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

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Abstract

Exploiting a discontinuous effect of an unemployment insurance (UI) reform in Brazil, this paper documents layoff and rehiring patterns consistent with collusion between firms and workers to extract rents from the UI system. Firms and workers time formal unemployment spells to coincide with workers' eligibility for UI benefits. These patterns are mostly driven by industries and municipalities with large informal labor markets. Using survey data, we find that workers are more likely to flow from formal to informal unemployment when they are eligible for UI benefits, and return from informal to formal employment to the same firm when UI benefits eligibility ends. Combined with a lower probability of hiring replacement workers when laying off workers eligible for UI benefits, this suggests that firms continue employing workers informally while they are on benefits. Firms seem to benefit from collusion through 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 (Rothstein and Valletta 2017). To dampen the adverse effects of job loss on household incomes, government-mandated unemployment insurance (UI) programs have been in place in developed countries for decades, and are increasingly implemented in mid-income and developing countries. Compared to the large literature on the adverse incentive effects of UI on labor supply in developed countries,[1] we understand little about how labor market characteristics in mid-income and developing countries, for example the presence of large informal labor markets, interact with the incentive effects of UI. Understanding how features of UI design affect workers' and firms' incentives in mid-income and developing countries has important policy implications, especially given the spread of unemployment insurance programs to those countries (Holzmann et al. 2011; ILO 2017).

In this paper, we exploit a sudden and unanticipated UI reform in Brazil in 2015, to explore novel unemployment inflow and outflow patterns generated by UI in labor markets with high degrees of informality. As a heterogenous mid-income country with ample cross-sectional variation in labor market characteristics, Brazil constitutes an ideal laboratory for our empirical analysis. We find strong evidence that formal unemployment inflow and outflow are timed to coincide with workers' eligibility for UI benefits. Firms lay off workers when they become eligible for benefits, and rehire them just when their eligibility for benefits ends. Firms seem to benefit from colluding with workers to time unemployment spells to coincide with UI benefits eligibility through lower equilibrium wages. These patterns occur primarily in industries and municipalities with a high degree of labor market informality, where firms are also less likely to hire a replacement worker when laying off a worker who is eligible for UI benefits. Survey evidence suggests that firms continue to employ workers informally while they receive UI benefits and rehire them formally when benefits cease.

The UI reform was announced on December 29, 2014 and implemented as a provisional measure on March 1, 2015. The announcement came as a surprise *after* affected workers had entered formal employment, which allows us to examine the effects of UI benefits on layoff

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

intensities free from workers' selection into jobs with different expected employment duration (Green and Riddell 1997). The nature of the reform provides a sharp discontinuity in the loss of eligibility for UI benefits. Prior to the reform, workers with an employment history of six consecutive months are eligible for UI benefits. After the reform, workers applying for benefits for the first (second) time require formal employment for 18 (12) months during the previous 24 (16) months to be eligible for benefits. Thus, a subset of workers with tenure between six and 18 (12) months lose eligibility for UI benefits after the reform, whereas workers with tenure below six months are never eligible for benefits. This discontinuity motivates our main identification strategy, a difference-in-differences methodology, in which we compare changes in employment and unemployment patterns before and after the reform for workers with tenure just above and just below the six-month threshold.

We start our analysis by examining how UI affects layoff intensities and reemployment. Our findings indicate that UI eligibility has strong effects on unemployment inflow. Specifically, unemployment inflow drops by twelve percent relatively for workers just above the six-month threshold who lose eligibility for UI benefits after the reform. We next assess how this pattern varies across different labor markets. Specifically, we examine whether the timing of UI inflow according to workers' eligibility for UI benefits is stronger in labor markets with higher degrees of labor market informality. Informal labor markets provide a unique alternative to formal employment in the light of UI. Workers are able to receive benefits while continuing to be employed informally. We find that thigher layoff intensities for workers eligible for UI benefits strongly correlate with the presence of informal labor markets. The drop in unemployment inflow after the reform is significantly stronger for workers in industries and municipalities with large informal labor markets.[2] 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 0.2 percentage point higher inflow into formal unemployment when workers are eligible for benefits.

Higher layoff intensities for individuals that qualify for UI could be induced by workers that may be less willing to exert effort when their outside option is to receive UI benefits. Alternatively, it could be optimal for firms that face labor demand fluctuations to (temporarily) lay off workers on benefits, anticipating that workers on benefits are less likely to search for alternative jobs (Katz 1986; Jurajda 2002). Additionally, the theoretical literature provides two rationales for implicit agreements between firms and workers leading to

[2]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 thresholds in Western European countries.

higher layoff intensities when workers are eligible for benefits. Models of implicit contracting (Feldstein 1976; Baily 1977) imply that firms may collude with workers to formally lay them off when they are eligible for UI benefits, to extract rents from the UI system, which they can share through lower equilibrium wages. Similarly, Christofides and McKenna (1996) develop a model in which firms are more likely to lay off workers when they are eligible for UI benefits for reputational effects in local labor markets eliciting higher labor supply. While the motivation for collusion differs slightly between these models, they all imply that firms time unemployment spells according to workers' eligibility for UI, to extract rents from the system.

To assess the presence of strategic behavior on the part of firms, we examine layoff and rehiring patterns consistent with collusion between firms and workers. We observe an extreme form of unemployment timing. Before the reform, workers who are laid off with a tenure of six months, just when they become eligible for UI benefits, are significantly more likely to be rehired by their previous employer precisely when their benefits cease. After the reform, when workers with a tenure of six months lose eligibility for UI benefits, this pattern vanishes. This precise timing of unemployment spells according to UI benefits eligibility explains about twenty percent of the higher layoff intensities at the eligibility threshold and is more pervasive when the potential rents that workers and firms can extract from the UI system are higher. A ten percentage point increase in the ratio between total benefits payments and the cost of formal employment and layoff leads to an eighteen percent increase in the timing of unemployment spells with UI benefits eligibility.[3]

While these results are consistent with collusion between firms and worker, temporary layoffs in response to demand fluctuations are also consistent with the documented patterns. To separate firm-worker collusion from demand fluctuations we exploit a quirk of the pre-reform UI system. A worker is only allowed to apply for UI benefits sixteen months after the last successful application. Thus, for a firm that only employs a worker formally to establish eligibility for UI benefits there is an incentive to hire the worker formally nine months after the previous layoff, employ her for six months and apply for UI benefits sixteen months after the previous UI benefits application. We find that there is a spike in formal reemployment by the same firm nine to ten months after layoff, whereas employment by a different firm is flat between six to twelve months after layoff. This reemployment pattern is consistent with firm-worker collusion, whereas temporary layoffs due to demand fluctuations do not imply different rehiring patterns after nine to ten months.

[3]Back of the envelope calculations suggest that the additional UI benefits payments due to strategic formal unemployment amount to 0.06 percent of GDP.

Informal labor markets they allow firms to hold on to workers while they are on benefits.[4] We provide indirect evidence that firms continue to employ workers informally while they receive UI benefits. Using survey data, we find that workers with a tenure of six months are about six percentage points more likely to enter informal employment upon layoff compared to workers with tenure of five months at layoff before the reform. Additionally, workers are more than ten percentage points more likely to work informally and return from informal to formal employment with the same firm, if they were laid off with a tenure of six months compared to workers laid off with a tenure of five months before the reform. After the reform, when workers with a tenure of six months at layoff lose eligibility for UI benefits these patterns vanish.

Additionally, we examine changes in the probability of hiring a replacement worker around the reform. When firms lay off a worker, they often hire a new worker as a replacement. However, if firms continue to employ a formally laid off worker informally, they do not need to replace the worker. Consistent with this conjecture, we find that in areas with large informal labor markets, firms are about five percentage points less likely to hire a replacement worker in the same occupation within a month if the laid off worker is eligible for UI benefits. After the reform, when all workers around the six month threshold are ineligible for UI benefits, differences in hiring replacement workers disappear. Together these findings suggest that firms employ workers informally while they are on UI benefits.

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

On examining changes in wages, we find that newly hired workers who require longer tenure to qualify for UI benefits after the reform experience a 0.5-0.8 percent higher increase in wages. Additionally, these workers are relatively less likely to enter formal employment after the reform. The increase in wages is in line with the implicit contracting mechanism

[4]Being able to keep employing workers informally makes formal layoff and rehiring patterns less disruptive for firms and workers.

suggesting that workers and firms share rents from the UI system through lower equilibrium wages. Further consistent with this argument, we find that the increase in wages and the decrease in formal employment is significantly higher in areas with a higher share of informal labor markets and areas in which the timing of unemployment spells coinciding with eligibility for UI benefits is more pervasive. A ten percentage point increase in collusive layoff and rehiring patterns at the local industry level is associated with a 1.9 percent higher relative increase in wages for workers adversely affected by the reform.

Altogether, the empirical evidence in the paper suggests that firms and workers collude to time formal unemployment spells in accordance with workers' eligibility for UI benefits, and continue to employ some workers informally while they are on benefits. This pattern is more pervasive in industries and municipalities with high degrees of labor market informality. The combined evidence on formal unemployment timing and continued informal employment of workers strongly points to informal labor markets as an important factor in fostering the documented patterns. We perform several robustness tests to strengthen the validity of our results. First, we control for seasonal patterns by performing the same analysis for the previous year, for which we observe none of the same patterns. Second, we confirm that workers do not substitute to other forms of job separation, such as voluntary departures, after the reform. Third, we show that the results are not affected by potential announcement effects of the reform two months before its implementation. Importantly, we do not find any of the patterns in placebo tests using workers with more than two successful past applications for UI benefits, who are not affected by the reform.

The main trade-off in designing UI systems is to limit disincentive effects while providing workers with insurance against adverse income shocks. By documenting novel incentive effects of UI in the presence of informal labor markets, our findings have important implications for UI design in mid-income and developing countries. Recent years have seen a rapid spread of UI programs to these countries. In the light of this development, it is important to understand how UI affects workers' incentives in these countries, in order to optimize the design of UI programs.[5] The results in this paper suggest that informal labor markets may play an important role in facilitating collusion between workers and firms to extract rents from the UI system by allowing firms to employ workers informally while they receive UI benefits. This suggests that traditional models of optimal UI insurance (Baily 1978; Chetty

[5]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). These papers do not directly examine how differences in labor market informality influence the effects of UI programs, with the exception of Gerard and Gonzaga (2014), who examine the effect of labor market informality on job search intensities.

2006) miss important dimensions in the context of mid-income and developing countries.

The tailoring of unemployment spells to workers' UI eligibility suggests that in some cases UI does not fulfil an insurance purpose, but rather acts to redistribute income towards firms and workers who learn to play the system. Our results suggest that this effect is larger when workers and firms can extract higher rents from the UI system. The main determinants of rents are the duration and level of UI benefits, and layoff costs. Thus, rents can be reduced by lowering replacement rates, or by increasing experience rating. While the theory of second-best (Lipsey and Lancaster 1956) cautions us against making claims on total welfare, these insights suggest that a UI system with lower replacement rates and stronger experience rating is less susceptible to exploitation in this context than a system with more generous benefits payments and weaker experience rating. More nuanced policy implications may include tweaks to the UI system that prevent repeated temporary layoffs of the same worker by the same firm. With respect to the role of informal labor markets, better monitoring and higher penalties for informal employment may reduce strategic formal unemployment, by reducing the expected rents to be extracted from the UI system while maintaining an informal employment relationship.

Additionally, the results in the paper provide new insights into the impact of UI benefits on strategic unemployment inflow and outflow, and the role of collusion between firms and workers.[6] Existing empirical studies provide mixed insights on the effects of different aspects of UI design on layoff intensities. Feldstein (1978), Saffer (1982), Topel (1983), and Card and Levine (1994) show that layoffs are negatively related to experience rating. Anderson and Meyer (1997) find that, in contrast to duration, benefit levels have a strong impact on UI take-up. Winter-Ebmer (2003) documents that unemployment inflow is higher when benefits duration is extended. Jurajda (2002) finds that higher benefits levels have no effect on layoff intensities. More recent studies find no or very weak evidence of unemployment inflow timing with respect to UI eligibility (Card, Chetty, and Weber 2007; Schmieder, von Wachter, and Bender 2012). We find evidence that layoff intensities of workers eligible for UI benefits are higher when total benefits are high relative to formal employment and layoff costs, and that informal labor markets play an important role in generating higher layoff intensities for workers eligible for UI benefits as part of collusion between workers and firms.

It is important to consider the relevance of the findings beyond the specific context of the study. Our data spans the entire population of formal employees in the private sector.

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

While our findings are obtained during a severe recession in Brazil, Carvalho, Corbi, and Narita (2017) obtain almost identical point estimates for our main test on layoff intensities for an extended four-year period around the reform.[7] Moreover, evidence on the effects of UI in recessionary periods is of particular interest, as UI benefits are often extended during downturns (Rothstein 2011; Valletta 2014; Farber and Valletta 2015; Kroft and Notowidigdo 2016). Finally, while informal labor markets are more prevalent in mid-income and developing countries, developed countries also feature a non-negligible degree of informality in parts of their labor markets (Hazans 2011). Hence, we think that the insights are relevant and informative beyond the specific context examined 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, which it is mandatory for employers to sign whenever a worker is hired, promoted, or dismissed. This information is reported to the Ministry of Labor every year. Formal employees are entitled to a minimum wage. Payroll taxes amount to twenty percent of the formal wage to finance the public pension system, plus eight and a half percent for the workers seniority account (FGTS). Other mandatory contributions such as the social integration program (PIS) and contributions to social security funding (COFINS) depend on the industries in which firms operate and are paid as a fraction of net profits and sales.[8] Funding of the UI system comes from these contributions.

UI applies to formally employed private sector workers. Benefits are paid for three to five months, depending on workers' formal employment history. Three payments are made if a worker was employed for between six and eleven months during the last 36 months, four payments are made if a worker was employed for between 12 and 23 months during the last

[7] Existing evidence on the relationship between recessionary environments and incentive effects of UI is mixed. While Schmieder, von Wachter, and Bender (2012) show that incentive effects of UI tend to be weaker during recessions, Card et al. (2015a) document that UI durations are more responsive to benefit levels if the aftermath of the Great Recession in the U.S.

[8] A thorough review of the history of labor law in Brazil is provided in Gonzaga (2003).

36 months, and five payments are made if a worker was employed for at least 24 months during the last 36 months. In 2015, the monthly payments range from 1 to 1.76 minimum salaries, depending on the average pre-layoff wage. Importantly, the UI system does not feature a strong experience rating mechanism. If a firm dismisses a worker without an acceptable reason, it must pay an additional fifty percent of the total contributions that have accumulated in the employee's FGTS. On average, this layoff cost amounts to 8-19 percent of the expected benefits accruing to the worker, depending on the pre-layoff wage (the penalty is relatively lower for lower pre-layoff wages). Eighty percent of this penalty is paid directly to the worker, rather than being used to fund the UI system. Thus, implied experience rating, after accounting for payments that remain within the firm-worker relationship, is only about 1.6-3.8 percent of the UI benefits paid to the worker through the UI system. In contrast, firing workers with a valid legal reason does not involve penalties. The hurdle of providing sufficient evidence is high, and judges tend to rule in favor of employees. Only 3.5 percent of all layoffs are classified as firings with justified cause. Workers receive benefits only when they are laid off rather than fired for justified cause.

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 six months prior to layoff, had to be laid off without a justified reason, not be earning other labor income, and not have successfully applied for UI benefits during the previous sixteen months. On December 30, 2014, the parliament passed a provisional measure that tightened eligibility criteria for UI benefits. The new criteria were set to be enforced from March 1, 2015. While it was anticipated that UI would be reformed at some point, both the sudden implementation and the content of the new law were fully unexpected.[9] Since the UI reform was announced unexpectedly only two months before its implementation, workers with a tenure of four to seven months during January to April 2015, who constitute the main sample in this paper, were already in formal employment before the announcement of the reform. The main driver for the quick implementation and the tightening in eligibility criteria came from attempts on the part of the government to reduce the growing budget deficit. The size and duration of UI benefits were not altered. Importantly, contributions to the UI system were unaffected by the reform. Thus, the reform had no direct effect on employers' demand for formal labor.

[9]Estadao Politica, December 29, 2014, "Forca Sindical nega ter sido consultada sobre ajuste em benefícios". Doornik et al. (2017) show that firms whose workers benefit more from a more generous UI system experienced a larger drop in their stock price following the announcement of the reform on December 29.

