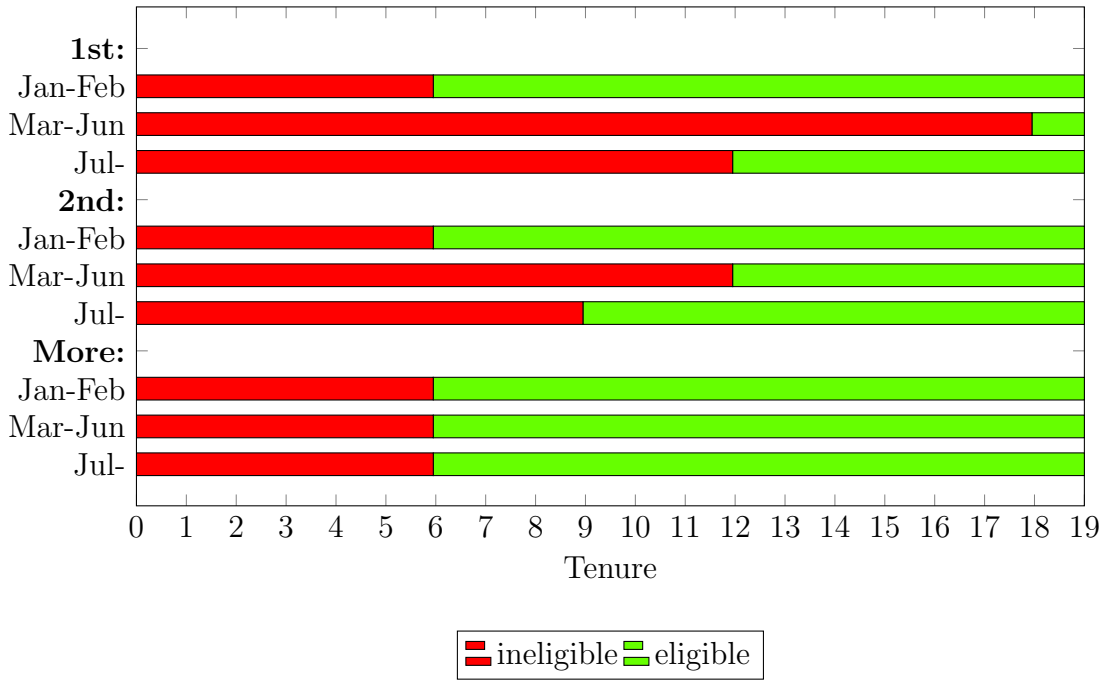
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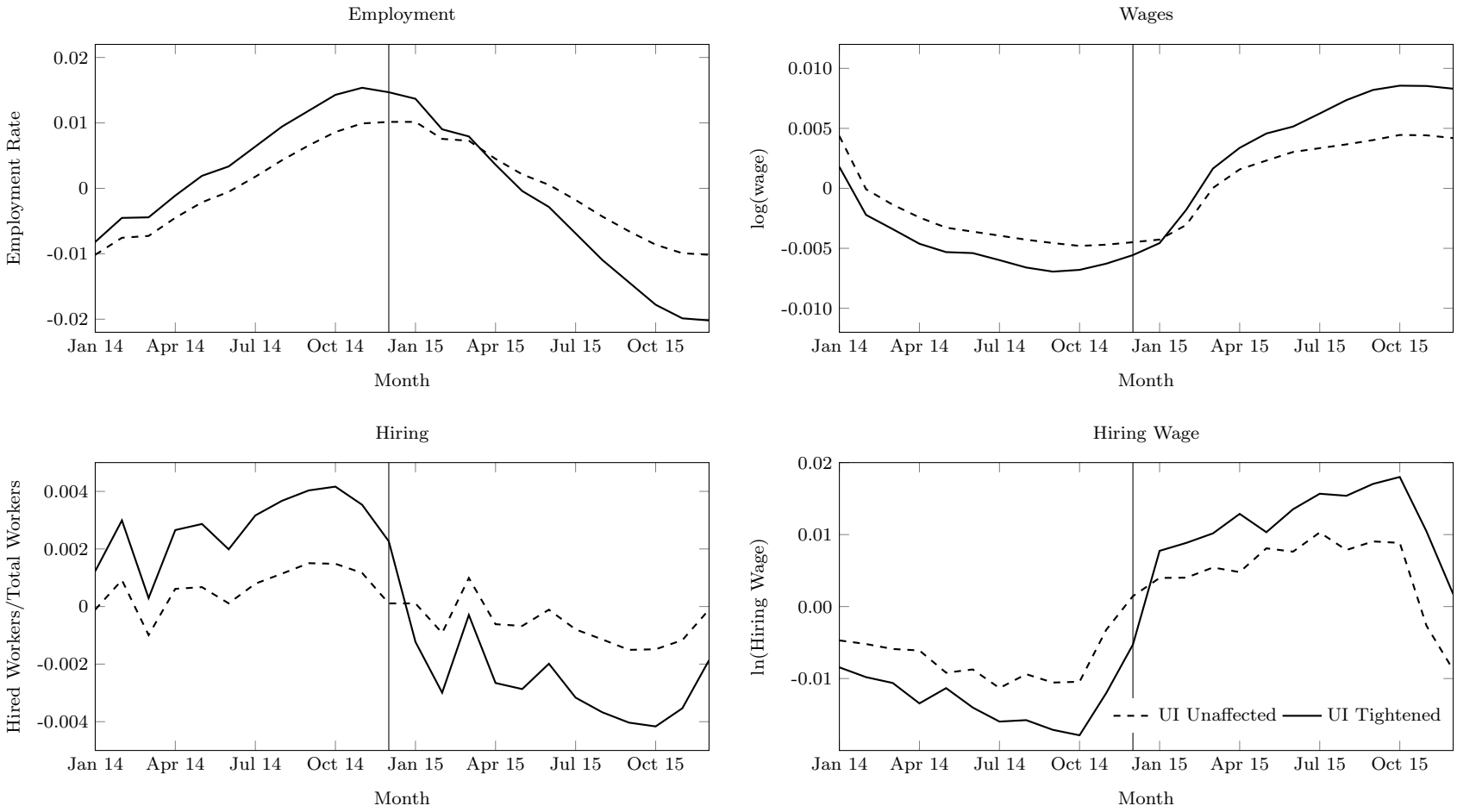
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Figure 1: **UI Eligibility Around the Reform**



