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

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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.

Abstract

To contain moral hazard effects on labor supply, a key feature in the design of unemployment insurance (UI) programs is to make benefits contingent on layoff. Using the universe of formal labor contracts in Brazil and a sharp discontinuity in the application of an unexpected UI reform, we find that workers are *more likely* to be laid off when they are eligible for UI benefits. Such strategic unemployment accounts for twelve percent of unemployment inflow around the eligibility threshold. Firms and workers collude to time unemployment inflow and outflow with eligibility for UI benefits, explaining at least 20 percent of strategic unemployment inflow. Firms seem to benefit from collusion by paying lower equilibrium wages.

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1 Introduction

Experiencing negative shocks to labor income is one of the most salient risks faced by households.[1] Yet, markets for insurance of labor income are virtually non-existent outside of government-mandated unemployment insurance (UI) programs due to severe adverse selection and moral hazard problems. Individuals who anticipate negative shocks to their labor income may (adversely) select into insurance contracts, making premias too high to sustain.[2] Additionally, once insured, workers' incentive to supply their labor is reduced. Government-run UI programs eliminate the adverse selection problem by making participation mandatory for formally employed private sector workers. Moreover, by conditioning UI benefits on layoff, rather than workers quitting voluntarily, moral hazard in labor supply is contained.[3] A potential threat to containing moral hazard in labor supply is that workers may be able to elicit layoffs when they are eligible for UI benefits, for example through shirking or collusion with employers to be laid off.[4]

In this paper, we exploit a sudden and unanticipated UI reform in Brazil in 2015 that tightened eligibility criteria for UI benefits to examine the effects of UI benefits on labor supply and the role of collusion between firms and workers. We find strong evidence of moral hazard in labor supply when workers are eligible for UI benefits. While the existing literature mainly focuses on the effect of UI on search intensities for reemployment, we find that workers strategically time unemployment *inflow and outflow* to coincide with eligibility for UI benefits. We find that collusion between firms and workers is an important channel to support this form of strategic unemployment. Based on our findings, firms seem to benefit from participating in collusion by paying lower equilibrium wages. Finally, we find that strategic unemployment and collusion are concentrated in labor markets with high levels of

[1] Rothstein and Valletta (2017) document that uninsured unemployment leads to a dramatic rise in family poverty rates.

[2] Hendren (2017) documents that food expenditures of individuals decrease 1-2 years prior to unemployment while their wages remain constant, suggesting they possess private information about future shocks.

[3] UI programs do not fully eliminate moral hazard problems in labor supply. Negative incentive effects of UI on search intensities for reemployment have been widely documented in the literature (Solon (1979), Moffitt (1985), Katz and Meyer (1990), Meyer (1990, 1995), Card and Levine (2000), Meyer and Mok (2007), Card et al. (2015a), Farber, Rothstein, and Valletta (2015), Johnston and Mas (2015), and Landais (2015) for the U.S., and Card, Chetty, and Weber (2007), Lalive (2008), Schmieder, von Wachter, and Bender (2012, 2016), and Card et al. (2015b) for Western Europe).

[4] Shirking behavior to elicit benefits is to some extent limited as workers fired for cause are ineligible. Feldstein (1976) and Baily (1977) argue that employers and employees may have an incentive to implicitely collude to use temporary layoffs when workers are eligible for UI benefits in the absence of full experience rating (Feldstein (1978), Saffer (1982), Topel (1983), and Card and Levine (1994) provide evidence that layoffs are negatively related to experience rating).

informality, suggesting that informal labor markets may play an important role in facilitating collusion between firms and workers.

The nature of the reform provides a sharp discontinuity in the loss of eligibility for UI benefits. Prior to the reform, which came into effect in March 2015, workers with an employment history of at least six consecutive months were eligible for UI benefits. To obtain the same 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 (see Figure 1). Thus, a large fraction of workers with a tenure of six months loses eligibility for UI benefits after the reform, whereas workers with a tenure of five months are not eligible for benefits both before and after the reform. This discontinuity motivates our main identification strategy, a difference-in-differences methodology, in which we compare changes in the incentives for workers with tenure of just below and just above the six months threshold before and after the reform. Importantly, the reform only affects workers' eligibility for UI benefits, but does *not* affect firms' contributions to the UI program. This allows us to isolate the effects of UI benefits on workers' incentives free from direct changes in firms' demand for formal labor. Additionally, the announcement of the reform came as a surprise *after* affected workers entered formal employment. This eliminates concerns about ex ante selection into employment affecting our results.

We start our analysis by examining how UI benefits affect workers' incentives to *exit* formal employment. Our findings indicate that UI eligibility has strong effects on unemployment inflow. Specifically, unemployment inflow relatively drops by 0.55 percentage points for workers just above the six months threshold who lose eligibility for UI benefits after the reform, which constitues a twelve percent decline in unemployment inflow. This suggests that twelve percent of all unemployment at the eligibility threshold can be attributed to strategic unemployment.[5] Consistent with the existing literature, we find that dismissed workers return to formal employment more quickly when they lose eligibility for UI benefits after the reform. Specifically, dismissed workers with a tenure just above the six months threshold who become ineligible for UI benefits after the reform, are about five percentage points more likely to return to formal employment within five months of layoff (the maximum duration of benefits) after the reform, compared to workers that are ineligible for benefits both before and after the reform, which implies a 13.5% lower reemployment probability for worker on UI benefits.

[5]Direct costs of laying off workers are relatively low in Brazil. The penalty that firms incur for laying off a worker amounts to about 8-19 percent of the expected UI benefits paid to workers.

Since UI eligibility is conditional on layoff, a natural question is whether firms collude with workers to time formal unemployment with eligibility for UI benefits. To assess the presence of collusion, we examine layoff and rehiring patterns that are consistent with collusion between firms and employers. A simple and direct mechanism of collusion is for firms to layoff workers when they become eligible for UI benefits and rehire them just when benefits run out. We find that before the reform, workers with a tenure of six months who are laid off just when they become eligible for UI benefits are significantly more likely to be rehired by their previous employer precisely when benefits run out.[6] After the reform when workers with a tenure of six months lose eligibility for UI benefits the pattern completely vanishes. This simple and direct collusion mechanism alone explains almost 20 percent of the strategic unemployment inflow before the reform. This is probably a conservative estimate of collusion, as there are likely to be other collusion mechanisms, for example arrangements involving more than one firm or longer rehiring cycles. This suggests that collusion between employers and workers is an important channel through which workers strategically time unemployment to coincide with UI benefits eligibility.

To examine how firms benefits from collusion with workers, we exploit the fact that the reform only applies to part of the workforce. While workers with less than two successful prior UI benefits applications see their eligibility criteria for UI benefits tightened, workers with at least two successful prior applications are unaffected by the reform. This provides us with a unique opportunitiy to examine the effects of UI on ex ante labor supply and wages using workers unaffected by the reform as a natural control group. On examining changes in wages, we find that newly hired workers with fewer than two successful past UI benefits applications experience a relatively higher increase in wages by 0.5 percent after the reform when they are no longer eligible for UI benefits after six months of employment.[7] This increase in wages is in line with the implicite contracting argument in Feldstein (1976) and Baily (1977), who argue that workers and firms may share subsidies from the UI system through lower equilibrium wages. Consistent with this argument, workers demand higher wages as expected future UI benefits payments are lower after the reform, which includes a reduction in their ability to share benefits from UI payments with their employers.

Exploiting that not all workers are affected by the reform, we further find evidence of an ex ante entitlement effect of UI. As pointed out by Mortensen (1977), UI may have a positive

[6] In case of shirking we would not expect the same firm to be *more* likely to rehire the same worker.

[7] We confirm that this increase in wages constitutes an increase in wages for the same workers rather than a change in hired workers' quality.

effect on labor supply, as future eligibility for UI benefits makes formal employment more attractive by increasing expected future payments. We observe that workers with fewer than two successful past applications for UI benefits are less likely to enter formal employment after the reform when their eligibility criteria are tightened, compared to workers with two or more past applications that are unaffected by the reform. Using survey data on formal and informal employment, we confirm that this drop in formal employment is driven by workers valuing formal employment less when qualifying for UI benefits becomes harder; formal employment falls relative to informal employment within the same industry and municipality. In combination with the observed increase in wages for these workers, this suggests that formal labor supply drops when eligibility criteria for UI benefits are tightened.

We further examine the role of informal labor markets for the results in the paper, which is interesting for several reasons.[8] First, informal labor markets provide a unique alternative to formal employment in the light of UI. Workers are able to receive UI benefits while continuing to be employed informally. This additional outside option may increase workers' incentives to withdraw formal labor when eligible for UI benefits.[9] Second, informal labor markets may facilitate collusion between firms and workers. Theories of implicit contracting in the presence of UI (Feldstein 1976; Baily 1977) require a form of attachment between workers and firms to allow for implicit contracts to be viable. Informal labor markets provide such a form of attachment, as they allow firms to continue employing workers informally while they are on UI benefits. Third, informal labor markets can break the mandatory nature of UI. Informal labor markets allow workers to opt out of UI by working informally when they value its benefits less than the costs associated with formal employment.

We find that the full set of results strongly correlates with the presence of informal labor markets. Exploiting cross-sectional variation in labor market informality across industries and municipalities, we find that the drop in unemployment inflow after the reform is almost exclusively driven by workers in industries and municipalities with large informal labor markets. Specifically, we find that a ten percentage point increase in the share of informal employment in a given industry or municipality corresponds to an about 0.25 percentage points

[8]Brazil constitutes an ideal laboratory to study the role of informal labor markets. It is a middle-income country where informal labor markets are prevalent (according to the International Labor Organization, 36.8 percent of all workers were employed informally in 2013. For comparison, in Europe, informal labor markets account for 17.4 percent of the total labor market during 2008-2009 (Hazans 2011)), and the country is very heterogeneous, providing ample variation in labor market informality across municipalities and industries.

[9]The ability to work informally reduces the likelihood of workers becoming financially constraint, which may further reduce their incentive to seek formal reemployment (Card, Chetty, and Weber 2007).

higher inflow into formal unemployment when workers are eligible for benefits. Consistent with the results on unemployment inflow, we find that the negative effect of UI benefits on reemployment probabilities is significantly stronger in industries and municipalities with a higher fraction of informal employment. Interestingly, the firing-rehiring pattern coinciding with UI benefits eligibility by the same firm is almost entirely driven by industries and municipalities with large informal labor markets. Together, these results suggest that informal labor markets play an important role in facilitating collusion between employers and workers and exacerbate the negative effects of UI on labor supply.

In terms of workers' decision to supply formal labor ex ante, we find that the relative decrease in formal employment and the relative increase in wages when it becomes harder to qualify for UI benefits after the reform is stronger when labor market informality is higher. Specifically, a ten percentage points increase in labor market informality at the industry (municipality) level corresponds to 0.83 (0.53) percent lower formal employment. Formal wages increase relatively more for workers affected by the reform in industries and municipalities where more informal employment options are available by 0.12 (0.44) percent for a ten percentage points increase in labor market informality. Survey data on formal and informal wages shows that within the *same* industry or municipality wages for formally employed workers increase relative to wages of informally employed workers by about three percent after the reform. Together the results suggest a shift in labor supply from formal to informal labor markets when qualifying for UI benefits becomes harder.[10]

We perform several additional robustness tests. First, we control for cyclical 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 the implementation of the reform. Importantly, we do not find any of the patterns for workers around the six months tenure threshold for workers with more than two successful past applications for UI benefits who are not affected by the reform.

The results in the paper provide new insights into the impact of UI benefits on strategic unemployment inflow, collusion between firms and workers, formal labor supply and wages.

[10]While exploiting two sources of labor market informality poses a higher hurdle for alternative factors to explain the results, we cannot rule out that other labor market characteristics also contribute to these effects. A more cautious interpretation of the results is that strategic unemployment and collusion are more prevelant in labor markets that more resemble labor markets in mid-income or developing countries.

While recent studies find no (Card, Chetty, and Weber (2007) for Austria) or very small (Schmieder, von Wachter, and Bender (2012) for Germany) unemployment timing with respect to UI eligibility, the results in this paper show that workers' decision to exit formal employment is strongly affected by their eligibility for UI benefits. The presence of strategic unemployment inflow reflects negative effects of UI on labor supply beyond the lower search intensities for reemployment documented in the literature. Additionally, strategic unemployment inflow poses a challenge to empirical studies on the incentive effects on UI. Most of these studies strongly rely on the assumption that there is no strategic inflow into unemployment for workers who are eligible for UI benefits. The presence of endogenous selection into unemployment around eligibility tresholds for UI benefits may bias measures of search intensities for reemployment. Moreover, we find evidence that collusion between firms and workers to exploit subsidies from the UI system is an important channel for strategic unemployment and that firms benefit from collusion by paying lower equilibrium wages. Finally, we find evidence of an entitlement effect. Workers incorporate expected future benefit payments in their optimization, and therefore are more likely to enter formal employment and demand lower wages when they are more likely to qualify for future UI payments.[11]

The paper also documents how informal labor markets interact with the incentive effects of UI benefits. Recent years have seen a rapid spread of UI programs to middle-income and developing countries with large informal labor markets.[12] In the light of this development, it is important to understand how UI affects workers' incentives in these countries to optimize the design of UI programs.[13] Additionally, even in developed countries, certain sectors of the labor market feature a significant presence of informal labor markets for which understanding its impact on the incentive effects of UI are important.[14] The results in this paper suggest that informal labor markets may play an important role in facilitating collusion between workers and firms to exploit UI benefits payments, since most strategic unemployment and

[11]It is important to note that we only capture the effect of UI on workers' incentives conditional on UI financing being held constant. In general, if financing the UI makes formal labor more costly, demand for formal labor may be adversely affected.

