The Role of Unions in Labor Markets with Informality: Evidence from Brazil*

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Abstract

This paper studies the impact of unions in a labor market with widespread informality and incomplete enforcement of labor law, a prominent feature in many developing economies. I analyze the effects of a reform in Brazil that made previously mandatory union dues for workers optional; I find that the resulting loss of resources led unions to reduce staff, close offices, and cut back on collective bargaining activities. I estimate the causal effect of weaker unions using a difference-in-differences approach based on spatial variation in baseline union effectiveness. Consistent with weakening unions' bargaining power, I find that the policy reduced formal sector wages by 0.9% in labor markets with relatively effective unions, compared to labor markets with less effective unions. However, counter to the policy's stated goal, it led to a 2.5% decrease in formal employment in these markets. I find suggestive evidence that this was accompanied by an increase in the number of workers hired without a formal contract, and find no significant effect on total wage employment. As unions weakened, their coordination with inspectors in targeting audits diminished, implying that unions and formal labor regulations are complements in enforcing labor laws. These patterns highlight the intricate ways in which unions affect contracting in economies with imperfect enforcement of labor regulations.

JEL Codes: J51, J58, O17

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1 Introduction

Unions play a prominent role in shaping labor market outcomes, widely credited with changes such as reducing wage inequality (Farber et al., 2021), improving working conditions (Corradini et al., 2023), and attenuating the effects of employer market power (Dodini et al., 2024) among others (Freeman and Medoff, 1984). At the same time, unions may change the composition of employment (Frandsen, 2021), decrease employment (Sojourner et al., 2015), and decrease likelihood of survival of unionized establishments (Wang and Young, 2023). However, nearly all the evidence on unions in the literature comes from developed countries, where contracting institutions guarantee that the wage work force is almost entirely formalized. This stands in contrast to the vast majority of labor markets across the globe in which informality of wage contracts is pervasive.[1] In such settings, unions may do more than bargain over wages and amenities for formally contracted workers, they can influence whether jobs themselves are formal or informal by affecting how labor laws are enforced. This paper uses a large and sudden weakening of unions in Brazil to show how unions affect labor markets when informality is widespread.

I study Brazil's 2017 reform that made previously mandatory union dues voluntary. After the reform, unions lost over 92% of their revenue, local union offices closed, union staff shrank, and bargaining activity reduced. The collapse was driven by free-riding, since workers remained covered by collective agreements even if they opted out of paying dues. The policy was intended to spur formal hiring by reducing rigidities in the labor market. In Brazil formal firms hire some workers informally, such hires are cheaper and more flexible but subject to penalties if detected.[2]

Before the 2017 reform, unions were a central and institutionalized part of Brazil's labor market. Every formally employed worker was automatically covered by a local union, and each contributed the equivalent of one day's wages per year as a mandatory union tax. These dues financed union offices and staff who maintained a local presence. Unions negotiated collective bargaining agreements (CBAs) with employers, they also liaised with labor inspectors to identify firms that hired workers informally, supporting audit activities. In this system, unions shaped both the conditions of formal employment and the degree of compliance with labor law, making their sudden funding loss in 2017 a powerful shock to the labor market.

The employment effects of weakening unions in a context such as Brazil are theoretically ambiguous due to the opposing channels through which unions may shape labor markets with informality. By bargaining for higher wages and amenities, unions raise the cost of formal labor and can discourage formal hiring. At the same time, unions have strong incentives to keep informal hiring low: the presence of informally hired workers as a substitute for formally contracted workers weakened their bargaining power over wages and amenities, and, only formally hired workers paid union dues. By helping enforcement agencies target labor audits, unions raise the expected cost of

[1] Informally hired workers constitute 60% of the global labor force, 93% of this informal employment is in emerging and developing countries (ILO).

[2] 31% of all wage workers are hired informally (Brazil Census 2010).

hiring workers informally. I develop a simple model which formalizes this tension and shows that the net impact of weakening unions depends on their initial balance of influence; specifically, the extent to which they affected formal wages relative to their role in raising the costs of informal contracts.

I estimate the local labor market impacts of the reform in a difference-in-differences framework by comparing labor markets that had more effective unions, to those that had less effective unions in the pre-period. Union effectiveness is captured by variation in collective bargaining activity. Institutional features of Brazil's union system have historically created substantial heterogeneity in bargaining activity across unions. Between 2012 and 2017, CBAs covered about 42% of all formally hired workers on average. I use this variation in CBA coverage to construct a pre-policy measure of each market's exposure to effective unions.

I define treated labor markets as those in the top quartile of pre-reform union effectiveness and use the remaining markets as the comparison group. Because high-exposure markets may have been on different employment trajectories even absent the reform, I use a Synthetic Difference-in-Differences (SDiD) estimator (Arkhangelsky et al., 2021), which reweights comparison markets to match pre-reform trends in treated markets. Identification rests on the assumption that, absent the reform, outcomes in the treated and synthetic control markets would have evolved similarly. I discuss the tradeoffs associated with this discretization and shows that the results are qualitatively unchanged across alternative thresholds and DiD specifications.

I combine multiple administrative and survey datasets covering Brazil's labor market from 2012 to 2021 to study the reform's effects. My main source is the *Relação Anual de Informações Sociais* (RAIS), which records the universe of formal employment, aggregated to the microregion level (a standard definition of local labor markets in Brazil).[3] To measure informality, I use the *Pesquisa Nacional por Amostra de Domicílios Contínua* (PNADC), a nationally representative household survey and the primary source of data on informality outside of the decennial Census. I complement these sources with administrative data on labor inspections, union activities and collective bargaining agreements, which I scrape from the register of all agreements (*Sistema Mediador*) to construct measures of union effectiveness by microregion. The analysis is restricted to urban labor markets, and to private sector employers and employees.[4]

I find that weakening (initially strong) unions led to lower formal wages and greater wage inequality. Formal workers' real monthly earnings declined by about 0.9% on average in labor markets with more effective unions, relative to labor markets with weaker unions. These effects appeared gradually, as many workers remained covered by collective bargaining agreements. To interpret the magnitude of this effect, consider that in the pre-period the difference in CBA coverage of workers between high and low-effectiveness labor markets was approximately 26%. A simple

[3] see Ponczek and Ulyssea (2022) and Dix-Carneiro and Kovak (2017)

[4] Rural areas are excluded because they are predominantly informal and have limited union activity and data coverage. Public-sector employers follow a separate bargaining structure and rarely hire informally.

linear extrapolation would suggest that a labor market with full CBA coverage could experience a wage decline on the order of 3.5% relative to a market with no coverage.[5] The wage-skill premium increased, indicating that unions had previously helped compress the wage distribution for formally contracted workers.

Turning to employment, I find that contrary to the reform's stated objective, weakening strong unions decreased formal employment. Formal wage employment fell by about 2.5% after the reform. The decline appeared soon after the policy took effect and grew over time, driven mainly by a reduction in new hiring rather than layoffs. At the same time, informal wage employment rose by roughly 2.4%, although these estimates are noisier because informality is measured at a more aggregated geographic level. I find no detectable effects on the level of overall wage employment as a result of the policy. These patterns indicate that firms substituted informal jobs for formal ones as unions weakened.

The key mechanism explaining these results is a change in labor inspection patterns following the weakening of unions. The reform did not affect inspection capacity or resources, and I find no effect on the total number of audits conducted. Instead, the reform altered where inspections occur. Before 2017, union staff played a key role in helping inspection officers identify and penalize firms that hired workers informally. As unions lost resources, they became less able to participate in inspection planning. In the absence of union guidance, inspections became more spatially constrained. Inspectors typically drive from their local offices to conduct audits (Almeida and Carneiro, 2012), so distance to an office shapes inspection activity. Consistent with this institutional feature, I find that after the reform inspections became increasingly concentrated in markets closer to inspection offices and declined in more distant areas. When unions weakened, enforcement became more distance-dependent.

Employment responses to weakened unions mirror these changes to audit activities. Declines in formal employment and increases in informal employment are concentrated in labor markets farther from inspection offices, where enforcement weakened most after the reform. These spatial patterns suggest that firms substituted formally hired workers with informal ones when the risk of inspection fell. The substitution is particularly pronounced among low-skill workers, who are easiest to hire informally (Ponczek and Ulyssea, 2022). This pattern aligns with recent evidence from Brazil and Mexico showing that workers on formal and informal contracts within the same firms often perform similar tasks, and that inspections push firms to formalize these jobs (Samaniego de la Parra and Fernández Bujanda, 2024; Brotherhood et al., 2023).

The 2017 reform introduced several other changes to Brazil's labor laws, and I study each component to assess whether they could plausibly explain the results. Besides making union dues optional, the reform expanded outsourcing rules, revised dispute-resolution procedures, and created

[5] High union-effective markets' exposure to CBAs was 0.49, while low-effectiveness market exposure was 0.23. The extrapolation comes from dividing the wage effect size 0.9% by the exposure difference 0.26, yielding 3.46%, though this calculation is intended only to provide scale.

new intermittent and part-time contract types. I show that none of these provisions account for the labor market patterns I observe.

A key alternative explanation is that employers hold monopsony power in local labor markets. Unions may counter this power, so weakening unions could raise firms' monopsony power and reduce both wages and employment. I test this mechanism directly by showing that high-exposure markets were not more concentrated prior to the reform, and that the effects of union weakening do not vary with local labor market concentration. I further account for other pre-existing labor market features that could generate similar outcomes, including differences in sectoral composition and local political shifts. Across all these checks, the results remain stable, indicating that the main channel operates through changes in enforcement following the decline in union power.

These findings have broader implications for labor markets where informality and flexible work are expanding. This paper shows that unions influence not only wages and formal working conditions, but also the enforcement of labor laws that sustain formality. In settings with limited state capacity, enforcement is often incomplete, and informality remains a close substitute for formal jobs. In such environments, unions can play a complementary role by helping steer employment toward formal contracts. Weakening unions may therefore have unintended spillover effects, shifting workers into informal jobs that are associated with worse labor market trajectories and limited upward mobility (Ulyssea et al., 2025).

These mechanisms may also matter beyond low and middle-income economies. Even in high-income countries, unions may be involved with the use of temporary or nonstandard work arrangements (Keune, 2013). To the extent that unions influence how firms substitute across contract types, changes in union power may affect the composition of employment even where informality is less prevalent. Understanding these interactions is essential as countries experiment with new forms of labor regulation and worker protection.

This paper makes three main contributions to the literature. First, it provides new evidence on the role of unions in labor markets with widespread informality. Most existing research on unions focuses on developed, formal economies, but in many developing countries formal and informal work coexist within the same firms. In Brazil, workers often move between informal and formal contracts within these firms (Brotherhood et al., 2023; Ulyssea et al., 2025). In such settings, unions affect not only wages and amenities for formally hired workers (Lagos, 2024) but also firms' ability to hire workers informally by shaping how labor laws are enforced. Unions therefore influence labor markets through two opposing channels: they raise formal wages through collective bargaining but also strengthen enforcement that makes informal hiring costly. Recognizing these institutional roles helps explain the seemingly counterintuitive impacts of weakening unions. This dual role may be overlooked in predominantly formalized labor markets but is central to understanding unions' overall impact in developing economies.

Second, the paper contributes to a longstanding literature on the causal impact of unions on employment and wages. Most existing studies examine expansions in union power at the firm

level, focusing on localized outcomes (Dodini et al., 2023; Barth et al., 2020; Sojourner et al., 2015; DiNardo and Lee, 2004). Closely related work by Lagos (2024) studies the effects of extending collective bargaining coverage in Brazil. I extend this research by incorporating the role of informality and estimating effects at the level of local labor markets rather than individual firms. Union and inspection activities may operate geographically, and their actions can spill over across sectors and firms within the same labor pool. For instance, inspections at one firm may influence the behavior of neighboring firms (Berlinski and Gagete-Miranda, 2024). Studying local labor markets therefore captures both direct and spillover effects of union weakening, providing a more complete view of how unions shape aggregate labor outcomes.

The nature of the reform closely resembles Right-to-Work (RTW) laws in the United States. Like RTW legislation, Brazil's 2017 reform made union dues optional while letting workers be covered under collective bargaining agreements (CBAs). However, the Brazilian context differs in key ways. Aggregate unionization rates remained unchanged and every formal worker continued to be unionized, but unions lost over 92% of their revenue due to free-riding. This creates a distinct setting in which I can isolate the effects of financial weakening from changes in unionization rates. Leveraging spatial variation in pre-reform union strength, I estimate the aggregate impact of reduced union resources on local labor markets. The results parallel recent Right-to-Work findings of wage declines (Fortin et al., 2023; Wexler, 2022; Chava et al., 2020), but differ in their employment implications. These studies find either no significant effect on employment or increases driven by lower labor costs. In contrast, I show that weakening unions shifts employment away from formal hiring (unionized) towards informal hiring (non-unionized) leaving overall wage employment unchanged.

Finally, the paper expands our understanding of unions as part of the institutional fabric that shapes labor markets. Their unique dual role is crucial for explaining why informal hiring rises when unions weaken. This paper extends classic studies of unions as economic agents focused on bargaining and organization (Farber, 2015, 1986; Freeman and Medoff, 1984; Oswald, 1986). I provide a comprehensive overview of union operations, derived from rich datasets documenting union activities and investments. The findings also connect to research on the role of unions in supporting enforcement institutions such as OSHA in the United States (Dean et al., 2023; Johnson, 2020; Morantz, 2013, 2011; Weil, 1999, 2004, 2005).

2 Background

2.1 Unions in Brazil

Every formally hired worker in Brazil is represented by a union. Any collective bargaining agreement (CBA) signed for their sector and location applies to their contract terms. Until 2017, all formal workers paid one day of wages each year as mandatory union dues, known as the union tax, which had been the main source of union financing since the 1940s.

Unions use revenues to maintain offices and hire local staff who organize meetings, negotiate CBAs, and coordinate with labor inspection offices to help enforce labor laws (see Section 2.2). Each municipality can have at most one union per sector (*Unicidade Sindical*). In municipalities without a sector union, worker contributions are transferred to the corresponding sector union in a neighboring municipality, which then represents the worker. For example, if the municipality of Itai lacks a retail workers' union, retail workers' union taxes are remitted to the nearest retail union. Between 2012 and 2021 roughly 12,700 worker unions operated in Brazil. Prior to 2017, each received annual payroll deductions every March from workers in their sector–municipality cell.

Union resources matter for bargaining outcomes. Before 2017, union revenues depended on the size and wage level of the formal workforce they represented. With these resources, unions staffed local offices and negotiated CBAs with firms and employer associations. Table A1 shows that, between 2012 and 2016, a 10% increase in union staff was associated with a 3.9% increase in the number of CBAs signed.

CBAs are unions' main tool for influencing labor market outcomes. They determine workplace conditions and wage floors beyond the regionally set minimum wage. There are two types: Acordos Coletivos (firm-level agreements) and Convenções Coletivas (sectoral agreements). Firm agreements apply to all workers within a single firm, while sectoral agreements apply to workers across multiple firms in the same sector, often spanning several municipalities.[6] Firm agreements account for 82% of all CBAs but cover about 26% of formal workers; sectoral agreements cover 42%.[7] Coverage is not mutually exclusive, as some workers are covered by both. Where both apply, firm agreements prevail, guaranteeing terms beyond those in sectoral CBAs.[8]

CBAs are positively correlated with wages and employment. In 2014, sector–microregion cells with at least one CBA had 6.6% higher monthly wages, 7% higher hourly wages, and 57.8% more employees (Table A2). These correlations are descriptive: higher wages and formal employment likely increased union revenues and enabled more CBA activity, contributing to these these observed associations.

Not all registered unions sign collective bargaining agreements (CBAs). Between 2012 and 2016, only 49% of active unions signed at least one CBA. This variation reflects many different factors. Under the *Unicidade Sindical* rule, unions held a legally protected monopoly within each sector–municipality cell and received revenue from the mandatory union tax regardless of bargaining activity, limiting competitive pressure to negotiate agreements. At the same time, historical studies show that unions with larger memberships, stronger leadership, and traditions

[6] Common clauses include salary adjustments, overtime pay, dismissal procedures, and working-hour compensation; see Lagos (2024) for a detailed analysis of CBA content between 2009–2016.

[7] Based on CBAs signed in 2014.

[8] In 2012, the *Tribunal Superior do Trabalho* introduced the doctrine of ultractivity (*Súmula 277*), allowing workers to remain covered by expired clauses while new CBAs were negotiated. Unions continued to negotiate CBAs every year, with about 80% of CBAs signed for one year, and 20% for up to two years.

of strike mobilization tended to bargain more actively, while some smaller unions operated primarily as service-providing or clientelist organizations rather than bargaining agents (Boito and Steiger, 1994; Cardoso, 2003). These institutional and historical differences generated substantial heterogeneity in bargaining activity across unions even before the 2017 reform.

The share of formally hired workers in a local labor market covered by a CBA serves as a measure of union effectiveness. Figure A1 shows the fraction of formal workers covered by CBAs in each local labor market (microregion). Although unions are based in individual municipalities, their agreements often extend to neighboring areas (usually within the same state) creating substantial spatial variation in coverage across Brazil.

I use this geographic variation in CBA coverage to construct a pre-policy measure of each microregion's exposure to effective unions. This measure correlates with several observable features of local labor markets, including the share of employment in communications, utilities, and manufacturing sectors. Observable characteristics explain roughly half of the cross-region variation (Appendix Table A3). The remaining differences, likely driven by historical organizing capacity, union-center affiliations, and other institutional factors, allows me to show that the the 2017 policy shock had a direct impact on union power that is not fully explained by the fixed characteristics of the microregions where unions were active. I discuss the implications for identification in Section 5.2.

2.2 Labor regulations and enforcement in Brazil

A large part of Brazil's labor force is informally employed. This is in part due to Brazil having among the strictest labor regulation regimes in the developing world (Almeida and Carneiro, 2012). All formally hired workers are entitled to pension, insurance, and severance benefits, but compliance is costly due to high labor taxes and complex procedures (Ponczek and Ulyssea, 2022). As a result, about 31% of all wage workers are hired informally (Census 2010).

Formal firms cannot legally hire workers without contracts. When inspections uncover informal employment, firms must formalize those workers and pay penalties ranging from one to six months of wages per worker in fines and payroll taxes. Such audits have long-term negative effects on firm growth (Brotherhood et al., 2023).

Yet not all firms are inspected. Globally, labor inspectorates often operate under severe resource constraints (Weil, 2008), thus they often prioritize where to intervene. In Brazil, two factors play an important role in shaping where inspections occur: distance from inspection offices and information provided through complaints.

The Ministry of Labor operates in a decentralized fashion within states. Each state has a labor office (*delegacia*), with local offices within subregions (*subdelegacia*). Inspectors located in each *subdelegacia* drive to different firms to carry out these inspections. Driving distances are thus one of main determinants of inspections, and have been shown to influence informal hiring by affecting the probability of a firm being penalized (Almeida and Carneiro (2012)).