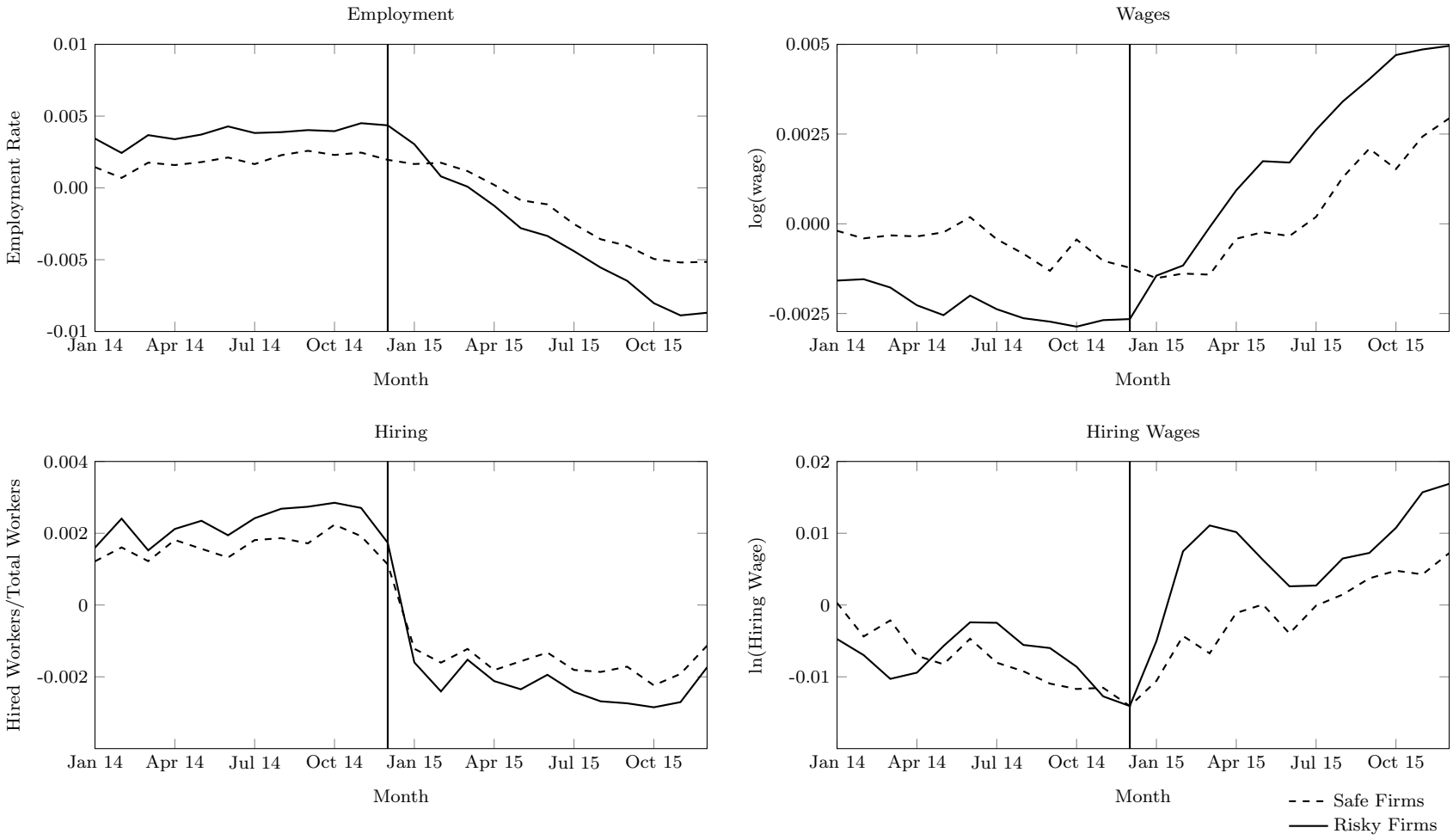
This figure illustrates eligibility criteria for UI benefits before and after the reform conditional on workers' tenure. The top lines depict eligibility criteria for workers with no prior UI spell, the middle lines for workers with one prior benefits spell, and the bottom lines for workers with at least two prior benefits spells. Tenure ranges that do not satisfy eligibility criteria are indicated in red; tenure ranges that satisfy eligibility criteria are indicated in green.

Figure 2: **Employment and Wages**



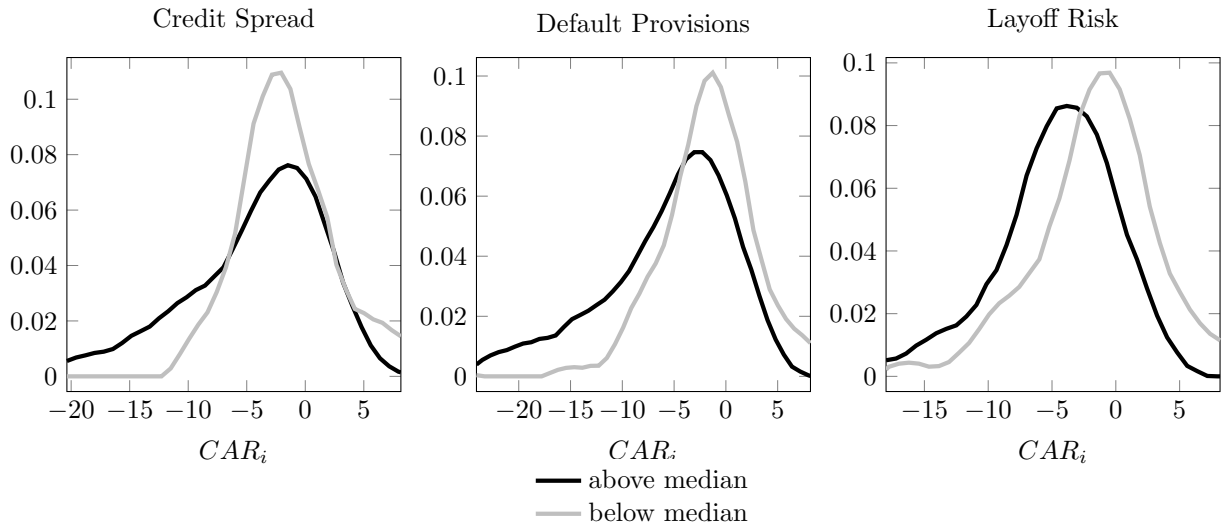
This figure depicts time-series changes in total employment, wages, hiring, and hiring wages around the announcement of the UI reform. The top left panel depicts total employment of workers with one or less prior UI spells (solid line) and workers with at least two prior UI benefits spells (dashed line) scaled by total employment at the end of 2014. The top right panel depicts the average log wage, the left bottom panel depicts hiring rates and the bottom right panel depicts hiring wages. All plots are adjusted for firm, worker group, and calendar month fixed effects.