[12]See Holzmann et al. (2011) for data on unemployment insurance around the world.

[13]Some recent studies 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; Gerard and Gonzaga 2014). However, these papers do not directly examine how differences in labor market formality influence the effect of UI programs with the exception of Gerard and Gonzaga (2014). Our results on the effect of informal labor markets on search intensities around the reform are consistent with their cross-sectional results.

[14]Kuhn and Riddell (2010) show for a comparison of U.S. and Canadian border regions that differences in UI systems can have strong effects on labor supply in the long-run when workers adjust their labor supply to optimize UI benefits eligibility.

collusion occurs when the share of informal labor markets is high.[15] Moreover, we find that UI benefits lead to a shift in labor supply from informal to formal labor markets, which is considered a major benefit of introducing social insurance programs in countries with large informal labor markets. In the case of UI, workers enter formal rather than informal employment in order to establish eligibility for UI benefits, leading to lower formal wages in equilibrium.

It is important to consider the relevance of the findings beyond the specific context of the study. First, our data spans the entire population of formal employees in the private sector. Thus, the results are not subject to any selection bias or specific to a subgroup of workers. Second, our findings are obtained during a severe recession in Brazil. Schmieder, von Wachter, and Bender (2012) show that incentive effects from UI are *weaker* during recessions in Germany. This suggests that the results we find are rather conservative estimates of the effects of UI insurance. Additionally, evidence on the effects of UI benefits in recessionary periods are of particular interest, as they provide a fiscal stimulus during recessions and are often extended during downturns (Rothstein 2011; Valletta 2014; Farber and Valletta 2015; Kroft and Notowidigdo 2016). Finally, while informal labor markets are less prevalent in developed countries, these countries also feature a non-negligible degree of informality in parts of their labor markets. Hence, we think that the results in this paper are relevant and informative beyond the specific context in this paper.

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. It is mandatory for employers to sign workers' cards whenever a worker is hired, promoted, or dismissed. This information is reported to the Ministry of Labor every year. Formal employees are entitled

[15] Stronger strategic unemployment in areas with higher informal labor markets is consistent with Card, Chetty, and Weber (2007) and Schmieder, von Wachter, and Bender (2012) who find no or only weak evidence of strategic unemployment around UI eligibility tresholds in developed countries.

to a minimum wage. Payroll taxes amount to twenty percent of the formal wage to finance the public pension system, plus eight percent for workers seniority account (FGTS).[16] Other mandatory contributions such as the social integration program (PIS) and contributions to social security funding (COFINS) depend on the industry that the firm operates in. These contributions are paid as a fraction of net profit and sales and funding of the UI system stems from these contributions.

UI applies to formally employed workers in the private sector. Benefits are paid for three to five months, depending on workers' time in formal employment. Three payments are made if a worker was employed between six and eleven months in the last 36 months, four payments are made if a worker was employed between 12 and 23 months in the last 36 months, and five payments are made if a worker was employed for at least 24 months in the last 36 months. In 2015, the monthly payment ranges from 1 to 1.76 minimum salaries, depending on the average pre-layoff wage. Importantly, the UI system does not feature a direct experience rating mechanism as in the U.S. If a firm dismisses a worker without a justified reason, it must pay an additional fifty percent of the total contribution that has accumulated in an employee's FGTS. This cost of laying of a worker amounts to 8-19 percent of the expected benefits payments to the worker depending on the pre-layoff wage (the penalty is lower for lower pre-layoff wages). 80 percent of this penalty is directly paid to the worker rather than used for funding the UI system. Firing workers with a valid legal justification does not involve penalties. However, this is rare (only 3.5 percent of all layoffs), since the hurdle to provide sufficient evidence is high, and judges tend to rule in favor of employees.

2.2 UI Reform

To be eligible for UI benefits prior to March 1 2015, a worker had to be employed over a consecutive period of at least 6 months prior to layoff, had to be fired without a justified reason, may not earn any other labor income, and may not have sucessfully 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

[16]This account can be withdrawn when a worker retires, is fired, or suffers from a serious illness.

unexpected.[17] Since the UI reform was announced unexpectedly only two months before its implementation, workers with tenure of more than two months at the implementation of the reform were already in formal employment before the announcement of the reform. The main driver for the quick implementation and the tightening in eligibility criteria were attempts on part of the government to reduce the growing budget deficit. The size and duration of UI benefits was not altered. Importantly, UI contributions of employers were unaffected by the reform. Thus, the reform had no direct effect on employers' demand for formal labor.

The reform affected workers with less than two successful past 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 months 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 and turned into law in July 2015 with minor adjustments.

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 Labor Ministry of 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, reason for layoff, etc.). We use data from RAIS for the period from 2013–2015. By the end of 2014, the database covers about 50 million formal employees. The datasets allows us to trace the duration of formal employment for each individual. We combine this data with information on the number of previous unemployment spells with UI benefits payments, also maintained by the Ministry of Labor, as the reform only applies to worker with fewer than two such spells. We exclude all public sector employees, since they do not participate in the UI program.

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

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). Finally, we use data from the previous year to control for cyclical effects.

In Table 1, we confirm 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. We find that both groups of workers are virtually identical in terms of age, average salary, gender, university education, the size of the firm they are employed at, and the industries that they are employed in. They do, however, differ in terms of the probability of becoming unemployed and returning to formal employment. Specifically, a worker with six or seven months tenure is 44 percent more likely to be laid off without a justified reason and 19 percent less likely to return to formal employment within five months after layoff.

To exploit cross-sectional variation in labor market informality, we combine the linked 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.[18] The census groups workers into twenty different industry classifications (see Table 2). 66 percent of domestic services employees are working informally. The most formal industry, electricity and gas, has only 5.5 percent of informal workers. In terms of geographic variation in informality, most municipalities fall within the range of 20 to 70 percent of labor market informality (Figure 2). Informality is not limited to some areas in Brazil but is prevalent throughout the country with somewhat higher average informality in the north (Figure 3).

Finally, we also take advantage of the National Household Sample Survey (PNAD). This quarterly survey collects information on formal and informal employment and wages for the working age population in 20 municipalities that are the respective state's "capital munici-

[18]We verify that the results are robust to alternative definitions of labor market informality provided in the census.

palities". The survey includes about 575,000 individuals in the end of 2014.

3 Empirical Strategy

This section outlines the empirical strategy employed in this paper to assess how UI affects workers' incentives to flow into and out of formal employment, the role of collusion between firms and workers in explaining these inflow and outflow patterns, the effects of UI on workers' ex ante decision to enter formal employment, its effects on equilibrium wages, and how the incentive effects of UI are influenced by the presence of informal labor markets.

3.1 Unemployment Inflow and Outflow

The sharp discontinuity in the reform's effect allows us to compare changes in unemployment inflow and outflow for workers just above the eligibility threshold (six or seven months tenure) and workers just below the threshold (four or five months tenure). Workers with tenure of six or seven months, are eligible for UI benefits only before the reform, whereas workers with tenure of four or five months are never eligible for benefits. Monthly data allows us to focus on a narrow time period of two months before and after the reform. Importantly, the unexpected announcement of the reform occured after workers entered formal employment eliminating concerns about differences in ex ante selection into formal employment under both regimes. Additionally, the reform only applied to a subset of the workforce, providing us with a natural control group of worker unaffected by the reform. Together, this allows us to identify how UI benefits affect workers' decision to flow into and out of unemployment.

We start by examining changes in unemployment inflow after the implementation of the reform for workers just below and just above the six months tenure threshold by estimating:

$$P[u_{unjust}]_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it} \quad (1)$$

where $P[u_{unjust}]_{it}$ is a dummy variable that takes the value of one if worker i is laid off in month t, and zero otherwise.[19] The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, and zero for workers with tenure of four or five months. The dummy variable $Reform_t$ takes the value of one for the two months after the

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

reform, and zero for the two months before the reform. The sample is limited to workers with less than two successful past applications for UI benefits since only these workers are directly affected by the reform. We further saturate equation (1) with month, municipality-month, municipality-industry-month, and municipality-industry-occupation-month fixed effects to control for location-specific, local industry-specific, and local occupation-specific shocks in unemployment inflow.

The parameter of interest is β_3. The coefficent β_3 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 with six months are eligible for UI benefits. A negative value of β_3 implies that UI benefits lead to higher unemployment inflow when workers are eligible for UI benefits, and vice versa.

We apply the same identification strategy to estimate the effect of UI benefits on unemployment outflow by replacing the dependent variable with $P[e \leq 5]_{it}$, a dummy variable that takes the value of one if worker i is reemployed within five months after being laid off, and zero otherwise. The five months time-period is motivated by the fact that UI benefits are available for a maximum of five months. Similar as before, we restrict the sample to workers that were laid off with tenure of four to seven months. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. Here, a positive value of β_3 implies that UI benefits lead to lower unemployment outflow, and vice versa.

3.2 Collusion

To assess the role of collusion between firms and workers for unemployment inflow and outflow patterns, we examine layoff and rehiring patterns that are consistent with firm-worker collusion. In case of collusion between firms and workers, we expect the same firm to layoff workers when they are eligible for UI benefits and to rehire them when eligibility for benefits is exhausted. Instead, if workers elicit layoffs without the involvement of employers, for example through shirking, we do not expect workers to be more likely to be rehired by the same firm when benefits run out. Specifically, we test whether firms collude with workers by laying them off when they become eligible for UI benefits and rehire them just when UI

benefits are exhausted by estimating:

$$P_{same}[4-9]_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it} \quad (2)$$

where $P_{same}[4-9]_{it}$ is a dummy variable that takes the value of one if a worker returns to the same firm four to nine months after being laid off, and zero if a worker is not ired by the same firm four to nine months after layoff.[20] The sample for this test comprises all workers laid off with tenure of four to seven months. The dummy variable $6Months_{it}$ takes the value of one for workers with a tenure of six or seven months at layoff, and zero for workers with a tenure of four or five months at layoff. Here, a negative value of β_3 implies that the same firm is more likely to rehire workers when they were eligible for UI benefits and these benefits have run out.

3.3 Formal Employment and Wages

Next, we assess the ex ante incentive effects of UI exploiting the fact that the reform only applies to part of the workforce. The prospect of future eligibility for UI benefits may lead to an entitlement effect, according to which workers value formal employment more (Mortensen 1977). To assess whether workers are less likely to work formally when it becomes harder to qualify for UI benefits, we compare changes in formal employment for workers affected by the reform and workers for whom eligibility criteria are unaffected by estimating:

$$Workers\ Hired_t = \alpha + \beta_1 \cdot Affected_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot Affected_{it} * Reform_t + \epsilon_{it} \quad (3)$$

where $Workers\ Hired_t$ is defined as the number of workers hired in a given industry in a given municipality in month t scaled by the number of workers employed in the respective local industry in the month when the reform was announced. To examine the net effect on total formal employment, we replace the dependent variable by the log of total employment in a local industry. Workers' incentives to enter formal employment are affected from the time they are aware of the reform's effects. Since the reform was announced on December 30, 2014, we define the $Reform_t$ dummy as one from January 2015. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits whose eligibility criteria are tightened by the reform, and zero for workers

[20] Workers are eligbile for at least three months of UI benefits. Workers may not have successfully applied for UI benefits for 16 months before reapplying. Thus, firms that hire and layoff workers to exploit the UI system might rehire workers only after nine months before laying them off after another six months for them to be able to reapply for UI benefits.

with two or more successful past applications for whom eligibility criteria remain unchanged. Coefficient β_3 measures the relative change in the number of affected workers hired and employed after the reform, compared to workers unaffected by the reform.

Using the same methodology, we examine changes in wages by replacing the dependent variable with the log of the average hiring wage in month t. Higher ex ante labor supply or collusion between firms and workers in the presence of UI may lead to lower equilibrium wages, consistent with the implicit contracting argument in Feldstein (1976) and Baily (1977).

To strengthen the evidence on wages, we use data on wages from formal and informal jobs from the quarterly PNAD survey. This allows us to compare changes in formal and informal wages by estimating:

$$log(wage)_{it} = \alpha + \beta_1 \cdot Reform_t + \beta_2 \cdot Formal\ Job_{it} + \beta_3 \cdot Reform_t * Formal\ Job_{it} + \epsilon_{it} \quad (4)$$

where $Formal\ Job_{it}$ takes the value of one if worker i is formally employed in quarter t, and zero if worker i is informally employed in quarter t. We can saturate equation (4) to compare changes in formal and informal wages within the same industry (industry-time fixed effects) and the same municipality (municipality-time fixed effects).

