

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

Bernardus Van Doornik[†]
David Schoenherr[‡] Janis Skrastins[§]

December 19, 2017

The views expressed in the paper are those of the authors and should not be cited to reflect the view of the Banco Central do Brasil.

Abstract

Exploiting a sharp discontinuity in the application of an unexpected unemployment insurance (UI) reform in Brazil, we find that workers are more likely to be laid off when they are eligible for UI benefits, accounting for twelve percent of unemployment inflow around the eligibility threshold. We document layoff and rehiring patterns consistent with collusion between firms and workers to time unemployment spells with eligibility for UI benefits, explaining at least twenty percent of the additional unemployment inflow. Firms seem to benefit from strategic behavior by paying lower equilibrium wages. Collusive patterns increase with the potential rents that can be extracted from the UI system. When laying off workers eligible for UI benefits, firms are less likely to hire a replacement worker. This suggests that firms continue to employ workers informally while on benefits, and is consistent with all documented effects being stronger in the presence of large informal labor markets.

JEL Codes: J21, J22, J46, J65, K31.

Keywords: unemployment insurance, labor supply, collusion, law and economics.

*We thank Orley Ashenfelter, Will Dobbie, Henry Faber, Maryam Farboodi, Dimas Fazio, Ilyana Kuziemko, Camille Landais, Alexandre Mas, Adrien Matray, Jean-Marie Meier, Atif Mian, Andrew Samwick, Jan Starmans, Adam Szeidl, Motohiro Yogo, and seminar participants at the Banco Central do Brasil, Bank of Latvia, Central European University, Chilean Banking Authority, Institute of Applied Economic Research (IPEA) Brazil, Northwestern University, Princeton University, PUC Rio de Janeiro, SSE Riga, Syracuse University, University of Chile, Washington University in St. Louis, the 2017 Annual Inflation Targeting Seminar of the Banco Central do Brasil, the 2017 Asian, European, Latin American, and North American Meetings of the Econometric Society, the 2017 Boulder Summer Conference, the 2017 German Economists Abroad Meeting, the 2017 Fall Meeting of the Labor and Finance Group, the 2017 Northeastern Finance Conference, and the 2017 Annual Conference of the SOIE for many helpful comments and suggestions. Karine Paiva provided excellent research assistance.

[†]Banco Central do Brasil, Setor Bancario Sul Q.3 BL B - Asa Sul, Brasilia DF 70074-900, bernardus.doornik@bcb.gov.br

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

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

1 Introduction

Experiencing negative shocks to labor income is one of the most salient risks faced by households.¹ To dampen the adverse effects of job loss on households' incomes, government-mandated unemployment insurance (UI) programs have been in place in developed countries for many decades, and are increasingly implemented in mid-income and developing countries. It is well documented that UI benefits have adverse incentive effects on search intensities for reemployment,² and there is some evidence of a positive relationship between workers' eligibility for UI benefits and layoff intensities (Christofides and McKenna 1995, 1996; Green and Riddell 1997; Baker and Rea 1998; Green and Sargent 1998; Jurajda 2002; Rebello-Sanz 2012). The main trade off in designing UI systems is to minimize these disincentive effects, while providing workers with insurance against adverse income shocks.

An important and unresolved question with direct policy implications is to understand the drivers of higher layoff intensities of worker eligible for UI benefits, and more broadly, which features of UI design encourage and facilitate the timing of unemployment spells with UI benefits eligibility. Empirical studies on the effects of different aspects of UI design on unemployment inflow are scarce and provide mixed evidence.³ They often lack a long time series of employer-employee matched data with information on workers' employment history and wages. Additionally, researchers grapple with unobserved actions and selection issues making it difficult to identify the role of firms in timing unemployment spells with eligibility for UI. For example, higher layoff intensities may be elicited by shirking, or workers may select into jobs with different expected employment duration under different UI regimes.⁴

In this paper, we exploit a sudden and unanticipated UI reform in Brazil in 2015, to examine the effects of UI benefits on labor supply and the role of collusion between firms and workers. We find strong evidence that unemployment inflow and outflow are timed to

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

²Solon (1979), Moffitt (1985), Katz and Meyer (1990), Meyer (1990, 1995), Card and Levine (2000), Meyer and Mok (2007), Card et al. (2015a), Farber, Rothstein, and Valletta (2015), Johnston and Mas (2015), and Landais (2015) for the U.S., and Card, Chetty, and Weber (2007), Lalive (2008), Schmieder, von Wachter, and Bender (2012, 2016), and Card et al. (2015b) for Western Europe.

³Feldstein (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 duration is extended. Jurajda (2002) finds that higher benefits levels have no effect on layoff intensities.

⁴Green and Riddell (1997) document that workers select less into seasonal jobs with shorter employment duration when a longer employment duration is required to qualify for UI benefits.

coincide with eligibility for UI benefits. Firms lay off workers when they become eligible for UI benefits, and rehire them just when benefits run out. Firms seem to benefit from participating in strategic layoff and rehiring decisions by paying lower equilibrium wages. We find that a ten percentage points increase in potential rents that firms and workers can extract from the UI system by timing unemployment spells with UI eligibility increases collusive layoff and rehiring patterns by eighteen percent. Additionally, firms are less likely to hire a replacement worker when laying off a worker who is eligible for UI benefits, in particular in industries and regions with large informal labor markets, which suggests that firms continue to employ workers informally while they receive UI benefits.

The UI reform was announced on December 29, 2014 and implemented as a provisional measure on March 1, 2015. The announcement of the reform came as a surprise *after* affected workers entered formal employment. This eliminates concerns about ex ante selection into employment affecting our findings. Moreover, the reform only affects workers' eligibility for UI benefits, but not firms' contributions to the UI program. This allows us to isolate the effects of UI benefits eligibility free from direct changes in the cost of formal employment. 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 were eligible for UI benefits. To obtain the same benefits after the reform, workers applying for benefits for the first (second) time require formal employment for 18 (12) months during the previous 24 (16) months. Thus, 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 below and just above the six months 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 relatively drops by twelve percent for workers just above the six months threshold who lose eligibility for UI benefits after the reform. Additionally, we find that dismissed workers return to formal employment more quickly when they lose eligibility for UI benefits after the reform. Specifically, dismissed workers with a tenure just above the six months threshold who lose eligibility for UI benefits, are about fourteen percent more likely to return to formal employment within five months of layoff (maximum duration of

benefits) after the reform, compared to workers that are ineligible for benefits both before and after the reform.

Since UI eligibility is conditional on layoff, a natural question is what role firms play in timing unemployment inflow and outflow, and which parameters of the UI system encourage such behavior. The theoretical literature provides two rationales for implicit agreements between firms and workers leading to 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 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 optimize layoff decisions to coincide with workers' eligible for UI benefits to establish a positive reputation in local labor markets eliciting higher labor supply.⁵ While the motivation for collusion slightly differs between these models, they all imply that firms time unemployment spells with workers' eligibility for UI to extract rents from the system.

To assess the presence of strategic behavior on part of firms, we examine layoff and rehiring patterns consistent with models that imply collusion between firms and workers. We observe an extreme form of unemployment timing. Firms lay off workers when they become eligible for UI benefits, and rehire them when benefits run out. 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 benefits run out. After the reform, when workers with a tenure of six months lose eligibility for UI benefits, these patterns vanish. This precise timing of unemployment spells with 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 points increase in the ratio between total benefits payments and formal employment and layoff costs leads to an eighteen percent increase in the timing of unemployment spells with UI benefits eligibility.

We next turn to examining how firms benefit from timing unemployment spells with eligibility for UI benefits. Implicit contracting mechanisms (Feldstein 1976; Baily 1977; Christofides and McKenna 1996) predict that firms time workers' unemployment spells with eligibility for UI benefits to pay lower equilibrium wages. We exploit the fact that the reform only applies to part of the workforce. While workers with fewer than two successful prior

⁵Jurajda (2002) argues that it can be optimal for firms to lay off workers on benefits in reaction to demand fluctuations, anticipating that workers on benefits search less hard for alternative jobs.

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 with fewer than two successful UI benefits applications experience a 0.5-0.8 percent higher increase in wages after the reform. 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 that suggest 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 in which the timing of unemployment spells with eligibility for UI benefits is more pervasive. A ten percentage points increase in collusive layoff and rehiring patterns at the local industry level is associated with a 1.9 percent higher increase in wages for workers affected by the reform.

Finally, we examine the role of informal labor markets for the results in the paper, as informal labor markets have important implications in the light of implicit contracting models.⁶ Informal labor markets provide a unique alternative to formal employment in the light of UI. Workers are able to receive UI benefits while continuing to be employed informally. Additionally, theories of implicit contracting in the presence of UI (Feldstein 1976; Baily 1977) require a form of attachment between workers and firms to allow for implicit contracts to be viable. Informal labor markets provide such a form of attachment, as they allow firms to hold on to workers while they are on UI benefits. Moreover, being able to continue to employ workers informally makes formal layoff and rehiring patterns less disruptive for firms and workers.

We find that the full set of results strongly correlates with the presence of informal labor markets. The drop in unemployment inflow after the reform is significantly stronger for workers in industries and municipalities with large informal labor markets. Specifically, we find that a ten percentage point increase in the share of informal employment in a given industry or municipality corresponds to an about 0.2 percentage points higher inflow into formal unemployment when workers are eligible for benefits. The timing of unemployment

⁶Brazil constitutes an ideal laboratory to study the role of informal labor markets. It is a middle-income country where informal labor markets are prevalent. According to the International Labor Organization, 36.8 percent of all workers were employed informally in 2013. For comparison, in Europe, informal labor markets account for 17.4 percent of the total labor market during 2008-2009 (Hazans 2011), and the country is very heterogeneous, providing ample variation in labor market informality across industries and municipalities.

spells with benefits eligibility is mostly driven by industries and municipalities with large informal labor markets, and formal wages increase 0.12 to 0.44 percent more for workers affected by the reform for a ten percentage points increase in labor market informality.

Additionally, we find strong indirect evidence that firms continue to employ workers informally while they receive UI benefits. When firms lay off a worker, they often hire a new worker as a replacement. If firms continue to employ a formally laid off worker informally, they do not need to hire a replacement worker. Consistent with this argument, we find that before the reform when firms lay off a worker who is just eligible for UI benefits they are about five percentage points less likely to hire a replacement worker than when they lay off a worker who is just ineligible for UI benefits, in particular in areas with large informal labor markets. After the reform, when workers around the six month threshold are all ineligible for UI benefits, this pattern disappears.

We perform several robustness tests to strengthen the validity of our results. First, we control for cyclical patterns by performing the same analysis for the previous year for which we observe none of the same patterns. Second, we confirm that workers do not substitute to other forms of job separation, such as voluntary departures, after the reform. Third, we show that the results are not affected by potential announcement effects of the reform two months before the implementation of the reform. Importantly, we do not find any of the patterns for workers around the six months tenure threshold for workers with more than two successful past applications for UI benefits, who are not affected by the reform.

The results in the paper provide new insights into the impact of UI benefits on strategic unemployment inflow and outflow, collusion between firms and workers, and formal labor supply and wages. While recent studies find no (Card, Chetty, and Weber (2007) for Austria) or weak (Schmieder, von Wachter, and Bender (2012) for Germany) timing of unemployment inflow with respect to UI eligibility, the results in this paper show that layoff intensities are strongly affected by workers' eligibility for UI benefits. Most evidence on higher layoff intensities when workers are eligible for UI benefits exists for Canada at a time when UI benefits eligibility also applied to workers quitting their job (Christofides and McKenna 1995, 1996; Green and Riddell 1997; Baker and Rea 1998; Green and Sargent 1998). The surprise announcement of the UI reform in Brazil allows us to examine the effect of UI benefits on layoff intensities free from workers' selection into jobs with different expected employment duration (Green and Riddell 1997). An additional advantage of our empirical setting is that it allows us to examine the role of implicit contracting between firms and

workers. We provide evidence that collusion between firms and workers to extract rents from the UI system is an important channel for strategic unemployment, and that firms seem to benefit by paying lower equilibrium wages.

Additionally, we document how informal labor markets interact with the incentive effects of UI benefits. Recent years have seen a rapid spread of UI programs to middle-income and developing countries with large informal labor markets (Holzmann et al. 2011). In the light of this development, it is important to understand how UI affects workers' incentives in these countries to optimize the design of UI programs.⁷ Even in developed countries, some sectors of the labor market feature a significant presence of informality for which understanding its impact on the incentive effects of UI is important.⁸ 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, in particular by allowing firms to employ workers informally while they receive UI benefits.

Our findings have important implications for UI design. 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 the UI system allows firms and workers to extract higher rents. Two 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 to increase the cost of layoffs. More nuanced policy implications may include tweaks to the UI system that prevent repeated temporary layoffs of the same worker by the same firm. With respect to the role of informal labor markets, better monitoring and higher penalties for informal employment may mitigate strategic formal unemployment by making it more costly to extract benefits payments while sustaining an informal employment relationship.

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. Thus,

⁷Some recent studies analyze UI programs in middle-income and developing countries (Gasparini, Haimovich, and Olivieri 2009; Gonzalez-Rozada, Ronconi, and Ruffo 2011; Amarante, Arim, and Dean 2013; Gerard and Gonzaga 2014). However, these papers do not directly examine how differences in labor market formality influence the effect of UI programs with the exception of Gerard and Gonzaga (2014). Our results on the effect of informal labor markets on search intensities around the reform are consistent with their cross-sectional evidence.