The reform affected workers with less than two successful prior applications for UI benefits. For these workers, eligibility criteria were substantially tightened. To be eligible for UI benefits after the reform, a longer pre-layoff employment history than the six-month threshold from before the reform was required. Specifically, workers who applied for the first time required documented employment of at least 18 months in the 24 months prior to layoff. Workers who applied for the second time required 12 months in formal employment during the last 16 months (see Figure 1). This provisional measure was applied from March 2015 and became law in July 2015 with some adjustments.[10]

2.3 Data

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

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

[10]The adjusted requirements from July 2015 required a first time applicant to have at least 12 months of employment in the last 18 months. A second time applicant had to have at least 9 months of employment in the last 12 months. These adjustments do not directly affect workers around the six-month threshold that we exploit for our empirical analysis.

In Table 1, we provide evidence that workers with a tenure of six or seven months, who are affected by the reform, and workers with a tenure of four or five months, who are not directly affected by the reform, are indistinguishable in terms of observable characteristics before the announcement of the reform. Both groups of workers are virtually identical in terms of age, salary, gender, education, the size of the firm that employs them, and the industries in which they are employed. They do, however, differ in terms of their probability of being laid off and returning to formal employment. Specifically, a worker with six or seven months' tenure is 44 percent more likely to be laid off and 19 percent less likely to return to formal employment within five months of being laid off.

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, and reports labor market informality shares for twenty different industry classifications (see Table A.1). 66 percent of domestic services employees are shown as 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 A.1). Informality is not limited to certain areas in Brazil, but is prevalent throughout the country, with somewhat higher average informality in the north (Figure A.3).

Finally, we use data from the monthly employment survey Pesquia Mensal de Emprego (PME). This survey interviews 44,189 individuals in six metropolitan areas (Recife, Salvador, Belo Horizonte, Rio de Janeiro, Sao Paulo, Porto Alegre). We extract data on all individuals that are laid off from formal employment with a tenure of four to seven months, and all workers informally employed that were laid of with a tenure of four to seven months in their previous formal job. The sample period for tests using the PME survey data is from July 2014 to June 2015. This provides us with a sample of 1,968 workers that are laid off with a tenure of four to seven months during the sample period, and 681 informally employed workers that had previously been laid off with a tenure of four to seven months. In addition to workers' tenure, the survey provides information on whether a worker is formally employed, informally employed, or unemployed, and different buckets for the number of employees of the firm in which a worker is employed.

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 and the presence of informal labor markets in explaining these inflow and outflow patterns, and the effects on wages.

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 above the threshold are eligible for UI benefits only before the reform, whereas workers below the treshold 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 occurred 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 workers unaffected by the reform. Together, this allows us to identify how UI benefits affect workers' decisions 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-month 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.[11] 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 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

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

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 above the threshold are eligible for UI benefits. A negative value of β_3 implies higher unemployment inflow when workers are eligible for UI benefits.

We examine the role of informal labor markets for the patterns we observe in the data, by exploiting two sources of variation in labor market informality: cross-sectional variation in informality across industries (Table A.1) and variation in labor market informality across municipalities (see Figures A.1 and A.3).[12] To formally assess how the presence of informal labor markets interacts with the documented effects, we add a continuous variable $Informal$, which is the share of informal employment in a given industry or municipality, and its interaction with the other dependent variables, to equation (1).

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 cases of collusion between firms and workers, we expect the same firm to lay off workers when they are eligible for UI benefits and to rehire them when their eligibility for benefits ends. In contrast, 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 their benefits eligibility ends. Specifically, we test whether firms collude with workers by laying them off when they become eligible for UI benefits and rehire them just when their UI benefits stop, 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 hired by the

[12] Labor market informality at the municipality level is not exclusively 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, its distribution is very similar and highly correlated with the actual measure (0.93) (Figure A.2).

same firm four to nine months after layoff.[13] 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. A negative value of β_3 implies that the same firm is more likely to rehire workers when their benefits end, consistent with collusion between firms and workers.

To sharpen the interpretation of the results and to control for alternative explanations, for example temporary layoffs in response to labor market fluctuations, we exploit a quirk of the UI system in Brazil that provides a unique prediction for reemployment timing by the same firm for firms that engage in collusion with their workers. Since workers can only apply for UI benefits in the sixteenth month after their last successful application for UI benefits, a firm-worker pair that seeks to repeatedly minimize formal employment to the minimum of months required to qualify for UI may therefore rehire a worker nine to ten months after the initial layoff (see Figure 2). To assess this possibility, we estimate:

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

where $P[9-10]_{it}$ is a dummy variable that takes the value of one if worker i is reemployed nine or ten months after layoff, and zero if worker i is reemployed seven to eight months after layoff. 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. The dummy variable $Reform_t$ takes the value of one for the months of 2015, and zero for the months of 2013. The dummy variable $Same_{it}$ takes the value of one if worker i is rehired by the same firm that laid her off, and zero if she is hired by a different firm.

A positive value of β_4 suggests that the same firm is more likely to rehire a worker after nine to ten months than a different firm if the worker was laid of just after becoming eligible for UI benefits. A negative value of β_7 would suggest that the pattern disappears after the reform when workers with tenure of six months are no longer eligible for UI benefits at layoff.

[13] Workers are eligible for at least three months of UI benefits. Workers must 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 formally rehire workers any time between four to nine months for them to be eligible for benefits sixteen months after the previous UI benefits application.

3.3 Informal Employment

Informal labor markets provide workers with the opportunity to claim UI benefits while continuing to be informally employed. This may exacerbate the incentive effects of UI and facilitate collusion between firms and workers (Feldstein 1976; Baily 1977). Since data on informal employment is not available at the employer-employee matched level, we provide indirect evidence of informal employment while workers are on UI benefits from survey data, by estimating:

$$P[informal]_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it} \quad (4)$$

where $P[informal]_{it}$ is a dummy variable that takes the value of one if worker i reports to be informally employed following layoff from formal employment, and zero if she reports to be formally employed at a different firm or unemployed following layoff from a formal job. 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. The dummy variable $Reform_t$ takes the value of one for the first six months of 2015, and zero for the last six months of 2014. A negative value for β_3 implies that workers are more likely to transition from formal to informal employment if they are eligible for UI benefits.

Replacing the dependent variable in equation (4) with a dummy variable $Same_{it}$ that takes the value of one if an informal worker works for is hired by the same firm that laid her off, and zero if she works for or is hired by a different firm, we examine whether workers are more likely to be informally employed by the firm that laid them off, or ot be rehired by the same firm after transitioning back from informal to formal work after UI benefits end. Since we cannot directly observe firm identities in the survey data, we proxy for the same firm by a firm being in the same number of employees bucket. Here, the dummy variable $6Months_{it}$ takes the value of one for workers that were laid off with a tenure of six or seven months at layoff, and zero for workers that were laid off with tenure of four or five months at layoff. A negative value for β_3 implies that workers are more likely to work for informally and be rehired by the same firm if they transitioned from formal to informal employment and back when they are eligible for UI benefits at layoff.

Finally, we use evidence on worker replacements from administrative data to provide further evidence on informal employment of workers on benefits, by estimating:

$$P[replacement\ hire]_{it} = \alpha + \beta_1 \cdot 6Months_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot 6Months_{it} * Reform_t + \epsilon_{it} \quad (5)$$

where $P[replacement\ hire]_{it}$ is a dummy variable that takes the value of one if the same firm hires a new worker in the same occupation within one month of laying off worker i, and zero if the firm does not hire a new worker within a month of laying off worker i.[14] 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 two months after the reform, and zero for the two months before the reform. A positive coefficient β_3 indicates that firms are less likely to hire a replacement worker after laying off a worker who is eligible for UI benefits.

3.4 Formal Employment and Wages

Next, we assess the ex ante incentive effects of UI for workers who are not currently formally employed, 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; Hamermesh 1979). 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:

$$Workers\ Hired_{ind,mun,t} = \alpha + \beta_1 \cdot Affected_{it} + \beta_2 \cdot Reform_t + \beta_3 \cdot Affected_{it} * Reform_t + \epsilon_{it} \quad (6)$$

where $Workers\ Hired_{ind,mun,t}$ 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 29, 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 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.

[14] To reduce the noise in the estimates, we only consider layoff months in which a firm lays off either a worker with a tenure of four or five months, or a worker with a tenure of six or seven months, not both.

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. If firms and workers share rents extracted from the UI system through lower wages, we would expect to find a positive value for β_3 in the wage regression.

3.5 Identifying Assumptions

The main identifying assumption for our tests are that workers with four or five month tenure are a good control group for workers with six or seven months tenure in the two months after the implementation of the reform. Specifically, our identification strategy requires that incentives of workers with tenure of four or five months are not changed in the two months after the reform (March and April) relative to the two months before the reform (January and February). Since we focus on a narrow time window of two months before and after the reform, it is unlikely that workers with tenure of four or five months experience an event that would affect their behavior differentially from workers with tenure of six or seven months besides the reform itself. Thus, the main challenge to the identifying assumption is that workers with tenure of four to five months are themselves affected by the reform, which could lead to a bias in our estimates.

The main reason why workers with a tenure of four and five months could be affected by the reform is that the marginal value of staying employed for another one or two months changes with the reform. Before the reform, tsaying employed one (two) additional month(s) allows a worker to qualify to UI benefits, whereas after the reform this incentive is lost.[15] While this could affect the incentives of workers with tenure of four or five months, during our pre-reform period in January and February the reform was already announced. Thus, for workers with tenure of four months in January and February and for workers with tenure of five months in February the marginal benefit of staying employed for another one or two months is already equally low as after the reform is implemented in March. Only for workers with tenure of five months in January incentives might be different.

There are two ways for us to mitigate this potential identification concern. First, we can exclude workers with tenure of five months from the control group, as workers with tenure of four months face the same incentives before and after the reform. Doing so does not affect our results. Second, we can compare the behavior of workers with tenure of four or

[15] We can control for seasonal effects that may affect workers with tenure of four and five or six and seven months differentially by comparing our results to the previous year.

five months in January and February who are affected by the reform (less than two prior UI benefits spells) to workers with tenure of four or five months that are unaffected by the reform (two or more prior UI benefits spells). Figure 3 compares UI inflow based on workers tenure for workers affected by the reform (dashed lines) and workers unaffected by the reform (solid lines) in January (black lines) and February (gray lines). The lines for affected and unaffected workers almost lie on top of each other for both months. In particular, there is no differential effect at four or five months tenure for both groups. Similarly, Figure 4 provides the same plots for the months of March and April. Again, we can see no seperate patterns for workers with tenure of four or five months for workers affected and unaffected by the reform. This graphical evidence suggests that workers with less than two previous UI benefits spells that constitute our control group are not affected by the reform both before the reform in January and February, as well as after the reform in March and April. Additionally, the graphical evidence in Figures 3 and 4 suggests that the group of workers affected and unaffected by the reform display almost identical behavior, which lends support to our second identifying assumptions underlying the comparison of the two groups in tests assessing the effect of UI benefits eligibility on ex ante labor supply.

4 Results

This section presents the results from the empirical analysis. We document that eligibility for UI benefits has large effects on unemployment inflow and outflow patterns. Firms and workers seem to time unemployment spells to coincide precisely with eligibility for UI benefits, in particular when the rents they can extract from the UI system are high. Additionally, we show that UI incentivizes workers to supply more formal labor at lower wages. Exploiting cross-sectional variation in labor market informality, we document that these effects are stronger in the presence of informal labor markets. Finally, we provide indirect evidence that firms continue to employ workers informally while they are on benefits.

4.1 UI Benefits and Unemployment Inflow

Figure 5 depicts layoff intensities for workers with different tenures, separately for the months from January to April 2015.[16] For workers with tenure of six to seventeen months, the probability of being laid off significantly decreases after the reform, in line with the shift of

[16]The plots are aligned at the April 2015 values for workers with five months tenure to facilitate comparison.

the UI benefits eligibility threshold from six to twelve and eighteen months. There is a sharp drop in the probability of being laid off for workers with a tenure of six months, who lose eligibility for benefits after the reform, relative to unemployment inflow for workers with a tenure of five months, who are ineligible for benefits before and after the reform.

We confirm the insights from the graphical analysis statistically in Table 2, 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 0.52 pp (twelve percent) for workers with a tenure of six or seven months, compared to workers with a tenure of four or five months.[17] Further saturating the specification with municipality-month fixed effects to account for local shocks in column II, the effect remains similar at 0.53 pp, and is unaffected by controlling for industry-specific local shocks (muni-industry-month fixed effects) in column III, or occupation-specific local shocks (muni-industry-occupation-month fixed effects) in column IV at 0.49 pp.[18]

The results cannot be explained by seasonal effects. We observe no similar patterns during the same months of the previous year (Figure A.4 and Table A.3), or for workers with more than two previous successful UI benefits applications who are not affected by the reform (Figure A.5 and Table A.4). Additionally, the results are robust to comparing workers with four to seven months' tenure in November and December 2014 (the months before the announcement of the reform) to the post-reform period in March and April 2015, which ensures that the announcement of the reform does not affect the results (Table A.6).[19]

We next examine the role of informal labor markets in explaining the previous findings. In Figure A.6, we split the sample into workers employed in industries with above (top panel) and below (bottom panel) median levels of labor market informality. The graphical evidence 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,

[17] We focus our analysis to the four month period around the reform for sharper identification. Carvalho, Corbi, and Narita (2017) document almost identical magnitudes when performing the same analysis for an extended time period around the reform (January 2012 to December 2015).

[18] The results are not driven by a"relabeling" of voluntary departures as layoffs. We observe no change in voluntary layoffs around the reform (Table A.2).

[19] In Figures A.8 to A.10 and Tables A.8 to A.10 we document similar incentive effects for unemployment outflow. Workers are more likely to return to formal employment when UI benefits cease, consistent with the braod evidence in the existing literature.

in Figure A.7, we find that in municipalities with above median levels of informality, unemployment inflow decreases by about two percentage points for workers who lose eligibility for UI benefits after the reform. In municipalities with below median levels of informality, the magnitude of the effect is smaller than one percentage point.

In Table A.7, we formally assess the way in which informal labor markets affect how layoff intensities interact with UI benefits eligibility. The top panel shows the results for variation in labor market informality at the industry level. We find that a ten percentage point increase in labor market informality leads to a 0.24 pp stronger decrease in unemployment inflow after the reform (column I). Controlling for local shocks that are specific to workers affected by the reform, the effect is similar at 0.18 pp (column II). Additionally, controlling for local industry shocks leaves the effect virtually unchanged at 0.17 pp (column III). When we further add controls for shocks to specific occupations within a local industry, the magnitude of the effect is unchanged at 0.17 pp (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 Figure A.6 and Table A.7 suggest that the presence of informal labor markets has a strong effect on the relationship between unemployment inflow and eligibility for UI benefits.

4.2 Collusion

To be eligible for UI benefits, workers need to be laid off by their employer. Layoffs may be induced through different mechanisms. They could be worker-induced, for example, workers may elicit layoffs through shirking. Higher layoff intensities of workers eligible for benefits could also be caused by firms that temporarily layoff workers eligible for UI benefits who are less likely to search for employment in other firms. Alternatively, firms may collude with workers to extract rents from the UI system by laying them off when they are eligible for UI benefits.

To assess whether collusion between firms play an important role in strategic unemployment, we explore whether firms that lay off workers when they become eligible for UI benefits rehire the same workers just when their benefits stop. Specifically, we examine the probability of a worker being rehired by the same firm four to nine months after layoff, when their benefits cease, by estimating equation (2). If higher unemployment inflow is driven by shirking, we do not expect firms to rehire the same worker. In contrast, if firms lay off workers in reaction to demand fluctuations or collude with workers to time unemployment spells with

UI benefits eligibility, we expect them to be more likely to rehire the same worker when her benefits end. We follow our main identification strategy comparing dismissed workers with six or seven months' tenure at layoff, who lose eligibility after the reform, to those with five months' tenure at layoff, who are always ineligible.