Figure 3: **Employment, Wages, and Firm Risk**



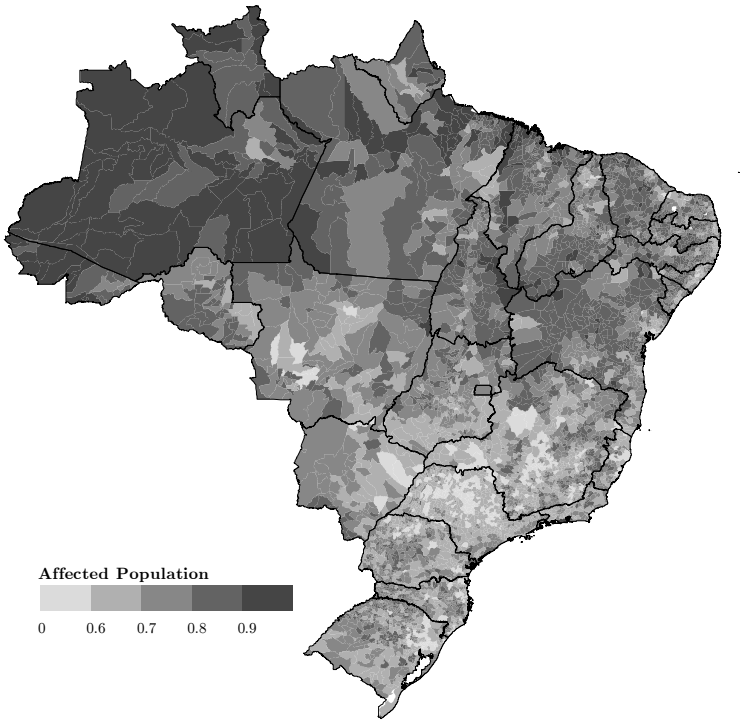
This figure depicts time-series changes in total employment, wages, hiring, and hiring wages around the announcement of the UI reform. The plots in the top left panel depict the difference in employment of workers with one or no prior UI spells and workers with at least two prior UI benefits spell for risky (solid line) and safe (dashed line) firms, where risky firms are defined as firms in the top quartile of credit risk. The top right panel depicts the same difference for the log average wage, the bottom left panel for hiring rates, the bottom right panel for log hiring wages. All plots are adjusted for firm, worker group, and calendar month fixed effects.

Figure 4: **Cumulative Abnormal Returns by Firm Risk**



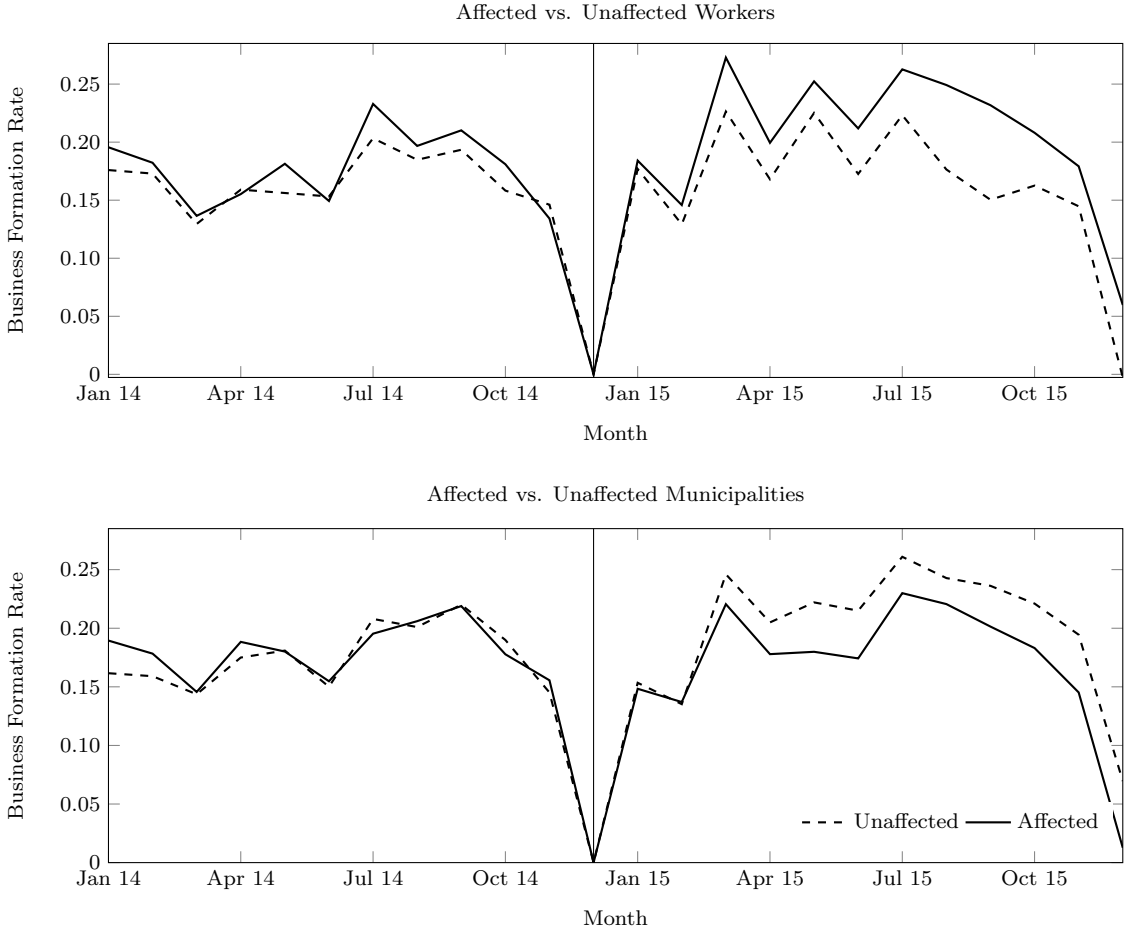
This figure depicts the distribution of cumulated abnormal returns of firms with above and below median levels of firm risk over the five days around the announcement of the UI reform on December 29, 2014. Firm risk is measured by firms' credit spread in the left panel, default provisions that banks hold for firms' debt in the middle panel, and layoff risk in the right panel.