⁸Kuhn and Riddell (2010) show for a comparison of U.S. and Canadian border regions that differences in UI systems can have strong effects on labor supply in the long-run.

the results are not subject to any selection bias or specific to a subgroup of workers. 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 time period around the reform (January 2012 to December 2015). Additionally, Schmieder, von Wachter, and Bender (2012) show that incentive effects from UI are *weaker* during recessions, which implies that the results we document may be rather conservative. Moreover, evidence on the effects of UI in recessionary periods are 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. Hence, we think that the insights are relevant and informative beyond the specific context in this paper.

2 Institutional Background and Data

This section provides information about Brazil’s UI system, the UI reform implemented in March 2015, and the data used for the empirical analysis in the paper.⁹

2.1 Unemployment Insurance in Brazil

In Brazil, every formal worker is required to hold a working card, which 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 workers seniority account (FGTS).¹⁰ Other mandatory contributions such as the social integration program (PIS) and contributions to social security funding (COFINS) depend on the industries that firms operate in and are paid as a fraction of net profit and sales. Funding of the UI system stems 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

⁹A thorough review of the history of labor law in Brazil is provided in Gonzaga (2003).

¹⁰This account is held with a state bank and can be withdrawn when a worker retires, is laid off, or suffers from a severe illness.

if a worker was employed between six and eleven months during the last 36 months, four payments are made if a worker was employed between 12 and 23 months during the last 36 months, and five payments are made if a worker was employed for at least 24 months during the last 36 months. In 2015, the monthly payments range from 1 to 1.76 minimum salaries, depending on the average pre-layoff wage. Importantly, the UI system does not feature a strong experience rating mechanism as in the U.S. If a firm dismisses a worker without a justified reason, it must pay an additional fifty percent of the total contributions that have accumulated in an 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 lower for lower pre-layoff wages). Eighty percent of this penalty is directly paid to the worker rather than used for funding the UI system. Thus, 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. Firing workers with a valid legal reason does not involve penalties. The hurdle to provide 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.

2.2 UI Reform

To be eligible for UI benefits prior to March 1 2015, a worker had to be employed over a consecutive period of at least 6 months prior to layoff, had to be laid off without a justified reason, may not earn other labor income, and may 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.¹¹ Since the UI reform was announced unexpectedly only two months before its implementation, workers with tenure of more than two months at the implementation of the reform were already in formal employment before the announcement of the reform. The main driver for the quick implementation and the tightening in eligibility criteria were attempts on part of the government to reduce the growing budget deficit. The size and duration of UI benefits was not altered. Importantly, UI contributions of employers were unaffected by

¹¹Estadao Politca, December 29, 2014, "Forca Sindical nega ter sido consultada sobre ajuste em beneficios".

the reform. Thus, the reform had no direct effect on employers' demand for formal labor.

The reform affected workers with less than two successful prior applications for UI benefits. For these workers eligibility criteria were substantially tightened. To be eligible for UI benefits after the reform, a longer pre-layoff employment history than the six months threshold from before the reform was required. Specifically, workers who applied for the first time required documented employment of at least 18 months in the 24 months prior to layoff. Workers who applied for the second time required 12 months in formal employment during the last 16 months (see Figure 1). This provisional measure was applied from March 2015 and turned into law in July 2015 with some adjustments. The adjusted requirements from July 2015 required a first time applicant to have at least 12 months of employment in the last 18 months. A second time applicant had to have at least 9 months of employment in the last 12 months. These adjustments do not directly affect workers around the six months threshold that we exploit for our empirical analysis. With respect to ex ante changes in workers' incentives, it is not clear whether the adjustments had any effect as some adjustments to the law were expected and were likely to be considered when workers decided to enter formal employment even in the six months before the adjustment.

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 period from 2013–2015. 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). Finally, we use data for the same months from the previous year to control for cyclical effects.

In Table 1, we provide evidence that workers with a tenure of six or seven months, who are affected by the reform, and workers with a tenure of four or five months, who are not directly affected by the reform, are indistinguishable in terms of observable characteristics. Both groups of workers are virtually identical in terms of age, salary, gender, education, the size of the firm they are employed at, and the industries that they are employed in. They do, however, differ in terms of the probability of 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. The census groups workers into twenty different industry classifications (see Table A.1). 66 percent of domestic services employees are working informally. The most formal industry, electricity and gas, has only 5.5 percent of informal workers. In terms of geographic variation in informality, most municipalities fall within the range of 20 to 70 percent of labor market informality (Figure 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).

3 Empirical Strategy

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

incentive effects of UI are influenced by the presence of informal labor markets.

3.1 Unemployment Inflow and Outflow

The sharp discontinuity in the reform’s effect allows us to compare changes in unemployment inflow and outflow for workers just above the eligibility threshold (six or seven months tenure) and workers just below the threshold (four or five months tenure). Workers with tenure of six or seven months, are eligible for UI benefits only before the reform, whereas workers with tenure of four or five months are never eligible for benefits. Monthly data allows us to focus on a narrow time period of two months before and after the reform. Importantly, the unexpected announcement of the reform 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 worker unaffected by the reform. Together, this allows us to identify how UI benefits affect workers’ decision to flow into and out of unemployment.

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

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

where $P[u_{unjust}]_{it}$ is a dummy variable that takes the value of one if worker i is laid off in month t , and zero otherwise.¹² 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 control for location-specific, local industry-specific, and local occupation-specific shocks in unemployment inflow.

The parameter of interest is β_3 . The coefficient β_3 compares the difference in unemployment inflow after the reform when neither group of workers is eligible for UI benefits to the

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

difference in unemployment inflow between both groups of workers before the reform when workers with six months are eligible for UI benefits. A negative value of β_3 implies that UI benefits lead to higher unemployment inflow when workers are eligible for UI benefits, and vice versa.

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

3.2 Collusion

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

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

where $P_{same}[4 - 9]_{it}$ is a dummy variable that takes the value of one if a worker returns to the same firm four to nine months after being laid off, and zero if a worker is not hired by the same firm four to nine months after layoff.¹³ The sample for this test comprises all workers

¹³Workers are eligible for at least three months of UI benefits. Workers may not have successfully applied for UI benefits for 16 months before reapplying. Thus, firms that hire and layoff workers to exploit the UI system may formally rehire workers only after nine months for them to be eligible for benefits after another six months of formal employment.

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 benefits have run out.

3.3 Formal Employment and Wages

Next, we assess the ex ante incentive effects of UI exploiting the fact that the reform only applies to part of the workforce. The prospect of future eligibility for UI benefits may lead to an entitlement effect, according to which workers value formal employment more (Mortensen 1977; 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:

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

where $Workers\ Hired_t$ is defined as the number of workers hired in a given industry in a given municipality in month t scaled by the number of workers employed in the respective local industry in the month when the reform was announced. To examine the net effect on total formal employment, we replace the dependent variable by the log of total employment in a local industry. Workers' incentives to enter formal employment are affected from the time they are aware of the reform's effects. Since the reform was announced on December 30, 2014, we define the $Reform_t$ dummy as one from January 2015. The dummy variable $Affected_{it}$ takes the value of one for workers with less than two successful past applications for UI benefits whose eligibility criteria are tightened by the reform, and zero for workers with two or more successful past applications for whom eligibility criteria remain unchanged. Coefficient β_3 measures the relative change in the number of affected workers hired and employed after the reform, compared to workers unaffected by the reform. Using the same methodology, we examine changes in wages by replacing the dependent variable with the log of the average hiring wage in month t .

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

informal wages by estimating:

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

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

3.4 Labor Market Informality

Finally, we examine the role of informal labor markets for the patterns we observe in the data. We exploit 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).¹⁴ 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 equations (1) to (3).

Informal labor markets provide workers with the opportunity to claim UI benefits while continuing to be informally employed. This may exacerbate incentive effects of UI and facilitate collusion between firms and workers (Feldstein 1976; Baily 1977). 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 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 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 . 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

¹⁴Labor 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, the distribution of this difference is very similar and highly correlated with the actual measure (0.93) (Figure A.2).

negative coefficient β_3 indicates that firms are more likely to informally employ workers laid off with a tenure of six or seven months when they are eligible for UI benefits, compared to the post-reform period when they are no longer eligible for UI benefits. To obtain cleaner estimates, we only consider layoff months in which a firm either lays off a worker with a tenure of four or five months or a worker with a tenure of six or seven months, not both.

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 precisely with eligibility for UI benefits, in particular when the rents they can extract from the UI system are higher. 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 evidence that firms continue to employ workers informally while they are on benefits.

4.1 UI Benefits and Unemployment Inflow

Figure 2 depicts layoff intensities for workers with different tenure, separately for the months from January to April 2015.¹⁵ 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 the UI benefits eligibility threshold from six to twelve and eighteen months. In particular, there is a sharp drop in the probability of being laid off for workers with tenure of six months, who lose eligibility for benefits after the reform, relative to unemployment inflow for workers with a tenure of five months, who are ineligible for benefits before and after the reform.

We confirm the insights from the graphical analysis statistically 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 tenure of six or seven months, compared to workers with a tenure of four or five months.¹⁶ Further saturating the specification with municipality-month fixed

¹⁵The plots are aligned at the April 2015 values for workers with five months tenure to facilitate comparison.

¹⁶While we focus our analysis to the four month period around the reform for sharper identification,

effects to account for local shocks in column II, the effect remains similar with 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 with 0.49 pp.¹⁷

The results cannot be explained by cyclical effects. We observe no similar patterns during the same months during 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 tenure of four to seven months 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.10).

4.2 UI Benefits and Unemployment Outflow

Figure 3 depicts employment probabilities conditional on unemployment duration for workers laid off during the months from January to April 2015, separately for workers with a tenure of six or seven months at layoff (top panel), and workers with a tenure of four or five months at layoff (bottom panel). For workers with six or seven months tenure at layoff, reemployment is significantly less likely to occur within five months (maximum benefit duration) during January and February when workers are eligible for UI benefits, compared to March and April when they are no longer eligible for benefits. In contrast, for workers with four or five months tenure at the time of layoff, who are never eligible for benefits, unemployment outflow does not show different patterns around the reform.

The results from estimating equation (1), depicted in Table 3, show that workers with a tenure of six or seven months who lose eligibility for UI benefits after the reform, become 5.16 pp (13.5 percent) more likely to return to formal employment, compared to workers with four or five months tenure at layoff (column I). The effect is similar with 5.06 pp when we compare workers in the same geographical area (columns II). Further restricting the comparison to workers within the same local industry does not affect the results with 5.19 pp (column III), as does comparing workers with the same occupation within a local

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).

¹⁷The results are not driven by a "relabeling" of unemployment from voluntary departures to layoffs. We observe no change in voluntary layoffs around the reform (Table A.2).

industry with 4.98 pp (column IV). We find no similar patterns in unemployment outflow for the same months in the previous year (Figure A.6 and Table A.5), or for workers unaffected by the reform (Figure A.7 and Table A.6). Lower search intensities for reemployment are consistent with findings in the prior literature that workers are less likely to return to formal employment when they are eligible for UI benefits.

4.3 Collusion

To be eligible for UI benefits, workers need to be laid off by their employer. Layoffs may be induced through different mechanisms. For example, workers may elicit layoffs through shirking, or firms may collude with workers to extract rents from the UI system by laying them off when they are eligible for UI benefits.

To assess whether collusion between firms and workers plays 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 benefits run out. Specifically, we examine the probability of being rehired by the same firm four to nine months after layoff when benefits run out 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 collude with workers to time unemployment spells with UI benefits eligibility, we expect them to be more likely to rehire the same worker when benefits run out. We follow our main identification strategy comparing dismissed workers with six or seven months of tenure at layoff, who lose eligibility after the reform, to those with five months tenure at layoff, who are always ineligible.

The results are gathered in Table 4. Column I shows that before the reform the probability to be rehired by the same employer four to nine months after layoff is about two percentage points higher 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 that are rehired between four to nine months after layoff to ensure that our results are not affected by changes in reemployment timing. The results confirm that the difference in the probability of being rehired by the

same firm four to nine months 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.7), and for workers with more than two previous UI benefits spells, who are not affected by the reform (Table A.8). 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 UI benefits eligibility is exhausted.

The results in Section 4.1 show that strategic unemployment inflow due to eligibility for UI benefits constitutes twelve percent of all layoffs. The probability of being rehired by the same firm four to nine months after layoff decreases from 7.0% to 5.3% after the reform for workers with a tenure of six or seven months at layoff. This implies that around 19.5 percent of strategic unemployment inflow due to UI benefits eligibility can be explained by this simple form of collusion between firms and workers.¹⁸ These estimates are likely to be conservative. First, we assume that colluders return to the same firm with a probability of one, whereas in reality there might be cases where reemployment in the same firm fails. 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 we examine. For example, several firms and workers as a group could engage in collusion in a way that our test do not identify as collusion, or formal reemployment may not be part of the collusion agreement, or delayed beyond nine months after layoff.

4.4 Total Rents and Strategic Unemployment

We next turn to examining whether unemployment inflow and outflow timing with UI benefits eligibility and reemployment by the same firm are more prevalent when rents that can be extracted from the UI system are higher in Table 5. 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

¹⁸The fraction of colluders can be computed as: $12\% * x + (1 - 12\%) * 5.3\% = 5.3\% + 1.7\%$, where 5.3% is the base rate of reemployment by the same firm in the absence of UI benefits, 12% is the fraction of strategically unemployed workers among all unemployed workers, and x is the fraction of colluders among strategically unemployed people (for whom reemployment by the same firm equals one).

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 layoff workers that are eligible for UI benefits when the potential rents that can be extracted from the UI system are high. When workers with tenure of six and seven months lose eligibility for UI benefits after the reform, these workers are relatively less likely to flow into unemployment by 0.14 pp per 10 percentage points increase in 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 10 percentage points 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 10 percentage points increase in rents that firms and workers can extract from the UI system.