3.4 Labor Market Informality

Finally, we examine the role of informal labor markets in affecting how workers incentives change when they are eligible for UI benefits. We exploit two sources of variation in labor market informality, cross-sectional variation in informality across industries (Table 2) and variation in labor market informality across municipalities (see Figures 2 and 3).[21] To formally assess how UI benefits affect workers' incentives in 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 equations (1) to (3). In the framework of existing theories on the incentive effects of UI, informal labor markets may alter the effects of UI on workers' incentives. Informal labor

[21] Labor market informality at the municipality level is not primarily determined by industry composition. When we compute the difference between the actual share of labor market informality and the share of informality as predicted by industry composition in the respective municipality, we observe that the distribution of this difference is highly correlated with the actual measure (0.93) and the distribution is very similar (Figure A.1).

markets provide workers with the opportunity to claim UI benefits while continuing to be (informally) employed. This may exacerbate incentive effects of UI and facilitate collusion between firms and workers (Feldstein 1976; Baily 1977). Additionally, making formal labor more attractive might have a particularly strong impact on workers' decision to enter formal labor markets when workers have the option to work informally.

4 Results

This section presents the empirical results. We document that eligibility for UI benefits has large effects on unemployment inflow and outflow patterns, part of which is explained by collusion between firms and workers. Additionally, UI ex ante incentivizes workers to take up formal work at lower wages. Exploiting cross-sectional variation in labor market informality, we document that all effects are stronger in the presence of informal labor markets.

4.1 UI Benefits and Unemployment Inflow

Figure 4 depicts the probability of being laid off for workers with different tenure, separately for the months from January to April 2015 (gray lines represent the pre-reform values, black lines the post-reform values).[22] While there are no significant changes in layoff probabilities for workers with a tenure of four to five months, for workers with tenure of six to seventeen months the probability of being laid off significantly decreases after the reform, consistent with the new eligibility threshold of 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 UI benefits after the reform, relative to unemployment inflow for workers with a tenure of five months who are ineligible for UI benefits even before the reform.

We confirm the insights from the graphical analysis statistically in Table 3 by estimating equation (1). Controlling for time-series variation in unemployment inflow (month fixed effects) in column I, we find that unemployment inflow relatively decreases by 0.55 percentage points for workers with tenure of six or seven months compared to workers with a tenure of four or five months, which is equivalent to a twelve percent decrease in unemployment inflow. Further saturating the specification with municipality-month fixed effects to account for local

[22]The values for workers with five month tenure are aligned at the April 2015 values to facilitate comparison.

shocks in column II, the effect remains identical with 0.55 percentage points. The results are not affected by controlling for industry-specific local shocks (municipality-industry-month fixed effects) in column III with 0.54 percentage points. Controlling for occupation-specific local shocks (municipality-industry-occupation-month fixed effects), we find that the effect remains almost constant at 0.51 percentage points for workers with six or seven months tenure compared to workers with four or five months tenure (column IV).[23]

The results cannot be explained by cyclical effects, as we observe no similar pattern during the same months in the year before the reform (Figure A.2 and Table A.3) or for workers with more than two previous successful UI benefits applications who are not affected by the reform (Table A.6). Additionally, the results are robust to comparing workers with tenure of four to seven months in November and December 2014, the two months *before* the announcement of the reform, to the post-reform period in March and April 2015 with similar magnitudes, which ensures that the announcement of the reform does not bias the results (Table A.9).

Higher unemployment inflow when workers are eligible for UI benefits has important implications. First, if workers are able to elicit UI benefits payments strategically when they become eligible for UI benefits, adverse moral hazard effects on labor supply are more severe than generally assumed. Second, while some recent studies (Card, Chetty, and Weber 2007; Schmieder, von Wachter, and Bender 2012) find that UI eligibility has only minor effects on strategic unemployment in developed countries and at higher tenure thresholds, selection into unemployment may be a threat for empirical studies on search intensities of unemployed workers around UI eligibility thresholds in other settings.

4.2 UI Benefits and Unemployment Outflow

Figure 5 depicts reemployment probabilities conditional on unemployment duration for workers laid off during the months from January to April 2015, separately for workers with a tenure of six or seven months (top panel) and workers with a tenure of four or five months (bottom panel) at layoff.[24] For workers with six or seven months tenure at the time of layoff, reemployment is significantly less likely to occur within five months during January

[23]The results in Table A.1 show that there is no "relabeling" of unemployment from voluntary departure to layoff when workers are eligible for UI benefits. We observe no change in voluntary layoffs around the reform.

[24]Reemployment probabilities in the first month include workers that directly transition to a new job.

and February when workers are eligible for UI benefits for three to five months, compared to March and April when they are no longer eligible for UI benefits. Instead, these workers are more likely to return after more than five months when they are no longer eligible for benefits. In contrast, for workers with four or five months tenure at the time of layoff, unemployment outflow does not show such patterns after the reform.

The results from estimating equation (1), depicted in Table 4, show that workers with a tenure of six or seven months who lose eligibility for UI after the reform become 5.16 percentage points more likely to return to formal employment, compared to workers with four or five months tenure at layoff (column I). This implies that reemployment within five months of layoff increases by about 13.5% from a base rate of 38% before the reform. The effect is similar with 5.06 percentage points when we compare workers in the same geographical area (columns II). Further restricting the comparison to workers within the same local industry does not affect the results with 5.19 percentage points (column III), as does comparing workers within the same occupation within a local industry with 4.98 percentage points (column IV).[25] Lower search intensities for reemployment are consistent with findings in the prior literature that workers are less likely to return to formal employment when they are eligible for UI benefits.

4.3 Collusion

To be eligible for UI benefits, workers need to be laid off by their employer, as voluntary departures do not qualify them for UI benefits. Layoffs may be induced through different mechanisms. For example, workers may elicit layoffs through shirking, or firms may collude with workers to extract rents from the UI system by laying them off when workers are eligible for UI benefits.

To assess whether collusion between firms and workers plays an important role in driving strategic unemployment, we explore whether firms that lay off workers when they become eligible for UI benefits rehire the same workers just when benefits run out. Specifically, we examine the probability of being rehired by the same firm four to nine months after a layoff when benefits run out by estimating equation (2). If higher unemployment inflow is driven by shirking, we do not expect firms to be more likely to rehire the same worker. In contrast,

[25] We find no similar patterns in umeployment outflow for the same months in the previous year (Figure A.3 and Table A.4), or for workers unaffected by the reform (Table A.7).

if firms collude with workers, we expect them to rehire the same worker when benefits run out. We follow our main identification strategy comparing dismissed workers with six or seven months of tenure at the layoff who lose eligibility after the reform to those with five months tenure at layoff who are always ineligible.

The results are gathered in Table 5. Column I shows that before the reform the probability to be rehired by the same employer four to nine months after layoff is about two percentage point higher for workers with a tenure of six or seven months at layoff compared to those with four or five months of tenure at layoff. After the reform, when both types of workers are ineligible for UI benefits, the difference in rehiring by the same firm four to nine months after layoff almost completely vanishes, dropping by 1.66 percentage points. Controlling for local industry shocks (municipality-industry-month fixed effects) in column II, and occupation-specific shocks within a local industry (municipality-industry-occupation-month fixed effects) in column III does not affect the results. In columns IV to VI, we restrict the sample to workers that are rehired between four to nine months after layoff to ensure that our results are not affected by changes in reemployment timing. The results confirm that the difference in the probability of being rehired by the same firm four to nine months is restricted to workers with six or seven months tenure at layoff before the reform when they are eligible for UI benefits. We find no similar patterns for the same months in the year before the reform (Table A.5), and for workers with more than two previous UI benefits spells who are not affected by the reform (Table A.8). These results are consistent with collusion between workers and their employers. Firms layoff workers when they qualify for UI benefits and rehire them when benefits are exhausted.

The results in Section 4.1 show that the additional formal unemployment inflow due to eligibility for UI benefits constitues twelve percent of all laid off workers. The probability of being rehired by the same firm four to nine months after layoff decreases by 1.7 percentage point after the reform for workers with a tenure of six or seven months at layoff from 7.0% to 5.3%. This implies that around 19.5 percent of strategic unemployment inflow due to UI benefits eligibility can be explained by this simple form of collusion between employers and workers.[26] These estimates are likely to be conservative. First, we assume that colluders return to the same firm with a probability of one, whereas in reality there might be cases

[26]The fraction of colluders can be computed as: $12\% * x + (1 - 12\%) * 5.3\% = 5.3\% + 1.7\%$, where 5.3% is the base rate of reemployment by the same firm in the absence of UI benefits, 12% is the fraction of strategically unemployed workers among all unemployed workers, and x is the fraction of colluders among strategically unemployed people (for whom reemployment by the same firm equals one for the most conservative estimate).

where reemployment in the same firm fails, for example due to changes in business conditions. Second, we only capture part of all possible types of collusions. For example, several firms and employees as a group could engage in collusion in a way that our test would does not identify as collusion, or formal reemployment may not be part of the collusion agreement, or delayed beyond nine months after layoff.

4.4 Employment and Wages

Figure 6 depicts the time-series evolution in formal hiring scaled by total employment (top Panel), the log of total employment (middle 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 by the reform (solid lines), and workers with at least two successful past UI benefits applications, who are unaffected by the reform (dashed lines).[27] From January 2015, the month after the announcement of the reform, we start to observe a relative drop in hiring of workers affected by the reform. This drop in hiring of workers who see eligibility criteria for UI benefits tightened is reflected in a continuous relative drop in total employment of these workers. A simultaneous relative increase in wages for newly hired workers that face stricter requirements to qualify for UI benefits after the reform suggests that the drop in their formal employment is driven by a drop in formal labor supply.

In Table 6, we examine changes in formal hiring, employment, and wages after the reform statistically by estimating equation (3). The results in columns I and II indicate that monthly formal hiring of workers who are less likely to qualify for UI benefits after the reform relatively decreases by 0.41 percentage points of the pre-reform labor force. Continued lower hiring leads to a relative drop in their formal employment by about five percent (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 relatively increase by about 0.5-0.7 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 workers rather than selection of higher quality workers. The change in wages for hired workers over their wage in their last job during the previous twelve months is 0.5 percent higher for workers affected by the reform.

[27] To facilitate comparison all plots are adjusted for calender month and worker group (affected vs. unaffected) fixed effects.

This decline in quantity (employment) and price (wages) suggests that labor supply is lower when workers are less likely to qualify for UI benefits consistent with an entitlement effect of UI (Mortensen 1977). Additionally, lower wages are consistent with colluding firms and workers implicitly agreeing on lower equilibrium wages to share rents from the UI system (Feldstein 1976; Baily 1977).

4.5 Labor Market Informality

This section presents the results on the role of labor market informality in explaining the previous findings. Informal labor markets provide workers with the opportunity to receive UI benefits while continuing to work (informally). This may exacerbate adverse effects of UI on formal labor supply and facilitate collusion between firms and workers by proving a source of attachment between firms and workers making implicit contracts feasible (Feldstein 1976; Baily 1977). Additionally, creating entitlement effects attached to formal labor to increase labor market formalization is an often invoked argument in favor of providing social insurance, especially in mid-income and developing markets that feature large informal labor markets.

In Figure 7, 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 reveals 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 workers, we observe a substantial drop in unemployment inflow in March and April when they lose eligibility for UI benefits. In contrast, for workers in industries with below median levels of informality, we observe a smaller change in unemployment inflow. Similarly, we find that in municipalities with above median levels of informality unemployment inflow decreases by about two percentage points for workers that lose eligibility for UI benefits after the reform. In municipalities with below median levels of informality, the magnitude of the effect is below one percentage point (Figure 8).[28]

In Table 7, we formally assess how informal labor markets affect workers' response to UI benefits. The top panel shows the results for variation in labor market informality at the industry level. We find that a ten percentage points increase in labor market informality leads to a 0.28 percentage points stronger decrease in unemployment inflow after the re-

[28]Median informality is slightly above 20 percent for industries and about 40 percent for municipalities.

form controlling for industry- and municipality-level shocks (column I). Controlling for local shocks that are specific to workers affected by the reform, the effect is similar with 0.23 percentage points (column II). Additionally, controlling for local industry shocks leaves the effect virtually unchanged with 0.20 percentage points (column III). When we further add controls for shocks to specific occupations within a local industry, the magnitude of the effect is similar with 0.22 percentage points (column IV). We find qualitatively identical results with similar magnitudes when we compare changes in unemployment inflow in municipalities with different levels of labor market informality (bottom panel). Together, the results in Table 7 show that UI benefits have a stronger effect on unemployment inflow in the presence of informal labor markets.

Figures 9 and 10 depict reemployment probabilities conditional on unemployment duration for labor markets with above median levels of informality (top panels) and below median levels of labor market informality (bottom panels) for worker with tenure of four or five months (left panels) and workers with tenure of six or seven month (right panels). The increase in reemployment probabilities for workers with six months tenure at layoff is somewhat higher in industries and municipalities with above median levels of labor market informality, compared to industries and municipalities with below median labor market informality.