Figure 5: **Fraction of Affected Population by Municipality**



This figure depicts the share of the working age population in a municipality affected by the UI reform.

Figure 6: **Entrepreneurship**



This figure depicts time-series changes in business formation rates (log of number of businesses) around the announcement of the UI reform. In the top panel, the plots depict business formation rates for workers with one or no prior UI spell (solid line) and workers with at least two prior UI benefits spells (dashed line). In the bottom panel, the plots depict business formation rates for municipalities with above median levels of the fraction of workers affected by the reform (solid line) and municipalities with below median levels of the fraction of workers affected by the reform (dashed line). All plots are normalized to be zero in December 2014.

Table 1: **Summary Statistics**

Panel A: Employment measures by group					
	All	Affected	Unaffected	Difference	
Number of Workers ('000)	24,772	17,182	7,590		
Average Wage	1,964	1,978	1,951	27	
Number of New Hires ('000)	716	468	248		
Average Wage for New Hires	1,721	1,722	1,719	3	
Education	0.652	0.655	0.649	0.006	
Age	36.5	35.0	39.8	-4.8	***
Tenure (months)	33.9	36.0	32.4	3.6	***
Skilled Occupation	0.222	0.237	0.207	0.030	***
Entrepreneur	0.082	0.080	0.089	-0.009	
Manufacturing	0.107	0.117	0.135	-0.018	
Agriculture	0.050	0.046	0.068	-0.022	
Construction	0.011	0.010	0.014	-0.004	
Panel B: Firm-level measures					
	Mean	Median	Std. Dev.		
12 months Layoff Rate	0.180	0.150	0.152		
Interest Spread	1.203	0.410	1.314		
Default Provisions	0.080	0.010	0.219		

This table reports descriptive statistics in December 2014. Panel A reports worker level information: the number of workers and their wages, the number of new hires and their wages, education, which equals one if a worker has completed secondary education and zero otherwise, age, tenure, skills, which equals one if a worker is employed in a high-skill occupation and zero otherwise, entrepreneur, which equals one if an individual owns a firm and zero otherwise, and the fraction of workers employed in manufacturing, agriculture, and construction. The values are reported separately for all workers, for workers with less than two prior UI benefits spells (affected), and for workers with at least two prior UI benefits spells (unaffected). The last column reports the difference between affected and unaffected workers, where *** denotes statistical significance at the 1% level. Panel B reports firm-level statistics: the probability of a newly hired worker being laid off within twelve months, the highest credit spread among all active bank liabilities, and loan default provisions held at the central bank (fraction of outstanding debt), respectively.

Table 2: **Employment, Hiring, and Wages**

	Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV
Panel A: Main Tests				
$Affected_j * Reform_t$	-0.0594***	0.0090***	-0.0052***	0.0252***
	(0.0007)	(0.0006)	(0.0001)	(0.0009)
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	21,280,320	17,144,583	21,280,320	4,696,198
R^2	0.520	0.772	0.415	0.720
Panel B: Workers Around Threshold				
$Affected_j * Reform_t$	-0.0218***	0.0052***	-0.0034***	0.0134***
	(0.0006)	(0.0007)	(0.0001)	(0.0010)
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	21,075,120	15,098,045	21,276,432	3,066,844
R^2	0.607	0.746	0.291	0.733

This table reports changes in employment and wages around the UI reform. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the average log hiring wage in column IV. In Panel B, the sample is limited to workers with one or two prior UI benefits spells. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table 3: **Firm Risk and Labor Supply - Employment**

Risk Measure:	Credit Spread		Default Provisions		Layoff Risk	
Dep. Var.:	$ERate$	$log(wage)$	$ERate$	$log(wage)$	$ERate$	$log(wage)$
	I	II	III	IV	V	VI
Panel A: Main Tests						
$Affected_j * Reform_t$	-0.0476***	0.0054***	-0.0435***	0.0046***	-0.0203***	-0.0083***
	(0.0017)	(0.0016)	(0.0016)	(0.0015)	(0.0020)	(0.0015)
$Affected_j * Reform_t * Risk_i$	-0.0027***	0.0006**	-0.0039***	0.0008***	-0.0067***	0.0029***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
Firm*Affected FE	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	16,766,784	13,835,418	16,766,784	13,835,418	21,280,320	17,144,583
R^2	0.544	0.760	0.544	0.760	0.520	0.768
Panel B: Workers Around Threshold						
$Affected_j * Reform_t$	-0.0103***	-0.0004	-0.0078***	-0.0020	0.0061***	-0.0053***
	(0.0014)	(0.0018)	(0.0013)	(0.0017)	(0.0015)	(0.0017)
$Affected_j * Reform_t * Risk_i$	-0.0024***	0.0008***	-0.0033***	0.0012***	-0.0048***	0.0018***
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Firm*Affected FE	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	16,707,168	12,251,579	16,707,168	12,251,579	21,075,120	15,098,045
R^2	0.629	0.737	0.629	0.737	0.607	0.746