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 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 UI benefits run out, and may struggle to time reemployment with the exhaustion of UI benefits, in particular during a recessionary period. We find that workers with a higher replacement rate are less likely to return to formal employment after benefits run out (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 benefits run out (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.

4.5 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 4 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 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 that 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 6, we examine changes in formal hiring, employment, and wages after the reform statistically by estimating equation (3). The results in columns I and II indicate that formal hiring of workers who are less likely to qualify for UI benefits after the reform relatively decreases 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 relatively increase 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 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 with UI benefits eligibility (Feldstein 1976; Baily 1977; Christofides and McKenna 1996). To provide more direct evidence on the role of strategic behavior on part of firms and workers to time formal unemployment with UI benefits eligibility on changes in formal em-

ployment 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 are collected in Table 7. 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 (columns 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 with UI benefits exhaustion (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 higher degree of strategic unemployment inflow by 0.04 percent per 10 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 7 suggest that formal labor supply of workers affected after 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.

4.6 Labor Market Informality

This section examines the role of informal labor markets in explaining the previous findings. A unique feature of informal labor markets is to 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.

In Figure 5, 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, in Figure 6, we find that in municipalities with above median levels of informality, unemployment inflow decreases by about two percentage points for workers that lose eligibility for UI benefits after the reform. In municipalities with below median levels of informality, the magnitude of the effect is smaller than one percentage point.

In Table 8, we formally assess how 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 points 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 with 0.18 pp (column II). Additionally, controlling for local industry shocks leaves the effect virtually unchanged with 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 with 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 5 and Table 8 suggest that the presence of informal labor markets has a strong effect on the relationship between unemployment inflow and eligibility for UI benefits.

Figures 7 and 8 depict reemployment probabilities for labor markets with above median levels of informality (top panels) and below median levels of labor market informality (bottom panels) for worker with tenure of four or five months at layoff (left panels) and workers with tenure of six or seven month at layoff (right panels). The increase in reemployment probabilities for workers with six months tenure at layoff is higher in industries and municipalities with above median levels of labor market informality than in industries and municipalities with below median labor market informality.

In Table 9, we assess statistically how informal labor market affect search intensities in the presence of UI benefits. The top panel shows the results for variation in labor market informality at the industry level. We find that reemployment within five months after layoff

increases by 0.38 pp more per ten percentage points increase in labor market informality when workers lose eligibility for UI benefits (column I). Controlling for local shocks specific to workers with different tenure at layoff strengthens the effect to 0.60 pp (column II). Controlling for local industry-specific shocks, the magnitude of the effect is 0.75 pp (column III). When we further add controls for local industry shocks to workers in the same occupation, the effect is almost identical with 0.72 pp (column IV). The bottom panel exploits variation in labor market informality at the municipality level with similar results that are statistically weaker due to a lower base rate of formal reemployment in municipalities with a higher share of informal labor markets. Together, the results suggest that search intensities are lower for workers on UI benefits in the presence of informal labor markets.

Next, we examine whether patterns consistent with collusion between workers and firms are concentrated in industries and municipalities with large informal labor markets in Table 10. The results in Panel A show that firms in more informal industries are significantly more likely to lay off workers when they are eligible for benefits to rehire them just when benefits run out. Specifically, a ten percentage points 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 run out. The results are similar at the municipality level with slightly higher magnitudes (Panel B). This cross-sectional evidence suggests that informal labor markets facilitate collusion between firms and workers.

The reduction in formal labor supply of workers whose eligibility criteria of UI benefits are tightened by the reform documented in Section 4.5 may be stronger when workers have the option to work informally while receiving UI benefits. Testing this conjecture formally in Table 11, 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).

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

4.7 Formal Unemployment and 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. 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 hire a replacement worker after laying off a worker with tenure six or seven months, who is eligible for UI benefits before the reform, and after laying off a worker with a tenure of four or five months, who is never eligible for UI benefits. Continued informal employment while workers are on benefits would predict that hiring rates for replacement workers are lower for workers laid off with a tenure of six or seven months in January and February when they are eligible for benefits.

We start with a graphical depiction of the probability that firms hire a new workers 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 9. 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 9, these differences are not driven by cyclical layoffs or replacement hiring patterns in the months from January to April. Additionally, Figure A.8 and Table A.9 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 hiring probabilities of replacement workers remain constant after the implementation of the reform. When we compare the graphical evidence for above and below median industries (middle panel) and municipalities (bottom panel), we observe that the effects are much stronger in industries and municipalities with larger informal labor markets.

We confirm the insights from the graphical analysis statistically in Table 12 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. These 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 13 statistically confirm that informal employment while workers are on benefits is more prevalent in industries (top panel) and municipalities (bottom panel) with a higher share of informal labor markets.

5 Conclusion

Exploiting a reform to UI benefits eligibility criteria in Brazil, we document that workers are more likely to exit formal employment when they qualify for UI benefits. 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 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 eligibility for benefits expires. Examining changes in wages around the reform indicates that firms benefit from colluding with workers through paying 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. One potential reason for this observation is that firms may be able to employ workers informally while they are on benefits, effectively leaving the relationship between the firm and the worker unaffected while extracting rents from the UI system. We find that when firms layoff workers when they 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 months 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 suggests that firms and workers revert to informal employment relationships to extract rents from the UI system.

Our findings have important implications for UI design. The timing of unemployment spells to workers' UI eligibility suggests that in 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 potential rents that firms and workers can extract from the UI system are larger. Rents can potentially be reduced by lowering replacement rates, 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.

References

- Amarante, V., R. Arim, and A. Dean. 2013. Unemployment insurance design and its effects: Evidence for Uruguay. Working paper, Universidad de los Andes-Cede.
- Anderson, P. M., and B. D. Meyer. 1997. Unemployment insurance take-up rates and the after-tax value of benefits. *Quarterly Journal of Economics* 112:913–37.
- Baily, M. N. 1977. On the theory of layoffs and unemployment. *Econometrica* 45:1043–63.
- Baker, M., and S. A. Rea. 1998. Employment spells and unemployment insurance eligibility requirements. *Review of Economics and Statistics* 80:80–94.
- Card, D., R. Chetty, and A. Weber. 2007. Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market. *Quarterly Journal of Economics* 122:1511–60.
- Card, D., A. Johnston, P. Leung, A. Mas, and Z. Pei. 2015a. The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression discontinuity design in missouri, 2003-2013. *American Economic Review* 105:126–30.

- Card, D., D. S. Lee, Z. Pei, and A. Weber. 2015b. Inference on causal effects in a generalized regression kink design. *Econometrica* 83:2453–83.
- Card, D., and P. B. Levine. 1994. Unemployment insurance taxes and the cyclical and seasonal properties of unemployment. *Journal of Public Economics* 53:1–29.
- . 2000. Extended benefits and the duration of UI spells: Evidence from the New Jersey extended benefit program. *Journal of Public Economics* 78:107–38.
- Carvalho, C. C., R. Corbi, and R. Narita. 2017. Unintended consequences of unemployment insurance: Evidence from stricter eligibility criteria in Brazil. *Economic Letters* forthcoming.
- Chetty, R. 2008. Moral hazard vs. liquidity and optimal unemployment insurance. *Journal of Political Economy* 116:173–234.
- Christofides, L. N., and C. McKenna. 1995. Unemployment insurance and moral hazard in employment. *Economic Letters* 49:205–10.
- . 1996. Unemployment insurance and job duration in Canada. *Journal of Labor Economics* 14:286–312.
- Farber, H. S., J. Rothstein, and R. G. Valletta. 2015. The effects of extended unemployment insurance benefits: Evidence from the 2012–2013 phase-out. *American Economic Review* 105:171–6.
- Farber, H. S., and R. G. Valletta. 2015. Do extended unemployment benefits lengthen unemployment spells? Evidence from recent cycles in the U.S. labor market. *Journal of Human Resources* 50:873–909.
- Feldstein, M. 1976. Temporary layoffs in the theory of unemployment. *Journal of Political Economy* 84:937–58.
- . 1978. The effects of unemployment insurance on temporary layoff unemployment. *American Economic Review* 68:834–46.
- Gasparini, L., F. Haimovich, and S. Olivieri. 2009. Labor informality bias of a poverty-alleviation program in Argentina. *Journal of Applied Economics* 12:181–205.

- Gerard, F., and G. Gonzaga. 2014. Informal labor and the efficiency of social programs: Evidence from 15 years of unemployment insurance in Brazil. Working paper, Columbia University.
- Gonzaga, G. 2003. Labor turnover and labor legislation in Brazil. *Economia* 4:1–57.
- Gonzalez-Rozada, M., L. Ronconi, and H. Ruffo. 2011. Protecting workers against unemployment in Latin America and the Caribbean: Evidence from Argentina. Working paper, IDB.
- Green, D. A., and C. W. Riddell. 1997. Qualifying for unemployment insurance: An empirical analysis. *Economic Journal* 107:67–84.
- Green, D. A., and T. C. Sargent. 1998. Unemployment insurance and job durations: Seasonal and non-seasonal jobs. *The Canadian Journal of Economics* 31:247–78.
- Hamermesh, D. S. 1979. Entitlement effects, unemployment insurance, and employment decisions. *Economic Enquiry* 17:317–32.
- Hazans, M. 2011. Informal workers across Europe: Evidence from 30 countries. Working paper, IZA.
- Holzmann, R., Y. Pouget, M. Vodopivec, and M. Weber. 2011. Severance pay programs around the world: History, rationale, status, and reforms. Working paper, IZA.
- Johnston, A. C., and A. Mas. 2015. Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut. Working paper, NBER.
- Jurajda, S. 2002. Estimating the effect of unemployment insurance compensation on the labor market histories of displaced workers. *Journal of Econometrics* 108:227–52.
- Katz, L. F., and B. D. Meyer. 1990. The impact of the potential duration of unemployment benefits on the duration of unemployment. *Journal of Public Economics* 41:45–72.
- Kroft, K., and M. J. Notowidigdo. 2016. Should unemployment insurance vary with the unemployment rate? Theory and evidence. *Review of Economic Studies* 83:1092–124.
- Kuhn, P., and C. Riddell. 2010. The long-term effects of unemployment insurance: Evidence from New Brunswick and Maine, 1940–1991. *Industrial and Labor Relations Review* 63:183–204.

- Lalive, R. 2008. How do extended benefits affect unemployment duration? A regression discontinuity design. *Journal of Econometrics* 142:785–806.
- Landais, C. 2015. Assessing the welfare effects of unemployment benefits using the regression kink design. *American Economic Journal: Economic Policy* 7:243–78.
- Meyer, B. D. 1990. Unemployment insurance and unemployment spells. *Econometrica* 58:757–82.
- . 1995. Lessons from the us employment insurance experiments. *Journal of Economic Literature* 33:91–131.
- Meyer, B. D., and W. K. C. Mok. 2007. Quasi-experimental evidence on the effects of unemployment insurance from New York State. Working paper, NBER.
- Moffitt, R. 1985. Unemployment insurance and the distribution of unemployment spells. *Journal of Econometrics* 28:85–101.
- Mortensen, D. T. 1977. Unemployment insurance and job search decisions. *Industrial and Labor Relations Review* 30:505–17.
- Rebello-Sanz, Y. 2012. Unemployment insurance and job turnover in Spain. *Labour Economics* 19:403–26.
- Rothstein, J. 2011. Unemployment insurance and job search in the great recession. *Brookings Papers on Economic Activity* 143–210.
- Rothstein, J., and R. G. Valletta. 2017. Scraping by: Income and program participation after the loss of extended unemployment benefits. Working paper, NBER.
- Saffer, H. 1982. Layoffs and unemployment insurance. *Journal of Public Economics* 19:121–9.
- Schmieder, J. F., T. von Wachter, and S. Bender. 2012. The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years. *Quarterly Journal of Economics* 127:701–52.
- . 2016. The effect of unemployment benefits and nonemployment duration on wages. *American Economic Review* 106:739–77.
- Solon, G. 1979. Labor supply effects of extended unemployment benefits. *Journal of Human Resources* 14:247–55.

- Topel, R. H. 1983. On layoffs and unemployment insurance. *American Economic Review* 73:541–59.
- Valletta, R. G. 2014. Recent extensions of US unemployment benefits: Search responses in alternative labor market states. *IZA Journal of Labor Policy* 3:1–25.
- Winter-Ebmer, R. 2003. Benefit duration and unemployment entry: A quasi-experiment in Austria. *European Economic Review* 47:259–73.

