Effects of the Minimum Wage on the Labor Market: New Evidence from State Wage Floors

Gustavo Oliveira* Paulo Vaz† Carlos Corseuil‡

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Abstract

This work investigates the effects of introducing a wage floor on labor market outcomes. Exploring a sample of pairs of neighboring municipalities that share a state border, we analyze the case of Paraná, which implemented the policy in 2006, using bordering municipalities in the states of Mato Grosso do Sul and Santa Catarina as controls. Employing the dynamic differences-in-differences strategy (also known as "Event-Study"), we find a positive and statistically significant effect of the policy on wages, as well as a negative effect on the probability of being unemployed at the end of the period. A possible mechanism guiding this result may come from the supply side of the market, with workers changing their employment behavior due to the wage increase. To test this hypothesis, estimations were made for the probability of a worker quitting and the probability of an employer firing them. Although not always statistically significant, we found negative and persistent effects on the likelihood of the employee leaving the job after the intervention, and more scattered effects on the probability of being fired. Alternative estimations controlling for observables were tested as a robustness check. These findings further indicate that this type of policy does not necessarily generate adverse effects on the level of employment.

Keywords: Minimum Wage. Labor Market. Pair of municipalities. Differences-in-Differences.

*Ph.D. candidate, Brazilian School of Economics and Finance, FGV EPGE.
†Member of the Faculty, Federal University of Pernambuco and GAPPE.
‡Director of Social Studies and Policies, IPEA

1 Introduction

After remaining predominantly theoretical since its inception, the debate over the effects of the minimum wage took a new form in the early 1990s with works that became known for inaugurating the 'new minimum wage literature' (CARD, 1992a; CARD, 1992b; KATZ; KRUEGER, 1992; NEUMARK; WASCHER, 1992). These works, one of which earned a Nobel Prize for one of the authors (David Card), stood out for building evidence using tools from the field of applied microeconometrics, rekindling the discussion by presenting very diverse results, ranging from negative to null and, in some cases, even positive effects on employment. With this in mind, the present work aims to investigate the effects of state minimum wage policies on labor market outcomes, using an unprecedented empirical setup at the national level, constituting a valuable contribution to the minimum wage literature in developing countries.

In Brazil, the national minimum wage policy was instituted by then-president Getúlio Vargas in the first half of the last century and was unified in 1984, representing a significant social achievement. With periodic adjustments and strong popular appeal, its implementation has generated substantial effects on income distribution in the country (ENGBOM; MOSER, 2022), resonating intensely among the poorer classes and gaining significant attention from academia. Thus, the tasks of identifying, monitoring, and evaluating its impacts, with the aim of having a clear perception of its costs and benefits, become of utmost importance for its maintenance by public authorities.

With this in mind, in 2000, Complementary Law (CL) No. 103, which allowed federative entities to establish local minimum wage laws with values higher than the federal minimum, added new possibilities in the field of empirical research. Since then, five states have explored this legislation (Rio de Janeiro and Rio Grande do Sul in 2001; Paraná in 2006; São Paulo in 2007; and Santa Catarina in 2010), taking advantage of one of its main features, which allowed them not to make it mandatory for public administration services, thus not imposing a significant burden on their treasuries.

Using an empirical strategy that exploits the border discontinuity between municipalities and microregions that share a state border to obtain comparable control groups, we found positive and statistically significant effects on employment in all estimated specifications. The chosen case study is that of Paraná, which implemented the minimum wage policy in 2006 with Law No. 15.118. The setup involved identifying workers who earned below the minimum wage during the baseline period (the semester immediately preceding the treatment) to monitor their outcomes in the formal labor market over the timeline from 2003 to 2008.

The remainder of this document is divided as follows: in the next section, we have a brief literature review on the minimum wage; section 3 presents the methodology employed, divided between sample description and empirical strategy; in section 4, the results are presented and discussed; and finally, in section 5, we have the conclusion.

2 Related Literature

As stated by Neumark, Salas e Wascher (2014) (NSW), the debate about the effects of the minimum wage on labor market outcomes has extended for over a century in economic literature. Despite various recent theoretical and methodological advances, and its enormous political and social appeal, academia has yet to reach a consensus on the subject, largely stalling due to conceptual and methodological disputes that have emerged over time.

In the theoretical field, starting from the neoclassical microeconomic foundation, which assumes perfectly competitive labor markets, the imposition of a minimum wage—which equates to an increase in the price of labor—tends to reduce employment levels according to this perspective. However, in more concentrated market structures, with labor market frictions or general equilibrium effects, the sensitivity of employment to the minimum wage can vary, even with a positive sign. Economists such as Stigler, Machlup, and Lester stand out in these two theoretical lines.

A recent example of this discussion is the study by Bhaskar, Manning e To (2002), which analyzes the effect of the minimum wage in an oligopsonistic market. They identify two effects on employment: the "Oligopsony" effect, which is positive due to the increase in labor market participation, and the negative "Exit" effect, resulting from the drop in company profits. They conclude that a minimum wage slightly above the market rate can increase or reduce employment, depending on the prevailing effects.

Moreover, Berger, Herkenhoff e Mongey (2022) develop a general equilibrium model of the labor market in an oligopsonistic context, estimating the welfare effects of minimum wage imposition and identifying optimal values under different configurations.

In the empirical realm, the traditional state panel approach uses the variation in the minimum wage between states over time to estimate its average national effect on employment. Although it has shown negative effects in many cases, especially for less-skilled workers (NEUMARK; WASCHER, 1992; NEUMARK; WASCHER, 2007), this approach is criticized for its difficulty in dealing with unobserved spatial heterogeneities, given the lack of a credible control group in the United States, where differences in economic performance between states complicate comparisons.

Several researchers have sought to empirically validate these results, focusing on case studies and using established identification strategies. A prominent example is the work of David Card, Nobel laureate, who examined the effects of a minimum wage increase in New Jersey (CARD; KRUEGER, 1993). He used the difference-in-differences strategy, comparing low-wage establishments in the fast-food industry in the state with those across the Delaware River in Pennsylvania. Contrary to expectations, no evidence of a drop in employment was found.

Another relevant study is by Dube, Naidu e Reich (2007), who analyzed the effects of implementing a municipal minimum wage in San Francisco. They observed an increase in hourly wages but found no statistically significant effect on employment.

Conversely, a study on the effects of the minimum wage during the 2008 recession (CLEMENS; WITHER, 2019) identified negative impacts on the employment rate of less-skilled workers. By segmenting individuals according to the intensity of the "treatment," the authors were able to identify heterogeneous effects of the policy through a triple-difference model and assess possible spillover effects between groups.

In an attempt to clarify this puzzle, and pointing out the flaws of these two methodological approaches, Dube, Lester e Reich (2010)—hereafter DLR1—explore a border discontinuity design to verify the effect of state minimum wages on employment and income indicators. With a sample of county pairs that share a state border, the authors leverage the variation in minimum wage policies between states to estimate its effect on low-wage sectors. Their results also do not indicate adverse effects on employment levels.

In another study using a border discontinuity design, Dube, Lester e Reich (2016) (DLR2) analyze the effects of increases in state minimum wages on employment flow and stock, as well as on average earnings. Using a rich alternative data set, the authors find no evidence of negative impacts on employment stock but detect negative and statistically significant effects on hiring and layoffs of two intensely affected groups—young workers and the restaurant industry.

However, this methodology is questioned by NRW, who evaluate the capacity of these research designs to obtain reliable comparison groups. The authors maintain the conclusion of their previous work that this type of intervention imposes a trade-off between higher wages for some and job losses for others, negatively impacting the most vulnerable workers.

In the same vein, in a recent work, Jha, Neumark e Rodriguez-Lopez (2022) (JNR) reevaluated the DLR1 study using a more suitable alternative concept of local economic area in the spatial discontinuity design. Instead of county pairs, they use pairs of multi-state commuting zones that share a state border to absorb potential local economic shocks. With this setup, the authors find negative and statistically significant results on employment levels. It is important to highlight that in Allegretto et al. (2017), we have a rebuttal to the criticisms presented by NRW, where the advantages of a border discontinuity design are highlighted, proven, and reaffirmed.

Analyzing changes in employment transitions, Dube, Lester e Reich (2011) (DLR3) identify a significant drop in employment flow. They use DLR1's empirical strategy to calculate elasticities and build a labor market equilibrium model with search frictions, validating their results through statistical procedures.

Meanwhile, Brochu e Green (2013) compare layoff, dismissal, and hiring rates under low and high minimum wage regimes in Canadian provinces, observing a negative relationship between the minimum wage and these rates. However, the study's setup raises doubts about the causality of these findings.

Furthermore, Cengiz et al. (2019) examine over a hundred cases of minimum wage adjustments, finding novel results on the total employment of low-wage workers. They use a non-traditional difference-in-differences approach to decompose the intervention's impact on wage ranges along the distribution, focusing on the lower part around the new minimum value to assess the number of jobs lost and created after the event.

Given the diversity of studies on the subject, the lack of consensus in the literature is evident. Several authors have dedicated themselves to meta-analyses to clarify possible trends. Neumark e Corella (2021) review 60 articles on the effects of the minimum wage in developing countries, noting that negative impacts are more common in institutional designs and sectors aligned with competitive model predictions.

On the other hand, Neumark e Shirley (2022) analyze the North American literature, highlighting a predominance of negative effects on employment, especially among young, low-educated workers and those directly affected by the policy.

Regarding developing countries, Broecke, Forti e Vandeweyer (2017) find a small impact of the minimum wage on employment. They do not identify significant differences from the literature on developed countries, highlighting a higher incidence of negative effects on vulnerable groups.

To try to overcome this puzzle, in a recent study, Azar et al. (2023) generate important evidence on the role of market structure on minimum wage effects. Building various proxies and delving into a partial equilibrium model of oligopsonistic competition, they find robust results pointing to a negative relationship between labor market concentration and employment sensitivity to the minimum wage. In the extreme case, their results point to substantial positive effects on employment levels as market power approaches a monopsonistic structure. Furthermore, the mechanism driving this finding is explained by the model, establishing that the higher the concentration, the lower the average market wage relative to the marginal productivity of labor, thus leaving room for wage increases without impacting employment. This work sheds light on one of the most commonly pointed hypotheses in the literature, never before tested, about the variety of different results on the subject, being the first to do so using a rigorous empirical strategy.