Although inspections are intended to be random, in practice they are often complaint-driven. Only formal firms are inspected. Reports can come from individual workers, unions, or other organizations, but union complaints receive priority (Cardoso and Lage, 2005). Audit plans are made in consultation with local committees (*Comissões de Colaboração com a Inspeção do Trabalho*), where union representatives are frequently invited. Owing to limited capacity, distances and reports influence the inspections intensity of a given location.

Unions benefit from effective enforcement because it reduces informal hiring. This strengthens their bargaining position: when firms can easily substitute informal for formal labor, labor demand for unionized workers becomes more elastic, limiting unions' ability to negotiate higher wages or better conditions. Constraining informality also expands the base of formal, dues-paying members. Unions therefore have clear incentives to support enforcement efforts and discourage informal hiring at formal firms.

Active unions are associated with more intensive inspections. Table A4 shows that sector–microregion cells with a CBA in 2014 had higher shares of both workers and firms inspected. Appendix Table A5 confirms a similar pattern when counting all firms and workers visited during the year.

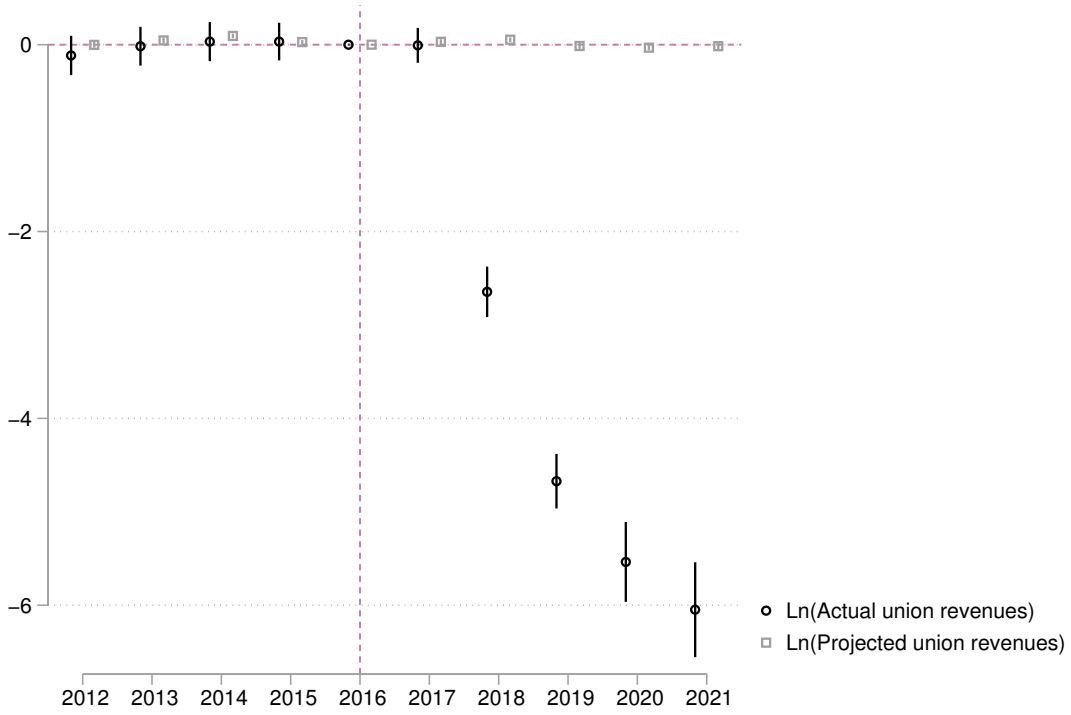
2.3 2017 Regulation

In 2017, Brazil enacted a major labor reform that made mandatory union tax deductions optional: workers now had to opt in to pay the one-day wage contribution. The change was part of a broader package aimed at making labor markets more flexible (see Section 7) and encouraging formal hiring. The proposal was announced in March 2017, debated throughout the year, and passed in November. Workers and unions, who did not anticipate the measure, staged large protests when it was first announced. Due to the timing of the announcement, I consider 2016 as the last pre-period year. Given that the union tax payroll-deductions for the year had already been made in March, revenues in this year were unaffected.

After 2017, all formally employed workers remained covered by collective bargaining agreements (CBAs) regardless of whether they contributed to unions. However, the reform revoked the ultractivity clause established by *Súmula 277* in 2012, meaning that once a CBA expired, workers lost coverage until a new agreement was signed.

The financial shock to unions was immediate and severe. Union revenues fell by about 93% in 2018 and continued to decline as unions exited over time. Figure 1 plots actual versus projected logged revenues from 2012–2021 relative to 2016, showing that the decline cannot be explained by changes in formal employment or wages.

Figure 1: Actual and Projected union revenues



Notes: This graph plots logged union revenues relative to 2016 summed up to the State by Year level. Each regression includes State fixed effects, with robust standard errors. The grey series plots revenues from administrative data on formal employment and wages. I calculate what revenues would look like over time if the policy did not make paying takes optional by computing taxes as a day of each worker's wages. The black series plots actual revenue data from administrative revenue sources. The gap in the grey and black series emerges in 2018, which is the first year the policy of optional union taxes binds.

The revenue data do not identify individual unions, but the magnitude of the collapse implies that most workers chose to opt out of paying. Whether these losses fell primarily on inactive unions or disrupted the activities of active ones is an open question. If workers had simply withdrawn support from inactive unions, contributions to active unions and their operations should have remained stable. The sharp declines in union activity documented later suggest otherwise: the reform weakened the functioning of even the active unions.[9]

2.4 How do unions respond to a decline in revenues?

> "The 2017 reform, in addition to disfiguring the CLT, abruptly took away the oxygen coming from the union contribution of the union movement in general and of commercial workers in particular. This momentarily reduced our ability to meet workers' demands. We lost members and stopped providing certain services."

[9]While some workers may have chosen to free-ride on union benefits, others may have perceived unions as unproductive (49% of unions sign any CBAs between 2012-16). If the latter were true, we would expect continued support for active unions and little change in their behavior, which is not what we observe.

– Ricardo Patah, President of the São Paulo Commerce Workers' Union and President of the General Workers' Union (UGT), as quoted in BBC News Brasil, May 1, 2025.

Unions responded to the policy almost immediately. Figures A2 and A3 show that, relative to 2016, unions lost 22% of their staff and number of unions negotiating CBAs drops by 30% between 2017 and 2021.

Consequently, number of collective bargaining agreements (CBAs) signed also fell sharply. Figure A4 plots this trend over time, after 2017, total CBAs declined by about 20%. Firm-level agreements dropped by 18%, and sectoral agreements by 10%. The contraction in both the number of CBAs and the unions engaged in bargaining shows that the reform substantially weakened union activity by cutting off their main source of revenue.

With unions weakened, how do inspections change? Inspections are shaped by both distance and reports from unions. If unions are weakened, inspectors may focus more on firms closer to their offices, strengthening the negative relationship between distance and inspection frequency. Table A6 confirms this pattern: after the reform, the share of firms and workers inspected declined in microregions farther from inspection offices relative to pre-reform years.

3 Conceptual Framework

Simple model: I begin with a simple model of unions to set ideas, where unions and firms decide on wages, and then firms decide employment. This is represented in Figure A5. In a market without unions, firms hire Q_e workers at wage W_e. Unions set wages above W_e; when unions act as a monopolist, they unilaterally set wages at W_u, and firms respond by hiring fewer workers Q_u (union as a monopolist model (Dunlop, 1944)). The wedge between W_u and W_e tells us about union barganing power (denoted by β). When unions bargain with firms, the wage lies between the competitive wage W_e and the union monopoly wage W_u ('right-to-manage' model (Leontief, 1946)). The essential takeaway from this model is that union power affects the wages and subsequent level of employment of workers, in a simple setting with one type of worker.[10]

In this representation of the labor market, as unions weaken, wages decline and employment expands. This model is helpful when thinking about benchmark predictions for the effects on unions on workers managed by them (unionized workers). I expand this model to include unionized and non-unionized workers. The goal from this exercise is to develop intuition for the effects of weakening unions on unionized (formal) workers, and non-unionized (informal) workers.

Setting: I study the firm's hiring problem by writing a simple model of firm-level union bargaining. I focus on formal firms in the private sector. The firm can hire two types of workers: unionized and non-unionized. Non-unionized workers are informally hired workers. (Recall, all formally hired workers are unionized). Let U and I be the quantities of unionized (formally hired)

[10]This model also assumes a perfectly competitive labor market, and sets up the union's objective as solely maximizing wages, when in practice they may also bargain over employment levels. I discuss the former in my discussion of empirical results, the latter yields qualitatively similar results in this simple model as long as $W_u > W_e$.

and informally hired workers respectively. I assume that formally and informally hired workers are substitutes in the production process.[11]

Wages for union workers are set through a bargaining process, whereas wages for informally hired workers are set through a competitive equilibrium. In general, hiring workers informally is cheaper. However, doing so runs the risk of getting caught by authorities and paying a penalty.

The firm hiring decision and union power: The firm's profit maximization problem is given by:

$$\max_{U,I} \Pi \equiv p(U+I)^{\gamma} - tU - \Big(w(I) + \chi(I)\Big)I \tag{1}$$

where p is the price per unit of output; t is the wage for unionized workers; $w(I)$ is the wage for informal workers; and $\chi(I)$ is the expected penalty per informally hired worker. Both $w(I)$ and $\chi(I)$ are increasing in I for reasons I detail below.

To help ensure I obtain interior solutions, I impose a few assumptions on the parameters. First, I assume that $\gamma \in (0,1)$, i.e. the production process exhibits diminishing returns to scale. Second, I assume that the bargained wage t is sufficiently high that it is profitable to hire at least some workers informally.

Wages for informal workers are governed by the following inverse labor supply function:

$$w = I^{\epsilon} \tag{2}$$

where $\epsilon > 0$ denotes the elasticity of labor supply.

Union bargaining power enters into the firm hiring decision in two ways. First, unions bargain with firms to set formal wages. As union bargaining power increases, the bargained wage also increases. I assume that the union wage t is a linear function of union bargaining power $\beta \in [0,1]$ as follows:

$$t = t_0 + \lambda\beta \tag{3}$$

where t_0 is the wage a firm would have liked to pay a worker in the absence of unions, and λ tells us about the wage component coming from union bargaining.[12] Second, union bargaining power increases the probability that a firm will be inspected, conditional on its distance from the inspection office. This increases the expected cost to firms of hiring workers informally. For

[11] This assumption is reasonable in this setting. Brotherhood et al. (2023) show that, within formal firms in Brazil, formally and informally hired workers have nearly identical demographic characteristics. Ulyssea (2018) further establishes that, in this setting, formal and informally hired workers perform similar tasks within the same firm.

[12] This linear functional form can be motivated by a Nash bargaining framework where firms and unions bargain to reach wage $t = \beta t_u + (1-\beta)t_0$, where t_u is the optimal wages from the union's perspective, and λ is thus the difference between the two wages $t_u - t_0$. See Dodini et al. (2023) for a more extensive discussion.

tractability, I assume that the expected penalty has the following linear form:

$$\begin{aligned} \chi &= q(\beta, d, I) \cdot \delta \\ &= kI \end{aligned} \tag{4}$$

The expected penalty χ is the product of the probability of detection (q) and the penalty amount (δ). The probability of detection in turn depends on union bargaining power, the number of informally hired workers, and the distance to the inspection office (d). I assume that $q = \frac{\beta I}{dI_{max}}$, where I_{max} is the maximum possible value of I. This functional form helps ensure that q remains a valid probability with values between 0 and 1. k is a simplification of these terms.[13]

The probability of inspection is increasing in the number of informally hired workers at the firm because the more workers a formal firm hires informally, the more visible to labor inspectors. When unions are more powerful, they are able to participate more in the inspection planning process, which also increases a firm's probability of inspection. Finally, distance to an inspection office matters for a firm's probability of inspection; the further away a firm is from an inspection office, the less likely they are to be inspected.

Proposition 1. *The optimal quantities of informally and formally hired workers (I^* and U^*, respectively) are given by:*

$$I^* = \frac{1}{2k}\Big[t - (\epsilon + 1)w(I^*)\Big] \tag{5a}$$

$$U^* = \left(\frac{\gamma p}{t}\right)^{\frac{1}{1-\gamma}} - I^* \tag{5b}$$

Proof. See Appendix C □

Notice here that because informally hired workers are cheaper, and formally and informally hired workers are substitutes, firms first choose their optimal quantity of informal contracts, and then select the number of workers they will hire on formal contracts. Summing U^* and I^* shows that total employment equals $\left(\frac{\gamma p}{t}\right)^{\frac{1}{1-\gamma}}$, implying that a decrease in the bargained wage t may raise overall employment in the firm.

Union power weakens as a result of the 2017 labor market regulation in Brazil. Next, I examine the effect this has on equilibrium wages and employment.

Proposition 2. *When union power β decreases, the effect on the number of formally and informally hired workers is ambiguous. In particular:*

$$\frac{\partial I^*}{\partial \beta} = \begin{cases} > 0 & \text{if } t'(\beta) > 2\chi'(\beta) \\ < 0 & \text{if } t'(\beta) < 2\chi'(\beta) \end{cases} \tag{6}$$

[13] $k = \frac{\beta\delta}{dI_{max}}$

$$\frac{\partial U^*}{\partial \beta} = \begin{cases} > 0 & \text{if } \frac{-\lambda}{(1-\gamma)\gamma p}\left(\frac{\gamma p}{t}\right)^{\frac{2-\gamma}{1-\gamma}} > I'^*(\beta) \\ < 0 & \text{if } \frac{-\lambda}{(1-\gamma)\gamma p}\left(\frac{\gamma p}{t}\right)^{\frac{2-\gamma}{1-\gamma}} < I'^*(\beta) \end{cases} \tag{7}$$

Proof. See Appendix Section C □

As unions weaken, the change in informal hiring is ambiguous. From equation 6 we see that the direction of the effect depends on whether union power has a larger effect on formal wages or the expected penalty associated with informal workers. If union bargaining power has a larger effect on the expected penalty, then as union power weakens, informal hiring might *increase*. The expected penalty also depends on distance, thus the magnitude of this response is also shaped by proximity to enforcement offices.

To understand the subsequent effect on the quantity of formally hired workers, notice in equation 7 that the LHS term of the inequality is always negative. When union power decreases, for the number of formally hired workers to decrease (condition 1), the LHS term of the inequality must be larger than $I'^*(\beta)$. This implies that $I'^*(\beta)$ must also be negative, which is the case in equation 6 when informal hiring increases as unions weaken (condition 2).

In sum, if weakening unions makes formally hired workers cheaper relative to informally hired workers by affecting the bargained wage more than the penalty, more workers are formally hired (and vice-versa).

Proposition 3. *When union power β decreases, formal wages decline, and the impact on informal wages is ambiguous:*

$$\frac{\partial t}{\partial \beta} = \lambda \tag{8}$$

$$\frac{\partial w(I)}{\partial \beta} = \begin{cases} > 0 & \text{if } I'^*(\beta) > 0 \\ < 0 & \text{if } I'^*(\beta) < 0 \end{cases} \tag{9}$$

Proof. See Appendix C □

As unions weaken, the formal wage will decline, as it is set weakly above the market wage by unions. For informally hired workers, wages in equilibrium are affected by changes in demand. If more informally hired workers are demanded by firms as unions weaken, their wage will weakly increase as firms move up to a higher point on the labor supply curve for informally hired workers.

Summary of predictions: When unions weaken, the effects on wages and employment are ambiguous. This is owing to the effects of union power on the prices of hiring workers formally and the cost of hiring workers informally:

1. $\Delta t < 0$: formal wages decline

2. $\Delta \chi < 0$: the expected penalty from inspections declines (conditional on distance)

3. $\Delta w > 0$ if the number of informally hired workers increases as a result of unions power weakening, $\Delta w < 0$ otherwise

4. $\Delta I^* > 0$ if informally hired workers are made cheaper than formally hired owing to reduced expected penalty when unions weaken, $\Delta I^* < 0$ otherwise

5. $\Delta U^* > 0$ when $\Delta I^* < 0$, and when $\Delta I^* > 0$ the effect on U^* depends on the relative strength of the scale effect from lower formal wages and the substitution toward informal hiring

The ambiguity in the effect on formally hired workers is starkly different from the predicted effects on unionized worker employment under a standard model. This highlights the importance of modeling the effect unions have on informal employment in this setting.

4 Data

I combine multiple administrative and survey datasets to examine how the 2017 reform affected Brazil's labor markets. The analysis focuses on urban areas, where both formal and informal employment are common and unions are most active. Data on rural unions are limited, and including them would confound variation in union power with broader structural differences in rural-urban labor markets. I also restrict the sample to private sector firms and wage employees, as public sector bargaining follows separate rules and public employers rarely hire informally.

4.1 Union data

I compile detailed information on union structures, revenues, and collective bargaining activity from three administrative sources. The ***Cadastro Nacional de Entidades Sindicais (CNES)*** provides data on all registered unions, including their location and years of activity, which I use to build a panel of active unions over time. Revenue data come from CAIXA's ***Contribuição Sindical Urbana*** (Urban Union Contribution) system, which records all mandatory payroll deductions remitted from firms on behalf of employees. Information on collective bargaining agreements (CBAs) comes from the ***Sistema Mediador***, which maintains the universe of registered agreements since 2009. I scraped the full database as of January 2024 and restrict the sample to CBAs valid between 2012 and 2021.

4.2 Inspections data

To measure proximity to enforcement offices, I use location data from the Ministry of Labor's public registry of local offices. Each state has a regional labor office (*delegacia*), and within states, subregional offices (*subdelegacia*) serve as the centers of local inspection activity. For each (*subdelegacia*), the registry reports the municipality and postal code. I calculate the distance between each municipality in Brazil and all subdelegacias, and assign to each municipality the minimum distance to obtain a measure of proximity to the nearest inspection office. I later aggregate these to proximity measures at the microregion level, following Ponczek and Ulyssea (2022).

I use administrative data from the Ministry of Labor's enforcement records to measure enforcement intensity. These firm-level data span 1995–2024 and report the number of inspections conducted. I aggregate these data to the local labor market level to construct an annual panel of inspection activity.

4.3 Labor market data

To measure formal and informal employment outcomes across local labor markets, I use the following two datasets:

Relação Anual de Informações Sociais (RAIS) RAIS reports the universe of formal employment in Brazil, capturing all employer–employee relationships as of December each year. I use a de-identified version that omits firm and worker identifiers. Each record includes an employee's status on December 31 (hired or terminated), monthly wage, and demographic characteristics. These data allow me to track formal wage employment and earnings over time at the microregion level.

Pesquisa Nacional por Amostra de Domicílios Contínua (PNADC) To measure informal wage employment, I use the Continuous National Household Sample Survey (PNADC), a nationally representative quarterly survey conducted by Brazil's statistical agency. To align with RAIS, I use only fourth-quarter waves from 2012–2021. The survey records individuals' demographic characteristics, employment status, wages, and sectors of work. For wage workers, it records whether they possess a signed work card (*Carteira de Trabalho*) from their employer—a standard measure of informality in Brazil. While these data are representative and detailed, they do not include fine-grained geographic identifiers, so I aggregate them to a larger spatial unit than the microregion.[14]

4.4 Unit of analysis

The analysis combines data sources that differ in spatial coverage and granularity, so I construct yearly panels of spatial units. The level of aggregation varies by dataset and depends on the available identifiers.

