# Unemployment Insurance, Strategic Unemployment, and Firm-Worker Collusion *

Bernardus Van Doornik†
David Schoenherr‡
Janis Skrastins§

**Abstract**

Exploiting a discontinuous effect of an unemployment insurance (UI) reform in Brazil, we find that more than ten percent of unemployment inflow at the eligibility threshold is due to UI-induced incentives. Most of this strategic unemployment inflow is related to workers transitioning to informal employment. We further document layoff and rehiring patterns consistent with collusion between firms and workers to extract rents from the UI system. Firms and workers time formal unemployment spells to coincide with workers' eligibility for UI benefits by rehiring them just when benefits cease. Firms and workers share rents extracted from the UI system through lower equilibrium wages. All of these patterns are mostly driven by industries and municipalities with large informal labor markets. Our findings suggest that optimal UI design in mid-income and developing countries should take into account adverse incentive effects generated by informal labor markets.

JEL Codes: J21, J22, J46, J65, K31.
Keywords: unemployment insurance, labor supply, collusion, law and economics.

*The views expressed in the paper are those of the authors and should not be cited to reflect the view of the Banco Central do Brasil. We thank Orley Ashenfelter, Michael Best, Will Dobbie, Henry Faber, Maryam Farboodi, Dimas Fazio, Gustavo Gonzaga, Gregor Jarosch, Ilyana Kuziemko, Camille Landais, Mina Lee, Alexandre Mas, Adrien Matray, Jean-Marie Meier, Atif Mian, Andrew Samwick, Bernado Silveira, Jan Starmans, Adam Szeidl, Motohiro Yogo, and seminar participants at the Banco Central do Brasil, the Bank of Latvia, Central European University, the Chilean Banking Authority, IPEA Brazil, Northwestern University, Princeton University, PUC Rio, SSE Riga, Syracuse University, the University of Chile, Washington University in St. Louis, the 2017 BCB Annual Inflation Targeting Seminar, the 2017 Asian, European, Latin American, and North American Meetings of the Econometric Society, the 2017 Boulder Summer Conference, the 2018 Meeting of the Econometric Society, the 2017 German Economists Abroad Meeting, the 2017 Fall Meeting of the Labor and Finance Group, the 2018 NBER Summer Institute Labor Studies and Public Economics Meetings, the 2017 Northeastern Finance Conference, and the 2017 Annual Conference of the SOLE for their many helpful comments and suggestions. Karine Paiva provided excellent research assistance.

†Banco Central do Brasil, Setor Bancario Sul Q.3 BL B Asa Sul, Brasilia DF 70074, bernardus.doornik@bcb.gov.br

‡Princeton University, 206B Julis R. Rabinowitz Building, Princeton NJ 08544, schoenherr@princeton.edu

§Washington University in St. Louis, One Brookings Drive, St. Louis MO 63130, jskrastins@wustl.edu

# 1 Introduction

Experiencing negative shocks to labor income is one of the most salient risks faced by households (Rothstein and Valletta 2017). To allay the adverse effects of job loss on household incomes, government-mandated unemployment insurance (UI) programs have been in place in developed countries for decades, and are increasingly implemented in mid-income and developing countries. Compared to the large literature on adverse incentive effects of UI on labor supply in developed countries,[1] we understand relatively little about how labor market characteristics in mid-income and developing countries, for example the presence of large informal labor markets, interact with the incentive effects of UI. Understanding how features of UI design affect workers' and firms' incentives in mid-income and developing countries has important policy implications, especially given the spread of UI programs to such countries (Holzmann et al. 2011; ILO 2017).

In this paper, we exploit an unanticipated UI reform in Brazil, to explore novel unemployment inflow and outflow patterns generated by UI in labor markets with high degrees of informality. As a heterogenous mid-income country with ample cross-sectional variation in labor market characteristics, Brazil constitutes an ideal laboratory for our analysis. We find that eligibility for UI benefits increases unemployment inflow by 11.8 percent at the eligibility threshold. Most of the additional unemployment inflow is related to workers flowing into informal employment. Moreover, we document that firms and workers collude to time formal unemployment inflow and outflow to coincide with workers' eligibility for UI benefits by rehiring them just when their benefits cease. Firms seem to benefit from colluding with workers through lower equilibrium wages. These patterns occur primarily in industries and municipalities with a high degree of labor market informality.

Our results have novel policy implications for optimal UI design in mid-income and developing countries that feature large informal labor markets. Our findings suggest that a significant fraction of workers on UI benefits are employed informally. Almost a quarter of informal employment while on benefits seems to be related to collusion between firms and workers, which is more prevalent when they can extract higher rents from the UI system.

[1] It is well documented that UI has adverse incentive effects on search intensities for reemployment (Solon 1979; Moffitt 1985; Katz and Meyer 1990a; Meyer 1990, 1995; Card and Levine 2000; Meyer and Mok 2007; Card et al. 2015a; Farber, Rothstein, and Valletta 2015; Johnston and Mas 2015; Landais 2015 for the U.S., Card, Chetty, and Weber 2007; Lalive 2008; Schmieder, von Wachter, and Bender 2012, 2016; Card et al. 2015b for Western Europe), and there is some evidence of a positive relationship between layoff intensities and workers' eligibility for UI benefits (Christofides and McKenna 1995, 1996; Green and Riddell 1997; Baker and Rea 1998; Green and Sargent 1998; Jurajda 2002; Rebello-Sanz 2012).

The main determinants of rents are eligibility rules, the duration and level of benefits, and layoff costs. This implies that higher experience rating, tighter eligibility rules, and lower overall payments could reduce rents from collusion. More nuanced policy implications include tweaks to eligibility rules to prevent repeated temporary layoffs of the same worker. Outside the UI system better monitoring and higher penalties for informal employment can mitigate strategic unemployment by increasing the expected costs of maintaining an informal employment relationship while receiving benefits. The advantage of policies that do not alter UI payments is that they do not affect the insurance value of UI, which may be high in the context of informal labor markets (Gerard and Gonzaga 2014).

The UI reform we exploit for our empirical analysis was announced on December 29, 2014 and implemented as a provisional measure on March 1, 2015. The nature of the reform provides a sharp discontinuity in the loss of eligibility for UI benefits. Prior to the reform, workers with tenure of six months are eligible for UI benefits. After the reform, workers applying for benefits for the first (second) time require formal employment for 18 (12) months during the previous 24 (16) months to be eligible for benefits. Thus, a subset of workers with tenure between six and 18 (12) months loses eligibility for UI benefits after the reform, whereas workers with tenure below six months were never eligible for benefits. This discontinuity motivates our main identification strategy: a difference-in-differences methodology, in which we compare changes in employment and unemployment patterns before and after the reform for workers with tenure above and below the six-month threshold.[2] In our strictest test, we focus on workers with four to seven months' tenure in the four months around the reform. This ensures that workers entered formal employment before learning about the reform allowing us to examine the effects of UI benefits free from endogenous selection into jobs with different expected employment durations (Green and Riddell 1997).

We start our analysis by examining how UI affects layoff intensities. Our findings indicate that UI eligibility has strong effects on unemployment inflow. Specifically, unemployment inflow relatively drops by 0.5 percentage points (11.8 percent) for workers just above the six-month threshold, who lose eligibility for UI benefits after the reform. We next assess how this pattern varies across labor markets with different degrees of informality. We find that higher layoff intensities for workers eligible for UI benefits strongly correlate with the presence of informal labor markets. The drop in unemployment inflow after the reform is significantly stronger for workers in industries and municipalities with large informal labor markets. Specifically, we find that a ten percentage point increase in the share of informal

[2]We provide direct evidence that workers below the six-month threshold who constitute our control group are not affected by the reform during our sample period in Section 3.5.

employment in a given industry or municipality corresponds to a 0.2 percentage point higher inflow into formal unemployment when workers are eligible for benefits.

Higher layoff intensities for individuals who qualify for UI benefits could be driven by several factors. Workers may exert less effort when their outside option is to receive UI benefits. Alternatively, it may be optimal for firms facing labor demand fluctuations to (temporarily) lay off workers on benefits, anticipating that these workers are less likely to search for alternative employment (Katz 1986; Jurajda 2002). Additionally, the theoretical literature provides two rationales for implicit agreements between firms and workers leading to higher layoff intensities for workers, who are eligible for UI benefits. Models of implicit contracting (Feldstein 1976; Baily 1977) imply that firms may collude with workers to lay them off when they are eligible for UI benefits, to extract rents from the UI system, which they can share through lower equilibrium wages. Similarly, Christofides and McKenna (1996) develop a model in which firms are more likely to lay off workers when they are eligible for UI benefits for reputational effects to elicit higher labor supply.

To differentiate between these alternative explanations, we examine layoff and rehiring patterns consistent with collusion between firms and workers. We observe an extreme form of unemployment timing. Before the reform, workers laid off with six months' tenure, just when they become eligible for UI benefits, are more likely to be rehired by their previous employer just when their benefits cease. Exploiting that a worker is only allowed to apply for UI benefits sixteen months after the last successful application, we find that firms are more likely to rehire the same workers nine to ten months after layoff, which allows the worker to reapply for benefits after another six months of formal employment. After the reform, when workers with a tenure of six months lose eligibility for UI benefits, these patterns vanish. This precise timing of unemployment spells according to UI benefits eligibility accounts for 21 percent of the higher layoff intensities at the eligibility threshold, and is more pervasive when the rents that workers and firms can extract from the UI system are higher.[3]

Informal labor markets provide a unique alternative to formal employment in the light of UI. Workers are able to receive benefits while continuing to be employed informally. We provide evidence that workers are employed informally while they receive UI benefits from survey data. We find that before the reform, workers with six months' tenure at layoff are about four percentage points more likely to enter informal employment upon layoff, compared to workers with five months' tenure at layoff. When workers with six months'

[3]Back-of-the-envelope calculations suggest that the additional UI benefits payments due to strategic formal unemployment amount to 0.0157 percent of GDP.

tenure lose eligibility for benefits after the reform, differences in informal employment upon layoff vanish. To assess whether workers continue to be employed informally by the same firm that laid them off, we examine changes in the probability of hiring a replacement for laid off workers around the reform. When firms lay off a worker, they often hire a new worker as a replacement. However, if firms continue to employ a formally laid off worker informally, they do not need to hire a replacement worker. Consistent with this conjecture, we find that firms are about 1.5 percentage points less likely to hire a replacement worker in the same occupation, if they lay off a worker who is eligible for UI benefits. After the reform, when all workers around the six months threshold are ineligible for UI benefits, differences in hiring rates for replacement workers disappear.[4]

Finally, we examine how firms benefit from timing formal unemployment spells to coincide with eligibility for UI benefits. Implicit contracting mechanisms (Feldstein 1976; Baily 1977; Christofides and McKenna 1996) predict that firms time workers' unemployment spells to coincide with eligibility for UI benefits to pay lower equilibrium wages. To assess whether this mechanism is present in the data, we exploit the fact that the reform only applies to a subset of workers. While workers with fewer than two successful prior UI benefits applications face tighter eligibility criteria, workers with at least two successful applications are unaffected by the reform. This allows us to examine the effects of UI on labor supply and wages, using workers unaffected by the reform as a natural control group.

On examining changes in wages, we find that workers who require longer tenure to qualify for UI benefits after the reform experience a 0.5-0.8 percent higher increase in wages.[5] The increase in wages is in line with implicit contracting mechanisms suggesting that workers and firms share rents from the UI system through lower equilibrium wages. Further consistent with this view, we find that the increase in wages and the decrease in formal employment are significantly greater in areas with a higher share of informal labor markets and where the timing of unemployment spells accordance to eligibility for UI benefits is more pervasive. A ten percentage point increase in collusive layoff and rehiring patterns at the local industry level is associated with a 1.9 percent higher increase in wages for workers affected by the reform. Based on our estimates, colluding workers pass on about half of their benefits payments to the firm (see Section 3.4 for details).

Altogether, our results suggest that eligibility for UI induces workers to flow from formal

[4]We calculate that 11.5 percent of strategically unemployed workers stay informally employed with the same firm that laid them off.

[5]These changes are not driven by the composition of hired workers. We observe the same patterns when comparing hiring wages to the previous wage of the same worker.

to informal employment. About a fifth of this strategic unemployment can be attributed to firms and workers colluding to time formal unemployment spells in accordance with workers' eligibility for UI benefits. We perform several robustness tests to strengthen the validity of our results. First, we control for seasonal patterns by performing the same analysis for the previous year, for which we observe none of the same patterns. Second, we confirm that workers do not substitute to other forms of job separation, such as voluntary departures, after the reform. Third, we show that the results are not affected by potential announcement effects of the reform two months before its implementation. Importantly, we do not find any of the patterns in placebo tests using workers with two or more successful past applications for UI benefits, who are not affected by the reform.

The main contribution of our paper is to shed light on incentive effects of UI in the context of mid-income and developing countries that feature large informal labor markets. Evidence on the interaction of UI and labor market informality is scant. Gerard and Gonzaga (2014) focus on the effect of UI on search intensities for formal reemployment for varying levels of labor market informality by exploiting variation in the *intensive* margin of UI (benefits duration). They find that the marginal effect of an additional month of unemployment benefits duration has only minor effects on search intensities.[6]

In contrast to Gerard and Gonzaga (2014), we exploit variation in the *extensive* margin of UI (eligibility) and provide a more comprehensive picture of how informal labor markets interact with different incentive effects of UI. Our findings suggest that the interaction between UI and informal labor markets is more complex and subtle. In particular, UI leads to higher unemployment inflow in the presence of informal labor markets. The tailoring of unemployment spells to coincide with workers' UI eligibility suggests that in some cases UI does not fulfill an insurance purpose, but rather acts to redistribute income towards firms and workers who learn to play the system.

While the theory of second-best (Lipsey and Lancaster 1956) cautions us against making general welfare claims, our results provide direct policy implications for mitigating adverse incentive effects. The most direct way to reduce strategic unemployment inflow within the UI system is to increase experience rating, which seems to be more crucial in the presence of large informal labor markets. Additionally, eligibility rules that prevent repeated temporary layoffs can reduce exploitation of the UI system.[7] An alternative way to reduce rent extraction from

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

[7]Based on the findings in Gerard and Gonzaga (2014), reducing benefits duration may be less effective in

the UI system when workers are employed informally, would be better monitoring and higher penalties for informal employment. The appeal of policies that can reduce rent extraction without altering the UI system is that they do not reduce the insurance effect of UI.

Our results also relate to the literature on higher layoff intensities.[8] Existing studies provide mixed insights regarding the effects of different aspects of UI design on layoff intensities. Anderson and Meyer (1997b) find that, in contrast to duration, benefit levels have a strong impact on UI take-up. Winter-Ebmer (2003) documents that unemployment inflow is higher when benefits duration is extended. Jurajda (2002) finds that higher benefits levels have no effect on layoff intensities. More recent studies find no or very weak evidence of unemployment inflow timing with respect to UI eligibility (Card, Chetty, and Weber 2007; Schmieder, von Wachter, and Bender 2012), unless UI benefits are very generous or act as transition to early retirement (Jaeger, Schoefer, and Zweimueller 2018). We find evidence that layoff intensities are higher when workers are eligible for UI benefits and when total benefits are high relative to formal employment and layoff costs. Furthermore, our results suggest that informal labor markets play an important role in generating higher layoff intensities for workers who are eligible for UI benefits.

Finally, our results relate to the literature on temporary layoffs and recalls in the presence of UI. Temporary layoffs account for a high fraction of unemployment inflow (Feldstein 1978; Katz and Meyer 1990b) with high recall rates for laid off workers (Nekoei and Weber 2015; Fujita and Moscarini 2017). Our evidence of more frequent temporary formal unemployment spells when the UI payments are high and layoff costs are low is in line with previous studies showing that temporary layoffs are more common under imperfect experience rating (Feldstein 1978; Topel 1983, 1985; Anderson and Meyer 1994; Card and Levine 1994). We complement these studies by showing that firms and workers use temporary formal layoffs to extract rents from the UI system. Additionally, our results suggest that the option to employ workers informally makes temporary layoffs a more prominent concern in designing an optimal UI system for mid-income and developing countries.

It is important to consider the relevance of the findings beyond the specific context of the paper. While our findings are obtained during a severe recession in Brazil, Carvalho, Corbi, and Narita (2017) obtain almost identical point estimates on layoff intensities for an extended period around the reform.[9] Moreover, evidence on the effects of UI in recessionary

reducing strategic unemployment inflow.

[8]Most of the evidence on higher layoff intensities for workers eligible for UI benefits is from Canada (Christofides and McKenna 1995, 1996; Green and Riddell 1997; Baker and Rea 1998; Green and Sargent 1998).

[9]Existing evidence on the relationship between recessionary environments and incentive effects of UI is

periods is of particular interest, as UI benefits are often extended (Rothstein 2011; Valletta 2014; Farber and Valletta 2015; Kroft and Notowidigdo 2016). Finally, while informal labor markets are more prevalent in mid-income and developing countries, developed countries also feature some degree of informality in parts of their labor markets (Hazans 2011).

# 2 Institutional Background and Data

This section provides information about Brazil's UI system, 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 is required to hold a working card, which it is mandatory for employers to sign whenever a worker is hired, promoted, or dismissed. This information is reported to the Ministry of Labor 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). Funding for the UI system comes from the social integration program (PIS), which is mainly financed through 0.65% tax on firm sales.

UI applies to formally employed private sector workers. Benefits are paid for three to five months, depending on workers' formal employment history. Three payments are made if a worker was employed for between six and eleven months during the last 36 months, four payments are made if a worker was employed for between 12 and 23 months during the last 36 months, and five payments are made if a worker was employed for at least 24 months during the last 36 months. In 2015, the monthly payments range from 1 to 1.76 minimum salaries, depending on the average pre-layoff wage. Importantly, the UI system does not feature a strong experience rating mechanism. If a firm lays off a worker, it must pay an additional fifty percent of the total contributions that have accumulated in the employee's FGTS. On average, this layoff cost amounts to 8-19 percent of the expected benefits accruing to the worker, depending on the pre-layoff wage (the penalty is relatively lower for lower pre-layoff wages). Eighty percent of this penalty is paid directly to the worker, rather than being used

mixed. While Schmieder, von Wachter, and Bender (2012) show that incentive effects of UI tend to be weaker during recessions, Card et al. (2015a) document that UI durations are more responsive to benefit levels in the aftermath of the Great Recession.

to fund the UI system. Thus, implied experience rating, after accounting for payments that remain within the firm-worker relationship, is only about 1.6-3.8 percent of the UI benefits paid to the worker through the UI system. In contrast, firing workers with a valid legal reason does not entail penalties. The hurdle of providing sufficient evidence is high, and judges tend to rule in favor of employees. Only 3.5 percent of all dismissals are classified as firings with justified cause. Workers receive benefits only when they are laid off rather than being fired.