The results gathered in Table 3 suggest that at least part of higher layoff intensities are driven by firms. Column I shows that for all workers returning to formal employment within one year of being laid off, the probability of being rehired by the same employer four to nine months after layoff is about two percentage points higher before the reform for workers with tenure of six or seven months at layoff, compared to workers with four or five months' tenure at layoff. After the reform, when both groups of workers are ineligible for UI benefits, the difference in rehiring by the same firm four to nine months after layoff vanishes. Controlling for local industry shocks (muni-industry-month fixed effects) in column II, and occupation-specific shocks within a local industry (muni-industry-occupation-month fixed effects) in column III does not affect the results. In columns IV to VI, we restrict the sample to workers who 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 after layoff 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.11), and for workers with more than two previous UI benefits spells, who are not affected by the reform (Table A.12). These results provide initial evidence of collusion between workers and employers. Firms lay off workers when they qualify for UI benefits, and rehire them just when their UI benefits eligibility ends.

Next, we examine whether these patterns are concentrated in industries and municipalities with large informal labor markets, in Table A.13. A unique feature of informal labor markets is that they 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. 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, and to rehire them just when their benefits end. Specifically, a ten percentage point increase in labor market informality leads to a 0.50-0.66 pp increase in the rehiring of workers by the same firm just when UI benefits stop. The results are similar at the municipality level, with slightly higher magnitudes (Panel B).

To separate firm-worker collusion from temporary layoffs driven by labor demand fluctuations, we exploit a unique prediction for formal reemployment by the same firm in Figure6. Since workers are only alowed to apply for UI benefits in the sixteenth months after the last successful UI benefits application, a firm that seeks to minimize formal employment spells between two Ui benfits applications should reemploy workers nine to ten months after the previous layoff if workers qualified for UI benefits. Accordingly, we find that workers are more likely to be hired by the same firm nine to ten months after being laid off if they were eligible for UI benefits at the time of layoff, whereas we observe no such differences seven to eight or eleven to twelve months after layoff. Additionally, we find no differences in reemployment probabilities seven to twelve months after layoff by other firms for workers laid off with a tenure of four to five or six to seven months, which means there is no higher incentive for firms that experience labor demand fluctuations to rehire workers nine to ten months after layoff.

In Table 4, we compare reemployment probabilities nine to ten months after layoff compared to reemployment probabilities seven to eight months after layoff by the same firm and other firms for workers with five or six weeks of tenure at layoff. We find that workers that are eligible for UI benefits before the reform are about five percentage points more likely to be rehired by the same firm than another firm nine or ten months after layoff rather than seven to eight months after layoff compared to workers ineligible for benefits. After the reform when both groups of workers are ineligible for UI benefits differences in rehiring probabilities by the same firm disappear. Due to the specific design of the pre-reform UI system, these findings provide compelling evidence of firm-worker collusion to extract rents from the UI system.

Assuming that colluding workers are rehired by the same firm with probability one, and that workers inducing layoff through shirking are rehired with probability zero, we compute the fraction of colluders in strategic unemployment inflow from our estimates and descriptive data. In the data, we observe a 5.3 percent probability that workers laid off with a tenure of six or seven months will be rehired by the same firm four to nine months after layoff during the post-reform period when they are not eligible for UI benefits. The estimates from Table 3 suggest that the additional probability of being rehired by the same firm for these workers is 1.7 percentage points when they are eligible for benefits at layoff during the pre-reform period. Combined with the information that the share of formal unemployment inflow due to eligibility for UI benefits estimated in Section 4.1 is 11.5 percent of all laid off workers, the fraction of colluders x is given by: $11.5\% * x * 1 + 11.5\% * (1-x) * 0 + (1-11.5\%) * 5.3\% = 5.3\% + 1.7\%$, which implies that around twenty percent of strategic unemployment inflow due

to UI benefits eligibility can be explained by this simple form of potential collusion between employers and workers.

This estimate is likely to be conservative. First, we assume that colluders return to the same firm with a probability of one, whereas there might be cases where formal reemployment is not part of the agreement to extract rents from the UI system. Second, we only capture one particular pattern consistent with collusion. Other forms of collusion that we do not capture may exist in addition to the simple layoff-rehiring pattern that we examine. For example, several firms and employees as a group could engage in collusion in a way that our test would not identify as collusion, or formal reemployment may not be part of the collusion agreement, or it may be delayed beyond nine months after layoff.

4.3 Total Rents and Strategic Unemployment

Next, in Table 5, we examine whether unemployment inflow and outflow timing in line with UI benefits eligibility and reemployment by the same firm are more prevalent when the rents that can be extracted from the UI system are higher. Specifically, we examine whether strategic unemployment and reemployment patterns consistent with collusive behavior are more common when the ratio of total rents (UI benefits payments) to total layoff costs (penalty) and formal employment costs (taxes) is higher. Since this surplus is correlated with benefits accruing to workers, which may elicit higher worker-induced layoff intensities, for example through shirking, we include the replacement rate in the empirical specification. This allows us to differentiate between worker-specific incentives and firm-worker collusion incentives.

The results in column I show that firms are more likely to lay off workers who are eligible for UI benefits when the potential rents that can be extracted from the UI system are high. When workers with six and seven months' tenure lose eligibility for UI benefits after the reform, these workers are relatively less likely to flow into unemployment, by 0.14 pp per ten percentage point increase in the rents that can be extracted from the UI system. We find similar effects for unemployment outflow in column II. Workers who lose eligibility for UI benefits are 0.90 pp more likely to return to formal employment within five months of layoff per ten percentage point increase in potential rents from the UI system. The results in column III document that layoffs and rehiring patterns by the same firm coinciding precisely with UI eligibility are 0.27 pp more common per ten percentage point increase in the rents that firms and workers can extract from the UI system. This is equivalent to an eighteen

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

With respect to replacement rates, we find that workers are somewhat more likely to flow into unemployment when they qualify for UI benefits when their replacement rate is higher (column I). However, the impact of the replacement rate on unemployment inflow is weaker compared to the effect of total rents accruing to firms and workers, and is not statistically significant. One factor making collusion relatively more attractive is that a worker inducing layoff through shirking is less likely to be reemployed by the same firm after their UI benefits stop, and may struggle to time reemployment to coincide with the end of UI benefits, especially during a recessionary period. We find that workers with a higher replacement rate are less likely to return to formal employment after their benefits cease (column II), consistent with Chetty (2008), who argues that lower liquidity constraints reduce job search intensities. Finally, when replacement rates are higher, making worker-induced layoffs more likely, workers are less likely to be rehired by the same firm when their benefits end (column III).

Together, these results strengthen the interpretation that collusion between firms and workers plays an important role in explaining higher layoff intensities when workers are eligible for UI benefits. Additionally, the results imply that reducing the potential surplus to firms and workers from extracting rents from the UI system may reduce collusive behavior, for example by lowering replacement rates or increasing experience rating. More nuanced policy implications may include tweaks to the UI system that prevent repeat temporary layoffs of the same worker by the same firm.

4.4 Informal Employment

In this subsection, we provide evidence on an important channel supporting collusion between firms and workers that highlights the important role of informal labor markets. Firms can employ workers informally while they are eligible for UI benefits, allowing them to extract payments from the UI system while sustaining the employment relationship. Since data on informal employment is not available at the employer-employee matched level, we resort to indirect evidence on informal employment while workers are on benefits.

We start by comparing the probability of a worker transitioning from formal to informal work for workers with tenure of five and six months at layoff before and after the reform

using PME survey data in columns I and II of Table 6.[20] We find that workers are about six percentage points more likely to transition from formal to informal employment when they are eligible for UI benefits before the reform. After the reform, when workers with five or six months tenure at layoff are both ineligible for UI benefits they are equally likely to transition to informal employment after being laid off.

In columns III and IV, we examine whether workers that are employed informally are more likely to work for informally the same firm rather than a different firm if they were eligible for UI benefits at layoff (six months tenure workers) than if they were ineligible (five months tenure workers).[21] We find that workers are more than ten percentage points more likely to work informally for the same rather than another firm if they were eligible for UI benefits at layoff before the reform. After the reform when workers laid off with six months tenure are no longer eligible for UI benefits they are no longer more likely to be employed informally by the same firm when existing formal employment.

In columns V and VI, we assess whether workers that were employed informally are more likely to return to formal employment with the same firm rather than a different firm within ten months of layoff, if they were eligible for UI benefits at layoff than if they were ineligible. We find that workers are more than ten percentage points more likely to return from informal to formal employment with the same firm if they were eligible for UI benefits at layoff before the reform. After the reform when workers laid off with six months tenure are no longer eligible for UI benefits they are no longer more likely to be employed by the same firm when existing informal employment. Together, the results in Table 6 suggest that workers are employed informally before being formally reemployed by the same firm when they are laid off just after becoming eligible for UI benefits.

We complement the survey evidence with indirect evidence from administrative data. When firms lay off a worker for performance reasons, they are likely to hire a different worker as a replacement. However, if firms lay off workers formally, but continue to employ them informally, they are less likely to hire a new worker. We compare the probability that firms will hire a replacement worker after laying off a worker with six or seven months' tenure, who is eligible for UI benefits before the reform, and after laying off a worker with four or five months' tenure, who is never eligible for UI benefits. Continued informal employment while workers are on benefits would predict that hiring rates for replacement workers would

[20] Our results are likely to be conservative as the PME data does not allow us to differentiate between worker affected and unaffected by the reform.

[21] The PME data does not include an identifier for firms. We proxy for same firm reemployment by reemployment by a firm in the same number of employees bucket provided in the data.

be lower for workers laid off with a tenure of six or seven months in January and February when they are eligible for benefits.

We start with a graphical depiction of the probability that firms will hire a new worker within one month of laying off a worker with a tenure of four to five months (dashed line), or a worker with a tenure of six to seven months (solid line) in the top panel in Figure 7. Before the reform, when workers with a tenure of six or seven months are eligible for UI benefits, firms are about five percentage points less likely to hire a replacement worker after laying off a worker with a tenure of six to seven months, compared to laying off a worker with a tenure of four or five months. Strikingly, this discrepancy disappears from the month of the implementation of the reform, when workers with a tenure of six or seven months are no longer eligible for UI benefits. As evident from the 2014 part of Figure 7, these differences are not driven by seasonal layoffs or replacement hiring patterns in the months from January to April. Additionally, Figure A.11 and Table A.15 show that for workers who are unaffected by the reform and continue to be eligible for UI benefits with a tenure of six months, differences in the probabilities of hiring replacement workers remain constant after the implementation of the reform. When we compare the graphical evidence for above and below median industries (Figure A.12) and municipalities (Figure A.13), we observe that the effects are much stronger in industries and municipalities with larger informal labor markets, which is further consistent with firms employing workers informally instead of hiring a replacement worker.

We confirm the insights from the graphical analysis statistically in Table 7, by estimating equation (5). The results in column I show that firms are about two and a half percentage points less likely to hire a new worker within a month of laying off a worker with tenure of six or seven months, compared to the probability of hiring a new worker within a month of laying off a worker with tenure of four or five months, before the reform. After the reform, this difference in hiring a replacement worker depending on tenure at layoff almost completely disappears. This suggests that firms continue to informally employ some of the workers formally laid off when they are eligible for UI benefits. The results are unaffected by restricting the comparison to workers within the same municipality (column II), local industry (column III), or the same occupation within a local industry (column IV). The results in Table A.16 statistically confirm that lower rates of replacement hiring while workers are on benefits are more prevalent in industries (top panel) and municipalities (bottom panel) with a higher share of informal labor markets. From our previous analysis, we know that twelve percent of unemployment inflow is strategic, and about twenty percent of the strategic unemployment inflow (2.4 percent of all unemployment inflow) seems to be explained by

direct collusion between firms and workers. The probability of replacement hiring for workers with a tenure of six or seven months increases by 1.19 percent after the reform compared to an average replacement probability of about fifty percent (Table 7, column IV). This suggests that about $0.0119/0.5 = 0.0238$ of workers laid off at the six-month threshold continue to work for the same firm informally while they are on benefits, which implies that the majority of workers who collude with firms to extract rents from the UI system continue to work for these firms informally.

4.5 Employment and Wages

Examining changes in formal employment and wages, we find evidence consistent with rent sharing between firms and workers through lower equilibrium wages. Figure 8 depicts the time-series evolution of 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 (solid lines), and for workers with at least two successful past UI benefits applications, who are unaffected by the reform (dashed lines). To facilitate comparison, all plots are adjusted for calendar month and worker group (workers affected vs. non-affected by the reform) fixed effects. Starting in January 2015, the month after the announcement of the reform, we observe a relative drop in the hiring of workers affected by the reform leading to a continued relative drop in total formal employment of these workers. A simultaneous relative increase in wages for newly hired workers who face stricter eligibility requirements for UI benefits suggests that the drop in formal employment is driven by a reduction in formal labor supply.

In Table 8, we examine changes in formal hiring, employment, and wages after the reform statistically by estimating equation (6). The results in columns I and II indicate that formal hiring of workers who are less likely to qualify for UI benefits after the reform decreases relatively by about 0.4 percent of the pre-reform labor force. Continued lower hiring leads to a relative drop in their formal employment by about six 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 increase relatively by 0.5-0.8 percent. Columns VII and VIII confirm that the increase in hiring wages for workers affected by the reform is driven by an increase in wages for the same worker. 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.

This decline in quantity (employment) and increase in price (wages) suggests that formal labor supply is lower when workers are less likely to qualify for UI benefits. This is consistent with colluding firms and workers implicitly agreeing on lower wages to share rents from the UI system, and firms benefiting from higher labor supply when timing workers' unemployment spells according to UI benefits eligibility (Feldstein 1976; Baily 1977; Christofides and McKenna 1996). To provide more direct evidence on the role of strategic behavior by firms and workers to time formal unemployment according to UI benefits eligibility in driving changes in formal employment and wages after the reform, we examine whether these changes are stronger where firms engage in strategic and collusive behavior before the reform. For each municipality-industry cell of the data, we compute the degree of strategic unemployment before the reform as the ratio of workers laid off with a tenure of six or seven months to the number of workers laid off with a tenure of four to seven months. Similarly, we compute the degree of collusive behavior as the fraction of workers rehired by the same firm after four to nine months among all workers laid off with a tenure of six or seven months.

The results collected in Table 9 are consistent with firms benefiting from collusion through paying lower wages. We find that hiring of workers with less than two successful past applications for UI benefits declines relatively more in local industries in which strategic unemployment inflow explains a larger fraction of total unemployment inflow (column I). Similarly, hiring of workers affected by the reform drops more in local industries in which rehiring by the same firm is timed to coincide with UI benefits eligibility before the reform (column II). Consequently, we observe a larger relative drop in employment for affected workers after the reform in local industries with higher strategic unemployment inflow and more reemployment outflow timed according to UI benefits stopping (columns III and IV). In terms of changes in wages, we find that the increase in wages for workers affected by the reform is higher in local industries with a greater degree of strategic unemployment inflow, by 0.04 percent per ten pp increase in the share of workers laid off with a tenure of six to seven months in total layoff with a tenure between four and seven months (column V). Strikingly, wages increase by an additional 1.89 percent per 10 pp increase in reemployment by the same firm (column VI). Together, the results in Table 9 suggest that formal labor supply of workers affected by the reform drops particularly strongly and wages increase more in areas where strategic unemployment inflow and outflow are more prevalent before the reform, consistent with a reduction in implicit rent-sharing between firms and workers when UI eligibility criteria are tightened.

The reduction in formal labor supply of workers whose eligibility criteria for UI benefits are tightened by the reform may be stronger when workers have the option to work informally

while receiving UI benefits. Testing this conjecture formally in Table A.17, we find that hiring (columns I and II) and employment (columns III and IV) drops particularly strongly 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).

5 Discussion

This section summarizes the insights from the empirical analysis, evaluates the magnitudes of the results, and discusses policy implications.

In Section 4.1, we document that about twelve percent of formal unemployment inflow at the six-month threshold is due to workers' eligibility for UI benefits (Table 2, column I).[22] Figure 5 shows that the share of strategic unemployment inflow is similar across all tenure regions in which workers are eligible for UI benefits. From 6 to 9 months' tenure, where collusion is most profitable to firms and workers, around twelve percent of unemployment inflow is strategic. For tenures of 10 to 17 months, strategic unemployment accounts for ten percent of unemployment inflow. This is consistent with a twenty percent share of collusion in strategic unemployment in the 6-9 month tenure region when collusion is most profitable for firms (Table 3, column I). For tenures of 18 months or longer, the March and April values of unemployment inflow converge with the January and February values. This suggests that strategic unemployment continues to account for about ten percent of unemployment inflow.