This table reports changes in employment and wages around the UI reform. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in odd columns, and the log average wage in even columns. In Panel B, the sample is limited to workers with one or two prior UI benefits spells. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table 4: **Firm Risk and Labor Supply - Hiring**

Risk Measure:	Credit Spread		Default Provisions		Layoff Risk	
Dep. Var.:	$HiringRate$	$log(wage)$	$HiringRate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV	V	VI
Panel A: Main Tests						
$Affected_j * Reform_t$	-0.0007***	0.0191***	-0.0049***	0.0210***	-0.0048***	0.0157***
	(0.0001)	(0.0022)	(0.0001)	(0.0020)	(0.0001)	(0.0022)
$Affected_j * Reform_t * Risk_i$	-0.0002***	0.0011***	-0.0001***	0.0009**	-0.0001***	0.0016***
	(0.0000)	(0.0003)	(0.0000)	(0.0004)	(0.0000)	(0.0003)
Firm*Affected FE	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	16,766,784	3,996,047	16,766,784	3,996,047	21,280,320	4,696,198
R^2	0.422	0.710	0.422	0.710	0.415	0.720
Panel B: Workers Around Threshold						
$Affected_j * Reform_t$	-0.0025***	0.0034	-0.0027***	0.0093***	-0.0021***	0.0047*
	(0.0001)	(0.0026)	(0.0001)	(0.0024)	(0.0001)	(0.0026)
$Affected_j * Reform_t * Risk_i$	-0.0002***	0.0018***	-0.0002***	0.0008*	-0.0002***	0.0015***
	(0.0001)	(0.0004)	(0.0000)	(0.0004)	(0.0001)	(0.0004)
Firm*Affected FE	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	16,707,168	2,625,494	16,707,168	2,625,494	21,075,120	3,066,844
R^2	0.373	0.722	0.373	0.722	0.363	0.733

This table reports changes in hiring and wages around the announcement of the UI reform. The dependent variable is the share of workers hired relative to the firm's December 2014 employment (odd columns), or the log of hiring wage (even columns). In Panel B, the sample is limited to workers with one or two prior UI benefits spells. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: **Employment Transitions**

Dep. Var.: $\Delta Risk_{ijt}$	I	II	III	IV	V	VI
Risk Measure:	Credit Spreads		Default Provisions		Layoff Risk	
$Affected_j * Reform_t$	0.0032***	-0.0020***	0.0029***	-0.0023***	0.0034**	-0.0009
	(0.0004)	(0.0005)	(0.0004)	(0.0006)	(0.0005)	(0.0009)
$Affected_j * Reform_t * Risk_i$		0.0009***		0.0010***		0.0007***
		(0.0001)		(0.0002)		(0.0001)
Firm*Affected FE	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	5,745,575	5,745,575	5,745,575	5,745,575	6,812,469	6,812,469
R^2	0.178	0.178	0.232	0.232	0.188	0.189

This table examines transitions of individuals to different firms around the UI reform. The dependent variable $\Delta Risk_{ijt}$ is the fraction of workers who transition from a riskier to a safer firm. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table 6: **Number of UI Spells and Macro-Economic Conditions**

Risk Measure:	Credit Spread				Default Provisions				Layoff Risk			
	Employed		Hiring		Employed		Hiring		Employed		Hiring	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Panel A: Number of UI Spells												
$UPeriods_j * Reform_t$	-0.0010***	-0.0039***	0.0005***	-0.0086***	-0.0034***	-0.0047***	0.0005***	-0.0077***	-0.0059***	-0.0020*	0.0001***	-0.0083***
	(0.0001)	(0.0011)	(0.0001)	(0.0016)	(0.0003)	(0.0010)	(0.0001)	(0.0015)	(0.0003)	(0.0011)	(0.0000)	(0.0016)
$Affected_j * Reform_t$	-0.0059***	-0.0025	-0.0007***	-0.0026	-0.0064***	-0.0038*	-0.0008***	0.0036	-0.0017***	-0.0053**	-0.0008***	-0.0009
	(0.0006)	(0.0025)	(0.0001)	(0.0034)	(0.0006)	(0.0023)	(0.0001)	(0.0032)	(0.0006)	(0.0023)	(0.0001)	(0.0034)
$UPeriods_j * Reform_t * Risk_i$	0.0007***	0.0004**	0.0001***	0.0005*	0.0014***	0.0007***	0.0001***	0.0004	0.0014***	0.0001	0.0001***	0.0005*
	(0.0001)	(0.0002)	(0.0000)	(0.0003)	(0.0001)	(0.0002)	(0.0000)	(0.0003)	(0.0001)	(0.0002)	(0.0000)	(0.0002)
$Affected_j * Reform_t * Risk_i$	-0.0006***	0.0011***	-0.0001***	0.0021***	-0.0006***	0.0015***	-0.0001***	0.0012**	-0.0012***	0.0018***	-0.0001***	0.0018***
	(0.0001)	(0.0004)	(0.0000)	(0.0005)	(0.0001)	(0.0004)	(0.0000)	(0.0006)	(0.0001)	(0.0004)	(0.0000)	(0.0005)
Firm*UPeriods FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm
Observations	33,414,336	22,038,270	33,414,336	4,200,883	33,414,336	22,038,270	33,414,336	4,200,883	42,150,240	26,957,014	42,150,240	4,879,260
R^2	0.536	0.760	0.256	0.545	0.536	0.689	0.256	0.545	0.536	0.699	0.249	0.555
Panel B: Macro-Economic Conditions												
$Affected_j * Crisis_t$	-0.0063***	-0.0017	-0.0182	-0.0057	-0.0057***	-0.0023*	-0.0132	-0.0061*	-0.0077***	-0.0015	-0.0104	-0.0092**
	(0.0007)	(0.0016)	(0.0142)	(0.0039)	(0.0005)	(0.0013)	(0.0095)	(0.0032)	(0.0006)	(0.0013)	(0.0064)	(0.0036)
$Affected_j * Crisis_t * Risk_i$	0.0001	-0.0001	0.0016	-0.0003	-0.0001	0.0001	0.0008	-0.0003	0.0001	-0.0001	0.0003	0.0007
	(0.0001)	(0.0002)	(0.0019)	(0.0006)	(0.0001)	(0.0002)	(0.0014)	(0.0006)	(0.0001)	(0.0002)	(0.0007)	(0.0005)
Firm*Affected FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm	firm
Observations	4,864,752	3,957,554	4,864,752	1,146,378	4,864,752	3,957,554	4,864,752	1,146,378	7,128,864	5,682,407	7,128,864	1,555,991
R^2	0.629	0.750	0.629	0.598	0.668	0.750	0.253	0.737	0.657	0.764	0.258	0.619

