Pontifícia Universidade Católica
do Rio de Janeiro

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Firms' Responses to a Dismissal Tax: Evidence from Brazil

Dissertação de Mestrado

Masters dissertation presented to the Programa de Pós–graduação em Economia, do Departamento de Economia da PUC-Rio in partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor : Prof. Renata Narita
Co-advisor: Prof. Gustavo Gonzaga

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Abstract

Dias de Aquino Nascimento, Bernardo; Narita, Renata (Advisor); Gonzaga, Gustavo (Co-Advisor). **Firms' Responses to a Dismissal Tax: Evidence from Brazil**. Rio de Janeiro, 2026. 62p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

I study the empirical effects of a dismissal tax over firms' behavior. By using a Brazilian law that created a 10% tax over dismissals without cause, I analyze how the tax influenced the firms' layoff and collusion decisions. I do this in the Brazilian context, allowing for fake layoff agreements. To this extent, I develop a conceptual framework to understand the incentives created by the legislation. Guided by this framework, I estimate event-study and difference-in-differences models that exploit cross-firm variation in exposure to the reform. I find that more exposed firms reduce layoffs, particularly among higher-tenure workers, while quits increase and recall rates decline, consistent with a reduction in collusive separations. Additional evidence shows that these effects vary with informality, enforcement intensity, and spell-level exposure. Overall, the results indicate that dismissal taxes affect different separation margins, highlighting the role of institutional context and strategic behavior in shaping firms' responses to labor market policies.

Keywords

Severance Pay; Dismissal Tax; Fake Layoff Agreements; Labor Legislation.

Resumo

Dias de Aquino Nascimento, Bernardo; Narita, Renata; Gonzaga, Gustavo. **Respostas das Firmas a um Imposto sobre Demissões: Evidências do Brasil**. Rio de Janeiro, 2026. 62p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Explorando uma lei brasileira que introduziu uma alíquota de 10% sobre demissões sem justa causa, eu analiso como o imposto afetou o comportamento empírico das firmas em relação a demissões e falsos acordos de demissão. Para isso, desenvolvo um arcabouço conceitual que busca compreender os incentivos gerados pela legislação. Guiado por esse modelo, estimo regressões de estudo de evento e de diferença-em-diferenças que exploram a variação na intensidade de tratamento entre firmas. Os resultados mostram que firmas mais expostas ao imposto reduzem demissões, especialmente entre trabalhadores com maior tempo de empresa. Ao mesmo tempo, os pedidos de demissão dos trabalhadores aumentam e as re-contratações diminuem, de forma consistente com uma redução nos falsos acordos. Evidências adicionais indicam que esses efeitos variam com o grau de informalidade, a intensidade de fiscalização e a exposição ao nível do trabalhador. Em conjunto, os resultados indicam que o imposto sobre demissões afeta diferentes margens de separação, destacando o papel do contexto institucional e do comportamento estratégico das firmas diante de políticas do mercado de trabalho.

Palavras-chave

Indenização Trabalhista; Imposto Demissional; Falsos Acordos; Legislação Trabalhista.

Table of Contents

List of Figures

List of Tables

List of Abbreviations

AN – Advance Notice
CLT – *Consolidação das Leis do Trabalho* (Labor code)
CNPJ – *Cadastro Nacional de Pessoa Jurídica* (Firm tax ID)
FE – Fixed Effects
FGTS – *Fundo de Garantia do Tempo de Serviço* (Severance account)
IBGE – *Instituto Brasileiro de Geografia e Estatística*
PME – *Pesquisa Mensal do Emprego* (Dataset)
RAIS – *Relação Anual de Informações Sociais* (Dataset)
SP – Severance Payment
UI – Unemployment Insurance

1
Introduction

Laying off a worker is costly for the firm. When an employer dismisses an employee without cause, it may incur severance payments, advance-notice obligations, administrative delays, litigation risk, and taxes. Economists commonly distinguish two broad components of these dismissal costs: transfers from the firm to the worker, and payments or procedures that fall outside the firm–worker pair.

Following the influential work of Lazear (1990), mandated severance payments (SP) should be offset by wage bargaining in frictionless settings. Therefore, transfers from the firm to the worker–SP–should not affect real labor-market outcomes. By contrast, taxes and third-party payments can create real distortions in firms' separation incentives. As synthesized by Ljungqvist (2002), the theoretical literature generally predicts that higher dismissal taxes reduce layoffs, although their implications for aggregate employment are model dependent. Despite this rich theory, causal empirical evidence on the effects of dismissal taxes remains scarce.

This work studies the empirical effects of a nationwide dismissal tax using a 2001 reform in Brazil. The reform introduced a 10% tax on dismissals without cause by adding a levy within the existing severance savings system, the *Fundo de Garantia do Tempo de Serviço* (FGTS). The tax applied to all private, formal-sector firms and thereby generated cross-firm variation in pre-reform exposure. I exploit this exposure to identify how increased dismissal taxes affect firms' separation margins in a context where illegal collusion between firms and workers may occur.

The FGTS is a government-mandated seniority savings account associated with each formal private-sector spell. Every month, employers must deposit 8% of the worker's wage into an individual account held in the worker's name. The account accrues interest and its balance can typically only be withdrawn in the event of a layoff, with a few exceptions.[1]

In addition to the FGTS balance, firms face government-mandated severance payments when dismissing a worker without cause. Prior to 2001, employers were required to pay to the dismissed employee a severance fine equivalent to 40% of the worker's accumulated FGTS balance. The 2001 reform introduced an additional 10% tax on the same balance, payable to the government.[2] This reform therefore increased the cost of dismissing a worker from 40% to 50% of the FGTS balance, while directing the additional 10% payment to the government.

The Brazilian institutional environment also creates opportunities for strategic behavior between firms and workers. To formalize the incentives

[1] Exceptions include retirement, the purchase of residential real estate, serious illnesses such as cancer or HIV/AIDS, accounts that have remained inactive for more than five years, and certain natural disasters.

[2] The 10% dismissal tax was abolished in January 2020. However, due to data limitations, simultaneous legislative changes, and the Covid-19 pandemic, I do not analyze the causal effects of its removal.

generated by the legislation, I develop a model with endogenous separations that captures an important aspect of the labor market in developing countries: collusive dismissals.

As documented in the literature,[3] there is evidence for such collusive dismissals, referred to as "fake layoff agreements". In these agreements, firms and workers jointly simulate a dismissal without cause. The worker gains access to the FGTS balance and unemployment insurance (UI) benefits, while the employer avoids most dismissal costs. Although firms are formally required to pay SP, the worker may acknowledge receipt of the payment even if it was not made, or return the amount to the employer after the dismissal. Because these agreements are illegal, they cannot be observed directly in the data. I therefore rely on the conceptual framework and indirect indicators to analyze their presence.

Allowing for this margin of adjustment in the model implies that dismissal taxes may affect not only legal separations but also the incentives to engage in collusive dismissals. By increasing the cost of formal layoffs, the reform also raises the cost of sustaining fake layoff agreements. The conceptual framework therefore predicts that an increase in dismissal taxes should reduce layoffs, increase quits, and reduce fake layoffs.

To test the implications of the conceptual framework, I estimate the effects of the dismissal tax using event-study and difference-in-differences designs. Although the reform applied to all firms simultaneously, its impact varied across them depending on the FGTS balances of their employees, which determine the tax base. I exploit this variation by constructing a measure of firms' pre-reform exposure to the treatment. Specifically, I calculate the dismissal tax firms would have paid prior to the reform as a share of payroll. This measure captures each firms' exposure intensity to the increase in dismissal costs and allows me to compare firms with higher and lower exposure after the reform. Also, note that this measure is determined by both firm turnover and individual layoff cost, suggesting that firms that with higher turnover rates or that dismiss more tenured and higher wage workers are more affected by the reform.

Comparing firms at the 75th and 25th percentiles of the exposure distribution, I find that the reform led to a 19.8% decrease in quarterly layoffs and a 7.9% increase in quarterly quits. I also examine recall rates as an indirect indicator of fake layoff agreements. A recall occurs when a worker is laid off and subsequently rehired by the same establishment. Such arrangements allow workers to access FGTS balances and UI while maintaining an informal employment relationship with the firm. Consistent with a decline in collusive dismissals, recall rates decline following the reform.

Another relevant source of variation in the prevalence of fake layoff agreements is labor market informality across regions. To capture this dimension, I combine RAIS data with municipal-level informality rates constructed from the 2000 Population Census. I then estimate a triple-difference-in-differences specification that allows the effect of the dismissal tax to vary with local informality.

[3] See Camargo (1996), Gonzaga (2003), Camargo (2006), Carvalho, Corbi and Narita (2018), Doornik, Schoenherr and Skrastins (2023), among others.

The results indicate that the firm-level layoff response to the dismissal tax is attenuated in more informal municipalities. A natural interpretation is that the reform is less binding in these areas, as the formal firms that are affected by the layoff tax are less relevant in those local labor markets. Consequently, the average response is weaker in high-informality regions, where a larger fraction of economic activity lies outside the scope of the policy.

In addition to informality, variation in enforcement intensity provides another important source of heterogeneity. Labor inspections increase the expected cost of engaging in fake layoff agreements by raising the probability that such behavior is detected and sanctioned. I exploit geographic variation in enforcement by using the distance between establishments and the nearest labor inspection office as a proxy for inspection intensity. Firms located closer to inspection offices are expected to face stronger monitoring and, consequently, weaker incentives to engage in collusive dismissals.

In this context, the introduction of the dismissal tax should have a larger effect on those firms, as the scope for evasion through fake layoff agreements is more limited. This happens because, under collusion, the workers pay a share of the additional layoff tax. As a result, I expect a stronger reduction in layoffs. I test this hypothesis by a triple difference-in-differences that allows the effect of the dismissal tax to vary with local enforcement.

Finally, a last approach is to study the behavior of layoff hazard rates around institutional thresholds associated with fake layoff agreements. Three such thresholds are particularly relevant in the Brazilian institutional setting. First, there are no dismissal costs when firing a worker with less than three months tenure. Second, workers become eligible for unemployment insurance after six months of tenure. Third, after twelve months of tenure, dismissals must be formally overseen by a representative of the Ministry of Labor or the worker's union.

These thresholds create incentives consistent with collusive dismissals. The layoff tax does not apply for workers with less than three months tenure and those workers also have no incentives to force their dismissal to gain UI. We should then expect no effect of the layoff tax over this tenure. At the six-month threshold, the incentives are more relevant. Here, the worker is eligible for UI and the firm must pay the layoff tax. However, the separation is not overseen by a representative, which means that there is no enforcement if the firm-worker pair are colluding in a fake layoff agreement. Hence, we should expect larger effects over layoffs at this tenure if fake layoff agreements are being limited by the layoff tax. Finally, at twelve months tenure, the tax is binding and collusion between firm and workers is less likely to occur, as there is a higher chance of getting caught. Therefore, we should expect even larger effects over layoffs at this tenure due to the increase in dismissal costs.

Overall, layoffs become more costly after three months tenure, fake layoff agreements become more attractive once workers become eligible for UI at six months of tenure, and the requirement of external oversight after twelve months increases the probability that collusive arrangements are detected. I therefore study how separation hazard rates around these cutoffs change following the introduction of the dismissal tax, comparing firms with higher and lower exposure to the reform.

At first glance, the hazard analysis does not reveal a clear aggregate reduction in separation probabilities following the introduction of the dismissal tax. To further investigate this, I estimate a Cox proportional hazard model that exploits variation in dismissal costs at the worker level. I construct a spell-level measure of treatment intensity defined as the hypothetical tax payment associated with dismissing a worker at tenure $T = \min\{\text{layoff date, reform date}\}$, based on pre-reform information.

The results indicate that workers with higher exposure to the reform experience significantly lower layoff hazards after its introduction. In particular, comparing workers at the 75th percentile of the exposure distribution to those at the 25th percentile implies a 12.5% reduction in layoff hazard rates. This evidence complements the firm-level analysis by showing that the increase in dismissal costs not only reduces overall layoffs, but also decreases the layoff for high-tenured workers at the moment of the change.

Related Literature: This paper contributes to the literature on dismissal costs and employment protection. The influential work of Lazear (1990), alongside Bentolila and Bertola (1990) and Bertola (1990), motivated a large theoretical literature studying the effects of firing costs on labor market outcomes. These models are synthesized in Ljungqvist (2002), which shows that higher dismissal costs generally reduce layoffs, while their effects on employment are theoretically ambiguous and depend on the specific model environment. For instance, Hopenhayn and Rogerson (1993) predicts that layoff costs reduce employment, whereas Mortensen and Pissarides (1999) finds that they may increase employment.

More recently, Garibaldi and Violante (2005) emphasize the dual nature of dismissal costs, distinguishing between transfers paid to workers and payments made to third parties, such as the government. They argue that the former has received comparatively less attention in the literature following the neutrality result in Lazear (1990). However, severance payments often represent the largest component of dismissal costs and may have important effects in the presence of labor market frictions.

Overall, this work contributes to this literature by providing causal empirical evidence on the effects of a nationwide increase in dismissal taxes. This setting allows for a direct comparison between theoretical predictions and observed firm behavior. Not only this, but this work also expands on a new theoretical margin of adjustment: fake layoff agreements.

There is also some empirical literature studying dismissal taxes. Behaghel, Crépon and Sédillot (2008), Schnalzenberger and Winter-Ebmer (2009), and Messe and Rouland (2014) analyze reforms that impose dismissal taxes targeted at older workers. The first and the last examine a French policy affecting workers above age 50, while the second studies a similar reform in Austria.

Behaghel, Crépon and Sédillot (2008) find that exempting workers hired after age 50 from the tax significantly increased their transition rates from unemployment to employment, with more modest effects on layoffs. Schnalzenberger and Winter-Ebmer (2009) show that tightening the tax reduces layoffs, while Messe and Rouland (2014) find that increasing dismissal taxes for larger firms led to greater training investments for workers just below the age-50

threshold.

In contrast to these studies, this dissertation analyzes a dismissal tax that applies broadly to the formal private sector rather than to a specific group of workers. Studying a nationwide reform provides evidence on how firms adjust to general increases in dismissal costs, thereby complementing the existing empirical literature focused on targeted policies.

The Brazilian literature has also examined how labor legislation shapes firms' separation incentives. A common assessment in this literature is that the institutional design of dismissal costs creates incentives that may increase turnover despite relatively strict employment protection (CAMARGO, 1996; BARROS; CORSEUIL; BAHIA, 1999; GONZAGA, 2003; CAMARGO, 2006).

Among these studies, Gonzaga (2003) is particularly related to this paper, as it analyzes both the 1988 constitutional reform and the 2001 introduction of the 10% dismissal tax studied here. Using worker-level data from the *Pesquisa Mensal de Emprego* (PME), Gonzaga finds that the reform increased previous job duration among unemployed workers from 1.3 to 2 years. In addition, using indirect indicators, the paper provides evidence consistent with a decline in fake layoff agreements following the introduction of the tax.

This paper complements Gonzaga (2003) in two main dimensions. First, while Gonzaga (2003) studies outcomes at the worker level, I analyze firms' separation behavior. Second, I use matched employer–employee administrative data rather than survey data. By exploiting this complementary data environment, this paper provides new evidence on how firms adjust their separation decisions in response to the dismissal tax.

A related literature studies strategic layoffs associated with UI eligibility in Brazil. Carvalho, Corbi and Narita (2018) and Doornik, Schoenherr and Skrastins (2023) analyze the 2015 reform that tightened the eligibility requirements for UI benefits and provide evidence consistent with strategic layoffs aimed at accessing these benefits.

Carvalho, Corbi and Narita (2018) examines the behavior of separation hazard rates around the UI eligibility threshold and documents discontinuities consistent with strategic behavior. Doornik, Schoenherr and Skrastins (2023) studies several indirect indicators of collusive dismissals, including recalls, transitions to informality, worker replacement patterns, and wage dynamics.