In Table 8, we assess statistically how informal labor market affect search intensities in the presence of UI benefits. The top panel shows the results for variation in labor market formality at the industry level. We find that reemployment probabilities in the five months after layoff increase by 0.38 percentage points more per ten percentage points increase in labor market informality when workers lose eligibility for UI benefits after controlling for industry-specific and municipality-specific shocks (column I). Controlling for local shocks specific to worker with different tenure at layoff strengthens the effect to 0.60 percentage points (column II). Controlling for local industry-specific shocks, the effect is almost identical with 0.75 percentage points (column III). When we further add controls for local industry shocks to workers in the same occupation, the magnitude of the effect is 0.72 percentage points (column IV). The bottom panel exploits variation in labor market formality at the municipality level with similar results that are statistically weaker. Together, the results suggest that search intensities are somewhat lower in the presence of informal labor markets, while the relative effect of informal labor markets is weaker than for unemployment inflow.

Next, we examine whether collusion between workers and their firms is concentrated in

industries and municipalities with large informal labor markets in Table 9. The results in Panel A show that firms in more informal industries are significantly more likely to lay off workers when they are eligible for benefits to rehire them after benefits run out. Specifically, a ten percentage increase in labor market informality leads to a 0.50-0.66 percentage points increase in strategic layoffs and rehiring. The results are similar at the municipality level with slightly higher magnitudes in Panel B. This cross-sectional evidence suggests that informal labor markets facilitate collusion between firms and workers by extracting rents through UI benefit payments, while maintaining an informal relationship.

The reduction in formal labor supply of workers whose eligibility criteria of UI benefits are tightened by the reform documented in Section 4.4 may be stronger when workers have the option to work informally enabling them to continue earning income from labor while receiving UI benefits. Testing this conjecture formally in Table 10, we find that hiring (columns I and II) and employment (columns III and IV) drops particularly stongly for affected workers in industries (Panel A) and municipalities (Panel B) with larger shares of informal labor markets. Consistent with the 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).[29]

Data on formal and informal wages from the quarterly PNAD survey allows us to examine changes in wages for formal and informal wages in the same industry or municipality in Table 11. We find that formal wages increase relative to informal wages by 3.33 percent within the same industry (column I). Within the same municipality, the relative increase in formal wages is 2.29 percent (column II). The effect is similar with 2.45 percent when we compare changes in formal and informal wages within the same local industry (column III). While stronger in more informal industries, the relative effect on formal to informal wages is not significantly different in industries or municipalities with a higher share of labor market informality (columns IV and V). The relative strength of the effect depends on demand and supply elasticities for formal and informal labor. Thus, it is ex ante not obvious whether the *relative* change in wages should be higher or lower when informal labor markets are larger.

Together, the results in this section suggest that informal labor markets exacerbate negative effects of UI on (formal) labor supply conditional on qualifying for UI benefits, facilitate collusion between firms and workers, and that UI has a stronger effect on ex ante formal

[29]In Table A.2, we document that the share of formal relative to informal workers within the same industry (columns I and II) or the same municipality (columns III and IV) drops after the reform.

labor supply when workers have the option to work informally.

5 Conclusion

Exploiting a reform to UI benefits eligibility criteria in Brazil, this paper documents that workers are more likely to exit formal employment when they qualify for UI benefits. The presence of this type of strategic unemployment inflow aggrevates negative moral hazard effects of UI on labor supply documented in the literature. Higher unemployment inflow when workers are eligible for UI benefits has important implications. If workers are able to elicit UI benefits payments strategically when they become eligible for UI benefits, adverse moral hazard effects on labor supply are more severe than generally assumed. Additionally, selection into unemployment may be a threat for empirical studies on search intensities of unemployed workers on UI benefits. Consistent with previous results in the literature, we also find that workers return to employment later when they are eligible for UI benefits. Additionally, our results suggest that firms collude with workers to extract rents from the UI system. To successfully apply for UI benefits workers need to be laid off by their employer. We find that firms layoff workers when they become eligible for UI benefits and rehire them just when benefits run out. Moreover, our findings indicate that firms may benefit from collusion through paying lower equilibrium wages.

Additionally, we find that easier access to UI benefits leads to higher supply of formal labor consistent with an entitlement effect (Mortensen 1977). Higher labor supply in turn leads to lower formal wages for newly hired workers. This increase in labor supply, together with the drop in wages suggests that UI has indirect effects on firms' labor costs. This could at least partially offset some of firms' costs associated with the financing UI programs, which implies that the net costs that firms incur for financing UI programs is lower than commonly argued (Levy 2008). Importantly, in our setting, firms' financing costs of the UI system are kept constant. This has the advantage that we can identify effects on the supply of labor free from direct effects on the demand for labor. The downside is that we cannot measure net effects of benefit payments *and* financing costs on equilibrium employment.

Finally, all results strongly correlate with labor market informality at the industry and municipality levels. This suggests that the opportunity to combine UI benefits payments with labor income from informal work exacerbates the negative effects of UI benefits on

(formal) labor supply. Consistent with Feldstein (1976) and Baily (1977), informal labor markets seem to facilitate collusion between firms and workers by allowing them to maintain a relationship through official unemployment. The increase in labor market formalization through UI thus comes with the caveat that some of the formalization is part of collusion between firms and workers to extract rents from the UI system.

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Table 1: **Summary Statistics for Workers Around 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
P[$U_{unjust.}$]	0.032	0.046	0.014
P[$e \leq 5$]	0.521	0.424	-0.097

This table reports descriptive statistics (age, salary, gender, education, firm size, industry distribution, monthly layoff probability, and the probability to return to work within five months after layoff) for workers with tenure of four to five months in the first column, and worker 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
Accomodation, Food	0.3155	0.0405
Real Estate	0.2850	0.0099
Trade, Repair of Motor Vehicles and Motorcycles	0.2562	0.1893
Water, Severage, 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 industry from the Census Brazil.

Table 3: **Unemployment Inflow**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0126***	0.0123***	0.0121***	0.0122***
	(0.0006)	(0.0006)	(0.0005)	(0.0005)
$6Months_{it} * Reform_t$	-0.0055***	-0.0055***	-0.0054***	-0.0051***
	(0.0008)	(0.0007)	(0.0006)	(0.0006)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	7,745,356	7,745,356	7,745,356	7,745,356
R^2	0.001	0.015	0.051	0.128

This table reports changes in unemployment inflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months in a given month. 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 or seven months, 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 to April 2015 and zero for the pre-reform period from January to February 2015. 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: **Unemployment Outflow**

Dep. Var.: $P[e \leq 5]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0872***	-0.0859***	-0.0861***	-0.0846***
	(0.0044)	(0.0044)	(0.0048)	(0.0052)
$6Months_{it} * Reform_t$	0.0516***	0.0506***	0.0519***	0.0498***
	(0.0037)	(0.0034)	(0.0039)	(0.0034)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	389,753	389,753	389,753	389,753
R^2	0.006	0.015	0.028	0.099

This table reports changes in unemployment outflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i enters formal employment within five months after being laid off and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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. 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 5: **Collusion**

Dep. Var.: $P_{same}[4-9]_{it}$	I	II	III	IV	V	VI
$6Months_{it}$	0.0205***	0.0190***	0.0181***	0.0178***	0.0169***	0.0158***
	(0.0022)	(0.0020)	(0.0023)	(0.0036)	(0.0033)	(0.0038)
$6Months_{it} * Reform_t$	-0.0166***	-0.0154***	-0.0149***	-0.0187***	-0.0174***	-0.0176***
	(0.0026)	(0.0025)	(0.0030)	(0.0047)	(0.0048)	(0.0056)
Month*Municipality FE	yes	-	-	yes	-	-
Month*Municipality*Industry FE	no	yes	-	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	yes	no	no	yes
Clustered SE	muni	muni	muni	muni	muni	muni
Observations	214,643	214,643	214,643	100,580	100,580	100,580
R^2	0.209	0.328	0.475	0.264	0.407	0.563

This table reports changes in reemployment of workers by the same firm after the exhaustion of UI benefits around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months at layoff that are rehired within nine months in columns I-III and within four to nine months in columns IV-VI. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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. 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 6: **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 last wage during the previous twelve months. 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. Bootstraped standard errors are reported in parentheses. *** denotes statistical significance at the 1% level.

Table 7: **Unemployment Inflow by Informality**

	I	II	III	IV
Dep. Var.: $P[u_{unjust.}]_{it}$	Industry-Level			
$6Months_{it}$	-0.0007			
	(0.0012)			
$6Months_{it} * Reform_t$	0.0010			
	(0.0015)			
$6Months_{it} * Informal$	0.0542***	0.0478***	0.0415***	0.0432***
	(0.0054)	(0.0050)	(0.0040)	(0.0040)
$6Months_{it} * Reform_t * Informal$	-0.0283***	-0.0226***	-0.0199***	-0.0215***
	(0.0064)	(0.0059)	(0.0051)	(0.0051)
Industry*Month FE	yes	yes	-	-
Month*Municipality FE	yes	-	-	-
Month*Municipality*Eligibility FE	no	yes	yes	yes
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	7,745,356	7,745,356	7,745,356	7,745,356
R^2	0.021	0.026	0.055	0.131
Dep. Var.: $P[u_{unjust.}]_{it}$	Municipality-Level			
$6Months_{it}$	0.0047***			
	(0.0010)			
$6Months_{it} * Reform_t$	0.0023**			
	(0.0010)			
$6Months_{it} * Informal$	0.0320***	0.0241***	0.0241***	0.0223***
	(0.0032)	(0.0033)	(0.0030)	(0.0031)
$6Months_{it} * Reform_t * Informal$	-0.0331***	-0.0299***	-0.0280***	-0.0253***
	(0.0039)	(0.0041)	(0.0038)	(0.0041)
Municipality*Month FE	yes	yes	-	-
Month*Industry FE	yes	-	-	-
Month*Industry*Eligibility FE	no	yes	yes	yes
Month*Industry*Municipality FE	no	no	yes	-
Month*Industry*Municipality*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	7,745,356	7,745,356	7,745,356	7,745,356
Adjusted R^2	0.021	0.022	0.052	0.128

This table reports changes in unemployment inflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months in a given month. 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 or seven months, 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 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% and 5% level, respectively.

Table 8: **Unemployment Outflow by Informality**

	I	II	III	IV
Dep. Var.: $P[e \leq 5]_{it}$	Industry-Level			
$6Months_{it}$	-0.0858***			
	(0.0166)			
$6Months_{it} * Reform_t$	0.0430***			
	(0.0095)			
$6Months_{it} * Informal$	-0.0086	-0.0212	-0.0446	-0.0651
	(0.0689)	(0.0653)	(0.0532)	(0.0495)
$6Months_{it} * Reform_t * Informal$	0.0378	0.0604**	0.0751***	0.0720***
	(0.0313)	(0.0303)	(0.0219)	(0.0204)
Industry*Month FE	yes	yes	-	-
Month*Municipality FE	yes	-	-	-
Month*Municipality*Eligibility FE	no	yes	yes	yes
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	389,753	389,753	389,753	389,753
R^2	0.018	0.019	0.029	0.100
Dep. Var.: $P[e \leq 5]_{it}$	Municipality-Level			
$6Months_{it}$	-0.0912***			
	(0.0250)			
$6Months_{it} * Reform_t$	0.0421*			
	(0.0241)			
$6Months_{it} * Informal$	0.0156	0.0089	0.0306	0.1074
	(0.1150)	(0.1198)	(0.1246)	(0.1210)
$6Months_{it} * Reform_t * Informal$	0.0526	0.0589	0.0463	0.0242
	(0.1168)	(0.1135)	(0.1099)	(0.1025)
Municipality*Month FE	yes	yes	-	-
Month*Industry FE	yes	-	-	-
Month*Industry*Eligibility FE	no	yes	yes	yes
Month*Industry*Municipality FE	no	no	yes	-
Month*Industry*Municipality*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	389,753	389,753	389,753	389,753
Adjusted R^2	0.018	0.019	0.029	0.100

This table reports changes in unemployment outflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i enters formal employment within five months after being laid off and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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% level, respectively.

Table 9: **Collusion by Informality**

	I	II	III	IV
Dep. Var.: $P_{same}[4-9]_{it}$	Industry-Level			
$6Months_{it}$	-0.0061			
	(0.0043)			
$6Months_{it} * Reform_t$	0.0005			
	(0.0065)			
$6Months_{it} * Informal$	0.0995***	0.0996***	0.0708***	0.0745***
	(0.0169)	(0.0152)	(0.0155)	(0.0193)
$6Months_{it} * Reform_t * Informal$	-0.0633***	-0.0664***	-0.0502**	-0.0607**
	(0.0244)	(0.0242)	(0.0218)	(0.0237)
Industry*Month FE	yes	yes	-	-
Month*Municipality FE	yes	-	-	-
Month*Municipality*Eligibility FE	no	yes	yes	yes
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	214,643	214,643	214,643	214,643
R^2	0.174	0.215	0.353	0.494
Dep. Var.: $P_{same}[4-9]_{it}$	Municipality-Level			
$6Months_{it}$	0.0042			
	(0.0053)			
$6Months_{it} * Reform_t$	0.0004			
	(0.0067)			
$6Months_{it} * Informal$	0.0697***	0.0453**	0.0551**	0.0402
	(0.0218)	(0.0210)	(0.0205)	(0.0239)
$6Months_{it} * Reform_t * Informal$	-0.0728***	0.0685***	0.0810***	0.0808**
	(0.0278)	(0.0269)	(0.0290)	(0.0359)
Municipality*Month FE	yes	yes	-	-
Month*Industry FE	yes	-	-	-
Month*Industry*Eligibility FE	no	yes	yes	yes
Month*Industry*Municipality FE	no	no	yes	-
Month*Industry*Municipality*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	214,643	214,643	214,643	214,643
Adjusted R^2	0.215	0.216	0.328	0.476

This table reports changes in reemployment of workers by the same firm after the exhaustion of UI benefits around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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% and 5% level, respectively.