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

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

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

Table 2: **Unemployment Inflow**

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0129*** (0.0007)	0.0125*** (0.0006)	0.0122*** (0.0005)	0.0123*** (0.0005)
$6Months_{it} * Reform_t$	-0.0052*** (0.0008)	-0.0053*** (0.0007)	-0.0053*** (0.0006)	-0.0049*** (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: Unemployment Outflow

Dep. Var.: $P[e \leq 5]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0872*** (0.0044)	-0.0859*** (0.0044)	-0.0861*** (0.0048)	-0.0846*** (0.0052)
$6Months_{it} * Reform_t$	0.0516*** (0.0037)	0.0506*** (0.0034)	0.0519*** (0.0039)	0.0498*** (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 4: Collusion

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

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

Table 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 (0.0014)	-0.0048 (0.0127)	0.0284*** (0.0089)
$6Months_{it} * Reform_t$	0.0077*** (0.0019)	0.0214* (0.0111)	-0.0282*** (0.0132)
$Surplus_{it}$	-0.0280*** (0.0016)	-0.2787*** (0.0308)	0.0121** (0.0053)
$Surplus_{it} * 6Months_{it}$	0.0289*** (0.0040)	-0.2242*** (0.0348)	0.0264*** (0.0100)
$Surplus_{it} * Reform_t$	-0.0187*** (0.0021)	-0.1123*** (0.0146)	-0.0072 (0.0075)
$Surplus_{it} * 6Months_{it} * Reform_t$	-0.0135*** (0.0041)	0.0904*** (0.0238)	-0.0274* (0.0147)
RR_{it}	0.0873*** (0.0041)	0.6539*** (0.0361)	-0.0658*** (0.0098)
$RR_{it} * 6Months_{it}$	-0.0247*** (0.0051)	0.0296 (0.0345)	-0.0333** (0.0136)
$RR_{it} * Reform_t$	-0.1114*** (0.0134)	0.1640*** (0.0173)	0.0002 (0.0126)
$RR_{it} * 6Months_{it} * Reform_t$	-0.0079 (0.0053)	-0.0980*** (0.0260)	0.0393** (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 columns 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 take 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% level, respectively.

Table 6: Formal Hiring, Employment, and Wages

	I	II	III	IV	V	VI	VII	VIII
Dep. Var.:	<i>Hired/Employed Workers</i>		<i>log(Workers)</i>		<i>log(Wage)</i>		<i>log(Wage) – log(Wage_{old})</i>	
<i>Affected_{it}</i>	0.0012*** (0.0001)	0.0009*** (0.0001)	0.6957*** (0.0053)	0.7906*** (0.0070)	-0.0979*** (0.0007)	-0.1078*** (0.0008)	0.0362*** (0.0007)	0.0346*** (0.0008)
<i>Affected_{it} * Reform_t</i>	-0.0037*** (0.0002)	-0.0043*** (0.0002)	-0.0612*** (0.0034)	-0.0631*** (0.0028)	0.0052*** (0.0007)	0.0082*** (0.0008)	0.0049*** (0.0010)	0.0050*** (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
<i>R</i> ²	0.054	0.346	0.720	0.956	0.414	0.526	0.029	0.355

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

Table 7: Formal Hiring, Employment, Wages, and Strategic Unemployment

	I	II	III	IV	V	VI
Dep. Var.:	<i>Hired/Employed Workers</i>	<i>Hired/Employed Workers</i>	<i>log(Workers)</i>	<i>log(Workers)</i>	<i>log(Wage)</i>	<i>log(Wage)</i>
<i>Affected_{it}</i>	0.0020*** (0.0001)	0.0025*** (0.0001)	0.8319*** (0.0060)	0.8041*** (0.0056)	-0.0905*** (0.0008)	-0.0911*** (0.0007)
<i>Affected_{it} * Reform_t</i>	-0.0033*** (0.0002)	-0.0047*** (0.0002)	-0.0556*** (0.0040)	-0.0619*** (0.0029)	0.0083*** (0.0011)	0.0095*** (0.0007)
<i>Affected_{it} * Strategic</i>	0.0011*** (0.0002)		-0.1214*** (0.0080)		-0.0079*** (0.0013)	
<i>Affected_{it} * Strategic * Reform_t</i>	-0.0047*** (0.0004)		-0.0267*** (0.0070)		0.0043** (0.0019)	
<i>Affected_{it} * Collusive</i>		-0.0054** (0.0021)		1.3515*** (0.0551)		-0.7207*** (0.0405)
<i>Affected_{it} * Collusive * Reform_t</i>		-0.0242*** (0.0029)		-0.1748*** (0.0407)		0.1893*** (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
<i>R</i> 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.

Table 8: 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*** (0.0053)	0.0476*** (0.0048)	0.0409*** (0.0038)	0.0420*** (0.0037)
$6Months_{it} * Reform_t * Informal$	-0.0242*** (0.0064)	-0.0183*** (0.0058)	-0.0168*** (0.0048)	-0.0174*** (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*** (0.0032)	0.0232*** (0.0033)	0.0237*** (0.0029)	0.0222*** (0.0031)
$6Months_{it} * Reform_t * Informal$	-0.0299*** (0.0039)	-0.0273*** (0.0041)	-0.0265*** (0.0038)	-0.0241*** (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% level, respectively.

Table 9: Unemployment Outflow by Informality

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

This table reports changes in unemployment outflow around the enactment of the UI reform from January to April 2015. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i enters formal employment within five months after being laid off and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March to April 2015, and zero for the pre-reform period from January to February 2015. The variable $Informal$ is the share of informal employment in a given industry in the top panel and municipality in the bottom panel. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 10: 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*** (0.0169)	0.0996*** (0.0152)	0.0708*** (0.0155)	0.0745*** (0.0193)
$6Months_{it} * Reform_t * Informal$	-0.0633*** (0.0244)	-0.0664*** (0.0242)	-0.0502** (0.0218)	-0.0607** (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*** (0.0218)	0.0453** (0.0210)	0.0551** (0.0205)	0.0402 (0.0239)
$6Months_{it} * Reform_t * Informal$	-0.0728*** (0.0278)	-0.0685*** (0.0269)	-0.0810*** (0.0290)	-0.0808** (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 reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March to April 2015, and zero for the pre-reform period from January to February 2015. The variable $Informal$ is the share of informal employment in a given industry in the top panel and municipality in the bottom panel. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** and ** denote statistical significance at the 1% and 5% level, respectively.

Table 11: **Formal Hiring, Employment, and Wages by Informality**

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

Table 12: Replacement Hiring

Dep. Var.: $P[\textit{replacement hire}]_{it}$	I	II	III	IV
$6\textit{Months}_{it}$	-0.0258*** (0.0030)	-0.0193*** (0.0030)	-0.0154*** (0.0029)	-0.0146*** (0.0035)
$6\textit{Months}_{it} * \textit{Reform}_t$	0.0175*** (0.0041)	0.0104** (0.0040)	0.0080** (0.0040)	0.0119** (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

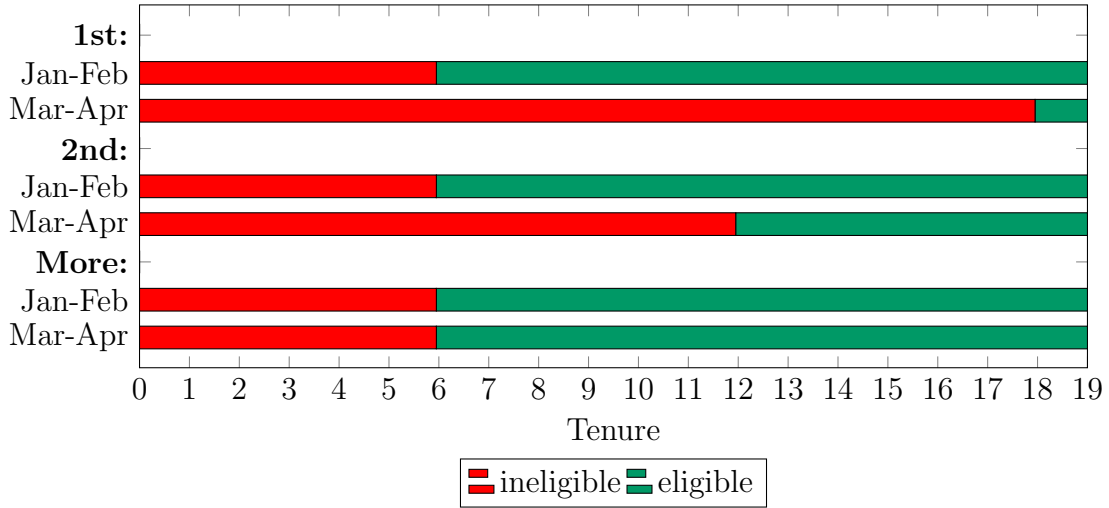
This table reports changes in the hiring of a replacement workers within one year of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker i , and zero otherwise. The dummy variable $6\textit{Months}_{it}$ 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 \textit{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 13: Replacement Hiring by Informality

	I	II	III	IV
Dep. Var.: $P[\textit{replacement hire}]_{it}$	Industry-Level			
$6\textit{Months}_{it}$	0.0082 (0.0097)			
$6\textit{Months}_{it} * \textit{Reform}_t$	-0.0123 (0.0065)			
$6\textit{Months}_{it} * \textit{Informal}$	-0.1028*** (0.0352)	-0.1074*** (0.0366)	-0.1183*** (0.0403)	-0.1364*** (0.0435)
$6\textit{Months}_{it} * \textit{Reform}_t * \textit{Informal}$	0.0850** (0.0485)	0.1014** (0.0503)	0.1375** (0.0545)	0.1385** (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.: $P[\textit{replacement hire}]_{it}$	Municipality-Level			
$6\textit{Months}_{it}$	0.0067 (0.0093)			
$6\textit{Months}_{it} * \textit{Reform}_t$	-0.0129 (0.0128)			
$6\textit{Months}_{it} * \textit{Informal}$	-0.1061*** (0.0363)	-0.1104*** (0.0369)	-0.0903** (0.0411)	-0.1175** (0.0528)
$6\textit{Months}_{it} * \textit{Reform}_t * \textit{Informal}$	0.0955* (0.0524)	0.1081** (0.0540)	0.1058* (0.0599)	0.1454* (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

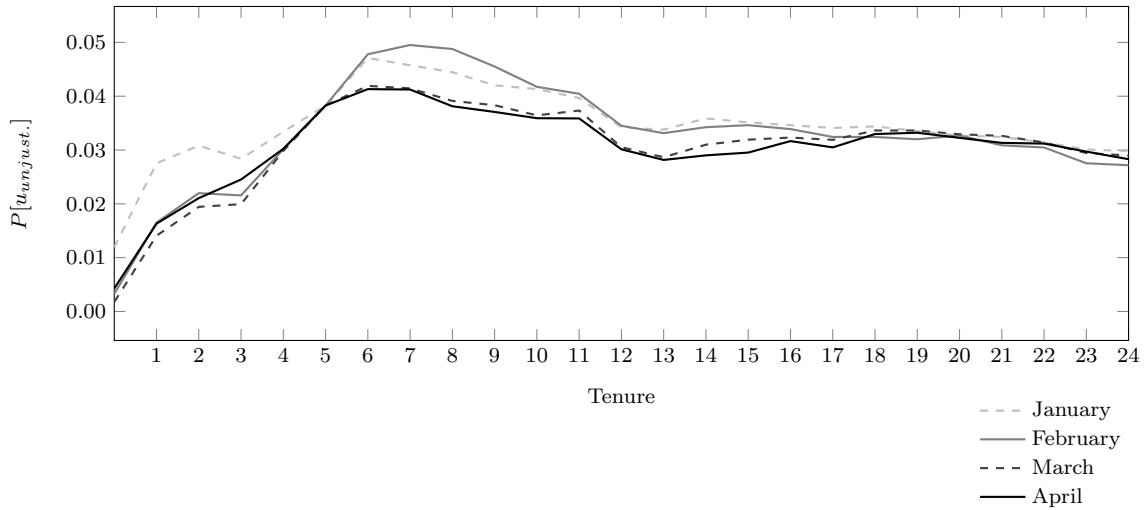
This table reports changes in the hiring of a replacement workers within one year of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015. The dependent variable is a dummy variable that takes the value of one if the same firm hires a new worker within a month of laying off worker i , and zero otherwise. The dummy variable $6\textit{Months}_{it}$ 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 \textit{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 $\textit{Informal}$ 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.

Figure 1: UI Eligibility Around the Reform



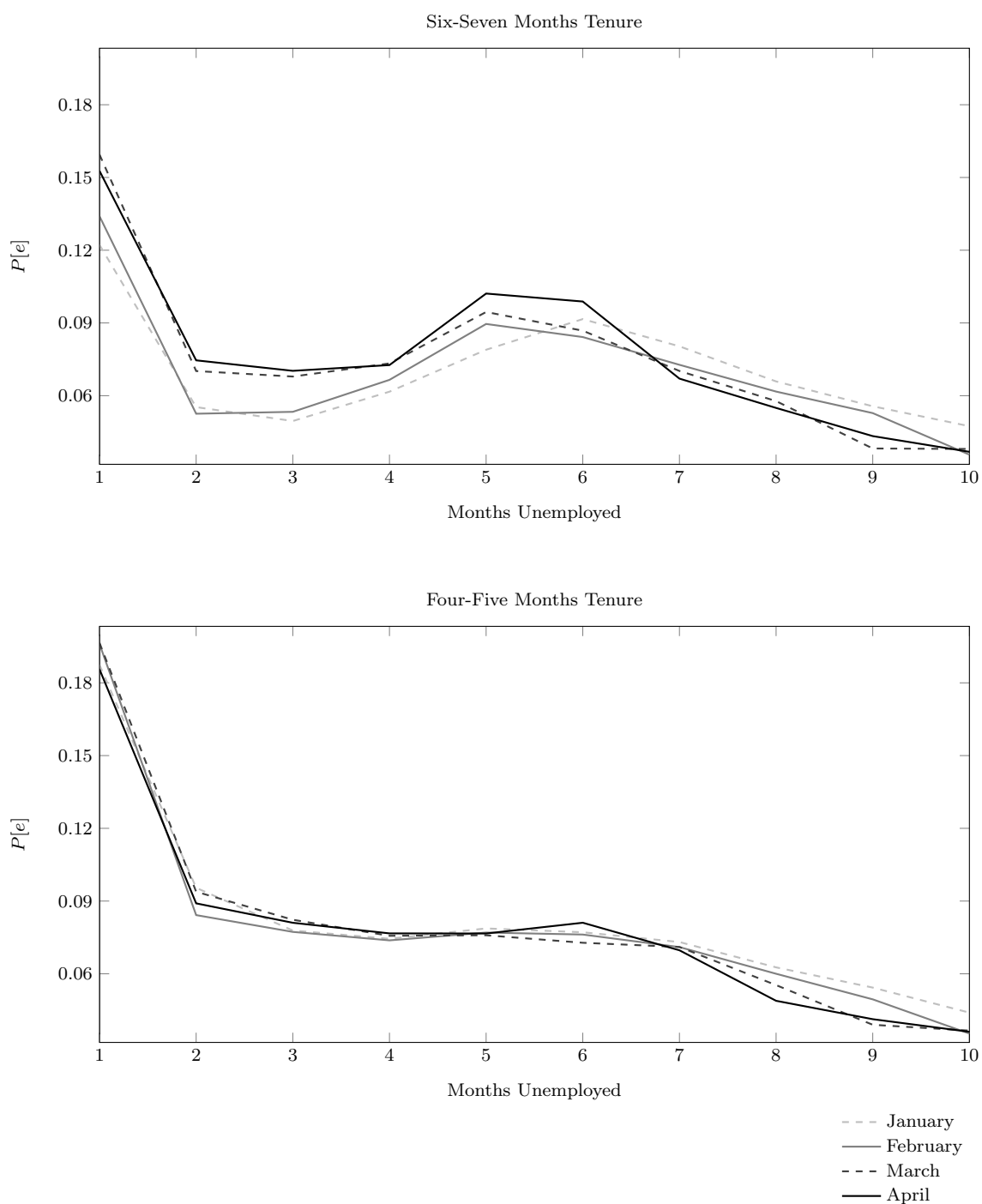
This figure illustrates eligibility criteria for UI benefits before and after the reform for workers with different tenure that apply for UI benefits for the first time, the second time, and the third time or more. Red areas indicate tenure not satisfying eligibility criteria, green areas indicate tenure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