## 2.2 UI Reform

To be eligible for UI benefits prior to March 1, 2015, a worker had to be employed for at least six consecutive months prior to layoff, had to be laid off without a justified reason, not be earning other labor income, and not have successfully applied for UI benefits during the previous sixteen 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. While it was anticipated that UI would be reformed at some point, both the sudden implementation and the content of the new law were fully unexpected.[10] Since the UI reform was announced unexpectedly only two months before its implementation, workers with a tenure of four to seven months during January to April 2015, who constitute the main sample in this paper, were already in formal employment before the announcement of the reform. The main driver for the quick implementation and the tightening of eligibility criteria came from attempts on the part of the government to reduce the growing budget deficit. The size and duration of UI benefits were not altered. Importantly, contributions to the UI system were unaffected by the reform. Thus, the reform had no direct effect on employers' demand for formal labor.

The reform affected workers with less than two successful prior applications for UI benefits. For these workers, eligibility criteria were substantially tightened. To be eligible for UI benefits after the reform, a longer pre-layoff employment history than the six-month threshold from before the reform was required. Specifically, workers who applied for the first time required documented employment of at least 18 months in the 24 months prior to layoff. Workers who applied for the second time required 12 months in formal employment during the last 16 months (see Figure 1). This provisional measure was applied from March 2015

[10]Estadao Politica, December 29, 2014, "Forca Sindical nega ter sido consultada sobre ajuste em beneficios". Doornik et al. (2018) show that firms whose workers benefit more from a more generous UI system experienced a larger drop in their stock price following the announcement of the reform on December 29.

and became law in July 2015 with some adjustments.[11]

## 2.3 Data

We use data from RAIS (Relacao Anual de Informacoes Sociais), a large restricted-access matched employee-employer administrative dataset from Brazil. The RAIS database records information on all formally employed workers in a given year and is maintained by the Ministry of Labor in Brazil. All formally-registered firms in Brazil are legally required to report annual information on each worker that the firm employs. RAIS includes detailed information on the employer (tax number, sector of activity, establishment size, geographical location), the employee (social security number, age, gender, education), and the employment relationship (wage, tenure, type of employment, hiring date, layoff date, etc.). We use data from RAIS for the 2013–2015 period. By the end of 2014, the database covers about 50 million formal employees. We exclude all public sector employees, since they do not participate in the UI program.

For our main identification strategy, we focus on employees with a consecutive formal working history of four to seven months at a given point in time. Additionally, we use information on the location of the firm (municipality), its two digit industry classification (National Classification of Economic Activities), and information on workers' occupations (Classificacao Brasileira de Ocupacoes) for our empirical analysis. Our main empirical specification compares the period before the implementation of the UI reform (January–February 2015), and the period after the implementation of the reform (March–April 2015). We use data for the same months from the previous year to control for seasonal effects.

In Table 1, we provide evidence that workers with a tenure of six or seven months, who are affected by the reform, and workers with a tenure of four or five months, who are not directly affected by the reform, are indistinguishable in terms of observable characteristics before the implementation of the reform. Both groups of workers are virtually identical in terms of age, salary, gender, education, the size of the firm that employs them, and the industries in which they are employed.

To exploit cross-sectional variation in labor market informality, we combine the linked

[11]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 9 months of employment in the last 12 months. These adjustments do not directly affect workers around the six-month threshold that we exploit for our empirical analysis.

employer-employee data from RAIS with information on labor market informality from the Brazilian census in 2010. The census asks whether or not an individual has a job, and whether or not this job is formal, and reports labor market informality shares for twenty different industry classifications (see Table 2). 66 percent of domestic services employees are shown as working informally. The most formal industry, electricity and gas, has only 5.5 percent informal workers. In terms of geographic variation in informality, most municipalities fall within the range of 20 to 70 percent labor market informality (Figure 3). Labor market informality at the municipality level captures variation that is distinct from industry composition. When we compute the difference between the share of labor market informality and the share of informality predicted by industry composition in the bottom panel, its distribution is highly correlated with the raw informality measure. Additionally, informality is not limited to certain areas in Brazil, but is prevalent throughout the country, with somewhat higher average informality in the north (Figure 4).

From the Ministry of Labor in Brazil, we obtain data on the history of UI benefits payments to workers, which we use to classify workers into those affected by the reform (less than two previous UI benefits spells), and those unaffected by the reform (two or more previous UI benefits spells).

Finally, we use data from the monthly employment survey Pesquia Mensal de Emprego (PME). This survey interviews 44,189 individuals in six metropolitan areas (Recife, Salvador, Belo Horizonte, Rio de Janeiro, Sao Paulo, Porto Alegre). We extract data on all individuals who are laid off from formal employment with a tenure of four to seven months, and all informally employed workers laid off with a tenure of four to seven months in their previous formal job. The sample period for tests using the PME survey data is from September 2014 to August 2015. This provides us with a sample of 1,968 workers who are laid off with a tenure of four to seven months during the sample period, and 486 informally employed workers who had previously been laid off with a tenure of four to seven months. In addition to workers' tenure, the survey provides information on whether a worker is formally employed, informally employed, or unemployed, and different buckets for the number of employees of the firm in which a worker is formally or informally employed. The unique combination of these different sources of variation provided by our institutional setting also allows us to provide strong evidence in support of our identifying assumptions, which we discuss in Section 3.5.

# 3 Empirical Analysis

This section describes the empirical analysis and presents our results. Our empirical design builds on three sources of variation provided by the institutional setting. First, we exploit a discontinuity in eligibility for UI benefits at six months' tenure before the implementation of the UI reform. Second, the design of the reform generates two sources of variation that provide us with natural control groups to account for changes in employment and unemployment patterns that are not related to the the UI system. After the reform, workers above and below the six month threshold are ineligible for UI benefits. This provides us with counterfactual patterns for workers above and below the threshold when neither group is eligible for UI benefits, which we exploit in a difference-in-differences framework. Additionally, workers with at least two previous UI benefits spells are not affected by reform. This provides us with an additional counterfactual to control for time-series changes in unemployment inflow and outflow patterns of workers around the six months threshold in a tripple-difference estimation.

To understand the effects of UI in the presence of informal labor markets, we combine several pieces of evidence. First we examine how eligibility for UI affects unemployment inflow and how this depends on the degree of labor market informality. Next, we assess the underlying mechanism that explains the unemployment inflow and outflow patterns that we observe. Combining survey evidence and administrative data, we examine whether workers flow from formal to informal employment upon layoff if they are eligible for UI benefits, and whether some of them stay employed informally with the same firm that laid them off formally. Finally, we examine whether firms and workers share rents from the UI system through lower equilibrium wages.

## 3.1 Unemployment Inflow

We start by examining unemployment inflow patterns for workers below and above the six month tenure threshold for workers affected by the reform. The top panel in Figure 5 depicts layoff intensities for workers with different tenures, separately for the months from January to April 2015. For workers with tenure of six to seventeen months, the probability of being laid off decreases after the reform (March and April), in line with the shift of the eligibility threshold from six to twelve or eighteen months. In particular, there is a sharp drop in the probability of being laid off for workers with tenure of six months, who lose eligibility for

benefits after the reform, relative to unemployment inflow for workers with a tenure of five months, who are ineligible for benefits before and after the reform.

We confirm the insights from the graphical analysis statistically by estimating

$$Layoff_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it}, \quad (1)$$

where $Layoff_{it}$ is a dummy variable that takes the value of one if worker $i$ is laid off in month $t$ and zero otherwise.[12] The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six months and above and zero for workers with tenure below six months. The dummy variable $Reform_t$ takes the value of one from March 2015 and zero before March 2015. The parameter of interest is $\beta_3$, which compares the difference in unemployment inflow after the reform, when neither group of workers is eligible for UI benefits, to the difference in unemployment inflow between both groups of workers before the reform, when workers above the threshold are eligible for UI benefits.

The results are gathered in Table 3. We depict the results for our strictest specification with municipality-industry-occupation-month fixed effects that controls for local shocks at the industry-occupation level. All results are qualitatively identical with month, municipality-month, or municipality-industry-month fixed effects. We start by estimating changes in unemployment inflow for workers with six to eleven months tenure relative to workers with four to five months tenure in the year after compared to the year before the reform (column I). We find that while workers above the eligibility threshold are one percentage point more likely to flow into formal unemployment when they are eligible for UI benefits before the reform than workers below the treshold who are ineligible for UI benefits, this difference declines by 0.39 percentage points when workers lose eligibility for UI benefits after the reform. Focusing on workers just around the threshold (four to seven months tenure) we find that unemployment relatively declines for workers above the threshold by 0.57 percentage points (column II).[13]

One concern with comparing layoff intensities for workers in the two years around the reform is that workers may select into jobs with different expected employment duration (Green and Riddell 1997). Our data allows us to focus on a narrow time period of two months before and after the reform. Since the unexpected announcement of the reform occurred only

[12] We refer to layoffs as separations between firms and workers that allow workers to apply for UI benefits, as opposed to workers being fired for justified reasons, in which case they are ineligible for UI benefits.

[13] Carvalho, Corbi, and Narita (2017) document almost identical magnitudes when performing the same analysis for an extended time period around the reform (January 2012 to December 2015).

two months before its implementation, workers with at least four months' tenure in March 2015 entered formal employment before the annoucement of the reform, which eliminates concerns about changes in ex ante selection into jobs with different expected employment duration. Comparing unemployment inflow for workers with tenure above and below the six months threshold in the two months before and after the reform (columns III and IV), we find the same pattern with similar magnitudes.[14] In the strictest specification focusing on the four months around the reform and workers with tenure around the threshold (column IV), we find that unemployment inflow decreases by 0.49 percentage points (or 11.8 percent) when workers lose eligibility for UI benefits.

One may be concerned that workers may be affected by the annoucement of the reform in January and February. For example, workers with tenure of six months may have a higher incentive to be laid off before the reform is implemented when they are still eligible for UI benefits. To ensure that our results are not affected by announcement effects of the reform, we compare unemployment inflow in March and April to unemployment inflow in November and December, the two months before the announcement of the reform, in column V. We find qualitatively identical results with slightly higher magnitudes. This suggests that the previous results are not affected by announcement effects of the reform.

We ensure that the findings are not affected by seasonal or other time-series effects by comparing the pattern around the reform in 2015 to the previous year and by comparing the changes in unemployment inflow for workers affected by the reform to those unaffected by the reform in a tripple-difference estimation. We observe no similar patterns in the year before the reform (Figure 5, middle panel, and Table 9, column II), and find that workers who are not affected by the reform do not experience any changes in unemployment inflow around the reform (Figure 5, bottom panel, and Table 9, column IV).

We next examine the role of informal labor markets for the patterns we observe, by exploiting two sources of variation in labor market informality: cross-sectional variation in informality across industries and municipalities. We find that higher unemployment inflow for workers eligible for UI benefits is stronger in more informal labor markets. In Figure 6, we split the sample into workers employed in industries with above (top panel) and below (bottom panel) median levels of labor market informality. The graphical evidence shows that higher unemployment inflow for workers with six or seven months' tenure before the reform is mainly driven by workers in industries with above median levels of informality. For these

[14]The results are not driven by a"relabeling" of voluntary departures as layoffs. We observe no change in voluntary quits around the reform (Table 9, column I).

workers, we observe a substantial drop in unemployment inflow in March and April when they lose eligibility for UI benefits. In industries with below median levels of informality, we observe a smaller change in unemployment inflow. Similarly, in Figure 7, we find that in municipalities with above median levels of informality (top panel), unemployment inflow decreases by about two percentage points for workers who lose eligibility for UI benefits after the reform. In municipalities with below median levels of informality (bottom panel), the magnitude of the effect is lower than one percentage point.

To formally assess how UI incentive-based unemployment inflow interacts with the presence of informal labor markets, we add a continuous variable $Informal$, which is the share of informal employment in a given industry or municipality, and its interaction with the other dependent variables, to equation (1). The results are displayed in Table 8, columns I and IV. The results in column I exploit variation in labor market informality at the industry level. We find that a ten percentage point increase in labor market informality leads to a 0.17 pp stronger decrease in unemployment inflow after the reform for workers with tenure of six to eleven months relative to workers with tenure of four to five months. The effect is slightly higher with 0.24 pp per ten percentage points increase in labor market informality at the municipality level (column IV).

## 3.2 Underlying Mechanism

Several mechanisms could lead to higher layoff intensities when workers are eligible for UI benefits. To be eligible, workers need to be laid off by their employer. Layoffs could be worker-induced, for example, workers may elicit layoffs through shirking. Higher layoff intensities could also be caused by firms that temporarily lay off workers due to demand shocks. Firms facing demand shocks may opt for workers eligible for UI benefits, as these workers are less likely to search for alternative employment (Jurajda 2002). Alternatively, firms may collude with workers to extract rents from the UI system by laying them off while they are eligible for benefits to rehire them when benefits cease. Collusion could be implicit by firms gaining a reputation for timing workers' formal unemployment spells with eligibility for UI benefits and workers responding by accepting lower wages (Christofides and McKenna 1996), or rather explicit with firms and workers mutually agreeing to time formal unemployment spells with UI eligibility in exchange for lower wages (Feldstein 1976; Baily 1977).

To differentiate between these mechanisms, we start by exploring whether firms that lay off workers when they become eligible for UI benefits rehire the same workers when their

benefits cease. Specifically, we estimate the probability of a worker being rehired by the same firm four to ten months after layoff, by estimating

$$Rehire[4-10]_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it}, \quad (2)$$

where $Rehire[4-10]_{it}$ is a dummy variable that takes the value of one if a worker returns to the same firm four to ten months after being laid off and zero otherwise. Since workers are eligible for at least three months of UI benefits, we start the rehiring window four months after layoff. We end the rehiring window ten months after layoff since workers may not apply for UI benefits within 16 months of their last successful application, and firms that repeatedly layoff and rehire the same worker may therefore have an incentive to rehire workers up to ten months after layoff for them to be eligible for benefits after another six months of formal employment. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six months or above at layoff and zero for workers with tenure below six months at layoff. The dummy variable $Reform_t$ takes the value of one for workers laid off in March 2015 or later and zero for workers laid off before March 2015.

The results are gathered in Table 4. We expect that if higher unemployment inflow is driven by shirking, firms are unlikely to rehire the worker. In contrast, if firms lay off workers due to demand fluctuations, or because they collude to time unemployment spells with eligibility for UI benefits, firms would be more likely to rehire the same worker when benefits end. We start by exmining rehiring patterns for workers laid off with tenure between four to eleven months in the year before or the year after the reform (column I). The results show that the probability of being rehired by the same employer four to ten months after layoff is about one percentage point higher before the reform for workers with tenure of six to eleven months at layoff, compared to workers with four or five months' tenure at layoff. After the reform, when both groups of workers are ineligible for benefits, the difference in rehiring by the same firm four to ten months after layoff vanishes. We find similar results when we restrict the sample to workers with tenure between four to seven months (column II).

To ensure that the results are not driven by changes in the probability of workers to be rehired by any firm four to ten months after layoff, we restrict the sample to workers that return to formal employment four to ten months after being laid off (columns III and IV). We find that the effect is about twice as large when we condition on reemployment four to ten months after layoff. Restricting the sample to the four months around the reform (columns V to VIII) shows the same patterns with slightly higher magnitudes. In the strictest specification for workers with tenure of four to seven months (column VIII), we find that

workers are 1.68 percentage points more likely to be rehired by the same firm four to ten months after layoff when they were eligible for UI benefits at layoff. We find no similar patterns for the same months in the year before the reform (Table 9, column III), and for workers with more than two previous UI benefits spells, who are not affected by the reform (Table 9, column V).

Higher reemployment by the same firm is consistent with collusion between firms and workers to extract rents from the UI system, but also with firms laying off worker temporary when facing demand fluctuations. To sharpen the interpretation of our results and to differentiate between firm-worker collusion and temporary layoffs in response to demand fluctuations, we exploit differences in rehiring incentives between the mechanisms. Since workers can only apply for UI benefits 16 month after their last successful application, a firm-worker pair that engages in repeated temporary layoffs may prefer workers to return to formal employment at two distinct points of unemployment duration. If they seeks to maximize formal employment spells, the firm should rehire the worker four to six months after layoff just when benefits cease. If they seeks to minimize formal employment spells, the firm should rehire the worker nine to ten months after layoff such that the worker requalifies for UI benefits after six months of formal employment (see Figure 2). In contrast, the main reason for firms facing demand fluctuations to lay off workers that are eligible for UI benefits is that these workers are less likely to search for alternative employment. Thus, firms should rehire workers when they become more likely to seek alternative employment, which suggests that same firm reemployment patterns should closely follow reemployment patterns by other firms. A demand fluctuation mechanism provides no incentive to rehire workers exactly nine to ten months after layoff.

We start by depicting employment rates by firms that laid off the worker (same firm) and firms that did not lay off the worker (different firms) in Figure 8. Specifically, we plot the difference in hazard rates of unemployment outflow for workers with tenure of six to seven months minus workers with tenure of four to five months. Results for workers laid off in 2013 are depicted by dashed lines and for workers laid off in 2015 by solid lines. The left top plot comprises workers with one or fewer previous unemployment spells that are affected by the reform. Before the reform, we observe that workers with tenure of six to seven months at layoff are less likely to be rehired by the same firm while they are all on UI benefits (one to three months after layoff) compared to workers with tenure of four to five months. Once benefits cease (four to six months after layoff), workers that qualified for UI benefits at layoff are more likely to be rehired by the same firm than workers who did not qualify for benefits. While we find no differences in rehiring rates by the same firm based seven to eight months

after layoff, workers that qualified for UI benefits at layoff are more lilely to be rehired by the same firm nine to ten months after layoff. This is consistent with firms rehiring workers such that they qualify for UI benefits again after six months of formal employment. After the reform, when workers with tenure of six to seven months at layoff are no longer eligible for UI benefits, differences in same firm-hiring patterns relative to workers with tenure of four to five months at layoff disappear. The bottom left panel shows that for workers unaffected by the reform, the pre-reform patterns persist in 2015 after the reform.

In terms of rehiring by different firms, we observe that workers are less likely to be hired while they are on benefits (one to five months after layoff) before the reform (top right plot, dashed line). Once benefits end, workers with tenure of six to seven months at layoff are equally likely to be hired by different firms as workers with tenure of four to five months tenure. After the reform, when workers with tenure of six to seven months are no longer eligible for UI benefits (solid line), rehiring rates are similar to workers with tenure of four to five months throughtout the twelve months after layoff. The bottom right panel shows that workers unaffected by the reform exhibit the same hiring patterns by different firms in 2013 and 2015. Together, the graphical evidence in Figure 8 suggests that the changes in rehiring rates by the same firm are driven by strategic layoff and reemployment decisions to time workers' UI spells with eligibility for UI rather than temporary layoffs in response to demand fluctuations.