This paper complements this literature by focusing on another institutional component of fake layoff agreements: the severance system. While those studies examine how UI incentives shape separation behavior, this paper analyzes how changes in dismissal taxes affect the incentives for firms and workers to engage in such arrangements. Methodologically, I follow a similar empirical approach by examining both hazard rates around institutional thresholds and a set of indirect indicators of collusive dismissals.

Taken together, the existing literature provides important theoretical and empirical insights on dismissal costs and strategic separations. However, causal empirical evidence on the effects of dismissal taxes remains limited, particularly in settings where such taxes apply broadly across the formal labor market and where firms and workers may engage in strategic behavior. This paper contributes to this literature by studying the introduction of a nationwide dismissal tax in Brazil. By combining administrative employer–employee data,

variation in firms' exposure to the reform, and evidence from hazard rates and indirect indicators of collusive behavior, the analysis provides new evidence on how dismissal taxes affect firms' separation decisions and the incentives for fake layoff agreements.

The remainder of the dissertation is organized as follows. Chapter 2 describes the institutional background. Chapter 3 presents the conceptual framework. Chapter 4 describes the data. Chapter 5 presents the firm-level results, and Chapter 6 presents the hazard analysis. Chapter 7 concludes.

2
Institutional Background

This chapter briefly describes the institutional features of the Brazilian labor legislation that are most relevant for firms' and workers' separation decisions.[1]

2.1
FGTS

The *Fundo de Garantia do Tempo de Serviço* (FGTS) is a mandatory severance savings system associated with formal private-sector employment relationships. Each month, employers must deposit 8% of the worker's wage into an individual FGTS account held in the worker's name. These accounts have been managed by the state-owned bank *Caixa Econômica Federal* since 1989. The contribution rate was originally designed so that the accumulated deposits would correspond approximately to one monthly wage per year of tenure.

Workers can typically access the accumulated balance only upon dismissal without cause or retirement, with a limited number of exceptions. As a result, the timing of access to these funds depends largely on the termination of the formal employment relationship.

The remuneration of FGTS accounts is relatively low. Balances accrue interest at a rate of 3% per year plus the *taxa referencial*–2.45% in 2001. In several periods, this return has been below inflation, reducing the real value of accumulated balances. Because workers may gain access to these funds if they are dismissed without cause, this institutional design creates incentives for strategic behavior in which workers and firms collude to simulate layoffs to withdraw the accumulated balance.

2.2
Dismissal Costs

Severance Pay. When a worker is dismissed without cause, the employer must pay a severance fine equivalent to 40% of the worker's accumulated FGTS balance. This payment is made directly from the firm to the worker, which allows for collusive arrangements between the two parties.

Advance Notice. Firms must provide a one-month advance notice prior to dismissal. During this period, workers are entitled to reduced working hours in order to search for a new job. In practice, firms often opt to compensate the worker with an additional monthly salary and terminate the employment relationship immediately, which is permitted by law.

Homologation ("*Homologação*"). If the worker has more than twelve months of tenure at the time of dismissal, the termination must be formally overseen by a representative of the worker's union or the Ministry of Labor.

[1] For detailed discussions of Brazilian labor institutions, see Amadeo and Camargo (1996), Colistete (2007), Firpo and Yeung (2018).

This representative verifies that all legally mandated payments have been correctly executed. As a result, dismissals involving workers with longer tenure require additional administrative procedures. This is a relevant heterogeneity threshold that will be explored in Chapter 5.

Court Cases. Brazil's labor legislation has historically generated a large volume of labor litigation (CORBI et al., 2022). Workers may file claims requesting compensation related to their employment relationship, and courts frequently grant fee exemptions to plaintiffs. As a result, workers often face limited financial risk when initiating legal proceedings. If the worker wins the case, the employer must pay the court-determined compensation. If the worker loses, the worker is typically not required to cover the employer's legal expenses. Consequently, the possibility of litigation represents an additional cost and source of uncertainty for firms when dismissing workers.[2]

Dismissal Tax. In 2001, the Brazilian Supreme Court ruled that FGTS balances affected by the 1989 and 1990 currency stabilization plans–*Plano Verão* and *Plano Collor*–should be adjusted by 68.6% in order to compensate for inflation-related losses. Implementing this ruling required a substantial increase in the assets credited to FGTS accounts. In total, the fund credited approximately 40.2 billion BRL, representing a 57% increase in assets relative to the previous year (FGTS, 2001).

To fund this adjustment, in September 2001 the government introduced a 10% dismissal tax on the FGTS balance of workers dismissed without cause. This tax applied to all formal, private sector firms. Consequently, employers dismissing a worker became liable not only for the existing dismissal costs described above but also for the additional tax payment. From the firm's perspective, the reform increased the total cost associated with the FGTS balance from 40% to 50%, with the additional 10% paid to the government. This reform constitutes the policy change analyzed in this paper.

Together, these institutional features determine the costs firms face when dismissing workers and shape the incentives for both legal and strategic separations. However, there is an additional institutional mechanism, the experience period. Firms do not face any real dismissal costs for laying off a worker during his first three months of tenure. Hence, all these dismissal costs become binding only after the 3-month tenure threshold. This is another source of tenure heterogeneity that will be explored in the empirical analysis.

2.3 Unemployment Insurance

Although unemployment insurance (UI) is not the primary focus of this paper, it is an important component of the institutional environment surrounding job separations in Brazil. To qualify for UI benefits, workers must have worked for at least six of the previous thirty-six months, must not receive other pension benefits, must be dismissed without cause, and must remain unemployed for at least one week.

[2]The 2017 labor reform modified several litigation incentives and contributed to a decline in the number of labor court cases. See Corbi et al. (2022) for a detailed discussion.

The duration of UI benefits depends on the worker's employment history. Workers with 6 to 11, 12 to 23, and 24 to 36 months of tenure at the same firm during the previous 36 months are eligible for 3, 4, and 5 monthly benefit payments, respectively. The value of each payment ranges from one to approximately 1.7 minimum wages, depending on the worker's average wage over the previous three months.

Because eligibility requires a dismissal without cause and the benefits can represent a substantial share of workers' income, the UI system may also create incentives for strategic separations in which firms and workers collude to simulate layoffs in order to access these benefits.[3]

2.4 Dismissal Types

Approximately 80% of formal employment relationships in Brazil consist of open-ended contracts regulated by the private-sector labor code, the *Consolidação das Leis do Trabalho* (CLT). These contracts may be terminated for several reasons, including dismissals with cause or transfers to other establishments. For the purposes of this dissertation, I focus on two main types of separation: dismissals without cause initiated by the employer (layoffs) and voluntary separations initiated by the worker (quits).

Under the CLT, firms may generally dismiss workers without cause, except in specific cases of temporary job stability.[4] When a layoff occurs, workers gain access to their accumulated FGTS balance and, if they meet the eligibility requirements, to UI benefits. In addition, employers must pay the dismissal costs described in the previous sections.

Workers may also terminate the employment relationship voluntarily. In the case of a quit, workers do not gain access to their FGTS balances and are not eligible for UI benefits. Moreover, the employer is not required to pay the dismissal costs associated with layoffs.

Finally, the Brazilian labor market also exhibits an informal mode of separation, commonly referred to as a fake layoff agreement. In this arrangement, the firm and the worker collude to simulate a layoff. The worker formally receives a layoff, which allows access to FGTS balances and UI benefits, while severance payments may be waived or returned to the employer. Through this arrangement, the firm avoids part of the dismissal costs associated with a formal layoff. Because such agreements are illegal, they cannot be directly observed in administrative data, but they represent an important margin of adjustment in the Brazilian institutional environment.

[3] A reform implemented in 2015 tightened the eligibility requirements for UI benefits. Minimum tenure requirements increased to 12 out of the previous 18 months for first-time applicants and to 9 out of the previous 12 months for second-time applicants. See Carvalho, Corbi and Narita (2018) and Doornik, Schoenherr and Skrastins (2023) for detailed analyses.

[4] Examples of job stability cases include pregnancy, maternity leave, and sick leave due to a workplace accident

2.5 Labor Inspections

Compliance with Brazilian labor legislation is enforced through inspections conducted by the Ministry of Labor. These inspections are carried out by civil servants known as labor inspectors–*Auditores Fiscais do Trabalho*–who visit establishments to verify whether firms comply with labor regulations, including the formal registration of workers, wage payments, working hours, contributions to the FGTS system, and the presence of fake layoff agreements (ALMEIDA; CARNEIRO, 2012a; CAYRES, 2015). When violations are detected, inspectors may issue notices of infraction, which can lead to monetary fines or require firms to regularize their labor practices.

Because enforcement resources are limited relative to the number of firms, inspections tend to focus on formal establishments, which are easier to identify and monitor (ALMEIDA; CARNEIRO, 2012a). The inspection system is geographically organized through regional labor offices, from which inspectors travel by car to establishments located in surrounding municipalities. As a result, logistical factors influence the probability that a firm is inspected.

This spatial organization of enforcement is particularly relevant in the context of fake layoff agreements between firms and workers. This practice involves the misreporting of separation conditions or payments and therefore exposes the parties to penalties if detected during an inspection (CAYRES, 2015). Because inspectors operate from regional offices and must travel to establishments, the expected probability of inspection partly depends on the distance between the firm and the nearest labor inspection outpost. In the empirical analysis, I exploit this geographic variation by using the distance between establishments and the nearest labor inspection office as a proxy for the likelihood that firms engaging in fake layoff agreements are detected.

3
Conceptual Framework

The goal of this chapter is to translate the institutional features of the Brazilian labor market into a simple framework that clarifies the incentives faced by firms and workers when deciding whether to separate. To do so, I adapt the model proposed by Carry and Schoefer (2024) to the Brazilian institutional environment. The model considers a static two-period setting with a single firm and a single worker matched under a contract with fixed wages.

In the first period, the firm–worker pair is in a productive match that generates positive surplus. At the beginning of the second period, an exogenous shock occurs that affects the value of the match. This shock may reflect changes in firm productivity or personal circumstances that alter the worker's valuation of employment. Following this shock, the firm and the worker decide whether to continue the match or separate.

I denote the worker's monthly wage by w and tenure at separation by T. Both of them are fixed and exogenous to the model, so each worker arrives at the separation decision with her individual values of w and T. When employed, the worker obtains job value $W(w)$, while the value of unemployment is denoted by U.

If the employment relationship ends through a layoff, the worker receives the balance accumulated in the severance savings account (FGTS), denoted by $F(w, T)$. In addition, the worker may be eligible for unemployment insurance benefits $UI(w, T)$, funded by the government. Laid-off workers are also entitled to a severance payment equal to 40% of the FGTS balance, represented by $0.4 \cdot F(w, T)$. Finally, workers may obtain additional compensation through potential labor court cases, which I denote by LC. In contrast, if the worker voluntarily quits the job, she does not gain access to the FGTS balance, severance payments, or UI benefits.

As for the firm, it obtains job value $J(w)$ while the match is active and vacancy value V if the match is terminated. When the firm initiates a separation, it must bear the dismissal costs described in Chapter 2. Translating these institutional rules into the model, firms face two severance-related payments tied to the worker's FGTS balance. First, a severance fine paid directly to the worker equal to $0.4 \cdot F(w, T)$. Second, the dismissal tax introduced in 2001, which corresponds to an additional payment to the government equal to $0.1 \cdot F(w, T)$. The analysis focuses on how this additional tax affects the separation decision after a shock.

In addition to these payments, firms may incur advance notice costs $AN(w)$, administrative costs associated with the homologation procedure $H(T)$, and potential litigation costs LC. In contrast, if the worker voluntarily quits, the firm is not required to pay any of these dismissal-related costs.

As previously discussed, the firm–worker pair may also choose to engage in a fake layoff agreement. This type of collusion can be mutually beneficial because it allows the worker to gain access to benefits associated with layoffs while enabling the firm to reduce dismissal costs. In this arrangement, the

worker formally receives the layoff-related benefits, obtaining $1.4 \cdot F(w,T)$ from the FGTS system and unemployment insurance $UI(w,T)$. However, the worker returns a share θ of the monetary dismissal costs–namely the advance notice payment $AN(w)$ and the severance-related costs $0.5 \cdot F(w,T)$–to the firm. Because the arrangement relies on illegal collusion between the parties, the worker does not initiate litigation, implying $LC = 0$.

From the firm's view, this agreement reduces the effective dismissal costs by a fraction θ and eliminates potential court-related expenses. However, if the arrangement is detected by the authorities, both parties face penalties. I denote these risk-adjusted fines by ξ^{worker} for the worker and ξ^{firm} for the firm.

Payoffs for each possible outcome are summarized in Table 3.1. An important feature of the framework is that firms and workers have different sets of available actions after the shock occurs.

The firm has three choices: continue the match C, lay off the worker L, or engage in a fake layoff agreement FL. The options C and FL require the worker's consent, while layoffs can be implemented unilaterally by the firm. Similarly, the worker can choose between continuing the match C, quitting the job Q, or participating in a fake layoff agreement FL. The options C and FL require the firm's agreement, whereas quitting can be chosen unilaterally by the worker.

The timing of the model is as follows. After the realization of the shock at the beginning of the second period, both parties evaluate the value of the match relative to their outside options and decide whether to continue the employment relationship, separate unilaterally, or engage in a collusive separation.

The most relevant case for the model is the scenario with low enforcement, in which the expected penalties from detection are small, implying low values of ξ^{firm} and ξ^{worker}.[1] In this environment, engaging in a fake layoff agreement remains profitable for both parties despite the risk of detection.

This situation is more likely to characterize firms further away from a labor inspection office, which tend to face less monitoring and may incur lower penalties when violations are detected. By contrast, the high enforcement case–where expected penalties are sufficiently large to deter collusion–is described in Appendix A and is more representative of firms that are easier to monitor. In Chapter 5, I explore this variation in the distance between municipalities and the nearest labor inspection office.

Preferences in the model are determined by comparing the payoffs associated with each possible action. For instance, $C \succ L$ if, and only if, the firm's payoff from continuing the match is greater than the payoff from laying off the worker. In the low-enforcement scenario, fake layoff agreements are attractive to both parties. As a result, the firm strictly prefers a fake layoff to a unilateral layoff, while the worker strictly prefers a fake layoff to quitting. Given these preferences, the possible outcomes are summarized in Table 3.2.

Figure 3.1 illustrates the agents' separation decisions following a shock. Panel (a) shows the decisions before the introduction of the dismissal tax, while Panel (b) presents the decisions after the tax.

[1] Algebraically, this requires $\theta \cdot [AN(w) + 0.4 \cdot F(w,T)] + LC > \xi^{firm}$ and $(1-\theta) \cdot [AN(w) + 0.4 \cdot F(w,T)] + F(w,T) + UI(w,T) > \xi^{worker}$.

Table 3.1: Payoffs in the Model

Before tax	Firm	Worker
Continue (C)	$J(w)$	$W(w)$
Separate		
Quit (Q)	V	U
Layoff (L)	$V - AN(w) - 0.4 \cdot F(w,T) - H(T) - LC$	$U + AN(w) + 1.4 \cdot F(w,T) + UI(w,T) + LC$
Fake layoff (FL)	$V - (1-\theta)[AN(w) + 0.4 \cdot F(w,T)] - H(T) - \xi^{firm}$	$U + (1-\theta) \cdot [AN(w) + 0.4 \cdot F(w,T)] + F(w,T) + UI(w,T) - \xi^{worker}$
After tax	**Firm**	**Worker**
Continue (C)	$J(w)$	$W(w)$
Separate		
Quit (Q)	V	U
Layoff (L)	$V - AN(w) - \underline{\mathbf{0.5}} \cdot F(w,T) - H(T) - LC$	$U + AN(w) + 1.4 \cdot F(w,T) + UI(w,T) + LC$
Fake layoff (FL)	$V - (1-\theta)[AN(w) + \underline{\mathbf{0.5}} \cdot F(w,T)] - H(T) - \xi^{firm}$	$U + (1-\theta) \cdot [AN(w) + 0.4 \cdot F(w,T)] - \underline{\theta \cdot \mathbf{0.1} \cdot \mathbf{F}(\mathbf{w}, \mathbf{T})} + F(w,T) + UI(w,T) - \xi^{worker}$

Notes: Firm and worker payoffs under different outcomes before and after the dismissal tax. Changes are underlined and highlighted in bold.