Table 10: **Formal Hiring, Employment, and Wages by Informality**

	I	II	III	IV	V	VI
Panel A:	Industry-Level					
Dep. Var.:	$Hired/Employed\ Workers$		$log(Workers)$		$log(Wage)$	
$Affected_{it}$	-0.0004***	-0.0017***	0.9052***	1.0045***	-0.0781***	-0.0986***
	(0.0002)	(0.0002)	(0.0088)	(0.080)	(0.0014)	(0.0014)
$Affected_{it} * Reform_t$	-0.0009***	-0.0014***	-0.0381***	-0.0469***	0.0021	0.0025
	(0.0003)	(0.0004)	(0.0074)	(0.0059)	(0.0015)	(0.0015)
$Affected_{it} * Informal$	0.0064***	0.0102***	-0.8236***	-0.8411***	-0.0761***	-0.0547***
	(0.0008)	(0.0008)	(0.0259)	(0.0211)	(0.0041)	(0.0042)
$Affected_{it} * Informal * Reform_t$	-0.0112***	-0.0115***	-0.0948***	-0.0694***	0.0124**	0.0129**
	(0.0012)	(0.0013)	(0.0274)	(0.0213)	(0.0050)	(0.0051)
Month FE	yes	-	yes	-	yes	-
Industry FE	yes	-	yes	-	yes	-
Municipality FE	yes	-	yes	-	yes	-
Industry-Municipality-Month FE	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
R^2	0.054	0.346	0.721	0.956	0.414	0.526
Panel B:	Municipality-Level					
Dep. Var.:	$Hired/Employed\ Workers$		$log(Workers)$		$log(Wage)$	
$Affected_{it}$	0.0013***	0.0022***	1.3988***	1.6950***	-0.0703***	-0.0680***
	(0.0004)	(0.0004)	(0.0180)	(0.0169)	(0.0032)	(0.0035)
$Affected_{it} * Reform_t$	0.0021***	0.0008*	-0.0394***	-0.0442***	-0.0258***	-0.0216***
	(0.0006)	(0.0007)	(0.0144)	(0.0123)	(0.0031)	(0.0036)
$Affected_{it} * Informal$	-0.0001	-0.0014**	-1.1439***	-1.4489***	-0.0422***	-0.0474***
	(0.0006)	(0.0006)	(0.0272)	(0.0256)	(0.0048)	(0.0053)
$Affected_{it} * Informal * Reform_t$	-0.0098***	-0.0084***	-0.0407*	-0.0370**	0.0477***	0.0441***
	(0.0010)	(0.0010)	(0.0216)	(0.0180)	(0.0045)	(0.0051)
Month FE	yes	-	yes	-	yes	-
Industry FE	yes	-	yes	-	yes	-
Municipality FE	yes	-	yes	-	yes	-
Industry-Municipality-Month FE	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
R^2	0.054	0.349	0.722	0.959	0.414	0.566

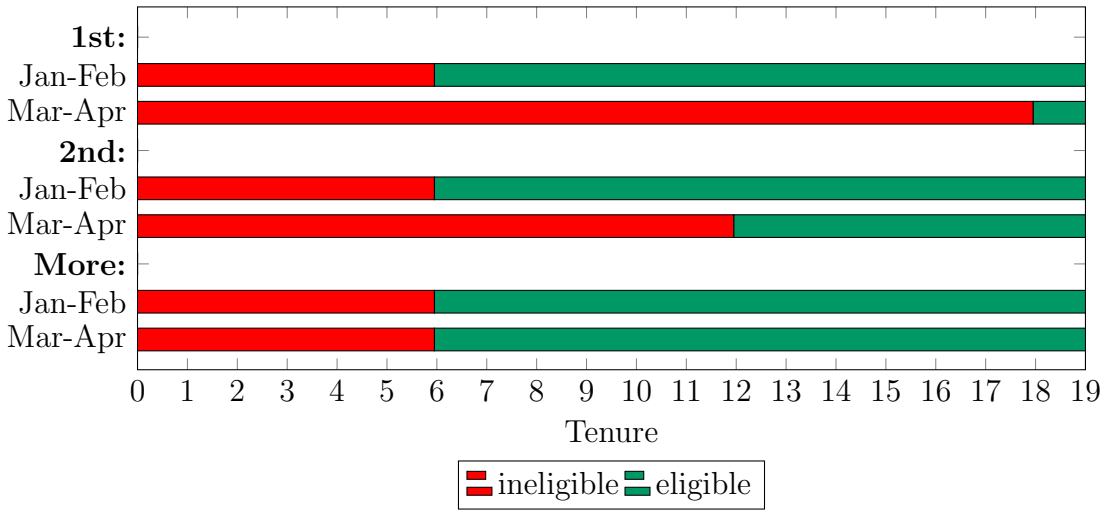
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired 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. 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 Panel A and a given municipality in Panel B. Bootstraped standard errors are reported in parentheses. *** and ** denote statistical significance at the 1% and 5% level, respectively.

Table 11: **Formal and Informal Wages**

	I	II	III	IV	V
Dep. Var.:	$log(Wage)_{it}$				
$Formal\ Job_{it} * Post_t$	0.0333***	0.0229***	0.0245***	0.0265**	0.0146
	(0.0062)	(0.0051)	(0.0051)	(0.0115)	(0.0097)
$Formal\ Job_{it} * Informal$				0.5700***	0.4236***
				(0.1028)	(0.0518)
$Formal\ Job_{it} * Reform_t * Informal$				0.0213	0.0293
				(0.0218)	(0.0197)
Formal Job FE	yes	yes	yes	yes	yes
Industry-Quarter FE	yes	no	-	-	-
Municipality-Quarter FE	no	yes	-	-	-
Municipality-Industy-Quarter FE	no	no	yes	yes	yes
Clustered SE	muni	muni	muni	muni	muni
Observations	1,593,043	1,593,043	1,593,043	1,593,043	1,593,043
Adjusted R^2	0.254	0.273	0.387	0.258	0.389

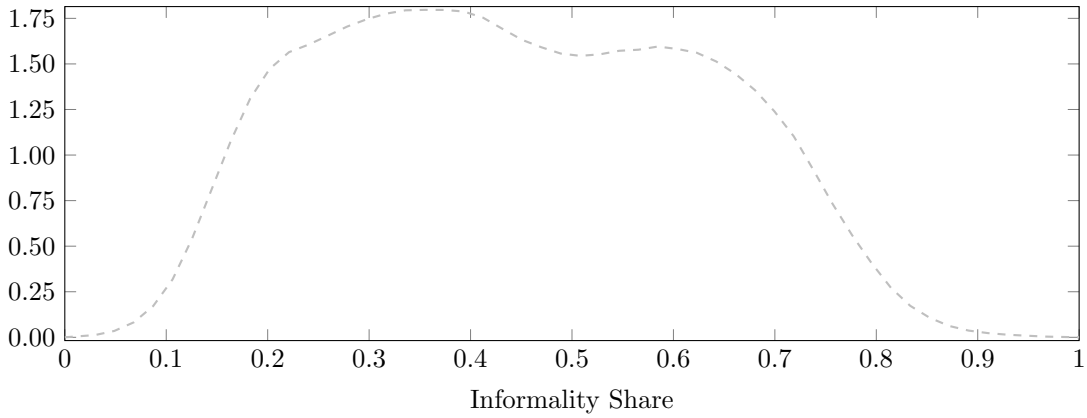
This table reports changes in formal and informal wages around the announcement of the UI reform from July 2013 to June 2016. The dependent variable is the log of worker i's wage in quarter t. The dummy variable $Reform_t$ takes the value of one for the post-announcement period from January 2015 to June 2016, and zero for the pre-announcement period from July 2013 to December 2014. The dummy variable $Formal\ Job_{it}$ takes the value of month if worker i is employed formally in quarter t, and zero if she is employed informally. The variable $Informal$ is the share of informal employment in a given industry (municipality). 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% level, respectively.

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



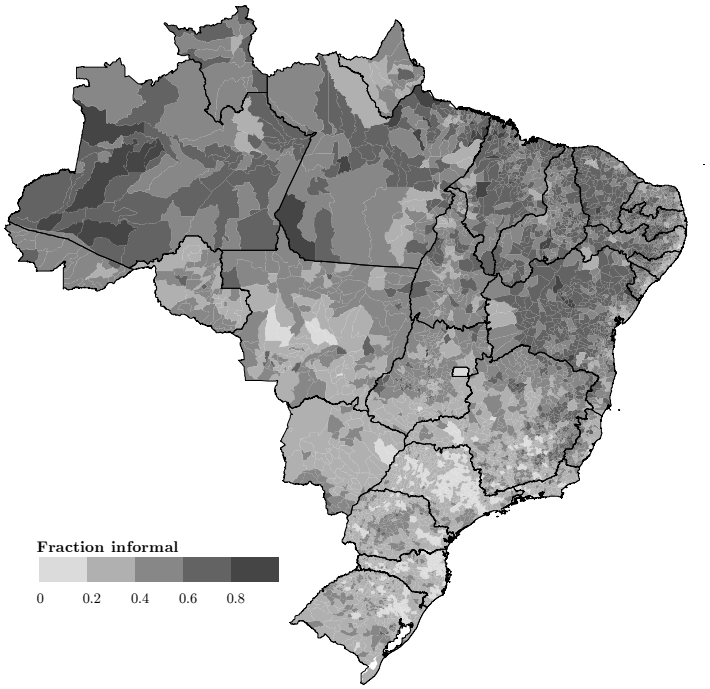
This figure illustrates eligibility creteria for UI benefits before and after the reform for workers with different tenure that apply for UI benefits for the first time, the second time, and the third time or more. Red areas indicate tenure not satisfying eligibility criteria, green areas indicate tenure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

Figure 2: **Distribution of Labor Market Informality across Municipalities**



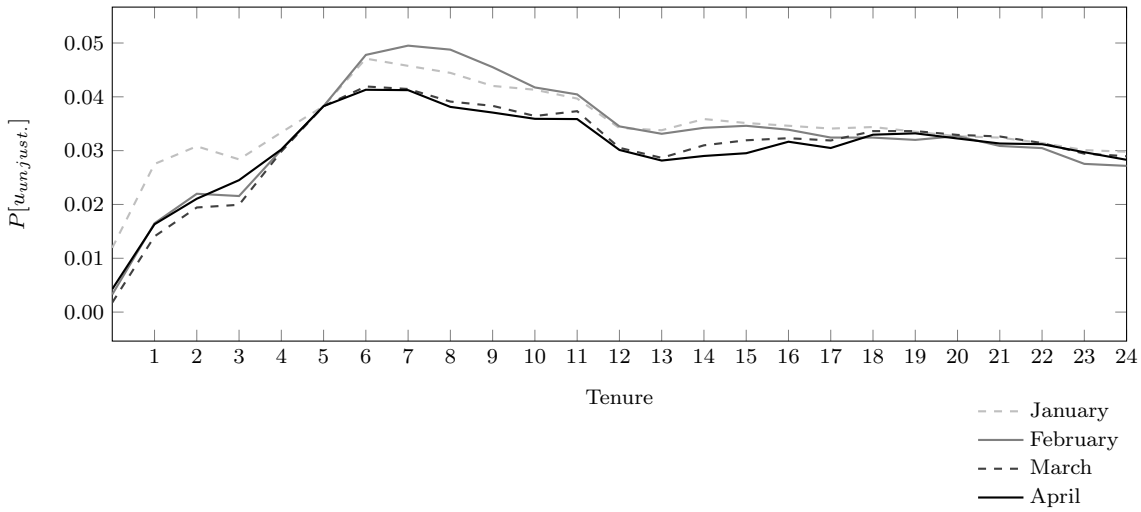
This figure depicts the distribution of the share of informal in total workers across all municipalities in Brazil from the Census Brazil.