To formally assess whether firms are more likely to rehire workers nine to ten months after layoff if they are eligible for UI benefits at layoff, we estimate

$$\begin{aligned} Hire[9-10]_{it} &= \alpha + \beta_1 \cdot 6Months_{it}\beta_2 \cdot Same_{it} + \beta_3 \cdot Reform_t + \beta_4 \cdot 6Months_{it} * Same_{it} \\ &\quad + \beta_5 \cdot 6Months_{it} * Reform_t + \beta_6 \cdot Same_{it} * Reform_t \\ &\quad + \beta_7 \cdot 6Months_{it} * Same_{it} * Reform_t + \epsilon_{it}, \end{aligned} \tag{3}$$

where $Hire[9-10]_{it}$ is a dummy variable that takes the value of one if worker $i$ is hired nine or ten months after layoff and zero if worker $i$ is hired seven to eight months after layoff. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six months or above at layoff and zero for workers with tenure of four to five months at layoff. The dummy variable $Reform_t$ is one for the year 2015, and zero for the year 2013. We exclude two months of January and February, which are under the pre-reform regime in 2015, and we skip 2014 since for most workers laid off in 2014 the hiring decision nine to ten months after layoff falls in the post-reform period and firms' incentive to rehire workers nine to ten months after layoff hinges on workers' ability to requalify for UI benefits after six months of

formal employment. The dummy variable $Same_{it}$ takes the value of one if worker $i$ is rehired by the same firm that laid her off and zero if she is hired by a different firm.

The results are gathered in columns IX and X of Table 4. The sample in column IX comprises all workers with tenure of four to eleven months at layoff in March to December of 2013 and 2015. We find that before the reform workers eligible for UI benefits are about four percentage points more likely to be rehired by the same rather than by another firm nine to ten months rather after layoff compared to the same difference seven to eight months after layoff. After the reform, when workers with four to eleven months' tenure at layoff are ineligible for UI benefits, differences in rehiring rates by the same firm nine to ten compared to seven to eight months after layoff disappear. We observe the same patterns when we restrict the sample to workers with four to seven months' tenure at layoff (column X). The magnitudes are somewhat higher for workers around the threshold, which is consistent with repeat-colluders combining six months of formal employment with ten months formal unemployment spells. These results suggest that higher reemployment rates by the same firm are driven by collusion between firms and workers to time unemployment spells with eligibility for UI benefits rather than temporary layoffs due to demand fluctuations.

In Table 5, we examine whether patterns consistent with firm-worker collusion are more prevalent when rents that can be extracted from the UI system are higher. Specifically, we examine whether strategic unemployment inflow and reemployment patterns consistent with collusive behavior are more common when the ratio of total rents (UI benefits payments) to the sum of total layoff costs (penalty) and formal employment costs (taxes) is higher.[15] Since this surplus is correlated with benefits accruing to workers, which may elicit higher worker-induced layoff intensities, we include the replacement rate to control for workers' incentives.

The results in column I show that firms are more likely to lay off workers who are eligible for UI benefits when the potential rents that can be extracted from the UI system are higher. When workers with six to seven months' tenure lose eligibility for UI benefits after the reform, these workers are relatively less likely to flow into unemployment, by 0.14 percentage points per ten percentage point increase in the rents that can be extracted from the UI system. The results in column II show that workers are 0.35 percentage points more likely to be rehired by the same firm when their benefits cease per ten percentage point increase in the rents that can be extracted from the UI system. This is equivalent to an about 25 percent

[15]Total benefits payments exceed layoff costs for virtually all workers. For 62.3 percent of workers, total benefits exceed the sum of layoff costs and taxes from six months of formal employment.

increase in collusive behavior per ten percentage point increase in rents.

With respect to replacement rates, we find that workers are less likely to be rehired by the same firm when their benefits end if replacement rates are high (column II). This confirms our earlier conjecture that workers are less likely to be rehired by the same firm if they induce layoff. Together, the results strengthen the interpretation that collusion between firms and workers plays an important role in explaining higher layoff intensities when workers are eligible for UI benefits and show that collusive behavior is more prevalent when potential rents that can be extracted from the UI system are higher.

Finally, we examine whether patterns of rehiring by the same firm are stronger in industries and municipalities with large informal labor markets. The results in Table 8, columns II and V show that firms in more informal industries are significantly more likely to lay off workers when they are eligible for benefits, and to rehire them just when their benefits end. Specifically, a ten percentage point increase in labor market informality at the industry level leads to a 0.59 pp increase in the rehiring of workers by the same firm just when UI benefits stop. The results are somewhat stronger with 0.67 pp when we exploit labor market informality at the municipality level (column V).

The results in this section suggest that part of the additional unemployment inflow is driven by collusion between firms and workers to time UI spells with eligibility for UI. A simple back-of-the-envelope calculation suggests that around 21 percent of the additional unemployment inflow due to UI is driven by such collusion. The base rate of reemployment by the same firm four to ten months after layoff after the reform when workers with tenure of six to seven months at layoff are ineligible for UI benefits is 5.30 percent. Thus, from every 100 workers, 5.30 workers are rehired by the same firm four to ten months after layoff. When workers with tenure of six to seven months are eligible for UI benefits before the reform, the probability of being rehired by the same firm is 1.68 percentage points higher (Table 4, column VIII), or 6.98 percent. Combined with the additional unemployment inflow of workers eligible for UI benefits by 11.8 percent, this implies that for every 111.8 workers, 7.80(=111.8*0.0698) workers are rehired by the same firm when they are eligible for UI benefits at layoff. Together, this suggests that the additional 11.8 workers that flow into formal unemployment due to UI benefits contribute 2.50(=7.80-5.30) workers that are reemployed by the same firm four to ten months after layoff, which means that 0.2119(=2.50/11.8) percent of the additional workers flowing to formal unemployment due to UI are rehired by the same firm.

Based on the results in Table 8, we can compute the share for different levels of labor

market informality using the same methodology. Figure 12 plots the share of strategic unemployment inflow and firm-worker collusion for different degrees of labor market informality. For labor markets with low informality, strategic unemployment inflow accounts for only about five percent of all unemployment inflow. In contrast, at high levels of labor market informality more than a quarter of all unemployment inflow is accounted for by UI induced incentives. The share of UI inflow explained by collusion between firms and workers to time unemployment spells with eligibility for UI benefits increases by around five percentage points from low to high informality labor markets. While the share of UI-induced unemployment inflow related to firm-worker collusion increases by only five percentage point, total unemployment inflow based on collusion between firms and workers increases strongly due to the high increase strategic unemployment inflow due to UI-induced incentives.

## 3.3 Informal Employment

Next, we assess whether firms employ workers informally while they are on UI benefits. Informal labor markets allow workers to receive UI benefits while being (informally) employed. Given the higher unemployment inflow documented in the previous section, it seems plausible that the patterns are driven by workers transitioning from formal to informal employment. By its nature informal employment is difficult to observe and not recorded in administrative data on an employer-employee matched level. Instead, we examine evidence on informal employment while workers are on UI benefits from PME survey data,[16] by estimating

$$Informal_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it}, \quad (4)$$

where $Informal_{it}$ is a dummy variable that takes the value of one if worker $i$ reports to be informally employed following a month in which she reports to be formally employed and zero if she reports to be unemployed. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six months or more at layoff, and zero for workers with tenure below six months. The dummy variable $Reform_t$ takes the value of one after and zero before the reform.

The results are displayed in Table 6. We find that workers with tenure of six to eleven months at layoff are almost two percentage points more likely to transition from formal to informal employment when they are eligible for UI benefits in the six months before the

[16]For the PME survey, households are interviewed over two separate four-month periods that are eight months appart from each other giving it a panel structure.

reform (columns I).[17] During the six months after the reform, when workers with six to eleven months tenure at layoff are ineligible for UI benefits, they are about equally likely to transition to informal employment after being laid off as workers with tenure of four to five months at layoff. When we restrict the sample to workers with tenure between four to seven months at layoff, we observe almost identical effects (column II). Focusing on the four months window around the reform (columns III and IV), the magnitude of the effect is even larger with workers that are no longer eligible for UI benefits after the reform becoming 5.62 to 6.88 percentage points less likely to transition from formal to informal employment upon layoff.

Since the PME survey data does not allow us to differentiate between workers affected and unaffected by the reform, the actual magnitude of the effect on treated workers is about 1.5 times the estimated effect (only two-thirds of workers are affected by the reform). For the sharpest test in column IV, this implies that workers affected by the reform are 10.32 percentage points less likely to transition from formal to informal employment when they are no longer eligible for UI benefits. Relative to the 11.5 percent of strategic unemployment inflow estimated in Section 3.1, this implies that 89.74 percent of strategic unemployment inflow at the six month threshold is related to informal employment, which is consistent with the observation that most of the additional unemployment inflow occurs in industries and municipalities with higher degrees of labor market informality.

Since we do not observe which firm hires a worker informally, we complement the survey evidence with indirect evidence from administrative data, to assess what fraction of workers are informally employed by the same firm that laid them off. When firms lay off a worker, they often hire a different worker as a replacement. However, if firms lay off workers formally, but continue to employ them informally, they are less likely to hire a new worker. We compare the probability that firms hire a replacement worker after laying off a worker with six or seven months' tenure, who is eligible for UI benefits before the reform, and after laying off a worker with four or five months' tenure, who is never eligible for UI benefits. Continued informal employment while workers are on benefits would predict that hiring rates for replacement workers are lower for workers laid off with a tenure of six or seven months when they are eligible for benefits before the reform.

We start with a graphical depiction of the rate at which firms hire a new worker in the same occupation within one month of laying off a worker with a tenure of four to five months

[17] PME data is only available until February 2016, which is why we restrict the sample period to six months before and after the reform.

(dashed line), or laying off a worker with a tenure of six to seven months (solid line) in the top panel in Figure 9. Before the reform, firms are about five percentage points less likely to hire a replacement worker after laying off a worker with tenure of six to seven months, compared to laying off a worker with a tenure of four or five months. Strikingly, this discrepancy disappears from the month of the implementation of the reform, when workers with a tenure of six to seven months are no longer eligible for UI benefits. These differences are not driven by seasonal layoffs or replacement hiring patterns in the months from January to April as evident from the patterns in the year before the reform and the bottom panel, which shows that for workers unaffected by the reform differences in the rate of hiring replacement workers persist after the implementation of the reform.

We confirm the insights from the graphical analysis statistically in Table 7, by estimating

$$New\ hire_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it}, \quad (5)$$

where $New\ hire_{it}$ is a dummy variable that takes the value of one if a firm hires a new worker in the same occupation within one month of laying off worker $i$ and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six months or more at layoff, and zero for workers with tenure of four to five months at layoff. The dummy variable $Reform_t$ takes the value of one after the reform and zero before the reform.

The results are gathered in Table 7. Before the reform, firms are about one and a half percentage points less likely to hire a new worker within a month of laying off a worker with tenure of six months or above, compared to the probability of hiring a new worker within a month of laying off a worker with tenure of four to five months. After the reform, this difference in hiring a replacement worker depending on tenure at layoff almost completely disappears. This suggests that some firms continue to informally employ some of the workers formally laid off when they are eligible for UI benefits. The results in Table 9, column VI show that for workers unaffected by the reform replacement hiring rates remain lower for workers with tenure of six to seven months at layoff.

When we compare the graphical evidence for above and below median industries (Figure 10) and municipalities (Figure 11), we observe that the effects are stronger in industries and municipalities with larger informal labor markets, which is further consistent with firms employing workers informally instead of hiring a replacement worker. The results in Table 8, columns III and VI statistically confirm that lower rates of replacement hiring while workers are on benefits are more prevalent in industries (column III) and municipalities (column VI) with a higher share of informal labor markets by 1.39 and 1.45 percentage points, respectively,

per ten percentage points increase in labor market informality.

From our previous analysis, we know that 11.8 percent of unemployment inflow is a strategic response to UI benefits eligibility. When workers are eligible for UI benefits, they are 1.19 percentage points less likely to be replaced by a new workers after the reform (Table 7, column IV). This suggests that (1.19/11.8)=10.08 percent of the additional unemployment inflow is related to workers that stay informally employed at the same firm. This means that among the strategically unemployed workers that transition to informal employment upon layoff, (10.08/89.74=)11.24 percent, whereas 88.76 percent are employed by other firms.

## 3.4 Rent-Sharing Through Wages

Finally, we examine whether firms and workers share rents from the UI system through lower equlibrium wages (Feldstein 1976; Baily 1977), exploiting the fact that the reform only applies to part of the workforce. Figure 13 depicts the time-series evolution of formal hiring scaled by total employment (top panel) and the log of average hiring wages (bottom panel), separately for workers with fewer than two successful past applications for UI benefits, who see their eligibility criteria for UI benefits tightened (solid lines), and for workers with at least two successful past UI benefits applications, who are unaffected by the reform (dashed lines). To facilitate comparison, all plots are adjusted for calendar month and worker group (workers affected vs. unaffected by the reform) fixed effects. Starting in January 2015, the month after the announcement of the reform, we observe a relative drop in the hiring of workers affected by the reform. A simultaneous relative increase in wages for newly hired workers who face stricter eligibility requirements for UI benefits suggests that the drop in formal employment is driven by a reduction in formal labor supply.

To formally assess whether workers demand lower wages when it is easier to extract rents from the UI system, we compare changes in wages for workers affected by the reform and workers for whom eligibility criteria are unaffected by the reform, by estimating

$$log(wage)_{gt} = \alpha + \beta_1 \cdot Affected_{gt} + \beta_2 \cdot Reform_t + \beta_3 \cdot Affected_{gt} * Reform_t + \epsilon_{it}, \quad (6)$$

where $log(wage)_{gt}$ is the average wage of the group of workers affected by the reform or the group of workers unaffected by the reform, hired in a given industry in a given municipality in month $t$. Workers' incentives are affected from the time they are aware of the reform's effects. Since the reform was announced on December 29, 2014, we define the $Reform_t$

dummy as one from January 2015. The dummy variable $Affected_{gt}$ takes the value of one for the group of workers with less than two successful past applications for UI benefits whose eligibility criteria are tightened by the reform and zero for the group of workers with two or more successful past applications for whom eligibility criteria remain unchanged.

The results are displayed in Table 10, where we also examine changes in formal hiring and employment by replacing the dependent variable in equation (6) with total hiring employment relative to total employment in December 2014. The results in columns I and II indicate that formal hiring of workers who are less likely to qualify for UI benefits after the reform decreases relatively by about 0.4 percent of the pre-reform labor force. Continued lower hiring is not driven by lower turnover rates. We also observe a relative drop of about six percent in affected workers' formal employment (columns III and IV). Columns V and VI show that wages of newly hired workers for whom qualifying for UI benefits becomes harder after the reform increase relatively by 0.5-0.8 percent. Columns VII and VIII confirm that the increase in hiring wages for workers affected by the reform is driven by an increase in wages for the same worker rather than a change in the composition of hired workers. The change in hiring wages compared to their last job during the previous twelve months is relatively higher by 0.5 percent for workers affected by the reform.

While lower formal labor supply is consistent with colluding firms and workers implicitly or explicitly agreeing on lower wages to share rents from the UI system, and firms benefiting from higher labor supply when timing workers' unemployment spells according to UI benefits eligibility (Feldstein 1976; Baily 1977; Christofides and McKenna 1996), the prospect of future eligibility for UI benefits may lead to an entitlement effect, according to which workers value formal employment more (Mortensen 1977; Hamermesh 1979; Bergolo and Cruces 2016). We strengthen the interpretation of the results by examining whether changes in wages are stronger in municipality-industry cells with higher degrees of firm-worker collusion. For each municipality-industry cell of the data, we compute the degree of strategic unemployment before the reform as the ratio of workers laid off with a tenure of six or seven months to the number of workers laid off with a tenure of four to seven months. Similarly, we compute the degree of collusive behavior as the fraction of workers rehired by the same firm after four to ten months among all workers laid off with a tenure of six or seven months and interact the independent variables in equation (6) with these proxies for collusive behavior.

The results collected in Table 11 confirm the interpretation that firms benefit from collusion through paying lower wages. We find that hiring of workers with less than two successful past applications for UI benefits declines relatively more in local industries in which strate-

gic unemployment inflow explains a larger fraction of total unemployment inflow (column I). Similarly, hiring of workers affected by the reform drops more in local industries in which rehiring by the same firm is timed to coincide with UI benefits eligibility before the reform (column II). Consequently, we observe a larger relative drop in employment for affected workers after the reform in local industries with higher strategic unemployment inflow and more reemployment outflow timed according to UI benefits stopping (columns III and IV). In terms of changes in wages, we find that the increase in wages for workers affected by the reform is higher in local industries with a greater degree of strategic unemployment inflow, by 0.04 percent per ten percentage points increase in strategic unemployment (column V). Strikingly, wages increase by an additional 1.9 percent per 10 percentage points increase in the fraction of workers being reemployed by the same firm (column VI). The coefficient in column VI implies that firms that engage in collusion with a worker pay this worker an about 19 percent lower wage.

Based on our estimates for the reduction in wages that firms pay to workers engaged in collusion, the total transfer from workers to firms amounts to 19 percent of their wage over six months. Compared to the total payments that the worker receives from the UI system, with an average 87 percent replacement rate for workers in the sample over three months, the share of the rents transferred to firms through lower formal wages is about 54 percent $((0.19 * 6)/((1 - 0.19) * 0.87 * 3))$. Together, the results in Table 11 suggest that formal labor supply of workers affected by the reform drops particularly strongly and wages increase more in areas where strategic unemployment inflow and outflow are more prevalent before the reform, consistent with lower implicit rent-sharing between firms and workers when UI eligibility criteria are tightened.

Our previous results suggest that the reduction in formal labor supply of workers whose eligibility criteria for UI are tightened by the reform may be stronger when workers have the option to work informally while receiving benefits. Testing this conjecture formally in Table 12, we find that hiring (columns I and II) and employment (columns III and IV) drop more for affected workers in industries (odd columns) and municipalities (even columns) with larger shares of informal labor markets. Consistent with a larger drop in formal labor supply, we find that formal wages for workers affected by the reform increase more in industries and municipalities with larger informal labor markets (columns V and VI).

## 3.5 Identifying Assumptions

Our empirical analysis relies on two main identifying assumptions. First, we need to assume that workers with four to five month tenure are a valid control group for workers with six to seven months tenure around the implementation of the UI reform in March 2015. Specifically, our identification strategy requires that incentives of workers with tenure of four to five months are not directly affected by the reform to serve as a valid control group. Our sharpest tests focus on a narrow time window of two months before and after the reform. Hence, it is unlikely that workers with tenure of four to five months are exposed to different shocks than workers with tenure of six to seven months. Thus, the main challenge to our identifying assumption is that incentives of workers with tenure of four to five months are otherwise affected by the reform, for example, because the marginal value of staying employed changes with the reform. Before the reform, staying employed one or two additional months allows these workers to qualify for UI benefits, whereas after the reform employment of at least twelve months is required to qualify for UI benefits.[18] Our second identifying assumption is that apart from the effects of the UI reform workers affected and unaffected by the reform experience similar time-series changes in labor market outcomes.