Spatial units for administrative data: Following recent work on labor markets in Brazil[15], I aggregate the 5,570 municipalities into microregions as defined by the Brazilian Statistical Agency (IBGE). Microregions group municipalities with similar geographic and economic characteristics and correspond roughly to commuting zones or local labor markets.[16]

Spatial units for household survey data: The household survey data do not include geographic identifiers for microregion. They do contain identifiers for 'strata', which are

[14]The decennial Census provides municipality-level data on informality but is not suitable for annual analysis. RAIS covers only formally registered employment, omitting informal workers employed at formal firms (Ulyssea et al., 2025).

[15]Ponczek and Ulyssea (2022) and Dix-Carneiro and Kovak (2017)

[16]IBGE replaced microregions with 'Immediate Regions' in 2017. I use this updated classification but refer to these as microregions for consistency with prior literature.

groupings of municipalities carried out by the IBGE for the purposes of this survey. I aggregate observations to the strata level, yielding 146 unique spatial units across 27 states. I refer to these as 'macroregions' throughout the paper.

5 Empirical strategy

To identify the effects of weakening unions on labor markets, I construct pre-reform measures of local exposure to union strength and estimate their impact using a difference-in-differences (DiD) design. The assumption is that labor markets with stronger unions before the reform were more affected when mandatory union contributions were abolished in 2017.

5.1 Union exposure

I proxy union effectiveness using collective bargaining agreements (CBAs), assuming that areas where a larger share of formally employed workers are covered by CBAs have stronger and more active unions. As discussed in Section 2, both the number of CBAs and their coverage vary across space. For each spatial unit, microregions for formal sector data and macroregions for household survey data, I compute the share of formally hired workers covered by at least one CBA in 2014, the midpoint of the pre-reform period:

For a given spatial unit:

$$H_i = \frac{\text{Number workers in sectors or firms signing CBAs in unit } i}{\text{Number formal workers in unit } i} \tag{10}$$

The resulting H_i is a continuous measure of pre-policy union effectiveness for each spatial unit.

Ideally, the data would include labor markets with no CBA coverage (H_i=0) and others with extensive coverage (H_i is large)), creating a clear contrast between unexposed and highly exposed areas. In practice, all labor markets exhibit some degree of union coverage (Figure B5). To construct a comparison that isolates markets with meaningfully different levels of exposure, I discretize H_i into a binary treatment indicator T_i. This approach focuses the analysis on differences between markets at the upper tail of the exposure distribution and those with lower exposure:

$$T_i = \begin{cases} 1, & \text{if } H_i \geq P75(H_i) \\ 0, & \text{otherwise} \end{cases} \tag{11}$$

This definition classifies 111 microregions as treated in the RAIS data and 37 macroregions in the PNADC data.

To strike a balance between identifying high-exposure areas and retaining sufficient variation within the treatment group, I fix the discretization threshold at the 75th percentile. Using the median as a cutoff would classify as treated many markets with relatively low union coverage: at the median, only 28% of formal workers are covered by unions. At the other extreme, defining treatment as coverage of 50% or higher corresponds to the 90th percentile of the H_i distribution,

leaving little variation in the data.[17] In Section 7.3, I assess the sensitivity of the results to alternative cutoff choices by re-estimating the model using a range of treatment thresholds.

5.2 Synthetic Difference-in-Differences

After defining the treated units, the next step is to find the right counterfactual group. Ex ante, given the observed and unobserved differences across local labor markets that shape exposure to effective unions (as discussed in Section 2.1), this choice is not obvious. Following Arkhangelsky et al. (2021), I take a data-driven approach to identifying the counterfactual group.

I use a Synthetic Difference-in-Differences (SDiD) estimator to construct the comparison group for treated units (T_i) in both datasets. The estimator uses all available pre-policy information to construct a data-driven counterfactual group. It re-weights units and years in the pre-policy period so that outcome trends in the comparison group match those of the treated group as closely as possible. Like the synthetic control method, it assigns positive weights to potential counterfactual units that sum to one. It extends that approach by also weighting pre-treatment years, giving more importance to years closer to the reform. In this way, SDiD combines features of the synthetic control and traditional DiD estimators: the former assigns unit weights only, while the latter weights all units and years equally. Once determined, the SDiD unit weights are applied throughout the analysis, with each post-treatment year receiving equal weight. This strategy is relatively new but has been successfully applied in recent work (see Abman and Lundberg (2024); Brown et al. (2024); Mangal (2024); Cerqua et al. (2023); Debnath et al. (2023) among others).

I use the SDiD strategy to estimate the effect of the reform on local labor markets. I compare labor markets which were the most exposed to union bargaining to a weighted average of those that were less exposed, before and after the policy was implemented. Identification relies on the parallel trends assumption: without the reform, both groups of markets would have followed similar paths in labor market outcomes.

I estimate the following specification for the main outcomes:

$$Y_{it} = \sum_{t=2012}^{2015} \beta_t \times T_i + \sum_{t=2017}^{2021} \beta_t \times T_i + \gamma_t + \delta_i + \epsilon_{it} \tag{12}$$

where Y_{it} is the outcome for spatial unit i at year t. I report coefficients for the high-exposure group, plotting the estimated β_t values over time in the main figures. The specification includes year fixed effects γ_t to capture time trends, and spatial unit fixed effects δ_i to account for time-invariant differences across markets. Following Arkhangelsky et al. (2021), I compute bootstrapped standard errors clustered at the spatial-unit level.

[17] The distribution of the continuous treatment variable at the macroregion level looks very similar; see Appendix Figure B6.

I estimate the average impact using the following specification:

$$Y_{it} = \theta_3 Post_t \times T_i + \gamma_t + \delta_i + \epsilon_{it} \tag{13}$$

where θ_3 is the main coefficient of interest. This specification includes the same fixed effects as specification 12.

5.2.1 Identification

Validity of the chosen comparison group: Identification relies on the parallel-trends assumption. The SDiD estimator is designed to ensure that pre-policy trends in treated and comparison markets align as closely as possible. I verify this visually by plotting event-study coefficients for all main outcomes. If the comparison group is valid, the pre-policy coefficients (β_t) should be close to zero. In Section 7.3, I present additional specifications and show that results are robust to alternative treatment definitions and estimators. In particular, I re-estimate the effects using a conventional TWFE design with a non-synthetic control group, re-estimate the model using the continuous exposure measure, vary the threshold defining high union exposure, and test sensitivity to dropping individual years from the sample.

Correlated changes: Other features of the labor market could produce similar patterns in the data, including imperfect competition, sector-specific shocks, and local elections. The 2017 labor reform also introduced additional provisions that might confound the estimated effects. I examine each of these potential channels in Sections 7.1 and 7.2 and show that they do not explain the observed results.

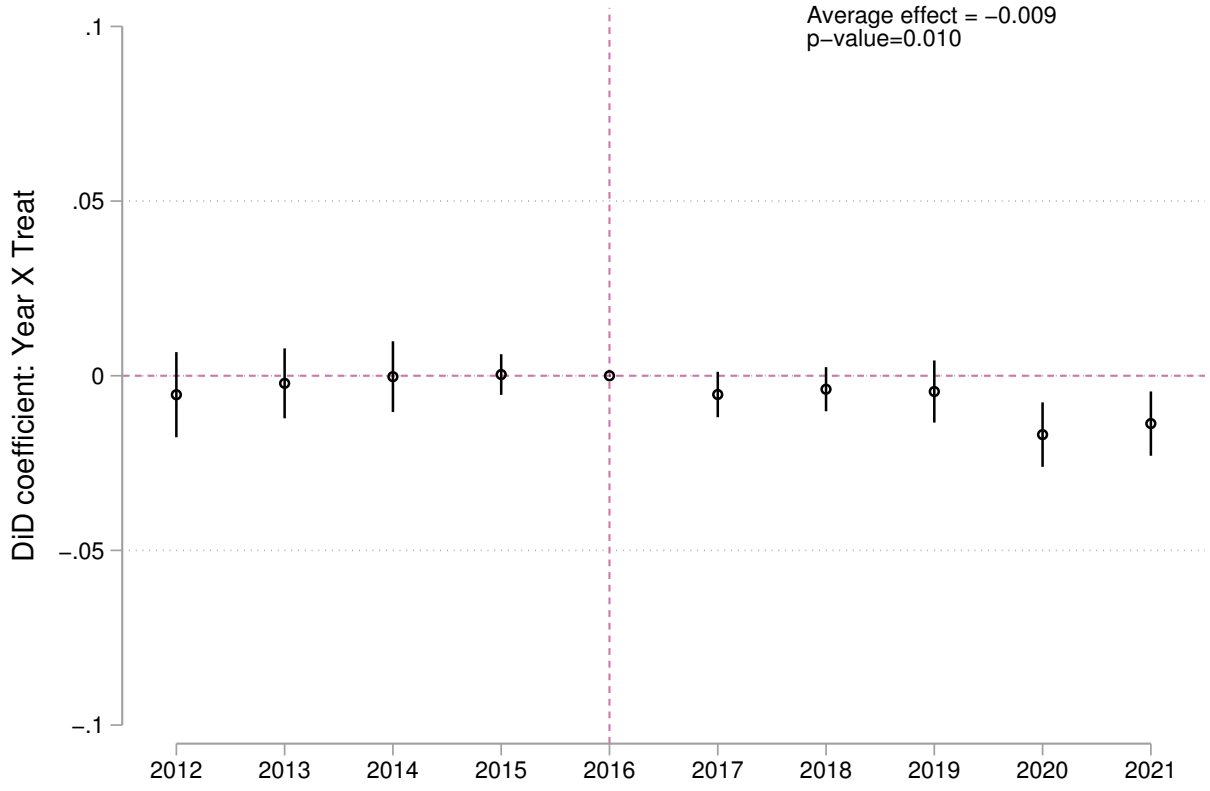
6 Results

6.1 Impacts on wages and employment:

I find evidence that weakening unions led to a decline in wages for formal workers. Following the 2017 reform, monthly real wages for formal workers fell by 0.9% in areas with ex ante high CBA coverage, relative to areas with low coverage. A simple calculation shows that workers who opted out of paying the union tax experienced an average gain of 0.27% in annual income, smaller than the decline in their monthly wages.[18] The law was implemented in late 2017, after unions had already collected that year's dues and signed CBAs earlier in the year. Because most CBAs last one to two years, their clauses remained in effect for some time. The decline in wages appears in 2020 (Figure 2), as these agreements expired and unions' resources diminished.

[18] The union tax was equivalent to one day of wages per year. One day corresponds mechanically to approximately 1/365 of annual earnings, or about 0.27%. Expressed in monthly terms, this is roughly $0.27/12 \approx 0.02\%$ per month.

Figure 2: Effect of the policy on the logged monthly wages for formally hired workers



Notes: This figure plots DiD coefficients over time for effects of the policy on average monthly wages for formally hired workers. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

A large body of research shows that unions may help compress the wage distribution (see Jäger et al. (2025) for a comparative review). Following Farber et al. (2021), I construct measures of wage inequality to assess the distributional impacts of the reform. Table A7 shows that the skill premium in wages increased after unions weakened, while effects on other inequality measures are muted. Appendix Table A8 unpacks this result and shows that the rise in the skill premium is driven by a 1.1% decline in wages for low-skill workers and a 1.4% increase for high-skill workers, though the latter estimate is noisier.[19]

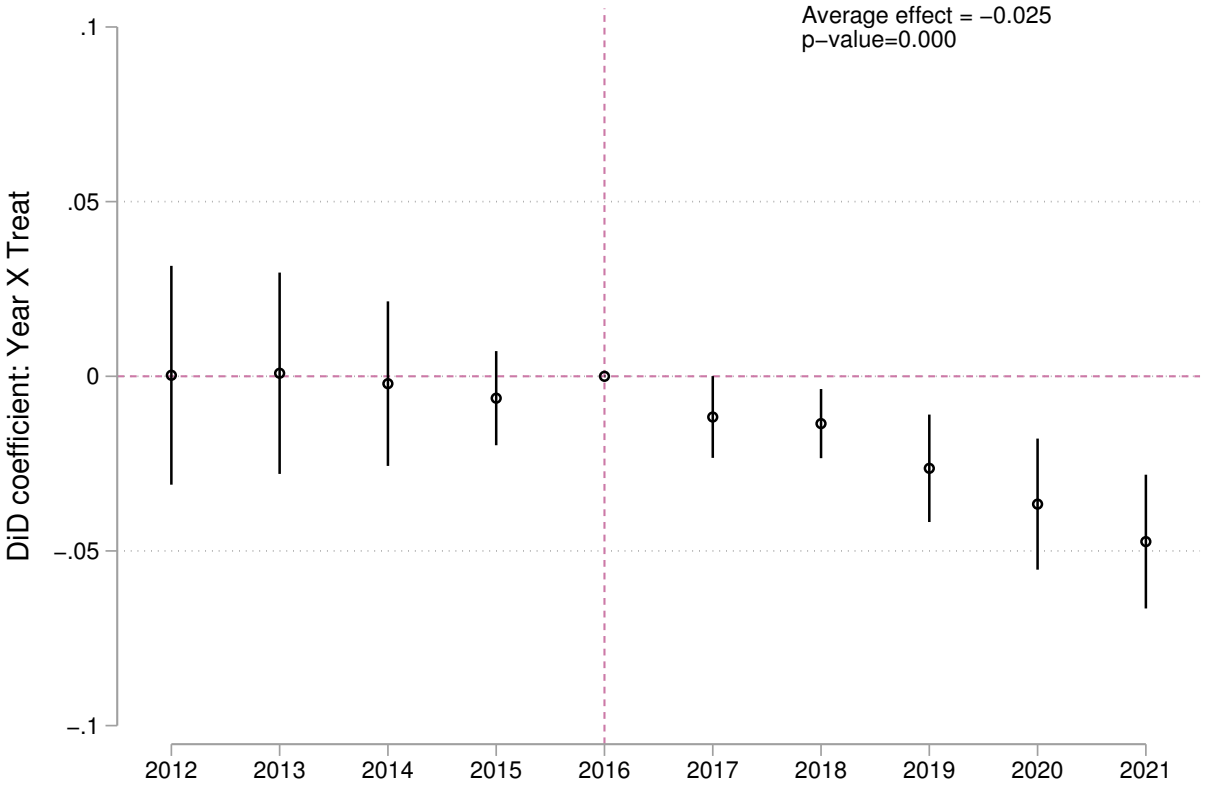
These patterns are consistent with the idea that unions help prop up the lower end of the wage distribution. When unions weaken, workers must negotiate directly with employers. High-skill workers may secure higher wages through individual bargaining, while low-skill workers, who previously benefited from union bargaining that raised the wage floor, experience declines in wages.

Next, I examine the effects on employment levels. Contrary to the goals of the policy, and to

[19]Following the literature on Brazilian labor markets, I define low-skill workers as those with less than a high school degree and high-skill workers as those with a college degree or higher.

predictions from standard models of unions, I find that weakening unions led to a 2.5% decline in formal employment in the years after the reform (Figure 3). The decline grows over time, mirroring the pattern of revenue loss and union exit shown in Figure A3.

Figure 3: Effect of the policy on the logged number of formally hired workers



Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of formally hired workers every year. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Table 1: Average effect of the policy on the wages and stock of formally employed workers

	(1) Logged average monthly wages	(2) Logged number formally employed workers
θ_3: Post $\times$ T_i	-0.009**	-0.025***
	[0.004]	[0.007]
Mean Control-Pre Raw Outcome	1,291	174,119
Treatment exposure to CBAs	0.49	0.49
Control exposure to CBAs	0.23	0.23
Observations	4,440	4,440

Notes: This table shows average effect (θ_3 in equation 13) of the policy on the monthly wages and stock of formally employed workers (logged). Post refers to 2017, when the policy takes effect. Observations are at the Microregion by Year level, and include microregion and year fixed effects. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

To help interpret the magnitudes of these effects, each results table reports the average CBA coverage in treated markets and in their (weighted) synthetic counterparts. For example, in Table 1, local labor markets where about half of formal workers are covered by CBAs experience a 0.9% decline in wages and a 2.5% decline in formal employment relative to markets where about one-quarter of workers are covered. A back-of-the-envelope scaling by the 26 p.p coverage difference implies an effect of roughly 3.5% on the wages of a worker covered by a CBA. This magnitude is comparable to the wage response reported by Derenoncourt et al. (2025), who find a 3.8% post-reform wage decline in high versus low union-premia markets.

In this setting, where formally and informally hired workers are substitutes in the production process, the model predicts that declines in formal employment may be met by increases in informal contracting. I examine this in Table 2 using household survey data. These data are aggregated to a larger spatial unit than the administrative data and are self-reported, making the estimates noisier. Even so, the main patterns are consistent across datasets. Column 1 shows a noisy decline in total wage employment; when decomposed by employment type, the composition shifts. The stock of formal employment falls by 5.3% (similar to the pattern observed in the administrative data) while informal wage employment rises by about 2.4%, though this estimate is less precise. I find no significant effects on wages at the macroregion level using the household survey data, however, patterns are consistent with the model's predictions: formal wages decline slightly, while wages for informally hired workers increase (Appendix Table A9).

To further assess comparability across datasets, I aggregate the microregion-level administrative data to the coarser macroregion level used in the household survey. Appendix Table A10 shows that the negative effects on formal employment remain similar in magnitude, while the wage effects

are also negative but become statistically insignificant at this level of aggregation.

Table 2: Average effect of the policy on overall, formal and informal wage employment

	(1) Logged number wage workers	(2) Logged number formally employed wage workers	(3) Logged number informally employed wage workers
θ_3: Post $\times$ T_i	-0.033	-0.053**	0.024
	[0.022]	[0.022]	[0.034]
Mean Control-Pre Raw Outcome	435,389	364,611	72,552
Treatment exposure to CBAs	0.51	0.51	0.51
Control exposure to CBAs	0.24	0.24	0.24
Observations	1,460	1,460	1,460

Notes: This table shows average effect (θ_3 in equation 13) of the policy on employment outcomes captured in the household survey data (PNADC). The outcome in column 1 is the logged total number of wage workers, column 2 is the logged number of formally hired wage workers, and column 3 is the logged number of informally hired wage workers. Post refers to 2017, when the policy takes effect. Observations are at the Macroregion by Year level, and include macroregion and year fixed effects. Bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

To better understand the employment effects, I draw on the *Cadastro Geral de Empregados e Desempregados* (CAGED) data, which capture labor market flows. Appendix Figures B3 and B4 show a significant decline in the number of new hires as a result of the reform, but no significant impact on separations, explaining the overall decline in formal employment. With unions weakened, new demand for labor in high exposure markets may have been met instead through informal hiring.

6.2 Mechanisms: Unions and enforcement capacity

As discussed in Section 2.2, enforcement of labor regulations in Brazil is incomplete: inspectors face limited capacity and must prioritize where to intervene. Audits require travel from local offices, enforcement is thus naturally skewed toward locations near inspection offices absent strong external information. Unions have strong incentives to help facilitate inspections of informally hired workers. When firms can easily hire informal, non-unionized workers, the demand for formally hired labor becomes more elastic, weakening unions' bargaining power.