Figure 2: Unemployment Inflow by Tenure



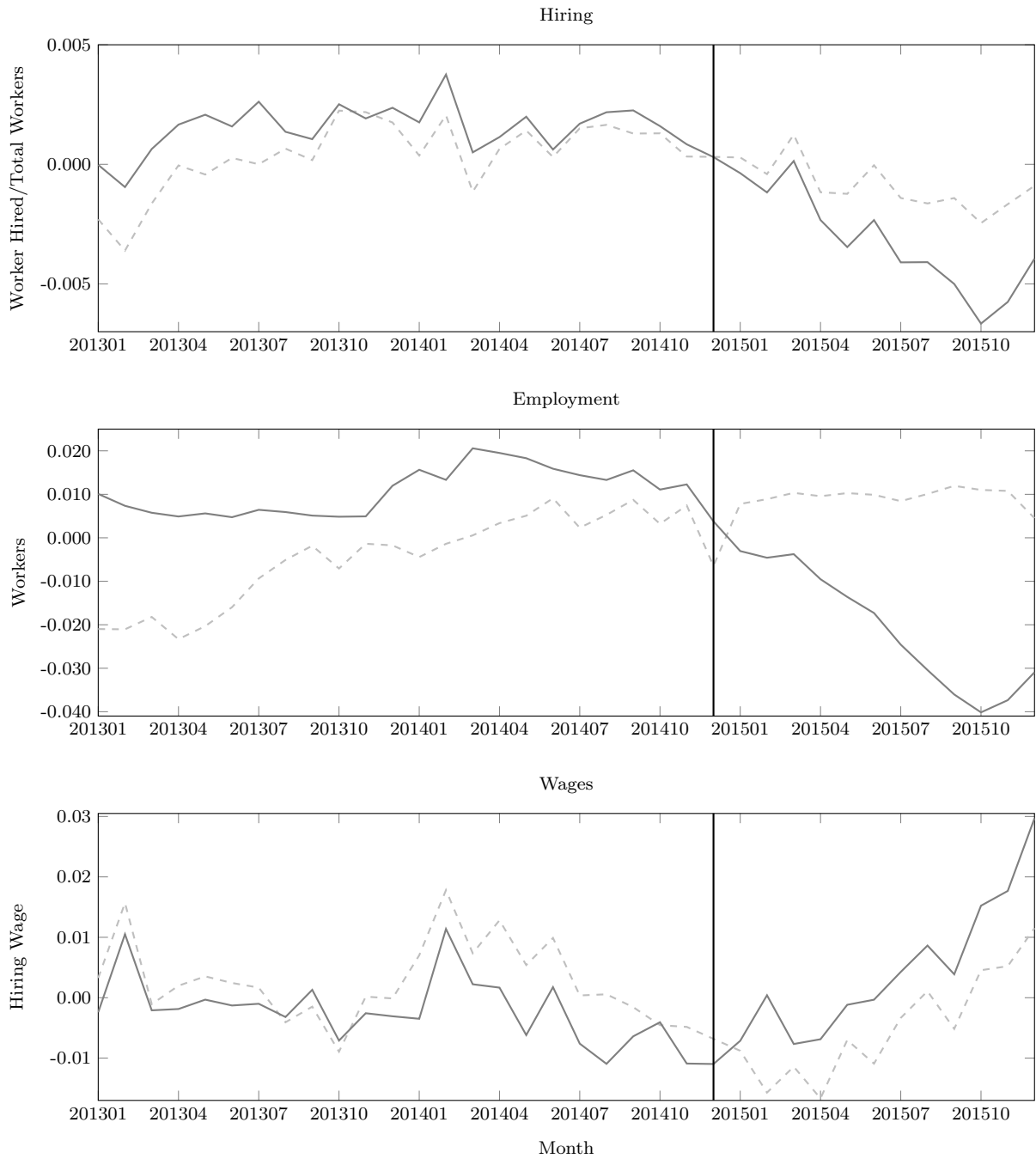
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are vertically aligned at the April probability of layoff for workers with a tenure of five months.

Figure 3: Unemployment Outflow by Tenure



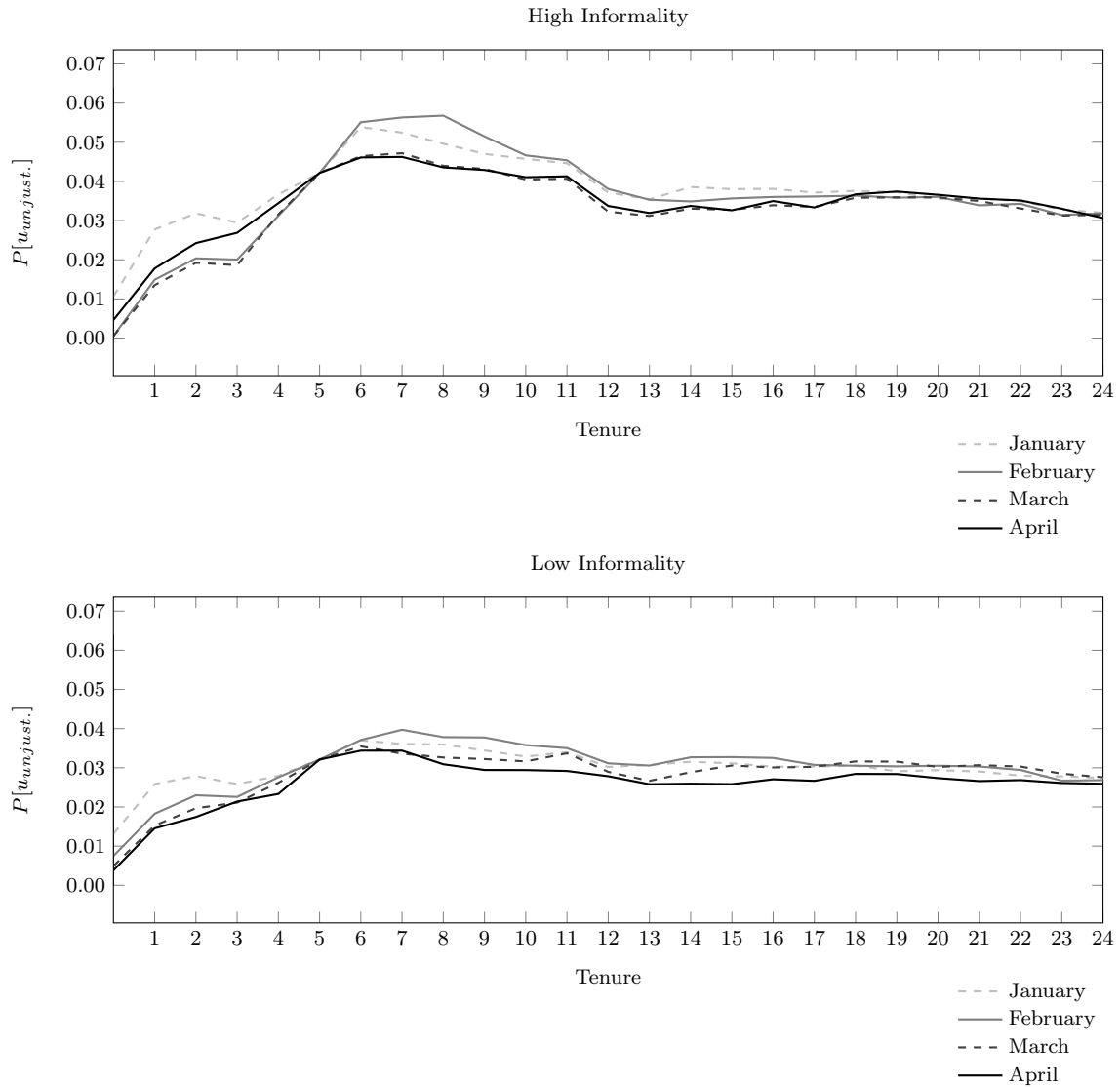
This figure depicts the probability of reemployment in the months after losing their job for workers with a tenure of six or seven months (top panel) and four or five months (bottom panel) at layoff, separately for the months from January to April 2015.

Figure 4: **Formal Hiring, Employment, and Wages**



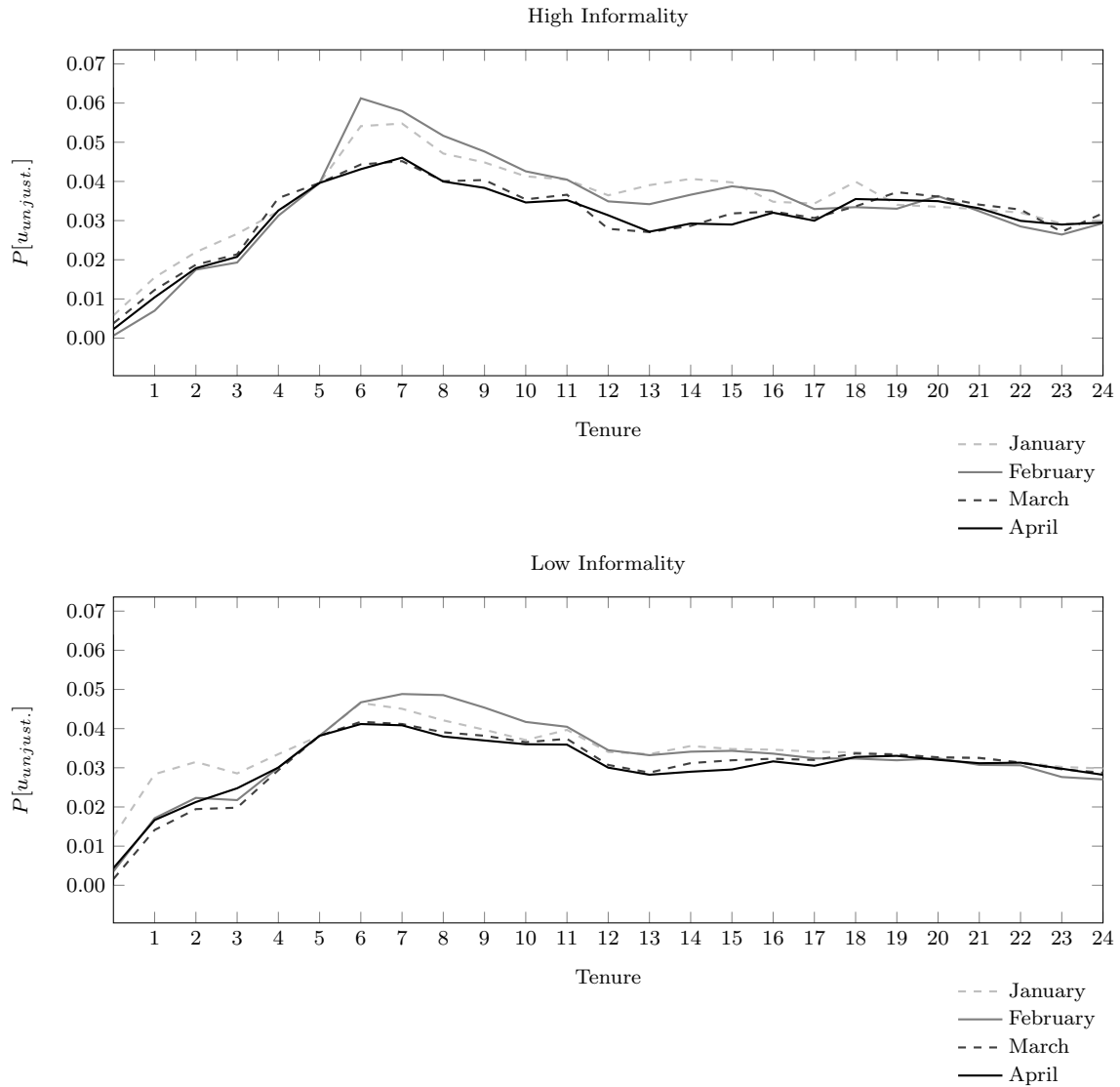
This figure depicts time-series changes in formal hiring, total employment, and wages around the announcement of the UI benefits reform from January 2013 to December 2015. The unit of observation is at the average across all industry-municipality level observations in a given month for workers with less 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 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.