Regarding the Brazilian case, building an equilibrium model in a frictional labor market subject to a minimum wage, Engbom e Moser (2022) find that this policy was responsible for 45% of the decline in the variance of logarithmic earnings observed over the analysis period, without substantially affecting employment levels.

As for the present case study, Corseuil, Foguel e Hecksher (2015) (CFH) present an evaluation of the impact of the implementation of state minimum wages in Brazil on labor market outcomes. Employing the synthetic control method and matching the occupational groups covered by the policy to the Brazilian Occupation Code (CBO), they find results for three distinct occupational groups. In at least one group for each analyzed federation unit (UF), a positive effect of the policy on wages was found, without implying a drop in employment.

Finally, it is important to note that, despite meta-analysis studies indicating a predominance of negative results on the effect of the minimum wage on employment, the most robust and reliable evidence in terms of identification finds positive effects, in most cases. Additionally, the literature is already beginning to produce reliable empirical evidence on the role of market power in the impact of this type of intervention.

3 Methodology

3.1 Data

For the construction of the sample, three databases were used:

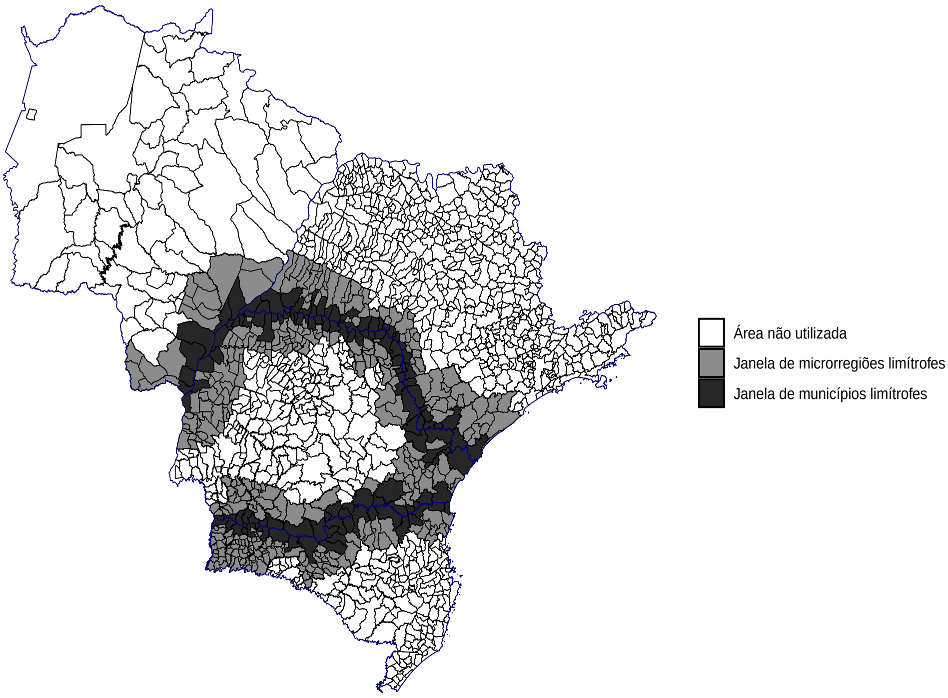
1. **RAIS**: The main source of information used is the Annual Social Information Report (Relação Anual de Informações Sociais - RAIS), the primary database on the labor market in the country, in its identified version. Covering the entire national formal sector, it contains detailed information about each employee-employer pair, such as type of contract, start and end dates of the employment relationship, as well as characteristics of the worker, such as occupation, gender, age, education, and remuneration. Through its variables of admission date and dismissal date, it is possible to construct a panel with higher temporal frequency for employment indicators.

2. **CBO's**: A database that reconciles the occupations present in the law with the Brazilian Classification of Occupations (CBO). Since wage ranges are set according to occupational groups defined by the states themselves, a process of reconciling the lists of occupations that would be covered by the intervention according to the CBO is necessary. Fortunately, this task has already been done by CFH and will be reused here. Also, due to an update made to the CBO in 2002, and given that the reconciliation was done for this latest version, the temporal cut is limited to information after this event.

3. **List of Border Municipalities**: A sample constructed by researcher Marcos Hecksher of IPEA, which connects pairs of municipalities that share a state border between states that implemented the policy and those that did not. In Figure 1 below, we have a map with the geographical cut of the municipalities and micro-regions we used, and in Figure 2, we have a map highlighting the treated and control municipalities for the main estimation.

The construction of the sample consisted of a series of restrictions to reach the ideal group of workers for the estimation. At first, only private sector jobs, whose occupations were covered by the policy, were kept, regardless of which side of the border they were on. Next, we identified and retained in the database only those individuals who were employed at baseline with remuneration lower than the wage range, regardless of which side of the border they were on, to keep only those who could be directly affected by the policy. Additionally, workers with inconsistent observations or measurement errors were removed. Finally, the database was limited to only workers with registered employment at baseline in one of the municipalities that share the border between the state of Paraná and the states of Mato Grosso do Sul and Santa Catarina. Since the state of São Paulo implements the policy during the analysis period, we excluded the pairs of municipalities PR-SP to avoid contaminating

Figure 1 – Pair of municipalities/micro-regions for the case of Paraná



Source: Prepared by the author (2023)

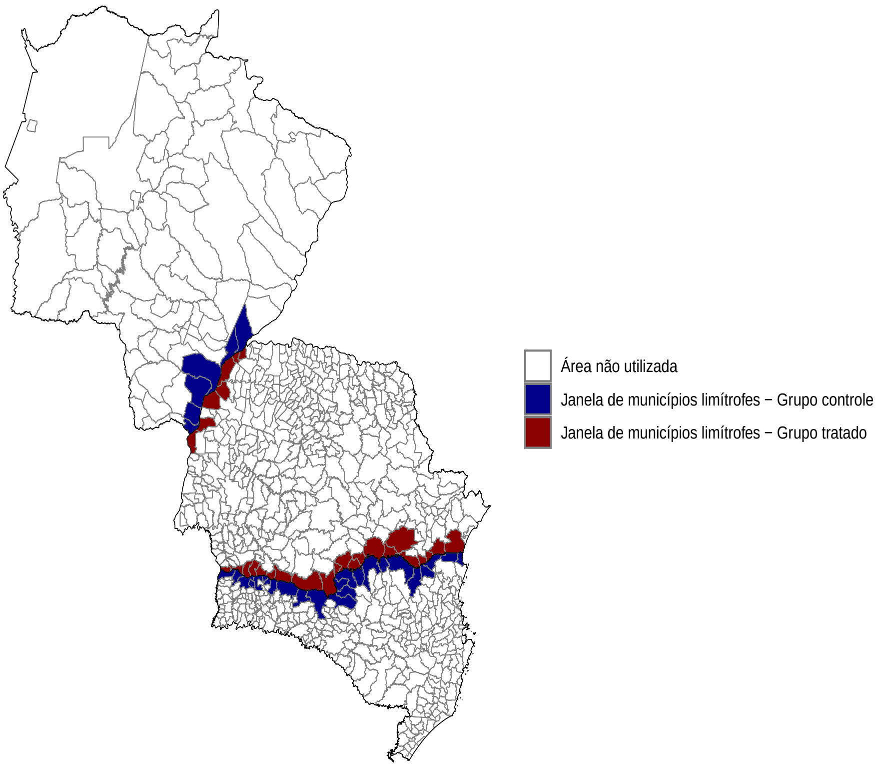
our results. In the end, we have a balanced panel of 17,167 workers being tracked over 12 semesters, starting in 2003 and ending in 2008. Table 1 in the appendix contains the descriptive statistics of the sample grouped by treatment status, with averages for all pre- and post-implementation semesters of the policy, as well as for the baseline. As can be observed, no significant changes in composition between the groups in terms of observable characteristics are found. The only highlight is the average December remuneration, whose difference between treated and control remains negative at baseline and in the pre-period, changing in the post-policy period.

3.2 Empirical Strategy

The empirical strategy is motivated by a sequence of works (DLR1, DLR2, DLR3, and JNR) that explore a border discontinuity design to address possible problems of spatial endogeneity present in previous studies' estimations. In DLR1 and DLR2, the authors construct a sample of pairs of contiguous American counties, where the minimum wage differs from one state to another, aiming to conceive a faithful counterfactual to estimate the effects of a minimum wage policy on labor market outcomes. JNR challenges the results of their predecessors, arguing that so-called multi-state commuting zones would be a better alternative to counties for absorbing local economic shocks.

In our case, we intend to use a sample of pairs of neighboring municipalities

Figure 2 – Set of pairs of municipalities in the treated and control groups



Source: Prepared by the author (2023)

that share a state border—where one state implemented the minimum wage and the other did not—in the construction of comparable treatment and control groups. In other words, we aim to isolate the effects of the minimum wage by circumventing bias problems present in previous methodological approaches on the effect of the minimum wage.

Regarding the identification strategy, we will use the method known as event study of differences-in-differences (DD), in which we estimate the parameter known as ATT (Average Treatment Effect on the Treated). Numerous textbooks in the field of causal inference introduce and delve into the details of this model (ANGRIST; PISCHKE, 2009; CUNNINGHAM, 2021; HUNTINGTON-KLEIN, 2021; PEIXOTO et al., 2012). In its canonical form, the DD design presents a basic configuration with two time periods—pre and post-intervention—and two groups—treated and control. Intuitively, it is named as such for representing two differences concerning the variable of interest. First, the temporal difference is taken, i.e., the difference in outcome between the post and pre-event periods (note that we consider event and intervention as synonyms here). Next, the difference between the groups of this first difference is taken.