The policy itself did not target enforcement capacity, so the overall number of inspections conducted should not change. Column 1 in Table 3 confirms this: the policy had no effect on the quantity of inspections, measured as the fraction of unique firms inspected in a microregion-year.[20] But if unions previously helped direct enforcement away from the default distance gradient, weakening unions should make enforcement intensity more distance-dependent, shifting inspections toward markets close to offices and away from more distant markets.

[20]Similarly, I find no effect on the total number of firms inspected.

I test this prediction in column 2, where I interact θ_3 from equation 13 with an indicator for a microregion's distance from the nearest inspection office (*Far from LO*). The results are striking; among labor markets that were previously most exposed to effective unions, the share of firms inspected increases by about 15% in areas closest to inspection offices but declines by about 23% in more distant markets.[21]

Table 3: Effect of the policy on inspections

	(1) Share of firms inspected	(2) Share of firms inspected
θ_3: Post × T_i	0.001	
	[0.001]	
θ_4: Post × T_i × Close to LO		0.003**
		[0.002]
θ_5: Post × T_i × Far from LO		-0.007**
		[0.003]
Mean Control-Pre Outcome	0.02	
Mean Treat-Close to LO-Pre Outcome		0.02
Mean Treat-Far from LO-Pre Outcome		0.03
Treatment exposure to CBAs	0.49	
Control exposure to CBAs	0	
Observations	4,440	4,440

Notes: This table shows the effect of the policy on fraction of firms inspected in a given microregion. Column 1 presents the average effect (θ_3 in equation 13), while column 2 interacts this coefficient with an indicator for 'Far from a labor office (LO)'. Close to an LO implies that the microregion is below median distance away from the nearest labor inspection office, while Far from LO regions are above median distance away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations are at the Microregion by Year level. Column 1 includes year and microregion fixed effects, while column 2 includes the same fixed effects interacted with an indicator for 'Far from LO'. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

As unions lose resources, they lay off staff and scale back activities within local labor markets. With reduced union involvement, inspections become more distance-dependent: before the reform, union participation helped inspectors identify firms hiring informally regardless of distance, but after the reform, distance plays a larger role in where inspections occur.[22]

[21] I use the distance–enforcement measure defined in Ponczek and Ulyssea (2022) for microregions. Within 111 high exposure microegions, 74 microregions (66%)% are near inspection offices and 33 microregions (34%)% are farther away. In the IBGE classification of macroregion(strata), some macroregions contain municipalities all of which have an inspection office within them. I classify these macroregions as close to inspection offices Within 37 high exposure macroregions, 12 macroregions (32%) are near inspection offices, and 25 macroregions (68%) are farther away.

[22] I rule out the possibility that inspections have reduced in quantity but become more effective. Appendix Table A11 shows that the number of informal workers detected falls in markets further away from inspection offices

If firms respond to lower inspection risk by substituting formally hired workers with informally hired workers, the employment effects of weakening unions should be concentrated in the more distant markets where enforcement fell the most. Indeed, with reduced enforcement in markets farther from inspection offices, employment patterns shift. Table 4 shows treatment effects on the stock of formally and informally hired workers by distance. The decline in formal employment is concentrated in markets more distant from inspection offices, where inspections fell the most. In these same markets, the stock of informal workers rises, though this estimate is less precise.

Table 4: Effect of the policy on the number of formally and informally hired wage workers (by distance to a labor office)

	(1) Logged number formally employed workers	(2) Logged number informally employed wage workers
θ_4: Post $\times$ T_i $\times$ Close to LO	-0.002	-0.063
	[0.008]	[0.047]
θ_5: Post $\times$ T_i $\times$ Far from LO	-0.049***	0.110*
	[0.017]	[0.063]
Mean Treat-Close to LO-Pre Raw Outcome	204,244	103,233
Mean Treat-Far from LO-Pre Raw Outcome	117,112	58,957
Observations	4,440	1,460

Notes: This table shows the effect of the policy on the number of formally and informally hired wage workers interacted with an indicator for 'Far from a labor office (LO)'. In column 1, Close to an LO implies that the microregion is below median distance away from the nearest labor inspection office, while Far from LO regions are above median distance away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations in column 1 are at the Microregion by Year level, this specification includes year and microregion fixed effects interacted with an indicator for 'Far from LO', and bootstrapped SEs clustered at the microregion level in brackets. In column 2, Close to an LO implies that each municipality in the macroregion contains a labor office (labelled as such for clarity, not used as an indicator for the interaction), while Far from LO regions are further away. Observations in column 2 are at the Macroregion by Year level, this specification includes year and macroregion fixed effects interacted with an indicator for 'Far from LO', and bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

These results suggest that firms may be substituting formally hired workers with informally hired workers when the likelihood of inspection falls. To test this mechanism more directly, I follow Ponczek and Ulyssea (2022) and focus on low-skill workers, who are easier to hire informally. Low-skill workers account for over 37% of Brazil's labor force, about one-quarter of formally hired workers, and more than half of informally hired workers.

Table 5 presents the effects of weakened unions on the employment of low-skill workers. Columns 1–2 use administrative data on formally hired workers, and columns 3–4 use house-

(column 2). This could reflect either fewer inspections or less informal hiring. The household survey data results rule out the latter.

hold survey data on informally hired workers.[23] In columns 1 and 3, I find no significant effect of the policy on overall employment levels for formally or informally hired low-skill workers. Once distance to labor offices is taken into account, however, the pattern changes. In column 2, formal employment among low-skill workers falls by 6.2% in markets farther from inspection offices, where enforcement weakened most. This decline is mirrored by a 23.6% increase in informal employment among low-skill wage workers in the same markets. In contrast, in markets closer to inspection offices where inspections intensified, the number of informally hired low-skill workers falls by 16.2%, while formal employment rises slightly but not significantly.

Table 5: Effect of the policy on the number of formally and informally hired low-skill wage workers (by distance to a labor office)

	(1) Logged number formally employed low-skill workers	(2) Logged number formally employed low-skill workers	(3) Logged number informally employed low-skill wage workers	(4) Logged number informally employed low-skill wage workers
θ_3: Post × T_i	-0.014		0.007	
	[0.013]		[0.048]	
θ_4: Post × T_i × Close to LO		0.016		-0.162**
		[0.013]		[0.062]
θ_5: Post × T_i × Far from LO		-0.062**		0.236***
		[0.027]		[0.081]
Mean Control-Pre Raw Outcome	50,441.76		32,614.54	
Mean Treat-Close to LO-Pre Raw Outcome		59,243.71		38,688.91
Mean Treat-Far from LO-Pre Raw Outcome		33,762.24		30,032.26
Treatment exposure to CBAs	0.49		0.51	
Control exposure to CBAs	0		0	
Observations	4,440	4,440	1,460	1,460

Notes: This table shows the effect of the policy on the number of formally and informally hired low-skill wage workers. Low-skill workers as those with less than a high-school degree. Columns 1 and 3 present the average effect (θ_3 in equation 13), while columns 2 and 4 interact this coefficient with an indicator for 'Far from a labor office (LO)'. In column 2, Close to an LO implies that the microregion is below median distance away from the nearest labor inspection office, while Far from LO regions are above median distance away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations in columns 1 and 2 are at the microregion by year level, column 1 includes year and microregion fixed effects, while column 2 includes the same fixed effects interacted with an indicator for 'Far from LO'. In columns 1 and 2, bootstrapped SEs clustered at the microregion level in brackets. In column 4, Close to an LO implies that each municipality in the macroregion contains a labor office (labelled as such for clarity, not used as an indicator for the interaction), while Far from LO regions are further away. Observations in columns 3 and 4 are at the macroregion by year level, column 1 includes year and macroregion fixed effects, while column 2 includes the same fixed effects interacted with an indicator for 'Far from LO'. In columns 3 and 4, bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Taken together, these results present a cohesive picture of how weaker unions shape labor markets. Formal wages fall as unions lose resources and bargaining power, while formal employment declines because enforcement becomes less effective. With inspections reduced, especially in areas farther from inspection offices, the expected penalty for informal hiring falls. Consistent with this mechanism, I find evidence that suggests firms in these areas substitute formally hired workers with informally hired ones.

[23] Appendix Table A12 reports household survey results for formally hired workers. The results show the same patterns as in the administrative data.

7 Ruling out alternate mechanisms

7.1 Labor Reform - Other components

The 2017 labor reform was intended to increase labor market flexibility (Brazil Labor Reform, 2017). Beyond making union contributions optional, it introduced several other provisions aimed at reducing the costs of formal employment. Table 6 summarizes the main changes.

Table 6: Major components of the labor reform

Labor Reform 2017 - Major components		
Component	*Summary*	*Implications*
Expansion of Outsourcing:	The reform removed previous restrictions that limited outsourcing to non-core activities, allowing companies to outsource more broadly	1) Formal workers instead show up as contractors registered as new firms, 2) Increases in informal self-employment (workers hired as self-employed contractors)
Revisions in Dispute Resolution Procedures:	Reform sought to discourage excessive litigation by tightening standards for fees and complaint requirements.	Reduced litigation, firms are more easily able to fire workers, added frictions to grievance redressal for workers
Introduction of Intermittent Work:	Introduction of intermittent and partial contracts (less than full-time, flexible terms decided by employer and employee)	Firms are able to hire workers formally on more flexible contracts

To ensure that my findings are not driven by these concurrent reforms, I examine each major component and assess whether it could plausibly explain the observed declines in formal employment and wages.

7.1.1 Expansion of outsourcing:

The reform lifted restrictions that had previously limited outsourcing to non-core activities, allowing firms to outsource any part of their operations. This change was intended to reduce costs and increase flexibility but raised concerns about job security. I examine two potential channels through which this provision could affect the results.

A) Reduction in formal wage employment: Formal employment could decline if firms respond by hiring workers as contractors (newly registered individual firms) rather than as employees. To test this, I examine changes in the total number of registered firms (Appendix Figure B1) and in the number of firms with zero employees (a proxy for individual contractors; Appendix Figure B2). I find no effect on either outcome.

B) Increases in informal self-employment: The main results report effects on informal wage-employment, which is a substitute for formal wage employment. But if formal firms outsource their activities, they may hire informal, self-employed individuals or businesses. To test this, I decompose informal employment into wage employment and self-employment (Appendix Table B1). Columns 5–6 show that the effect on informal self-employment is small and statistically insignificant.

7.1.2 Revisions in dispute resolution procedures:

The reform also changed how labor disputes are handled in court, aiming to reduce litigation and speed up case resolution. Most importantly, workers became responsible for court and attorney fees if their cases were dismissed. These costs had previously been borne entirely by employers (Corbi et al., 2022). This change substantially increased the expected cost of filing a lawsuit, and the number of new labor cases fell by about 34% in 2018 relative to 2017.[24]

Ease of firing formally employed workers: If firms now face a lower risk of court action, they may dismiss formal workers more easily and replace them with informal hires. To test this, I examine annual flows of new hires and separations using CAGED data. Appendix Figures B3 and B4 show no significant change in dismissals after the reform, but a clear decline in new formal hiring, opposite to what we would expect if easier firing were driving the results.

7.1.3 Introduction of new contract types:

The 2017 reform also introduced new forms of formal contracting, including part-time and intermittent work, aimed at increasing flexibility in hiring and working hours. These contracts were designed to mimic the flexibility of informal arrangements while retaining formal registration. If firms were primarily shifting workers from full-time formal contracts to these new flexible contracts, or from informal arrangements into intermittent or part-time formal contracts, we would not expect to observe a relative decline in formal employment as a result of the reform. The evidence therefore suggests that this channel is unlikely to explain the results. Instead, the patterns are more consistent with weakened enforcement driving the reduction in formal hiring.

Each of these features may have an impact on the overall labor market, however I establish that they do not systematically explain the pattern of results I observe in my framework.

7.2 Other mechanisms

7.2.1 Imperfect competition in the labor market:

Formal sector wage and employment declines as a result of weakening unions could also be explained by a model of imperfect competition in the labor market. Figure A6 (adapted from Dodini et al. (2024)) illustrates this intuition. When firms have wage-setting power, they face an upward-sloping labor supply curve, and in the absence of unions they set wages below the competitive level and employ fewer workers. Unions can counteract this distortion by bargaining wages upward. If the union-bargained wage lies between the monopsonistic wage and the competitive wage, employment and wages are both higher than under imperfect competition. In this case, a reduction in union power moves the economy back toward the monopsonistic outcome, leading to simultaneous declines in wages and employment. Thus, in an imperfectly competitive labor market, weakening unions can generate declines in both formal-sector wages and employment even without any role

[24] "Após reforma trabalhista, ações trabalhistas caem 34% em 2018." *Veja*, January 28, 2019. Available at: https://veja.abril.com.br/economia/apos-reforma-trabalhista-acoes-trabalhistas-caem-34-em-2018

for informality.

One test of the monopsony channel is that the effects of unions weakening should be stronger in markets with a greater degree of imperfect competition. I investigate the importance of this channel in two ways.

First, I test if unions were most effective in markets that are more concentrated. If this were true, when unions weaken, firms are now able to exert monopsony power in treated labor markets, which would explain the pattern of results. I create a Herfindahl-Hirschman Index (HHI) of formal labor market concentration.[25] In Table B2 I show that prior to 2017, my treatment measure of union effectiveness is not correlated with the HHI index. This establishes that the markets where union activity was weakened the most (higher values of the treatment measure), were not the most concentrated markets in the pre-period.

Second, the main results on formal wages and employment could vary by levels of local labor market competition within treatment units. If this were the case, I should observe negative effects concentrated in labor markets with higher market concentration. In Table B3, I interact the main DiD coefficient θ_3 from equation 13 with a continuous measure of pre-period labor market concentration. I do not observe heterogeneity in effects based on how concentrated treated labor markets are.

Taken together, these results suggest that concentration, as measured here, is unlikely to be the primary mechanism driving the observed declines in formal wages and employment. This does not imply that employer market power is absent in Brazil (see Felix (2021) for a discussion). Rather, formal-sector HHI may be an incomplete proxy for effective monopsony power in a setting with substantial informality. Firms compete along both formal and informal margins, but the data do not permit a measure of employer power that aggregates formal and informal hiring within firms.

7.2.2 Sectoral composition and concurrent shocks:

Unions in Brazil are organized by sector and geography, and some sectoral unions span multiple municipalities. If sectors with stronger unions are spatially concentrated, my treatment measure may partly reflect the location of specific industries rather than union effectiveness. If this is the case, sector-specific shocks in 2017 could generate patterns in wages and employment that resemble the effects I attribute to union weakening.

To address this concern, I construct a shift-share measure at the microregion level:

$$E_{mt} = \sum_{j=1}^{J} \frac{E_{mj}}{E_m} \times E_{jt} \tag{14}$$

where m indexes microregions, j sectors, and t time. I fix sector employment shares $\frac{E_{mj}}{E_m}$ in 2014

[25] The HHI is one measure of monopsony power, and is a widely used both in estimating product and labor market concentration. For each microregion, I compute each formal firm's share of total formal employment, square these shares and add up the resulting numbers. A higher value indicates a more concentrated market. This measure pertains to formal labor market concentration only, as the firm-level data do not capture informal employment.

and use national employment changes in sector j, excluding the microregion itself, to construct the shifts E_{jt}.

This measure captures national sector-level shocks weighted by each microregion's pre-reform sector composition. Microregions that were more exposed to a given sector in 2014 will be more affected by national employment changes in that sector. As the sector shares are fixed and the shocks are measured at the national level, this control absorbs national sectoral trends that differentially affect microregions based on their initial exposure.

The shift-share term E_{mt} is time-varying and therefore does not drop out when included alongside unit and time fixed effects. Controlling for this measure leaves the main point estimates and standard errors nearly unchanged (Appendix Table B4). This suggests that differential exposure to sectoral shocks is not driving the main results.

7.2.3 Elections:

A potential concern is that local political changes, rather than the 2017 reform, could explain the observed weakening of unions. In particular, the Workers' Party (PT), which is historically aligned with labor unions, lost substantial representation in the 2016 municipal elections. If unions in areas where the PT lost seats also experienced declines in activity, the observed effects could reflect shifts in political patronage rather than the reform itself. To test this, in Appendix Table B5 I examine whether the change in PT vote share between the 2014 general and 2016 municipal elections is correlated with my treatment measure of union exposure. After controlling for the 2014 PT vote share, which captures pre-existing political alignment, I find no systematic correlation. This indicates that the weakening of unions and their subsequent effects are not driven by concurrent electoral changes.

7.3 Robustness Checks

I examine the robustness of the main results to alternative identification choices and estimation methods.

7.3.1 Using a non-synthetic control group:

A potential concern with the SDiD estimator is that the counterfactual group is algorithmically constructed to generate parallel trends, and thus not helpful in understanding the differences in union exposed and less-exposed labor markets. I re-estimate equation 13 using a conventional two-way fixed effects (TWFE) difference-in-differences specification. In this exercise, treatment is defined as microregions in the top quartile of the continuous exposure measure, and the control group includes all remaining microregions (bottom 75%). I include state-by-time fixed effects to flexibly absorb region-specific shocks and add the sector-share controls described previously. Appendix Figures B7 and B8 show that the estimated effects on formal wages and employment are similar in magnitude to those obtained using the SDiD estimator, although the wage estimates are less precisely estimated. This consistency across estimators indicates that the SDiD proce-

dure constructs a credible counterfactual for highly exposed areas rather than generating spurious differences through weighting.

7.3.2 Using the continuous exposure measure:

In the main analysis, I discretize a continuous measure of pre-reform union exposure into high and low-exposure groups. This approach avoids imposing a linear relationship between treatment intensity and outcomes. In practice, the marginal effect of increasing union coverage from 20 to 30% of workers is unlikely to be comparable to that from 80 to 90%, since union spillovers and enforcement complementarities are likely to vary with union-density across labor markets.

To assess whether this discretization affects results, I re-estimate the specification using the continuous exposure measure directly. Appendix Figures B9 and B10 show that the qualitative patterns are similar: areas with greater pre-reform union exposure experience larger post-reform declines in wages and formal employment. However, the continuous DiD specification displays deviations from parallel trends.

The point estimates are also comparable once scaled to reflect differences in treatment intensity. In the continuous specification, moving from zero to full union coverage implies an estimated wage decline of roughly 5.5%. In the main SDiD specification, the estimated 0.9% decline corresponds to moving from the average coverage in low-exposure areas (23%) to that in high-exposure areas (49%). Extrapolating this estimate to a full shift from zero to complete coverage implies a wage decline of approximately 3.6%. This magnitude is close to the continuous estimate once the deviations from parallel trends in that specification are taken into account. I rely on the SDiD estimates in the main analysis, which provide a more credible counterfactual for highly union-exposed labor markets.

7.3.3 Varying the discrete treatment threshold:

I next assess whether the results are sensitive to the choice of cutoff used to define high-exposure labor markets. The SDiD estimator performs best when there are sufficient donor (control) units to construct a weighted counterfactual, which favors a smaller treated group. At the same time, choosing too high a threshold reduces the number of treated units and thus the precision of estimates. To illustrate this tradeoff, I re-estimate the SDiD using alternative thresholds, defining treated areas as those in the top 95th, 90th, 85th, 80th, 75th, and 50th percentiles of the continuous exposure distribution. Appendix Figures B11 and B12 present the results.