Figure 5: Unemployment Inflow by Informality - Industry Level



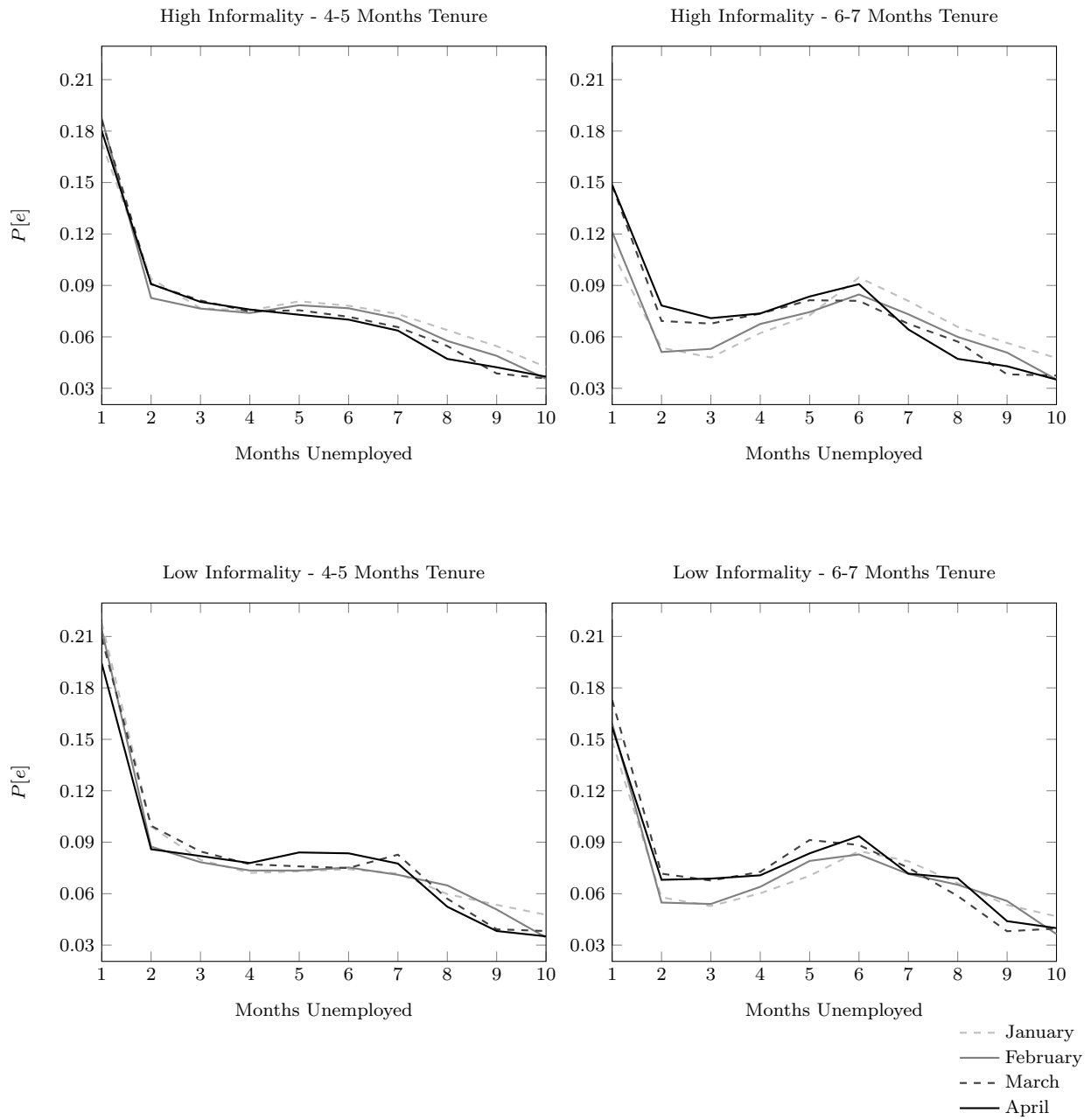
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in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

Figure 6: Unemployment Inflow by Informality - Municipality Level



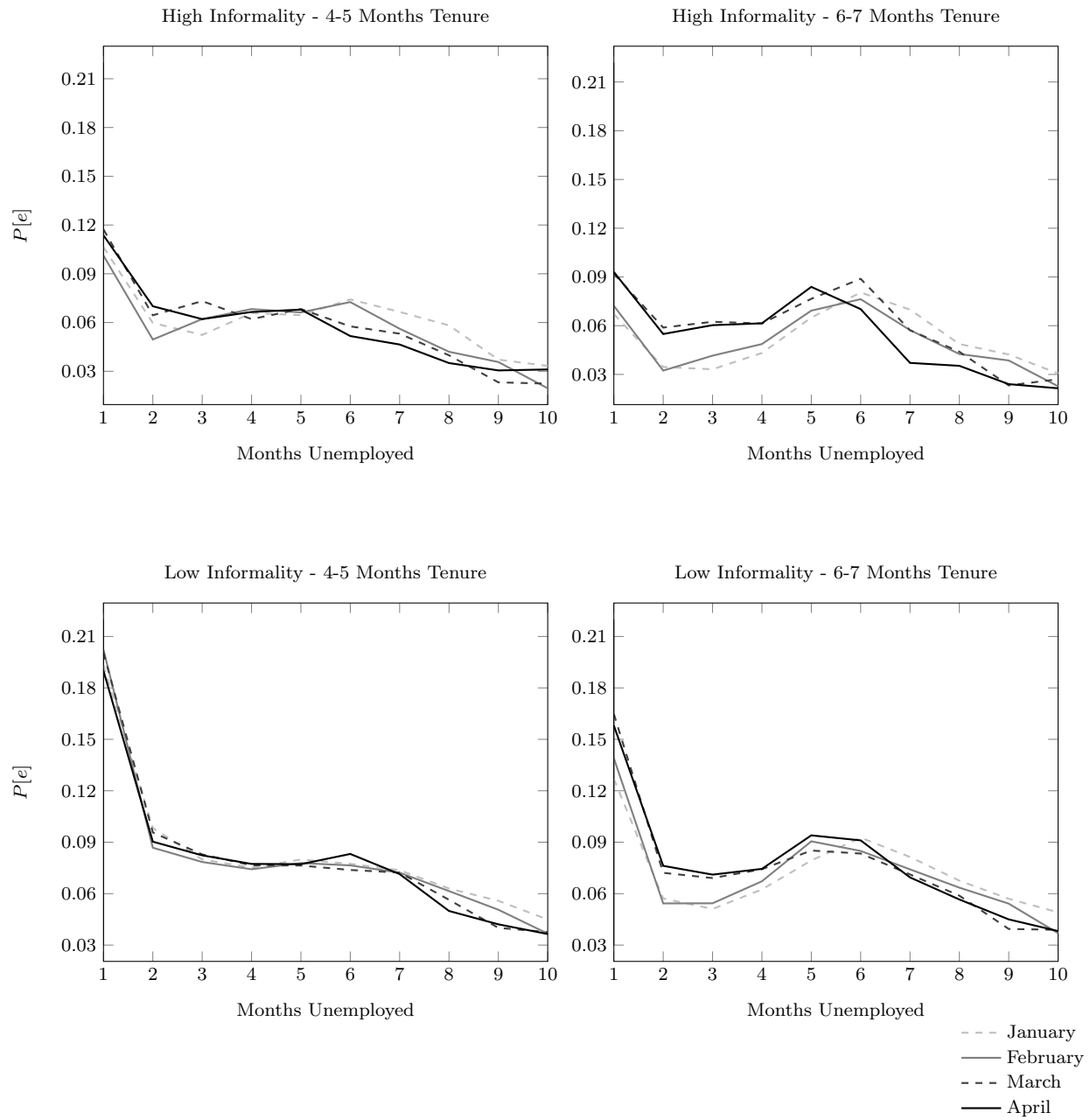
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months. The sample is restricted to workers in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

Figure 7: Unemployment Outflow by Informality - Industry Level



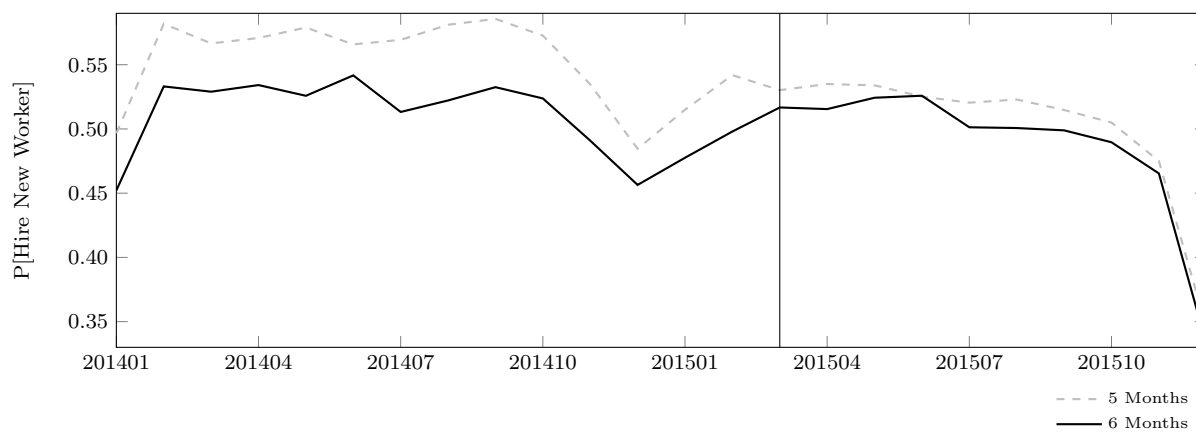
This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (right panels) and four or five months (left panels) at layoff separately for the months from January to April 2015. The sample is restricted to workers in industries with above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

Figure 8: Unemployment Outflow by Informality - Municipality Level



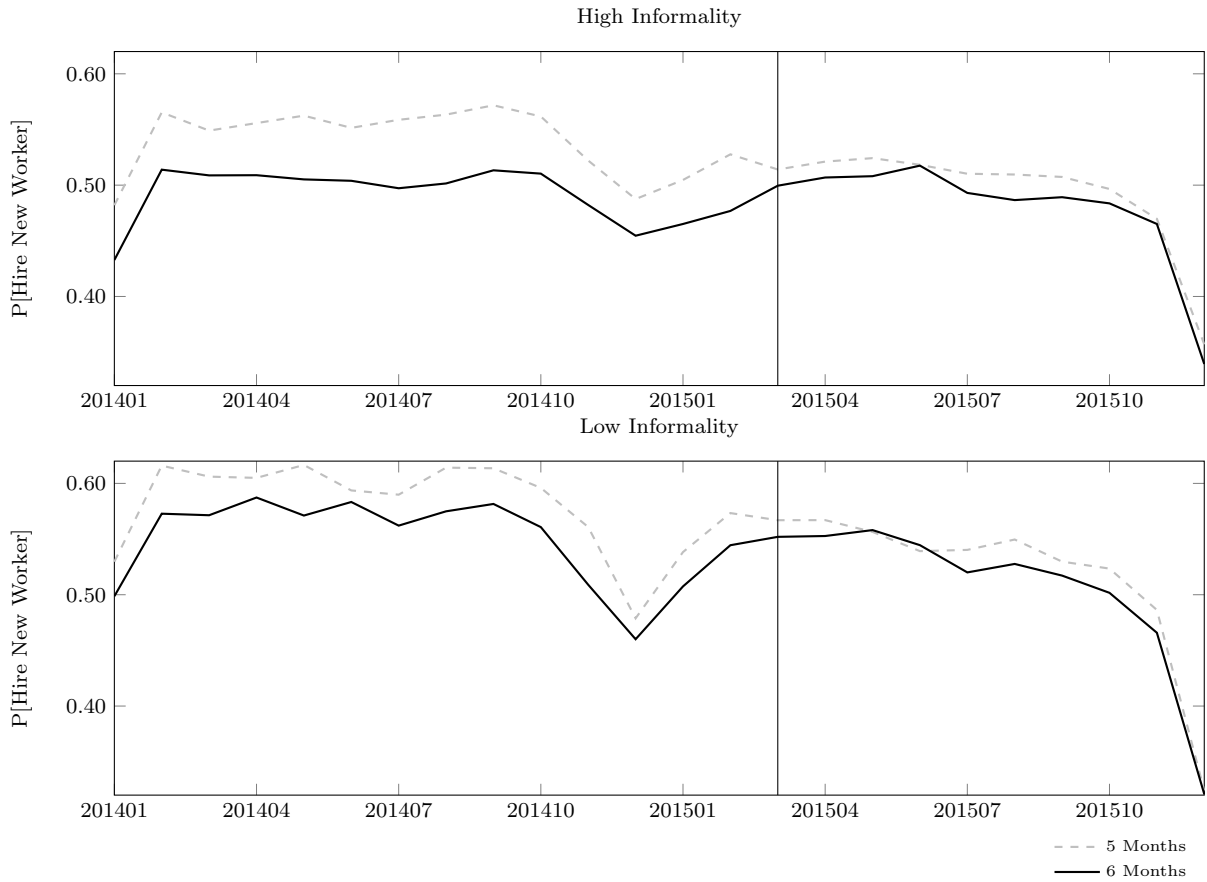
This figure depicts the probability of reemployment in the months after layoff for workers with tenure of six or seven months (right panels) and four or five months (left panels) at layoff, separately for the months from January to April 2015. The sample is restricted to workers in municipalities with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