Since 23.78 percent of unemployment payments in 2014 went to workers laid off with a tenure of 6-9 months, this implies that 10.48 percent (0.2378*12+(1-0.2378)*10) of unemployment benefits payments go to strategically unemployed workers. In 2014, the year before the reform, total UI benefits payments were 32.8bn BRL, or 0.6 percent of GDP, 10.48 percent of which amounts to 3.44bn BRL, or 0.063 percent of GDP. Since the UI system in Brazil is mainly financed from taxes on firms' sales and profits, the costs of financing strategic unemployment impose an additional tax burden of 3.44bn BRL on firms' sales and profits. Other types of distortions that are harder to quantify include disruptions at the firm level due to higher labor turnover, generating additional costs to screen and hire new workers, or reducing labor productivity due to lower firm-specific human capital.

[22] Carvalho, Corbi, and Narita (2017) estimate that the magnitude of the effect is almost identical using a four year window around the reform.

The annual costs and distortions from strategic unemployment and firm-worker collusion are larger with informal labor markets. The estimates from Table A.7, column IV, imply that strategic unemployment inflow increases by 0.17 (0.24) percentage points per ten percentage point increase in labor market informality at the industry (municipality) level.[23] To compute the fraction of strategic unemployment inflow explained by collusion, we use the estimates from Table A.13, column IV, which imply that reemployment by the same firm upon exhaustion of UI benefits is 0.61 (0.81) percentage point higher per ten percentage points increase in labor market informality. Figure A.14 plots the share of strategic unemployment inflow and firm-worker collusion for different degrees of labor market informality.[24] The total UI benefits payments to strategic workers as a fraction of GDP can increase with labor market informality to more than 0.1 percent of GDP in areas with high labor market informality.

Several parameters of the UI system may reduce strategic unemployment inflow and firm-worker collusion. The results in Section 4.3 show that strategic unemployment inflow and collusion are more prevalent when the rents that can be extracted from the UI system are larger. Rents can be reduced by lowering total UI payments, for example, by reducing the level and duration of benefits payments. Requiring longer formal employment to qualify for UI benefits decreases the value of total benefits relative to the costs of establishing eligibility for UI benefits through formal employment. More nuanced tweaks to the UI system may include measures such as reducing UI benefits payments for repeated temporary layoffs that directly target the patterns emerging from firm-worker collusion. Additionally, higher experience rating that increases firms' layoff costs, similar to the U.S. system, reduces the rents that can be extracted from the system. Finally, better monitoring of informal labor markets or higher penalties for informal employment may also discourage firms from employing workers informally while they are on UI benefits.

[23]To translate the volume of strategic unemployment inflow into the share of strategic unemployment in total unemployment inflow, we divide the volume of strategic unemployment inflow by total unemployment inflow. For x percent labor market informality at the industry level, the share of unemployment inflow attributable to eligibility for UI benefits is $0.0017x/(0.0397 + 0.0017x)$.

[24]Assuming that colluding workers are rehired by the same firm with probability one, and that workers who induce layoff through shirking are not rehired by the same firm allows us to compute the fraction of colluders as: $x = \frac{\Delta P[unjust.]_i}{\Delta P_{same}[4-9]_i} + P_{same}[4-9]_{i,base}$, where $\Delta P[unjust.]_i$ is the percentage points increase in unemployment inflow due to strategic unemployment, $\Delta P_{same}[4-9]_i$ is the percentage point increase in reemployment by the same firm four to nine months after layoff, and $P_{same}[4-9]_{i,base}$ is the rate of reemployment by the same firm in the absence of eligibility for UI benefits. The intuition behind this formula and its derivation are provided in Section 4.2.

6 Conclusion

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

Further examining the underlying mechanism, we find that all results strongly correlate with labor market informality at the industry and municipality levels. Survey evidence suggests that workers are more likely to transition to informal employment and to return to the same firm when they are laid off just after becoming eligible for UI benefits. In addition, when firms layoff workers who are eligible for benefits at the six-month threshold before the reform, they are less likely to hire a replacement worker, compared to when they lay off a worker with a tenure just below the six-month threshold, who is ineligible for UI benefits. This pattern is particularly strong in labor markets with a higher degree of informality, and disappears after the reform when workers with a tenure of six months are no longer eligible for UI benefits. This indirect evidence strenghtens the interpretation of the results that firms and workers revert to informal employment relationships for the period that workers are eligible for UI benefits, to extract rents from the UI system.

Our findings have important implications for UI design. The timing of unemployment spells to fit workers' UI eligibility suggests that some part of the UI system does not fulfil an insurance purpose, but transfers rents towards firms and workers who exploit the system. We find that this effect is larger when the potential rents that firms and workers can extract from the UI system are larger. Rents can be reduced by lowering replacement rates and the duration of benefits, or by increasing experience rating to increase the cost of layoffs. More nuanced policy implications may include tweaks to the UI system that prevent repeat temporary layoffs of the same worker by the same firm. The strong correlation with labor market informality suggests that these considerations are particularly important for mid-income and developing countries with large informal labor markets, and that reducing labor market informality, or better monitoring, may reduce rent extraction from the UI system.

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

	4-5 Months Tenure	6-7 Months Tenure	Difference
Age (Years)	32	32	0
Salary (Real)	1,239	1,277	38
Male	0.611	0.606	-0.005
University Education	0.064	0.069	0.005
Firm Size (Employees)	66	70	4
Fraction in Construction	0.122	0.121	-0.001
Fraction in Manufacturing	0.156	0.151	-0.005
Fraction in Agriculture	0.033	0.035	0.002
P$[U_{unjust.}]$	0.032	0.046	0.014
P$[e \leq 5]$	0.521	0.424	-0.097

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

Table 2: **Unemployment Inflow**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0129***	0.0125***	0.0122***	0.0123***
	(0.0007)	(0.0006)	(0.0005)	(0.0005)
$6Months_{it} * Reform_t$	-0.0052***	-0.0053***	-0.0053***	-0.0049***
	(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	8,532,451	8,532,451	8,532,451	8,532,451
R^2	0.001	0.016	0.053	0.127

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 3: **Same-Firm Rehiring**

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 at the end of UI benefit eiligibility 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 who 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 reemployed by the same firm four to nine months after being laid off, and zero if worker i is 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 4: **Collusion**

Dep. Var.: $P[9-10]_{it}$	I	II	III	IV
$6Months_{it}$	0.0025	-0.0010	-0.0032*	-0.0038*
	(0.0020)	(0.0018)	(0.0019)	(0.0021)
$Same_{it}$	-0.1493***	-0.0836***	-0.0788***	-0.0839***
	(0.00126)	(0.0067)	(0.0075)	(0.0087)
$6Months_{it} * Reform_t$	-0.0035	0.0004	0.0013	0.0016
	(0.0034)	(0.0035)	(0.0038)	(0.0044)
$6Months_{it} * Same_{it}$	0.0941***	0.0479***	0.0460***	0.0434***
	(0.0127)	(0.0072)	(0.0079)	(0.0093)
$Same_{it} * Reform_t$	0.0882***	0.0338***	0.0362***	0.0385***
	(0.0247)	(0.0104)	(0.0113)	(0.0137)
$6Months * Same_{it} * Reform_t$	-0.1151***	-0.0434***	-0.0508***	-0.0510***
	(0.0324)	(0.0131)	(0.0150)	(0.0182)
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	505,983	505,983	505,983	505,983
R^2	0.010	0.101	0.236	0.446

This table reports pattern in unemployment outflow in 2013 and 2015. The sample is limited to workers with tenure of four to seven months at layoff that return to formal employment seven to ten months after layoff. The dependent variable is a dummy variable that takes the value of one if worker i enters formal employment nine or ten months after being laid off and zero if she returns to formal employment seven or eight 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 $Same_{it}$ takes the value of one if worker i is rehired by the same firm that laid her off, and zero otherwise. The dummy variable $Reform_t$ takes the value of one for the year 2015 and zero for the year 2013. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: **Unemployment Spell Timing and UI System Parameters**

	I	II	III
Dep. Var.:	$P[u_{unjust.}]_{it}$	$P[e \leq 5]_{it}$	$P_{same}[4-9]_{it}$
$6Months_{it}$	-0.0008	-0.0048	0.0284***
	(0.0014)	(0.0127)	(0.0089)
$6Months_{it} * Reform_t$	0.0077***	0.0214*	-0.0282***
	(0.0019)	(0.0111)	(0.0132)
$Surplus_{it}$	-0.0280***	-0.2787***	0.0121**
	(0.0016)	(0.0308)	(0.0053)
$Surplus_{it} * 6Months_{it}$	0.0289***	-0.2242***	0.0264***
	(0.0040)	(0.0348)	(0.0100)
$Surplus_{it} * Reform_t$	-0.0187***	-0.1123***	-0.0072
	(0.0021)	(0.0146)	(0.0075)
$Surplus_{it} * 6Months_{it} * Reform_t$	-0.0135***	0.0904***	-0.0274*
	(0.0041)	(0.0238)	(0.0147)
RR_{it}	0.0873***	0.6539***	-0.0658***
	(0.0041)	(0.0361)	(0.0098)
$RR_{it} * 6Months_{it}$	-0.0247***	0.0296	-0.0333**
	(0.0051)	(0.0345)	(0.0136)
$RR_{it} * Reform_t$	-0.1114***	0.1640***	0.0002
	(0.0134)	(0.0173)	(0.0126)
$RR_{it} * 6Months_{it} * Reform_t$	-0.0079	-0.0980***	0.0393**
	(0.0053)	(0.0260)	(0.0199)
Month*Municipality*Industry*Occupation FE	yes	yes	yes
Clustered SE	muni	muni	muni
Observations	8,512,307	388,745	214,165
R^2	0.131	0.116	0.476

The results in this table document how layoff and hiring patterns vary with parameters with the UI system. In column I, the dependent variable $P[u_{unjust.}]_{it}$ is a dummy variable that is one if a worker i is laid off in month t, and zero otherwise. The dependent variable in column II $P[e \leq 5]_{it}$ is a dummy variable that takes the value of one if worker i returns to formal employment within five months of being laid off and zero otherwise. In column III, the dependent variable $P_{same}[4-9]_{it}$ is a dummy variable that takes the value of one if worker i is reemployed by the same firm four to nine 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, 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 in March and April 2015 and zero for the pre-reform period from January to February 2015. The variable $Surplus_{it}$ is the ratio of UI benefits that worker i is entitled to relative to the sum of the costs of formally employing the worker and layoff costs. The variable RR_{it} is the ratio of monthly benefits accruing to worker i relative to the worker's current wage. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1% 5%, and 10% levels, respectively.

Table 6: **Informal Employment**

	I	II	III	IV	V	VI
Dep. Var.:	$P[informal]_{it}$		$Same_{it}$			
$6Months_{it}$	0.0554**	0.0595**	0.1061*	0.1404**	0.1190**	0.1522**
	(0.0270)	(0.0273)	(0.0574)	(0.0601)	(0.0594)	(0.0615)
$6Months_{it} * Reform_t$	-0.0867**	-0.0896**	-0.2054**	-0.2405***	-0.1436*	-0.1412*
	(0.0405)	(0.0413)	(0.0801)	(0.0823)	(0.0784)	(0.0810)
Month FE	yes	-	yes	-	yes	-
Month*Municipality FE	no	yes	no	yes	no	yes
Observations	1,968	1,968	486	486	467	467
R^2	0.008	0.032	0.034	0.191	0.018	0.165

This table reports probabilities of workers transitioning from formal to informal employment in columns I and II, and being employed or reemployed by the same firm in columns III to VI in the year around the reform using PME survey data. The dependent variable is a dummy variable that takes the value of one if worker i transitions to informal employment following layoff, and zero if she becomes unemployed or transfers to alternative formal employment in columns I and II. In columns III and IV, the dependent variable takes the value of one if an informally employed worker is employed in a firm in the same number of employees bucket as her last formal job, and zero otherwise, in columns V and VI, the dependent variable takes the value of one if a laid off worker returns from informal employment to a firm in the same number of employees bucket as the one that laid her off, and zero if she return to formal employment at a firm in a different number of employees bucket that the one that laid her 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 August 2015 and zero for the pre-reform period from September 2014 to February 2015. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects. ** and * denote statistical significance at the 5%, and the 10% levels, respectively.

Table 7: **Replacement Hiring**

Dep. Var.: $P[replacement\ hire]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0258***	-0.0193***	-0.0154***	-0.0146***
	(0.0030)	(0.0030)	(0.0029)	(0.0035)
$6Months_{it} * Reform_t$	0.0175***	0.0104**	0.0080**	0.0119**
	(0.0041)	(0.0040)	(0.0040)	(0.0048)
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	255,283	255,283	255,283	255,283
R^2	0.001	0.061	0.193	0.423

This table reports changes in the hiring of a replacement worker within one month of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker i, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or 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. ***, and ** denote statistical significance at the 1%, and the 5% levels, respectively.

Table 8: **Formal Hiring, Employment, and Wages**

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

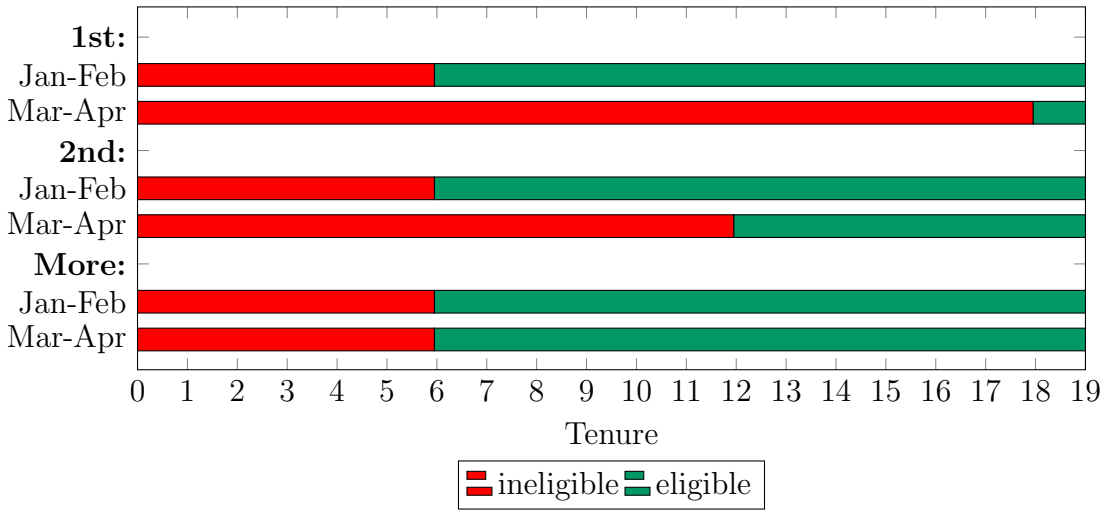
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired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

Table 9: **Formal Hiring, Employment, Wages, and Strategic Unemployment**

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

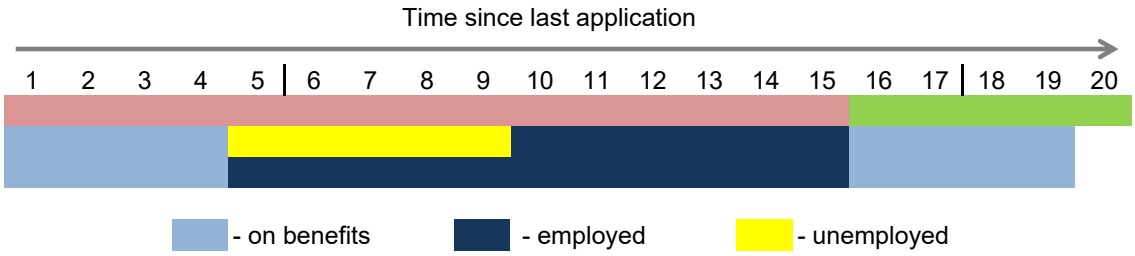
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired 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 dummy variable *Strategic* is the pre-reform share of workers in a given municipality-industry cell that is laid off with a tenure of six or seven months in all workers laid off with a tenure of four to seven months. The dummy variable *Collusive* is the pre-reform share in workers rehired by the same firm after four to nine months among workers laid off with a tenure of six or seven months. Standard errors are clustered at the municipality level. *** and ** denote statistical significance at the 1% and the 5% levels, respectively.