This table reports changes in employment, wages, hiring, and hiring wages around the announcement of the reform. In Panel A, the sample is restricted to workers with one to four prior UI spells. In Panel B, the sample period covers the year before and after the onset of the global financial crisis. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Crisis_t$ takes the value of one for the crisis period from October 2008 to June 2009, and zero for the pre-crisis period from January 2008 to September 2008. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. The variable $UPeriods_j$ is the number of UI benefits spells that worker j experienced. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: **Worker-Level Estimation**

Risk Measure:		Credit Spread	Default Provisions	Layoff Risk
	I	II	III	IV
Dep. Var.:	$log(wage_i)$			
Panel A: Existing Employees				
$Affected_j * Reform_i$	0.0066***	0.0037**	0.0046***	-0.0001
	(0.0003)	(0.0008)	(0.0008)	(0.0008)
$Affected_j * Reform_i * Risk_i$		0.0006***	0.0005**	0.0012***
		(0.0002)	(0.0002)	(0.0001)
Firm-Time FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	3,355,550	2,903,434	2,903,434	3,355,550
R^2	0.987	0.986	0.986	0.987
Dep. Var.:	$\Delta log(wage_i)$			
Panel B: New Hires				
$Affected_j * Reform_i$	0.0077***	0.0066***	0.0071***	0.0075***
	(0.0012)	(0.0014)	(0.0016)	(0.0012)
$Affected_j * Reform_t * Risk_i$		0.0010***	0.0007**	0.0006**
		(0.0003)	(0.0003)	(0.0003)
Firm*Time FE	yes	yes	yes	yes
Affected*Risk FE	yes	yes	yes	yes
Risk*Time FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	5,734,684	4,332,283	4,332,283	5,734,684
R^2	0.162	0.167	0.166	0.263

This table reports changes in wages for current employees (Panel A) and newly hired workers (Panel B), estimated at the worker level. Due to computational constraints, we select a ten percent random sample of workers in Panel A. The dependent variable is log wage. In Panel B, the dependent variable is the difference between the log wage of newly hired worker compared to the wage at their previous job. The variable $Affected_i$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. The variable $Reform_t$ takes the value of one for workers hired from January 2015 to December 2016, and zero for workers hired from January 2013 to December 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: **Firm Risk and Natural Disasters**

	Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV
$Affected_j * Reform_t$	0.0585**	-0.0802**	0.0023	-0.0909*
	(0.0251)	(0.0373)	(0.0016)	(0.0481)
$Affected_j * Reform_t * Risk_i$	-0.0218***	0.0113**	-0.0008***	0.0108**
	(0.0045)	(0.0050)	(0.0003)	(0.0052)
Estimation	IV	IV	IV	IV
F-stat	35.01	27.32	35.01	27.11
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	21,280,320	17,144,583	21,280,320	4,696,198
R^2	0.520	0.768	0.414	0.718

This table reports changes in employment, wages, hiring, and hiring wages around the announcement of the reform based on the instrument variable strategy described in equation (8). The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the log average hiring wage in column IV. The variable $Affected_j$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: **Firm Value**

Dep. Var.: $CAR[-1;+3]$	I	II	III	IV	V	VI	VII	VIII	IX
Risk Measure:	Credit Spread			Default Provisions			Layoff Risk		
$Risk_i$	-1.72***	-1.72**	-2.02**	-1.19	-1.23	-2.35**	-1.83***	-2.07***	-2.38**
	(0.48)	(0.54)	(0.69)	(0.93)	(0.86)	(0.95)	(0.52)	(0.62)	(0.85)
Clustered SE	ind	ind	ind	ind	ind	ind	ind	ind	ind
Observations	140	127	111	140	127	111	155	140	121
R^2	0.031	0.031	0.042	0.012	0.013	0.044	0.028	0.036	0.046

This table reports cumulative abnormal returns in the five days around the announcement of the reform on December 29, 2014. The sample consists of all listed firms in Brazil with at least 50 (columns I, IV, and VII), 100 (columns II, V, and VIII), or 200 (columns III, VI, and IX) trading days during 2014. The variable $Risk_i$ takes the value one for firms with above median levels of risk, and zero for below median firms. Standard errors are reported in parentheses. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table 10: **Entrepreneurship**

Dep. Var.:	log(business formation)	$\Delta wage$	P[closure 12m]	P[loan 12m]
	I	II	III	IV
Panel A: Affected vs Unaffected Workers				
$Affected_j * Reform_t$	0.0338***	0.0030	0.0043***	-0.0029***
	(0.0031)	(0.0040)	(0.0006)	(0.0007)
Municipality-Affected FE	yes	yes	yes	yes
Month-Mesoregion FE	yes	yes	yes	yes
Clustered SE	muni	muni	muni	muni
Observations	376,953	135,506	376,953	376,953
R^2	0.843	0.171	0.138	0.245
Panel B: Share of Affected Worker				
$\%Affected_m * Reform_t$	-0.1212***	0.0426**	-0.0375***	0.0419***
	(0.0338)	(0.0176)	(0.0094)	(0.0096)
Municipality FE	yes	yes	yes	yes
Month-Mesoregion FE	yes	yes	yes	yes
Clustered SE	muni	muni	muni	muni
Observations	228,689	100,964	228,689	228,689
R^2	0.877	0.156	0.076	0.191