As its main identification assumption, DD presents the concept of "parallel trends". This concept establishes that the trajectory of the variable of interest for the comparison group should statistically mimic the trajectory of the same for the treated group, in the absence of the intervention. Naturally, this assumption cannot be directly tested, but there are indirect ways to verify it through suggestive evidence. The lead coefficients, representing the anticipated effect of the policy, are the most usual test example in this type of design. Algebraically, we will have:

$$Y = \alpha Treated + \lambda Post + \beta Treated \times Post + \epsilon \tag{1}$$

where *Treated* is a binary variable that takes the value 1 for observations in the treatment group, and 0 otherwise. *Post* is a dummy that represents the temporal dimension of the intervention, taking the value 1 for all observations after the event. Our coefficient of interest is β, which captures the causal effect of the event on Y and comes from the interaction between *Treated* and *Post*. Putting it another way, β can take the following form:

$$\begin{aligned} \beta = [E(Y|Treated = 1, Post = 1) - E(Y|Treated = 1, Post = 0)] - \\ [E(Y|Treated = 0, Post = 1) - E(Y|Treated = 0, Post = 0)] \end{aligned} \tag{2}$$

As mentioned earlier, we will estimate an event study, which is an extension of the DD to capture the dynamic effect of the policy. Additionally, it is possible to perform the pre-event parallel trends test to verify the validity of the model. In this work, we will consider as "treated" all workers registered in the state that enacted the policy, who were active in the baseline period (i.e., in the period exactly before implementation) with remuneration lower than their respective wage range, and the case study here will be Paraná. The choice of this state is due to factors such as issues of intertemporal inconsistency between the definitions of occupation defined in the policy and the CBO's—cases of RJ and RS—as well as the absence of credible control groups—cases of SP and SC.

The main challenge of this strategy, as well stated by Dube e Lindner (2021), is due to the easy mobility of agents to move from a municipality with a state minimum wage in effect to the neighboring municipality, from the state that did not implement the policy, and vice versa. In light of this, we assume as a hypothesis that the migration flow between these municipalities is not relevant enough to bias our estimates, considering that the decision to migrate is not trivial for the income group we are analyzing—workers earning between the state minimum wage and the national minimum wage. The greatest weakness of this assumption, however, lies in the lack of clarity about the response of firms, given that the decision to migrate for them may be more advantageous in terms of profit maximization. It is possible, of course, to test this hypothesis, where we chose to explore it more deeply in future work.

What is intended is to identify the group of individuals who would be most directly affected by the intervention, in order to track their outcomes of interest m periods before and n periods after "receiving the treatment." For the control group, workers from neighboring municipalities on the state border, whose state did not implement the policy, and who also earned below their respective wage range (according to their occupations) at baseline, will be used.

The reduced form equation for our specification, which seeks to capture the causal effect of the state minimum wage on labor market outcomes, is defined in a two-dimensional panel configuration and takes the following form:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\tau=-6}^{-2} \beta_{1\tau} T_\tau \times D_i + \sum_{\tau=0}^{5} \beta_{2\tau} T_\tau \times D_i + \beta_3 D_i + X'_{ij}\gamma + \epsilon_{it} \tag{3}$$

where Y_{it} represents our variable of interest for the job i at time t. D_i is a dummy that takes the value 1 for jobs registered in municipalities of PR, and T_t are time period dummies for each period of the database. The coefficients of interest, which represent the dynamic causal effect of the policy on our dependent variable, are $\beta_{2\tau}$. The $\beta_{1\tau}$ are the model identification validation coefficients and correspond to the pre-intervention parallel trends test. The term X'_{ij} consists of a vector of characteristics of workers and firms at baseline (therefore invariant over time), whose descriptive statistics can be found in table 1 of section A of the appendix. Also, λ_t and α_i are, respectively, our time and individual fixed effects. Finally, ϵ_{it} represents our error term clustered at the state level, which is our treatment unit here, to circumvent problems of spatial autocorrelation.

Additionally, an alternative way to control for observables used here is the method known as Inverse Probability Weighting (IPW). Detailed by Hirano, Imbens e Ridder (2003) and different from propensity score matching, the procedure consists of weighting the observations by the inverse probability of their actual treatment assignment, conditional on a set of covariates. In other words, the estimation is performed so that the workers in the treatment group most similar to the workers in the control group have greater weight, and vice versa.

Finally, as our database only has information on income at an annual frequency during the analysis period, the estimation for the remuneration variable will be indexed to the year, while the other variables of interest will be indexed at the semiannual frequency. The variables of interest to be analyzed are:

i Probability of being unemployed at the end of the semester;

ii Probability of employee-initiated termination;

iii Probability of employer-initiated termination;

iv Probability of termination for other reasons;

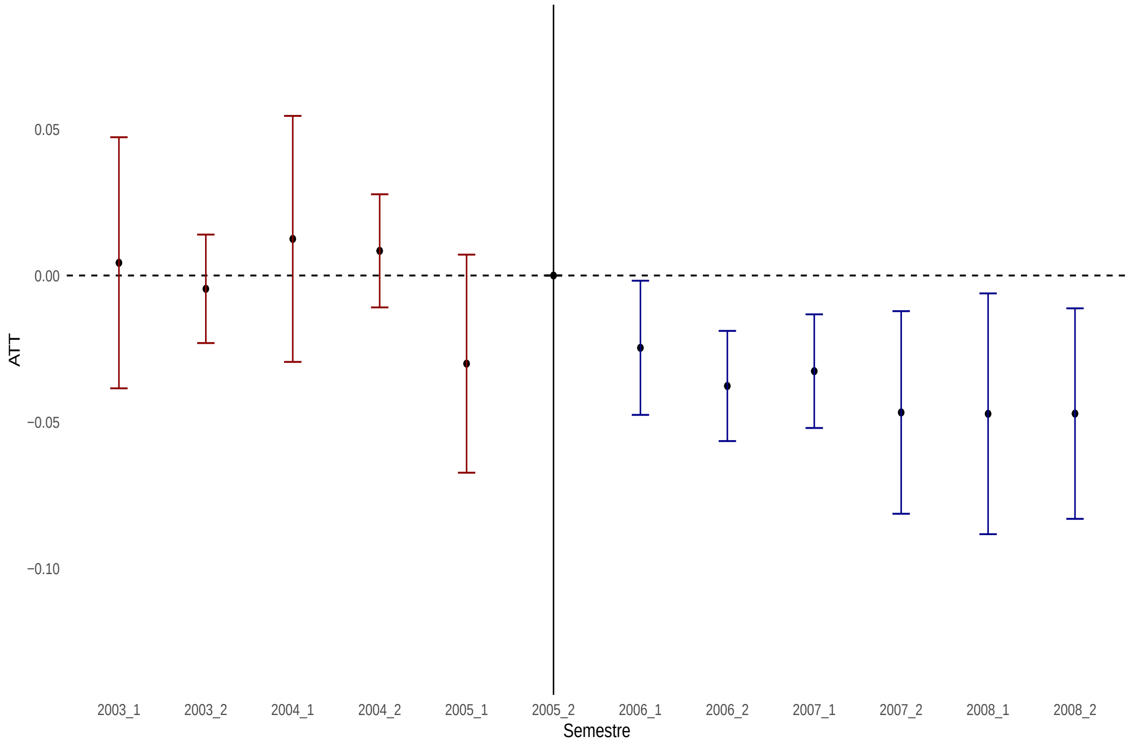
v December remuneration.

4 Results

In Table 2 of Section A in the appendix, the estimation results for our main specification are presented, both with and without controls for observable variables, and with individual and semester fixed effects. Figure 3 shows the event study analysis results for the dependent variable of the probability of termination at the end of the period, with individual and semester fixed effects. As observed, after the implementation of the policy, there is a substantial and statistically significant drop in the result, which persists in all subsequent periods. Specifically, in the first semester in which the policy took effect, there is a 2.47 percentage points decrease in the difference in the probability of being terminated between the treated and control groups. For comparison, considering the unconditional average of 51.1% for this variable in the five periods prior to the baseline, this result translates to a 5% decrease in the probability of termination at the end of the period. In the following periods, this negative effect intensifies, reaching 4.7 percentage points in 2008, corresponding to a 9.2% decrease relative to the unconditional average of 51.1%. Furthermore, we do not find statistical significance in the analysis of the pre-intervention coefficients, providing strong evidence of the validity of the parallel trends assumption in the difference-in-differences (DD) approach. The potential mechanisms driving this result will be further discussed and explored in the mechanism subsection that follows.