The fact that the design of the reform generates two sources of variation allows us to provide rather direct evidence supporting our identifying assumptions by keeping constant one of the sources of varition and varying the other. The top panel of Figure 14 compares UI inflow conditional on workers' tenure for workers affected by the reform (dashed lines) and workers unaffected by the reform (solid lines) in January (black lines) and February (gray lines). The lines for affected and unaffected workers are almost identical for both months. In particular, there is no differential effect at four to seven months' tenure for either group. This suggests that the announcement of the reform does not have a differential effect on the incentives of workers with tenure of four to five relative to workers with tenure of six to seven months. Additionally, workers affected and unaffected by the reform do not experience different trends before the reform, consistent with our identifying assumptions.

In the bottom panel of Figure 14 we provide the same plots for March and April to assess whether the reform directly affects the incentives of workers with tenure of four to five months. Again, we observe identical patterns for workers with tenure of four to five months regardless of whether they belong to the group of workers to whome the reform applies or

[18]This concern, however, is mitigated by our choice of pre-reform window from January to February. During this period the reform had already been announced. Thus, for workers with tenure of four months in January and February and for workers with tenure of four or five months in February, the marginal benefit of staying employed is the same as after the reform.

not. This suggests that workers with tenure of four to five months are not directly affected by the reform and lends further support to our identifying assumption. Additionally, the graphical evidence shows no different patterns for workers with tenure of four to five months for workers to whom the reform applies and workers to whom the reform does not apply, whereas we see clear differences between workers affected and unaffected by the reform above the six months threshold. This evidence suggests that workers with tenure of four to five months are not directly affected by the reform and continue to behave the same way as the group of workers to whom the reform does not apply.

# 4 Discussion

In this section, we summarize our results, quantify the costs of strategic unemployment for the UI system, and discuss the policy implications of our empirical analysis.

## 4.1 Summary of Results

We start by summarizing the main findings in Figure 15. Our findings suggest that UI affects formal unemployment inflow with 11.8 percent of unemployment inflow at the eligibility threshold being driven by UI. About 90 percent of the additional unemployment inflow is related to workers transitioning to informal employment upon layoff, and 11.5 percent of workers transitioning to informal employment stay with the same firm that laid them off. Additionally, we find that about 21 percent of the additional workers flowing to formal unemployment due to UI-induced incentives return to the same firm that laid them off just when their benefits end. Consistent with the high fraction of strategic unemployment inflow being related to workers transitioning to informal employment the effects are concentrated in labor markets (industries, municipalities) with high degrees of informality.

## 4.2 Costs of Strategic Unemployment to the UI System

Before discussing detailed policy implications, we proceed with providing a back-of-the-envelope estimate of quantify the costs of strategic unemployment to the UI system. We document that about 11.8 percent of formal unemployment inflow above the six-month

threshold is due to workers' eligibility for UI benefits (Table 3, columns II and IV).[19] In 2014, 19.4 percent of UI benefits were paid to workers with tenure of 17 months or lower at layoff. The reform does not allow us to estimate strategic unemployment for tenures of 18 months and higher. We compute total UI benefits payments under the most conservative assumption of no strategic unemployment inflow beyond 17 months tenure, and a less conservative assumption of having the same level of strategic unemployment inflow beyond 17 months to obtain a range that most likely contains the actual value.

Since 11.8 percent of unemployment inflow can be attributed to strategic unemployment for workers with tenure of six to 17 months, the share of payments that went to workers that entered formal unemployment due to their eligibility for UI is (0.194*0.118=)2.29 percent. If we assumed that strategic unemployment remains constant above 17 months tenure 11.8 percent of all UI payments due to UI-induced unemployment is 11.8 percent. In 2014, total UI benefits payments were 32.8bn BRL, or 0.6 percent of GDP, 2.29 percent of which amounts to 0.75bn BRL, or 0.0137 percent of GDP, and 11.8 percent of which is 3.87bn BRL, or 0.0708 percent of GDP. Since the UI system in Brazil is mainly financed from taxes on firms' sales, rent extraction from the UI system generates a transfer system to firms for whom it is easier to game the system (Anderson and Meyer 1997a), and reduces the funds available for UI insurance by 2.29-11.8 percent. The annual costs and distortions from strategic unemployment increase with the size of informal labor markets. The estimates from Table 8, column IV, imply that strategic unemployment inflow increases by 0.17 (0.24) percentage points per ten percentage point increase in labor market informality at the industry (municipality) level. We add a plot of the total costs as a fraction of GDP for different levels of labor market informality in Figure 12. For markets with low levels of informality, costs to the UI system due to strategic unemployment inflow are negligible, but reach about 0.05 percent of GDP for high levels of labor market informality even under the most conservative assumption of no strategic unemployment beyond 17 months tenure.

## 4.3 Policy Implications

The trade off in the designing of UI systems is between their insurance and incentive effects. The only paper that examines this trade off in the context of labor market informality is Gerard and Gonzaga (2014). The main variation they exploit is on the intensive margin of UI comparing search intensities for formal employment for workers eligible for four and five

[19] We estimate almost identical effects for the tenure range of 12-17 months for the subset of workers for which eligibility criteria change from six to 18 months' tenure after the reform.

months of UI benefits. Based on low marginal effects of extending UI benefits eligibility by one month on search intensities, Gerard and Gonzaga (2014) argue that incentive effects of UI are lower in the context of informal labor markets. We complement their analysis by examining extensive margin variation in eligibility for UI insurance, which allows us to identify the effect of UI on unemployment inflow in the presence of informal labor markets. Our findings suggest that the interaction between UI and informal labor markets is more complex and subtle. Higher strategic unemployment inflow due to UI seems to be an important aspect of UI-induced incentives in the presence of informal labor markets, as the majority of strategic unemployment is related to workers transitioning to informal employment. This implies that a higher fraction of UI benefits is paid to strategically unemployed workers than the estimates based on intensive margin variation in Gerard and Gonzaga (2014).

Additionally, paying UI benefits to workers that are informally employed may introduce several inefficiencies and distortions. Ceteris paribus, the marginal value of insurance is lower for workers that receive income from informal employment than for unemployed workers, which implies that the insurance value of UI payments is lower. This is particularly true for workers that choose to be informally employed rather than being forced into informal unemploymetn due to a lack for formal employment opportunities, which as we document is true for a significant fraction of strategically unemployed workers. By paying the same amount of benefits to informally employed individuals, funds that could be used to increase insurance for unemployed workers are diverted leaving unemployed workers relatively underinsured. Moreover, providing UI benefits to informally employed workers subsidizes the labor costs of firms for whom it is easier to hire workers informally,[20] while taxing firms that employ workers formally. Additionally, subsidizing labor costs of some firms while taxing others leads to distortions in the allocation of resources in the economy that imply further efficiency costs (Restuccia and Rogerson 2013).

While we are cautious about making general welfare statements, our findings provide policy implications to reduce potential rents from timing unemployment spells according to UI benefits eligibility, which may reduce distortions of the UI system in the presence of large informal labor markets. Ideally, we would like to reduce adverse incentive effects of UI without reducing its insurance effects. First, as an alternative to changes to the UI system, better monitoring and higher penalties for informal employment may reduce firms' incentive to higher workers informally while they are on UI benefits. These policies could reduce adverse incentive effects of UI without affecting the insurance effect of UI. Insurance

[20]In unreported results, we find that this applies to smaller firms and to firms that hire workers that earn lower wages.

effects of UI may even be improved if reducing rent extraction from the UI system by strategically unemployed workers frees up funds for unemployed workers that benefit more from benefits. We show that strategic unemployment inflow and collusion are more prevalent when the rents that can be extracted from the UI system are larger. Rents can be reduced by lowering replacement rates, shortening benefits duration, increasing the threshold to qualify for benefits, or increasing layoff costs and experience rating. The relatively minor incentive effects of extended benefits duration documented by Gerard and Gonzaga (2014) suggest that higher experience rating may be the more effective policy implication to reduce rents in the context of informal labor markets. More nuanced policy implications may include tweaks to the UI system that prevent repeat temporary layoffs of the same worker by the same firm.

# 5 Conclusion

Exploiting a reform to UI benefits eligibility criteria in Brazil, we document that workers are more likely to exit formal employment when they qualify for UI benefits. We find that firms and workers time unemployment spells to coincide precisely with eligibility for UI benefits. Firms lay off workers just when they become eligible for UI benefits, and the same firm rehires a worker when her eligibility for benefits expires. Examining changes in wages around the reform indicates that firms benefit from colluding with workers through lower equilibrium wages, consistent with models of implicit contracting in the presence of UI (Feldstein 1976; Baily 1977; Christofides and McKenna 1996).

Survey evidence suggests that workers are more likely to transition to informal employment and to return to the same firm when they are laid off just after becoming eligible for UI benefits. In addition, when firms layoff workers who are eligible for benefits at the six-month threshold before the reform, they are less likely to hire a replacement worker, compared to when they lay off a worker with a tenure just below the six-month threshold, who is ineligible for UI benefits. This indirect evidence supports the interpretation of the results that firms and workers revert to informal employment relationships for the period that workers are eligible for UI benefits, to extract rents from the UI system. Moreover, all of the documented patterns are mostly concentrated in labor markets with a higher degree of informality, and disappear after the reform when workers with a tenure of six months are no longer eligible for UI benefits. This further suggests that informal labor markets play an important role in strategic unemployment inflow in response to UI eligibility.

Our findings have implications for UI design. The timing of unemployment spells to fit workers' UI eligibility suggests that some part of the UI system does not fulfil an insurance purpose, but transfers rents towards firms and workers who exploit the system. This effect is larger when the potential rents to be extracted from the UI system are larger. While we are cautious about general welfare statements, our findings suggest that reducing potential rents could reduce adverse incentive effects of UI in the presence of informal labor markets. Rents can be reduced by lowering replacement rates and the duration of benefits, or by increasing experience rating to increase the cost of layoffs. More nuanced policy implications may include tweaks to the UI system that prevent repeated temporary layoffs of the same worker by the same firm. The strong correlation with labor market informality suggests that these considerations are particularly important for mid-income and developing countries with large informal labor markets, and that reducing labor market informality, or better monitoring in combination with higher fines, may reduce adverse effects of the UI system.

# References


Amarante, V., R. Arim, and A. Dean. 2013. Unemployment insurance design and its effects: Evidence for Uruguay. Working paper, Universidad de los Andes-Cede.

Anderson, P. M., and B. D. Meyer. 1994. The effects of unemployment insurance taxes and benefits on layoffs using firm and individual data. Working paper, NBER.

———. 1997a. The effects of firm specific taxes and government mandates with an application to the U.S. unemployment insurance program. *Journal of Public Economics* 65:119–45.

———. 1997b. Unemployment insurance take-up rates and the after-tax value of benefits. *Quarterly Journal of Economics* 112:913–37.

Baily, M. N. 1977. On the theory of layoffs and unemployment. *Econometrica* 45:1043–63.

Baker, M., and S. A. Rea. 1998. Employment spells and unemployment insurance eligibility requirements. *Review of Economics and Statistics* 80:80–94.

Bergolo, M., and G. Cruces. 2016. The anatomy of behavioral responses to social assistance when informal employment is high. Working paper, IZA.

Card, D., R. Chetty, and A. Weber. 2007. Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market. *Quarterly Journal of Economics* 122:1511–60.

Card, D., A. Johnston, P. Leung, A. Mas, and Z. Pei. 2015a. The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression discontinuity design in Missouri, 2003-2013. *American Economic Review* 105:126–30.

Card, D., D. S. Lee, Z. Pei, and A. Weber. 2015b. Inference on causal effects in a generalized regression kink design. *Econometrica* 83:2453–83.

Card, D., and P. B. Levine. 1994. Unemployment insurance taxes and the cyclical and seasonal properties of unemployment. *Journal of Public Economics* 53:1–29.

———. 2000. Extended benefits and the duration of UI spells: Evidence from the New Jersey extended benefit program. *Journal of Public Economics* 78:107–38.

Carvalho, C. C., R. Corbi, and R. Narita. 2017. Unintended consequences of unemployment insurance: Evidence from stricter eligibility criteria in Brazil. *Economic Letters* forthcoming.

Christofides, L. N., and C. McKenna. 1995. Unemployment insurance and moral hazard in employment. *Economic Letters* 49:205–10.

———. 1996. Unemployment insurance and job duration in Canada. *Journal of Labor Economics* 14:286–312.

Doornik, B., D. Fazio, D. Schoenherr, and J. Skrastins. 2018. Unemployment insurance as a subsidy to risky firms. Working paper, Banco Central do Brasil.

Farber, H. S., J. Rothstein, and R. G. Valletta. 2015. The effects of extended unemployment insurance benefits: Evidence from the 2012-2013 phase-out. *American Economic Review* 105:171–6.

Farber, H. S., and R. G. Valletta. 2015. Do extended unemployment benefits lengthen unemployment spells? Evidence from recent cycles in the U.S. labor market. *Journal of Human Resources* 50:873–909.

Feldstein, M. 1976. Temporary layoffs in the theory of unemployment. *Journal of Political Economy* 84:937–58.

———. 1978. The effects of unemployment insurance on temporary layoff unemployment. *American Economic Review* 68:834–46.

Fujita, S., and G. Moscarini. 2017. Recall and unemployment. *American Economic Review* 107:3875–916.

Gasparini, L., F. Haimovich, and S. Olivieri. 2009. Labor informality bias of a poverty-alleviation program in Argentina. *Journal of Applied Economics* 12:181–205.

Gerard, F., and G. Gonzaga. 2014. Informal labor and the efficiency of social programs: Evidence from 15 years of unemployment insurance in Brazil. Working paper, Columbia University.

Gonzalez-Rozada, M., L. Ronconi, and H. Ruffo. 2011. Protecting workers against unemployment in Latin America and the Caribbean: Evidence from Argentina. Working paper, IDB.

Green, D. A., and C. W. Riddell. 1997. Qualifying for unemployment insurance: An empirical analysis. *Economic Journal* 107:67–84.

Green, D. A., and T. C. Sargent. 1998. Unemployment insurance and job durations: Seasonal and non-seasonal jobs. *The Canadian Journal of Economics* 31:247–78.

Hamermesh, D. S. 1979. Entitlement effects, unemployment insurance, and employment decisions. *Economic Enquiry* 17:317–32.

Hazans, M. 2011. Informal workers across Europe: Evidence from 30 countries. Working paper, IZA.

Holzmann, R., Y. Pouget, M. Vodopivec, and M. Weber. 2011. Severance pay programs around the world: History, rationale, status, and reforms. Working paper, IZA.

ILO. 2017. *World social protection report 2017-19*. ILO Publications.

Jaeger, S., B. Schoefer, and J. Zweimueller. 2018. Marginal jobs and job surplus: A test of the efficiency of separations. Working paper, MIT.

Johnston, A. C., and A. Mas. 2015. Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut. Working paper, NBER.

Jurajda, S. 2002. Estimating the effect of unemployment insurance compensation on the labor market histories of displaced workers. *Journal of Econometrics* 108:227–52.

Katz, L. F. 1986. Layoffs, recall and the duration of unemployment. Working paper, NBER.

Katz, L. F., and B. D. Meyer. 1990a. The impact of the potential duration of unemployment benefits on the duration of unemployment. *Journal of Public Economics* 41:45–72.

———. 1990b. Unemployment insurance, recall expectations, and unemployment outcomes. *Quarterly Journal of Economics* 105:973–1002.

Kroft, K., and M. J. Notowidigdo. 2016. Should unemployment insurance vary with the unemployment rate? Theory and evidence. *Review of Economic Studies* 83:1092–124.

Lalive, R. 2008. How do extended benefits affect unemployment duration? A regression discontinuity design. *Journal of Econometrics* 142:785–806.

Landais, C. 2015. Assessing the welfare effects of unemployment benefits using the regression kink design. *American Economic Journal: Economic Policy* 7:243–78.

Lipsey, R. G., and K. Lancaster. 1956. The general theory of second best. *Review of Economic Studies* 24:11–32.

Meyer, B. D. 1990. Unemployment insurance and unemployment spells. *Econometrica* 58:757–82.

———. 1995. Lessons from the US employment insurance experiments. *Journal of Economic Literature* 33:91–131.

Meyer, B. D., and W. K. C. Mok. 2007. Quasi-experimental evidence on the effects of unemployment insurance from New York State. Working paper, NBER.

Moffitt, R. 1985. Unemployment insurance and the distribution of unemployment spells. *Journal of Econometrics* 28:85–101.

Mortensen, D. T. 1977. Unemployment insurance and job search decisions. *Industrial and Labor Relations Review* 30:505–17.

Nekoei, A., and A. Weber. 2015. Recall expectations and duration dependence. *American Economic Review, Papers and Proceedings* 105:142–6.

Rebello-Sanz, Y. 2012. Unemployment insurance and job turnover in Spain. *Labour Economics* 19:403–26.

Restuccia, D., and R. Rogerson. 2013. Misallocation and productivity. *Review of Economic Dynamics* 16:1–10.

Rothstein, J. 2011. Unemployment insurance and job search in the great recession. *Brookings Papers on Economic Activity* 143–210.

Rothstein, J., and R. G. Valletta. 2017. Scraping by: Income and program participation after the loss of extended unemployment benefits. Working paper, NBER.

Schmieder, J. F., T. von Wachter, and S. Bender. 2012. The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years. *Quarterly Journal of Economics* 127:701–52.

———. 2016. The effect of unemployment benefits and nonemployment duration on wages. *American Economic Review* 106:739–77.

Solon, G. 1979. Labor supply effects of extended unemployment benefits. *Journal of Human Resources* 14:247–55.

Topel, R. H. 1983. On layoffs and unemployment insurance. *American Economic Review* 73:541–59.

———. 1985. Unemployment and unemployment insurance. *Research in Labor Economics* 7:91–135.

Valletta, R. G. 2014. Recent extensions of US unemployment benefits: Search responses in alternative labor market states. *IZA Journal of Labor Policy* 3:1–25.

Winter-Ebmer, R. 2003. Benefit duration and unemployment entry: A quasi-experiment in Austria. *European Economic Review* 47:259–73.

Table 1: **Summary Statistics for Workers Around the Threshold**

| | 4-5 Months Tenure | 6-7 Months Tenure | Difference |
|---|---|---|---|
| Age (Years) | 32 | 32 | 0 |
| Salary (Real) | 1,239 | 1,277 | 38 |
| Male | 0.611 | 0.606 | -0.005 |
| University Education | 0.064 | 0.069 | 0.005 |
| Firm Size (Employees) | 66 | 70 | 4 |
| Fraction in Construction | 0.122 | 0.121 | -0.001 |
| Fraction in Manufacturing | 0.156 | 0.151 | -0.005 |
| Fraction in Agriculture | 0.033 | 0.035 | 0.002 |

This table reports pre-reform descriptive statistics (age, salary, gender, education, firm size, industry distribution) for workers with tenure of four to five months in the first column, and workers with tenure of six to seven months in the second column, respectively. The third column depicts the difference between workers with six and seven months' tenure and workers and four and five months' tenure.