Figure 3: **Labor Market Informality by Municipality**



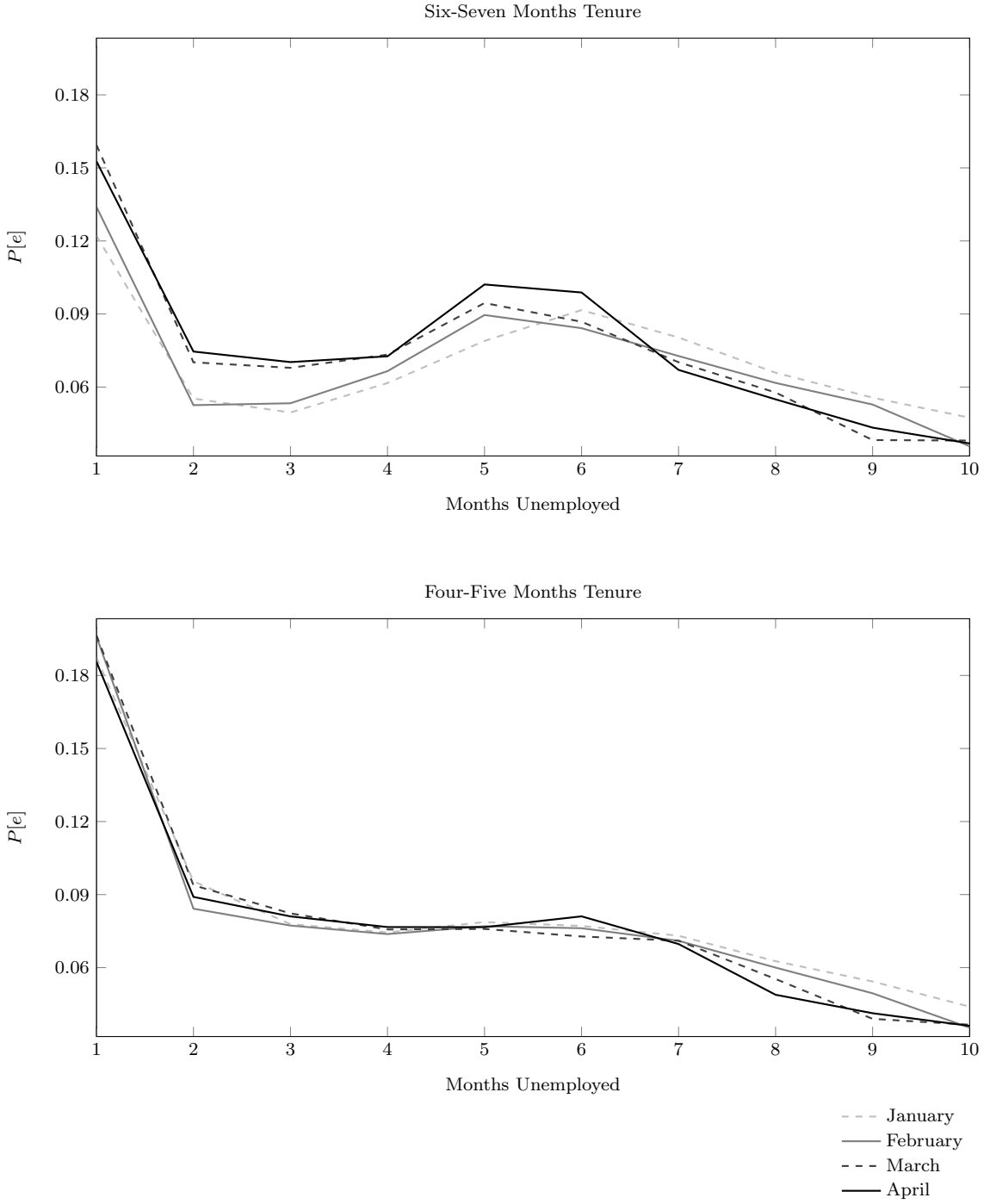
This figure depicts the share of informal in total workers for all municipalities in Brazil from the Census Brazil.

Figure 4: **Unemployment Inflow by Tenure**



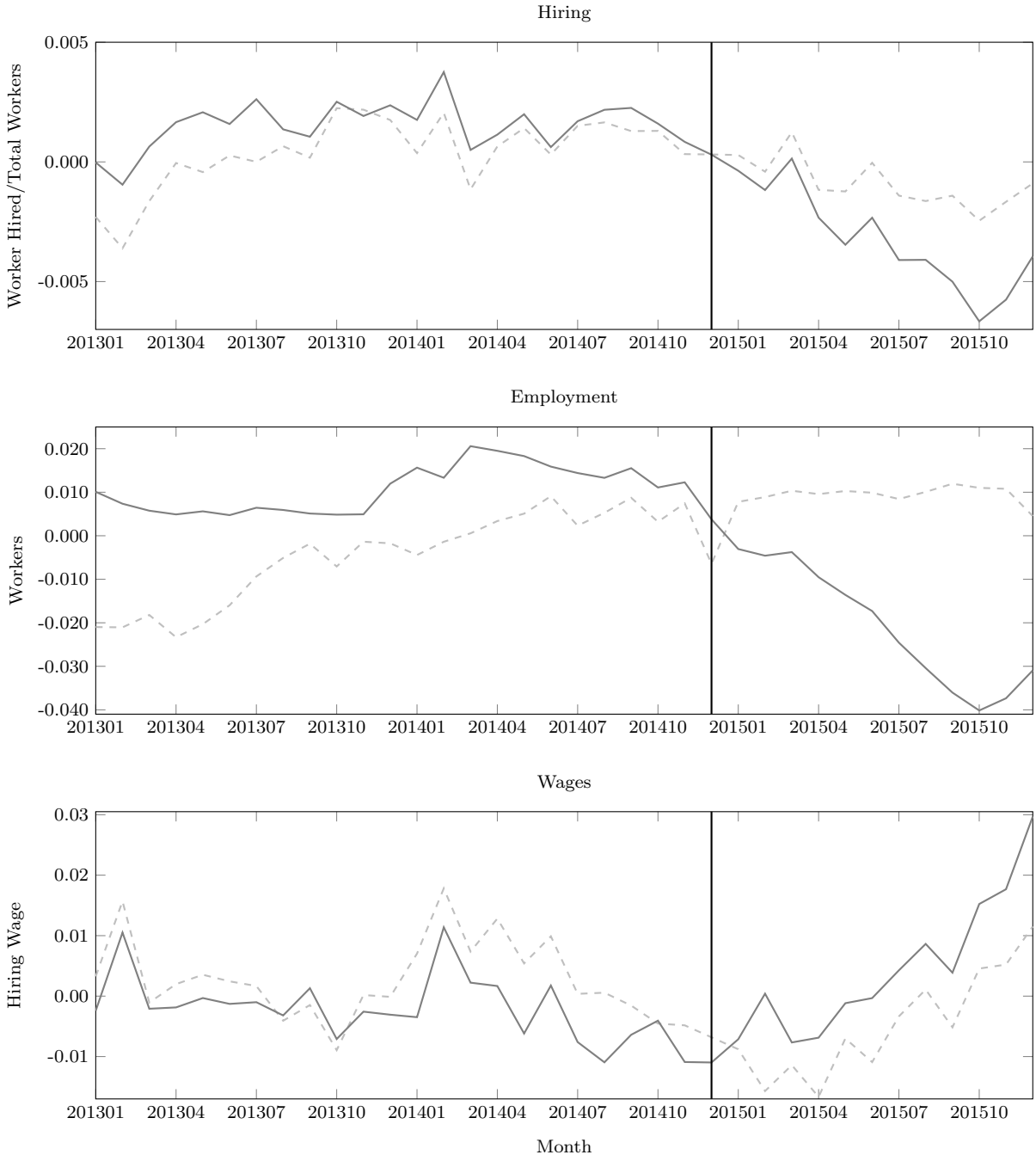
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are vertically aligned at the April probability of layoff for workers with a tenure of five months.

Figure 5: **Unemployment Outflow by Tenure**



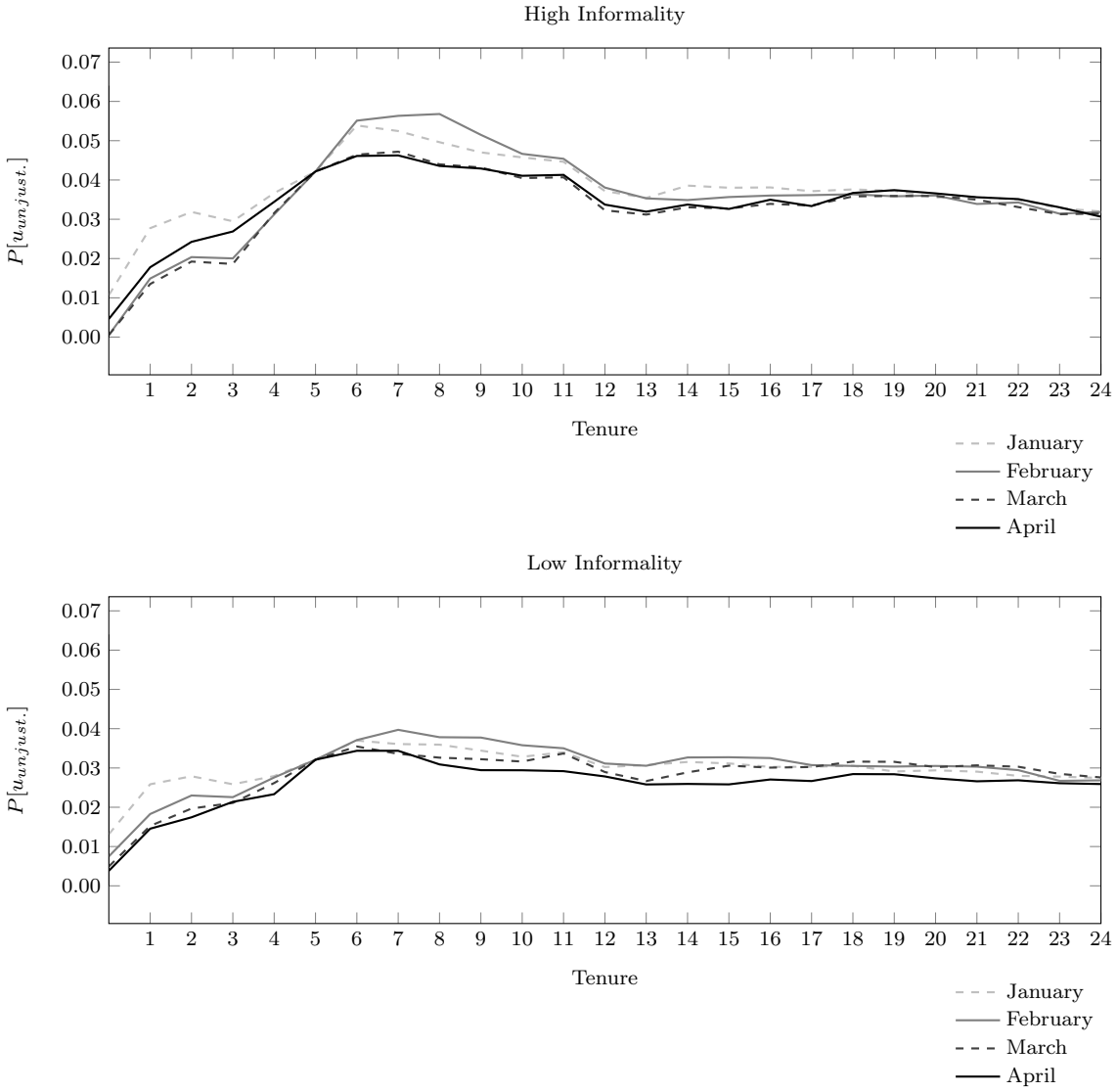
This figure depicts the probability of reemployment in the months after losing their job for workers with a tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2015.