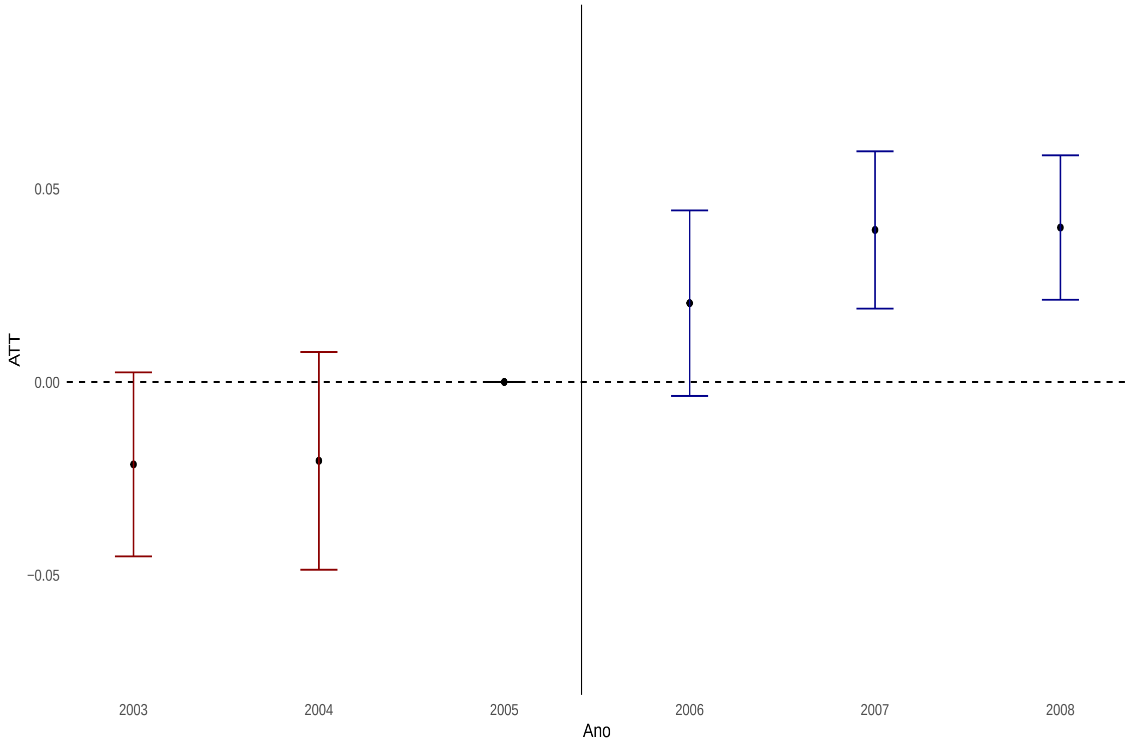
Additionally, Figure 4 presents the estimation of the annual wage variable for the subsample of workers with non-zero wages throughout the time frame, with individual and time fixed effects, and controls for covariates. In 2003 and 2004, no statistical differences were found between the groups; however, from 2006 onwards, the situation changes, with the coefficient shifting upwards and the confidence intervals moving away from the horizontal axis. Furthermore, in Section C of the appendix, we add density plots for the annual wage variable, grouped by treatment status, showing a clear shift in distribution between the treated and control groups after the policy. Lastly, Table 3 in Section A of the appendix reports the results of the Wald test for the joint significance of the pre-intervention coefficients. It provides both the Wald statistics and their p-values, representing the probability of rejecting the null hypothesis given a significance level. Considering a significance level of 5%, we conclude that both for the estimation without controls for observables and for the estimations with covariates and with IPW, we reject the null hypothesis that the pre-policy coefficients are statistically equal to zero, thus presenting a set of unfavorable evidence against the parallel trends assumption in DD.

Figure 3 – Effect on the Probability of Termination at the End of the Period



Note: Event study graph for the probability of termination at the end of the period with individual and time fixed effects for workers who were employed and earning below their respective wage floor at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 4 – Effect on December Remuneration



Note: Event study graph for December remuneration with individual and time fixed effects and controls for covariates for workers who were employed and earning below their respective wage floor at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

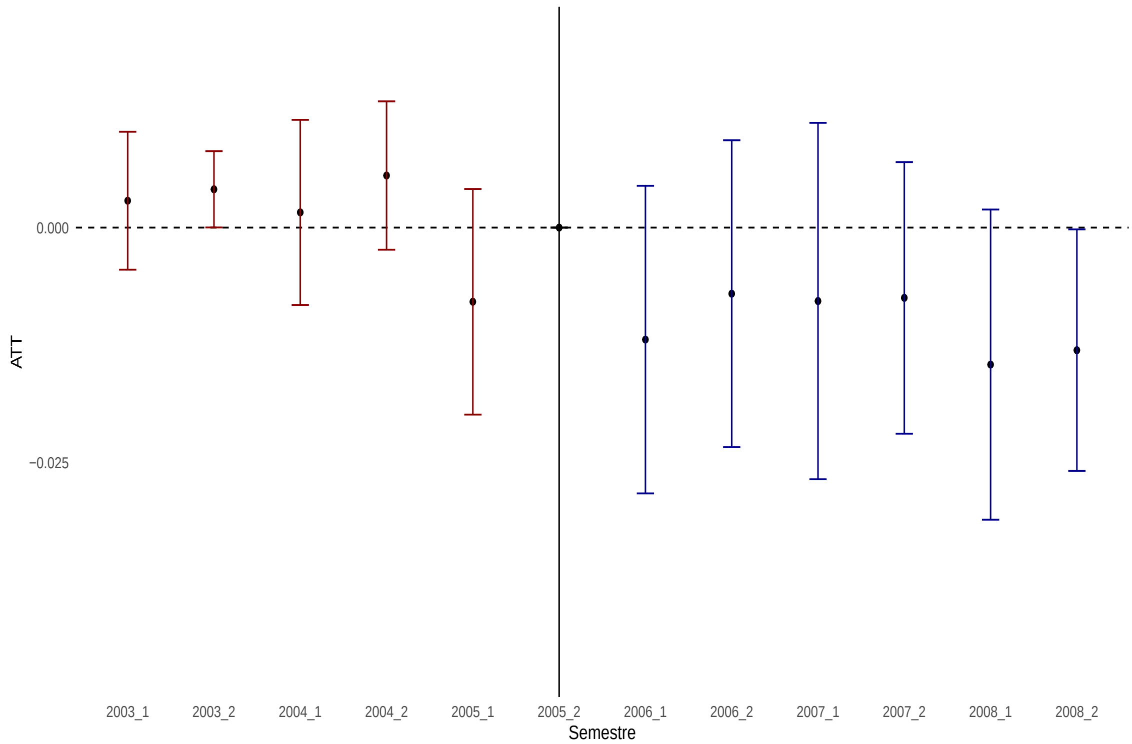
4.1 Mechanism

In the mechanism analysis, a plausible explanation - consistent with the literature - for our main result directs attention to the supply side in the labor market. In this scenario, with the wage increase induced by the policy, those workers who typically exhibit higher job turnover in pursuit of higher earnings are incentivized to alter this behavior due to the imposition of the wage floor. Another hypothesis might be associated with a reduced incentive for workers to accept agreements involving false dismissals, a phenomenon relatively common in Brazil. On the demand side, the found effect might be driven by increased dismissal costs related to the labor charges incurred by employers.

A way to test these possible channels is by using the reason for termination, which is a coded variable present in RAIS. In this variable, among the many available options, those of interest to us are: "employee-initiated termination without cause," "employer-initiated termination without cause," and "termination for other reasons," which we construct as dummies that take the value 1 if the respective condition is valid at the end of the period. Below, in Figures 5, 6, and 7, we have the event study estimates for them, with individual and period fixed effects and no controls for observables.

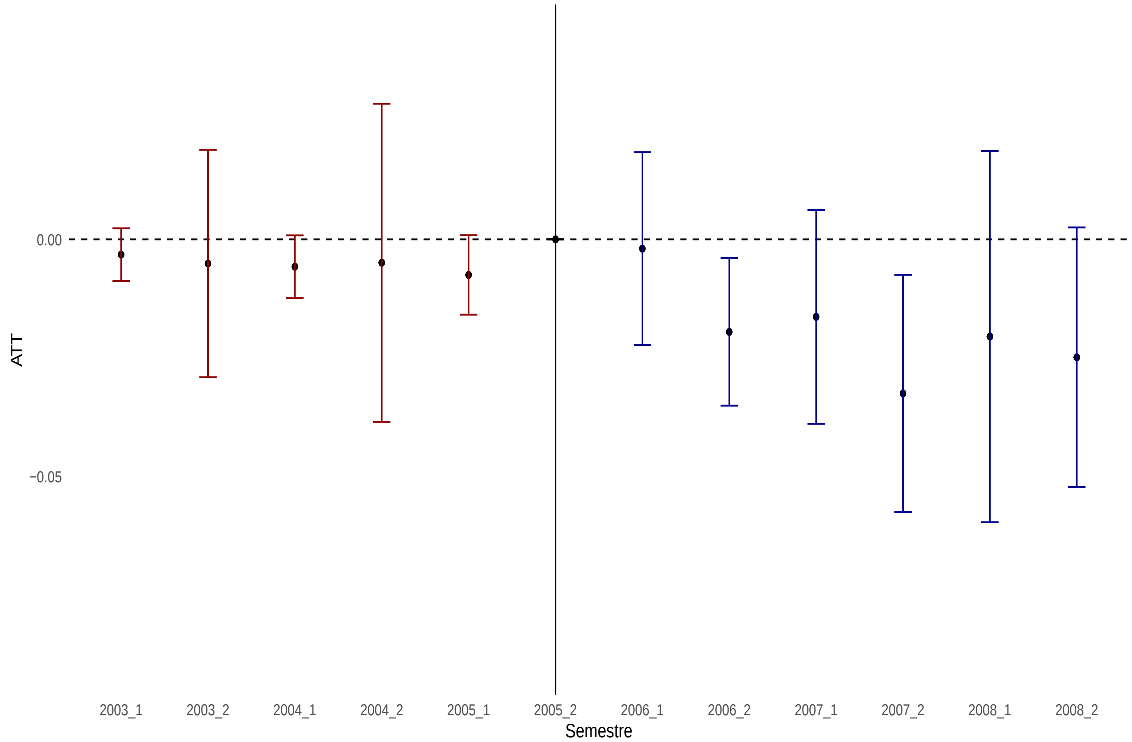
An important point to note, which may have interfered with the estimation results, is the presence of a seasonal component between semesters. For example, for the variable of the probability of employer-initiated termination, a significant increase in value dispersion can be observed in the 2nd semesters of each year. Regarding this, Corseuil et al. (2019) point out the significant increase in terminations in the last days of each year, showing that just from December 31, 2015, to January 1, 2016, terminations almost reached 700,000.