Table 2: **Informality by Industry**

| Industry | Informal Employment | Employment Share |
|---|---|---|
| Domestic Services | 0.6617 | 0.0002 |
| Agriculture, Livestock, Forestry, Fisheries, Aquaculture | 0.5693 | 0.0546 |
| Other Services | 0.4788 | 0.0350 |
| Arts, Culture, Sports, Recreation | 0.4315 | 0.0075 |
| Construction | 0.4074 | 0.0796 |
| Accommodation, Food | 0.3155 | 0.0405 |
| Real Estate | 0.2850 | 0.0099 |
| Trade, Repair of Motor Vehicles and Motorcycles | 0.2562 | 0.1893 |
| Water, Sewerage, Waste Management, Decontamination | 0.2211 | 0.0067 |
| Professional, Scientific, and Technical Activities | 0.2144 | 0.0459 |
| Transport, Storage, Postal Services | 0.2012 | 0.0393 |
| Education | 0.1828 | 0.0402 |
| Manufacturing | 0.1547 | 0.1417 |
| Human Health, Social Services | 0.1542 | 0.0365 |
| Information, Communication | 0.1441 | 0.0387 |
| Public Administration, Defense, Social Security | 0.1422 | 0.1311 |
| Extractive Industries | 0.1408 | 0.0045 |
| Administrative Activities and Complementary Services | 0.1389 | 0.0821 |
| Financial Activities and Related Insurance and Services | 0.0903 | 0.0145 |
| Electricity and Gas | 0.0556 | 0.0020 |

This table lists the share of informal employment for all industries in the sample and the share of workers employed in the respective industries from the Census Brazil.

Table 3: **Unemployment Inflow**

| Dep. Var.: $Layoff_{it}$ | I | II | III | IV | V |
|---|---|---|---|---|---|
| Sample period | 24 months | | 4 months | | |
| Tenure range | [4-11] | [4-7] | [4-11] | [4-7] | |
| $6Months_{it}$ | 0.0100*** | 0.0106*** | 0.0131*** | 0.0123*** | 0.0129*** |
| | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.006) |
| $6Months_{it} * Reform_t$ | -0.0039*** | -0.0057*** | -0.0040*** | -0.0049*** | -0.0055*** |
| | (0.0004) | (0.0004) | (0.0004) | (0.0006) | (0.0006) |
| Month*Municipality*Industry*Occupation FE | yes | yes | yes | yes | yes |
| Clustered SE | muni | muni | muni | muni | muni |
| Observations | 111,508,879 | 52,329,380 | 16,257,568 | 8,532,451 | 8,729,104 |
| $R^2$ | 0.032 | 0.040 | 0.095 | 0.127 | 0.143 |

This table reports changes in unemployment inflow around the enactment of the UI reform. The dependent variable is a dummy variable that takes the value of one if worker $i$ is laid off in month $t$ and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to eleven months in columns labeled [4-11], and for workers with tenure of six to seven months in columns labeled [4-7], and zero for workers with tenure of four to five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015 in columns labeled 24 months, and takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015 in columns labeled 4 months, except column V where the pre-reform period is from November 2014 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 4: **Same-Firm Rehiring**

| | I | II | III | IV | V | VI | VII | VIII | IX | X |
|---|---|---|---|---|---|---|---|---|---|---|
| Dep. Var.: | $Rehire[4-10]_{it}$ | | | | | | | | $Hire[9-10]_{it}$ | |
| Sample period | 24 months | | | | 4 months | | | | 20 months | |
| Tenure range | [4-11] | [4-7] | [4-11] | [4-7] | [4-11] | [4-7] | [4-11] | [4-7] | [4-11] | [4-7] |
| $6Months_{it}$ | 0.0124*** | 0.0088*** | 0.0208*** | 0.0141*** | 0.0212*** | 0.0168*** | 0.0198*** | 0.0148*** | -0.0088*** | -0.0038* |
| | (0.0005) | (0.0005) | (0.0010) | (0.0013) | (0.0015) | (0.0022) | (0.0025) | (0.0039) | (0.0018) | (0.0021) |
| $6Months_{it} * Reform_t$ | -0.0082*** | -0.0068*** | -0.0140*** | -0.0123*** | -0.0156*** | -0.0141*** | -0.0170*** | -0.0168*** | -0.0010 | 0.0016 |
| | (0.0005) | (0.0006) | (0.0015) | (0.0019) | (0.0022) | (0.0029) | (0.0045) | (0.0055) | (0.0028) | (0.0044) |
| $Same_{it}$ | | | | | | | | | -0.0617*** | -0.0839*** |
| | | | | | | | | | (0.0071) | (0.0087) |
| $6Months_{it} * Same_{it}$ | | | | | | | | | 0.0363*** | 0.0434*** |
| | | | | | | | | | (0.0071) | (0.0093) |
| $Same_{it} * Reform_t$ | | | | | | | | | 0.0231* | 0.0385*** |
| | | | | | | | | | (0.0120) | (0.0137) |
| $6Months * Same_{it} * Reform_t$ | | | | | | | | | -0.0364*** | -0.0510*** |
| | | | | | | | | | (0.0135) | (0.0182) |
| Month*Muni*Ind*Occupation FE | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Clustered SE | muni | muni | muni | muni | muni | muni | muni | muni | muni | muni |
| Observations | 4,632,338 | 2,367,552 | 1,464,028 | 726,233 | 422,848 | 227,298 | 210,244 | 108,405 | 1,002,005 | 505,983 |
| $R^2$ | 0.311 | 0.370 | 0.464 | 0.533 | 0.405 | 0.469 | 0.488 | 0.553 | 0.373 | 0.446 |

This table reports changes in the hiring of workers around the UI reform. The dependent variable in columns I to VIII is a dummy variable that takes the value of one if worker $i$ is reemployed by the same firm four to ten months after being laid off, and zero otherwise. In columns IX to X, the dependent variable is a dummy variable that takes the value of one if worker $i$ is hired nine to ten months after being laid off, and zero if worker $i$ is hired seven to eight months after layoff. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to eleven months in columns labeled [4-11], and for workers with tenure of six to seven months in columns labeled [4-7], and zero for workers with tenure of four to five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015 in columns labeled 24 months, and takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015 in columns labeled 4 months. In columns III, IV, VII, and VIII the sample is limited to workers rehired within four to ten months after layoff. 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: **Unemployment Spell Timing and UI System Parameters**

| | I | II |
|---|---|---|
| Dep. Var.: | $Layoff_{it}$ | $Rehire[4-10]_{it}$ |
| $6Months_{it}$ | -0.0008 | 0.0325*** |
| | (0.0014) | (0.0084) |
| $6Months_{it} * Reform_t$ | 0.0077*** | -0.0344*** |
| | (0.0019) | (0.0122) |
| $Surplus_{it}$ | -0.0280*** | 0.0122** |
| | (0.0016) | (0.0050) |
| $Surplus_{it} * 6Months_{it}$ | 0.0289*** | 0.0303*** |
| | (0.0040) | (0.0097) |
| $Surplus_{it} * Reform_t$ | -0.0187*** | -0.0058 |
| | (0.0021) | (0.0075) |
| $Surplus_{it} * 6Months_{it} * Reform_t$ | -0.0135*** | -0.0350** |
| | (0.0041) | (0.0140) |
| $RR_{it}$ | 0.0873*** | -0.0619*** |
| | (0.0041) | (0.0093) |
| $RR_{it} * 6Months_{it}$ | -0.0247*** | -0.0443*** |
| | (0.0051) | (0.0133) |
| $RR_{it} * Reform_t$ | -0.1114*** | -0.0085 |
| | (0.0134) | (0.0124) |
| $RR_{it} * 6Months_{it} * Reform_t$ | -0.0079 | 0.0558*** |
| | (0.0053) | (0.0188) |
| Month*Municipality*Industry*Occupation FE | yes | yes |
| Clustered SE | muni | muni |
| Observations | 8,512,307 | 227,298 |
| $R^2$ | 0.131 | 0.468 |

The results in this table document how layoff and rehiring patterns vary with parameters with the UI system. In column I, the dependent variable $Layoff_{it}$ is a dummy variable that is one if a worker $i$ is laid off in month $t$, and zero otherwise. In column II, the dependent variable $Rehire[4-10]_{it}$ is a dummy variable that takes the value of one if worker $i$ is reemployed by the same firm four to ten months after being laid off, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to seven months, and zero for workers with tenure of four to five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period in March and April 2015 and zero for the pre-reform period from January to February 2015. The variable $Surplus_{it}$ is the ratio of UI benefits that worker $i$ is entitled to relative to the sum of the costs of formally employing the worker and layoff costs. The variable $RR_{it}$ is the ratio of monthly UI benefits accruing to worker $i$ relative to the worker's current wage. 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: **Informal Employment**

| Dep. Var.: $Informal_{it}$ | I | II | III | IV |
|---|---|---|---|---|
| Sample period | 12 months | | 4 months | |
| Tenure range | [4-11] | [4-7] | [4-11] | [4-7] |
| $6Months_{it}$ | 0.0177** | 0.0259*** | 0.0277 | 0.0445** |
| | (0.0077) | (0.0093) | (0.0175) | (0.0214) |
| $6Months_{it} * Reform_t$ | -0.0369*** | -0.0380*** | -0.0562*** | -0.0688*** |
| | (0.0111) | (0.0134) | (0.0206) | (0.0253) |
| Month*Municipality FE | yes | yes | yes | yes |
| Observations | 10,756 | 6,282 | 3,836 | 2,257 |
| $R^2$ | 0.004 | 0.016 | 0.009 | 0.014 |

This table reports rates of informal employment upon layoff using PME survey data. The dependent variable is a dummy variable that takes the value of one if worker $i$ transitions for formal to informal employment upon layoff and zero if worker $i$ transitions to unemployment upon layoff. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to eleven months in columns labeled [4-11], and for workers with tenure of six to seven months in columns labeled [4-7], and zero for workers with tenure of four to five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period from September 2014 to February 2015 and zero for the pre-reform period from March 2015 to August 2015 in columns labeled 12 months, and takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015 in columns labeled 4 months. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

Table 7: **Replacement Hiring**

| Dep. Var.: $New\ hire_{it}$ | I | II | III | IV |
|---|---|---|---|---|
| Sample period | 24 months | | 4 months | |
| Tenure range | [4-11] | [4-7] | [4-11] | [4-7] |
| $6Months_{it}$ | -0.0179*** | -0.0080*** | -0.0146*** | -0.0146*** |
| | (0.0019) | (0.0016) | (0.0022) | (0.0035) |
| $6Months_{it} * Reform_t$ | 0.0115*** | 0.0104*** | 0.0150*** | 0.0119** |
| | (0.0016) | (0.0021) | (0.0032) | (0.0048) |
| Month*Municipality*Industry*Occupation FE | yes | yes | yes | yes |
| Clustered SE | muni | muni | muni | muni |
| Observations | 2,662,475 | 1,309,983 | 550,533 | 255,283 |
| $R^2$ | 0.372 | 0.439 | 0.383 | 0.423 |

This table reports changes in the hiring of a replacement worker within one month of the layoff of another worker by the same firm around the enactment of the UI reform. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker $i$, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or eleven months in columns labeled [4-11] for workers with tenure of six to seven months in columns labeled [4-7] and zero for workers with tenure of four or five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March 2015 to February 2016 and zero for the pre-reform period from March 2014 to February 2015 in columns labeled 24 months, and takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015 in columns labeled 4 months. 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 the 5% levels, respectively.

Table 8: **Variation by Labor-Market Informality**

| | I | II | III | IV | V | VI |
|---|---|---|---|---|---|---|
| | Industry-Level | | | Municipality-Level | | |
| Dep. Var.: | $Layoff_{it}$ | $Rehire[4-10]_{it}$ | $New\ hire_{it}$ | $Layoff_{it}$ | $Rehire[4-10]_{it}$ | $New\ hire_{it}$ |
| $6Months_{it} * Informal$ | 0.0420*** | 0.0736*** | -0.1364*** | 0.0222*** | 0.0403 | -0.1175** |
| | (0.0037) | (0.0194) | (0.0435) | (0.0031) | (0.0274) | (0.0528) |
| $6Months_{it} * Reform_t * Informal$ | -0.0174*** | -0.0587** | 0.1385** | -0.0241*** | -0.0672* | 0.1454* |
| | (0.0046) | (0.0245) | (0.0678) | (0.0041) | (0.0347) | (0.0839) |
| Month*Muni*Eligibility FE | yes | yes | yes | no | no | no |
| Month*Ind*Eligibility FE | no | no | no | yes | yes | yes |
| Month*Muni*Ind*Occupation FE | yes | yes | yes | yes | yes | yes |
| Clustered SE | muni | muni | muni | muni | muni | muni |
| Observations | 8,532,451 | 227,298 | 255,283 | 8,532,451 | 227,283 | 255,283 |
| $R^2$ | 0.130 | 0.486 | 0.438 | 0.127 | 0.468 | 0.423 |

This table reports the results exploiting changes in labor market informality at the industry and municipality levels. The dependent variable is a dummy variable that takes the value of one if worker $i$ is laid off in month $t$ and zero otherwise in columns I, and IV, a dummy variable that takes the value of one if worker $i$ is reemployed by the same firm four to ten months after being laid off, and zero otherwise in columns II and V, and a dummy variable that takes the value of one if the same firm hires a new worker in the same industry within a month of laying off worker $i$ and zero otherwise in columns III and VI. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six to seven months (at layoff in columns II, III, V, and VI) and zero for workers with tenure of four to five months. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015. The variable $Informal$ is the share of informal employment in a given industry in the top panel and municipality in the bottom panel. 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: **Robustness Tests - Seasonality and Time-Series Shocks**

| | I | II | III | IV | V | VI | VII |
|---|---|---|---|---|---|---|---|
| | Other Separations | Seasonality | | Unaffected Workers | | | Excl. 5M Tenure |
| Dep. Var.: | $Quit_{it}$ | $Layoff_{it}$ | $Rehire[4-10]_{it}$ | $Layoff_{it}$ | $Rehire[4-10]_{it}$ | $New\ hire_{it}$ | $Layoff_{it}$ |
| $6Months_{it}$ | -0.0067*** | 0.0126*** | 0.0149*** | | | | 0.0155*** |
| | (0.0003) | (0.0005) | (0.0021) | | | | (0.0007) |
| $6Months_{it} * Reform_t$ | -0.0006 | 0.0007** | -0.0015 | | | | -0.0041*** |
| | (0.0005) | (0.0003) | (0.0023) | | | | (0.0006) |
| $6Months_{it} * 2015_t$ | | -0.0003 | 0.0023 | | | | |
| | | (0.0003) | (0.0021) | | | | |
| $6Months_{it} * 2015_t * Reform_t$ | | -0.0056*** | -0.0130*** | | | | |
| | | (0.0006) | (0.0032) | | | | |
| $Affected_{it}$ | | | | 0.0039*** | 0.0035*** | 0.0111*** | |
| | | | | (0.0005) | (0.0012) | (0.0023) | |
| $6Months_{it} * Affected_{it}$ | | | | 0.0022*** | -0.0003 | 0.0012 | |
| | | | | (0.0005) | (0.0032) | (0.0038) | |
| $6Months_{it} * Affected_{it} * Reform_t$ | | | | -0.0041*** | -0.0060** | 0.0126*** | |
| | | | | (0.0006) | (0.0030) | (0.0042) | |
| Month*Muni*Ind*Occupation FE | yes | yes | yes | yes | yes | yes | yes |
| Month*6Months FE | no | no | no | yes | yes | yes | no |
| Clustered SE | muni | muni | muni | muni | muni | muni | muni |
| Observations | 8,532,451 | 17,580,342 | 514,204 | 10,743,987 | 326,440 | 431,520 | 6,292,433 |
| $R^2$ | 0.110 | 0.125 | 0.458 | 0.045 | 0.456 | 0.373 | 0.145 |

This table presents results from different robustness tests. The dependent variable is a dummy variable that takes the value of one if worker $i$ quits in month $t$ and zero otherwise in column I, a dummy variable that takes the value of one if worker $i$ is laid off in month $t$ and zero otherwise in columns II, IV, and VII, a dummy variable that takes the value of one if worker $i$ is reemployed by the same firm four to ten months after being laid off, and zero otherwise in columns III and V, and a dummy variable that takes the value of one if the same firm hires a new worker in the same industry within a month of laying off worker $i$ and zero otherwise in column VI. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months (at layoff in columns III, V, and VI) and zero for workers with tenure of four or five months. The dummy variable $Reform_t$ takes the value of one for March and April, and zero for January and February. The dummy variable $2015_t$ takes the value of one for the year 2015, and zero for the year 2014. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits, and zero for workers with two or more successful past applications. 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 10: **Formal Hiring, Employment, and Wages**

| | I | II | III | IV | V | VI | VII | VIII |
|---|---|---|---|---|---|---|---|---|
| Dep. Var.: | $Hired/Employed\ Workers$ | | $log(Workers)$ | | $log(Wage)$ | | $log(Wage) - log(Wage_{old})$ | |
| $Affected_{it}$ | 0.0012*** | 0.0009*** | 0.6957*** | 0.7906*** | -0.0979*** | -0.1078*** | 0.0362*** | 0.0346*** |
| | (0.0001) | (0.0001) | (0.0053) | (0.0070) | (0.0007) | (0.0008) | (0.0007) | (0.0008) |
| $Affected_{it} * Reform_t$ | -0.0037*** | -0.0043*** | -0.0612*** | -0.0631*** | 0.0052*** | 0.0082*** | 0.0049*** | 0.0050*** |
| | (0.0002) | (0.0002) | (0.0034) | (0.0028) | (0.0007) | (0.0008) | (0.0010) | (0.0012) |
| Month FE | yes | - | yes | - | yes | - | yes | - |
| Industry FE | yes | - | yes | - | yes | - | yes | - |
| Municipality FE | yes | - | yes | - | yes | - | yes | - |
| Industry-Municipality-Month FE | no | yes | no | yes | no | yes | no | yes |
| Observations | 4,030,596 | 4,030,596 | 3,929,941 | 3,929,941 | 1,816,004 | 1,816,004 | 1,482,341 | 1,482,341 |
| $R^2$ | 0.054 | 0.346 | 0.720 | 0.956 | 0.414 | 0.526 | 0.029 | 0.355 |

This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired relative to the total number of workers, in columns III and IV, the dependent variable is the log of total employment, in columns V and VI, the dependent variable is the log of the average wage of hired workers, in columns VII and VIII, the dependent variable is the log difference between newly hired workers' wage and their wage twelve months ago. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits, and zero for workers with two or more successful past applications. The dummy variable $Reform_t$ takes the value of one for the post-announcement period from January to December 2015, and zero for the pre-announcement period from January 2013 to December 2014. Standard errors are clustered at the municipality level. *** denotes statistical significance at the 1% level.