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



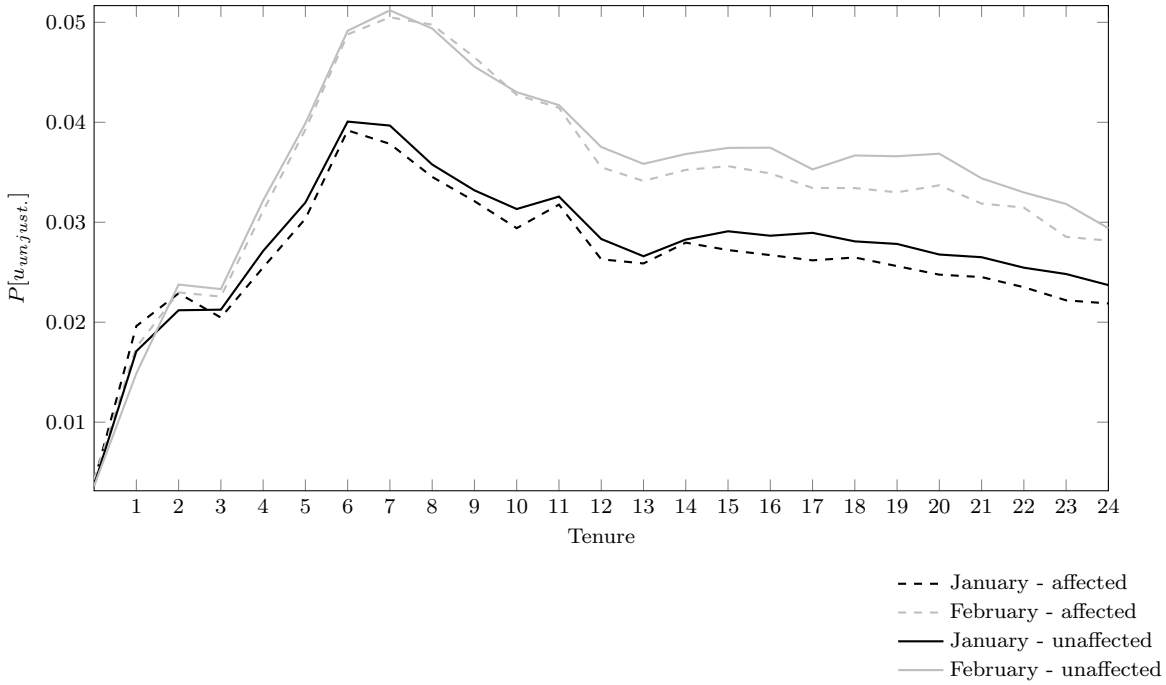
This figure illustrates eligibility criteria for UI benefits before and after the reform, for workers with different tenures who apply for UI benefits for the first time, the second time, and the third time or more. Red areas indicate tenure not satisfying eligibility criteria, green areas indicate tenure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

Figure 2: **Repeated UI Eligibility Before the Reform**



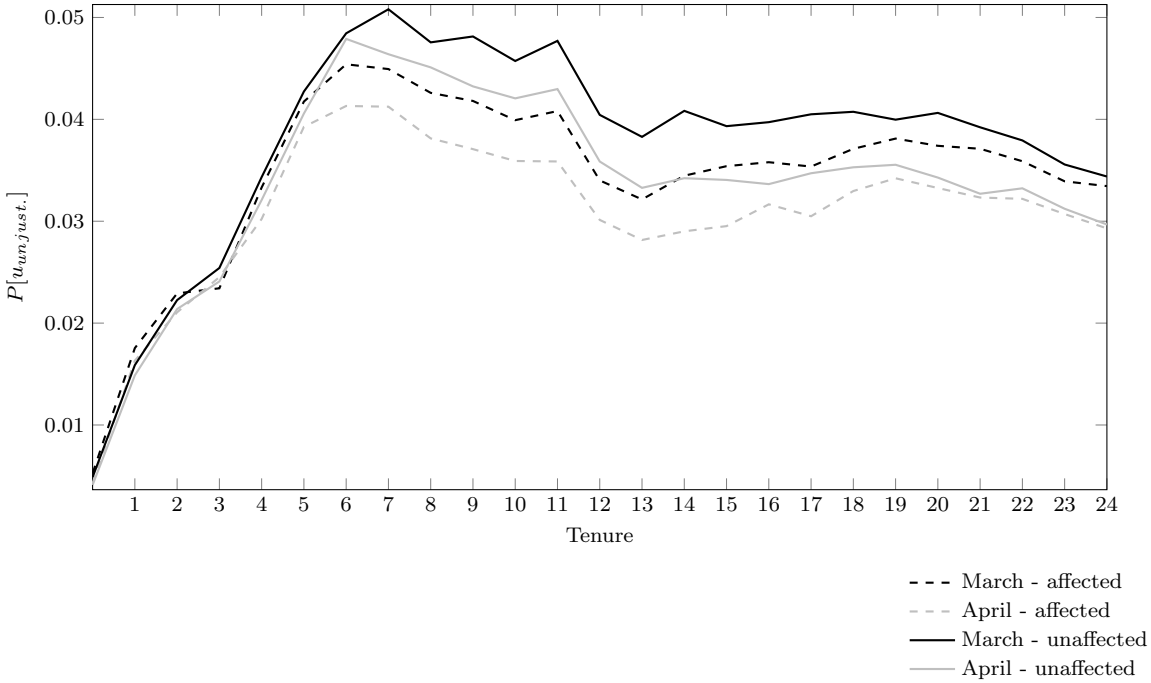
This figure illustrates eligibility for UI benefits before the passage of the reform. The top line indicates that workers are ineligible to apply for fifteen months after a successful UI benefits application. The second and third line provide examples of repeated UI benefits applications for workers eligible for UI benefits. The second line illustrates the case with the longest formal unemployment spell that allows applying for UI benefits twice within sixteen months, the third line illustates the case with the longest formal employment spell that allows applying for UI benefits twice within sixteen months.

Figure 3: **Unemployment Inflow by Tenure - Affected and Unaffected**



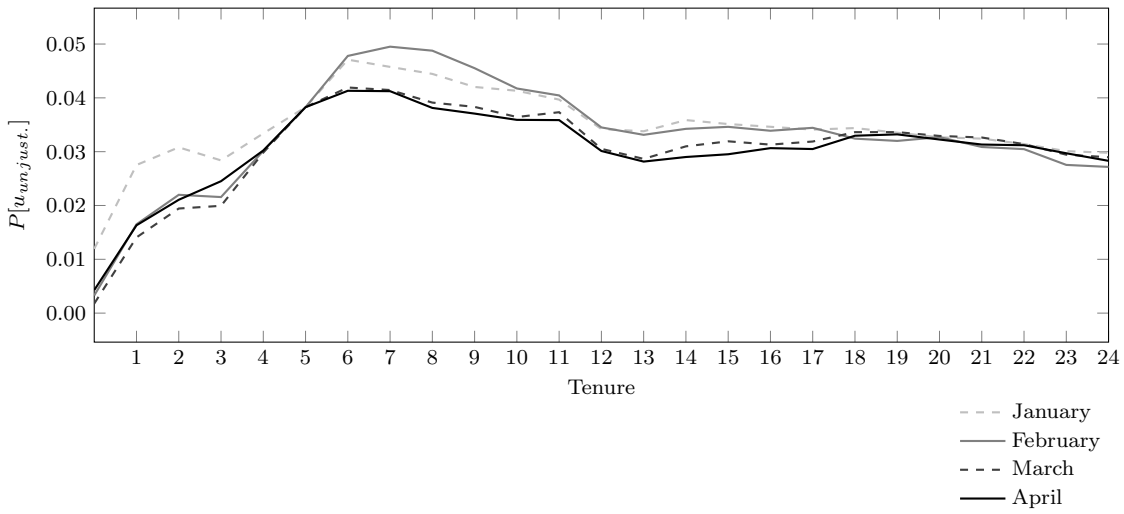
This figure depicts the probability of workers with different tenure to be laid off for workers affected by the reform (less than two UI spells in the past - dashed lines) and workers unaffected by the reform (at least two UI benefits spells in the past - solid lines) for the months from January and February 2015, separately.

Figure 4: **Unemployment Inflow by Tenure - Affected and Unaffected**



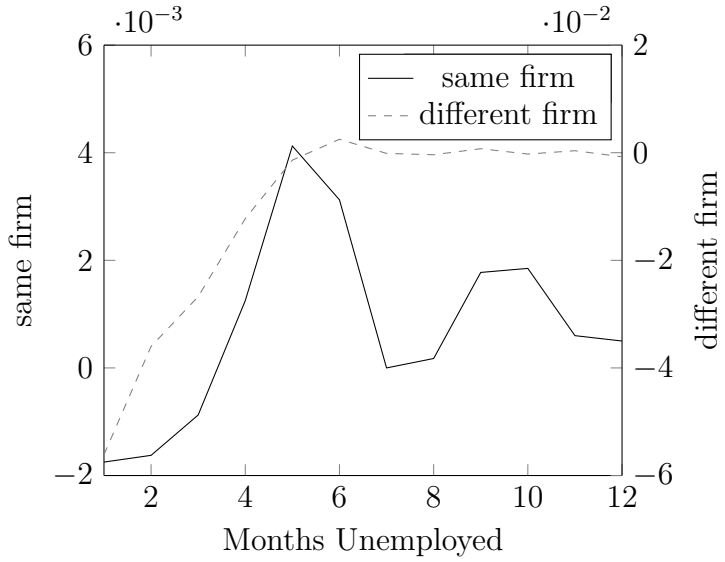
This figure depicts the probability of workers with different tenure to be laid off for workers affected by the reform (less than two UI spells in the past - dashed lines) and workers unaffected by the reform (at least two UI benefits spells in the past - solid lines) for the months from March and April 2015, separately.

Figure 5: **Unemployment Inflow by Tenure**



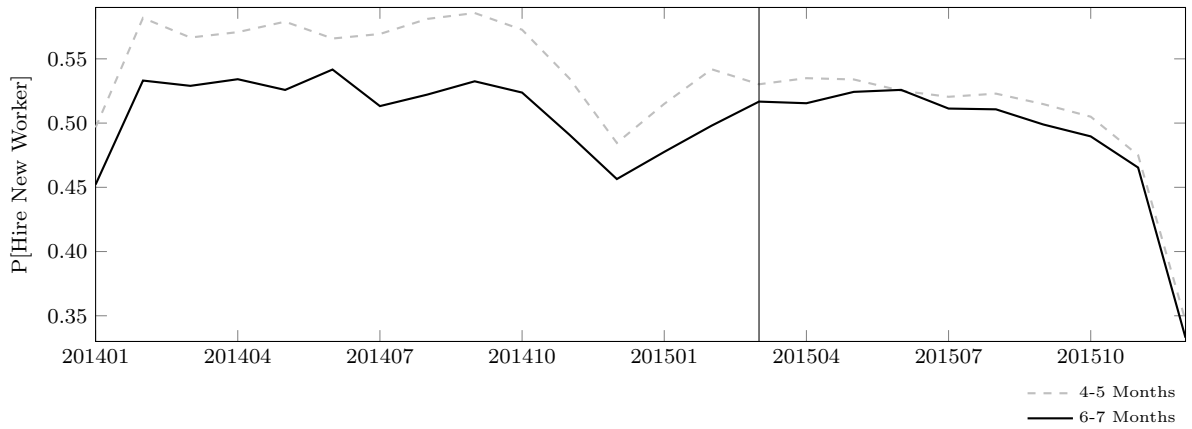
This figure depicts the probability of workers with different tenures being laid off for the months from January to April 2015, separately. To facilitate comparison, the plots are vertically aligned at the April probability of layoff for workers with a tenure of five months.

Figure 6: **Unemployment Outflow by Tenure - Same vs. Different Firms**



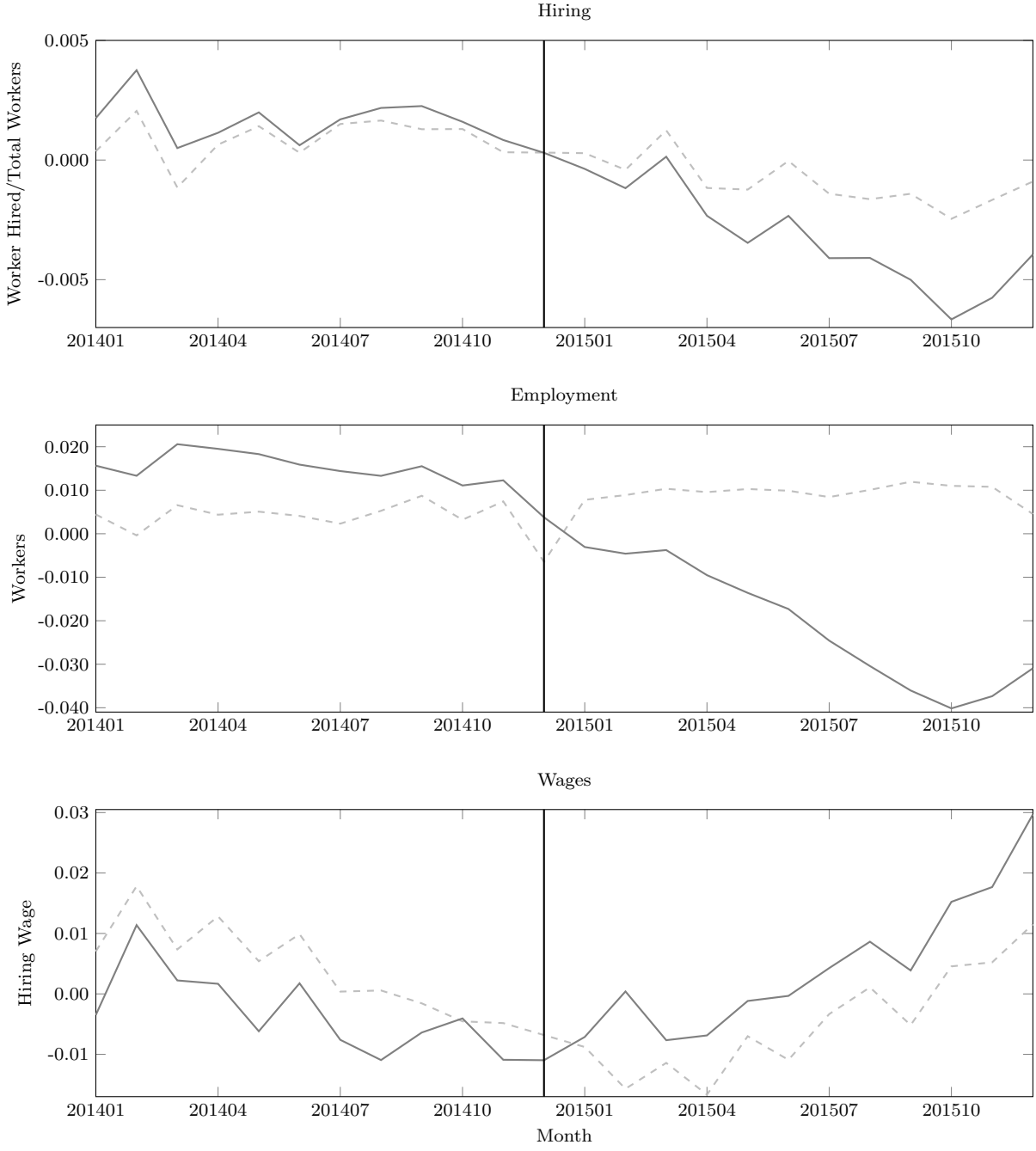
This figure depicts the difference in probability of reemployment for worker that were laid off with a tenure of six months and workers laid off with a tenure of five months for the year of 2013. The left y-axis depicts the difference in the proability to be employed by the same firm that laid off the worker, the right y-axis depict the difference in the probability to be hired by a different firm than the firm that laid off the worker. The x-axis depicts the number of months since layoff.

Figure 7: **Layoffs and Replacement Hires**



This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015. The vertical line indicates the implementation of the UI reform in March 2015.

Figure 8: **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 less than two successful past UI applications (solid lines) and workers with two or more successful past applications. In the top panel, the y-axis reports the share of workers hired relative to the total number of workers, in the 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 calendar month fixed effects and the average value of the y-axis variable over the full sample period for each group. The vertical line indicates the announcement of the UI reform in December 2014.