Figure 9: Layoffs and Replacement Hires



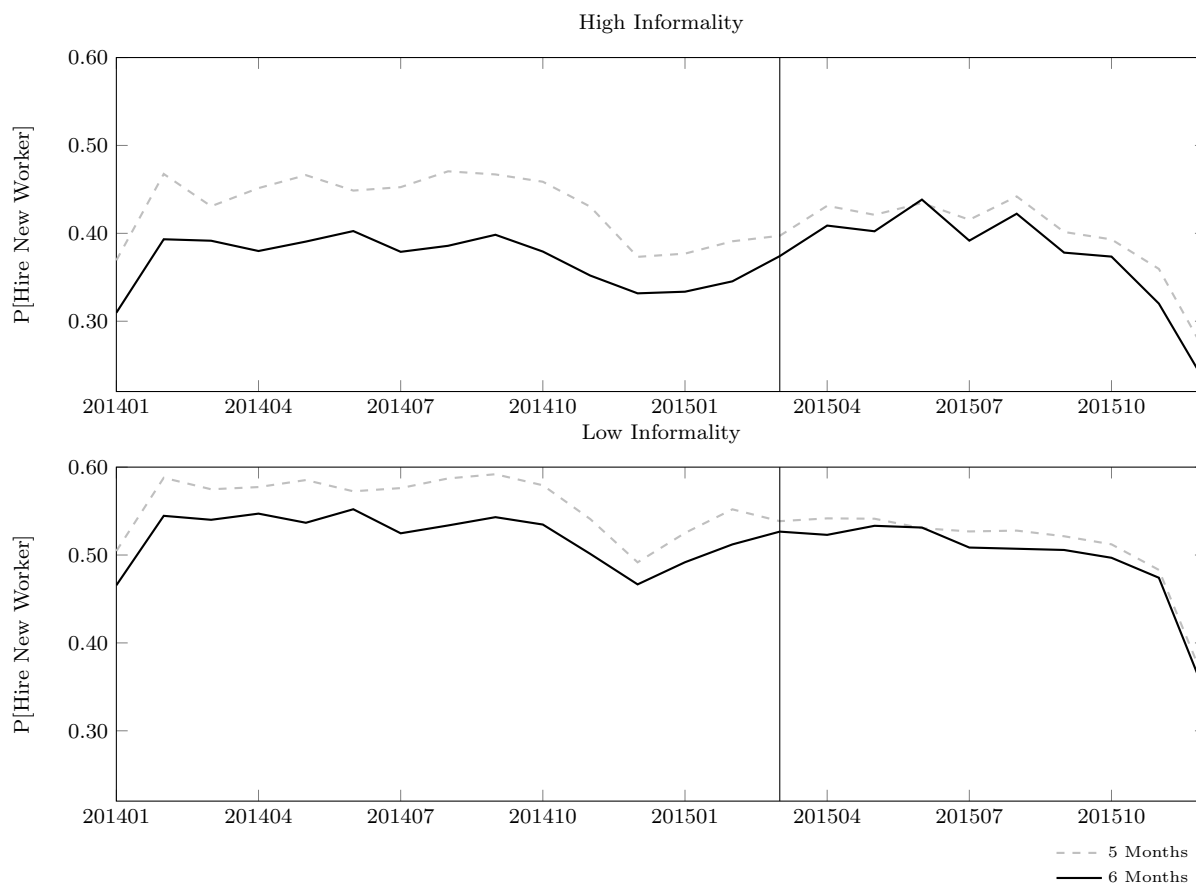
This figure depicts the probability that firms hires a new worker within one month of laying off a worker with a tenure of four to five months (dashed line) and a worker with a tenure of six to seven months (solid line) from January 2014 to December 2015.

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



This figure depicts the probability that firms hires 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).

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



This figure depicts the probability that firms hires 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).

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 industry from the Census Brazil.

Table A.2: Unemployment Inflow - Substitution

Dep. Var.: $P[u_{other}]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0075*** (0.0005)	-0.0077*** (0.0004)	-0.0073*** (0.0004)	-0.0067*** (0.0003)
$6Months_{it} * Reform_t$	0.0005 (0.0009)	-0.0005 (0.0006)	-0.0003 (0.0005)	-0.0006 (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 - Cyclicalit

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0130*** (0.0007)	0.0125*** (0.0006)	0.0124*** (0.0005)	0.0126*** (0.0005)
$6Months_{it} * 2015_t$	-0.0001 (0.0004)	-0.0001 (0.0004)	-0.0001 (0.0003)	-0.0003 (0.0003)
$6Months_{it} * Reform_t$	0.0003 (0.0006)	0.0004 (0.0004)	0.0006* (0.0003)	0.0007** (0.0003)
$6Months_{it} * 2015_t * Reform_t$	-0.0055*** (0.0009)	-0.0058*** (0.0007)	-0.0058*** (0.0006)	-0.0056*** (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 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months in a given month. The dependent variable is a dummy variable that takes the value of one if worker i is laid off in month t and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, and zero for workers with tenure of four or five months. The dummy variable $Reform_t$ takes the value of one for March and April, and zero for January and February. The dummy variable 2015_t takes the value of one for the year 2015, and zero for the year 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level, respectively.

Table A.4: Unemployment Inflow - Unaffected Workers

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

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

Table A.5: Unemployment Outflow - Cyclicity

Dep. Var.: $P[e \leq 5]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0947*** (0.0029)	-0.0950*** (0.0027)	-0.0962*** (0.0028)	-0.0964*** (0.0027)
$6Months_{it} * 2015_t$	0.0075* (0.0043)	0.0091** (0.0038)	0.0101*** (0.0036)	0.0117*** (0.0036)
$6Months_{it} * Reform_t$	0.0187*** (0.0041)	0.0182*** (0.0042)	0.0218*** (0.0046)	0.0226*** (0.0049)
$6Months_{it} * 2015_t * Reform_t$	0.0328*** (0.0034)	0.0324*** (0.0032)	0.0301*** (0.0030)	0.0273*** (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 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months at layoff. The dependent variable is a dummy variable that takes the value of one if worker i returns to formal employment within five months of layoff, and zero otherwise. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months, and zero for workers with tenure of four or five months. The dummy variable $Reform_t$ takes the value of one for March and April, and zero for January and February. The dummy variable 2015_t takes the value of one for the year 2015, and zero for the year 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.6: Unemployment Outflow - Unaffected Workers

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

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

Table A.7: Collusion - Cyclicity

Dep. Var.: $P_{same}[4-9]_{it}$	I	II	III	IV
$6Months_{it}$	0.0271*** (0.0050)	0.0180*** (0.0020)	0.0159*** (0.0017)	0.0159*** (0.0020)
$6Months_{it} * 2015_t$	0.0033 (0.0034)	0.0028 (0.0021)	0.0033 (0.0021)	0.0024 (0.0023)
$6Months_{it} * Reform_t$	-0.0039 (0.0085)	-0.0046** (0.0022)	-0.0022 (0.0022)	-0.0020 (0.0025)
$6Months_{it} * 2015_t * Reform_t$	0.0121* (0.0071)	0.0123*** (0.0029)	0.0134*** (0.0030)	0.0130*** (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 exhaustion of UI benefits around the enactment of the UI reform from January to April 2015 to the period from January 2014 to April 2014. The sample is limited to workers with tenure of four to seven months at layoff that are rehired within nine months. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. The dummy variable $Reform_t$ takes the value of one for March and April, and zero for January and February. The dummy variable 2015_t takes the value of one for the year 2015, and zero for the year 2014. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. ***, **, and * denote statistical significance at the 1%, 5%. and 10% level, respectively.

Table A.8: Collusion - Unaffected Workers

Dep. Var.: $P_{same}[4-9]_{it}$	I	II	III	IV
$6Months_{it}$	0.0565*** (0.0133)	0.0205*** (0.0038)	0.0162*** (0.0034)	0.0160*** (0.0042)
$6Months_{it} * Reform_t$	0.0039 (0.0028)	-0.0074 (0.0050)	-0.0033 (0.0050)	-0.0052 (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 exhaustion of UI benefits 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 sample is limited to workers with tenure of four to seven months at layoff that are rehired within nine months in columns I-III and within four to nine months in columns IV-VI. The dependent variable is a dummy variable that takes the value of one if worker i is formally reemployment by the same firm four to nine months after being laid off, and zero if employed by another firm within four to nine months after being laid off. The dummy variable $6Months_{it}$ takes the value of one for workers with tenure of six or seven months at layoff, and zero for workers with tenure of four or five months at layoff. The dummy variable $Reform_t$ takes the value of one for the post-reform period from March to April 2015 and zero for the pre-reform period from January to February 2015. Standard errors are reported in parentheses. The bottom part of the table reports information on fixed effects and the clustering of standard errors. *** denotes statistical significance at the 1% level.

Table A.9: Replacement Hiring - Unaffected Workers

Dep. Var.: $P[\text{replacement hire}]_{it}$	I	II	III	IV
$6Months_{it}$	-0.0266*** (0.0031)	-0.0221*** (0.0031)	-0.0203*** (0.0034)	-0.0202*** (0.0043)
$6Months_{it} * Reform_t$	0.0073 (0.0044)	0.0037 (0.0045)	0.0019 (0.0048)	0.0041 (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

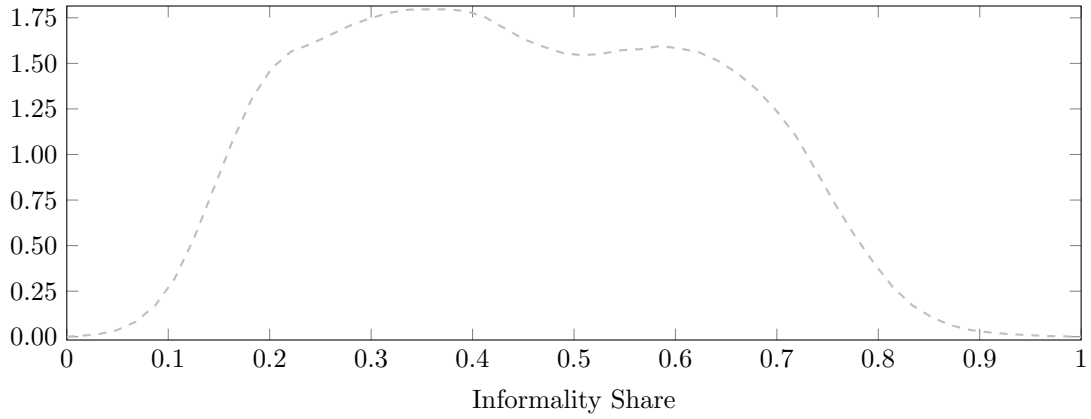
This table reports changes in the hiring of a replacement workers within one year of the layoff of another worker by the same firm around the enactment of the UI reform from January to April 2015 for workers who receive UI benefits at least twice in the past. The dependent variable is a dummy variable 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.10: Unemployment Inflow - Announcement Effects

Dep. Var.: $P[u_{unjust.}]_{it}$	I	II	III	IV
$6Months_{it}$	0.0130*** (0.0007)	0.0126*** (0.0007)	0.0129*** (0.0006)	0.0129*** (0.0006)
$6Months_{it} * Reform_t$	-0.0053*** (0.0009)	-0.0055*** (0.0007)	-0.0059*** (0.0007)	-0.0055*** (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

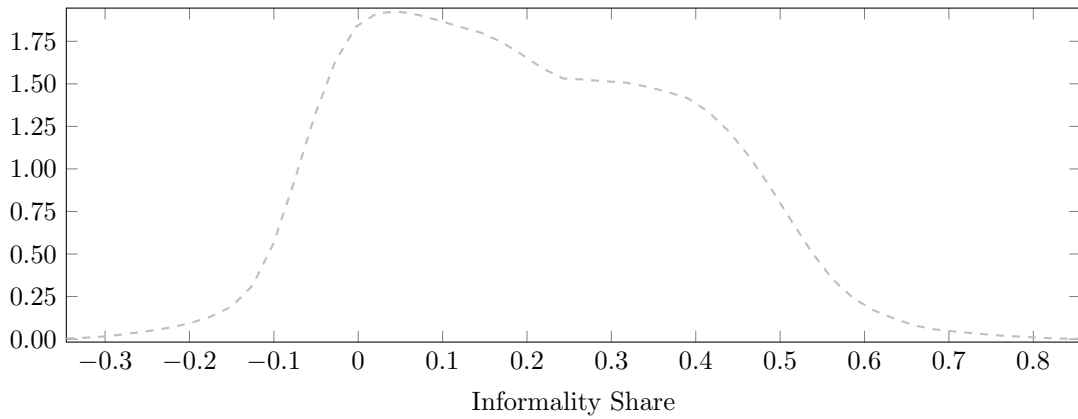
This table reports changes in unemployment inflow around the announcement of the UI reform. The sample is limited to workers with tenure of four to seven months in a given month. The dependent variable is a dummy variable that takes the value of one if worker i is laid off in month t , and zero otherwise. 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.