Table 11: **Formal Hiring, Employment, Wages, and Strategic Unemployment**

| | I | II | III | IV | V | VI |
|---|---|---|---|---|---|---|
| Dep. Var.: | $Hired/Employed\ Workers$ | | $log(Workers)$ | | $log(Wage)$ | |
| $Affected_{it}$ | 0.0020*** | 0.0025*** | 0.8319*** | 0.8041*** | -0.0905*** | -0.0911*** |
| | (0.0001) | (0.0001) | (0.0060) | (0.0056) | (0.0008) | (0.0007) |
| $Affected_{it} * Reform_t$ | -0.0033*** | -0.0047*** | -0.0556*** | -0.0619*** | 0.0083*** | 0.0095*** |
| | (0.0002) | (0.0002) | (0.0040) | (0.0029) | (0.0011) | (0.0007) |
| $Affected_{it} * Strategic$ | 0.0011*** | | -0.1214*** | | -0.0079*** | |
| | (0.0002) | | (0.0080) | | (0.0013) | |
| $Affected_{it} * Strategic * Reform_t$ | -0.0047*** | | -0.0267*** | | 0.0043** | |
| | (0.0004) | | (0.0070) | | (0.0019) | |
| $Affected_{it} * Collusive$ | | -0.0054** | | 1.3515*** | | -0.7207*** |
| | | (0.0021) | | (0.0551) | | (0.0405) |
| $Affected_{it} * Collusive * Reform_t$ | | -0.0242*** | | -0.1748*** | | 0.1893*** |
| | | (0.0029) | | (0.0407) | | (0.0501) |
| Industry-Municipality-Month FE | yes | yes | yes | yes | yes | yes |
| Observations | 4,030,596 | 4,030,596 | 3,929,941 | 3,929,941 | 1,816,004 | 1,816,004 |
| $R^2$ | 0.754 | 0.754 | 0.963 | 0.963 | 0.867 | 0.867 |

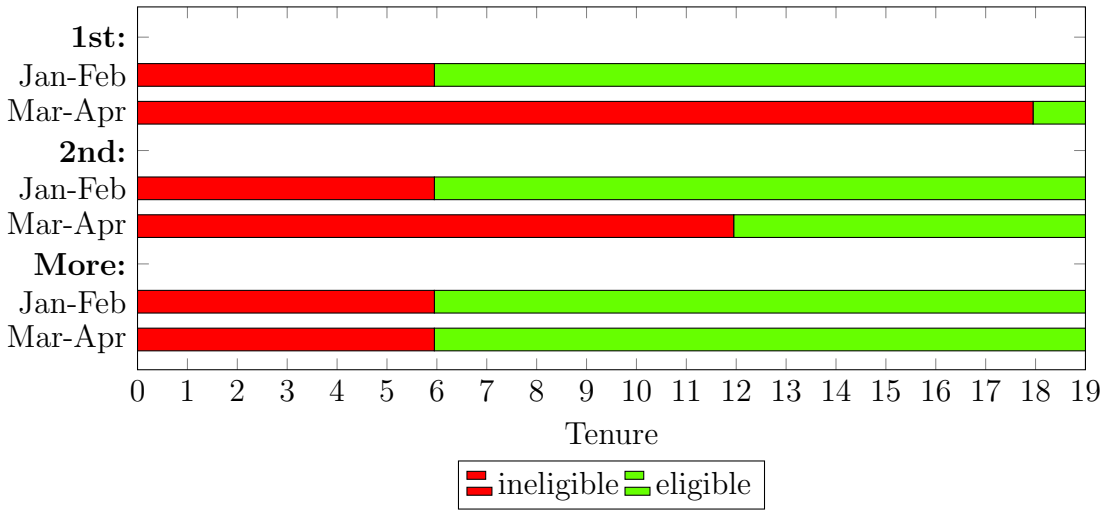
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation are the groups of affected and non-affected workers at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired relative to the total number of workers, in columns III and IV, the dependent variable is the log of total employment, in columns V and VI, the dependent variable is the log of the average wage of hired workers. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits, and zero for workers with two or more successful past applications. The dummy variable $Reform_t$ takes the value of one for the post-announcement period from January to December 2015, and zero for the pre-announcement period from January 2013 to December 2014. The dummy variable *Strategic* is the pre-reform share of workers in a given municipality-industry cell that is laid off with a tenure of six or seven months in all workers laid off with a tenure of four to seven months. The dummy variable *Collusive* is the pre-reform share in workers rehired by the same firm after four to ten months among workers laid off with a tenure of six or seven months. Standard errors are clustered at the municipality level. *** and ** denote statistical significance at the 1% and the 5% levels, respectively.

Table 12: **Formal Hiring, Employment, and Wages by Informality**

| | I | II | III | IV | V | VI |
|---|---|---|---|---|---|---|
| Dep. Var.: | $Hired/Employed\ Workers$ | | $log(Workers)$ | | $log(Wage)$ | |
| | Ind | Muni | Ind | Muni | Ind | Muni |
| $Affected_{it}$ | -0.0017*** | -0.0022*** | 1.0045*** | 1.6950*** | -0.0986*** | -0.0680*** |
| | (0.0002) | (0.0004) | (0.0080) | (0.0169) | (0.0014) | (0.0035) |
| $Affected_{it} * Reform_t$ | -0.0014*** | -0.0008 | -0.0469*** | -0.0442*** | 0.0025 | 0.0216*** |
| | (0.0004) | (0.0007) | (0.0059) | (0.0123) | (0.0015) | (0.0036) |
| $Affected_{it} * Informal$ | 0.0102*** | -0.0014** | -0.8411*** | -1.4489*** | -0.0547*** | -0.0474*** |
| | (0.0008) | (0.0006) | (0.0211) | (0.0256) | (0.0042) | (0.0053) |
| $Affected_{it} * Informal * Reform_t$ | -0.0115*** | -0.0084*** | -0.0694*** | -0.0370** | 0.0129** | 0.0441*** |
| | (0.0013) | (0.0010) | (0.0213) | (0.0180) | (0.0051) | (0.0051) |
| Industry-Municipality-Month FE | yes | yes | yes | yes | yes | yes |
| Observations | 4,030,596 | 4,030,596 | 3,929,941 | 3,929,941 | 1,816,004 | 1,816,004 |
| $R^2$ | 0.346 | 0.349 | 0.956 | 0.959 | 0.526 | 0.566 |

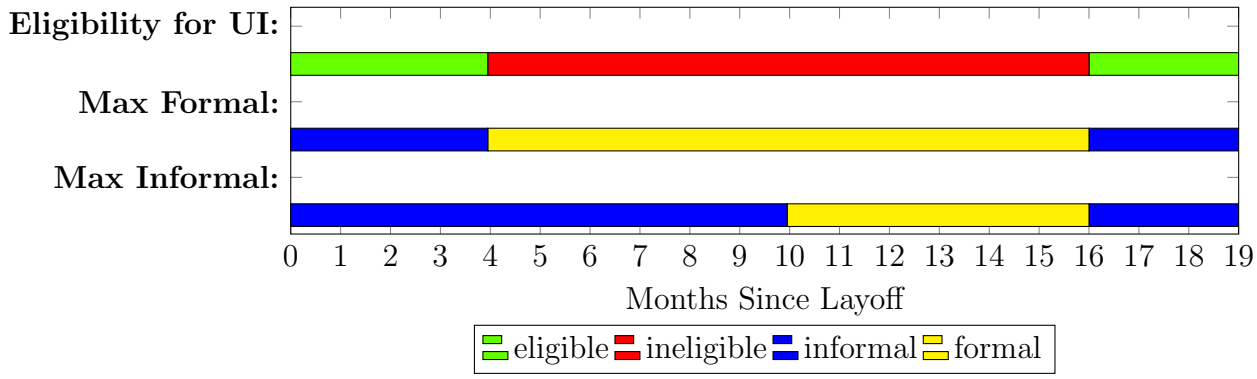
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation are the groups of affected and non-affected workers at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired relative to the total number of workers, in columns III and IV, the dependent variable is the log of total employment, in columns V and VI, the dependent variable is the log of the average wage of hired workers. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits, and zero for workers with two or more successful past applications. The dummy variable $Reform_t$ takes the value of one for the post-announcement period from January to December 2015, and zero for the pre-announcement period from January 2013 to December 2014. The variable $Informal$ is the share of informal employment in a given industry in odd columns and a given municipality in even columns. Standard errors are clustered at the municipality level. *** and ** denote statistical significance at the 1% and 5% level, respectively.

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



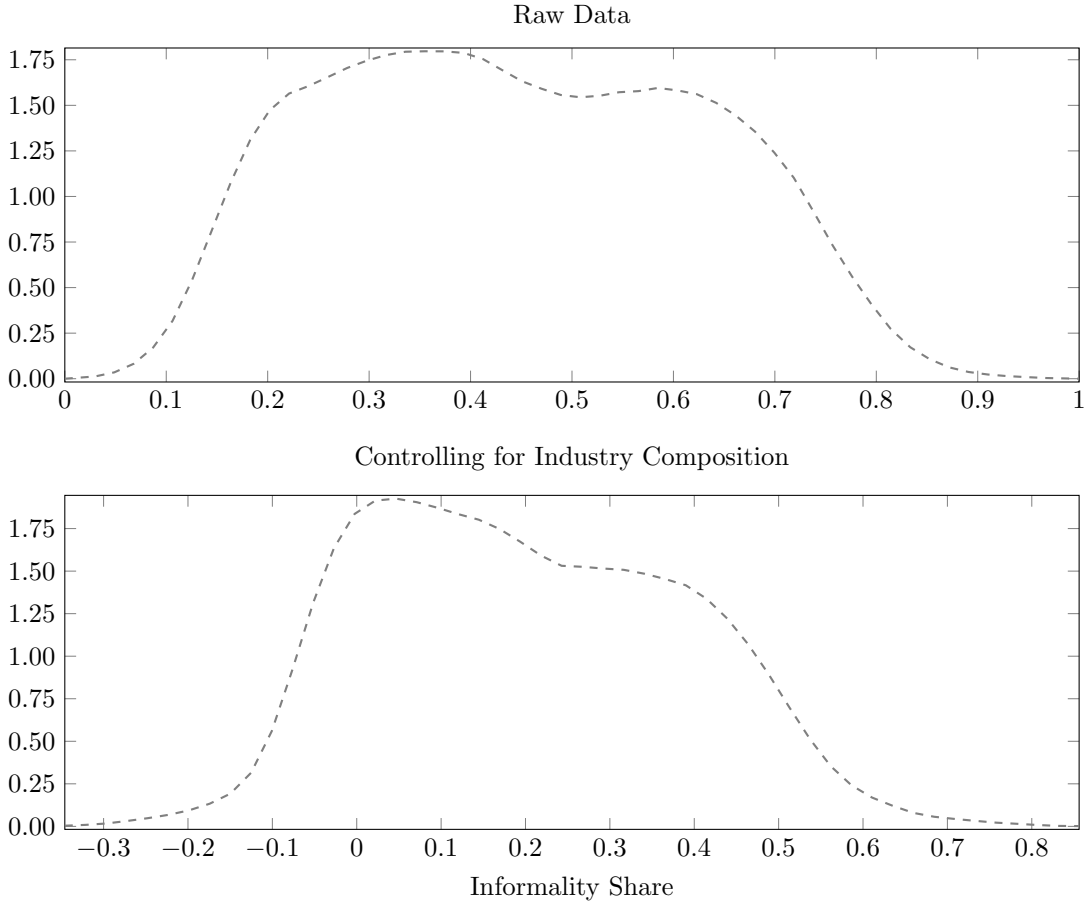

This figure illustrates eligibility criteria for UI benefits before and after the reform, for workers with different tenures who apply for UI benefits for the first time, the second time, and the third time or more. Red areas indicate tenure not satisfying eligibility criteria, green areas indicate tenure satisfying eligibility criteria.

Figure 2: **Repeated UI Eligibility**



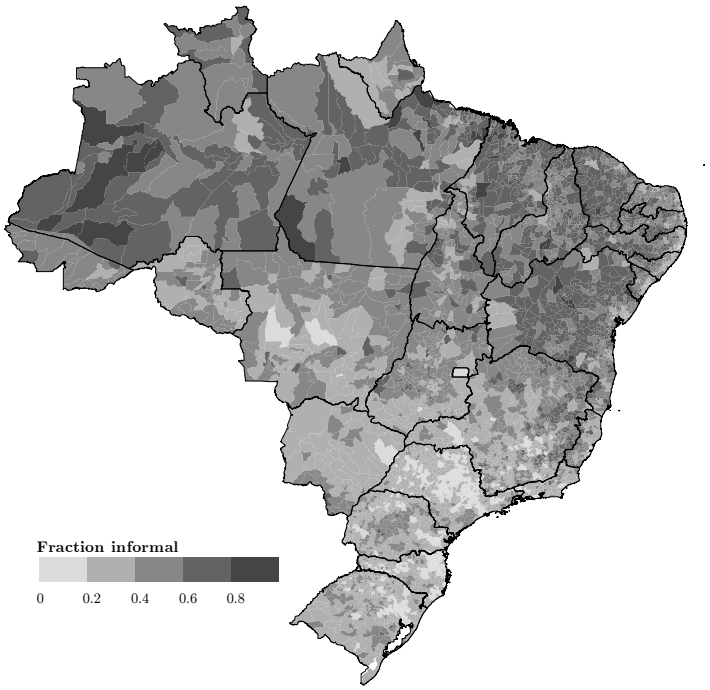

This figure plots examples of formal and informal employment spells under the pre-reform regime for firm-worker pairs engaging in repeated collusion. The top bar depicts workers' eligibility for UI benefits conditional on successful application for four months of UI benefits at time zero, where green bars indicate eligibility for UI benefits, and red lines indicate ineligibility for UI benefits. The second bar depicts informal (blue) and formal (orange) employment spells for a worker-pair maximizing formal employment spells while extracting rents from the UI system by employing the worker informally while she is eligible for benefits, the third bar depicts informal and formal employment spells for a firm-worker pair minimizing formal employment spells while extracting rents from the UI system by employing the worker informally while she is eligible for benefits.

Figure 3: **Distribution of Labor Market Informality across Municipalities**



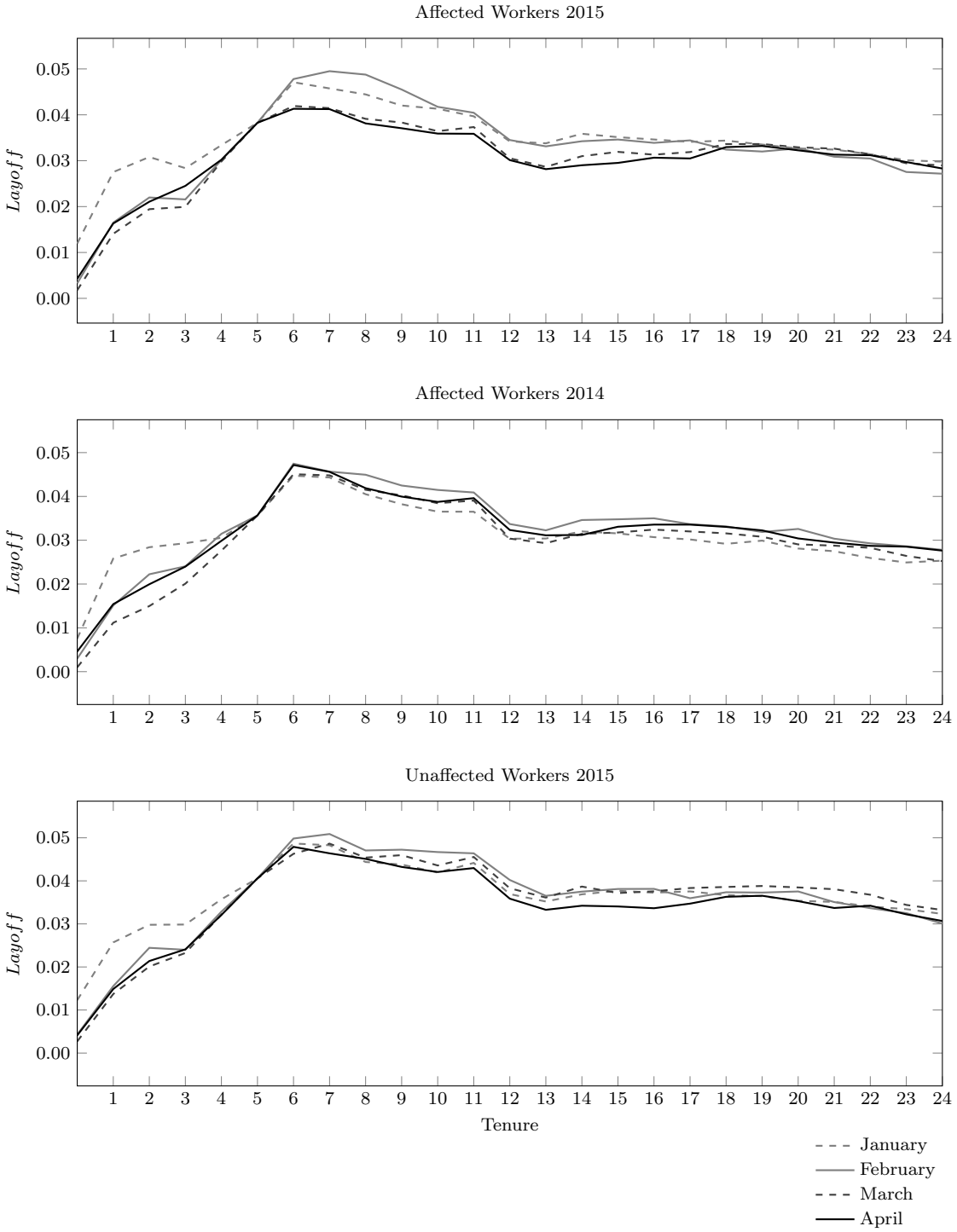

This top panel of this figure depicts the distribution of labor market informality across all municipalities in Brazil from the Census Brazil. The bottom panel depicts the distribution of the difference labor market informality and labor market informality as predicted from the industry composition of the respective municipality.