Figure 6: **Formal Hiring, Employment, and Wages**



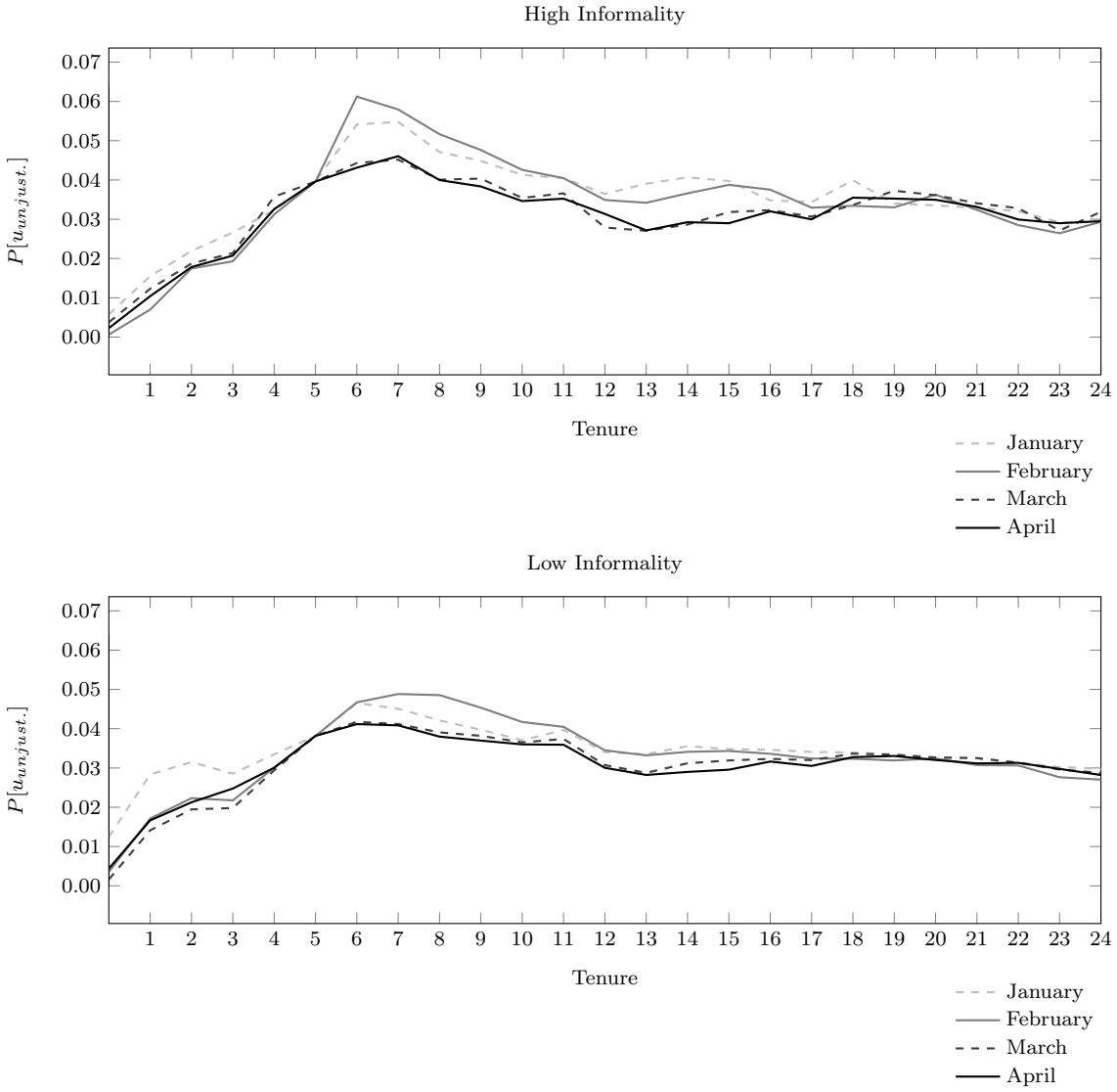
This figure depicts time-series changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the average across all industry-municipality level observations in a given month for workers with two or less 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 middle panel, the y-axis depicts the log of total employment, in the bottom panel, the y-axis reports the log of the average wage of hired workers. The plots are adjusted for calender month fixed effects and the average value of the y-axis variable over the full sample period for each group.

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



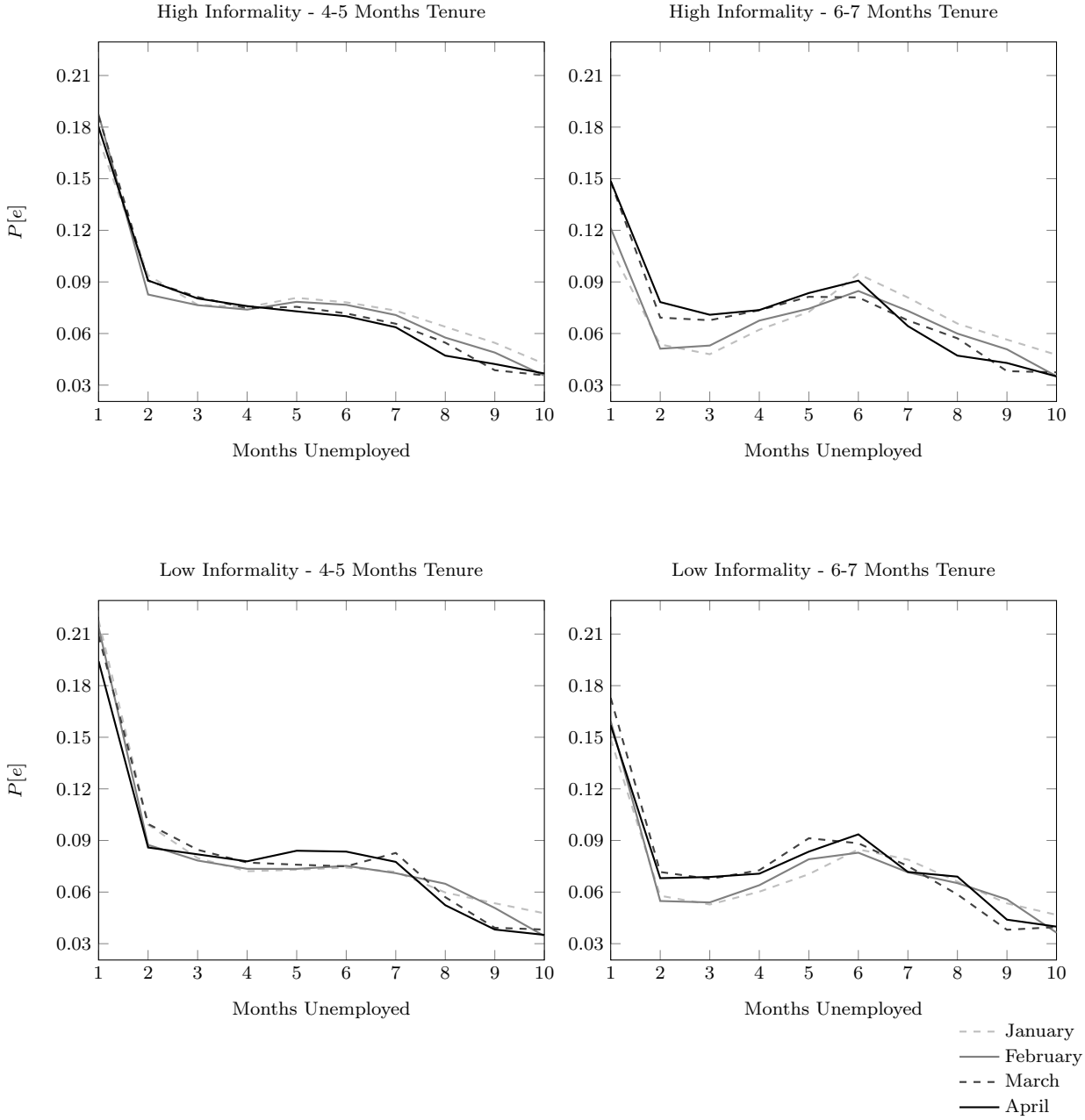
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in 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 8: **Unemployment Inflow by Informality - Municipality Level**



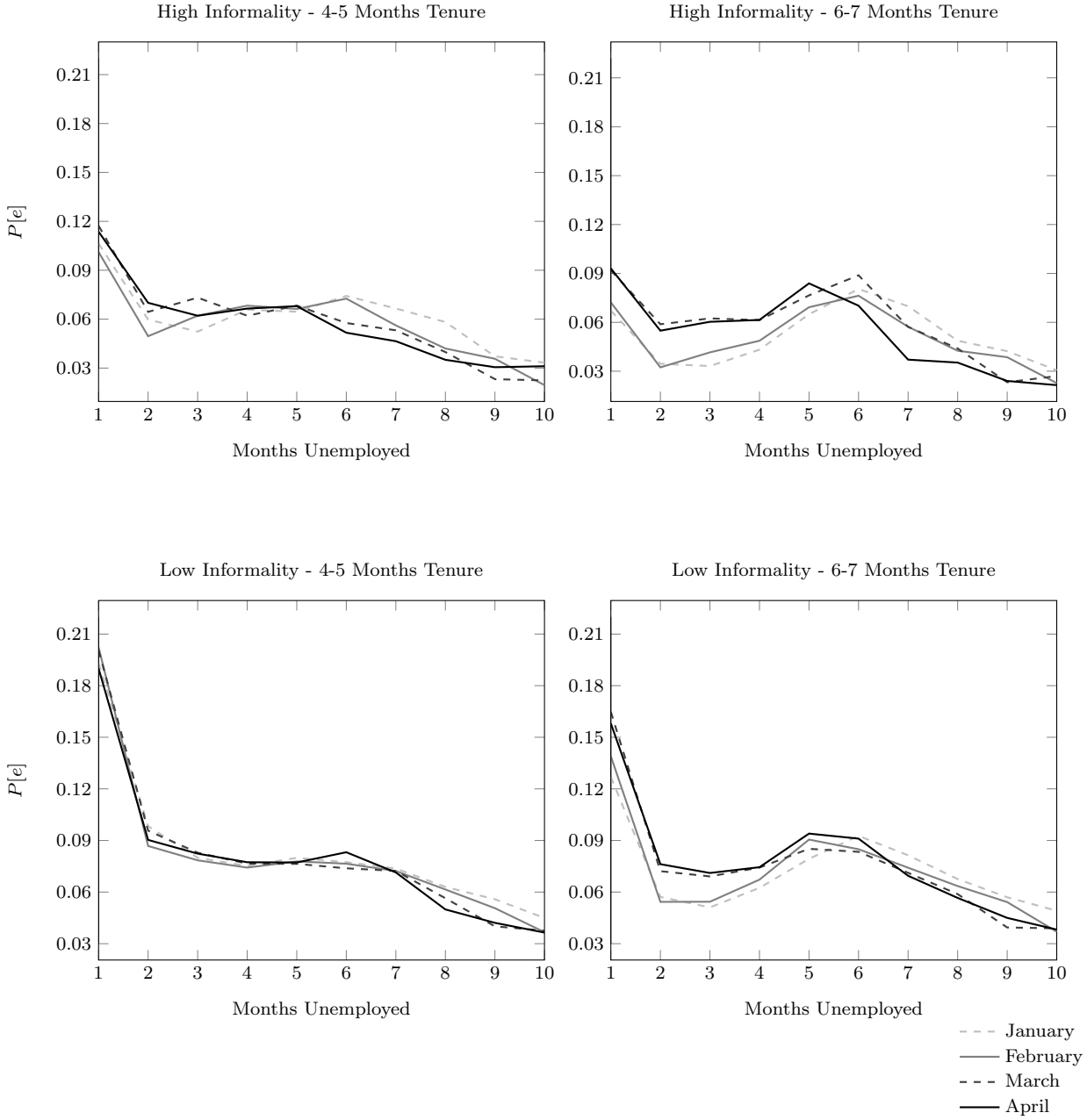
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in 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 9: **Unemployment Outflow by Informality - Industry Level**



This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (right panels) and four or five months (left panels) at layoff separately for the months from January to April 2015. The sample is restricted to workers in industries with above median levels of labor market informality in the top panels, and workers in industry with below median levels of labor market informality in the bottom panels.

Figure 10: **Unemployment Outflow by Informality - Municipality Level**



This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (right panels) and four or five months (left panels) at layoff, separately for the months from January to April 2015. The sample is restricted to workers in municipalities with above median levels of labor market informality in the top panels, and workers in municipalities with below median levels of labor market informality in the bottom panels.

Appendix A. Additional Figures and Tables

Table A.1: **Unemployment Inflow - Substitution**

Dep. Var.: $P[u_{other}]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0075***	-0.0078***	-0.0073***	-0.0067***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)
$6Months_{it} * Reform_t$	0.0003	-0.0006	-0.0003	-0.0006
	(0.0009)	(0.0006)	(0.0005)	(0.0005)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	7,745,356	7,745,356	7,745,356	7,745,356
R^2	0.000	0.021	0.048	0.112

This table reports changes in voluntary unemployment inflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months in a given month. The dependent variable is a dummy variable that takes the value of one if worker i quits her job in month t, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, 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 to April 2015, and zero for the pre-reform period from January to February 2015. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.2: **Formal Employment Share**

	I	II	III	IV
Dep. Var.:	$Formal_{it}$			
$Reform_t$	-0.0085***	-0.0066***	-0.0087***	-0.0072***
	(0.0026)	(0.0021)	(0.0026)	(0.0021)
Industry FE	no	yes	no	yes
Municipality FE	no	no	yes	yes
Clustered SE	muni	muni	muni	muni
Observations	1,706,817	1,706,817	1,706,817	1,706,817
R^2	0.105	0.197	0.161	0.233

This table reports changes in formality around the announcement of the UI reform in January 2015. The dependent variable is a dummy variable if a worker is formally employed and zero if a worker is informally employed. 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 reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.3: **Unemployment Inflow - Cyclicality**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0128***	0.0124***	0.0124***	0.0127***
	(0.0006)	(0.0006)	(0.0005)	(0.0005)
$6Months_{it} * 2015_t$	-0.0002	-0.0002	-0.0003	-0.0004
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
$6Months_{it} * Reform_t$	0.0005	0.0005	0.0006*	0.0007**
	(0.0005)	(0.0004)	(0.0003)	(0.0003)
$6Months_{it} * 2015_t * Reform_t$	-0.0060***	-0.0060***	-0.0060***	-0.0058***
	(0.0009)	(0.0007)	(0.0006)	(0.0006)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	- -
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	16,039,805	16,039,805	16,039,805	16,039,805
Adjusted R^2	0.001	0.015	0.051	0.126

This table compares changes in unemployment inflow around the enactment of the UI benefits reform from January to April 2015 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months in a given month. 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 or seven months, 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. 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, respectively.