Appendix A. Additional Figures and Tables

Table A.1: **Informality by Industry**

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

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

Table A.2: **Unemployment Inflow - Substitution**

Dep. Var.: $P[u_{other}]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0075***	-0.0077***	-0.0073***	-0.0067***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)
$6Months_{it} * Reform_t$	0.0005	-0.0005	-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	8,532,451	8,532,451	8,532,451	8,532,451
R^2	0.000	0.022	0.047	0.110

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.3: **Unemployment Inflow - Seasonality**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0130***	0.0125***	0.0124***	0.0126***
	(0.0007)	(0.0006)	(0.0005)	(0.0005)
$6Months_{it} * 2015_t$	-0.0001	-0.0001	-0.0001	-0.0003
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
$6Months_{it} * Reform_t$	0.0003	0.0004	0.0006*	0.0007**
	(0.0006)	(0.0004)	(0.0003)	(0.0003)
$6Months_{it} * 2015_t * Reform_t$	-0.0055***	-0.0058***	-0.0058***	-0.0056***
	(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	17,580,342	17,580,342	17,580,342	17,580,342
R^2	0.001	0.017	0.052	0.125

This table compares changes in unemployment inflow around the enactment of the UI benefits reform from January to April 2015 against the period from January 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.

Table A.4: **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 having received UI benefits at least twice 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.5: **Unemployment Inflow - Excluding Five Months Tenure Workers**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0158***	0.0154***	0.0153***	0.0155***
	(0.0008)	(0.0007)	(0.0006)	(0.0007)
$6Months_{it} * Reform_t$	-0.0043***	-0.0044***	-0.0046***	-0.0043***
	(0.0008)	(0.0008)	(0.0007)	(0.0007)
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	5,714,331	5,714,331	5,714,331	5,714,331
R^2	0.002	0.017	0.059	0.146

This table reports changes in unemployment inflow around the announcement of the UI reform. The sample is limited to workers with tenure of four, six, or 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 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 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.6: **Unemployment Inflow - Announcement Effects**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0130***	0.0126***	0.0129***	0.0129***
	(0.0007)	(0.0007)	(0.0006)	(0.0006)
$6Months_{it} * Reform_t$	-0.0053***	-0.0055***	-0.0059***	-0.0055***
	(0.0009)	(0.0007)	(0.0007)	(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	8,729,104	8,729,104	8,729,104	8,729,104
R^2	0.001	0.018	0.065	0.143

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.

Table A.7: **Unemployment Inflow by Informality**

	I	II	III	IV
Dep. Var.: $P[u_{unjust.}]_{it}$	Industry-Level			
$6Months_{it}$	-0.0008			
	(0.0012)			
$6Months_{it} * Reform_t$	0.0003			
	(0.0015)			
$6Months_{it} * Informal$	0.0548***	0.0476***	0.0409***	0.0420***
	(0.0053)	(0.0048)	(0.0038)	(0.0037)
$6Months_{it} * Reform_t * Informal$	-0.0242***	-0.0183***	-0.0168***	-0.0174***
	(0.0064)	(0.0058)	(0.0048)	(0.0046)
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	8,532,451	8,532,451	8,532,451	8,532,451
R^2	0.023	0.027	0.056	0.130
Dep. Var.: $P[u_{unjust.}]_{it}$	Municipality-Level			
$6Months_{it}$	0.0049***			
	(0.0010)			
$6Months_{it} * Reform_t$	0.0017			
	(0.0010)			
$6Months_{it} * Informal$	0.0317***	0.0232***	0.0237***	0.0222***
	(0.0032)	(0.0033)	(0.0029)	(0.0031)
$6Months_{it} * Reform_t * Informal$	-0.0299***	-0.0273***	-0.0265***	-0.0241***
	(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	8,532,451	8,532,451	8,532,451	8,532,451
R^2	0.023	0.023	0.053	0.127

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% levels, respectively.

Table A.8: **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 A.9: **Unemployment Outflow - Seasonality**

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
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 against the period from January 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% levels, respectively.

Table A.10: **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 having received UI benefits at least twice 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.11: **Collusion - Seasonality**

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
R^2	0.003	0.205	0.322	0.467

This table compares changes in reemployment of workers by the same firm after the end of their UI benefit eligibility around the enactment of the UI reform from January to April 2015 against the period from January to April 2014. The sample is limited to workers with tenure of four to seven months at layoff who are rehired within nine months. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployed by the same firm four to nine months after being laid off, and zero if worker i is 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% levels, respectively.

Table A.12: **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 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	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 end of their UI benefit eligibility around the enactment of the UI reform from January to April 2015, for workers having received UI benefits at least twice in the past. The sample is limited to workers with tenure of four to seven months at layoff who 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 reemployed by the same firm four to nine months after being laid off, and zero if worker i is 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.13: **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
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 reemployed by the same firm four to nine months after being laid off, and zero if worker i is 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% levels, respectively.

Table A.14: **Replacement Hiring - One Year Around Reform**

Dep. Var.: $P[replacement\ hire]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0258***	-0.0208***	-0.0187***	-0.0174***
	(0.0019)	(0.0030)	(0.0029)	(0.0035)
$6Months_{it} * Reform_t$	0.0211***	0.0170***	0.0159***	0.0154***
	(0.0025)	(0.0025)	(0.0025)	(0.0029)
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	688,693	688,693	688,693	688,693
R^2	0.004	0.063	0.196	0.425

This table reports changes in the hiring of a replacement worker within one month of the layoff of another worker by the same firm around the enactment of the UI reform from September 2014 to August 2015. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker i, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or 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 from March to August 2015 and zero from September 2014 to August 2015. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, and ** denote statistical significance at the 1%, and the 5% levels, respectively.

Table A.15: **Replacement Hiring - Unaffected Workers**

Dep. Var.: $P[replacement\ hire]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0266***	-0.0221***	-0.0203***	-0.0202***
	(0.0031)	(0.0031)	(0.0034)	(0.0043)
$6Months_{it} * Reform_t$	0.0073	0.0037	0.0019	0.0041
	(0.0044)	(0.0045)	(0.0048)	(0.0057)
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	176,237	176,237	176,237	176,237
R^2	0.001	0.069	0.217	0.460

This table reports changes in the hiring of a replacement worker within one month of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015 for workers who receive UI benefits at least twice in the past. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within one month of laying off worker i, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six 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. ***, and ** denote statistical significance at the 1%, and the 5% levels, respectively.

Table A.16: **Replacement Hiring by Informality**

	I	II	III	IV
Dep. Var.: $P[replacement\ hire]_{it}$	Industry-Level			
$6Months_{it}$	0.0082			
	(0.0097)			
$6Months_{it} * Reform_t$	-0.0123			
	(0.0065)			
$6Months_{it} * Informal$	-0.1028***	-0.1074***	-0.1183***	-0.1364***
	(0.0352)	(0.0366)	(0.0403)	(0.0435)
$6Months_{it} * Reform_t * Informal$	0.0850**	0.1014**	0.1375**	0.1385**
	(0.0485)	(0.0503)	(0.0545)	(0.0678)
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	255,283	255,283	255,283	255,283
R^2	0.072	0.100	0.214	0.438
Dep. Var.: $P[replacement\ hire]_{it}$	Municipality-Level			
$6Months_{it}$	0.0067			
	(0.0093)			
$6Months_{it} * Reform_t$	-0.0129			
	(0.0128)			
$6Months_{it} * Informal$	-0.1061***	-0.1104***	-0.0903**	-0.1175**
	(0.0363)	(0.0369)	(0.0411)	(0.0528)
$6Months_{it} * Reform_t * Informal$	0.0955*	0.1081**	0.1058*	0.1454*
	(0.0524)	(0.0540)	(0.0599)	(0.0839)
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	255,283	255,283	255,283	255,283
R^2	0.072	0.073	0.194	0.423

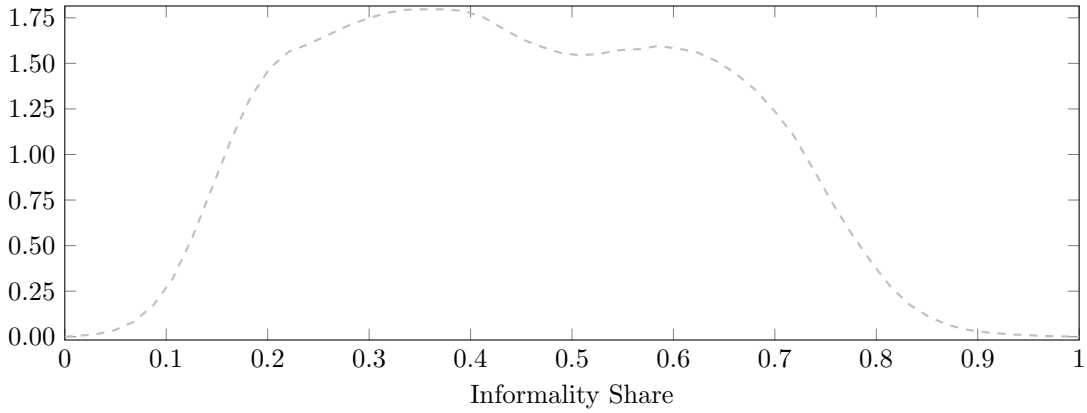
This table reports changes in the hiring of a replacement worker within one month of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker i, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or 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% levels, respectively.

Table A.17: **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

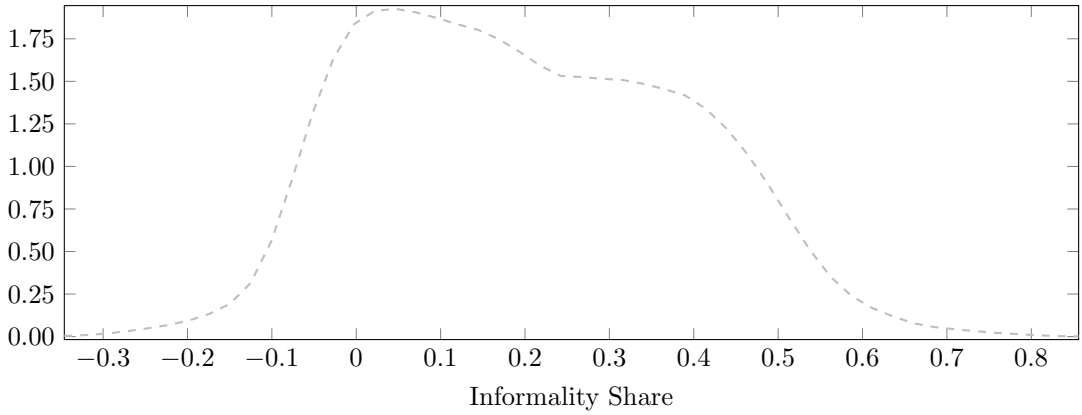
This table reports changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the industry-municipality-month level. In columns I and II, the dependent variable is the share of workers hired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

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



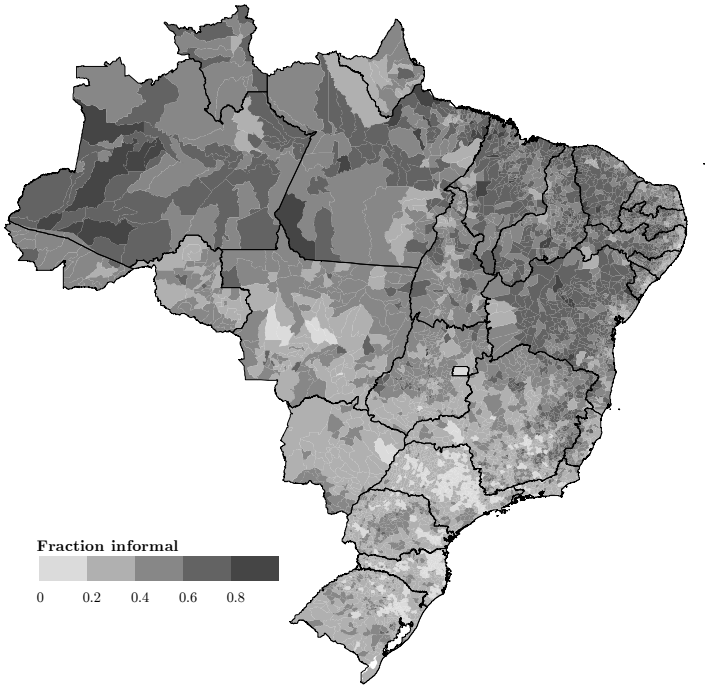
This figure depicts the distribution of the share of informal in total workers across all municipalities in Brazil from the Census Brazil.

Figure A.2: **Distribution of Labor Market Informality across Municipalities - Industry Control**



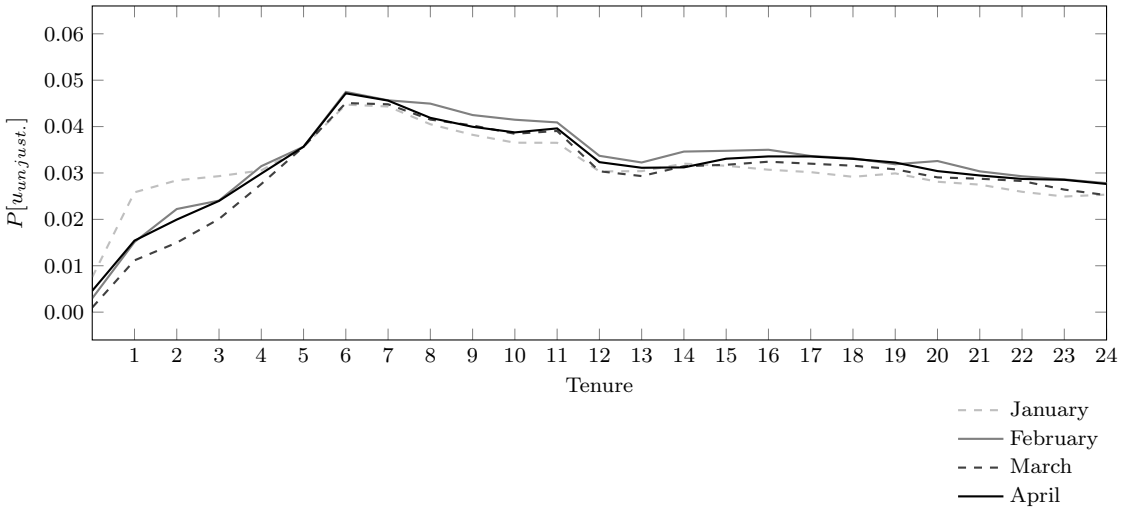
This figure depicts the distribution of difference in the share of informal in total workers and the same share as predicted from the industry composition of the respective municipality across all municipalities in Brazil.

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



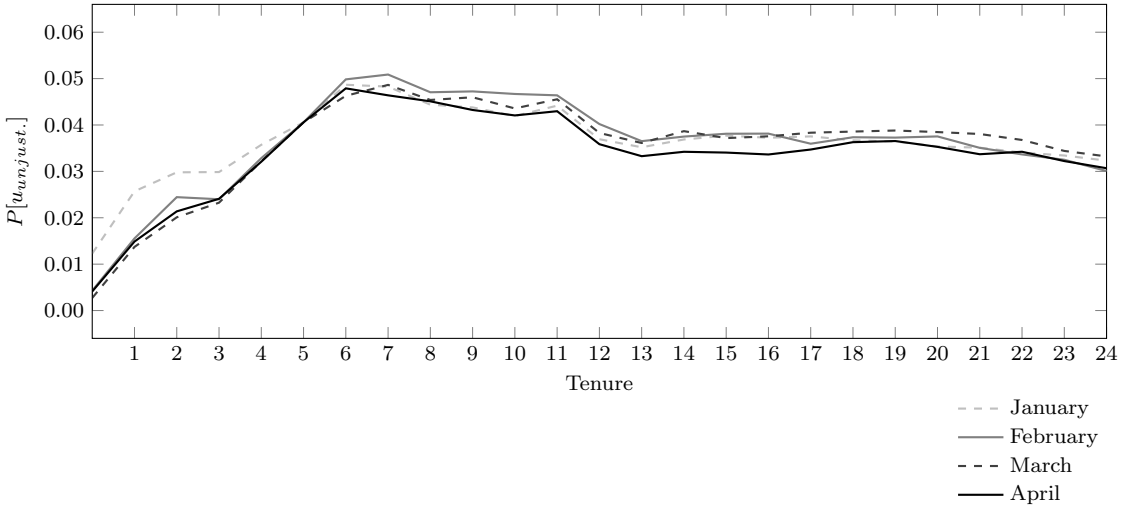
This figure depicts the share of informal in total workers for all municipalities in Brazil from the Census Brazil.