Figure 4: **Labor Market Informality by Municipality**



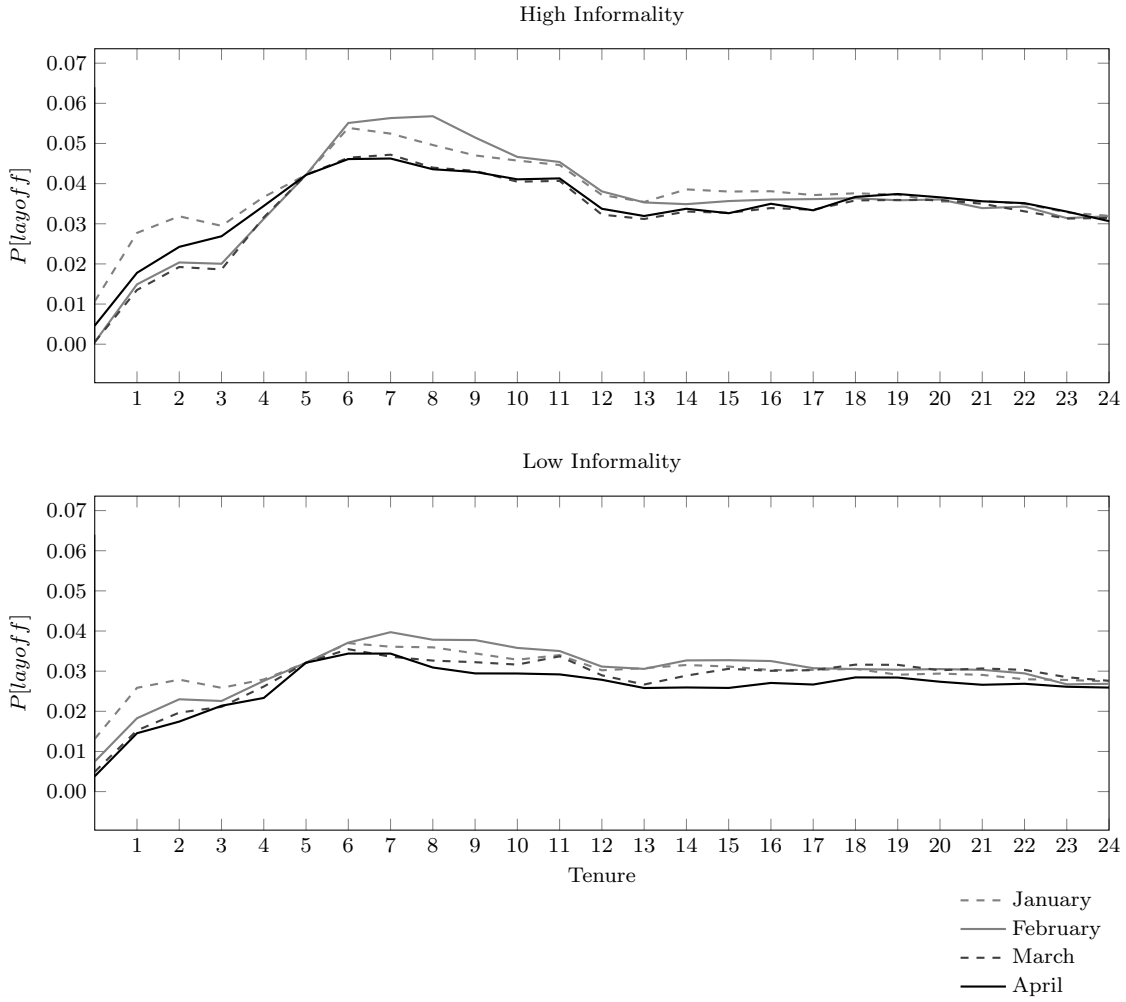

This figure depicts the share of informal in total workers for all municipalities in Brazil from the Census Brazil.

Figure 5: **Unemployment Inflow by Tenure**



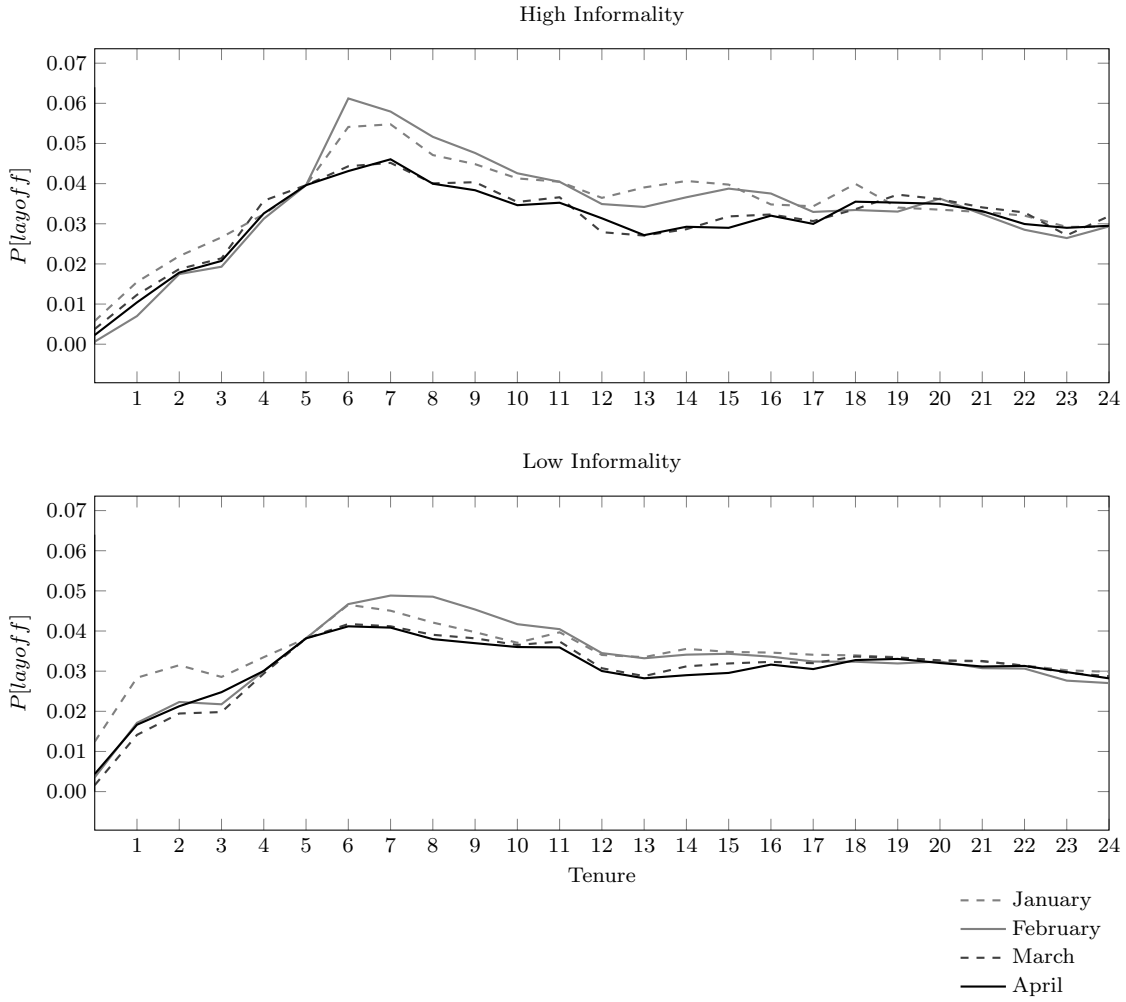

This figure depicts hazard rates of layoffs for workers with different tenure for the months from January to April. To facilitate comparison, the plots are vertically aligned at the April probability of layoff for workers with a tenure of five months. The top (2015 rates) and middle (2014 rates) panel depicts rates for workers affected by the reform. The bottom panel depicts the 2015 rates for workers unaffected by the reform.

Figure 6: **Unemployment Inflow by Informality - Industry Level**



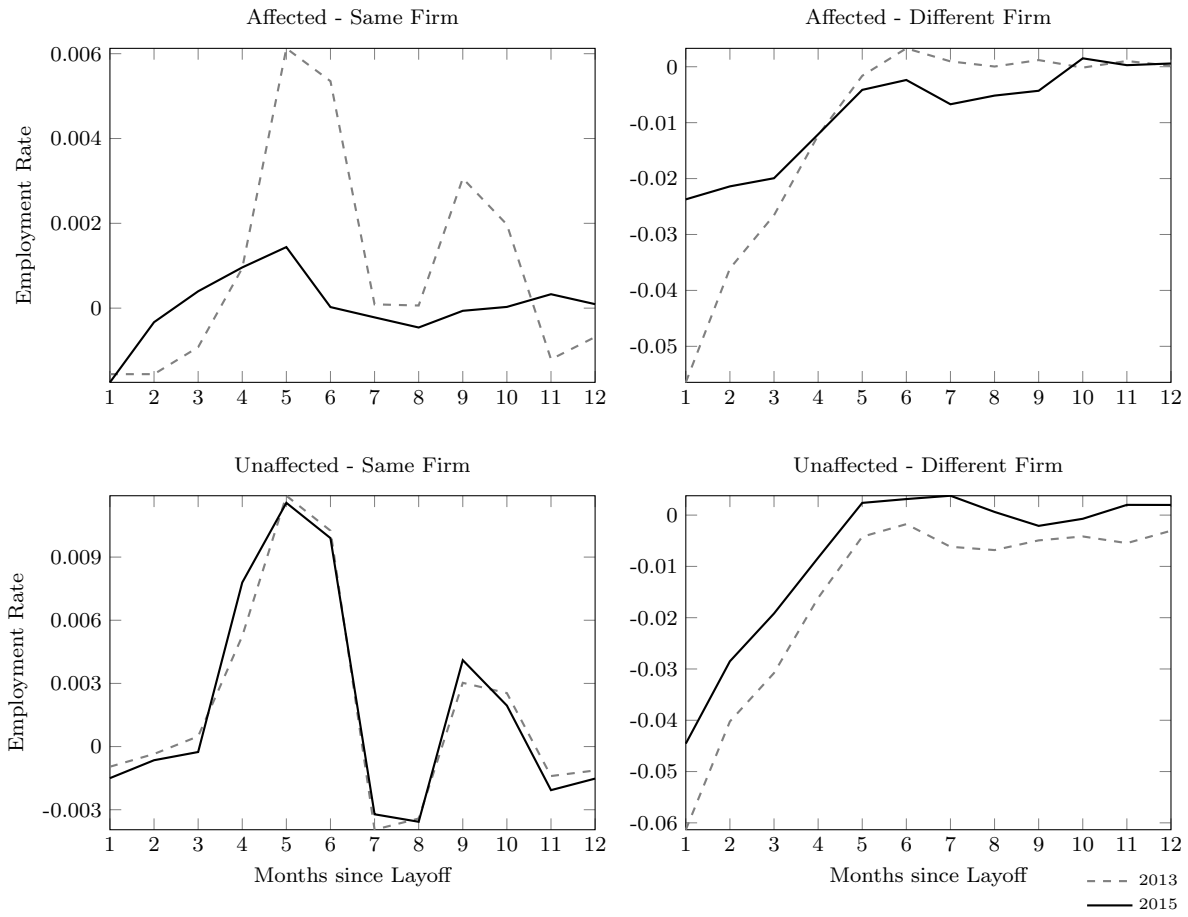

This figure depicts the probability of workers with different tenure being laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in industries with above median levels of labor market informality in the top panel, and workers in industries with below median levels of labor market informality in the bottom panel.

Figure 7: **Unemployment Inflow by Informality - Municipality Level**



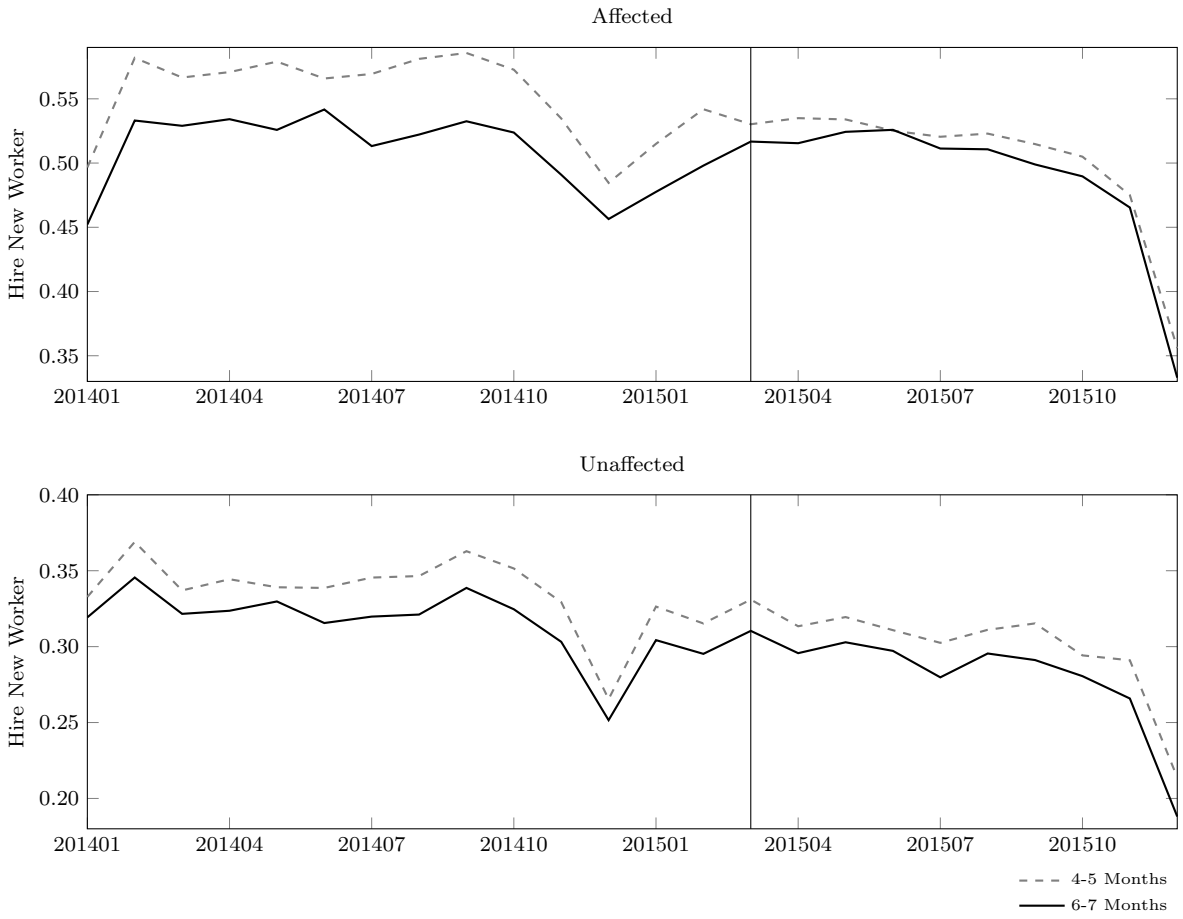

This figure depicts the probability of workers with different tenures being laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in municipalities with above median levels of labor market informality in the top panel, and workers in municipalities with below median levels of labor market informality in the bottom panel.

Figure 8: **Unemployment Outflow by Tenure - Same vs. Different Firms**



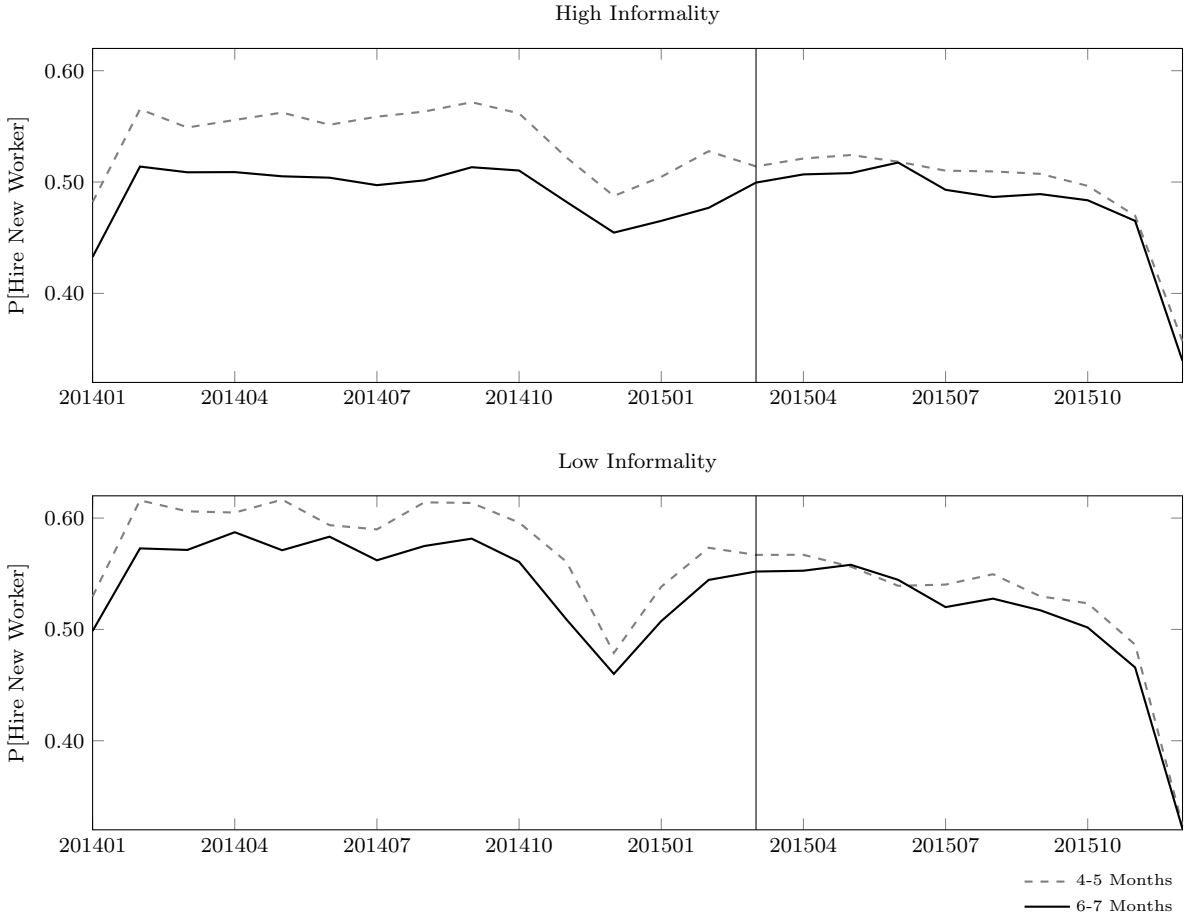

This figure depicts the difference in employment rates for worker that were laid off with a tenure of six to seven months and workers laid off with a tenure of four to five months for the years of 2013 and 2015. The x-axis lists the number of months since layoff. The left plots depict the difference in hazard rates of unemployment outflow to the same firm that laid off the worker, the right plots depict the difference in hazard rates of unemployment outflow to a different firm than the one that laid off the worker. To top plots show the results for workers with no more than one previous UI benefits spells, who are affected by the reform, the bottom plots show the results for workers with two or more previous UI spells, who are unaffected by the reform.