Figure 5 – Effect on the Probability of Quitting



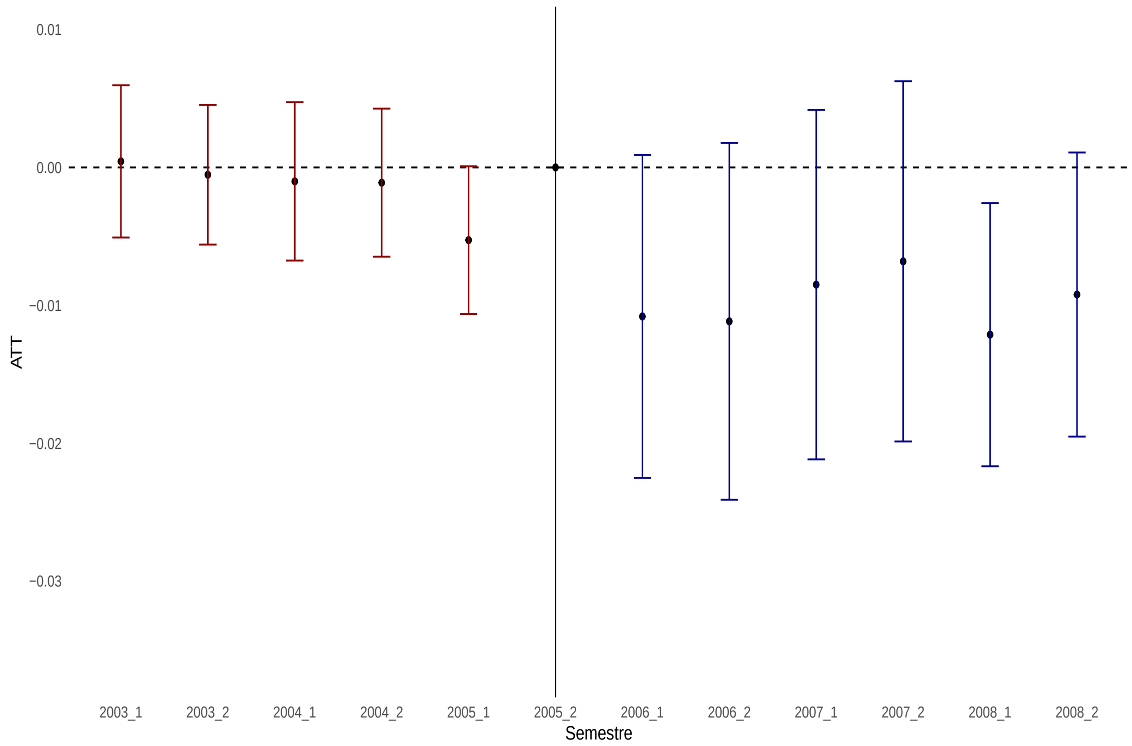
Note: Event study graph for the probability of employee-initiated termination at the end of the period with individual and time fixed effects for workers who were employed and earning below their respective wage floor at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 6 – Effect on the Probability of Being Fired



Note: Event study graph for the probability of employer-initiated termination at the end of the period with individual and time fixed effects for workers who were employed and earning below their respective wage floor at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 7 – Effect on the Probability of Termination for Other Reasons



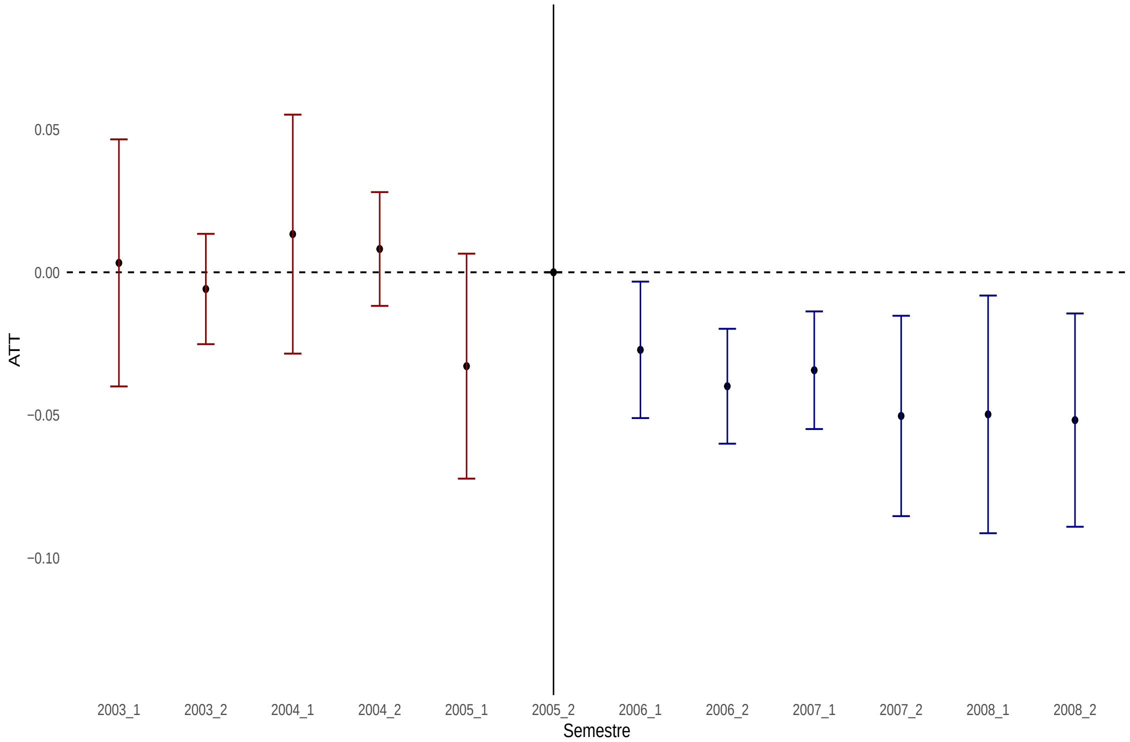
Note: Event study graph for the probability of termination for other reasons at the end of the period with individual and time fixed effects for workers who were employed and earning below their respective wage floor at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Although presenting a similar relative trajectory to that found in Figure 4, the results of the dynamic DD estimation here differ substantially in terms of statistical significance. For the probability of quitting at the end of the period, in Figure 5, we have parallel trends in almost all pre-periods and a permanent decline in the periods following the event, but it is only slightly statistically different from zero in the second semester of 2008. For the probability of being fired, shown in Figure 6, although the pre-intervention results are not significant, they exhibit an unusual pattern possibly associated with the end-of-year seasonality mentioned earlier. With intense variation in the scale of its standard errors, its pre-treatment estimates show small confidence intervals in the first semesters and large ones in the second semesters of each year. After the policy, however, this phenomenon disappears, and the coefficients show a clear decline, varying both in value dimension and confidence intervals. This results in negative and statistically significant values in our coefficient in the second semesters of each year after 2005 - except for the last semester of 2008 - and negative but statistically insignificant coefficients in the first semesters. Finally, for the probability of termination for other reasons, which includes all other codes present in the termination reason variable in RAIS, Figure 7 follows the same pattern as Figure 5, with parallel pre-trends and a post-policy decline, but without statistical significance for almost all periods.

4.2 Robustness

As a robustness check, estimations were carried out with a subsample of workers whose contractual weekly working frequency at baseline was exactly 44 hours. This choice was made to consider only those individuals with the maximum legal monthly working hours, as the wage floor is based on this reference. Figure 8 below shows the dynamic DD graph for the estimation of the probability of termination at the end of the period.

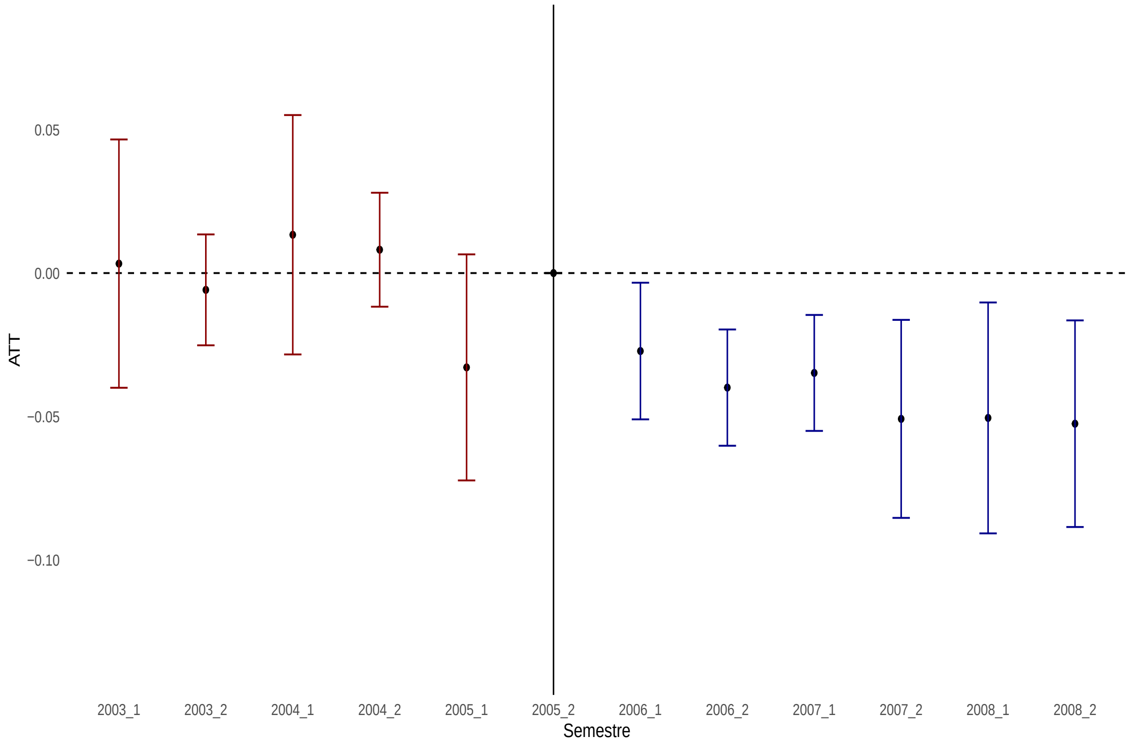
Figure 8 – Effect on the Probability of Termination at the End of the Period



Note: Event study graph for the probability of termination at the end of the period with individual and time fixed effects for the subsample of workers who had 44 contractual hours at baseline.
Source: Prepared by the author (2023)