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



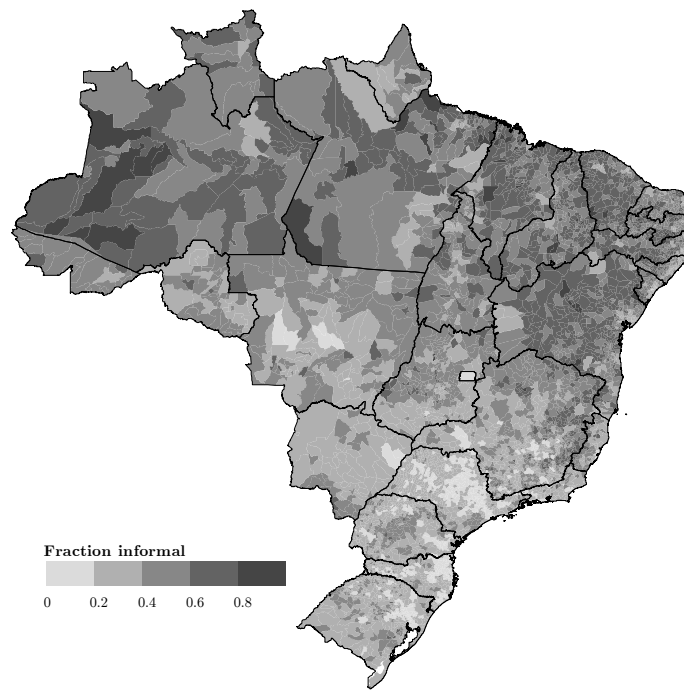
This figure depicts the distribution of the share of informal in total workers across all municipalities in Brazil from the Census Brazil.

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



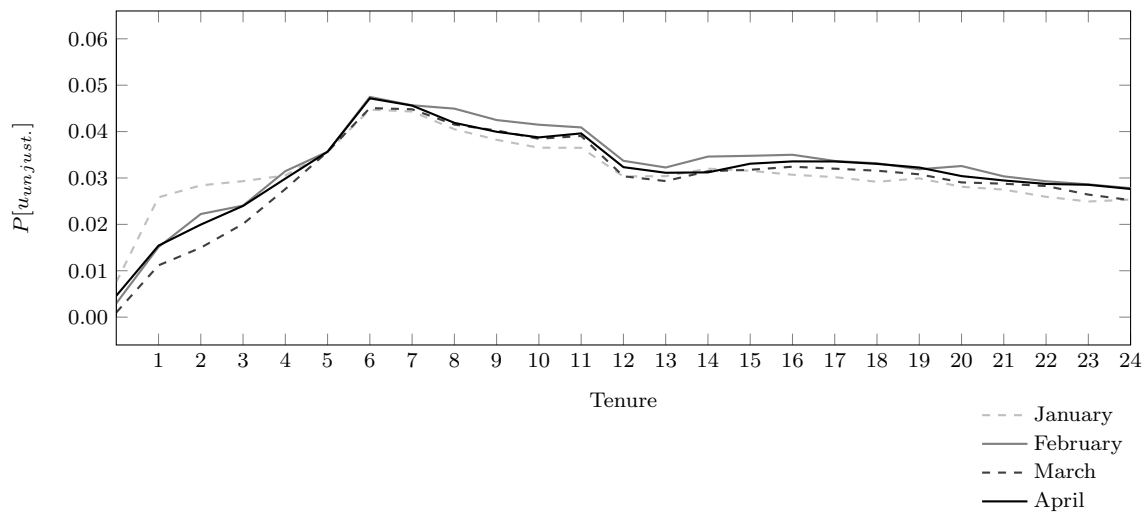
This figure depicts the distribution of difference in the share of informal in total workers and the same share as predicted from the industry composition of the respective municipality across all municipalities in Brazil.

Figure A.3: Labor Market Informality by Municipality



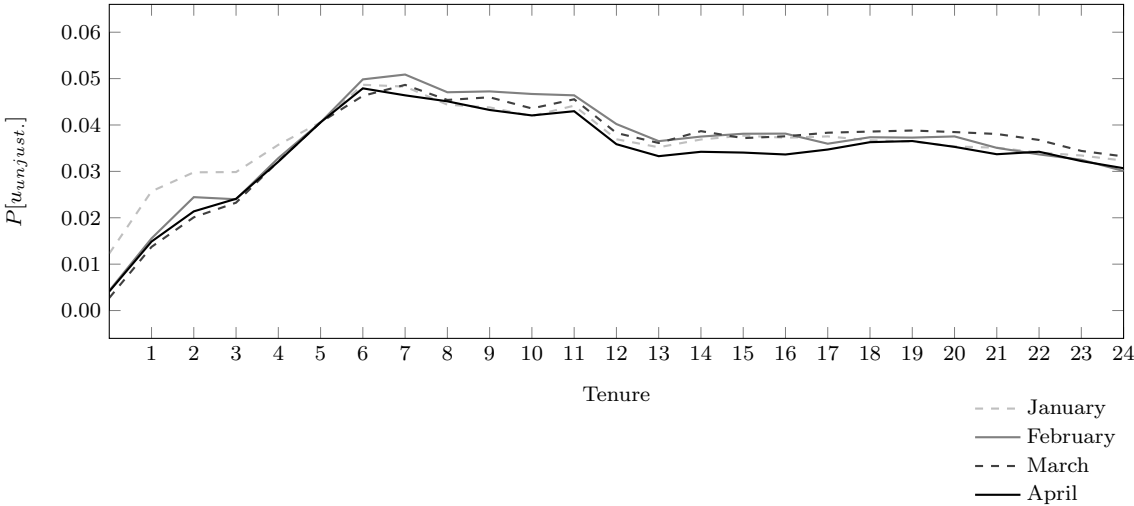
This figure depicts the share of informal in total workers for all municipalities in Brazil from the Census Brazil.

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



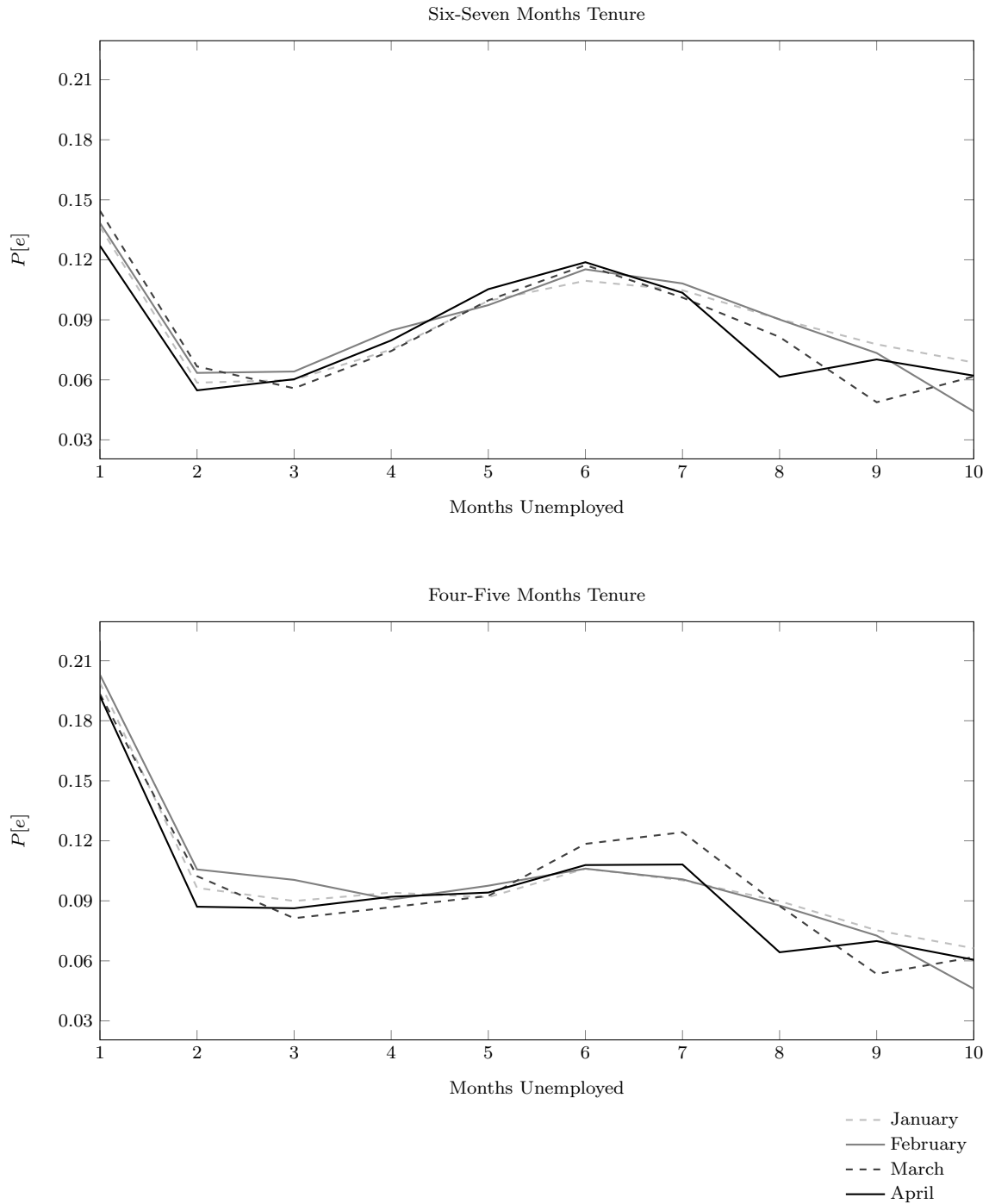
This figure depicts the probability of workers with different tenure to be laid off for the months from January to April 2014, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months.

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



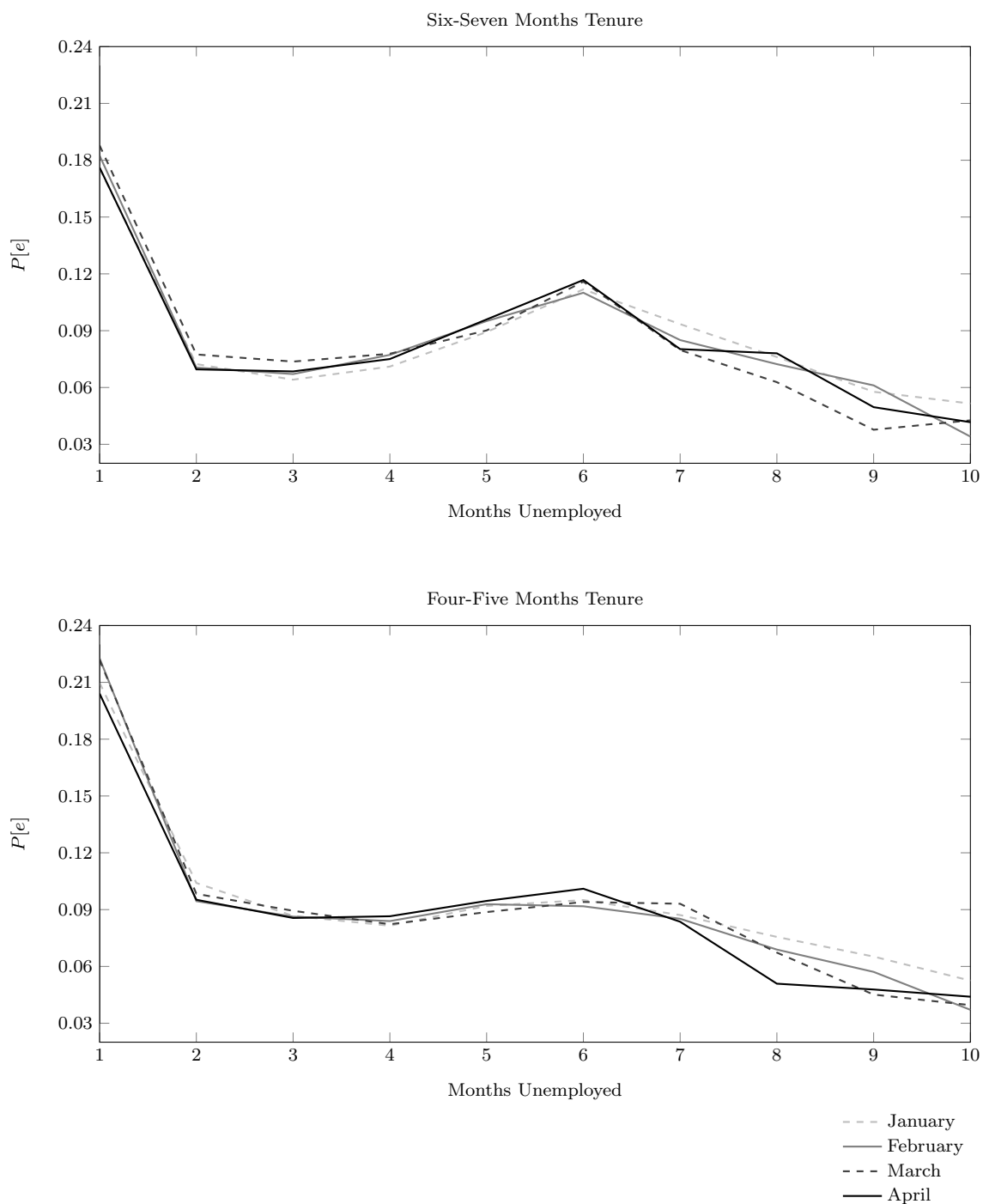
This figure depicts the probability of workers unaffected by the reform with different tenure to be laid off for the months from January to April 2015, separately. To facilitate comparison the plots are aligned at the April probability of layoff for workers with a tenure of five months.

Figure A.6: 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.7: Unemployment Outflow by Tenure - Unaffected Workers



This figure depicts the probability of reemployment in the months after layoff for workers unaffected by the reform 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.8: Layoffs and Replacement Hires - Unaffected Workers



This figure depicts the probability that firms hires 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.