Table 3.2: Potential Outcomes of the Model Based on Preferences

	Firm		
Worker	$C \succ FL \succ L$	$FL \succ C \succ L$	$FL \succ L \succ C$
$C \succ FL \succ Q$	Continue (C)	Continue (C)	Layoff (L)
$FL \succ C \succ Q$	Continue (C)	Fake layoff (FL)	Fake layoff (FL)
$FL \succ Q \succ C$	Quit (Q)	Fake layoff (FL)	Fake layoff (FL)

Notes: The table summarizes equilibrium outcomes as a function of firm and worker preference rankings. Under low enforcement, fake layoffs dominate unilateral separations. Collusion arises only when both parties prefer a fake layoff to continuation; otherwise, the outcome is continuation or the unilateral separation preferred by the party that exits.

In the figure, the x-axis represents the value of the match to the worker, $W(w)$, while the cutoffs correspond to the worker's outside options: first quitting, then a fake layoff. Similarly, the y-axis represents the value of the match to the firm, $J(w)$, with the cutoffs corresponding to the firm's outside options: first a layoff, then a fake layoff. The dashed regions display the potential outcomes described in Table 3.2. Within each region, the firm's preferences are shown in red, the worker's preferences in blue, and the resulting outcome is indicated in bold in black. Panel (a) shows the equilibrium regions before the introduction of the 10% dismissal tax, while Panel (b) shows the regions after the tax. The shift in the cutoffs induced by the tax is highlighted in bold and underlined along both axes.

The arrows illustrate how the equilibrium regions change after the tax, leading to different separation outcomes. First, the yellow area predicts an increase in quits. This occurs because the value of a fake layoff decreases for the firm, making it less willing to collude in such arrangements. Workers who still prefer to separate but cannot obtain the firm's agreement to a fake layoff must instead quit.

Second, the orange area predicts a decrease in layoffs. After the introduction of the dismissal tax, the firm's payoff from dismissing a worker declines, reducing its incentive to initiate layoffs.

Third, the green area predicts a decrease in fake layoffs. Because the tax reduces the payoff from fake layoffs for both firm and worker, some matches that would previously have ended in collusive separations now continue.

Finally, the red area predicts a substitution from fake layoffs to layoffs. In this region, workers prefer to continue the match instead of colluding in a fake layoff agreement. If the firm still prefers to separate rather than continue, they will be forced to layoff the worker instead. As a result, separations that would previously have occurred through collusion may instead occur as formal layoffs.

It is important to note that fake layoff agreements are not directly observable in the administrative data, as they are recorded as layoffs. Consequently, the observed layoff outcomes in Chapter 5 may reflect a combination of true layoffs and collusive separations. In particular, the changes predicted by the model may arise both from the decrease in layoffs highlighted in the orange area and from the reduction in fake layoffs highlighted in the green area.

In addition to changes in the layoff tax, other parameters may influence the separation decision. In particular, the distribution of bargaining power in fake layoff agreements affects the incentives of both parties. Recall that θ denotes the share of dismissal-related payments returned by the worker to the firm. When analyzing the workers and firms payoffs–Table 3.1–the tax increases the layoff cost by $0.1 \cdot F(w,T)$. In the case of a fake layoff agreement, this cost is split, $(1-\theta) \cdot 0.1 \cdot F(w,T)$ to the firm and $\theta \cdot 0.1 \cdot F(w,T)$ to the worker. Therefore, who bears the loss depends on θ.

When the firm has greater bargaining power, $\theta \to 1$, the worker bears most of the tax increase in a fake layoff. Hence, fake layoffs become more attractive to the firm and less attractive to the worker. In the low-enforcement environment considered here, the model predicts fewer quits, more layoffs, and an ambiguous effect on fake layoffs.

The reduction in quits arises because the worker who wishes to separate may now reach a collusive agreement with the firm more often. Since fake layoffs remain more attractive than quitting, the worker accepts the arrangement even when the share retained from the agreement is smaller. In Figure 5.1, this effect appears as a reduction in the yellow area.

The increase in layoffs occurs because the worker who prefers to remain employed may reject unfavorable collusive arrangements. If the firm still prefers to terminate the match, the separation then occurs through a unilateral layoff. In Figure 5.1, this substitution from fake layoffs to layoffs is captured by the expansion of the red area.

The overall effect on fake layoffs is therefore ambiguous. On the one hand, when the worker prefers a fake layoff to continuing the match, the firm is more willing to engage in collusion, which increases the likelihood that the match ends through a fake layoff. In Figure 5.1, this is reflected in the reduction of the horizontal green area. On the other hand, when the worker prefers to continue the match, unfavorable collusive terms may lead the worker to reject the agreement. If the firm also prefers to continue the match rather than dismiss the worker unilaterally, the employment relationship continues instead, reducing fake layoffs. In Figure 5.1, this mechanism corresponds to the expansion of the vertical green area.

Overall, the intuition here is that the layoff tax bites fake layoffs weakly on the firm side. Conversely, when workers have greater bargaining power ($\theta \to 0$), the model predicts the opposite patterns.

Finally, note that $F(w,T)$, $UI(w,T)$, and $H(T)$ increase with tenure. As a result, layoffs become more costly for firms as worker tenure increases. The model therefore predicts that the effect of the dismissal tax on layoffs should be higher for more tenured workers. In contrast, the effect on quits is expected to be stronger among workers with lower tenure. These workers have weaker incentives to engage in fake layoff agreements because their accumulated FGTS balances are smaller and they may qualify for fewer unemployment insurance installments. These tenure-related effects should be particularly pronounced around the 12-month tenure threshold, at which point the administrative cost $H(T)$ becomes binding. Moreover, this threshold may also increase the likelihood of transitioning to the high-enforcement scenario discussed earlier.

Taken together, the conceptual framework highlights how dismissal costs,

bargaining power, and worker tenure shape equilibrium separation outcomes. The model therefore generates several empirical predictions that can be tested in the data.

Prediction 1 (Layoffs). The introduction of the dismissal tax reduces the number of layoffs initiated by firms, as the tax increases the monetary cost of dismissing workers and therefore lowers the firm's incentive to terminate matches unilaterally.

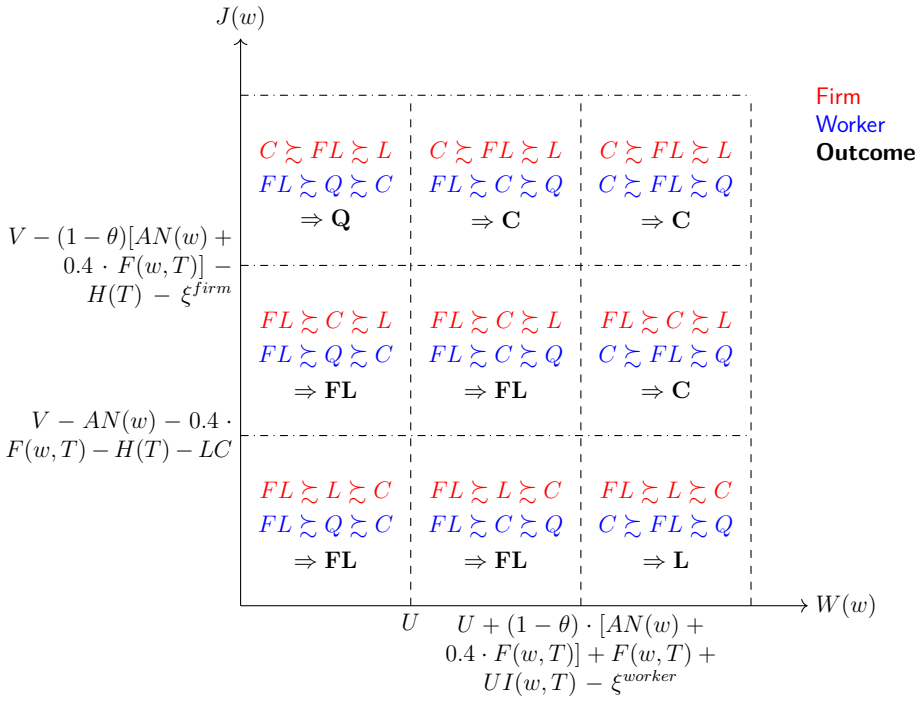
Prediction 2 (Quits). The introduction of the dismissal tax increases the frequency of quits. As fake layoff agreements become less attractive to firms, some workers who would previously separate through collusion may instead quit when the firm refuses to participate in such arrangements.

Prediction 3 (Observed layoffs and collusion). Fake layoff agreements are not directly observable in administrative data, as they are recorded as layoffs. Consequently, observed layoffs reflect a combination of true dismissals and collusive separations. The model therefore predicts that decreases in the incidence of fake layoffs may appear in the data as decreases in observed layoffs, particularly in environments where collusion is more feasible. Additionally, there are other indirect indicators of collusion that may be tested, such as recalls.

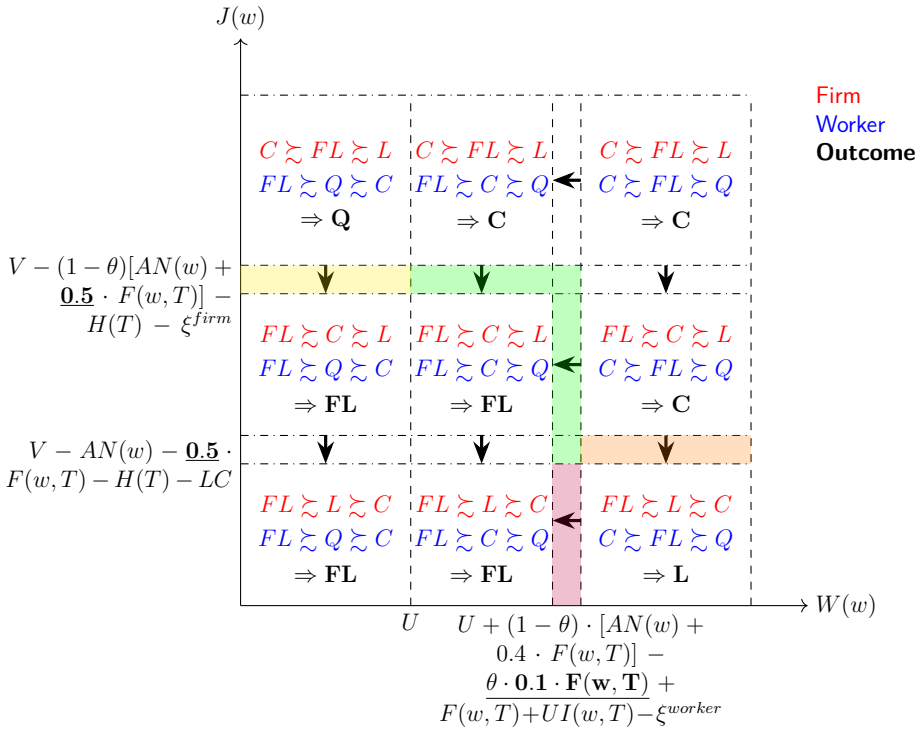
Prediction 4 (Tenure heterogeneity). The effect of the dismissal tax on layoffs should be higher for more tenured workers, as severance-related costs increase with tenure. Conversely, the effect on quits should be higher among workers with lower tenure, who have weaker incentives to engage in fake layoff agreements due to smaller FGTS balances and potentially fewer UI installments.

The empirical analysis in the following chapters tests these predictions using administrative data on worker separations from RAIS, combined with information on labor market informality and variation in the intensity of labor inspections across municipalities.

Figure 3.1: Potential Outcomes of the Model Before and After the Tax



(a) Before the 10% tax



(b) After the 10% tax

Notes: The figure shows the model's equilibrium outcomes. The x-axis denotes the worker's match value $W(w)$ and the y-axis the firm's match value $J(w)$, with cutoffs given by each side's outside options. Dashed regions correspond to preference profiles (Table 3.2). Within each region, firm preferences are in red, worker preferences in blue, and the resulting outcome is in bold. Panel (a) shows the pre-tax equilibrium; Panel (b) shows the post-tax equilibrium. Arrows indicate shifts in thresholds after the tax. The shaded areas highlight predicted changes: increases in quits (yellow), decreases in layoffs (orange), decreases in fake layoffs (green), and substitution from fake layoffs to layoffs (red).

4
Data

This chapter describes the datasets used in the empirical analysis. The main source of information is the Brazilian matched employer-employee administrative dataset, RAIS. I complement this data with information on municipal labor market informality derived from the 2000 Population Census and with data on labor inspections.

4.1
RAIS

I use administrative data from the "*Relação Anual de Informações Sociais*" (RAIS), a matched employer-employee dataset that covers the universe of formal employment relationships in Brazil. Every year, all registered firms—both public and private-—are legally required to report information on their workforce to the Ministry of Labor, including all active, newly created, and terminated formal employment contracts during the previous year. The dataset contains detailed worker and firm characteristics, such as age, gender, schooling, tenure, wages, occupation, hiring and separation dates, contract type, establishment size, location, and economic activity, among many other variables.

Although an anonymized version of RAIS is publicly available, this study relies on an encrypted version of the identified dataset in order to construct a longitudinal panel of workers and establishments. Access to the encrypted data is restricted and was obtained through the secure offline server maintained by PUC-Rio. All analyses were conducted in compliance with the confidentiality requirements governing the use of administrative microdata.

I construct a quarterly panel spanning from the first quarter of 1999 to the fourth quarter of 2003. The analysis focuses on private-sector establishments that were already operating prior to the introduction of the dismissal tax in the third quarter of 2001. Public-sector employment relationships are excluded because they are governed by a different legal regime and are therefore not subject to the CLT labor code, FGTS contributions, or the dismissal tax introduced in 2001. I further restrict the sample to open-ended employment contracts, which represent the dominant form of formal employment and are directly affected by the dismissal costs discussed in the previous chapter.

To avoid contamination from the retroactive correction of FGTS balances related to workers hired before the 1989-1990 stabilization plans, I restrict the sample to workers hired 1991 onward. Finally, due to computational constraints, I draw a 20% random sample of establishments from the full dataset. The resulting panel contains approximately 7.7 million worker-quarter observations across 457,509 establishments.

4.2 Informality

To capture variation in labor market informality across locations, I combine the RAIS data with information from the 2000 Brazilian Population Census conducted by the *Instituto Brasileiro de Geografia e Estatística* (IBGE). The census collects detailed information on individuals' employment status and on whether their job is performed under a formal labor contract. Using these responses, I compute the share of workers employed informally in each municipality.

Informality includes domestic workers without a formal labor contract, employees without a formal labor contract, self-employed workers who do not contribute to social security, unpaid family workers, and workers engaged in production for own consumption. The informality rate is calculated as the sum of these categories divided by total municipal employment, excluding employers.

This measure captures differences in the prevalence of informal labor markets across regions of the country. Municipalities with higher levels of informality are linked with higher indirect levels of strategic layoff behavior (DOORNIK; SCHOENHERR; SKRASTINS, 2023). Thus, it is a relevant heterogeneity analysis for the study of fake layoff agreements in this case.

4.3 Labor Inspection

I complement the analysis with information on the enforcement of labor regulations using administrative data on labor inspections compiled by the Brazilian Ministry of Labor and assembled by Almeida and Carneiro (2012a).[1] This data provides information on the geographical distribution of labor inspection offices and their distance to Brazilian municipalities as of 2002.