When treatment is defined narrowly (top 5%), estimated effect sizes are largest but standard errors widen due to reduced variation in treated-units. As the threshold is relaxed, estimates become more precise but pre-trends begin to diverge slightly as the counterfactual group becomes less distinct. Across all specifications, however, the direction and relative magnitude of effects on both wages and employment remain consistent, confirming that the results are not driven by a particular choice of treatment threshold.

7.3.4 Dropping individual years to test for mean reversion:

A concern with the SDiD estimator is that the pre-treatment weighting procedure mechanically enforces parallel pre-trends, potentially generating post-treatment divergence even in the absence of a true effect. This could occur if the estimator overfits idiosyncratic shocks in the pre-period, producing an appearance of treatment effects that reflect mean reversion rather than causal impact. To assess this possibility, I re-estimate the main specifications while sequentially excluding individual years from the sample. Appendix Figures B13 and B14 present the results. Across all re-estimations, the estimated effects vary only slightly: between -0.7 and -1% for wages and between -2 and -2.6% for employment. These patterns are consistent with the conventional TWFE results reported in subsection 7.3.1, which do not rely on synthetic weighting and yield similar estimates. Together, these exercises indicate that the observed effects are not driven by mean reversion or overfitting of pre-treatment noise, reinforcing the credibility of the main findings.

8 Conclusion

Making union contributions optional, after decades of mandatory collection, sharply reduced union revenues in Brazil. Unions responded by closing offices, dismissing staff, and scaling back collective bargaining. Workers continued to benefit from existing agreements but had little incentive to keep contributing, leading to widespread free-riding and a sustained decline in union capacity.

Local labor markets with more effective pre-reform unions experienced the largest declines in formal wages and employment. These effects were concentrated in areas farther from inspection offices, where enforcement weakened most after unions lost resources. While there is no evidence of overall changes in wage employment levels, the results indicate that firms substituted formal jobs with informal ones. Unions, in this sense, complemented inspection audits, helping ensure compliance with labor regulations and limiting informal hiring.

These results provide new evidence on how unions shape labor markets in economies where informality and imperfect enforcement coexist. They highlight the dual role of unions: while collective bargaining can raise formal wages, union participation also sustains formality by deterring informal contracting. Weakening unions can therefore generate unintended tradeoffs: lowering formal labor costs but eroding enforcement mechanisms that keep firms compliant.

The findings carry broader implications for labor market policy in countries where informality is widespread and growing, including through gig work and other flexible arrangements. As governments seek to modernize labor regulation, understanding the institutional role of unions is critical. Policies that erode union capacity may appear to reduce labor costs in the short run but can undermine compliance and formal employment in the long run.

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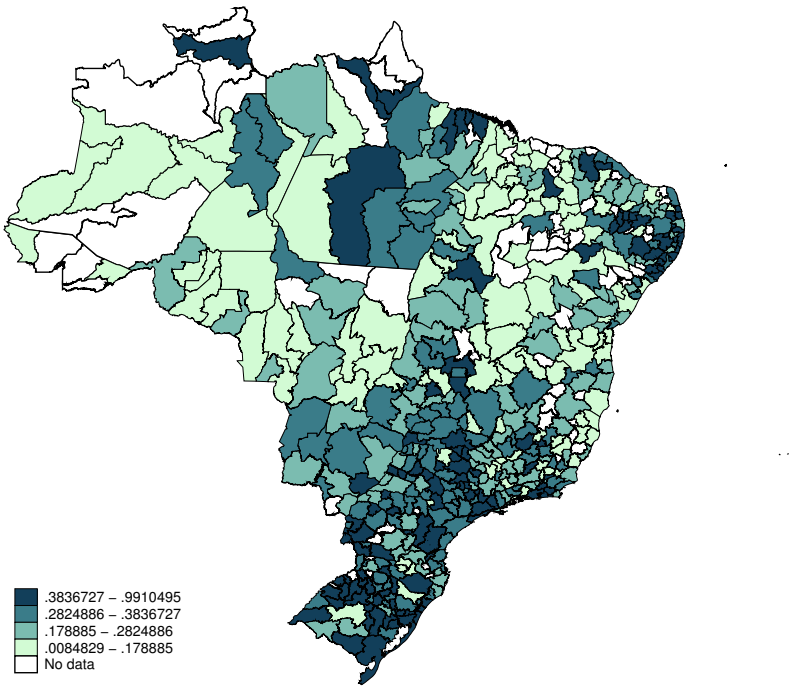
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Appendix A: Additional Figures and Tables

Figure A1: Microregions in Brazil and CBA coverage



Notes: This map plots the microregions of Brazil with a continuous measure of CBA coverage, defined as the proportion of the microregion's formally employed workers covered by any CBA in 2014. Microregions with no data are rural. The colours denote the four quartiles of the coverage measure, and darker microregions have a larger fraction of workers covered.

Table A1: Relationship between union staff and number of CBAs signed

	(1) Logged number of CBAs signed
Logged number of union staff	0.394*** [0.005]
Constant	0.447*** [0.005]
Observations	63485

Notes: This table shows the descriptive relationship between the number of staff employed by unions in a given year, and the number of CBAs they sign in that year. Observations are at the Union ID by Year level, which includes a balanced panel of 12,697 unions from 2012-2016. This specification includes year and microregion fixed effects. Robust SEs in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A2: Relationship between CBAs signed and employment outcomes for formally hired workers

	(1) Logged average monthly wages	(2) Logged average hourly wages	(3) Logged number formally employed workers
CBA Signed	0.066***	0.071***	0.578***
	[0.009]	[0.010]	[0.036]
Constant	7.073***	1.896***	9.083***
	[0.010]	[0.010]	[0.036]
Observations	28666	28666	28666

Notes: This table shows the descriptive relationship between an indicator for a CBA signed in a Sector by Microregion cell in 2014, and employment outcomes for formally hired workers in 2014. Observations are at the Sector by Microregion level, which includes a 84 sectors and 444 microregions. This specification includes sector and microregion fixed effects. Robust SEs in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A3: Correlates of CBA coverage

	(1) Treatment Exposure: Fraction workers in microregion covered by CBAs)	
Accommodation And Food	0.538*	[0.298]
Administrative Activities And Complementary Services	0.330	[0.246]
Arts, Culture, Sport And Recreation	-1.469	[2.752]
Business; Repair Of Motor Vehicles And Motorcycles	0.008	[0.130]
Construction	0.460***	[0.146]
Domestic Services	8.111	[14.399]
Education	0.155	[0.259]
Electricity And Gas	-0.397	[0.972]
Extractive Industries	-0.090	[0.122]
Financial, Insurance And Related Services Activities	-0.233	[1.442]
Human Health And Social Services	0.063	[0.332]
Information And Communication	2.513**	[1.006]
Manufacturing Industries	0.346***	[0.087]
Other Service Activities	0.548	[2.021]
Professional, Scientific And Technical Activities	0.458	[0.671]
Real Estate Activities	3.174	[3.336]
Transport, Storage And Mail	0.602**	[0.284]
Water, Sewage, Waste Management Activities And Decontamination	0.586	[0.916]
HHI	0.000	[0.000]
Frac popln formally employed	-0.108	[0.164]
R^2	0.491	
Observations	442	

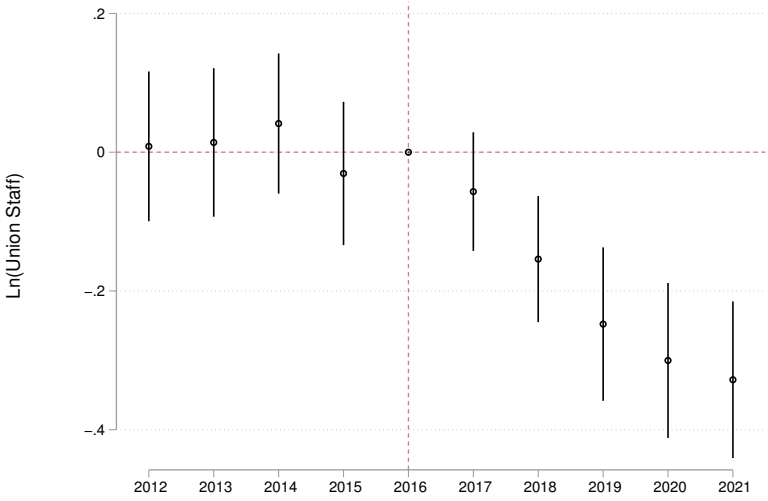
Notes: This table shows the correlates of the treatment exposure measure (fraction of a microregion covered by CBAs) regressed on fractions of each microregion's population employed in a given sector, measure of market concentration for the microregion (HHI), the fraction of the microregion that is formally employed. The regression includes state fixed effects. These data are for 2014, the year in which I calculate treatment variation. The regression drops 2 singleton observations and the unit of analysis is the microregion. Robust SEs in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A4: Relationship between union activity and inspections

	Share of workers inspected	Share of firms inspected
	(1)	(2)
CBA Signed	0.072***	0.012***
	[0.005]	[0.002]
Constant	0.178***	0.038***
	[0.003]	[0.001]
Observations	27977	27977

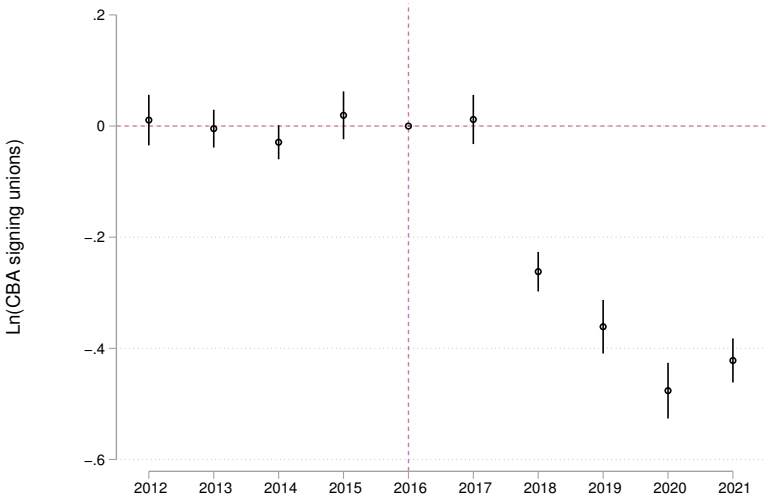
Notes: This table shows the descriptive relationship between an indicator for a CBA signed in a Sector by Microregion cell in 2014, and the fraction of (unique) workers and firms inspected in 2014 in the same cell. Observations are at the Sector by Microregion level, which includes a 84 sectors and 444 microregions. This specification includes sector and microregion fixed effects. Robust SEs in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Figure A2: Logged number of staff employed by unions



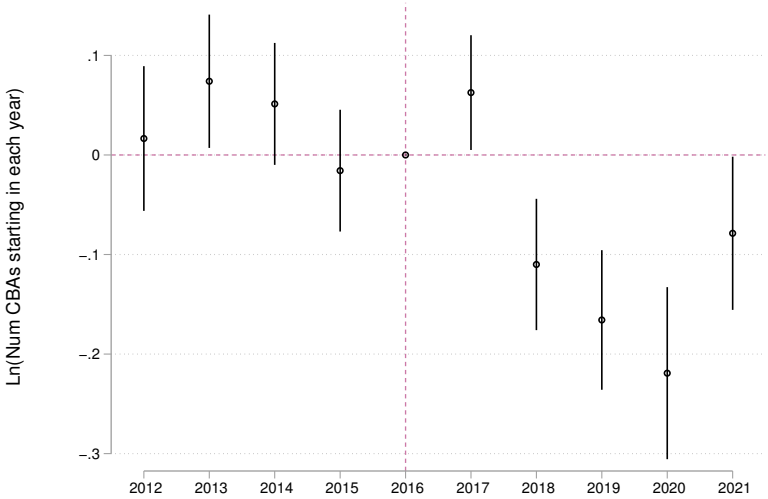
Notes: This graph plots the logged number of employees hired by unions every year summed up to the State by Year level. Each regression includes State fixed effects, with robust standard errors. 2016 is the omitted year. Unions begin to lose revenues in 2018, and begin to shrink their workforce.

Figure A3: Logged number of unions which sign CBAs



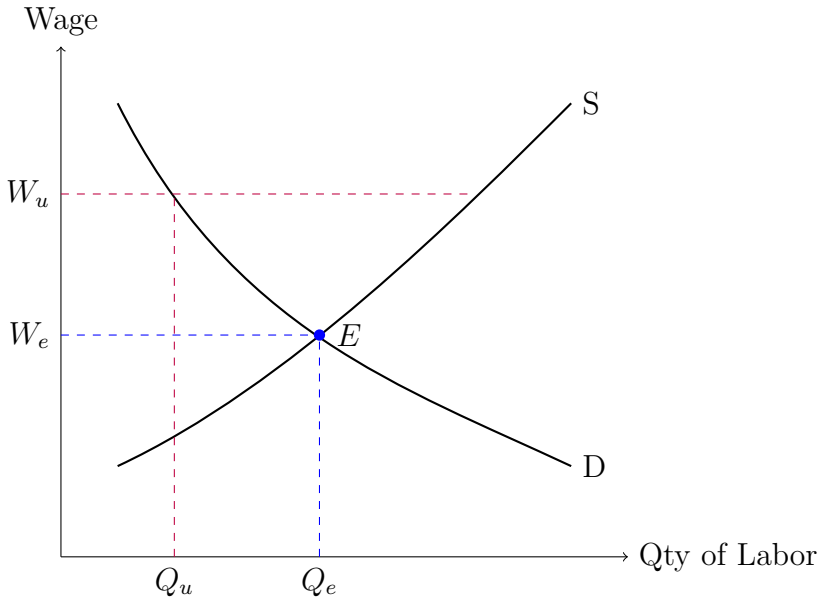
Notes: This graph plots the logged number of unions which engaged with CBA signing activities every year, summed up to the State by Year level. Each regression includes State fixed effects, with robust standard errors. 2016 is the omitted year. As the policy takes effect, the number of unions who engage with bargaining activities declines. This highlights that the policy even affected active unions.

Figure A4: Logged number of CBAs signed



Notes: This graph plots the logged number CBAs which take effect in a given year, summed up to the State by Year level. Each regression includes State fixed effects, with robust standard errors. 2016 is the omitted year. As the policy takes effect, the number of CBAs signed drops sharply. In this period, CBAs were typically valid for one year. When a CBA lapses, workers are not covered by any clauses till a new CBA is signed.

Figure A5: A simple model of union power, wages and employment



Notes: This figure denotes a simple model of equilibrium wages and employment with union power. In a market without unions, firms hire Q_e workers at wage W_e. Unions set wages above W_e; when unions act as a monoplist, they unilaterally set wages at W_u, and firms respond by hiring fewer workers Q_u (union as a monopolist model (Dunlop, 1944)). The wedge between W_u and W_e tells us about union barganing power (denoted by β in my conceptual framework). When unions bargain with firms, the wage lies between the competitive wage W_e and the union monopoly wage W_u ('right-to-manage' model (Leontief, 1946)). The essential takeaway from this model is that union power affects the wages and subsequent level of employment of workers, in a simple setting with one type of worker.

Table A5: Relationship between union activity and inspections

	Logged number workers inspected	Logged number firms inspected
	(1)	(2)
CBA signed	0.874***	0.228***
	[0.030]	[0.011]
Constant	1.365***	0.547***
	[0.014]	[0.005]
Observations	37296	37296

Notes: This table shows the descriptive relationship between an indicator for a CBA signed in a Sector by Microregion cell in 2014, and the number of workers and firms inspected in 2014 in the same cell. Observations are at the Sector by Microregion level, which includes a 84 sectors and 444 microregions. This specification includes sector and microregion fixed effects. Robust SEs in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A6: Relationship between inspections and distance before and after the 2017 reform

	Share of workers inspected	Share of firms inspected
	(1)	(2)
Post × Logged Distance	-0.003**	-0.032**
	[0.002]	[0.012]
Observations	4440	4440

Notes: This table shows the impact of distance on the fraction of workers and firms inspected in a microregion from 2012-2021. Post refers to 2017, when the policy takes effect. Distance refers to the log of the distance between a microregion and an inspection labor office. This is computed by calculating municipality-level distance measures, and then aggregating it to the microregion level. Observations are at the Microregion by Year level, and include microregion and year fixed effects. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A7: Average effect of the policy on measures of wage inequality for formally employed workers

	(1) High-skill premium	(2) Ln(90/10)	(3) Ln(90/50)	(4) SD Monthly wages
θ_3: Post × T_i	0.034***	-0.021	-0.005	14.754
	[0.011]	[0.086]	[0.006]	[19.198]
Treatment exposure to CBAs	0.49	0.49	0.49	0.49
Control exposure to CBAs	0.22	0.22	0.22	0.22
Observations	4,440	4,440	4,440	4,440

Notes: This table shows average effect (θ_3 in equation 13) of the policy on the various measures of wage inequality for formally employed workers. The outcome in column 1 is the log of the ratio of high-skill monthly wages to low-skill wages, in column 2 it is the the log of the ratio of the 90th percentile of monthly wages to the 10th percentile of monthly wages, column 3 is similar to column 2 except with the 50th percentile of wages as the denominator, and column 4 is the standard deviation of monthly wages. Low-skill workers as those with less than a high-school degree, while high-skill workers are those with a college degree or higher. Post refers to 2017, when the policy takes effect. Observations are at the Microregion by Year level, and include microregion and year fixed effects. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A8: Unpacking the wage skill-premium for formally employed workers

	(1) Logged average monthly wages low-skill workers	(2) Logged average monthly wages high-skill workers
θ_3: Post × T_i	-0.011***	0.014
	[0.004]	[0.008]
Mean Control-Pre Raw Outcome	1,009	2,892
Treatment exposure to CBAs	0.49	0.49
Control exposure to CBAs	0.22	0.23
Observations	4,440	4,440

Notes: This table shows average effect (θ_3 in equation 13) of the policy on the wages for formally employed low and high-skilled workers. Low-skill workers as those with less than a high-school degree, while high-skill workers are those with a college degree or higher. Post refers to 2017, when the policy takes effect. Observations are at the Microregion by Year level, and include microregion and year fixed effects. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A9: Average effect of the policy on wages (survey data)

	(1) Logged average monthly wages wage workers	(2) Logged average monthly wages formally employed wage workers	(3) Logged average monthly wages informally employed wage workers
θ_3: Post × T_i	0.015	-0.003	0.036
	[0.015]	[0.011]	[0.028]
Mean Control-Pre Raw Outcome	1,238	1,349	864
Treatment exposure to CBAs	0.51	0.51	0.51
Control exposure to CBAs	0.24	0.24	0.24
Observations	1,460	1,460	1,460