Figure 9: **Layoffs and Replacement Hires**



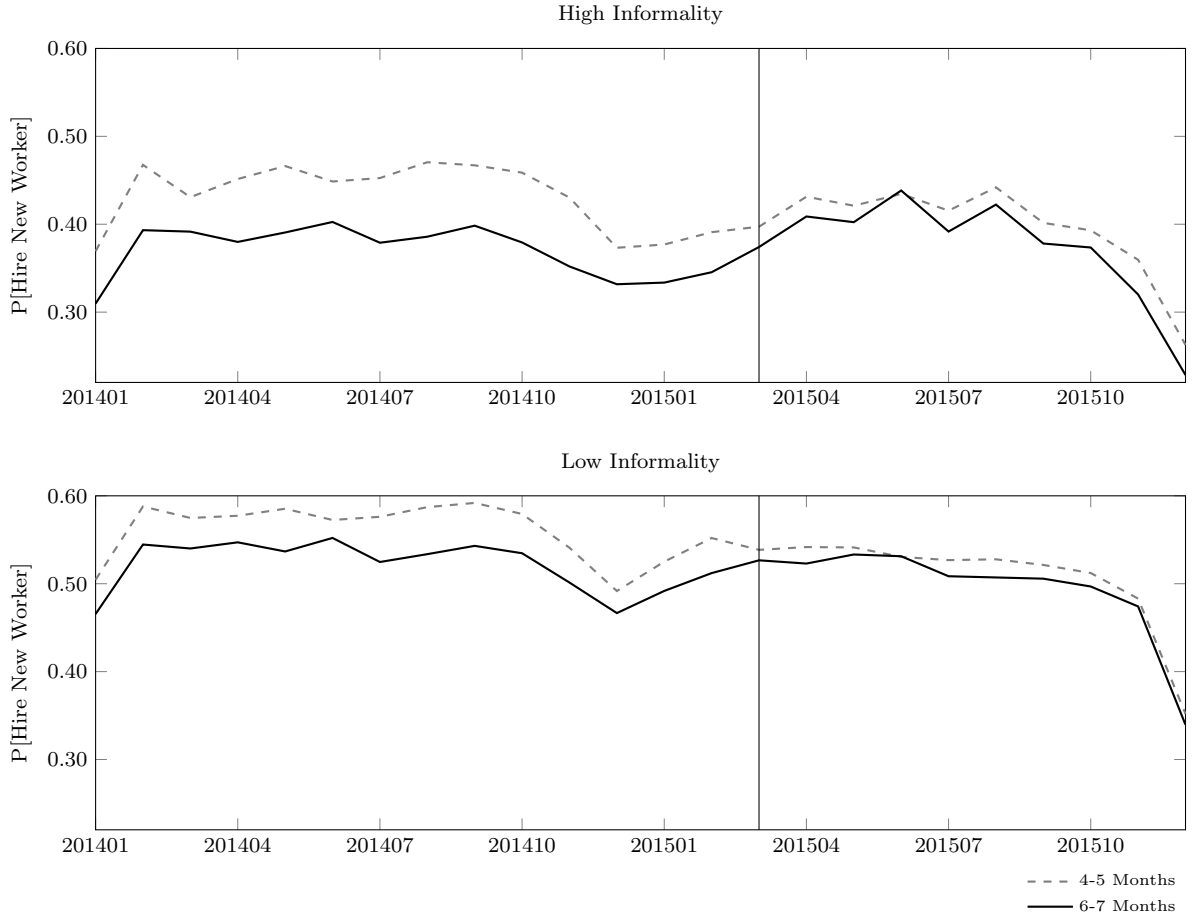

This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015. The vertical line indicates the implementation of the UI reform in March 2015. The top panel comprises all workers with less than two previous unemployment spells, who are affected by the reform, the bottom panel comprises all workers with two or more previous unemployment spells, who are unaffected by the reform.

Figure 10: **Layoffs and Replacement Hires by Informality - Industry Level**



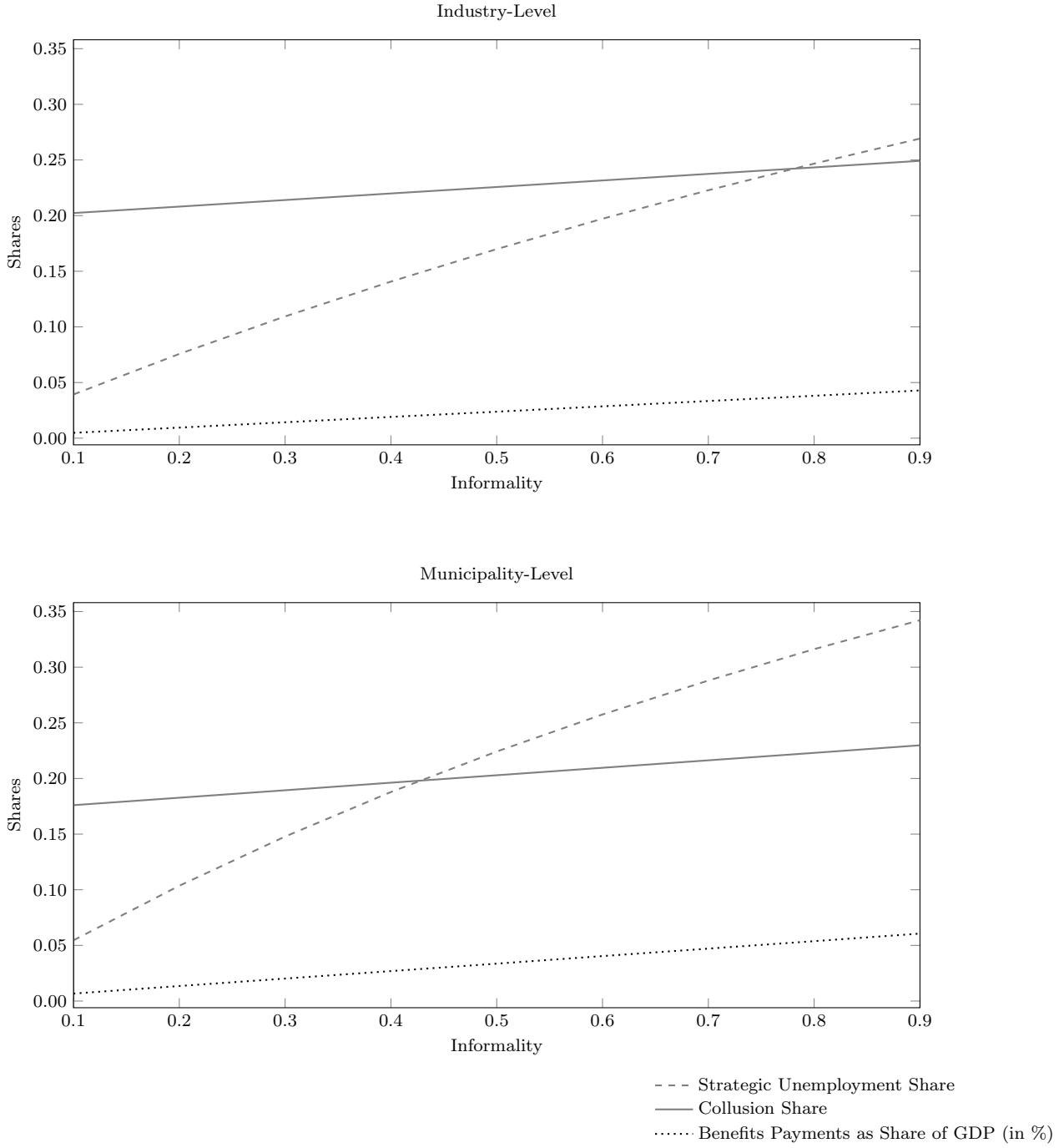

This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015 for industries with above median levels of labor market informality (top panel) and industries with below median levels of labor market informality (bottom panel). The vertical line indicates the implementation of the UI reform in March 2015.

Figure 11: **Layoffs and Replacement Hires by Informality - Municipality Level**



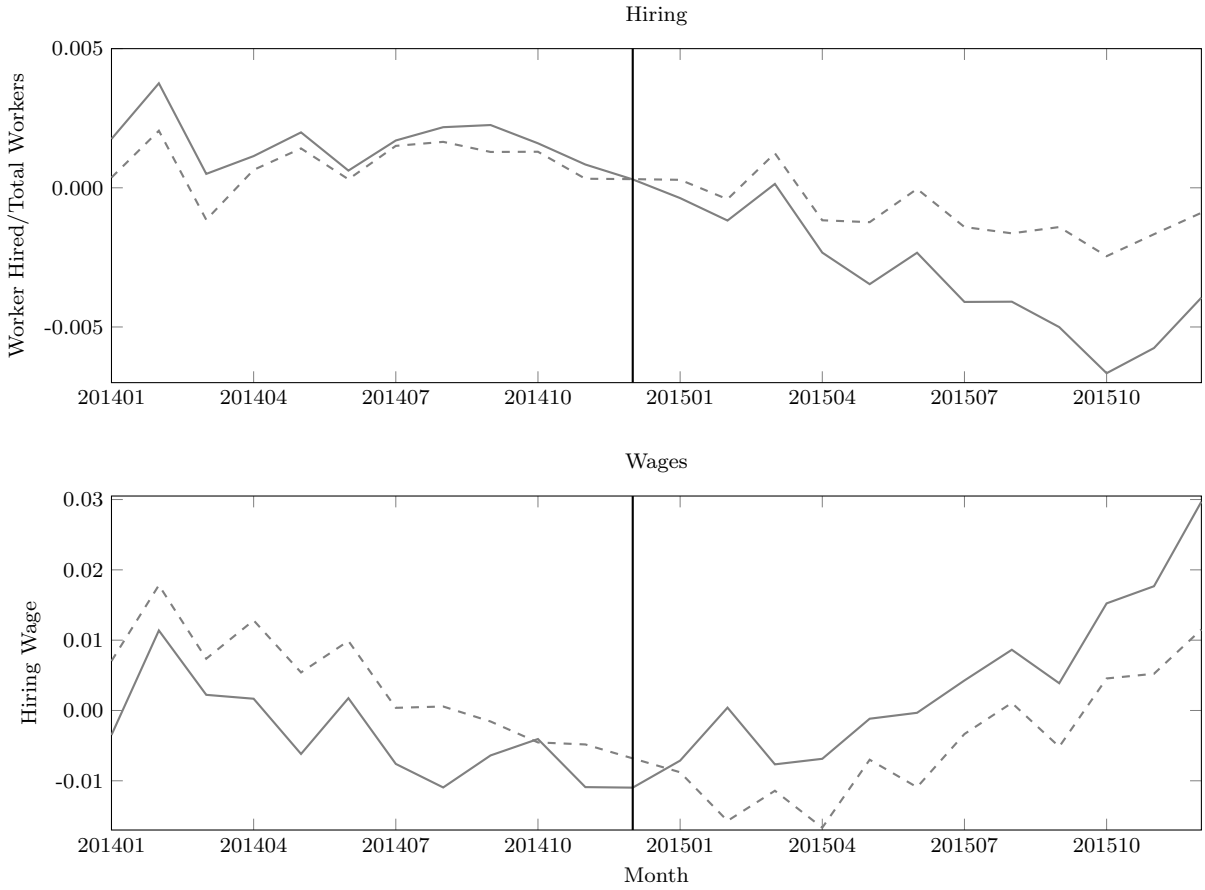

This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015 for municipalities with above median levels of labor market informality (top panel) and municipalities with below median levels of labor market informality (bottom panel). The vertical line indicates the implementation of the UI reform in March 2015.

Figure 12: **Strategic Unemployment and Firm-Worker Collusion Shares by Informality**



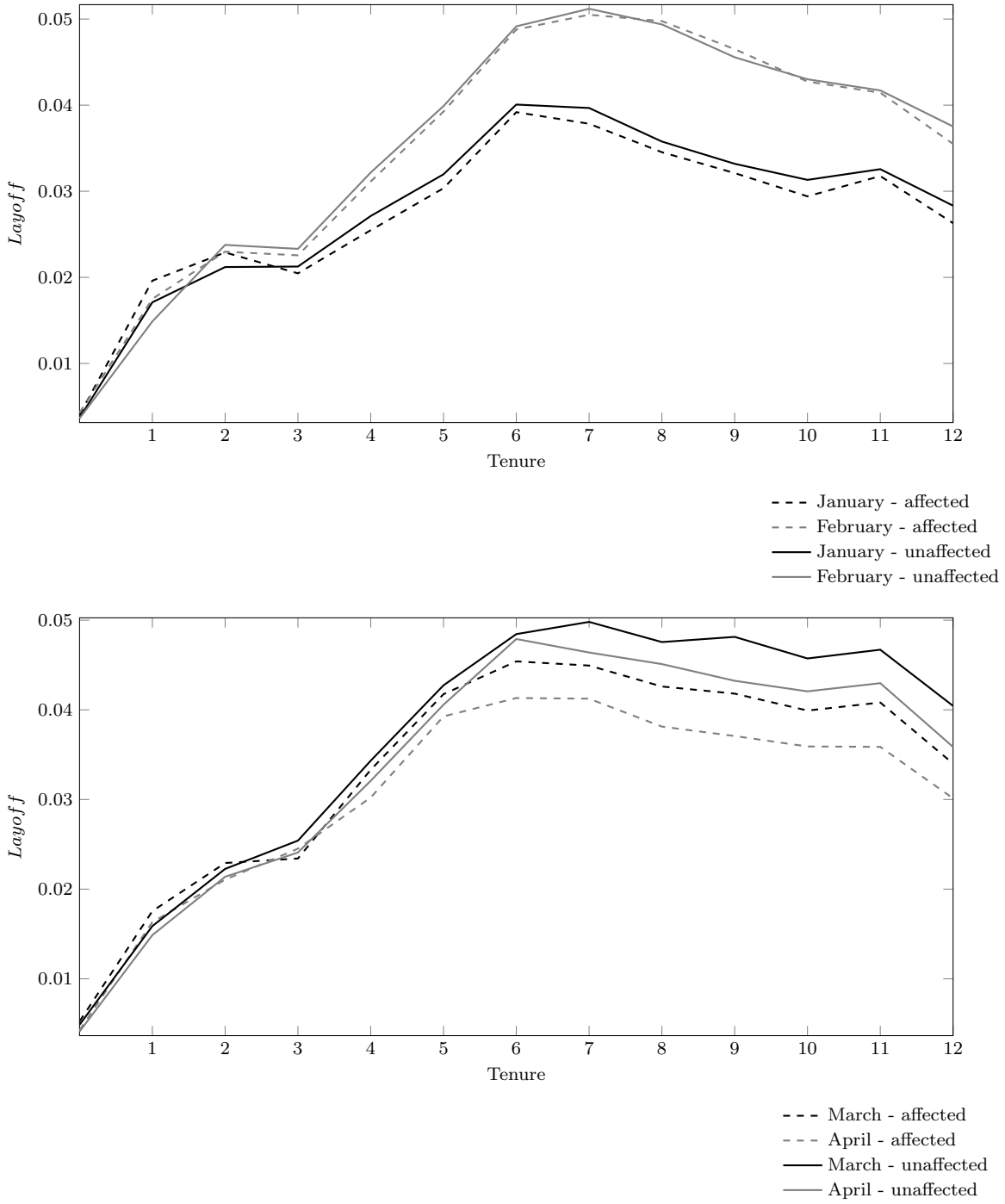

This figure depicts the fraction of unemployment inflow due to eligibility for UI benefits (dashed lines), the fraction of the additional unemployment inflow driven by firm-worker collusion (solid lines), and the total amount of annual UI benefits payments due to strategic UI inflow as a fraction of GDP for different degrees of labor market informality.

Figure 13: **Formal Hiring and Wages**



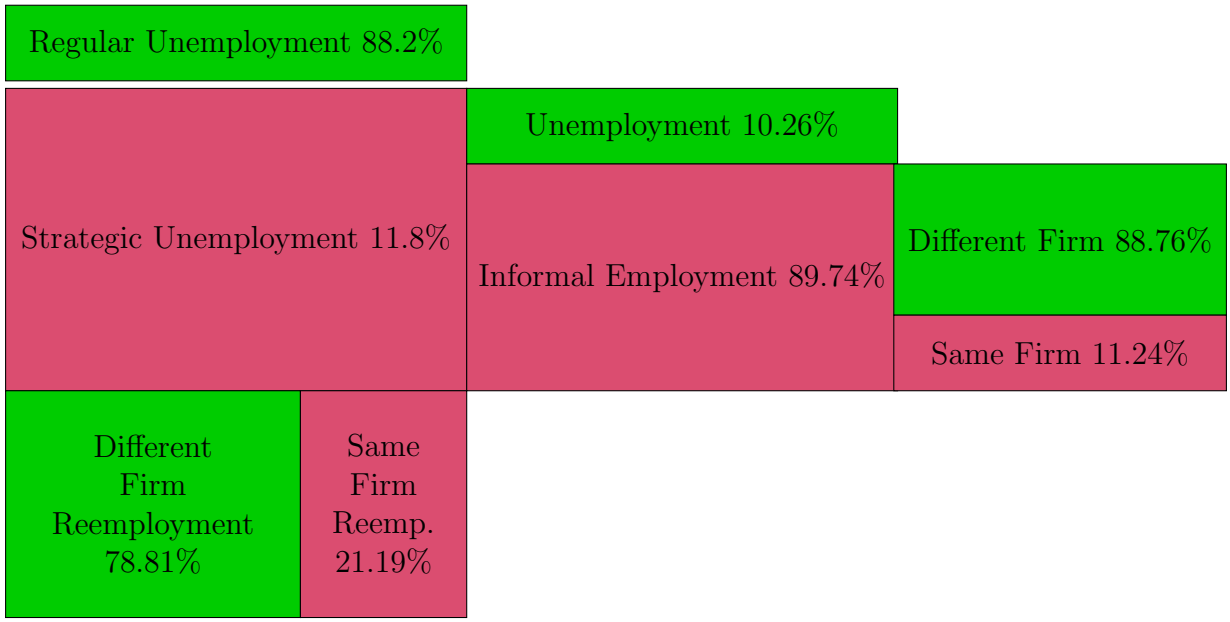

This figure depicts time-series changes in formal hiring and wages around the announcement of the UI benefits reform from January 2014 to December 2015. The unit of observation is at the average across all industry-municipality level observations in a given month for workers with less than two successful past UI applications (solid lines) and workers with two or more successful past applications. In the top panel, the y-axis reports the share of workers hired relative to the total number of workers, in the bottom panel, the y-axis reports the log of the average wage of hired workers. The plots are adjusted for calendar month fixed effects and the average value of the y-axis variable over the full sample period for each group. The vertical line indicates the announcement of the UI reform in December 2014.

Figure 14: **Unemployment Inflow by Tenure - Affected and Unaffected Workers**




This figure depicts hazard rates of unemployment inflow for workers with different tenure for workers affected by the reform (less than two UI spells in the past - dashed lines) and workers unaffected by the reform (at least two UI benefits spells in the past - solid lines) for the months from January and February 2015 in the top panel and March and April 2015 in the bottom panel.

Figure 15: **Strategic Unemployment and Informal Employment - Summary**




This figure summarizes the patterns of unemployment inflow, informal employment, and informal employment and reemployment by the same firm that laid off the worker and other firms.