I merge the RAIS panel with geographic information on travel time–measured in hours by car–between municipalities and the nearest labor inspection office.[2] This measure captures variation in the ease with which inspectors can access establishments and is used as a proxy for enforcement intensity. In particular, municipalities that are more distant from inspection offices are expected to face lower monitoring intensity and, consequently, a lower probability that illegal practices–such as collusive fake layoff agreements–are detected.

The enforcement measure is subject to some limitations. First, for municipalities created shortly before the data were compiled, information is not always directly available. In these cases, Almeida and Carneiro (2012a) assign travel times based on the nearest neighboring municipality. Second, the measure relies on road travel, which may not accurately reflect actual transportation conditions in certain regions. In the state of Amazonas, river transport is the primary mode of mobility, rendering road-based distances largely uninformative. Therefore, municipalities in this state are excluded from the analysis. A similar issue arises in parts of the state of Pará, where road-based distances can be substantially overstated relative to river routes, leading

[1] For details on the construction of this dataset, see Almeida and Carneiro (2012a).
[2] This dataset is available at Almeida and Carneiro (2012b).

to implausibly large travel times. These municipalities are also excluded. Finally, due to its administrative structure and small geographic size, the Distrito Federal is merged with the state of Goiás.

5
Firm Level Analysis

The empirical strategy exploits the introduction of the dismissal tax in September 2001. Prior to the reform, no such tax existed. After the reform, a 10% tax on the worker's FGTS balance applied to all dismissals without cause in the private sector. Because the reform affected all firms simultaneously, there is no cross-sectional variation in treatment timing. However, the magnitude of the tax depends on the FGTS balance of dismissed workers, which varies across firms.

I exploit this variation in treatment exposure to identify the effects of the dismissal tax on firms' separation decisions. My measure of exposure to the reform is the firms' pre-reform tax liabilities. Intuitively, this variable captures how much each firm would have paid in dismissal taxes, as a share of its payroll, had the tax been in place prior to its introduction.

To construct this measure, I proceed in three steps. First, I calculate the statutory FGTS balances for all workers i^L laid off by firm f before the third quarter of 2001. Although the dataset does not contain the exact FGTS balances, they can be approximated using the statutory accumulation rules. These statutory balances should closely track the actual balances held in the accounts.[1]

Second, I apply the 10% dismissal tax to these balances to compute the hypothetical tax payments associated with each layoff. Third, I normalize the resulting amount by the firm's total payroll over the same period, defined as the sum of wages paid to all workers i employed by firm f.

Formally, the exposure measure is defined as:

$$\overline{Tax}_f = \frac{\sum_{t=Q1.1999}^{Q3.2001} \sum_{i^L \in f} 0.1 \cdot F(w,T)_{i^L,f,t}}{\sum_{t=Q1.1999}^{Q3.2001} \sum_{i \in f} w_{i,f,t}} \tag{5-1}$$

The distribution of the exposure measure is relatively skewed across firms. The mean of $\overline{Tax}_f$ is 0.55%, while the median is 0.21%. The 25th percentile is 0%, and the 75th percentile is 0.66%. The lower tail reflects the presence of establishments with no observed layoffs during the pre-reform period. In particular, a non-negligible share of establishments consists of a single worker who is also the owner, implying that no separations occur within the sample period.

I do not exclude these establishments from the analysis because they remain subject to the dismissal tax. If such firms hire additional workers and subsequently dismiss them, they would face the tax in the same way as other firms. For robustness, Appendix B displays the results when using only establishments with at least 2 workers.

[1] See Gerard and Naritomi (2021) for a discussion of this approximation. Using survey evidence from unemployment insurance applicants in São Paulo, the authors show that statutory FGTS balances constructed from RAIS data closely match the balances reported by workers. I follow the same approach to construct the balances in this paper.

Table 5.1: Descriptive Statistics by Exposure: Firm-Level Analysis

	Below median $\overline{Tax}_f$	Above median $\overline{Tax}_f$
$\overline{Tax}_f$ (%)	0.02	1.04
	(0.04)	(1.27)
Number of active contracts	5.89	8.21
	(44.63)	(42.88)
Number of hires	0.89	1.30
	(8.73)	(8.02)
Number of separations	0.53	1.15
	(5.87)	(7.20)
Number of layoffs	0.22	0.75
	(2.51)	(4.33)
Number of quits	0.12	0.17
	(1.04)	(1.21)
Number of recalls	0.01	0.03
	(0.42)	(0.41)
Weekly hours	42.54	43.01
	(4.63)	(3.52)
Tenure (months)	22.11	13.40
	(16.02)	(12.19)
Age (years)	31.78	30.98
	(8.79)	(7.21)
Share male (%)	57.17	63.29
	(41.62)	(37.71)
Share college educated (%)	4.38	3.58
	(15.32)	(11.62)
Observations	2,916,813	4,777,786
Share of observations (%)	37.91	62.09
Number of firms	176,681	280,828
Share of firms (%)	38.62	61.38

Notes: The table reports quarterly baseline means and standard deviations for establishments below and above the median of the exposure measure $\overline{Tax}_f$. The variable $\overline{Tax}_f$ captures the firm's pre-reform exposure to the dismissal tax, defined as hypothetical tax payments relative to payroll prior to the reform. Standard deviations are reported in parentheses. All variables are measured at the establishment-quarter level using pre-reform data (Q1.1999–Q3.2001).

Table 5.1 presents descriptive statistics by exposure to the dismissal tax. Firms above the median exposure tend to be larger and exhibit higher levels of labor market activity. In particular, these establishments have more active contracts on average and display higher rates of both hires and separations. Layoffs represent the main mode of separation in the sample, substantially exceeding quits across both groups.

The table also shows that more exposed firms tend to employ slightly younger workers and a higher share of male workers, while differences in hours worked are small across groups. Workers in more exposed firms also exhibit shorter tenure, suggesting that these establishments operate in higher-turnover labor markets.

I estimate the effects of the dismissal tax using the following event-study

specification:

$$Y_{f,s,m,t} = \alpha_f + \eta_s + \nu_m + \zeta_t + \sum_{\tau=Q1.1999,\ \tau\neq Q3.2001}^{Q4.2003} \beta_\tau \left(\overline{Tax}_f \cdot \mathbb{I}[t=\tau]\right) + \epsilon_{f,s,m,t} \tag{5-2}$$

where $Y_{f,s,m,t}$ denotes the outcome of establishment f, in sector s, municipality m, and quarter t. $Y_{f,s,m,t}$ can be the number of layoffs by firm f in quarter t, the number of workers that quit in firm f in quarter t, or the number of workers that were recalled by firm f in quarter t. The specification includes establishment fixed effects α_f, sector fixed effects η_s, municipality fixed effects ν_m, and time fixed effects ζ_t. The coefficients of interest, β_τ, trace the dynamic relationship between firm exposure to the dismissal tax, $\overline{Tax}_f$, and the outcome variable over time. The quarter immediately preceding the reform (Q3.2001) is omitted and serves as the reference period.

To account for differential pre-trends across firms with different levels of exposure, the outcomes are linearly detrended using baseline coefficients following Dustmann et al. (2022). This approach allows firms with higher and lower exposure to exhibit different linear trends prior to the reform. The identifying assumption is that any differences in outcome trends between these groups would have continued along the same linear path in the absence of the dismissal tax.

Under this assumption, the estimated coefficients capture trend breaks between firms with higher and lower exposure after the introduction of the tax. The magnitude of the estimated effects depends on the comparison between firms at different points of the exposure distribution. In the main results, I report effects comparing firms at the 75th and 25th percentiles of the exposure distribution.

I estimate Equation 5-2 separately for layoffs, quits, and recalls, allowing for heterogeneity by tenure around the 12-month threshold. Figures 5.1–5.3 present the event-study estimates for each outcome. Appendix C replicates the same exercise using the 3-month tenure threshold, which corresponds to the threshold from which dismissal costs become active.

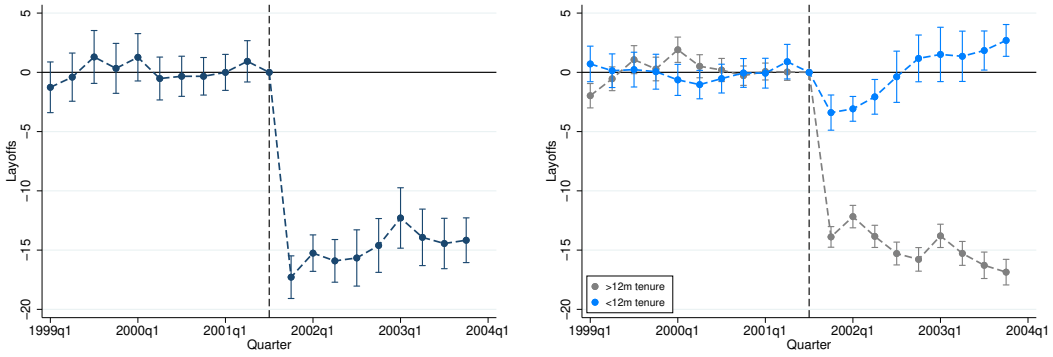
Figure 5.1 displays the results for layoffs. Panel (a) shows a clear break in the trend of quarterly layoffs after the introduction of the dismissal tax in Q3.2001. Prior to the reform, the difference in trends between firms with higher and lower exposure appears relatively stable, supporting the identifying assumption that pre-existing differences follow a common linear pattern. After the tax is introduced, layoffs decline more strongly among firms with higher exposure to the reform.

Panel (b) further decomposes this effect by worker tenure at separation. The reduction in layoffs is concentrated among workers with more than 12 months of tenure. This pattern is consistent with the conceptual framework presented in Chapter 3. More tenured workers accumulate larger FGTS balances, implying that the monetary value of the dismissal tax is higher for these separations.

For workers with less than 12 months of tenure, the reduction in layoffs is short-lived and quickly returns to pre-reform levels. According to the model in

Chapter 3, the cost of laying off these workers remains relatively inexpensive due to their lower accumulated FGTS balances. Therefore, it is still cheap to lay them off. In addition, separations below the 12-month threshold do not require the homologation procedure, meaning that a representative of the worker's union or the Ministry of Labor does not need to verify that dismissal payments were properly executed. From the model, there is also a weaker enforcement of dismissal costs for these shorter-tenure workers, which could mean that the fake layoffs are still common for this group.

Figure 5.1: Event-Study Results: Layoff Effects of the Dismissal Tax



(a) Full Sample (b) Tenure Heterogeneity

Notes: The graph displays the effect over the total number of layoffs. Panel (a) uses the full sample described in Chapter 4. Panel (b) distinguishes between layoffs of workers with more than 12 months of tenure at separation (gray line) and less than 12 months of tenure (blue line). Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company–i.e., common ownership.

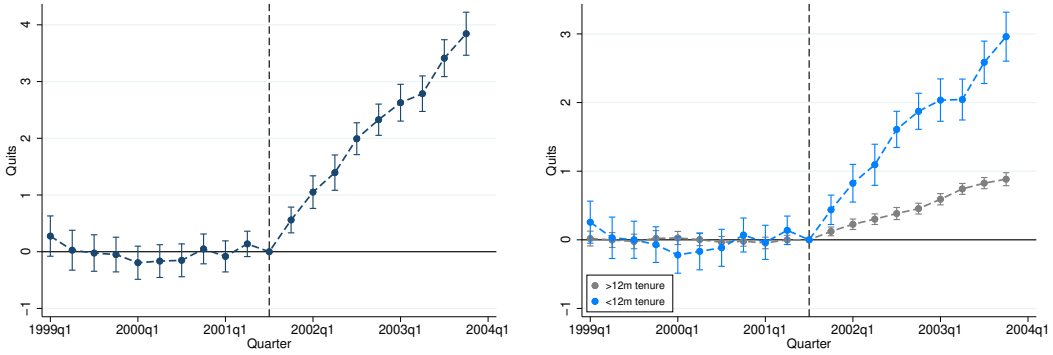
Turning to quits, Figure 5.2 shows a clear increase in voluntary separations after the introduction of the dismissal tax. Prior to the reform, the difference in quit trends between firms with higher and lower exposure appears relatively stable, supporting the identifying assumption that pre-existing differences follow a common linear pattern. After the tax is introduced, quits increase more strongly in firms with greater exposure to the reform.

This pattern is consistent with the conceptual framework developed in Chapter 3. As the dismissal tax reduces the payoff from collusive fake layoff agreements, some separations that would previously have occurred through collusion instead occur through voluntary quits. The gradual increase in the difference between high- and low-exposure firms may reflect an adjustment process as firms and workers learn about the new incentives created by the tax.

Panel (b) shows that this effect is concentrated among workers with less than 12 months of tenure at separation. This pattern is also consistent with the conceptual framework. Workers with shorter tenure have smaller accumulated FGTS balances and therefore weaker incentives to engage in fake layoff agreements. Once the dismissal tax is introduced, the gains from collusion decline further, making voluntary quits relatively more likely for these workers. For workers with longer tenure, the potential rents from collusion remain larger

due to their higher FGTS balances, which dampens the increase in quits for this group.

Figure 5.2: Event-Study Results: Quit Effects of the Dismissal Tax



(a) Full Sample (b) Tenure Heterogeneity

Notes: The graph displays the effect over the total number of quits. Panel (a) uses the full sample described in Chapter 4. Panel (b) distinguishes between workers who quit with more than 12 months of tenure at separation (gray line) and less than 12 months of tenure (blue line). Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company–i.e., common ownership.

Lastly, I examine recalls, defined as cases in which a worker is laid off by a firm and later rehired by the same firm. In Brazil, workers can be formally rehired by the same employer after a three-month interval following a layoff, although in practice some recalls occur even earlier. There is evidence for recalls as an indirect indicator of fake layoff agreements (DOORNIK; SCHOENHERR; SKRASTINS, 2023). In such arrangements, the worker may separate formally in order to access FGTS withdrawals and UI benefits and subsequently return to the same firm once these benefits are exhausted. In some cases, workers may even remain informally employed at the firm while receiving these benefits.

Figure 5.3 presents the event-study estimates for recalls. Panel (a) shows a clear break in the trend of quarterly recalls following the introduction of the dismissal tax. Prior to the reform, the difference in recall trends between firms with higher and lower exposure appears relatively stable. After the tax is introduced, firms with greater exposure recall fewer workers.

Similar to the quit results, the response of recalls appears gradual. During the first two quarters after the reform, recall behavior changes little. One possible explanation is that some recalls during this period correspond to workers who had been laid off prior to the introduction of the tax, meaning that firms had not yet faced the higher dismissal costs.

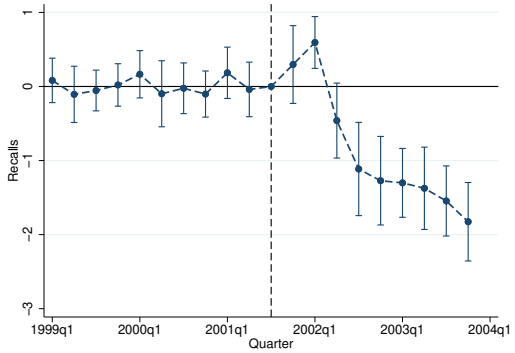
In addition, the increase observed in the first quarter of 2002 may reflect workers who were laid off shortly before the tax was introduced and recalled approximately four months later, once their UI benefits expired. This pattern is consistent with fake layoff agreements, in which workers temporarily separate in order to access benefits and subsequently return to the same firm. The temporary spike in recalls may therefore represent one final round of

rent extraction before the higher dismissal costs make such arrangements less attractive.