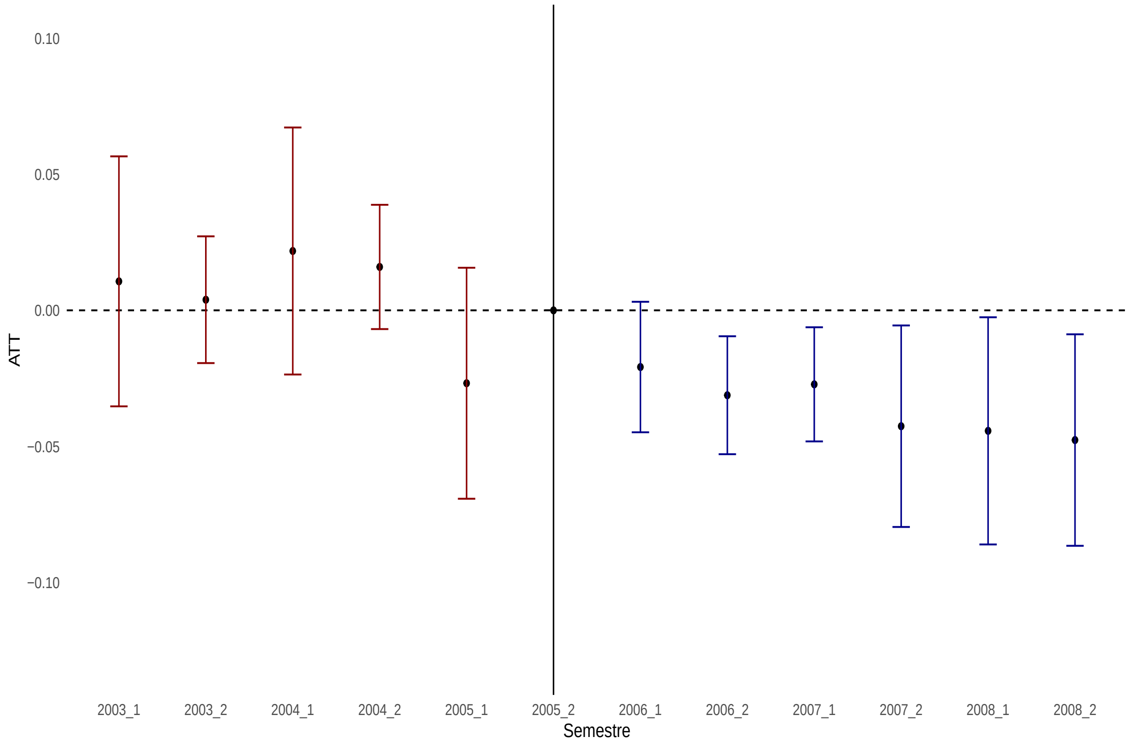
Additionally, alternative estimations were performed controlling for covariates, consisting of adding a vector of worker and firm characteristics at baseline to the regression, and another using IPW-type weighting conditional on these same covariates. Figures 9 and 10 present the respective results.

Figure 9 – Effect on the Probability of Termination at the End of the Period



Note: Event study graph for the probability of termination at the end of the period with individual and time fixed effects and covariate controls for the subsample of workers who had 44 contractual hours at baseline.
Source: Prepared by the author (2023)

Figure 10 – Effect on the Probability of Termination at the End of the Period



Note: Event study graph for the probability of termination at the end of the period with individual and time fixed effects and IPW weighting for the subsample of workers who had 44 contractual hours at baseline.
Source: Prepared by the author (2023)

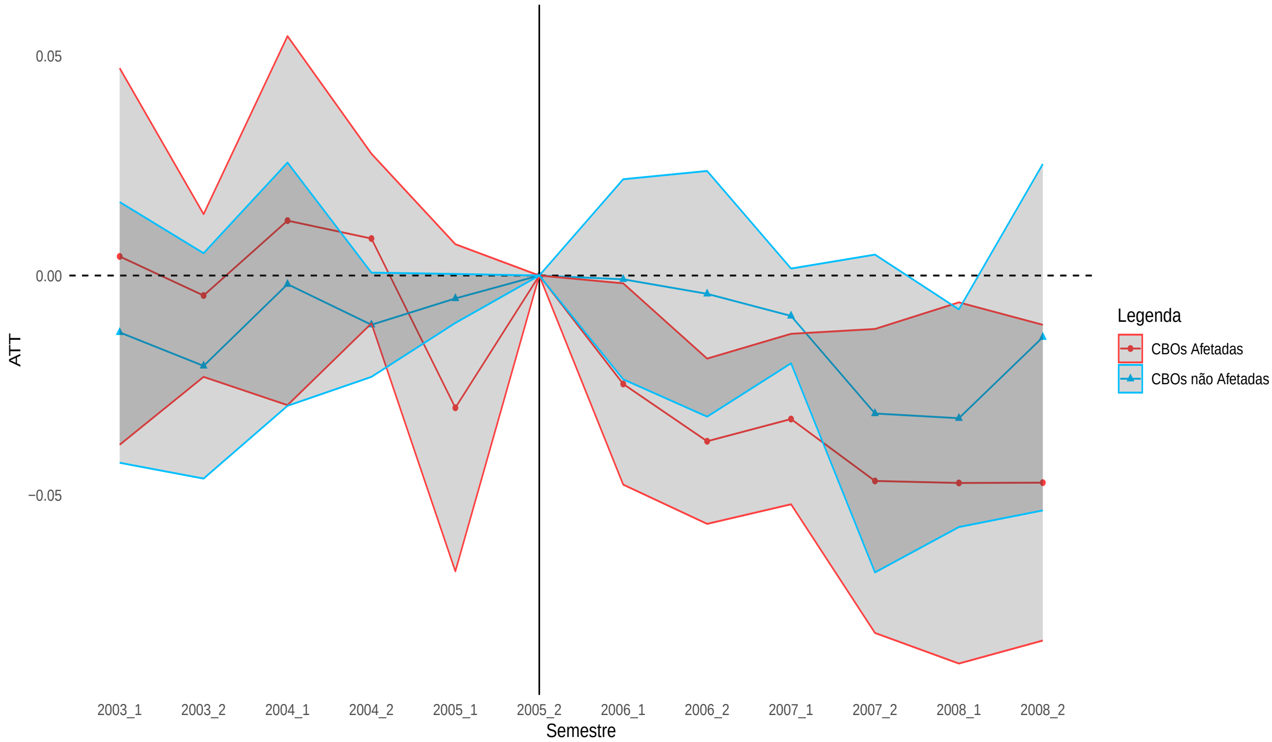
From the above graphs, we can conclude that our results remain robust, indicating the quality of our counterfactual group.

4.3 Placebo

Additionally, to verify if our result is genuinely driven by the policy, we conducted a placebo estimation with an alternative sample composed of workers from border municipalities with CBOs not affected by the policy and earning below the highest wage floor - to homogenize the worker samples - at baseline.

Below, in Figure 10, we have the dynamic DD estimation results for both the main sample and the placebo sample.

Figure 11 – Effect on the Probability of Termination at the End of the Period for Affected and Unaffected CBOs



Note: Event study graph for the probability of termination at the end of the period with individual and time fixed effects, separated by groups of workers with CBOs at baseline that were and were not affected by the policy.
Source: Prepared by the author (2023)

As we can see, the results for the unaffected CBO sample (in blue) show no statistical effect for almost all years, and the point estimates do not show the same signs as the estimates for the main sample (in red), providing strong additional evidence that the effect found in the sample of workers with affected CBOs is a causal effect.

Lastly, in Section B of the appendix, unconditional mean graphs of the dependent variables from all the estimations presented above can be found.

5 Conclusion

Using a sample of pairs of border municipalities between the treated state and untreated states, this study aimed to infer the causal effect of implementing a state minimum wage on various labor market variables. Beyond the significant results found, there are still many avenues to explore, such as an alternative robustness check using pairs of border microregions, similar to what was done by JNR, analyzing heterogeneity by constructing a treatment intensity variable representing the distance between remuneration and the minimum wage at baseline, as well as estimations at a more aggregate level focusing on the effects on firms. Additionally, another possibility is to further explore the case of São Paulo, which implemented the policy one year after Paraná but, in this design, did not have comparable control groups.

In the end, despite all this, our results contribute to the extensive recent literature on minimum wage, aligning with much evidence that indicates the absence of adverse effects on employment, based on a well-established strategy and a credible counterfactual group. Certainly, these findings constitute a valuable input for public authorities in maintaining this type of policy.

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APPENDIX A – TABLES

Table 1 – Descriptive Statistics

	Control - Baseline	**Treatment - Baseline**	**Control - Pré**	**Treatment - Pré**	**Control - Pós**	**Treatment - Pós**
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Firm Size	3.1	3.1	1.8	1.9	2.6	2.8
	(2.0)	(1.9)	(2.0)	(2.0)	(1.9)	(1.9)
Employment Duration	19.7	20.1	13.3	13.0	25.6	27.0
	(28.0)	(25.9)	(25.3)	(23.1)	(29.9)	(27.6)
% of Men	0.6	0.5	0.6	0.5	0.6	0.5
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
% w/ Incomplete Primary Education	0.3	0.3	0.3	0.3	0.3	0.3
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.4)
% of Youth	0.4	0.4	0.4	0.4	0.4	0.4
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
December Wage	373.6	362.1	203.9	186.7	330.6	344.0
	(57.5)	(52.1)	(236.5)	(201.1)	(461.6)	(361.2)
N	9,171	7,996	9,171	7,996	9,171	7,996

Source: Prepared by the author (2023)