This table examines changes in new business formation and quality around the reform. The dependent variable is the log number of new businesses formed in a given month (column I), the change in wages for workers hired by new firms relative to their last prior wage (column II), the probability of a new business closing within twelve months (column III), and the probability of a business taking out a loan within twelve months (column IV). In Panel A, we aggregate the dependent variables at the municipality-worker group level, in Panel B we aggregate the dependent variables at the municipality level. $Affected_j$ takes the value of one for workers affected by the reform and zero for workers unaffected by the reform. $\%Affected_m$ is the fraction of working age population in a municipality that is affected by the reform. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The bottom part reports information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Appendix A. Additional Tables

Table A.1: **Aggregate Employment Effects**

Risk Measure:	Credit Spread	Default Provisions	Layoff Risk
Dep. Var.: $ERate_i$	I	II	III
$Reform_t * Risk_i$	-0.0031***	-0.0050***	-0.0047***
	(0.0006)	(0.0006)	(0.0006)
Firm FE	yes	yes	yes
Month*Muni*Industry FE	yes	yes	yes
Clustered SE	firm	firm	firm
Observations	1,132,655	1,132,655	1,432,815
R^2	0.265	0.265	0.289

This table reports firm-level changes in total firm-level employment around the reform. The dependent variable is total employment of firm i in month t over total employment in December 2014. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest up to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.2: **Firm Risk and Natural Disasters**

Dep. Var.:	*Layoff Risk*$_i$	
	Pre-Reform	Post-Reform
	I	II
Shock$_i$	2.2283***	1.9979***
	(0.2736)	(0.3515)
Exposure$_i$	0.4831***	0.9605***
	(0.23218)	(0.2674)
Shock Size$_i$	-0.3652***	-0.3386***
	(0.0484)	(0.0617)
$log(employees)_i$	0.0369	0.0222
	(0.0307)	(0.0228)
Municipality FE	yes	yes
Clustered SE	muni	muni
Observations	36,158	36,158
F-stat	20.12	19.07
R^2	0.106	0.093

This table reports the predicted level of firm risk based on our IV estimation (equation (8)). The dependent variable in column I is the probability of being laid off within twelve months after being hired, measured in the two years before the reform (2013-2014). In column II, the dependent variable is the probability of being laid off within twelve months after being hired, measured in the two years after the reform (2015-2016). Both variables sort firms into deciles based on layoff risk. The independent variable *Shock*$_i$ equals one if firm i was connected to a supplier that was affected by a natural disaster during 2013 to 2014, *Exposure*$_i$ measures a firm's exposure to the natural disaster as the fraction of the firm's cash flows that goes to affected suppliers, *Shock Size*$_i$ measures the size of the shock as the log of the sum of private and public damage in the affected municipality, and $log(employees)_i$ is the log of the number of employees of a firm at the end of 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.3: **Occupations**

	Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV
$Affected_{ijo} * Reform_t$	0.0002	-0.0017	-0.0015***	0.0089***
	(0.0006)	(0.0012)	(0.0001)	(0.0020)
$Affected_{ijo} * Reform_t$	-0.0041***	0.0020***	-0.0002***	0.0009***
$*$ *Occupation Layoff Risk*$_{io}$	(0.0001)	(0.0002)	(0.0001)	(0.0003)
Firm*Affected*Occupation FE	yes	yes	yes	yes
Firm*Occupation*Month FE	yes	yes	yes	yes
Age Group*Occupation*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	39,701,952	26,985,527	39,701,952	5,415,273
R^2	0.474	0.807	0.362	0.720

This table reports changes in employment, wages, hiring, and hiring wages around the reform. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the log average hiring wage in column IV. The variable $Affected_{ij}$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable *Occupation Layoff Risk*$_{io}$ takes the values of one for the safest to ten for the riskiest decile of firm-occupation pairs. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.4: **Tenure-Wage Profiles**

Risk Measure:		Credit Spread	Default Provisions	Layoff Risk
Dep. Var.: $wage\ growth^{e}_{ijt}$	I	II	III	IV
Panel A: Existing Employees				
$Affected_{ij} * Reform_t$	0.0069***	-0.0028***	-0.0017***	-0.0096***
	(0.0002)	(0.0006)	(0.0005)	(0.0006)
$Affected_{ij} * Reform_t * Risk_i$		0.0016***	0.0016***	0.0028***
		(0.0001)	(0.0001)	(0.0001)
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	13,695,842	11,090,833	11,090,833	13,695,842
R^2	0.451	0.436	0.436	0.452
Panel B: New Hires				
$Affected_{ij} * Reform_t$	0.0056***	0.0016	0.0028*	0.0010
	(0.0007)	(0.0016)	(0.0015)	(0.0016)
$Affected_{ij} * Reform_t * Risk_i$		0.0007***	0.0005**	0.0008***
		(0.0003)	(0.0004)	(0.0002)
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	4,661,610	3,962,864	3,962,864	4,661,610
R^2	0.260	0.258	0.709	0.260

This table reports changes in excess wage growth for current (Panel A) and newly hired (Panel B) workers around the reform. The dependent variable ($wage\ growth^{e}_{ijt}$) is the excess wage growth for workers of firm i in group j in month t. In Panel A, excess wage it is computed as wage over the wage predicted based on average wage growth of workers with the same tenure. In Panel B, excess wage growth is computed as wage growth relative to the previous job over the predicted wage growth based on a worker's tenure at their last job. The variable $Affected_{ij}$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. The variable $Risk_i$ takes the values of one for the safest to ten for the riskiest decile of firms. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.5: **Tenure Control**

	Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV
Panel A: Credit Spread				
$Affected_{ij} * Reform_t$	-0.0228***	-0.0405***	-0.0017***	0.0150***
	(0.0026)	(0.0049)	(0.0001)	(0.0029)
$Affected_{ij} * Reform_t * Risk_i$	-0.0009***	0.0013***	-0.0002***	0.0010***
	(0.0003)	(0.0003)	(0.0000)	(0.0003)
Observations	16,766,784	13,835,418	16,766,784	3,996,047
R^2	0.498	0.736	0.478	0.597
Panel B: Default Provisions				
$Affected_{ij} * Reform_t$	-0.0175***	-0.0388***	-0.0023***	0.0167**
	(0.0024)	(0.0048)	(0.0001)	(0.0027)
$Affected_{ij} * Reform_t * Risk_i$	-0.0022***	0.0009***	-0.0001***	0.0009**
	(0.0003)	(0.0003)	(0.0000)	(0.0006)
Observations	16,766,784	13,835,418	16,766,784	3,996,047
R^2	0.497	0.736	0.478	0.597
Panel C: Layoff Risk				
$Affected_{ij} * Reform_t$	-0.0171***	-0.0536***	-0.0021***	0.0124***
	(0.0028)	(0.0047)	(0.0001)	(0.0028)
$Affected_{ij} * Reform_t * Risk_i$	-0.0017***	0.0029***	-0.0001***	0.0014***
	(0.0003)	(0.0003)	(0.0000)	(0.0003)
Firm*Affected FE	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes
Tenure Controls	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm
Observations	21,280,320	17,144,583	21,280,320	4,696,198
R^2	0.476	0.745	0.477	0.607

This table reports changes in employment and wages around the reform. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the log average hiring wage in column IV. The variable $Affected_{ij}$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table A.6: **Municipality Informality**

	High Informality				Low Informality			
	Employed Workers		Hired Workers		Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV	V	VI	VII	VIII
Panel A: Credit Spread								
$Affected_{ij} * Reform_t * Risk_i$	-0.0026***	0.0007**	-0.0003***	0.0013***	-0.0027***	0.0010***	-0.0002***	0.0009**
	(0.0004)	(0.0003)	(0.0000)	(0.0004)	(0.0004)	(0.0003)	(0.0000)	(0.0003)
Observations	7,735,104	6,280,801	7,735,104	1,652,425	9,031,680	7,554,617	9,031,680	2,343,622
R^2	0.504	0.727	0.369	0.555	0.499	0.773	0.365	0.606
Panel B: Default Provisions								
$Affected_{ij} * Reform_t * Risk_i$	-0.0044***	0.0015***	-0.0001***	0.0008*	-0.0036***	0.0007**	-0.0001***	0.0008**
	(0.0004)	(0.0004)	(0.0000)	(0.0005)	(0.0004)	(0.0003)	(0.0000)	(0.0004)
Observations	7,735,104	6,280,801	7,735,104	1,652,425	9,031,680	7,554,617	9,031,680	2,343,622
R^2	0.504	0.727	0.374	0.623	0.499	0.773	0.379	0.682
Panel C: Layoff Risk								
$Affected_{ij} * Reform_t * Risk_i$	-0.0076***	0.0030***	-0.0001***	0.0018***	-0.0060***	0.0030***	-0.0001	0.0015***
	(0.0005)	(0.0004)	(0.0000)	(0.0005)	(0.0004)	(0.0003)	(0.0001)	(0.0004)
Firm*Affected FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm	firm	firm
Observations	9,802,368	7,684,813	9,802,368	1,905,973	11,477,952	9,459,770	11,477,952	2,790,225
R^2	0.470	0.689	0.362	0.547	0.480	0.753	0.371	0.622

This table reports changes in employment and wages around the reform in municipalities with above (columns I through IV) and below (columns V through VIII) median labor market informality. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the log average hiring wage in column IV. The variable $Affected_{ij}$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.7: **Industry Informality**

	High Informality				Low Informality			
	Employed Workers		Hired Workers		Employed Workers		Hired Workers	
Dep. Var.:	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$	$ERate$	$log(wage)$	$HiringRate$	$log(wage)$
	I	II	III	IV	VI	VII	VIII	IX
Panel A: Credit Spread								
$Affected_{ij} * Reform_t * Risk_i$	-0.0020***	0.0007**	-0.0001***	0.0007*	-0.0031***	0.0009***	-0.0003***	0.0014***
	(0.0004)	(0.0003)	(0.0000)	(0.0004)	(0.0004)	(0.0003)	(0.0001)	(0.0004)
Observations	8,300,160	6,955,557	8,300,160	2,087,264	8,240,256	6,690,093	8,240,256	1,848,129
R^2	0.487	0.745	0.374	0.554	0.516	0.782	0.354	0.635
Panel B: Default Provisions								
$Affected_{ij} * Reform_t * Risk_i$	-0.0031***	0.0011***	-0.0001	0.0003	-0.0047***	0.0009**	-0.0001***	0.0015***
	(0.0004)	(0.0004)	(0.0001)	(0.0005)	(0.0004)	(0.0003)	(0.0000)	(0.0005)
Observations	8,300,160	6,955,557	8,300,160	2,087,264	8,240,256	6,690,093	8,240,256	1,848,129
R^2	0.487	0.745	0.376	0.554	0.516	0.782	0.374	0.626
Panel C: Layoff Risk								
$Affected_{ij} * Reform_t * Risk_i$	-0.0085***	0.0035***	-0.0001***	0.0011**	-0.0050***	0.0019***	-0.0001***	0.0019***
	(0.0004)	(0.0003)	(0.0000)	(0.0005)	(0.0005)	(0.0003)	(0.0000)	(0.0005)
Firm*Affected FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm*Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Age Group*Month FE	yes	yes	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm	firm	firm
Observations	10,370,304	8,558,609	10,370,304	2,437,381	10,605,888	8,335,793	10,605,888	2,184,202
R^2	0.475	0.715	0.369	0.562	0.957	0.760	0.367	0.640

This table reports changes in employment and wages around the reform in industries with above (columns I through IV) and below (columns V through VIII) median labor market informality. The dependent variable is the employment of workers affected or unaffected by the reform by firm i in month t over total employment in December 2014 in column I, the log average wage in column II, the number of workers hired relative to a firm's employment in December 2014 in column III, and the log average hiring wage in column IV. The variable $Affected_{ij}$ takes the value of one for workers with less than two prior UI benefits spells and zero for workers with two or more prior unemployment spells. The variable $Reform_t$ takes the value of one for the post-reform period from January 2015 to December 2016, and zero for the pre-reform period from January 2013 to December 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.