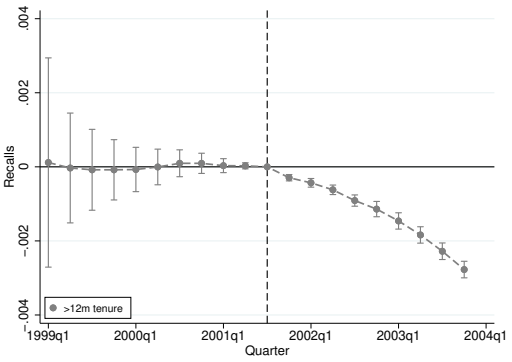
As the tax becomes binding for new separations, recall rates decline among firms with higher exposure to the reform. This pattern is consistent with firms and workers adjusting their behavior in response to the reduced payoff from collusive separations.

Panels (b) and (c) further show that the reduction in recalls is concentrated among workers with less than 12 months of tenure at separation. This pattern is consistent with the conceptual framework. Workers with shorter tenure have smaller FGTS balances and therefore lower potential gains from fake layoff agreements. When the dismissal tax is introduced, the already limited gains from collusion decline further, reducing the likelihood that such arrangements occur. For workers with tenure above the 12-month threshold, the recall trend also begins to decline after the reform, but the magnitude of the effect remains small.

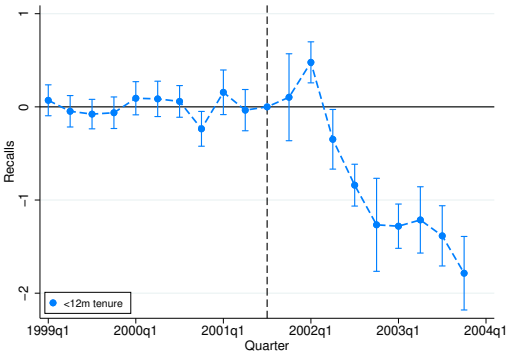
Figure 5.3: Event-Study Results: Recall Effects of the Dismissal Tax



(a) Full Sample



(b) Above 12 Months Tenure at Layoff



(c) Below 12 Months Tenure at Layoff

Notes: The graph displays the effect over the total number of recalls. Panel (a) uses the full sample described in Chapter 4. Panel (b) restricts the sample to workers who were separated with more than 12 months of tenure and were later recalled by the same establishment. Panel (c) restricts the sample to workers who were separated with less than 12 months of tenure and were later recalled by the same establishment. Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company–i.e., common ownership.

Taken together, the decline in layoffs and recalls, combined with the increase in quits, provides evidence consistent with a reduction in collusive separations following the introduction of the dismissal tax.

In addition to the event-study specification in Equation 5-2, I estimate a difference-in-differences (DiD) specification based on the same identifying variation. While the event-study estimates trace the dynamic response to the reform, the DiD specification provides a convenient summary measure of the average effect of the dismissal tax on firm separation behavior. Equation 5-3 presents the baseline specification:

$$Y_{f,s,m,t} = \alpha_f + \eta_s + \nu_m + \zeta_t + \beta_{post} \cdot \overline{Tax}_f \cdot Post_t + \delta \cdot \overline{Tax}_f \cdot Trend_t + \epsilon_{f,s,m,t} \tag{5-3}$$

$Y_{f,s,m,t}$ still represents the same three variables as before for firm f, in sector s, municipality m, at quarter t. The fixed effects follow the same definitions as in Equation 5-2. In this specification, the coefficient β_{post} captures the continuous DiD estimate of the effect of the dismissal tax on firm outcomes. The variable $\overline{Tax}_f$ corresponds to the firm's pre-reform exposure to the dismissal tax, defined as in the previous section, and $Post_t$ is an indicator equal to one for quarters after the reform-$t \in [Q4.2001, Q4.2003]$.

The specification also includes exposure-specific linear trends, $\overline{Tax}_f \cdot Trend_t$, to account for potential differences in pre-reform trends between firms with high and low exposure to the tax (DUSTMANN et al., 2022). Under this specification, β_{post} captures the differential change in outcomes after the reform for firms with higher exposure to the dismissal tax. The resulting estimates are reported in Table 5.2, together with additional specifications that assess the robustness of the results.

Table 5.2 reports the difference-in-differences estimates of the effect of firms' exposure to the dismissal tax on separation outcomes. Each column presents a different specification of Equation 5-3. Column (1) reports the same specification as 5-3. Column (2) drops municipal and sector fixed effects (FE). Column (3) does not use any trend interaction. Column (4) interacts the time trend with firms' baseline workforce composition, measured by average worker age, the share of college-educated workers, and the share of male workers. Column (5) controls directly for these baseline firm characteristics, without the trend component. Across specifications, the estimates remain stable, except when I exclude the trend component from the analysis. This suggests that firms with different levels of exposure would have followed similar trajectories in the absence of the reform, conditional on these controls.

This concern is central in settings where treatment intensity may be correlated with baseline characteristics, as differences in underlying trends may otherwise confound the estimates. By allowing for exposure-specific trends, the specification mitigates these concerns and isolates the variation in outcomes driven by the policy change. Consistent with this interpretation, the estimated effects emerge precisely after the introduction of the dismissal tax and are not present in specifications that fail to account for differential trends, reinforcing a causal interpretation of the results.

To provide a sense of the economic magnitude of the effects shown in the

Table 5.2: Difference-in-Differences Results: Effects of the Dismissal Tax

	(1)	(2)	(3)	(4)	(5)
Panel A: Layoffs					
Total Layoffs	-17.378***	-17.431***	-16.537***	-16.313***	-17.097***
	(0.939)	(0.941)	(0.520)	(0.519)	(0.473)
Layoffs >12m	-13.839***	-13.853***	-11.503***	-11.965***	-12.218***
	(0.796)	(0.411)	(0.224)	(0.228)	(0.206)
Layoffs <12m	-3.539***	-3.577***	-4.776***	-5.035***	-4.879***
	(0.345)	(0.776)	(0.428)	(0.422)	(0.390)
Panel B: Quits					
Total Quits	1.217***	1.216***	-1.065***	0.307***	-1.026***
	(0.246)	(0.246)	(0.136)	(0.137)	(0.124)
Quits >12m	0.246***	0.251***	-0.421***	0.132***	-0.419***
	(0.079)	(0.079)	(0.044)	(0.043)	(0.039)
Quits <12m	0.9717***	0.965***	-0.643***	0.175	-0.607***
	(0.177)	(0.221)	(0.122)	(0.123)	(0.112)
Panel C: Recalls					
Total Recalls	0.126	0.133	0.255***	0.086	0.216***
	(0.131)	(0.132)	(0.073)	(0.066)	(0.066)
Recalls >12m	0.001	0.001	-0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Recalls <12m	0.014	0.015	0.293***	0.108***	0.283***
	(0.035)	(0.035)	(0.019)	(0.031)	(0.017)
Firm FE (α_f)	yes	yes	yes	no	no
Sector FE (η_s)	yes	no	yes	yes	yes
Municipality FE (ν_m)	yes	no	yes	yes	yes
Year-month FE (ζ_t)	yes	yes	yes	yes	yes
$\overline{Tax}_f \cdot Trend_t$	yes	yes	no	no	no
Baseline means $\cdot\, Trend_t$	no	no	no	yes	no
Baseline means	no	no	no	no	yes

Notes: This table reports difference-in-differences estimates of the effect of the 2001 dismissal tax on firm-level outcomes. The coefficient shown corresponds to β_{post} from Equation 5-3, capturing the differential post-reform change for firms with higher pre-reform exposure to the tax. Baseline means are the pre-reform average age of workers in firm f, the pre-reform average share of male workers in firm f, and the pre-reform average share of college-educated workers in firm f. They are included in the specification as alternatives to the $\overline{Tax}_f \cdot Trend_t$ component, with and without the $Trend_t$ interaction term. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

event-study graphs–Figures 5.1 to 5.3–, I compare firms at the 75th and 25th percentiles of the exposure distribution. The difference in exposure between these firms is 0.66 percentage points. Using the preferred specification, the implied effect on quarterly layoffs is $(0.66\% - 0.0\%) \times (-17.378) = -0.115$. Relative to the baseline mean of layoffs among firms at the 75th percentile– 0.579 layoffs per quarter–, this corresponds to a 19.8% reduction in layoffs.

Consistent with the event-study evidence, this effect is concentrated among workers with more than 12 months of tenure. For these workers, the higher accumulated FGTS balances increase the tax base, making dismissals more costly for firms. In contrast, the effect for workers with less than 12 months of tenure is smaller. This pattern is consistent with the institutional role of the homologation requirement, which only applies to dismissals of

workers with tenure above one year.

Turning to quits, the estimates indicate a modest increase in voluntary separations among more exposed firms. Comparing firms at the 75th and 25th percentiles of the exposure distribution implies an increase of approximately 7.9% in quarterly quits. This pattern is once again consistent with the conceptual framework developed in Chapter 3, which predicts that when the dismissal tax reduces the gains from collusive layoffs, some separations that would previously have occurred through collusion instead occur through voluntary quits.

Finally, recall rates do not appear statistically significant when examining the aggregate post-reform effect among more exposed firms. This likely reflects the dynamic pattern in which recalls initially rise before subsequently declining, offsetting the overall effect. Nonetheless, recall behavior is illustrated in Figure 5.3. Taken together, the observed decline in both layoffs and recalls, alongside an increase in quits, suggests that the dismissal tax reduced not only genuine layoffs but also fake layoff agreements between firms and workers.

5.1 Informality

This subsection provides an important heterogeneity analysis that further sheds light on the prevalence of fake layoff agreements. Doornik, Schoenherr and Skrastins (2023) document that informal labor markets can facilitate strategic formal layoffs, as workers may receive UI benefits while continuing to work informally.

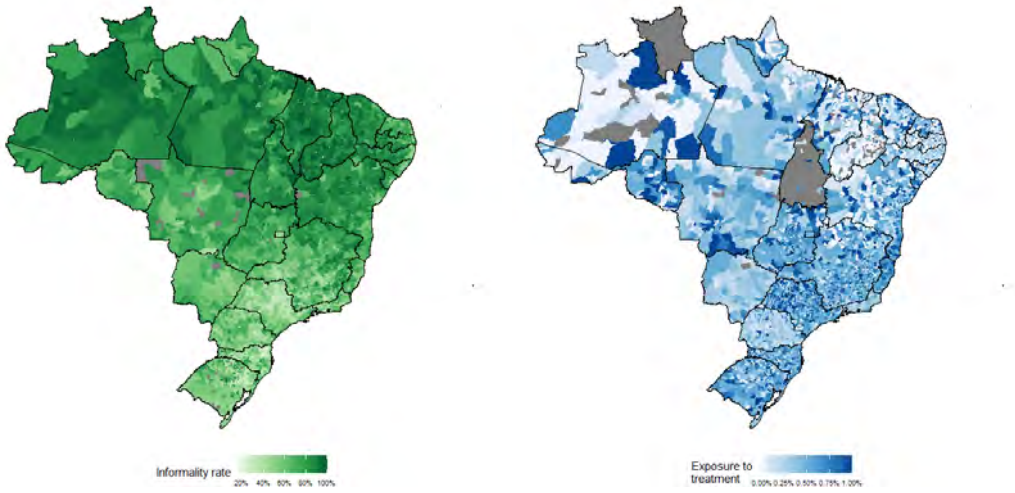
In such arrangements, a worker may be formally dismissed in order to access UI benefits while remaining informally employed, potentially by the same firm or within the same local labor market. Once benefits expire, the worker may return to formal employment with the same employer. Doornik, Schoenherr and Skrastins (2023) show that several indirect indicators consistent with strategic layoffs are more pronounced in industries and municipalities with higher levels of labor market informality. In particular, the decline in layoffs following the tightening of UI eligibility is larger in areas with greater informality.

Motivated by these findings, the goal of this section is to examine whether a similar pattern arises in the context of the dismissal tax reform. Specifically, if firms and workers engage in fake layoff agreements, we would expect the effects of the dismissal tax to be more pronounced in regions with higher informality rates.

First, Figure 5.4 shows no obvious geographic correlation between labor market informality and exposure to the dismissal tax. If anything, informality appears to be more concentrated in the North and Northeast of Brazil, whereas the firms most exposed to the reform are concentrated in the South and Southeast. Thus, the raw spatial patterns suggest that the two variables are, at most, weakly negatively correlated.

Descriptive statistics also indicate little systematic relationship between exposure to the dismissal tax and local informality rates. Firms with exposure below the median are located in municipalities with an average informality rate of 47.2% (s.d. 12.5%), while firms above the median exposure are located

Figure 5.4: Geographical Distribution of Informality Rates and Dismissal Tax Exposure



(a) Informality Rates (b) Exposure to Dismissal Tax

Notes: Panel (a) displays the geographical distribution of municipal informality rates based on the 2000 Census. Panel (b) displays the geographical distribution of synthetic dismissal tax payments prior to the reform as a share of total firms' payrolls in the municipality.

in municipalities with an average informality rate of 48.2% (s.d. 12.3%). Consistent with the geographic patterns shown in Figure 5.4, these statistics suggest that municipalities with higher exposure to the dismissal tax are not systematically associated with higher or lower levels of labor market informality.

To test whether the patterns documented by Doornik, Schoenherr and Skrastins (2023) also arise in the context of the dismissal tax reform, I estimate a triple-difference-in-differences specification that interacts the treatment exposure with municipal informality rates. In addition to the event-study specification, I estimate the following regression:

$$\begin{aligned} Y_{f,s,m,t} =& \alpha_f + \eta_s + \nu_m + \zeta_t + \beta_{post} \cdot \overline{Tax}_f \cdot Post_t + \gamma_{post} \cdot Inform_m \cdot Post_t + \\ & \sigma_{post} \cdot \overline{Tax}_f \cdot Post_t \cdot Inform_m + \delta \cdot \overline{Tax}_f \cdot Trend_t + \epsilon_{f,s,m,t} \end{aligned} \tag{5-4}$$

where $Inform_m$ denotes the informality rate in municipality m. The coefficient of interest, σ_{post}, captures whether the effect of the dismissal tax varies systematically with the degree of local labor market informality.

All remaining variables follow the same definitions as in Equation 5-3. Because the specification includes establishment, sector, municipality, and time fixed effects, the lower-order terms of the interaction are absorbed by the fixed effects structure. As in the baseline specification, I include a trend to account for differential pre-trends across establishments with different levels of exposure to the dismissal tax. Under this specification, the identifying assumption remains that any differences in outcome trends between firms with higher and lower exposure would have continued along the same linear path in the absence of the reform (DUSTMANN et al., 2022).