Table 2 – Effect on the Probability of Being Discharged at the End of the Period

Dependent Variable:	Prob. of Discharge					
	(1)	(2)	(3)	(4)	(5)	(6)
Semester -6 x Treatment	0.0043	0.0033	0.0043	0.0033	0.0121	0.0107
	(0.0219)	(0.0221)	(0.0219)	(0.0221)	(0.0232)	(0.0235)
Semester -5 x Treatment	-0.0045	-0.0059	-0.0045	-0.0059	0.0055	0.0039
	(0.0094)	(0.0099)	(0.0095)	(0.0099)	(0.0113)	(0.0119)
Semester -4 x Treatment	0.0125	0.0134	0.0125	0.0134	0.0213	0.0218
	(0.0214)	(0.0214)	(0.0214)	(0.0213)	(0.0231)	(0.0232)
Semester -3 x Treatment	0.0084	0.0081	0.0084	0.0081	0.0163	0.0160
	(0.0099)	(0.0102)	(0.0098)	(0.0101)	(0.0113)	(0.0117)
Semester -2 x Treatment	-0.0301	-0.0329	-0.0301	-0.0329	-0.0241	-0.0268
	(0.0190)	(0.0201)	(0.0190)	(0.0201)	(0.0205)	(0.0217)
Semester 0 x Treatment	-0.0247**	-0.0272**	-0.0247**	-0.0272**	-0.0183	-0.0208
	(0.0117)	(0.0122)	(0.0116)	(0.0121)	(0.0117)	(0.0122)
Semester 1 x Treatment	-0.0377***	-0.0399***	-0.0377***	-0.0399***	-0.0290**	-0.0312**
	(0.0096)	(0.0103)	(0.0097)	(0.0103)	(0.0102)	(0.0111)
Semester 2 x Treatment	-0.0327***	-0.0343***	-0.0331***	-0.0348***	-0.0260**	-0.0272**
	(0.0099)	(0.0105)	(0.0098)	(0.0103)	(0.0101)	(0.0107)
Semester 3 x Treatment	-0.0467**	-0.0503**	-0.0472**	-0.0508***	-0.0393**	-0.0426**
	(0.0176)	(0.0179)	(0.0174)	(0.0176)	(0.0185)	(0.0189)
Semester 4 x Treatment	-0.0472**	-0.0498**	-0.0478**	-0.0505**	-0.0420*	-0.0443*
	(0.0210)	(0.0212)	(0.0203)	(0.0205)	(0.0211)	(0.0213)
Semester 5 x Treatment	-0.0471**	-0.0518**	-0.0478**	-0.0525**	-0.0431**	-0.0477**
	(0.0183)	(0.0191)	(0.0177)	(0.0184)	(0.0191)	(0.0198)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Semester Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	Yes	Yes	No	No
IPW	No	No	No	No	Yes	Yes
44 Contract Hours	No	Yes	No	Yes	No	Yes
Obs.	206,004	200,256	206,004	200,256	206,004	200,256
R^2	0.38766	0.38748	0.39033	0.39013	0.38957	0.38930
R^2 *Within*	0.00083	0.00093	0.00519	0.00525	0.00076	0.00085

Standard errors clustered at the UF level in parentheses
*Significance codes: ***: 0.01, **: 0.05, *: 0.1*
Source: Prepared by the author (2023)

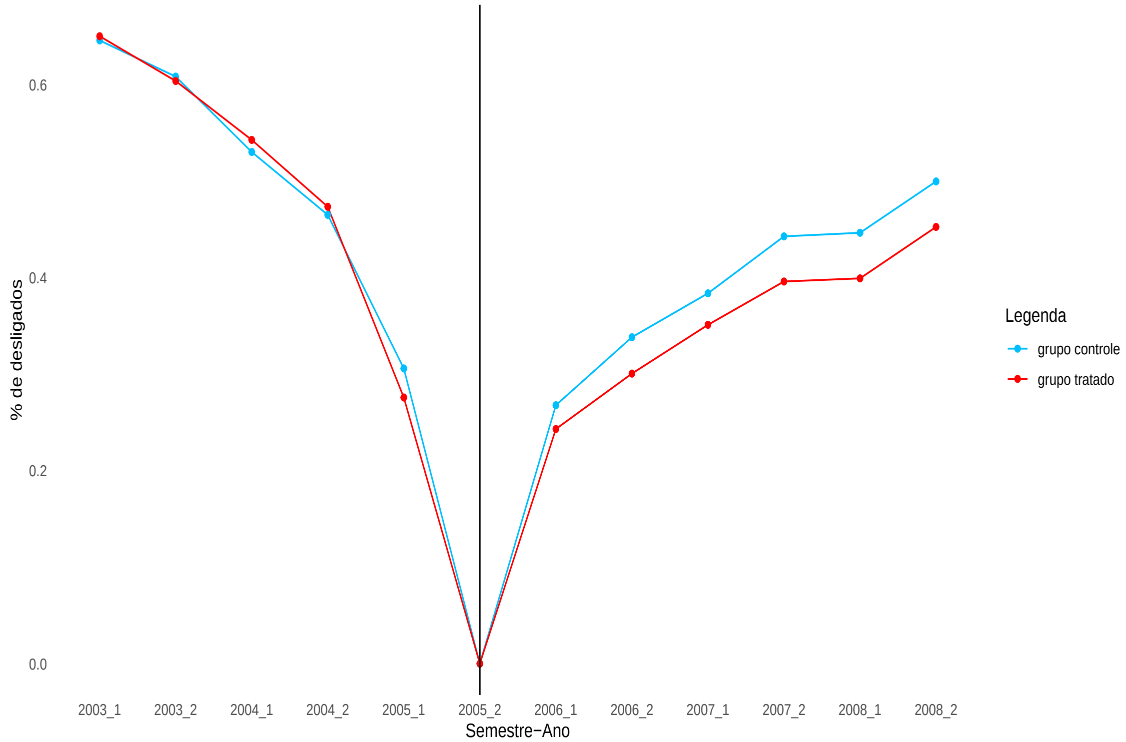
Table 3 – Wald Test for Parallel Trends Pre-Intervention

	(1)	(2)	(3)	(4)	(5)	(6)
Wald Statistic	1613.158	1856.529	1667.05	1951.054	924.3725	1018.776
P-Value	0	0	0	0	0	0
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Semester Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	Yes	Yes	No	No
IPW	No	No	No	No	Yes	Yes
44 Contract Hours	No	Yes	No	Yes	No	Yes

Source: Prepared by the author (2023)

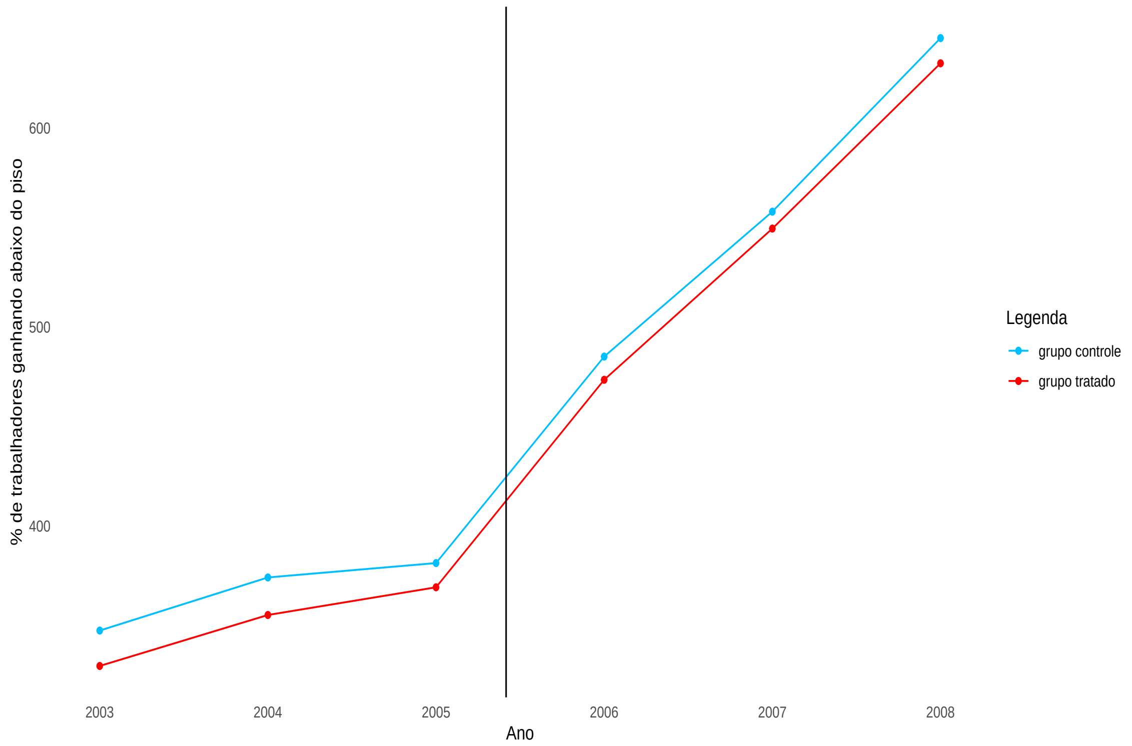
APPENDIX B – UNCONDITIONAL AVERAGES

Figure 12 – Probability of Discharge for Employees at Baseline



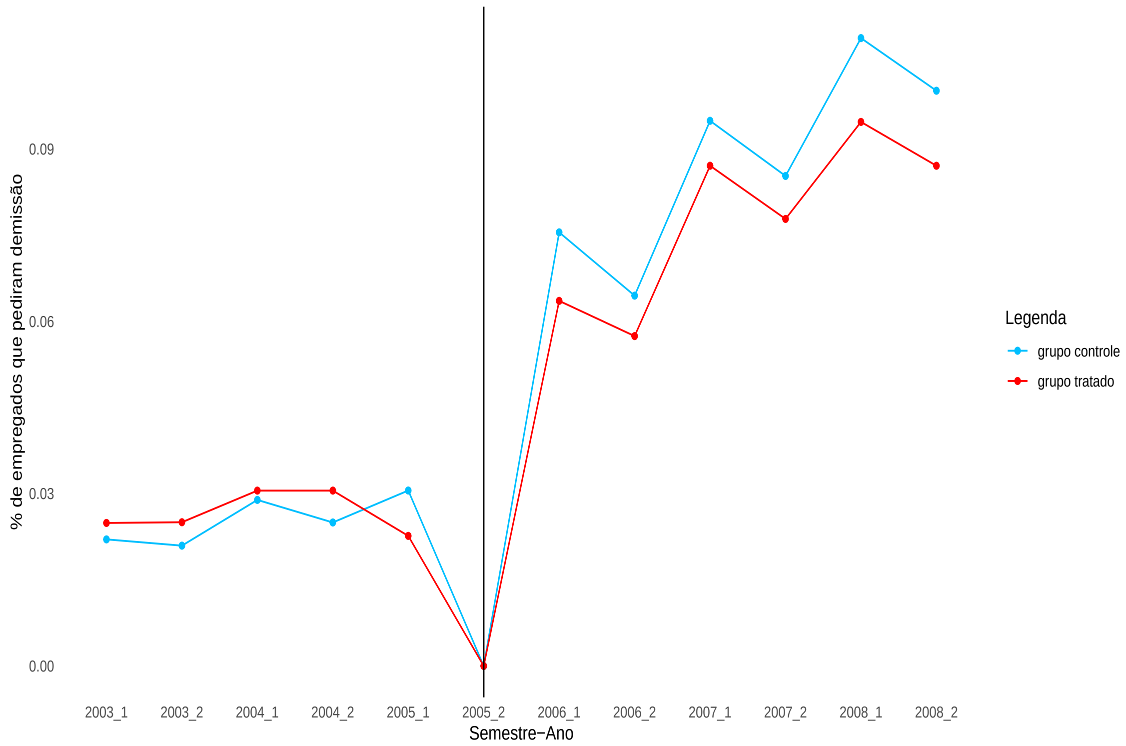
Note: Unconditional parallel trends chart of the probability of being discharged at the end of the period for workers who were employed and earning below the respective minimum wage range at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 13 – Average December Earnings for Employees Across the Time Cut