Table A.4: **Unemployment Outflow - Cyclicality**

Dep. Var.: $P[e \leq 5]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0947***	-0.0950***	-0.0962***	-0.0964***
	(0.0029)	(0.0027)	(0.0028)	(0.0027)
$6Months_{it} * 2015_t$	0.0075*	0.0091**	0.0101***	0.0117***
	(0.0043)	(0.0038)	(0.0036)	(0.0036)
$6Months_{it} * Reform_t$	0.0187***	0.0182***	0.0218***	0.0226***
	(0.0041)	(0.0042)	(0.0046)	(0.0049)
$6Months_{it} * 2015_t * Reform_t$	0.0328***	0.0324***	0.0301***	0.0273***
	(0.0034)	(0.0032)	(0.0030)	(0.0032)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	- -
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	813,917	813,917	813,917	813,917
Adjusted R^2	0.007	0.014	0.029	0.098

This table compares changes in unemployment outflow around the enactment of the UI benefits reform from January to April 2015 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i returns to formal employment within five months of layoff, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, 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. 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% level, respectively.

Table A.5: **Collusion - Cyclicality**

Dep. Var.: $P_{same}[4-9]_{it}$	I	II	III	IV
$6Months_{it}$	0.0271***	0.0180***	0.0159***	0.0159***
	(0.0050)	(0.0020)	(0.0017)	(0.0020)
$6Months_{it} * 2015_t$	0.0033	0.0028	0.0033	0.0024
	(0.0034)	(0.0021)	(0.0021)	(0.0023)
$6Months_{it} * Reform_t$	-0.0039	-0.0046**	-0.0022	-0.0020
	(0.0085)	(0.0022)	(0.0022)	(0.0025)
$6Months_{it} * 2015_t * Reform_t$	0.0121*	0.0123***	0.0134***	0.0130***
	(0.0071)	(0.0029)	(0.0030)	(0.0035)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	- -
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	482,365	482,365	482,365	482,365
Adjusted R^2	0.003	0.205	0.322	0.467

This table compares changes in reemployment of workers by the same firm after the exhaustion of UI benefits around the enactment of the UI reform from January to April 2015 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months at layoff that are rehired within nine months. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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. 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% level, respectively.

Table A.6: **Unemployment Inflow - Unaffected Workers**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0122***	0.0107***	0.0098***	0.0094***
	(0.0011)	(0.0007)	(0.0006)	(0.0006)
$6Months_{it} * Reform_t$	-0.0010	-0.0016	-0.0014	-0.0012
	(0.0017)	(0.0011)	(0.0009)	(0.0010)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	1,919,863	1,919,863	1,919,863	1,919,863
R^2	0.001	0.042	0.099	0.209

This table reports changes in unemployment inflow around the enactment of the UI reform from January to April 2015 for workers who receive UI benefits for at least three times in the past. The sample is limited to workers with tenure of four to seven months in a given month. 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 or seven months, 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 to April 2015 and zero for the pre-reform period from January to February 2015. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.7: **Unemployment Outflow - Unaffected Workers**

Dep. Var.: $P[e \leq 5]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0723***	-0.0742***	-0.0728***	-0.0771***
	(0.0031)	(0.0028)	(0.0025)	(0.0029)
$6Months_{it} * Reform_t$	0.0033	0.0044	0.0042	0.0070
	(0.0083)	(0.0086)	(0.0079)	(0.0069)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	80,189	80,189	80,189	80,189
R^2	0.008	0.016	0.045	0.168

This table reports changes in unemployment outflow around the enactment of the UI reform from January to April 2015 for workers who receive UI benefits for at least three times in the past. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i enters formal employment within five months after being laid off and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.8: **Collusion - Unaffected Workers**

Dep. Var.: $P_{same}[4-9]_{it}$	I	II	III	IV
$6Months_{it}$	0.0565***	0.0205***	0.0162***	0.0160***
	(0.0133)	(0.0038)	(0.0034)	(0.0042)
$6Months_{it} * Reform_t$	0.0039	-0.0074	-0.0033	-0.0052
	(0.0028)	(0.0050)	(0.0050)	(0.0059)
Month*Municipality FE	yes	-	-	yes
Month*Municipality FE	no	yes	-	yes
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	67,181	67,181	67,181	67,181
R^2	0.022	0.416	0.521	0.611

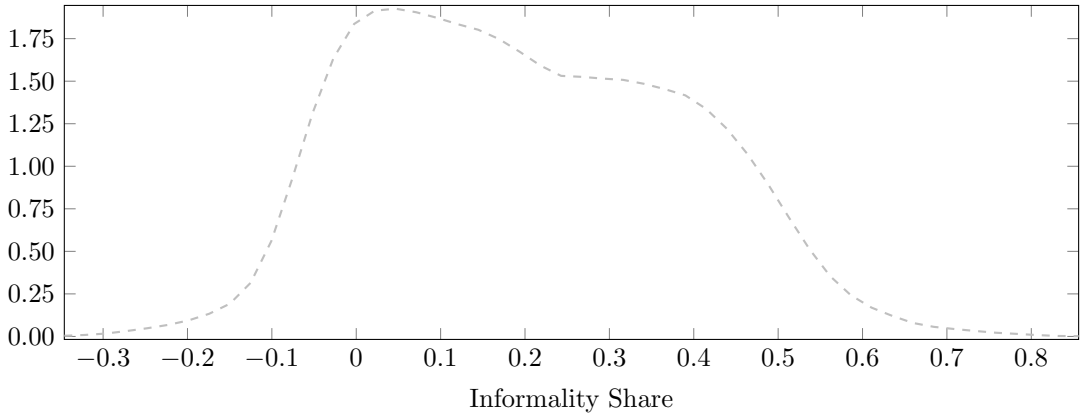
This table reports changes in reemployment of workers by the same firm after the exhaustion of UI benefits around the enactment of the UI reform from January to April 2015 for workers who receive UI benefits for at least three times in the past. The sample is limited to workers with tenure of four to seven months at layoff that are rehired within nine months in columns I-III and within four to nine months in columns IV-VI. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. 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. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.9: **Unemployment Inflow - Announcement Effects**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0126***	0.0123***	0.0127***	0.0127***
	(0.0007)	(0.0006)	(0.0005)	(0.0005)
$6Months_{it} * Reform_t$	-0.0055***	-0.0056***	-0.0060***	-0.0055***
	(0.0008)	(0.0007)	(0.0006)	(0.0006)
Month FE	yes	-	-	-
Month*Municipality FE	no	yes	-	-
Month*Municipality*Industry FE	no	no	yes	-
Month*Municipality*Industry*Occupation FE	no	no	no	yes
Clustered SE	muni	muni	muni	muni
Observations	7,939,811	7,939,811	7,939,811	7,939,811
R^2	0.001	0.016	0.062	0.142

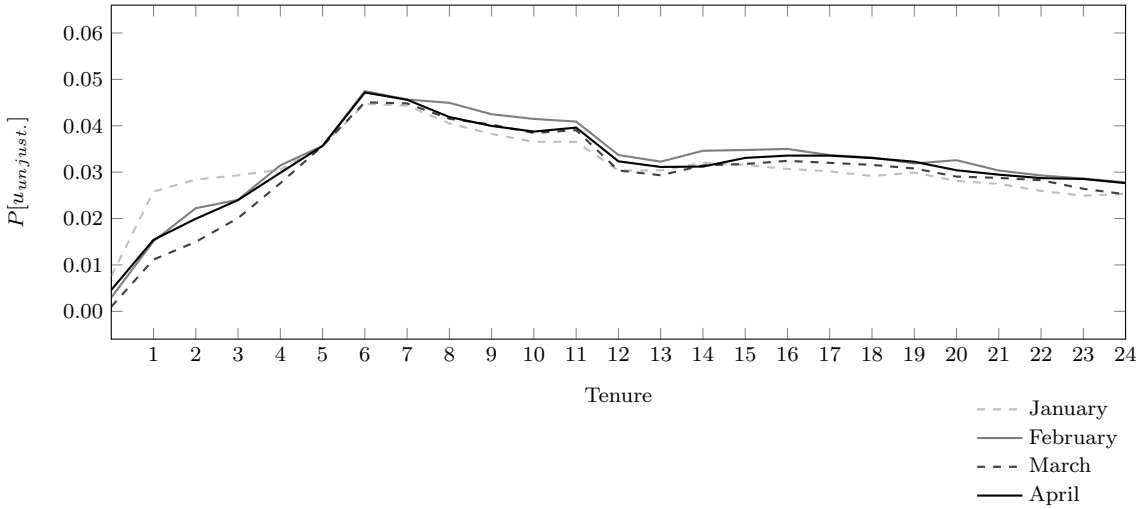
This table reports changes in unemployment inflow around the announcement of the UI reform. The sample is limited to workers with tenure of four to seven months in a given month. The dependent variable is a dummy variable that takes the value of one if worker i is laid off in month t, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, 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 to April 2015 and zero for the pre-announcement period from November 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.

Figure A.1: **Distribution of Labor Market Informality across Municipalities - Industry Control**



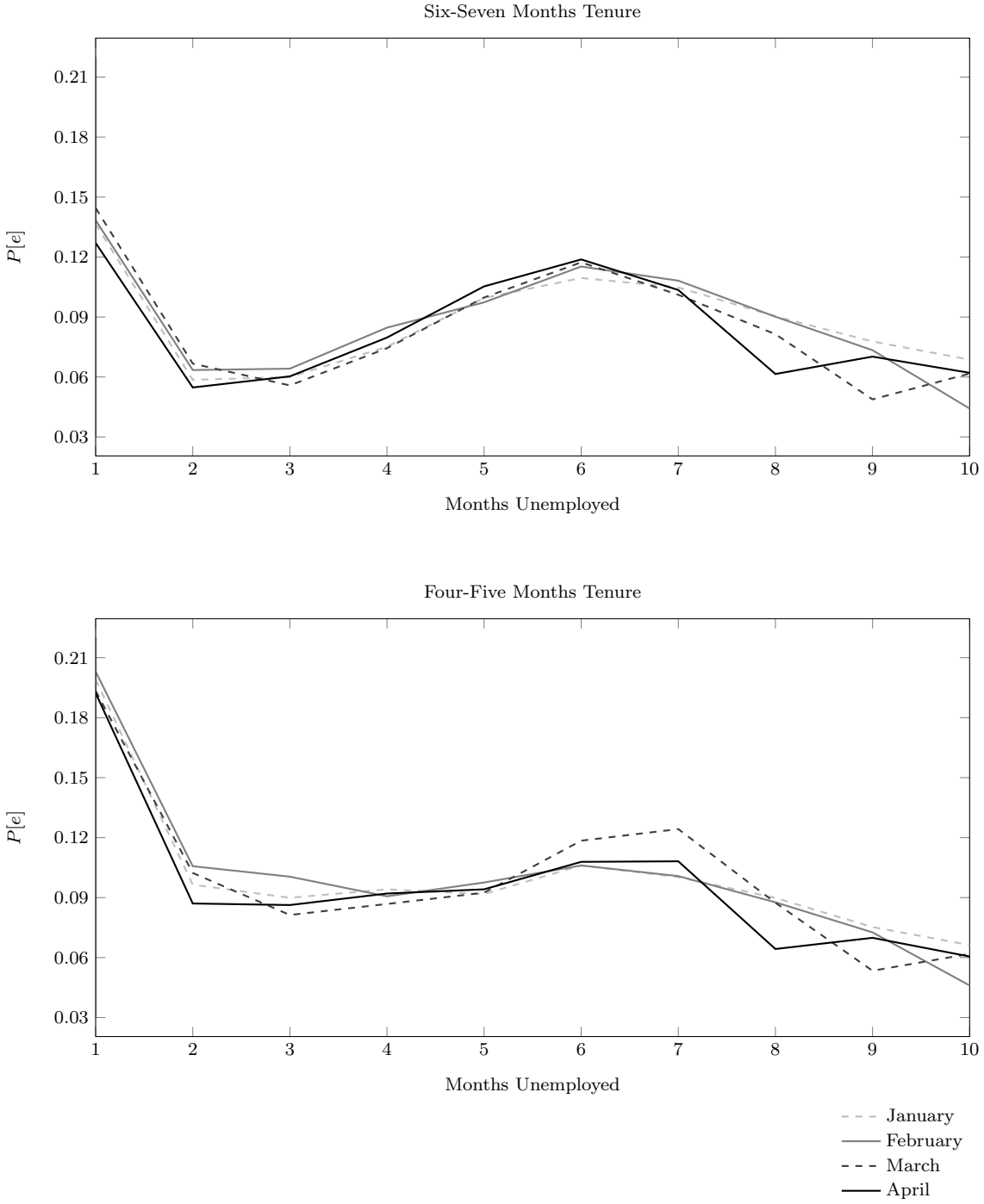
This figure depicts the distribution of difference in the share of informal in total workers and the same share as predicted from the industry composition of the respective municipality across all municipalities in Brazil.

Figure A.2: **Unemployment Inflow by Tenure - Previous Year**



This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2014, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months.

Figure A.3: **Unemployment Outflow by Tenure - Previous Year**



This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2014.