Figure A.4: **Unemployment Inflow by Tenure - Previous Year**



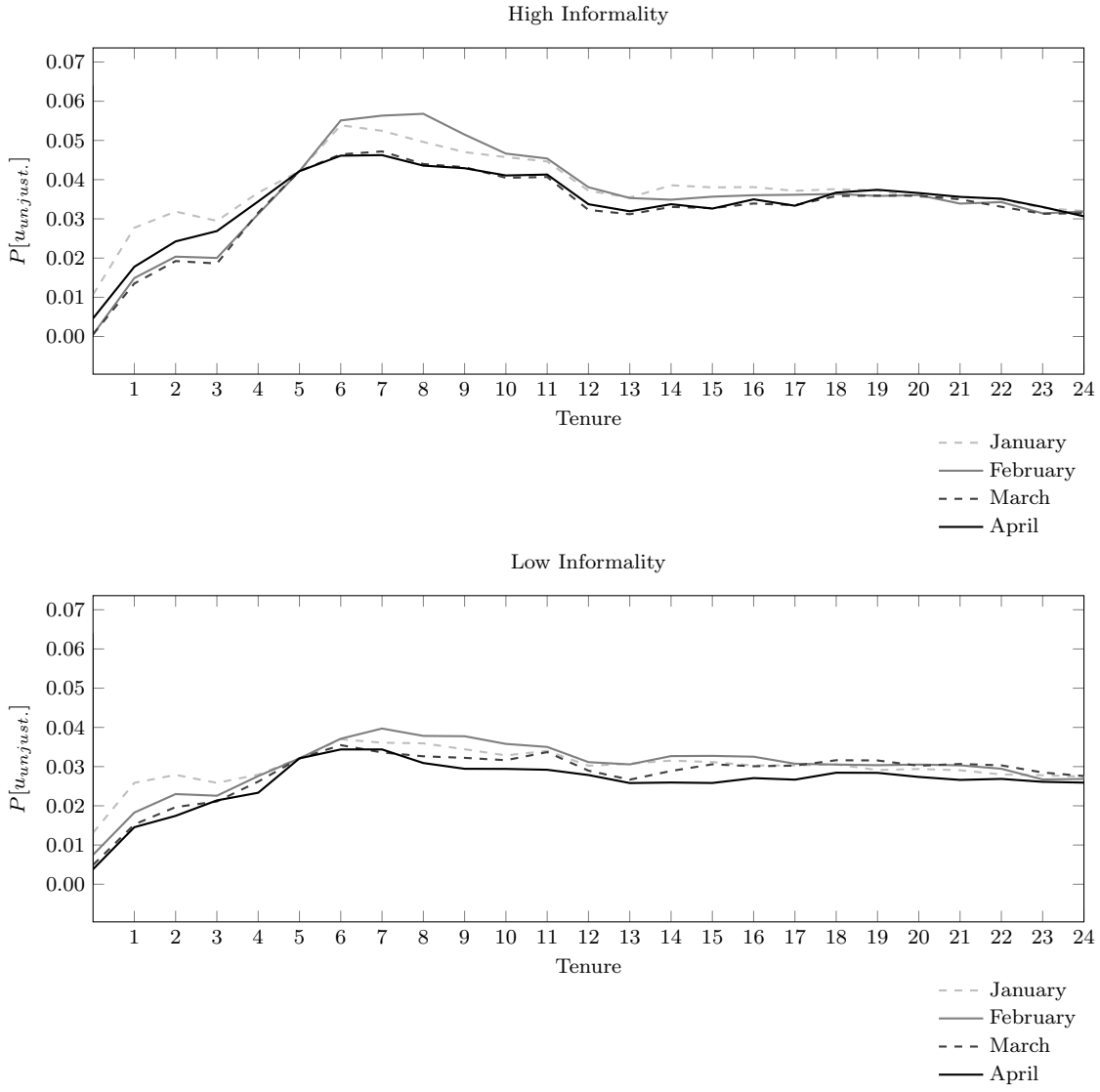
This figure depicts the probability of workers with different tenures being laid off for the months from January to April 2014, separately. To facilitate comparison, the plots are aligned at the April probability of layoff for workers with a tenure of five months.

Figure A.5: **Unemployment Inflow by Tenure - Unaffected Workers**



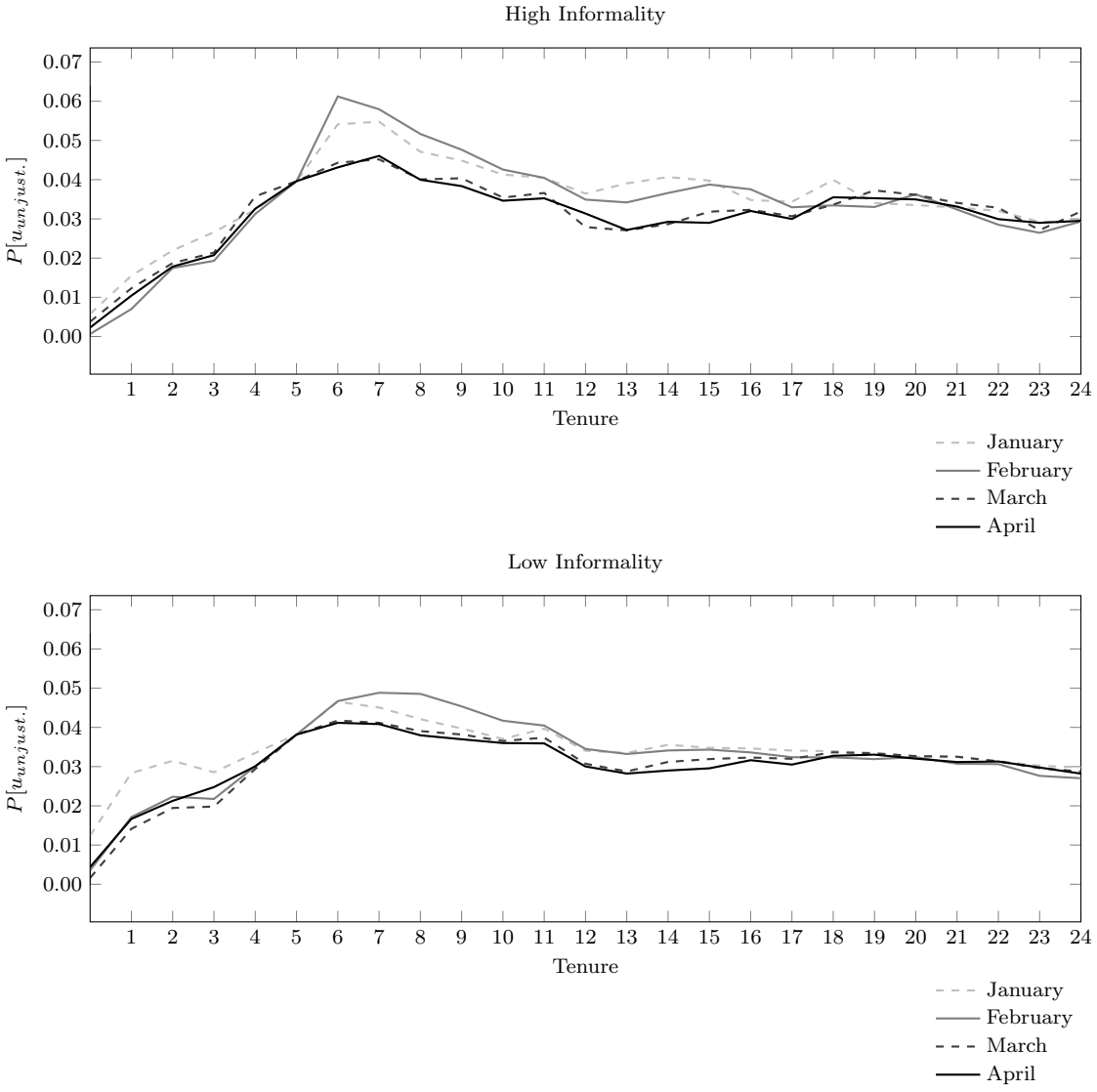
This figure depicts the probability of workers who received UI benefits at least twice in the past with different tenures being laid off for the months from January to April 2015, separately. To facilitate comparison, the plots are aligned at the April probability of layoff for workers with a tenure of five months.

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



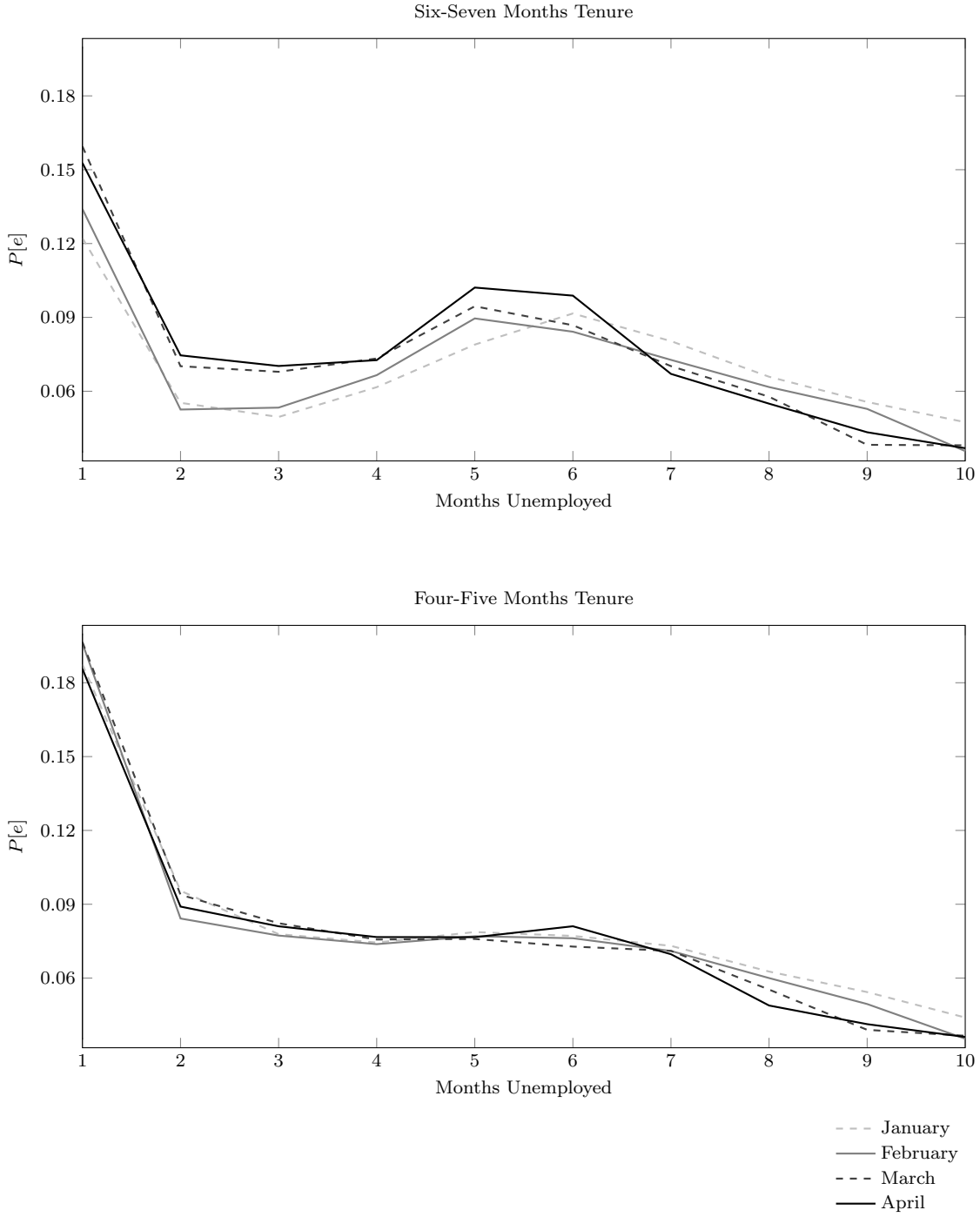
This figure depicts the probability of workers with different tenure being laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in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

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



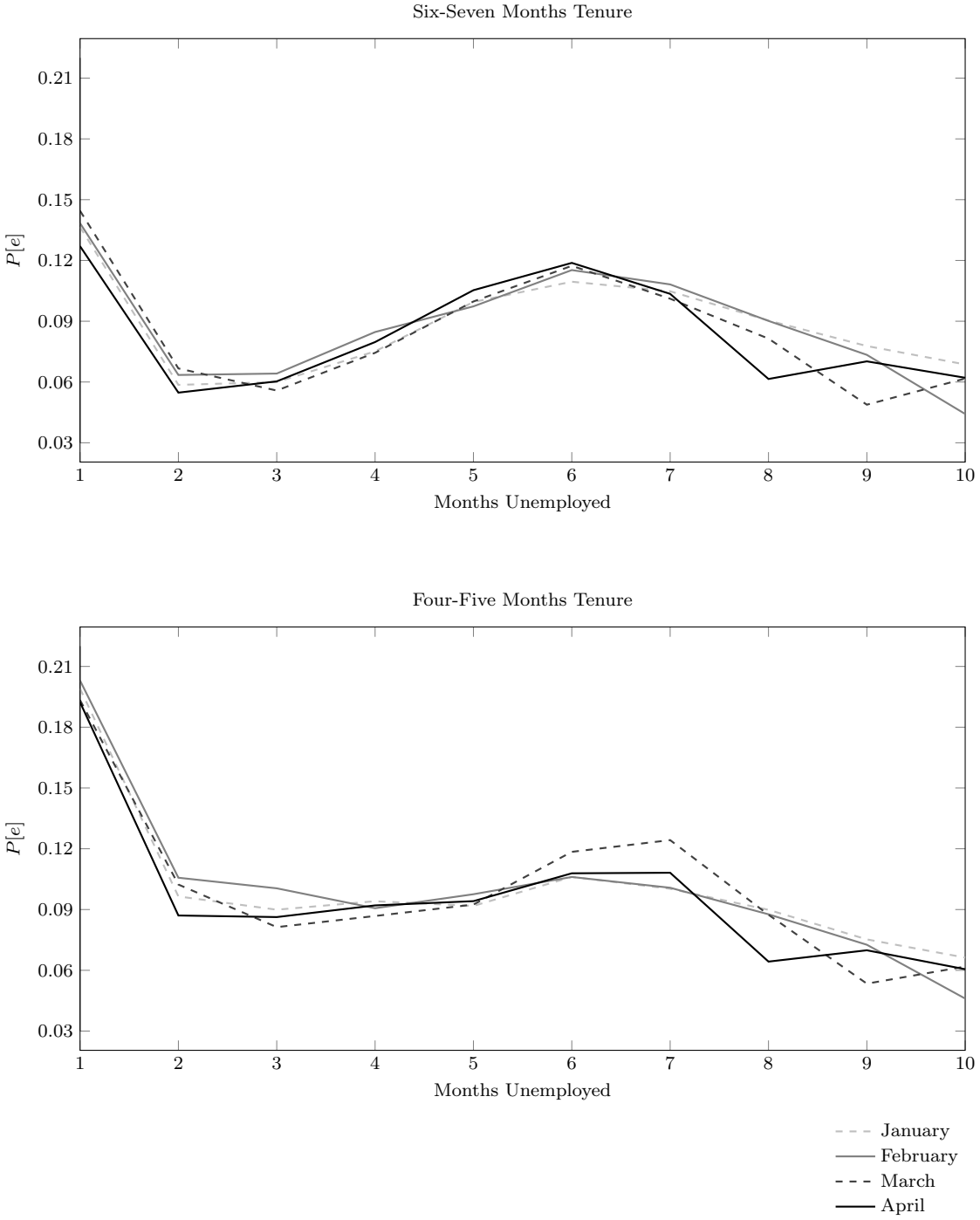
This figure depicts the probability of workers with different tenures being laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in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