Table 5.3: Triple Difference-in-Differences Results: Informality Heterogeneity

	(1)	(2)	(3)	(4)	(5)
Panel A: Total Layoffs					
$\overline{Tax}_f \cdot Post_t$	-23.299***	-23.358***	-22.430***	-17.945***	-21.671***
	(2.271)	(2.270)	(2.128)	(2.104)	(1.931)
$Inform_m \cdot Post_t$	-0.007	-0.005	-0.007	0.530***	-0.007
	(0.039)	(0.039)	(0.039)	(0.041)	(0.037)
$\overline{Tax}_f \cdot Inform_m \cdot Post_t$	12.669***	12.691***	12.647***	3.661	9.829**
	(4.417)	(4.412)	(4.417)	(4.367)	(4.017)
Panel B: Total Quits					
$\overline{Tax}_f \cdot Post_t$	0.462	0.411	-1.844***	1.132**	-1.653***
	(0.595)	(0.594)	(0.558)	(0.558)	(0.507)
$Inform_m \cdot Post_t$	0.007	0.007	0.006	0.264***	0.007
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
$\overline{Tax}_f \cdot Inform_m \cdot Post_t$	1.622	1.726	1.678	-1.691	1.358
	(1.158)	(1.155)	(1.158)	(1.158)	(1.054)
Panel C: Total Recalls					
$\overline{Tax}_f \cdot Post_t$	0.200	0.192	0.327	0.149	0.232
	(0.318)	(0.317)	(0.298)	(0.267)	(0.270)
$Inform_m \cdot Post_t$	0.029***	0.030***	0.029***	0.012**	0.028***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$\overline{Tax}_f \cdot Inform_m \cdot Post_t$	-0.140	-0.110	-0.143	-0.130	-0.023
	(0.618)	(0.617)	(0.618)	(0.554)	(0.562)
Firm FE (α_f)	yes	yes	yes	yes	yes
Sector FE (η_s)	yes	no	yes	yes	yes
Municipality FE (ν_m)	yes	no	yes	yes	yes
Year-month FE (ζ_t)	yes	yes	yes	yes	yes
$\overline{Tax}_f \cdot Trend_t$	yes	yes	no	no	no
Baseline means $\cdot\, Trend_t$	no	no	no	yes	no
Baseline means	no	no	no	no	yes

Notes: This table reports triple difference-in-differences estimates of the effect of the 2001 dismissal tax on firm-level outcomes. The coefficients shown corresponds to β_{post}, γ_{post} and σ_{post} from Equation 5-4. Baseline means are the pre-reform average age of workers in firm f, the pre-reform average share of male workers in firm f, and the pre-reform average share of college-educated workers in firm f. They are included in the specification as alternatives to the $\overline{Tax}_f \cdot Trend_t$ component, with and without the $Trend_t$ interaction term. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

β_{post} captures the differential change in outcomes after the reform for firms with higher exposure to the dismissal tax. Meanwhile, γ_{post} captures the differential change in outcomes after the reform for firms in municipalities with higher informality rates. The coefficient of primary interest is σ_{post}, which measures whether the impact of the dismissal tax varies systematically with the level of labor market informality in the municipality. The resulting estimates are reported in Table 5.3.

Table 5.3 presents the triple-difference-in-differences estimates of the effect of firms' exposure to the dismissal tax on separation outcomes, allowing for heterogeneity with respect to municipal informality. Each column presents a different specification of Equation 5-4. The columns follow the same specification as in Table 5.2. In Panel A, the interaction between tax exposure and

informality is positive in most specifications, indicating that the reduction in layoffs is attenuated in more informal municipalities.

This pattern contrasts with the results in Doornik, Schoenherr and Skrastins (2023), which finds that higher informality is associated with stronger layoff responses around institutional incentives. One possible reconciliation lies in the different levels of aggregation. In the firm-level setting considered here, municipalities with lower informality are those where a larger share of firms operates within the formal sector and is therefore directly affected by the dismissal tax. As a result, the aggregate response to the reform is stronger in these locations. In more informal municipalities, by contrast, a smaller fraction of firms is exposed to the policy, which mechanically dampens the estimated effect at the municipal level.

Panels B and C show that this heterogeneity is not present for quits and recalls, where the triple interaction is generally small and statistically insignificant. Taken together, the results suggest that the role of informality in shaping the effects of the dismissal tax operates primarily through the layoff margin, and that differences with the existing literature are likely driven by the distinction between firm-level and worker-level responses rather than conflicting underlying mechanisms.

5.2 Labor Inspection

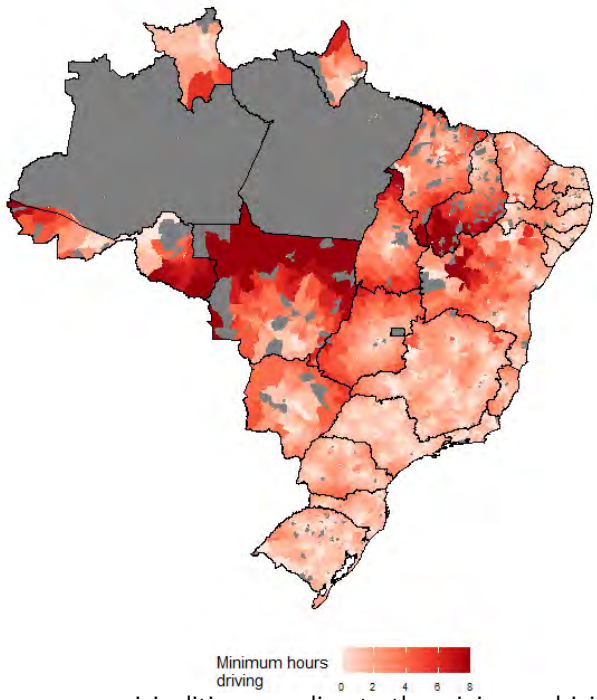
This section provides a second heterogeneity analysis that sheds light on the role of labor inspections in sustaining fake layoff agreements. As discussed in Chapter 2, labor inspectors operate from regional offices and must travel by car to establishments to monitor compliance. Municipalities that are farther from a labor inspection office are therefore harder to audit, which lowers the expected probability that a fake layoff agreement is detected.

This margin is distinct from the informality channel analyzed in the previous section. While informality captures the availability of an outside option that may facilitate strategic layoffs, enforcement captures the expected cost of detection. If the dismissal tax discourages fake layoff agreements, its effect should be stronger in municipalities that are easier to inspect, because collusive arrangements there face a higher probability of being uncovered.

Figure 5.5 shows that labor enforcement is not spatially concentrated in the same places as municipal exposure to the dismissal tax. The map suggests no obvious geographic correlation between pre-reform dismissal tax exposure and the distance to the nearest labor inspection office. As discussed in Chapter 4, most municipalities in the states of Amazonas and Pará are excluded from the analysis, as transportation in these regions relies primarily on river routes rather than road networks.

Descriptive statistics point in the same direction. Firms below the median exposure are located in municipalities with average travel times to the nearest labor inspection office of 45 minutes (s.d. 1:02). Meanwhile, firms above the median exposure have average travel times of 49 minutes (sd. 1:14). This suggests that exposure to the dismissal tax is not systematically correlated with enforcement intensity, which is reassuring for the triple-difference design.

Figure 5.5: Labor Inspection Intensity



Notes: The figure maps municipalities according to the minimum driving time from the nearest labor inspection office. Higher driving times indicate weaker labor enforcement.

To test whether the effect of the dismissal tax varies with the likelihood of detection, I estimate the following triple-difference specification:

$$\begin{aligned} Y_{f,s,m,t} =& \alpha_f + \eta_s + \nu_m + \zeta_t + \beta_{post} \cdot \overline{Tax}_f \cdot Post_t + \gamma_{post} \cdot Hours_m \cdot Post_t + \\ & \upsilon_{post} \cdot \overline{Tax}_f \cdot Post_t \cdot Hours_m + \delta \cdot \overline{Tax}_f \cdot Trend_t + \epsilon_{f,s,m,t} \end{aligned} \tag{5-5}$$

where $Hours_m$ denotes the minimum driving time from municipality m to the nearest labor inspection office. Larger values of $Hours_m$ indicate weaker enforcement, since establishments are harder to reach and inspect. The coefficient of primary interest is υ_{post}, which captures whether the post-reform effect of dismissal tax exposure varies systematically with auditability.

All remaining variables follow the same definitions as in Equation 5-3 and the same discussion from Equation 5-4 holds here. The resulting estimates are reported in Table 5.4.

Table 5.4 reports the triple difference-in-differences estimates of the effect of firms' exposure to the dismissal tax on separation outcomes. Each column presents a different specification of Equation 5-4. The columns follow the same specification as in Table 5.2.

The results in Table 5.4 show that the estimated effects of the dismissal tax vary across specifications, reflecting the sensitivity to alternative assumptions regarding baseline trends, as emphasized in Dustmann et al. (2022). In particular, once differential pre-trends across firms are flexibly accounted for, the evidence points to a more nuanced role of enforcement. The estimates suggest that labor inspection intensity primarily affects layoffs, with no significant

Table 5.4: Triple Difference-in-Differences Results: Labor Inspection Heterogeneity

	(1)	(2)	(3)	(4)	(5)
Panel A: Total Layoffs					
$\overline{Tax}_f \cdot Post_t$	-17.770***	-17.781***	-16.660***	-16.865***	-17.255***
	(1.057)	(1.060)	(0.672)	(0.668)	(0.611)
$Hours_m \cdot Post_t$	0.001	0.002	0.001	0.067***	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$\overline{Tax}_f \cdot Hours_m \cdot Post_t$	1.711***	1.641***	1.707***	1.779***	1.669***
	(0.572)	(0.572)	(0.572)	(0.557)	(0.514)
Panel B: Total Quits					
$\overline{Tax}_f \cdot Post_t$	1.178***	1.171***	-0.836***	0.306*	-0.818***
	(0.282)	(0.282)	(0.180)	(0.182)	(0.163)
$Hours_m \cdot Post_t$	0.009***	0.008***	0.008***	0.015***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$\overline{Tax}_f \cdot Hours_m \cdot Post_t$	-0.061	-0.059	-0.054	0.029	-0.028
	(0.153)	(0.152)	(0.153)	(0.152)	(0.137)
Panel C: Total Recalls					
$\overline{Tax}_f \cdot Post_t$	0.210	0.215	0.229**	0.021	0.160*
	(0.158)	(0.159)	(0.101)	(0.091)	(0.091)
$Hours_m \cdot Post_t$	0.001*	0.003***	0.003***	0.002**	0.003***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
$\overline{Tax}_f \cdot Hours_m \cdot Post_t$	-0.068	-0.069	-0.068	0.077	-0.032
	(0.086)	(0.086)	(0.086)	(0.076)	(0.077)
Firm FE (α_f)	yes	yes	yes	yes	yes
Sector FE (η_s)	yes	no	yes	yes	yes
Municipality FE (ν_m)	yes	no	yes	yes	yes
Year-month FE (ζ_t)	yes	yes	yes	yes	yes
$\overline{Tax}_f \cdot Trend_t$	yes	yes	no	no	no
Baseline means $\cdot\, Trend_t$	no	no	no	yes	no
Baseline means	no	no	no	no	yes

Notes: This table reports triple difference-in-differences estimates of the effect of the 2001 dismissal tax on firm-level outcomes. The coefficients shown corresponds to β_{post}, γ_{post} and υ_{post} from Equation 5-5. Baseline means are the pre-reform average age of workers in firm f, the pre-reform average share of male workers in firm f, and the pre-reform average share of college-educated workers in firm f. They are included in the specification as alternatives to the $\overline{Tax}_f \cdot Trend_t$ component, with and without the $Trend_t$ interaction term. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

impact on other separation margins. As previously discussed, distance from a labor inspection office reduces the probability of being detected in a fake layoff agreement, weakening enforcement and allowing firms in more remote municipalities to continue engaging in such illegal collusion.

6
Hazard Analysis

To further examine how the dismissal tax affects separation behavior, I analyze the evolution of layoff hazard rates before and after the reform. In the labor economics literature, the layoff hazard rate represents the instantaneous probability that a worker is dismissed without cause at a given tenure, conditional on remaining employed up to that point. Studying hazard rates is particularly informative in this setting, as it allows me to trace how separation probabilities evolve along the tenure distribution, especially around institutional thresholds where dismissal costs and incentives change discontinuously.

For instance, Carvalho, Corbi and Narita (2018) show that changes in unemployment insurance eligibility generate differential responses in layoff hazards precisely at the tenure thresholds affected by the reform. They document discontinuities in dismissal probabilities around these thresholds, with patterns consistent with workers and firms responding to the incentives embedded in the UI system.

Motivated by these findings, the goal of this chapter is to examine whether similar behavioral responses arise in the context of the dismissal tax reform. In contrast to the sharp discontinuities associated with eligibility thresholds, the dismissal tax affects separation incentives more broadly through an increase in firing costs. I therefore assess whether the reform leads to shifts in the level of layoff hazards along the tenure distribution, particularly in regions where incentives for strategic separations are more relevant. Figure 6.1 presents the evolution of layoff hazards before and after the reform.

Figure 6.1 shows no systematic aggregate change in layoff hazard rates following the introduction of the dismissal tax. However, there may still be some differential effects depending on the workers' FGTS balances.

The causal analysis in this chapter requires a measure of treatment exposure defined at the spell level. In contrast to the firm-level analysis, which relies on the average exposure $\overline{Tax}_f$, I construct a spell-level measure that captures the dismissal cost associated with each worker i at a given tenure.

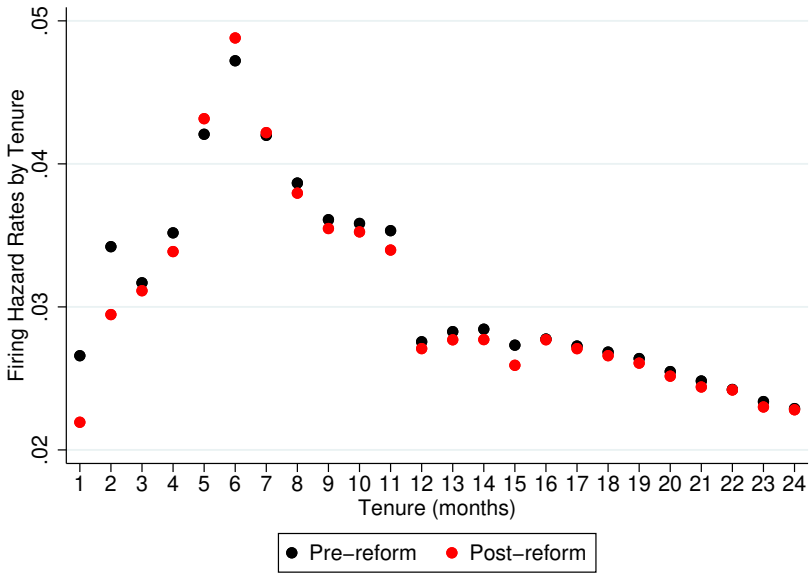
Specifically, I define exposure as the hypothetical dismissal tax that the firm would incur if the worker were laid off at tenure T, previous to the reform. This is given by:

$$Tax_{i,T} = 0.1 \cdot F(w,T)_i, \quad T = \min\{\text{layoff date}, Q3.2001\} \tag{6-1}$$

where $F(w,t)_i$ denotes worker i's accumulated FGTS balance as a function of wages and tenure. This measure varies across workers and over time, reflecting the fact that dismissal costs increase with both tenure and earnings.

To ensure that treatment intensity is predetermined, $Tax_{i,T}$ uses pre-reform information. Hence, $T = \min\{\text{layoff date}, Q3.2001\}$, which represents the accumulated FGTS balance-in real terms-of worker i in the spell previous to the reform, or at layoff. As a result, this measure may understate the realized dismissal costs for workers whose tenure continues to accumulate after the reform, but it preserves the relative ranking of exposure across workers

Figure 6.1: Layoff Hazard Rates by Tenure Before and After the Dismissal Tax Reform



Notes: This figure plots layoff hazard rates by tenure (in months) for workers with 1 to 24 months of tenure, separately for the pre-reform (black dots) and post-reform (red dots) periods. The hazard rate is defined as the probability of dismissal at a given tenure, conditional on remaining employed up to that tenure.

and avoids endogeneity concerns associated with time-varying measures of dismissal costs.

This specification allows me to capture the relevant variation in dismissal costs at the spell level, which is particularly important in a hazard framework where separation probabilities are evaluated at each tenure. As expected, spells with higher exposure are associated with longer tenure and higher wages. Table 6.1 reports summary statistics for this variable.

The median of Tax_i is 76.55 BRL. As expected, spells above the median exposure are associated with higher wages and longer tenure, reflecting the fact that the dismissal tax is proportional to the worker's accumulated FGTS balance. Workers in the high-exposure group are also slightly older, more likely to be male, and more likely to have a college degree. Overall, the descriptive statistics indicate that higher exposure is concentrated among spells with stronger labor market attachment and higher earnings, which is consistent with the construction of the treatment measure.

In addition, in contrast to the previous chapters, this analysis does not rely on a 20% random subsample. Instead, I use the full universe of formal employment spells available in RAIS. This richer dataset allows for a more precise characterization of separation dynamics over the tenure distribution.