Notes: This table shows average effect (θ_3 in equation 13) of the policy on wage outcomes captured in the household survey data (PNADC). The outcome in column 1 is the logged monthly wage for all wage workers, column 2 is the logged monthly wage for formally hired wage workers, and column 3 is the logged monthly wage for informally hired wage workers. Post refers to 2017, when the policy takes effect. Observations are at the Macroregion by Year level, and include macroregion and year fixed effects. Bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A10: Average effect of the policy on the wages and stock of formally employed workers (macroregion level)

	(1) Logged average monthly wages	(2) Logged number formally employed workers
θ_3: Post $\times$ T_i	-0.002	-0.026***
	[0.003]	[0.008]
Mean Control-Pre period Outcome (raw)	1,463	440,550
Treatment exposure to CBAs	0.51	0.51
Control exposure to CBAs	0.24	0.24
Observations	1,460	1,460

Notes: This table shows average effect (θ_3 in equation 13) of the policy on the monthly wages and stock of formally employed workers (logged). Post refers to 2017, when the policy takes effect. Observations are at the Macroregion by Year level, and include macroregion and year fixed effects. Bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A11: Effect of the policy on the number of informally hired workers found in inspections

	(1) Logged number informally employed workers found	(2) Logged number informally employed workers found
θ_3: Post × T_i	-0.172	
	[0.115]	
θ_4: Post × T_i × Close to LO		0.104
		[0.135]
θ_5: Post × T_i × Far from LO		-0.494**
		[0.230]
Mean Control-Pre Raw Outcome	3,591.02	
Mean Treat-Close to LO-Pre Raw Outcome		4,470.18
Mean Treat-Far from LO-Pre Raw Outcome		1,906.96
Treatment exposure to CBAs	0.49	
Control exposure to CBAs	0	
Observations	4,440	4,440

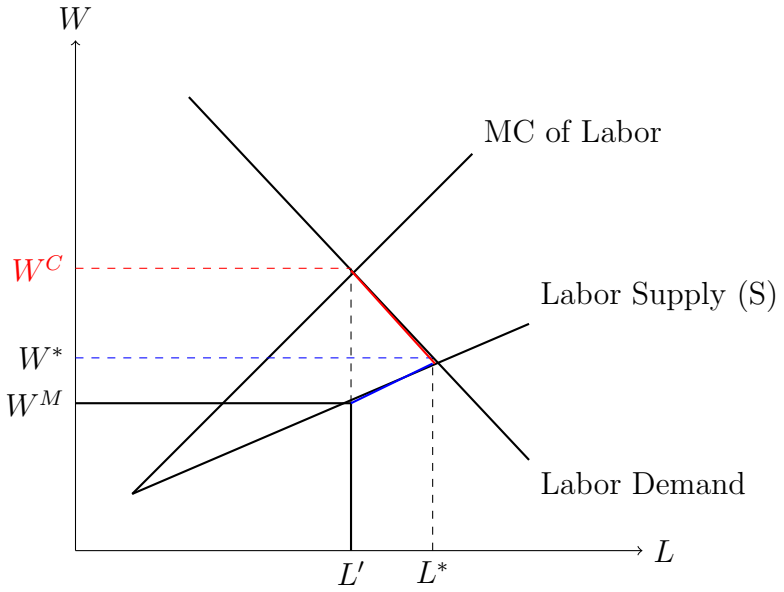
Notes: This table shows the effect of the policy on the logged number of informal workers found at firms in an inspection activity in a given microregion. Column 1 presents the average effect (θ_3 in equation 13), while column 2 interacts this coefficient with an indicator for 'Far from a labor office (LO)'. Close to an LO implies that the microregion is below median distance away from the nearest labor inspection office, while Far from LO regions are above median distance away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations are at the Microregion by Year level. Column 1 includes year and microregion fixed effects, while column 2 includes the same fixed effects interacted with an indicator for 'Far from LO'. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table A12: Effect of the policy on the number of low-skill wage workers (by distance to a labor office, survey data only)

	(1) Ln(Num low-skill formal workers)	(2) Ln(Num low-skill formal workers)	(3) Ln(Num low-skill informal workers)	(4) Ln(Num low-skill informal workers)
θ_3: Post × T_i	-0.079**		0.007	
	[0.036]		[0.048]	
θ_3: Post × T_i × Close to LO		-0.055		-0.162**
		[0.059]		[0.062]
θ_3: Post × T_i × Far from LO		-0.001		0.236***
		[0.073]		[0.081]
Mean Control-Pre Raw Outcome	98,838.02		32,614.54	
Mean Treat-Close to LO-Pre Raw Outcome		134231.84		38,688.91
Mean Treat-Far from LO-Pre Raw Outcome		83,501.68		30,032.26
Treatment exposure to CBAs	0.51		0.51	
Control exposure to CBAs	0		0	
Observations	1,460	1,460	1,460	1,460

Notes: This table shows the effect of the policy on the number of formally and informally hired low-skill wage workers using household survey data (PNADC). Columns 3 and 4 are the same as Table 5. Low-skill workers as those with less than a high-school degree. Columns 1 and 3 present the average effect (θ_3 in equation 13), while columns 2 and 4 interact this coefficient with an indicator for 'Far from a labor office (LO)'. In columns 2 and 4, Close to an LO implies that each municipality in the macroregion contains a labor office while Far from LO regions are further away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations are at the Macroregion by Year level, columns 1 and 3 include year and macroregion fixed effects, while columns 2 and 4 includes the same fixed effects interacted with an indicator for 'Far from LO'. Bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

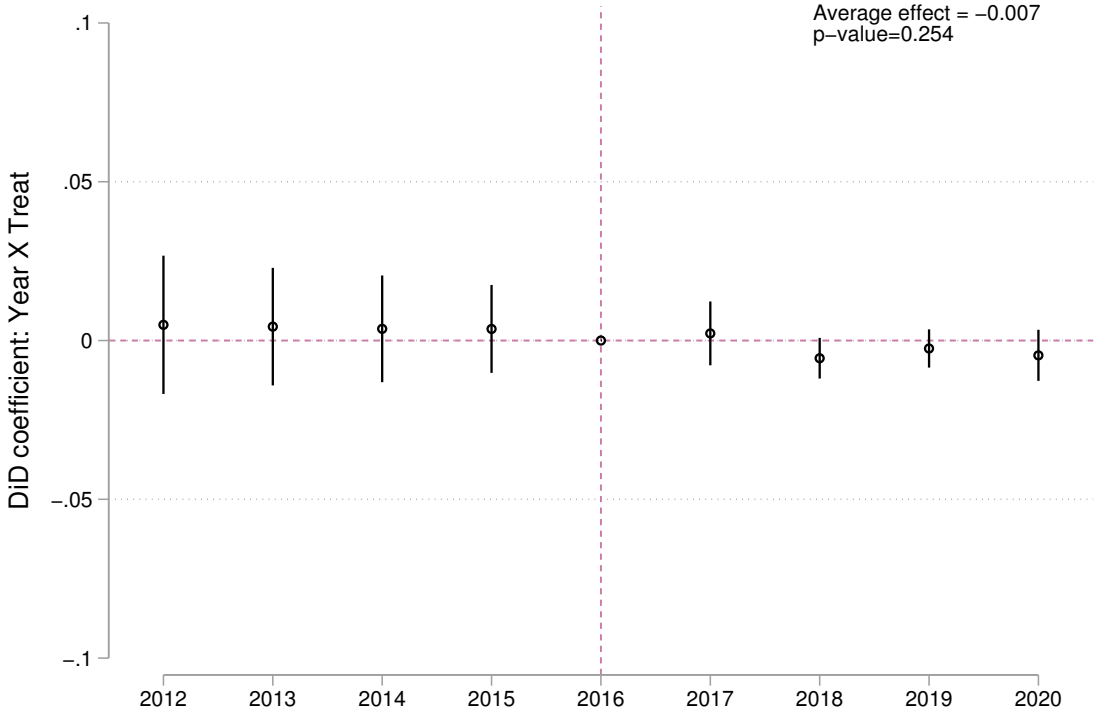
Figure A6: Wages and Employment in a Monopsony framework



Notes: If individual firms have wage-setting power, they each face an steeper upward sloping supply curve of labor, denoted by MC, as opposed to the market supply curve denoted by S. In the absence of unions, firms would hire workers at $L^{'}$ and pay them W^M. Unions bargain wages above W^M. When the bargained wage lies between the monopsonistic wage W^M and the competitive wage W^*, wages and employment increase. When unions weaken, I establish that formal wages and employment decline. In this model, this would imply moving from W^* towards W^M and subsequently L^* towards $L^{'}$.

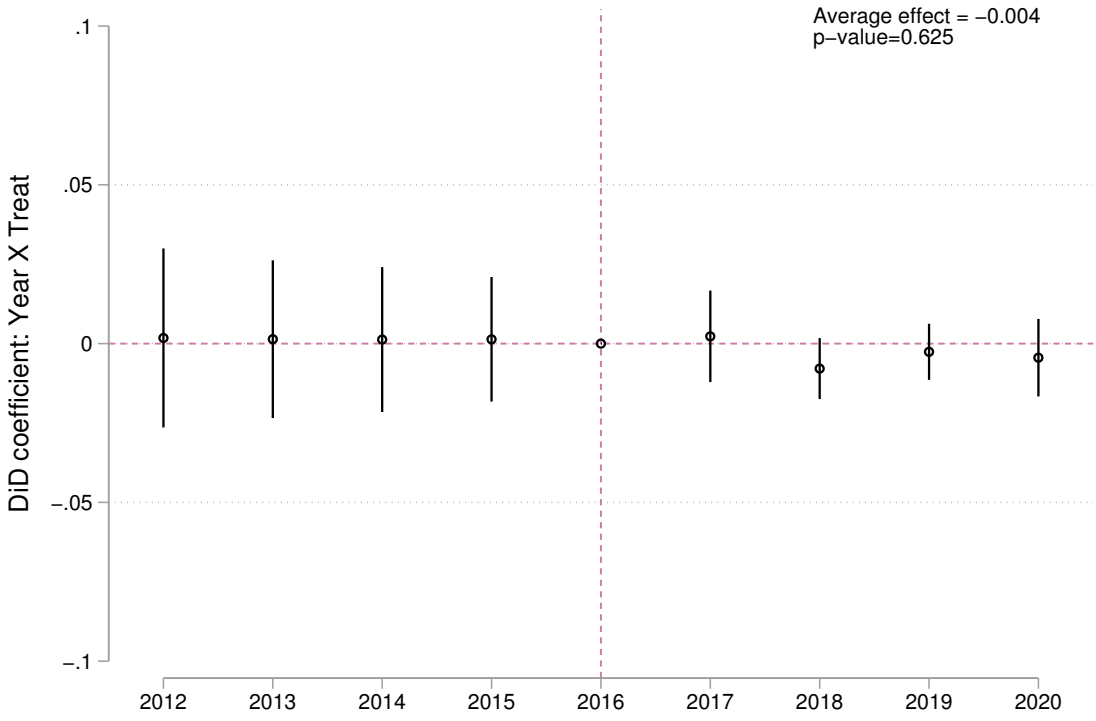
Appendix B: Alternate Mechanisms

Figure B1: Logged number of formally registered firms



Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of registered firms every year. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B2: Logged number of formally registered firms with zero employees



Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of registered firms who report having zero employees every year. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Table B1: Effect of the policy on the number of informally hired workers (by distance to a labor office, survey data only)

	(1) Logged number informally employed workers	(2) Logged number informally employed workers	(3) Logged number informally employed wage workers	(4) Logged number informally employed wage workers	(5) Logged number informally employed self-employed workers	(6) Logged number informally employed self-employed workers
θ_3: Post × T_i	0.032		0.024		0.034	
	[0.022]		[0.034]		[0.023]	
θ_3: Post × T_i × Close to LO		-0.022		-0.063		-0.005
		[0.031]		[0.047]		[0.032]
θ_3: Post × T_i × Far from LO		0.058		0.110*		0.032
		[0.041]		[0.063]		[0.043]
Mean Control-Pre Raw Outcome	225200.46		72,552.21		153548.29	
Mean Treat-Close to LO-Pre Raw Outcome		334555.62		103232.57		232456.32
Mean Treat-Far from LO-Pre Raw Outcome		176326.19		58,957.20		118205.13
Treatment exposure to CBAs	0.51		0.51		0.51	
Control exposure to CBAs	0		0		0	
Observations	1,460	1,460	1,460	1,460	1,460	1,460

Notes: Notes: This table shows the effect of the policy on the number of informally hired workers using household survey data (PNADC). Columns 1 and 2 capture the total number of informally hired workers, which includes wage-workers (columns 3 and 4), and self-employed non-wage workers (columns 5 and 6). Columns 1, 3 and 5 present the average effect (θ_3 in equation 13), while columns 2, 4 and 6 interact this coefficient with an indicator for 'Far from a labor office (LO)'. In columns 2, 4 and 6, Close to an LO implies that each municipality in the macroregion contains a labor office while Far from LO regions are further away. Far from LO is the indicator that enters the regression, while Close to LO is the omitted group. Observations are at the Macroregion by Year level, columns 1, 3 and 5 include year and macroregion fixed effects, while columns 2, 4 and 6 includes the same fixed effects interacted with an indicator for 'Far from LO'. Bootstrapped SEs clustered at the macroregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table B2: Correlation between labor market concentration and the treatment exposure (pre-period)

	(1) HHI Index	(2) HHI Index
Continuous Exposure Measure	-34.580	
	[311.937]	
Discretized Exposure Measure		39.969
		[55.359]
Constant	343.601***	323.558***
	[98.776]	[29.458]
Observations	2220	2220

Notes: This table shows the correlation between the treatment measure and the HHI index in the years before the policy (2012-2016). In column 1, I regress the index on the continuous treatment measure, while in column 2 I regress the index on the binary indicator for treatment used in all the specifications. Observations are at the Microregion by Year level. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table B3: Average effect of the policy on the wages and stock of formally employed workers: Heterogeneity by baseline labor market concentration

	(1) Logged average monthly wages	(2) Logged number formally employed workers
θ_3: Post $\times$ T_i	-0.005	-0.015**
	[0.004]	[0.008]
θ_3: Post $\times$ T_i $\times$ Baseline HHI Index	-0.000	-0.000
	[0.000]	[0.000]
Observations	4440	4440

Notes: This table shows the average effect (θ_3 in equation 13) of the policy on the monthly wages and stock of formally employed workers (logged), interacted with baseline HHI computed at the microregion level. Baseline HHI refers to the HHI index, averaged between 2012-2016 at the microregion level. Observations are at the Microregion by Year level. These specifications include year and microregion fixed effects interacted with the baseline HHI measure. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table B4: Average effect of the policy on the wages and stock of formally employed workers: Controlling for the shift-share instrument

	(1) Logged average monthly wages	(2) Logged number formally employed workers
θ_3: Post $\times$ T_i	-0.009**	-0.025***
	[0.004]	[0.007]
Mean Control-Pre Raw Outcome	1,291	174,119
Treatment exposure to CBAs	0.49	0.49
Control exposure to CBAs	0.23	0.23
Observations	4,440	4,440

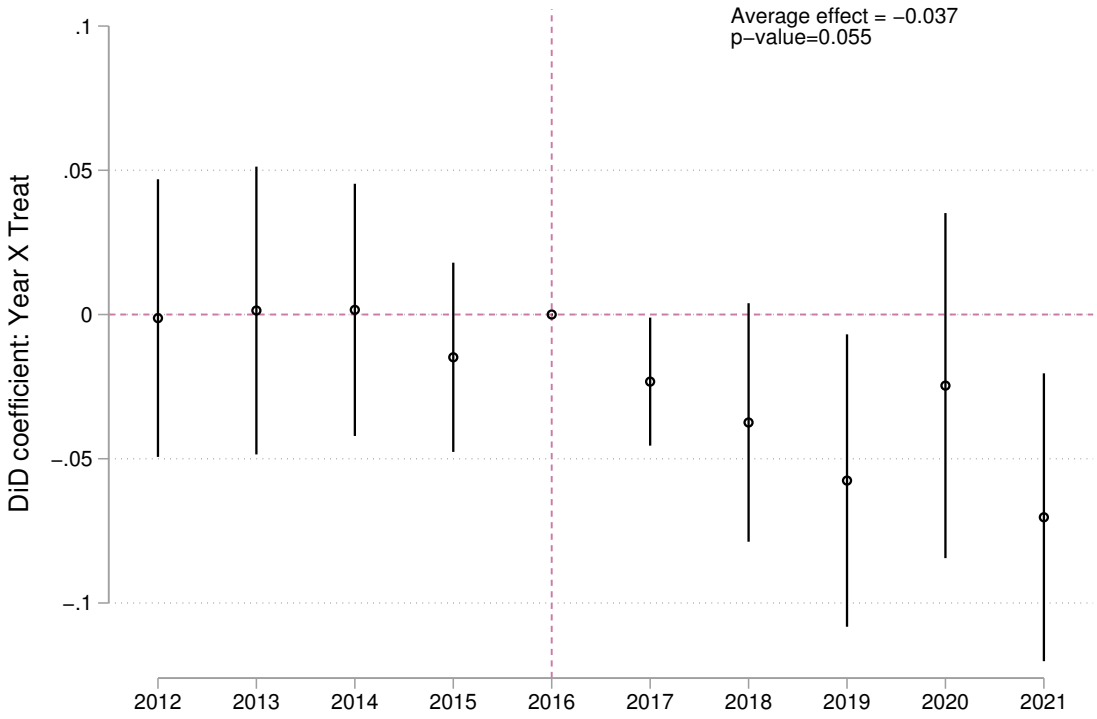
Notes: This table shows the average effect (θ_3 in equation 13) of the policy on the monthly wages and stock of formally employed workers (logged) after controlling for the time-varying shift share instrument. Observations are at the Microregion by Year level, and include microregion and year fixed effects. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Table B5: Correlation between 2016 election outcomes and treatment exposure (pre-period)

	(1) 2016 Worker Party Share	(2) 2016 Worker Party Share
Discretized Exposure Measure	0.018	
	[0.015]	
Continuous Exposure Measure		0.093
		[0.154]
Observations	443	443

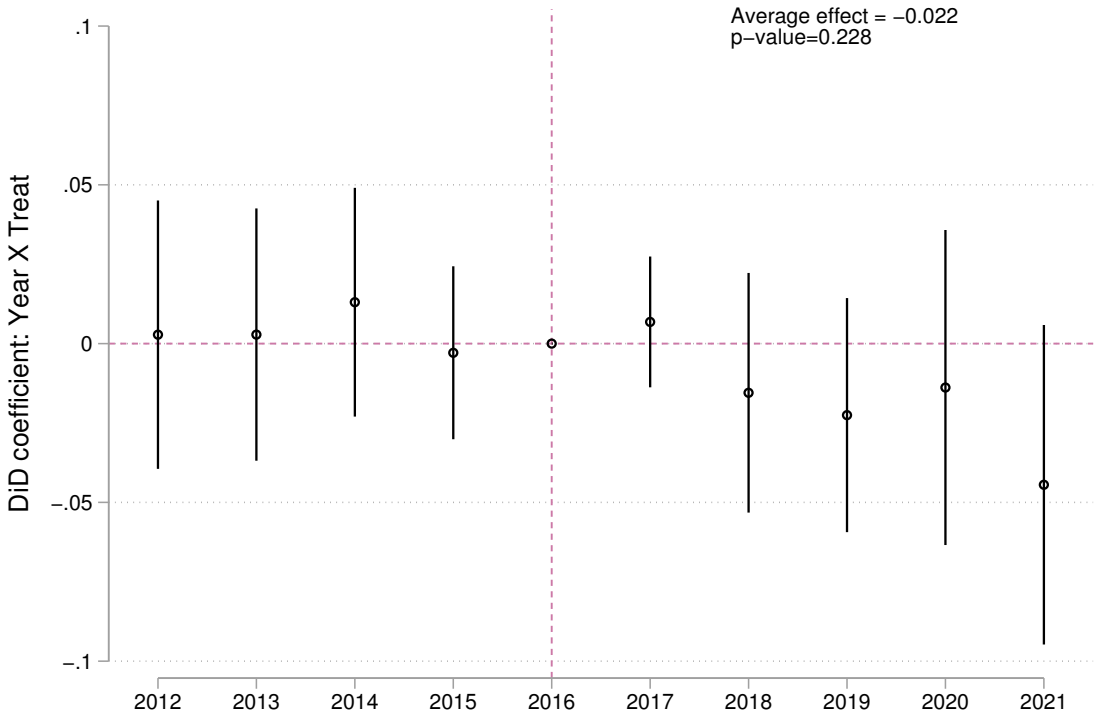
Notes: This table shows the relationship between 2016 vote shares for the Worker Party and the treatment exposure measures, after controlling for 2014 vote shares. Regressions include SDiD weights for microregions. Observations are at the Microregion level. Bootstrapped SEs clustered at the microregion level in brackets. * $p < .1$, $^{**}p < .05$, *** $p < .01$.