Figure A.8: **Unemployment Outflow by Tenure**



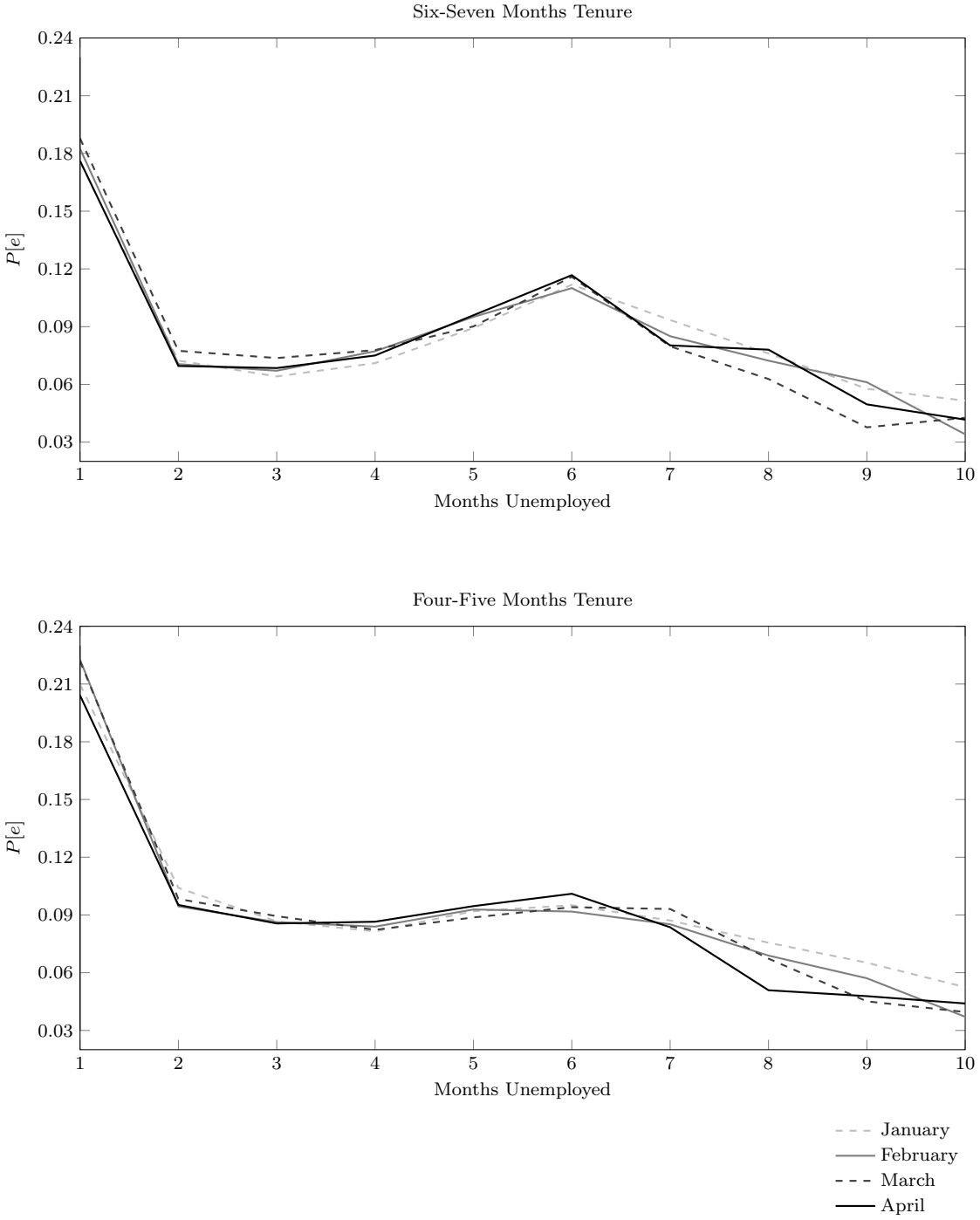
This figure depicts the probability of reemployment in the months after losing their job for workers with a tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2015.

Figure A.9: **Unemployment Outflow by Tenure - Previous Year**



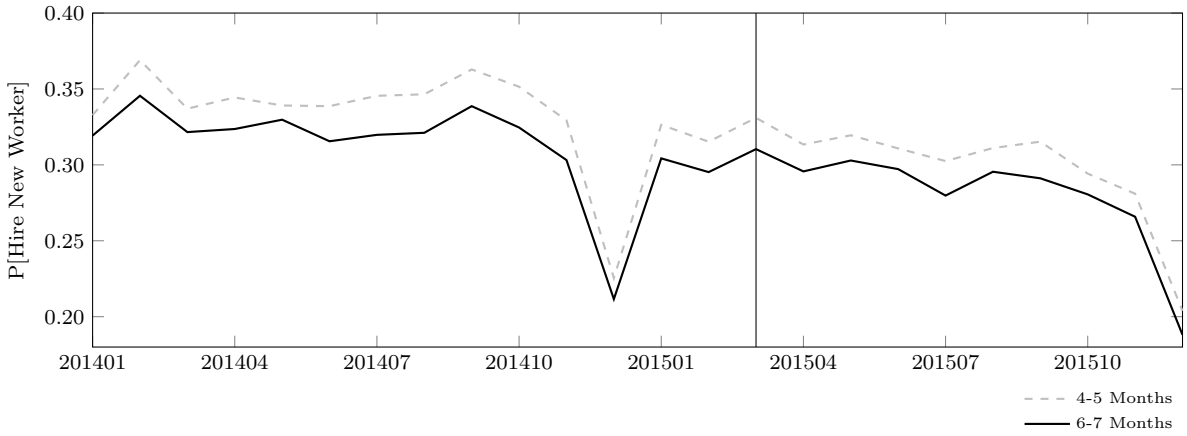
This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2014.

Figure A.10: **Unemployment Outflow by Tenure - Unaffected Workers**



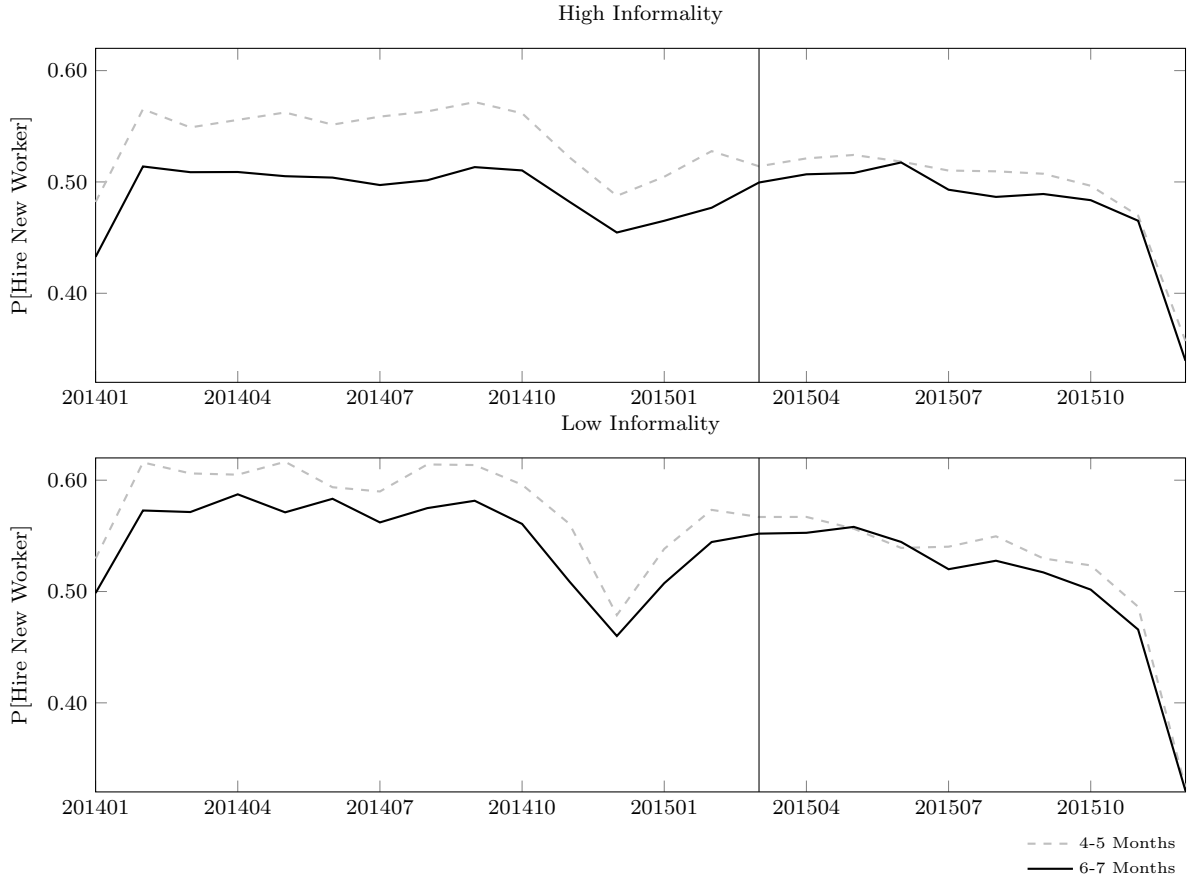
This figure depicts the probability of reemployment in the months after layoff for workers who received UI benefits at least twice in the past with tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2015.

Figure A.11: **Layoffs and Replacement Hires - Unaffected Workers**



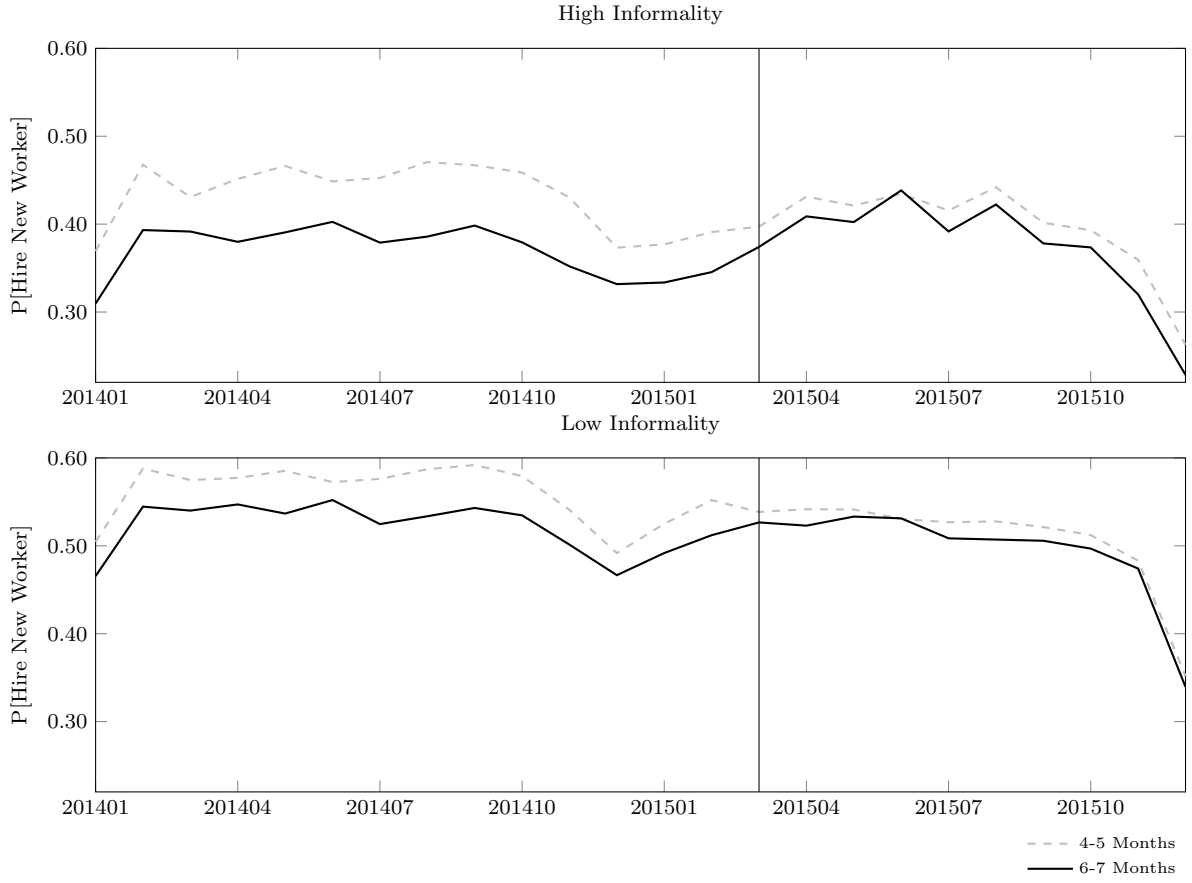
This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015 for workers who received UI benefits at least twice in the past. The vertical line indicates the implementation of the UI reform in March 2015.

Figure A.12: **Layoffs and Replacement Hires by Informality - Industry Level**



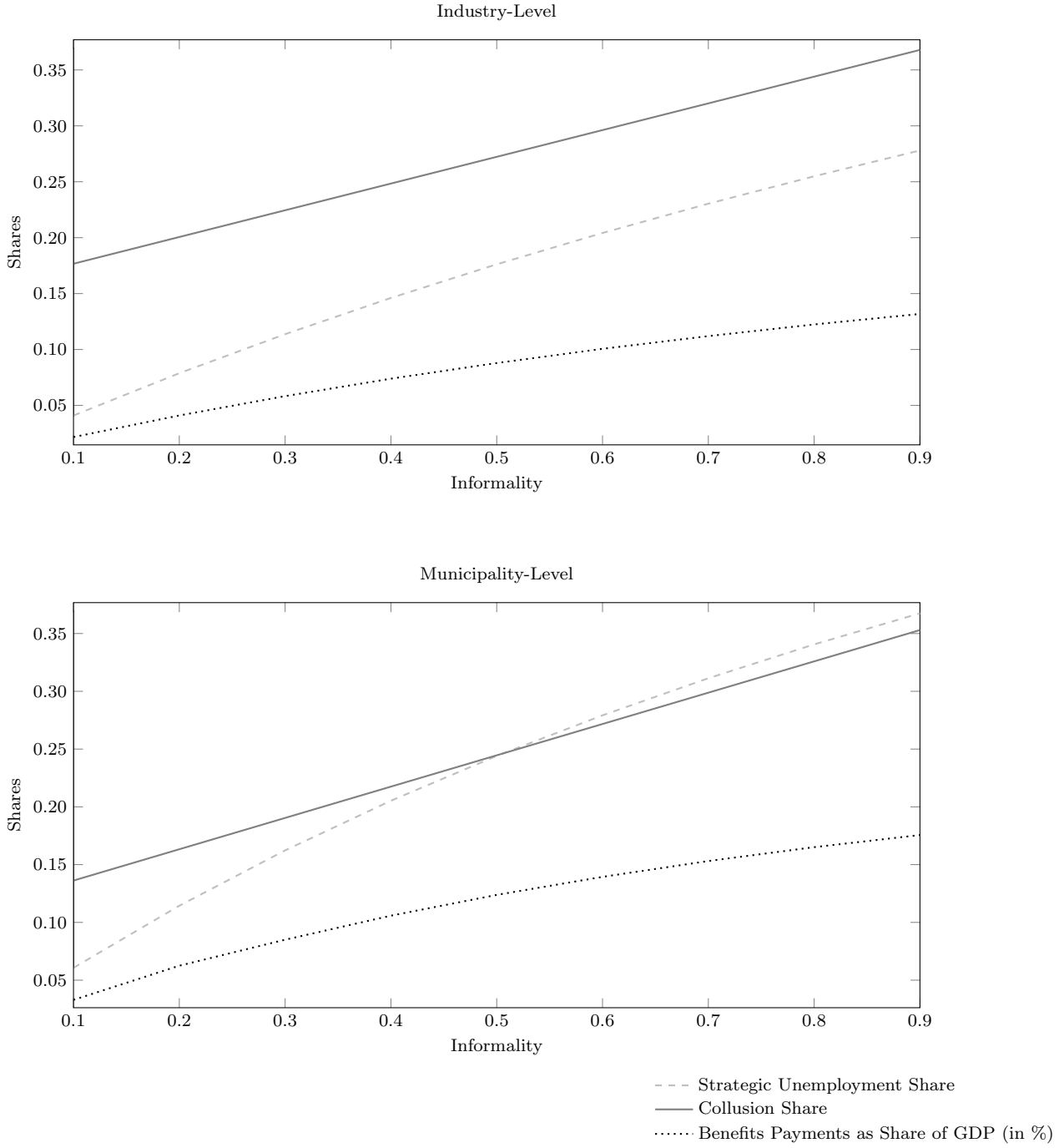
This figure depicts the probability of firms hiring a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015 for industries with above median levels of labor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

Figure A.13: **Layoffs and Replacement Hires by Informality - Municipality Level**



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

Figure A.14: **Strategic Unemployment and Firm-Worker Collusion Shares by Informality**



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