To quantify the effect of the dismissal tax on separation behavior, I estimate a Cox proportional hazard model with time-varying covariates. This approach is well suited to the analysis of duration outcomes, as it allows separation probabilities to vary flexibly with tenure without requiring parametric assumptions on the baseline hazard (MARINESCU, 2009). In addition, it enables the use of within-spell variation to identify how dismissal

Table 6.1: Descriptive Statistics by Exposure: Hazard Analysis

	Below median Tax_i	Above median Tax_i
Tax_i (BRL)	19.13	291.04
	(23.82)	(418.99)
Real wage (BRL)	1,607.62	3,514.18
	(2,201.75)	(5,175.00)
Tenure (months)	5.53	21.33
	(8.26)	(16.68)
Age (years)	29.69	31.59
	(9.89)	(9.79)
Share male (%)	69.23	69.40
	(46.15)	(46.07)
Share college educated (%)	1.98	6.18
	(13.95)	(19.78)
Observations	10,510,844	30,680,942
Share of observations (%)	25.52	74.48
Number of spells	10,486,079	27,877,576
Share of spells (%)	27.33	72.67

Notes: The table reports means and standard deviations for worker-spells below and above the median of the exposure measure Tax_i. The variable Tax_i captures a worker's pre-reform exposure to the dismissal tax, defined as the hypothetical tax payment due upon dismissal without cause. Standard deviations are reported in parentheses. All variables are measured at the spell level.

costs affect the timing of separations. The empirical specification is given by:

$$\lambda_i(t, Z) = \lambda_0(t) \exp\left[\beta \cdot Z_{i,t} + \gamma \cdot Tax_i \cdot Post_t\right] \tag{6-2}$$

where $Z_{i,t}$ is a vector of worker-level controls, including age, region, sector, college education, and legal gender. The variable Tax_i denotes the worker's pre-reform exposure to the dismissal tax, as defined in Equation 6-1. The indicator $Post_t$ equals one for periods after the reform.

To account for the timing of the policy, employment spells that span the reform are split into pre- and post-reform segments, updating tenure and time-varying covariates accordingly. This ensures that identification comes from within-spell variation in exposure to the reform. The estimation sample is restricted to workers observed prior to the reform, so that treatment intensity is predetermined.

Therefore, the coefficient γ captures how the change in dismissal costs induced by the reform differentially affects the hazard of separation for workers with higher exposure. Table 6.2 reports the results from this specification.

Table 6.2 shows that workers with higher exposure to the dismissal tax face lower layoff hazards after the reform. The estimated hazard ratio is 0.99945, implying that a one-BRL increase in exposure reduces the layoff hazard by approximately 0.055%.

To provide a more interpretable magnitude, I compare workers at the 75th and 25th percentiles of the exposure distribution. Because the 25th percentile corresponds to zero exposure, this comparison captures the full effect of the reform. Evaluated at the 75th percentile (244.03 BRL), the implied

Table 6.2: Empirical Effects of the Dismissal Tax: Hazard Analysis

	(1)	(2)	(3)
$Tax_i \times Post_t$	0.99945***	0.99946***	0.99946***
	(2.78e-06)	(2.77e-06)	(2.78e-06)
Individual controls	no	yes	yes
Region and sector controls	no	no	yes

Notes: The table reports Cox proportional hazard estimates of the effect of the 2001 dismissal tax on layoff hazards. Coefficients are reported as hazard ratios associated with γ in Equation 6-2; values below one indicate a reduction in separation risk. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

hazard ratio is approximately 0.875, corresponding to a 12.5% reduction in the layoff hazard relative to a worker with no exposure.[1]

The estimates are stable across specifications, including controls for individual characteristics as well as region and sector fixed effects.

[1]The calculation to reach this number is $\exp\left(\hat{\beta} \cdot (P_{75} - P_{25})\right) - 1 \exp(0.99945 \cdot (244.03 - 0)) - 1 = 0.875 - 1 = -0.125$

7
Conclusion

In summary, this work studies how firms respond to an increase in dismissal taxes by exploiting the 2001 Brazilian reform that introduced a 10% tax over the dismissed workers' severance savings accounts. While the theoretical literature has long emphasized the distinction between severance payments and dismissal taxes, causal empirical evidence on the latter remains limited. This paper contributes to filling this gap by providing evidence from a nationwide reform that generates meaningful variation in firms' exposure to increased dismissal costs.

A key feature of the Brazilian institutional environment is the presence of collusive "fake layoff agreements," which create an additional margin of adjustment beyond standard separation decisions. To account for this, I develop a conceptual framework in which firms and workers jointly determine whether separations occur as layoffs, quits, or fake layoffs. The model predicts that an increase in dismissal taxes reduces layoffs, increases quits, and decreases the incidence of fake layoffs, with heterogeneous effects across worker tenure.

The empirical analysis provides strong support for these predictions. Using event-study and difference-in-differences designs that exploit cross-firm variation in exposure to the reform, I document a clear reduction in layoffs among more exposed firms, particularly for workers with longer tenure, for whom dismissal costs are higher. At the same time, quits increase, especially among lower-tenure workers, consistent with a decline in the feasibility of collusive separations. Complementary evidence from recall patterns further supports a reduction in fake layoff agreements, suggesting that the reform not only affected legal separations but also altered strategic interactions between firms and workers.

The analysis also uncovers important sources of heterogeneity in the effects of the reform. Using triple-difference-in-differences specifications, I show that the reduction in layoffs is weaker in more informal municipalities and in locations farther from labor inspection offices. These patterns suggest that both informality and limited enforcement reduce the extent to which the dismissal tax is binding, allowing firms greater scope to continue engaging in collusive arrangements and to evade dismissal costs.

Complementing the firm-level results, the hazard analysis provides additional evidence on the timing of separations. While aggregate hazard rates show limited changes, worker-level estimates reveal that individuals with higher exposure to the reform experience significantly lower layoff risks.

Taken together, these findings highlight that the effects of dismissal taxes depend not only on their statutory design, but also on enforcement capacity, the prevalence of informality, and the margins along which firms and workers adjust their behavior.

An important avenue for future research is to further explore the interaction between dismissal costs and informality. Because the dismissal tax applies only to the formal sector, firms may respond along the extensive margin by shifting employment toward informal arrangements. While this paper provides

suggestive evidence through heterogeneity analyses, data limitations prevent a direct examination of firm-level transitions into informality. Understanding this margin is crucial for a comprehensive evaluation of dismissal policies in developing economies, where informality remains a central feature of labor markets.

8
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A
Conceptual Framework with High Enforcement

This appendix analyzes the predictions of the conceptual framework presented in Chapter 3 under a high enforcement environment. In this case, the expected penalties associated with collusion are sufficiently large to deter fake layoff agreements. Formally, the expected fines exceed the gains from collusion for both parties:

$$\theta \cdot [AN(w) + 0.4 \cdot F(w,T)] + LC < \xi^{firm} \tag{A-1}$$

$$(1-\theta) \cdot [AN(w) + 0.4 \cdot F(w,T)] + F(w,T) + UI(w,T) < \xi^{worker} \tag{A-2}$$

Under these conditions, fake layoffs are no longer profitable for either the firm or the worker. Consequently, the firm always prefers a unilateral layoff to engaging in a fake layoff, while the worker strictly prefers quitting rather than colluding. As a result, fake layoffs are never implemented in equilibrium and the relevant separation margins become layoffs initiated by the firm and quits initiated by the worker.

The structure of the model remains otherwise identical to the baseline framework. The same payoff functions described in Table 3.1 apply, and the sets of available actions for firms and workers remain unchanged. However, because fake layoffs are strictly dominated by the other options, the effective strategic interaction simplifies to a comparison between continuing the match and unilateral separation.

Based on these preferences, the possible outcomes of the model are summarized in Table A.1.

Table A.1: Potential Outcomes of the Model Based on Preferences: High Enforcement Scenario

	Firm		
Worker	$C \succ L \succ FL$	$L \succ C \succ FL$	$L \succ FL \succ C$
$C \succ Q \succ FL$	Continue (C)	Layoff (L)	Layoff (L)
$Q \succ C \succ FL$	Quit (Q)	Undefined	Undefined
$Q \succ FL \succ C$	Quit (Q)	Undefined	Undefined

Notes: The table summarizes equilibrium outcomes under high enforcement. Fake layoffs are dominated due to high expected penalties, so only quits and layoffs arise. "Undefined" denotes cases where firm and worker prefer different unilateral separations and no mutually agreed outcome exists.

Figure A.1 illustrates the agents' separation decisions following a shock in the high-enforcement environment. Panel (a) shows the equilibrium regions before the introduction of the dismissal tax, while Panel (b) shows the equilibrium regions after the tax.

The same intuition described in Figure 3.1 applies in the high-enforcement scenario. However, because collusive separations are no longer feasible, the tax affects only the incentives for unilateral separations.

First, the yellow area predicts an increase in quits. The dismissal tax reduces the value of layoffs to the firm, making firms less willing to terminate matches

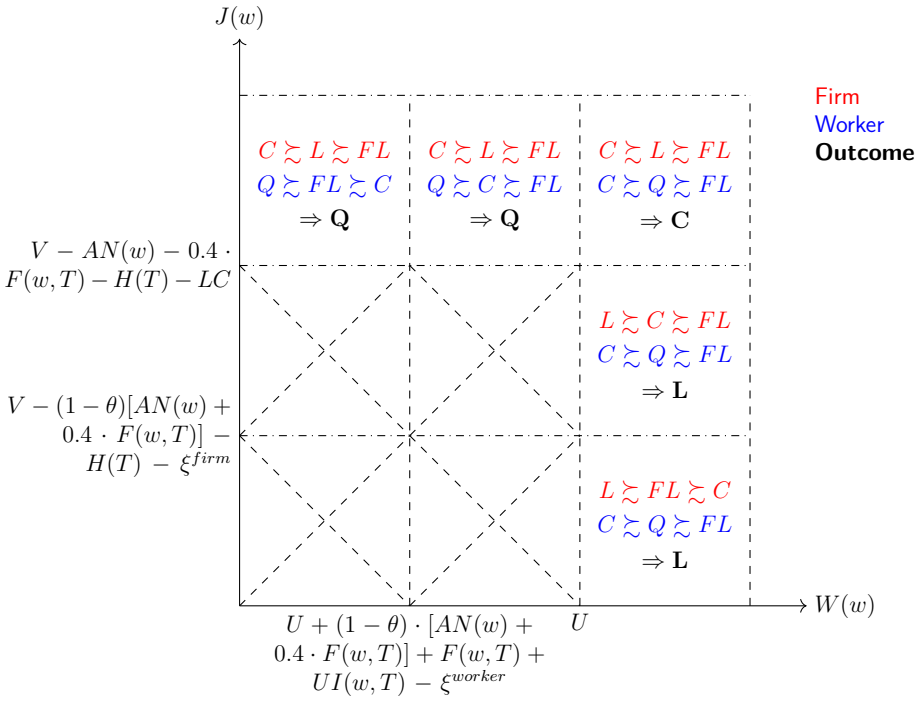
unilaterally. As a result, some matches in which the worker prefers to separate but the firm prefers continuation now end through voluntary quits.

Second, the orange area predicts a decrease in layoffs. The dismissal tax directly increases the monetary cost of dismissing workers, which reduces the firm's incentive to initiate layoffs. Consequently, some matches that would previously have ended in layoffs now continue.

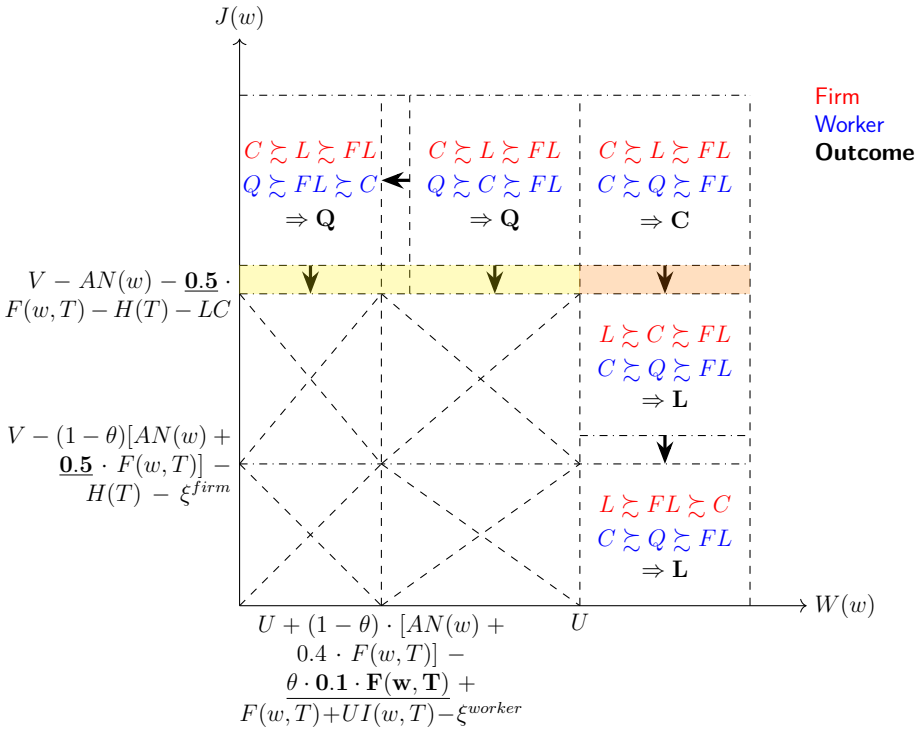
Importantly, these predictions are consistent with those derived in the baseline framework presented in Chapter 3. Even though collusive separations are no longer feasible under high enforcement, the dismissal tax still increases quits and reduces layoffs. The key difference between the two environments lies in the mechanism: under low enforcement, the tax operates partly by reducing fake layoff agreements, whereas under high enforcement it affects only unilateral separations.

Even though the bargaining power predictions do not apply here, the additional tenure-related predictions discussed in Chapter 3 remain valid. In particular, because severance-related payments increase with tenure, the effect of the dismissal tax on layoffs should be stronger among more tenured workers.

Figure A.1: Potential Outcomes of the Model Before and After the Tax: High Enforcement Scenario



(a) Before the 10% tax



(b) After the 10% tax

Notes: The figure shows equilibrium outcomes under high enforcement. The x-axis denotes the worker's match value $W(w)$ (with quitting as the outside option) and the y-axis the firm's match value $J(w)$ (with layoffs as the outside option). Fake layoffs are dominated, so only quits and layoffs arise. Dashed regions correspond to preference profiles (Table A.1). Panel (a) shows the pre-tax equilibrium; Panel (b) shows the post-tax equilibrium. Arrows indicate shifts in thresholds after the tax. The shaded areas highlight predicted changes: increases in quits (yellow) and decreases in layoffs (orange).

B
Firms with at Least Two Workers Robustness

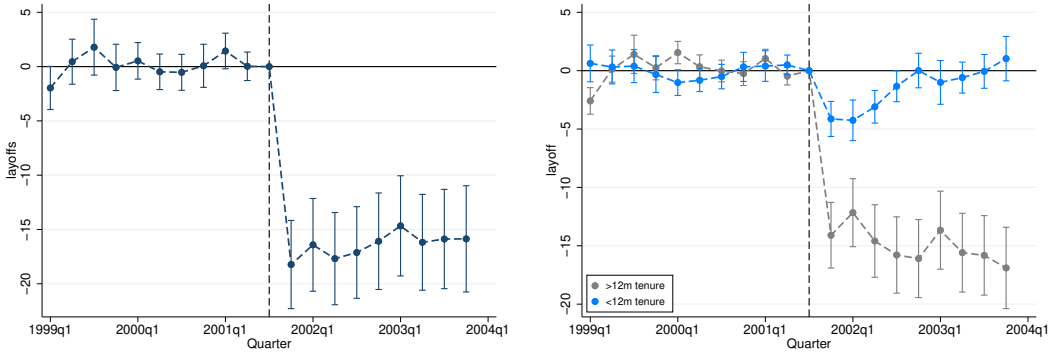
This appendix replicates the firm-level analysis excluding establishments with a single worker. As discussed in Chapter 5, a non-negligible share of establishments in RAIS consists of single-worker firms in which the worker is also the owner. These firms mechanically exhibit no separations during the sample period and therefore have zero pre-reform exposure to the dismissal tax. To ensure that the main results are not driven by the presence of these establishments, I re-estimate the event-study and difference-in-differences specifications restricting the sample to firms with at least two workers.