Figure B3: Logged number of formal hires (CAGED)



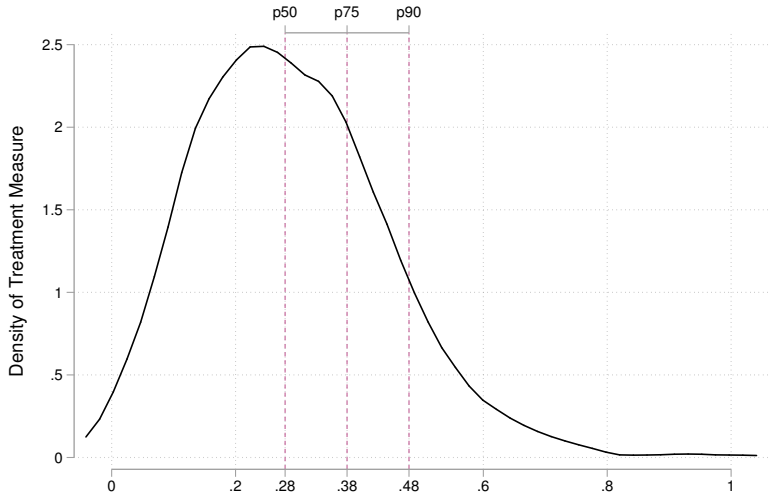
Notes: This figure plots DiD coefficients over time for effects of the policy on the number of formally hired workers in a given year. Data on labor market flows come from the *Cadastro Geral de Empregados e Desempregados* (CAGED). I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B4: Logged number of formal workers fired (CAGED)



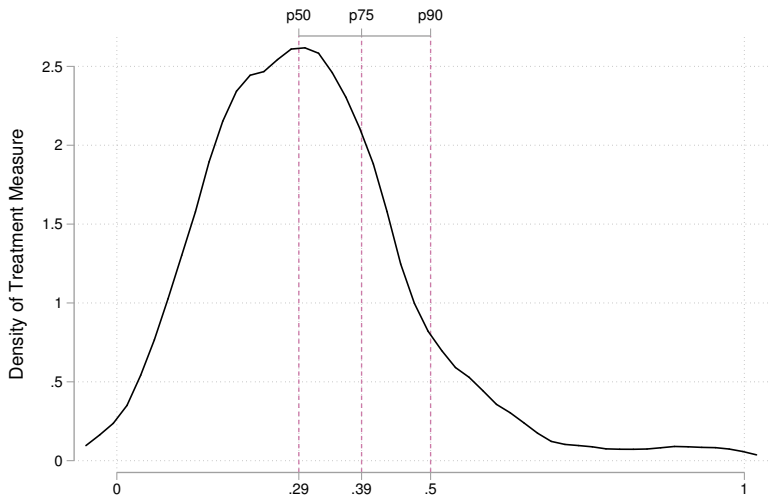
Notes: This figure plots DiD coefficients over time for effects of the policy on the number of formal workers fired in a given year. Data on labor market flows come from the *Cadastro Geral de Empregados e Desempregados* (CAGED). I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B5: Distribution of treatment measure (microregion)



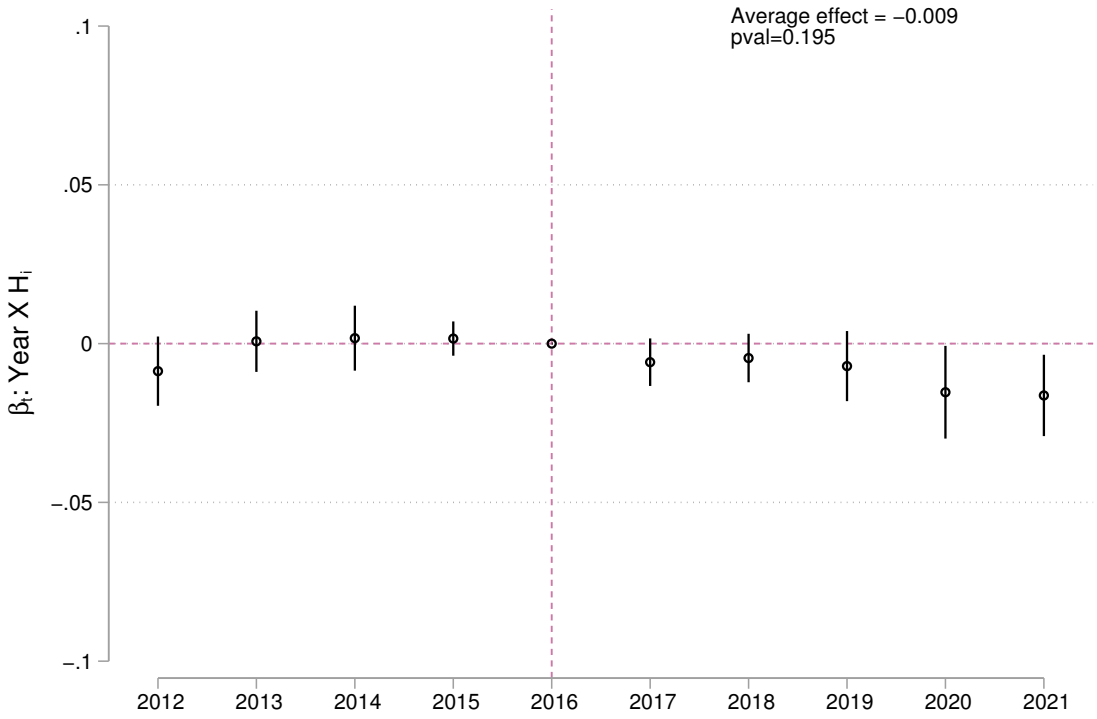
Notes: This graph plots the kernel density estimate of the treatment measure at the microregion level. The treatment measure captures the fraction of formally employed workers in the microregion covered by a CBA in 2014. In my empirical framework, microregions as classified as treated if their treatment value is greater than or equal to 0.38 (75th percentile of the continuous measure). This yields 111 treatment microregions.

Figure B6: Distribution of treatment measure (macroregion)



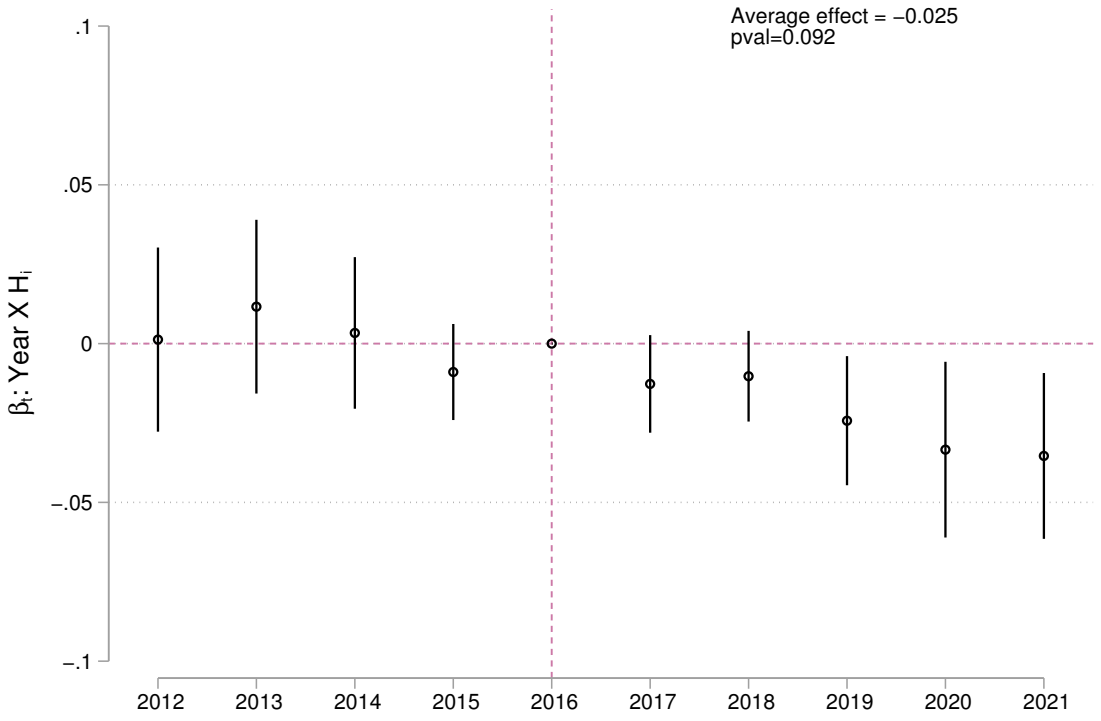
Notes: This graph plots the kernel density estimate of the treatment measure at the macroregion level. The treatment measure captures the fraction of formally employed workers in the macroregion covered by a CBA in 2014. In my empirical framework, macroregions as classified as treated if their treatment value is greater than or equal to 0.41 (75th percentile of the continuous measure). This yields 18 treatment macroregions.

Figure B7: Logged monthly wages: DiD without Synthetic Control Group



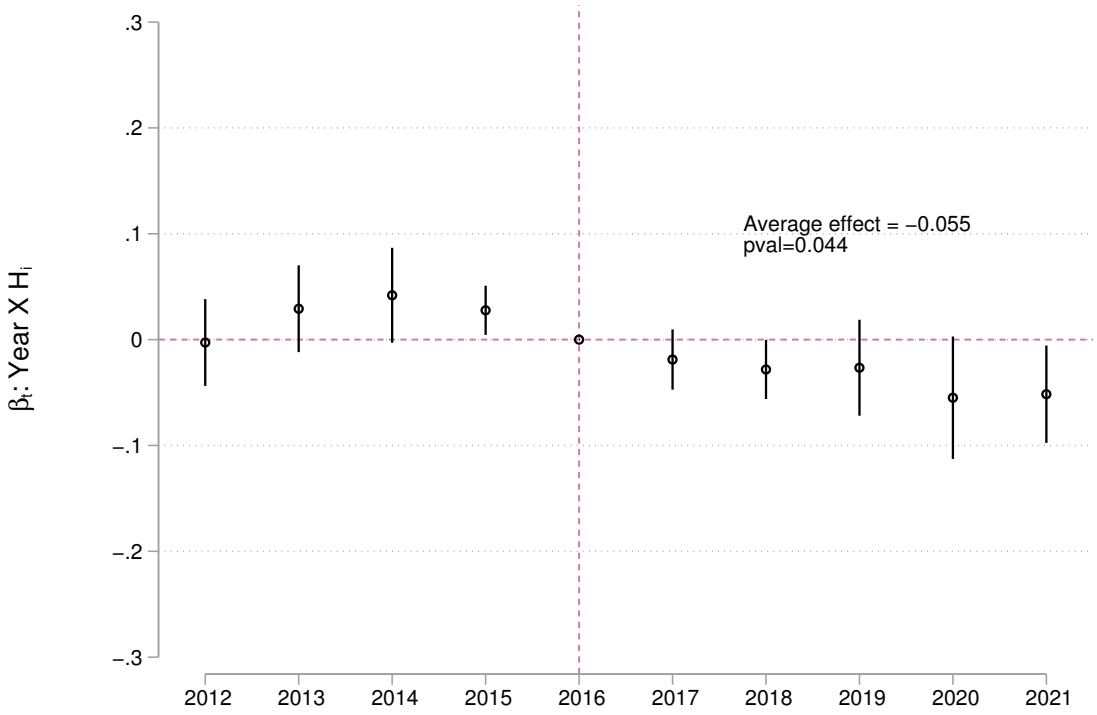
Notes: This figure plots DiD coefficients over time for effects of the policy on average monthly wages for formally hired workers. The treatment group is the same as the main specification (top 25 percentile of the continuous measure: fraction of microregion's formal workers covered by a CBA in the pre-period). The control group is the remaining microregions (bottom 75 percentile of the distribution). I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, state by year fixed effects, controls for the shift-share instrument, and bootstrapped standard errors are clustered at the microregion level.

Figure B8: Logged number of formally hired workers: DiD without Synthetic Control Group



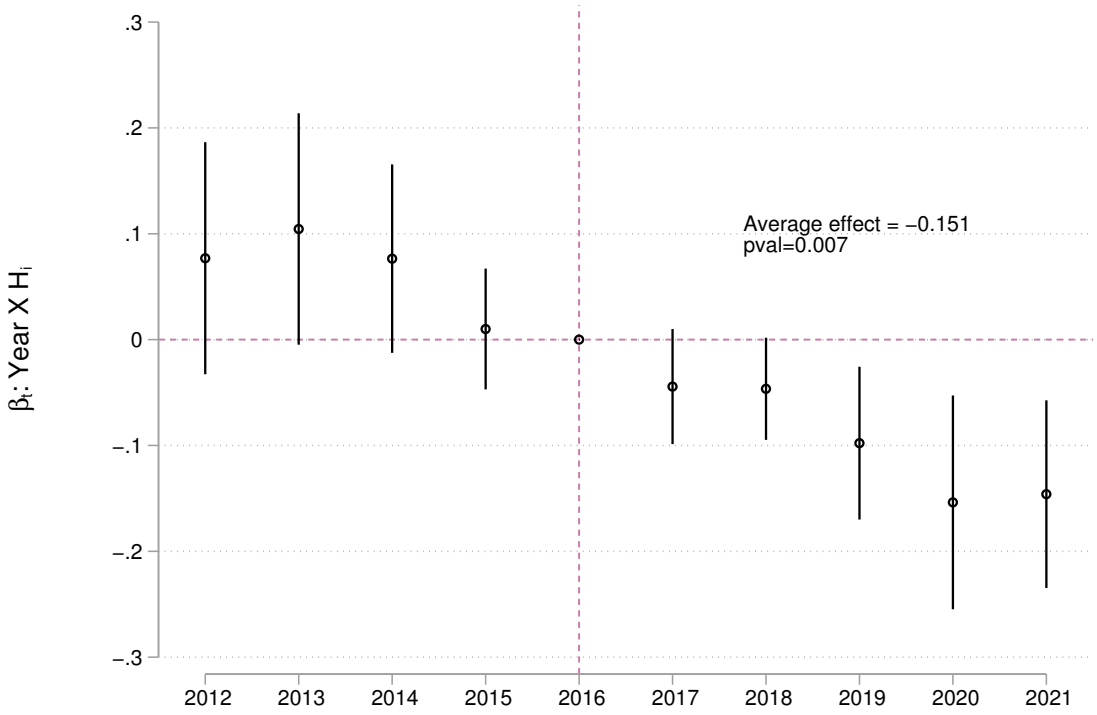
Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of formally hired workers. The treatment group is the same as the main specification (top 25 percentile of the continuous measure: fraction of microregion's formal workers covered by a CBA in the pre-period). The control group is the remaining microregions (bottom 75 percentile of the distribution). I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, state by year fixed effects, controls for the shift-share instrument, and bootstrapped standard errors are clustered at the microregion level.

Figure B9: Logged monthly wages: Continuous DiD



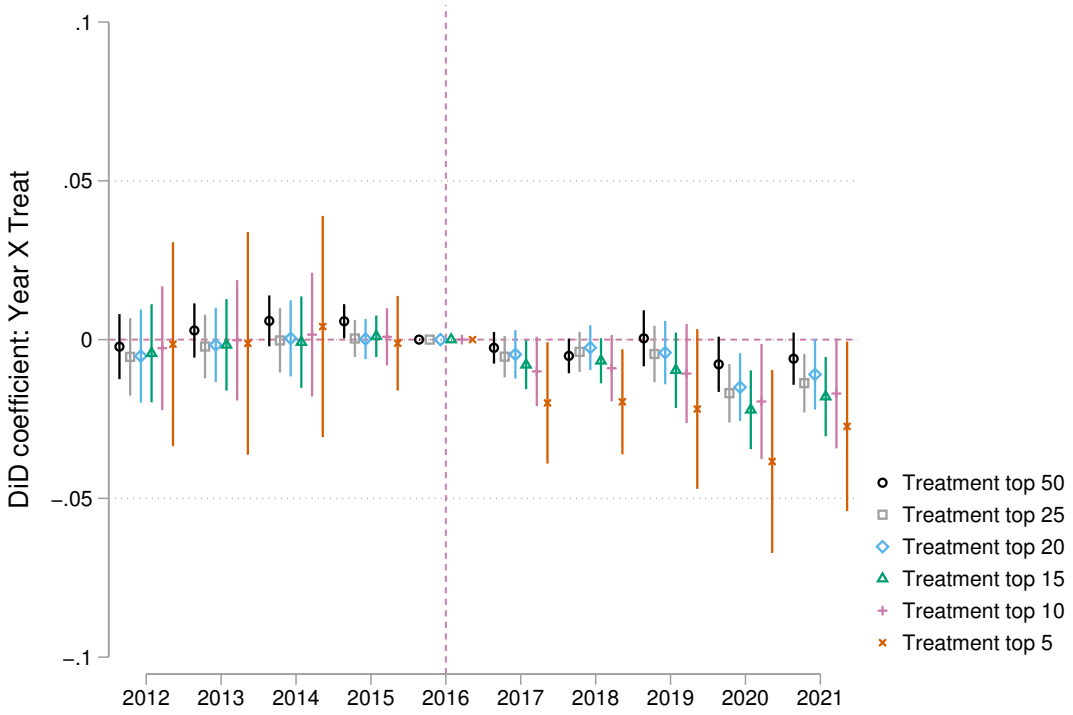
Notes: This figure plots DiD coefficients over time for effects of the policy on average monthly wages for formally hired workers. This specification makes use of the continuous treatment measure: fraction of microregion's formal workers covered by a CBA in the pre-period. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, state by year fixed effects, controls for the shift-share instrument, and bootstrapped standard errors are clustered at the microregion level.