Figures B.1–B.3 reproduce the event-study analysis for layoffs, quits, and recalls using this restricted sample. The results closely mirror those obtained in the full sample. First, layoffs decline among firms with higher exposure to the dismissal tax after the reform. As in the main analysis, this reduction is concentrated among workers with more than 12 months of tenure, for whom the tax base is larger due to higher accumulated FGTS balances. For workers with shorter tenure, the effect remains small and statistically insignificant in the longer run.

Second, quits increase among more exposed firms following the introduction of the tax. This pattern is again consistent with the conceptual framework developed in Chapter 3. As the dismissal tax reduces the gains from collusive layoffs, some separations that would previously have occurred through fake layoff agreements instead occur through voluntary quits.

Finally, recall rates decline after the reform among firms with greater exposure to the tax. Because recalls are interpreted as an indirect indicator of fake layoff

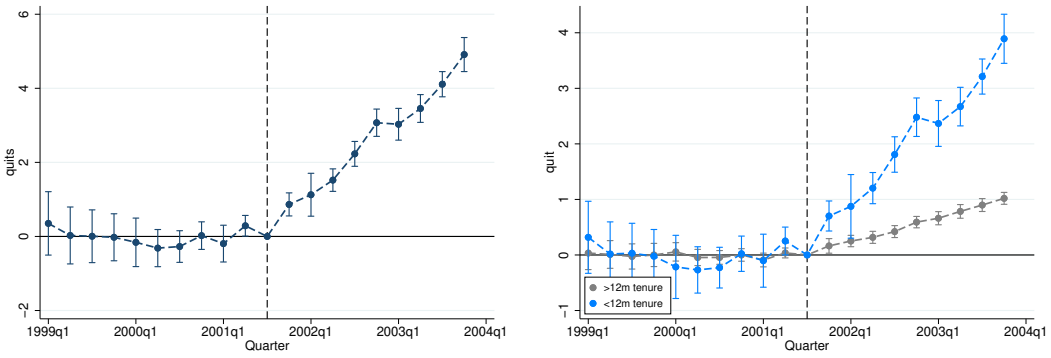
Figure B.1: Event-Study Results: Layoff Effects of the Dismissal Tax for Firms with More than One Worker



(a) Full Sample (b) Tenure Heterogeneity

Notes: The graph displays the effect over the total number of layoffs. Panel (a) uses the sample only with firms with more than one worker—no single-worker firms. Panel (b) distinguishes between layoffs of workers with more than 12 months of tenure at separation (gray line) and less than 12 months of tenure (blue line). Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company—i.e., common ownership.

Figure B.2: Event-Study Results: Quit Effects of the Dismissal Tax for Firms with More than One Worker



(a) Full Sample (b) Tenure Heterogeneity

Notes: The graph displays the effect over the total number of quits. Panel (a) uses the sample only with firms with more than one worker—no single-worker firms. Panel (b) distinguishes between workers who quit with more than 12 months of tenure at separation (gray line) and less than 12 months of tenure (blue line). Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company—i.e., common ownership.

agreements, this pattern provides further evidence that the dismissal tax reduced the incentives for firms and workers to engage in collusive separations.

Table B.1 presents the corresponding difference-in-differences estimates from Equation 5-3 for the restricted sample. The results are quantitatively similar to those obtained in the baseline analysis. Firms with greater exposure to the reform experience a reduction in layoffs, accompanied by an increase in quits. The stability of the estimates across specifications indicates that the main findings are not driven by single-worker establishments.

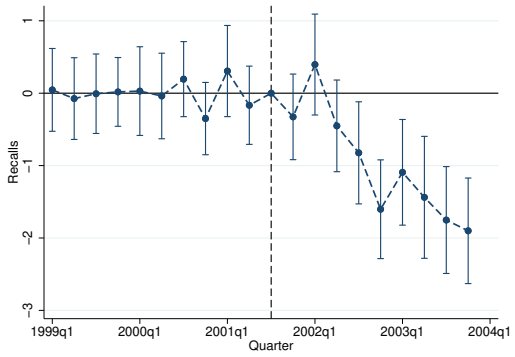
Taken together, these robustness results confirm that the patterns documented in the main analysis are not an artifact of the large number of small establishments in the data. Instead, the results persist when restricting the sample to firms with at least two workers, reinforcing the interpretation that the dismissal tax reduced the incidence of both true and fake layoffs.

Table B.1: Difference-in-Differences Results: Effects of the Dismissal Tax for Firms with More than One Worker

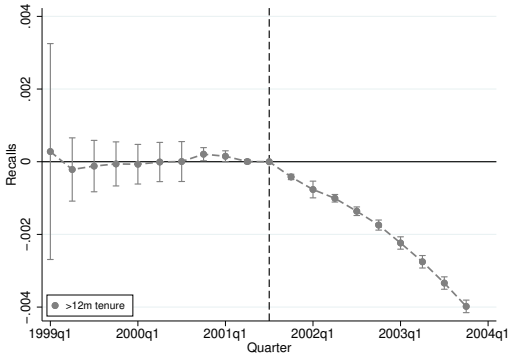
	(1)	(2)	(3)	(4)	(5)
Panel A: Layoffs					
Total Layoffs	-24.438***	-24.403***	-25.728***	16.252***	14.396***
	(1.416)	(1.424)	(0.789)	(1.449)	(1.105)
Layoffs >12m	-19.600***	-19.588***	-17.772***	12.164***	11.187***
	(0.743)	(0.744)	(0.414)	(1.004)	(0.844)
Layoffs <12m	-4.838***	-4.815***	-7.956***	4.088***	3.209***
	(1.041)	(1.049)	(0.580)	(0.563)	(0.345)
Layoffs >3m	-24.474***	-24.444***	-24.671***	16.084***	14.281***
	(1.238)	(1.242)	(0.690)	(1.353)	(1.057)
Layoffs <3m	0.035	0.041	-1.057***	0.168	0.115
	(0.473)	(0.476)	(0.263)	(0.305)	(0.185)
Panel B: Quits					
Total Quits	1.555***	1.502***	-1.545***	-1.251***	0.006
	(0.383)	(0.383)	(0.213)	(0.140)	(0.111)
Quits >12m	0.305***	0.295***	-0.569***	-0.312***	0.139***
	(0.092)	(0.092)	(0.051)	(0.037)	(0.048)
Quits <12m	1.249***	1.207***	-0.975***	-0.939***	-0.133*
	(0.363)	(0.363)	(0.202)	(0.123)	(0.070)
Quits >3m	0.749***	0.728***	-1.022***	-0.712***	0.063
	(0.193)	(0.193)	(0.107)	(0.071)	(0.089)
Quits <3m	0.805***	0.774**	-0.522***	-0.540***	-0.057
	(0.303)	(0.303)	(0.169)	(0.092)	(0.035)
Panel C: Recalls					
Total Recalls	-0.116	0.004	0.146	0.185***	0.358***
	(0.260)	(0.274)	(0.145)	(0.043)	(0.048)
Recalls >12m	0.001	0.001	0.001	-0.001*	-0.001*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Recalls <12m	-0.052	-0.031	0.384***	0.251***	0.174***
	(0.104)	(0.104)	(0.058)	(0.035)	(0.020)
Recalls >3m	0.024	0.026	0.389***	0.140***	0.080***
	(0.051)	(0.051)	(0.029)	(0.012)	(0.008)
Recalls <3m	-0.076	-0.056	-0.006	0.112***	0.094***
	(0.081)	(0.082)	(0.045)	(0.028)	(0.014)
Firm FE (α_f)	yes	yes	yes	yes	yes
Sector FE (η_s)	yes	no	yes	yes	yes
Municipality FE (ν_m)	yes	no	yes	yes	yes
Year-month FE (ζ_t)	yes	yes	yes	yes	yes
$\overline{Tax}_f \cdot Trend_t$	yes	yes	no	no	no
Baseline means $\cdot\, Trend_t$	no	no	no	yes	no
Baseline means	no	no	no	no	yes

Notes: This table reports difference-in-differences estimates of the effect of the 2001 dismissal tax on firm-level outcomes for firms with more than one worker. The coefficient corresponds to β_{post} from Equation 5-3. Baseline means are the pre-reform average age of workers in firm f, the pre-reform average share of male workers in firm f, and the pre-reform average share of college-educated workers in firm f. They are included in the specification as alternatives to the $\overline{Tax}_f \cdot Trend_t$ component, with and without the $Trend_t$ interaction term. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

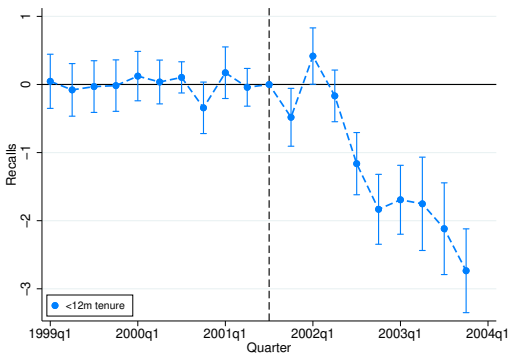
Figure B.3: Event-Study Results: Recall Effects of the Dismissal Tax for Firms with More than One Worker



(a) Full sample



(b) Above 12 months



(c) Below 12 months

Notes: The graph displays the effect over the total number of recalls. Panel (a) uses the sample only with firms with more than one worker—no single-worker firms. Panel (b) restricts the sample to workers who were separated with more than 12 months of tenure and were later recalled by the same establishment. Panel (c) restricts the sample to workers who were separated with less than 12 months of tenure and were later recalled by the same establishment. Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company—i.e., common ownership.

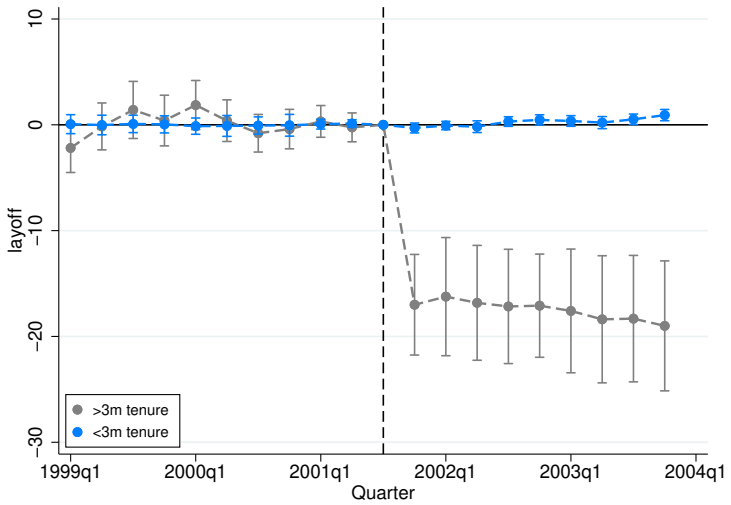
C
3 Months Threshold Heterogeneity

This appendix examines whether the results presented in Chapter 5 are sensitive to another institutional tenure threshold in Brazilian labor legislation. Under the *Consolidação das Leis do Trabalho* (CLT), employment relationships begin with a probationary period known as the *contrato de experiência*. During the first three months of tenure, firms can terminate the employment relationship without facing the dismissal costs discussed in Chapter 2. In particular, employers are not required to pay the 10% layoff tax tied to the FGTS balance when the worker is dismissed during this period. As a result, the dismissal tax should have no relevant effects over workers with tenure below three months.

Figure C.1 replicates the layoff event-study analysis distinguishing between workers with more than three months of tenure and those with less than three months of tenure at separation. Layoffs decline among firms with higher exposure to the dismissal tax, and this effect is concentrated among workers with tenure above the three-month threshold. In contrast, layoffs of workers within the probationary period exhibit no systematic response to the reform, which is consistent with the absence of dismissal costs for these separations.

Table C.1 presents the corresponding difference-in-differences estimates using the three-month tenure cutoff. Following the main specification in column (1), firms with higher exposure to the dismissal tax experience reductions in layoffs only for workers above the probationary threshold, while the effects on separations involving workers within the experience period are insignificant. Recalls show no

Figure C.1: Event-Study Results: Layoff Effects of the Dismissal Tax at 3 Months Tenure



Notes: This figure plots the event-study outcomes when distinguishing between laid off workers with more than 3 months tenure at layoff (gray line) and less than 3 months tenure at layoff (blue line). Outcomes are linearly detrended using the pre-reform coefficients β_τ for $\tau \in$ [Q1.1999, Q3.2001]. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level, the firm identification number. This clustering allows for correlation in outcomes across establishments belonging to the same company-i.e., common ownership.

Table C.1: Difference-in-Differences Results: Effects of the Dismissal Tax at the 3 Month Tenure Threshold

	(1)	(2)	(3)	(4)	(5)
Panel A: Layoffs					
Layoffs >3m	-13.839***	-17.260***	-15.931***	-15.965***	-16.611***
	(0.796)	(0.797)	(0.441)	(0.433)	(0.401)
Layoffs <3m	-0.148	-0.169	-0.605***	-0.347*	-0.486***
	(0.346)	(0.346)	(0.191)	(0.197)	(0.174)
Panel B: Quits					
Quits >3m	0.706***	0.715***	-0.655***	0.295***	-0.647***
	(0.141)	(0.141)	(0.078)	(0.078)	(0.071)
Quits <3m	0.512***	0.501***	-0.410***	0.012	-0.379***
	(0.177)	(0.177)	(0.098)	(0.098)	(0.089)
Panel C: Recalls					
Recalls >3m	-0.025	-0.020	0.281***	0.033	0.282***
	(0.006)	(0.073)	(0.039)	(0.037)	(0.036)
Recalls <3m	-0.003	-0.035	-0.011	-0.076**	-0.001
	(0.005)	(0.060)	(0.033)	(0.031)	(0.030)
Firm FE (α_f)	yes	yes	yes	no	no
Sector FE (η_s)	yes	no	yes	yes	yes
Municipality FE (ν_m)	yes	no	yes	yes	yes
Year-month FE (ζ_t)	yes	yes	yes	yes	yes
$\overline{Tax}_f \cdot Trend_t$	yes	yes	no	no	no
Baseline means $\cdot\, Trend_t$	no	no	no	yes	no
Baseline means	no	no	no	no	yes

Notes: This table reports difference-in-differences estimates of the effect of the 2001 dismissal tax on firm-level outcomes in the data sample without single-worker firms. The coefficient shown corresponds to β_{post} from Equation 5-3, capturing the differential post-reform change for firms with higher pre-reform exposure to the tax. Baseline means are the pre-reform average age of workers in firm f, the pre-reform average share of male workers in firm f, and the pre-reform average share of college-educated workers in firm f. They are included in the specification as alternatives to the $\overline{Tax}_f \cdot Trend_t$ component, with and without the $Trend_t$ interaction term. Standard errors are clustered at the 8-digit *Cadastro Nacional da Pessoa Jurídica* (CNPJ) level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

behavior for workers below nor above the 3 months tenure threshold, while quits increase.

Overall, these results reinforce the interpretation of the main findings. Because firms face essentially no dismissal costs during the experience period, the introduction of the dismissal tax primarily affects separations involving workers who have already passed the three-month threshold. The absence of strong effects among workers with very short tenure therefore provides additional evidence that the observed responses are driven by the change in dismissal costs introduced by the reform.