Figure B10: Logged number of formally hired workers: Continuous DiD



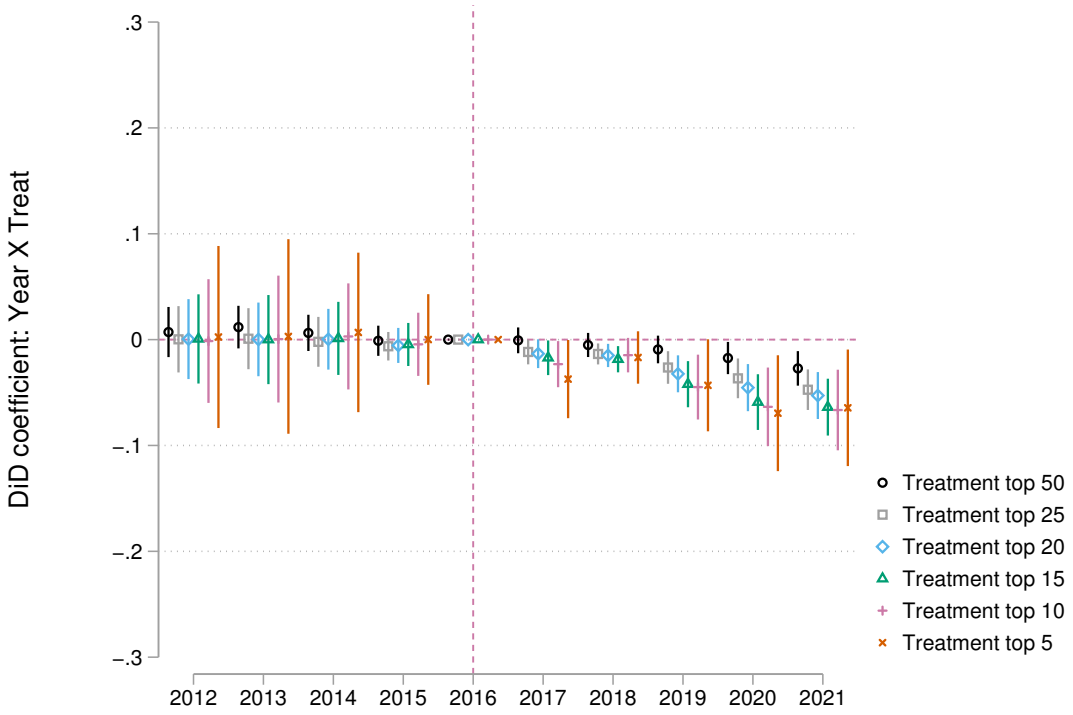
Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of formally hired workers. This specification makes use of the continuous treatment measure: fraction of microregion's formal workers covered by a CBA in the pre-period. I also report the average effect size. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, state by year fixed effects, controls for the shift-share instrument, and bootstrapped standard errors are clustered at the microregion level.

Figure B11: Logged monthly wages: Threshold robustness



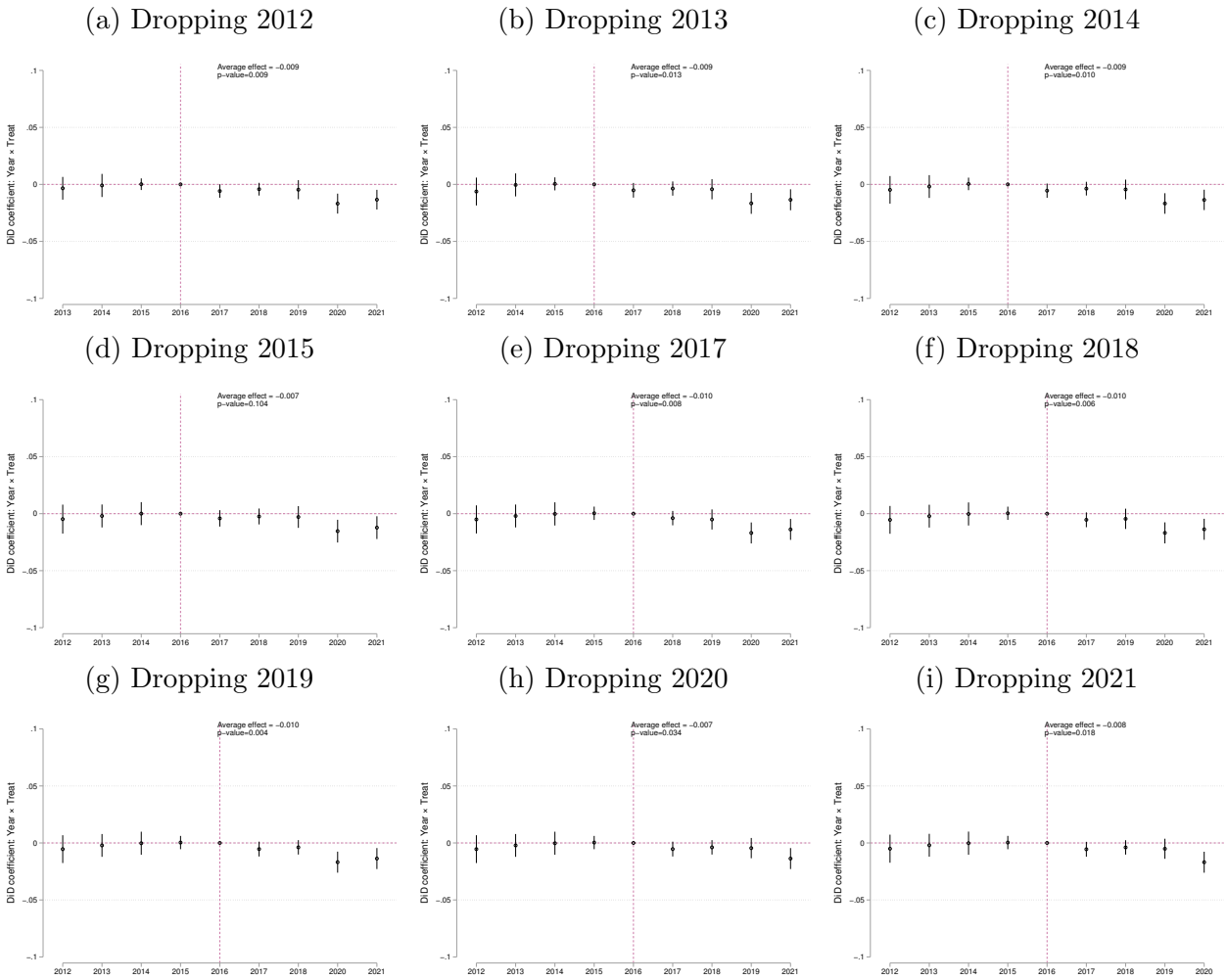
Notes: This figure plots DiD coefficients over time for effects of the policy on the average monthly wages for formally hired workers. I report results for different exposure thresholds, where treatment is defined as the top 5 to 50% of the continuous treatment measure: fraction of microregion's formal workers covered by a CBA in the pre-period. The SDiD estimator creates a synthetic comparison group which is a weighted combination of comparison (non-treatment) spatial units and pre-policy time periods in each specification. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B12: Logged number of formally hired workers: Threshold robustness



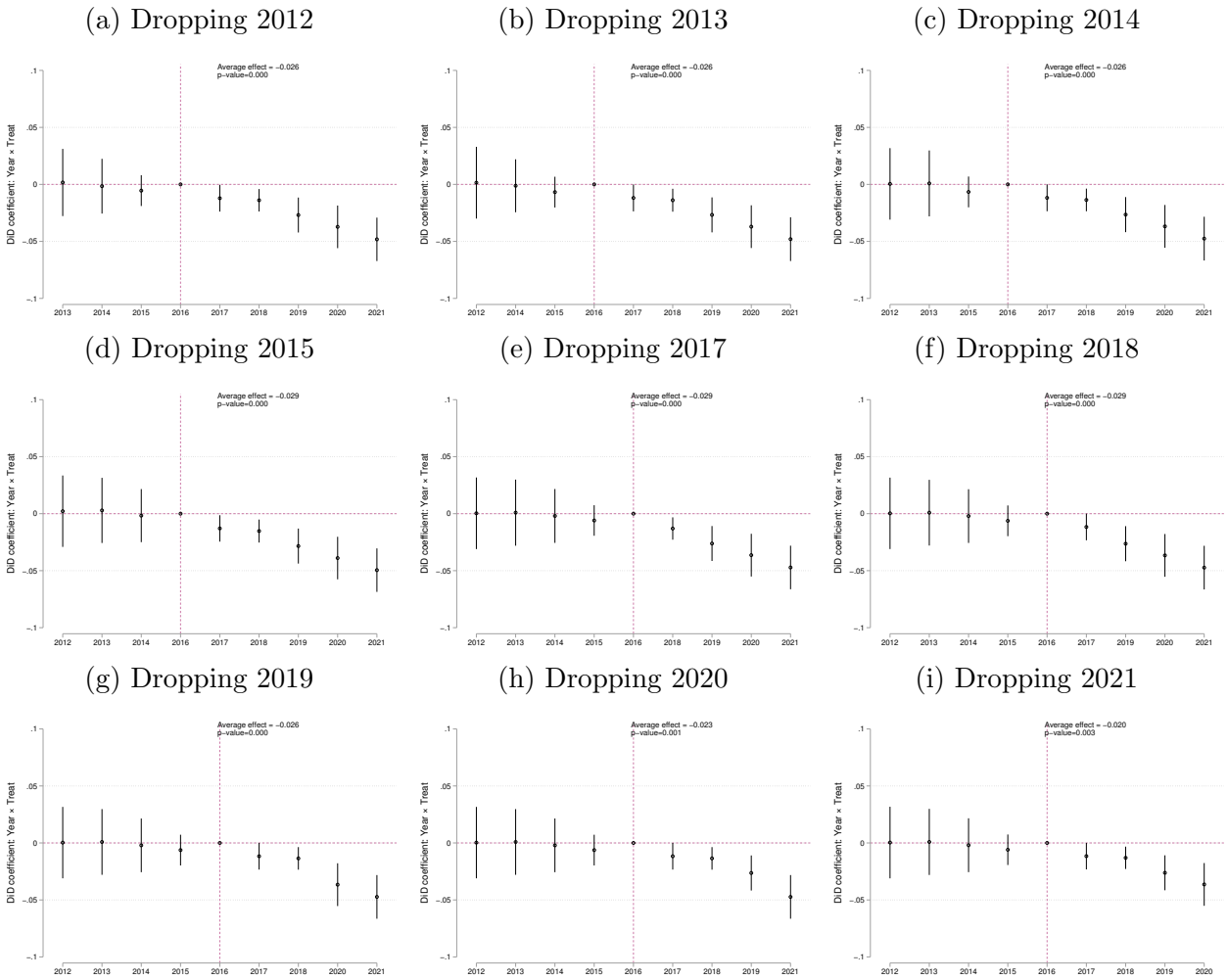
Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of formally hired workers. I report results for different exposure thresholds, where treatment is defined as the top 5 to 50% of the continuous treatment measure: fraction of microregion's formal workers covered by a CBA in the pre-period. The SDiD estimator creates a synthetic comparison group which is a weighted combination of comparison (non-treatment) spatial units and pre-policy time periods in each specification. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B13: Logged monthly wages: Dropping individual years



Notes: This figure plots DiD coefficients over time for effects of the policy on the average monthly wages for formally hired workers. In each panel, I drop one year and re-estimate the difference-in-differences specification. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Figure B14: Logged number of formally hired workers: Dropping individual years



Notes: This figure plots DiD coefficients over time for effects of the policy on the stock of formally hired workers. In each panel, I drop one year and re-estimate the difference-in-differences specification. Observations are at the microregion by year level, with a sample of 444 urban microregions. The specification includes microregion fixed effects, bootstrapped standard errors are clustered at the microregion level.

Appendix C: Theory Appendix

In this section, I present proofs of the propositions from the main text.

Optimal quantity of workers hired (Proposition 1):

Proof. The optimal quantities of formally and informally hired workers (U^* and I^*, respectively) are given by the following first order conditions:

$$\frac{\partial \Pi(U^*, I^*)}{\partial U} \equiv \gamma p (U^* + I^*)^{\gamma - 1} - t = 0 \tag{15a}$$

$$\frac{\partial \Pi(U^*, I^*)}{\partial I} \equiv \gamma p (U^* + I^*)^{\gamma - 1} - \frac{\partial}{\partial I}\left[\Big(w(I^*) + \chi(I^*)\Big) I^*\right] = 0 \tag{15b}$$

Now we solve for U^* by rearranging equation 15a:

$$\begin{aligned} t &= \gamma p (U^* + I^*)^{\gamma - 1} \\ \left(\frac{t}{\gamma p}\right)^{\frac{1}{\gamma - 1}} &= U^* + I^* \\ U^* &= \left(\frac{t}{\gamma p}\right)^{\frac{1}{\gamma - 1}} - I^* \\ U^* &= \left(\frac{\gamma p}{t}\right)^{\frac{1}{1 - \gamma}} - I^* \end{aligned}$$

The optimal number of formally hired workers (U^*) depends on the number of informally hired workers (I^*). We obtain an expression for I^* as follows. First we start with equation 15b:

$$\gamma p (U^* + I^*)^{\gamma - 1} = \frac{\partial}{\partial I}\left[\Big(w(I^*) + \chi(I^*)\Big) I^*\right]$$

Substitute the left hand side using 15a to obtain:

$$t = \frac{\partial}{\partial I}\left[\Big(w(I^*) + \chi(I^*)\Big) I^*\right]$$

Then simplify:

$$\begin{aligned} t &= \frac{\partial}{\partial I}\Big[w(I^*) \cdot I^* + \chi(I^*) \cdot I^*\Big] \\ &= w(I^*) + I^* \cdot w'(I^*) + \chi(I^*) + I^* \cdot \chi'(I^*) \end{aligned}$$

where we use $w(I) = I^\epsilon$, so $w'(I) = \epsilon I^{\epsilon - 1}$

$$= (\epsilon + 1)(I^*)^\epsilon + \chi(I^*) + I^* \cdot \chi'(I^*)$$

Using the linearity of χ, and substituting for $w(I^*)$ we get:

$$t = (\epsilon + 1)w(I^*) + kI^* + kI^*$$

Rearranging we get:

$$I^* = \frac{1}{2k}\Big[t - (\epsilon + 1)w(I^*)\Big]$$

□

The impact of decreasing union power on the quantity of workers hired is ambiguous (Proposition 2):

Proof. Begin by implicitly differentiating the following expression for I^* with respect to β:

$$t = (\epsilon + 1)(I^*)^{\epsilon} + \chi(I^*) + I^*\chi'(I^*)$$

This yields:

$$t'(\beta) = \epsilon(\epsilon + 1)(I^*)^{\epsilon-1}\frac{\partial I^*}{\partial \beta} + \frac{\partial \chi(I^*)}{\partial \beta} + \frac{\partial}{\partial \beta}(I^*\chi'(I^*))$$
$$t'(\beta) = \frac{\partial I^*}{\partial \beta}\Big[\epsilon(\epsilon + 1)(I^*)^{\epsilon-1} + \chi'(I^*)\Big] + \frac{\partial \chi(I^*)}{\partial \beta} + I^*\frac{\partial}{\partial \beta}(\chi'(I^*))$$

Unpacking the terms:

$$\frac{\partial \chi(I^*)}{\partial \beta} = \frac{I^*}{dI_{max}}\delta$$
$$\chi'(I^*) = \frac{\beta}{dI_{max}}\delta$$
$$I^*\frac{\partial}{\partial \beta}(\chi'(I^*)) = \frac{I^*}{dI_{max}}\delta$$
$$I^*\frac{\partial}{\partial \beta}(\chi'(I^*)) = \frac{\partial \chi(I^*)}{\partial \beta}$$

Plugging in:

$$t'(\beta) = \frac{\partial I^*}{\partial \beta}\Big[\epsilon(\epsilon + 1)(I^*)^{\epsilon-1} + \chi'(I^*)\Big] + 2\frac{\partial \chi(I^*)}{\partial \beta}$$
$$\frac{\partial I^*}{\partial \beta} = \left[\frac{t'(\beta) - 2\chi'(\beta)}{\epsilon(\epsilon + 1)(I^*)^{\epsilon-1} + \chi'(I^*)}\right]$$

The denominator of this expression is always positive, thus:

$$\left[\frac{\partial I^*}{\partial \beta}\right] = \begin{cases} > 0 & \text{if } t'(\beta) > 2\chi'(\beta) \\ < 0 & \text{if } t'(\beta) < 2\chi'(\beta) \end{cases}$$

Now, differentiate the following equation for U^* with respect to β:

$$\begin{aligned}\frac{\partial U^*}{\partial \beta} &= \frac{\partial}{\partial \beta}\left[\left(\frac{t}{\gamma p}\right)^{\frac{1}{\gamma-1}} - I^*\right] \\ &= \frac{\partial}{\partial \beta}\left(\frac{t_0+\lambda\beta}{\gamma p}\right)^{\frac{1}{\gamma-1}} - \frac{\partial}{\partial \beta} I^* \\ &= \frac{\partial}{\partial \beta}\left(\frac{t_0+\lambda\beta}{\gamma p}\right)^{\frac{1}{\gamma-1}} - \frac{\partial I^*}{\partial \beta} \\ &= \frac{1}{\gamma-1}\left(\frac{t_0+\lambda\beta}{\gamma p}\right)^{\frac{2-\gamma}{\gamma-1}} \cdot \frac{\lambda}{\gamma p} - \frac{\partial I^*}{\partial \beta}\end{aligned}$$

Thus:

$$\frac{\partial U^*}{\partial \beta} = \begin{cases} > 0 & \text{if } \frac{\lambda}{(\gamma-1)\gamma p}\left(\frac{t}{\gamma p}\right)^{\frac{2-\gamma}{\gamma-1}} > I'^*(\beta) \\ < 0 & \text{if } \frac{\lambda}{(\gamma-1)\gamma p}\left(\frac{t}{\gamma p}\right)^{\frac{2-\gamma}{\gamma-1}} < I'^*(\beta) \end{cases}$$

□

The impact of decreasing union power on wages for informal workers is ambiguous (Proposition 3):

Proof. By assumption, wages for formally hired workers are give by $t = t^0 + \lambda\beta$. Therefore

$$\frac{\partial t}{\partial \beta} = \lambda > 0$$

For informally hired workers, wages are governed by $w = I^\epsilon$. Therefore:

$$\frac{\partial w(I^*)}{\partial \beta} = \epsilon (I^*)^{\epsilon-1} \cdot \frac{\partial I^*}{\partial \beta}$$

The term $\epsilon(I^*)^{\epsilon-1}$ denotes a positive quantity. Thus;

$$\frac{\partial w(I^*)}{\partial \beta} = \begin{cases} > 0 & \text{if } (I^*)'(\beta) > 0 \\ < 0 & \text{if } (I^*)'(\beta) < 0 \end{cases}$$

□