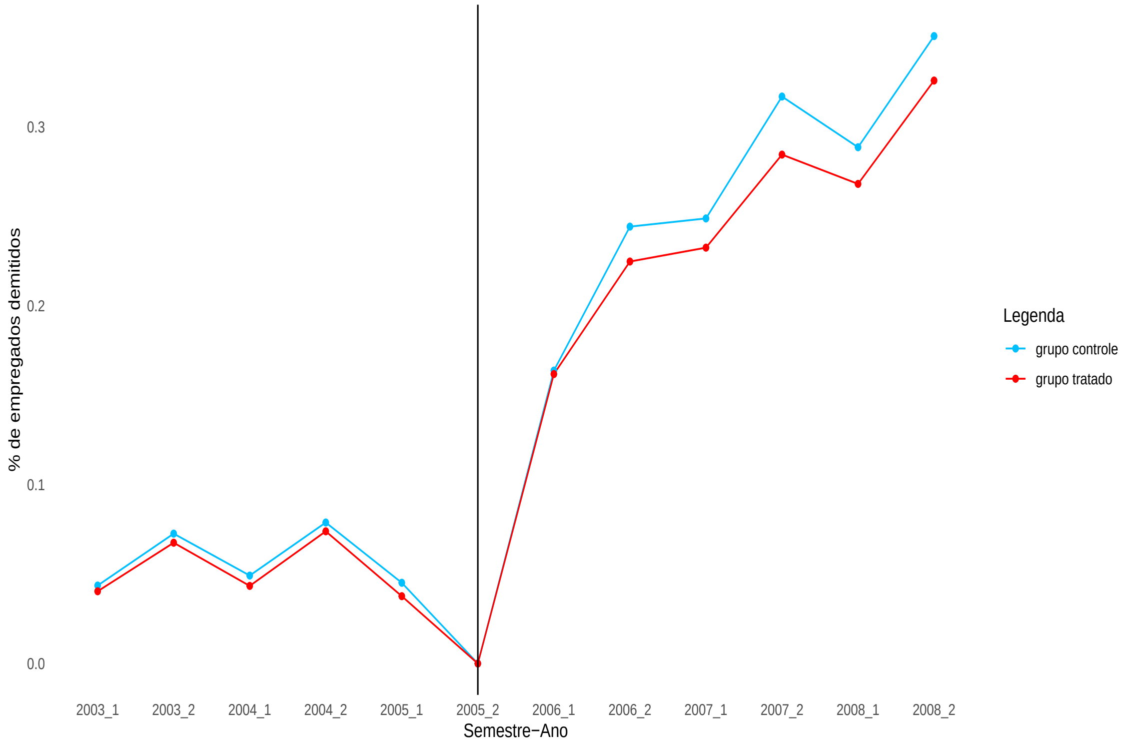
Note: Unconditional parallel trends chart of the average December earnings for workers who were employed and earning below the respective minimum wage range at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 14 – Probability of Quitting for Employees at Baseline



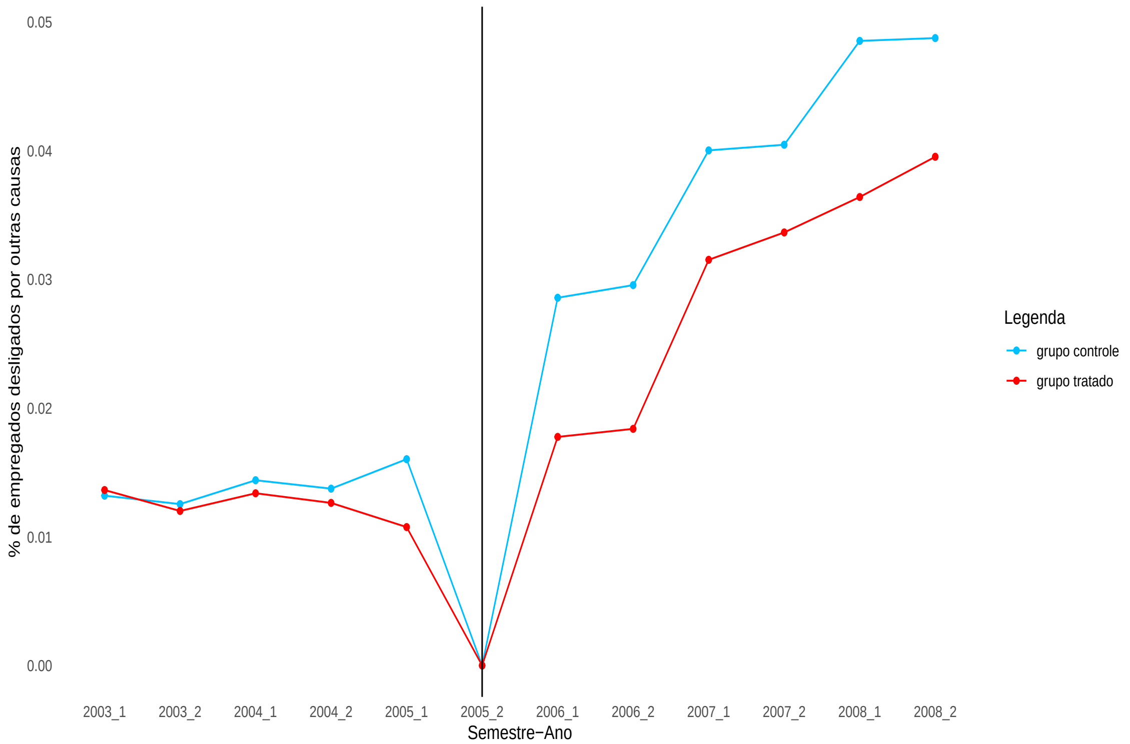
Note: Unconditional parallel trends chart of the probability of quitting by the employee at the end of the period for workers who were employed and earning below the respective minimum wage range at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

Figure 15 – Probability of Being Fired for Employees at Baseline



Note: Unconditional parallel trends chart of the probability of being fired by the employer at the end of the period for workers who were employed and earning below the respective minimum wage range at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

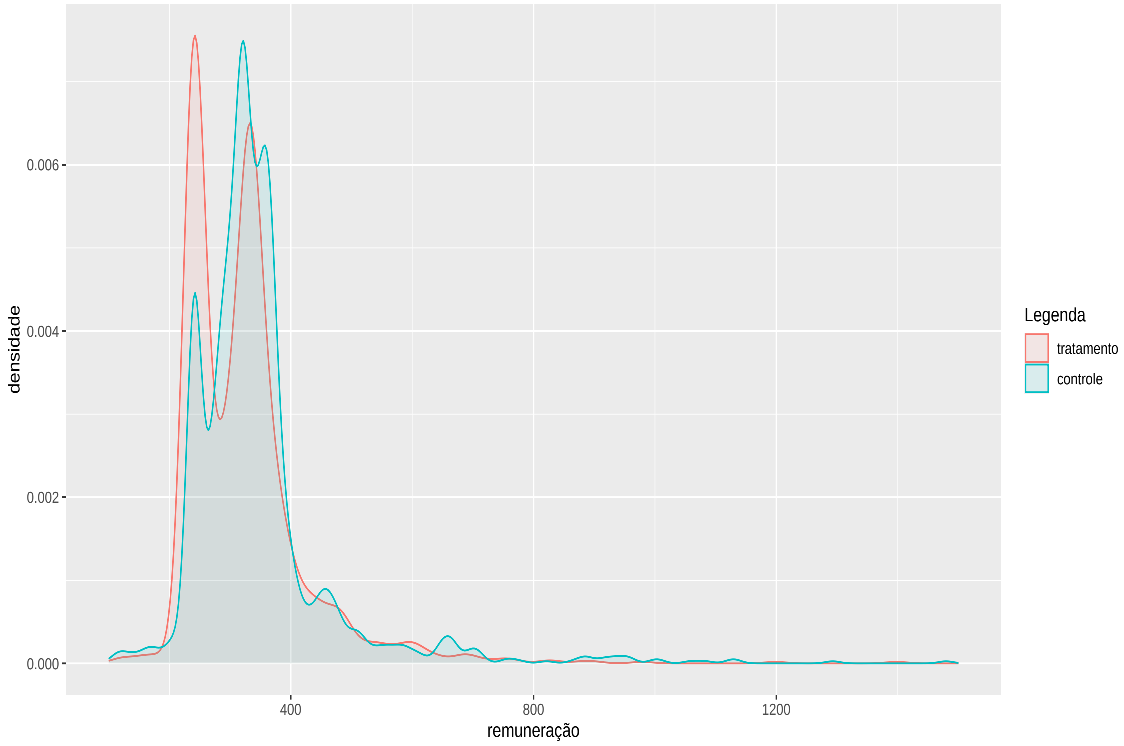
Figure 16 – Probability of Discharge for Other Reasons for Employees at Baseline



Note: Unconditional parallel trends chart of the probability of discharge for other reasons at the end of the period for workers who were employed and earning below the respective minimum wage range at baseline, regardless of being in the treatment or control group.
Source: Prepared by the author (2023)

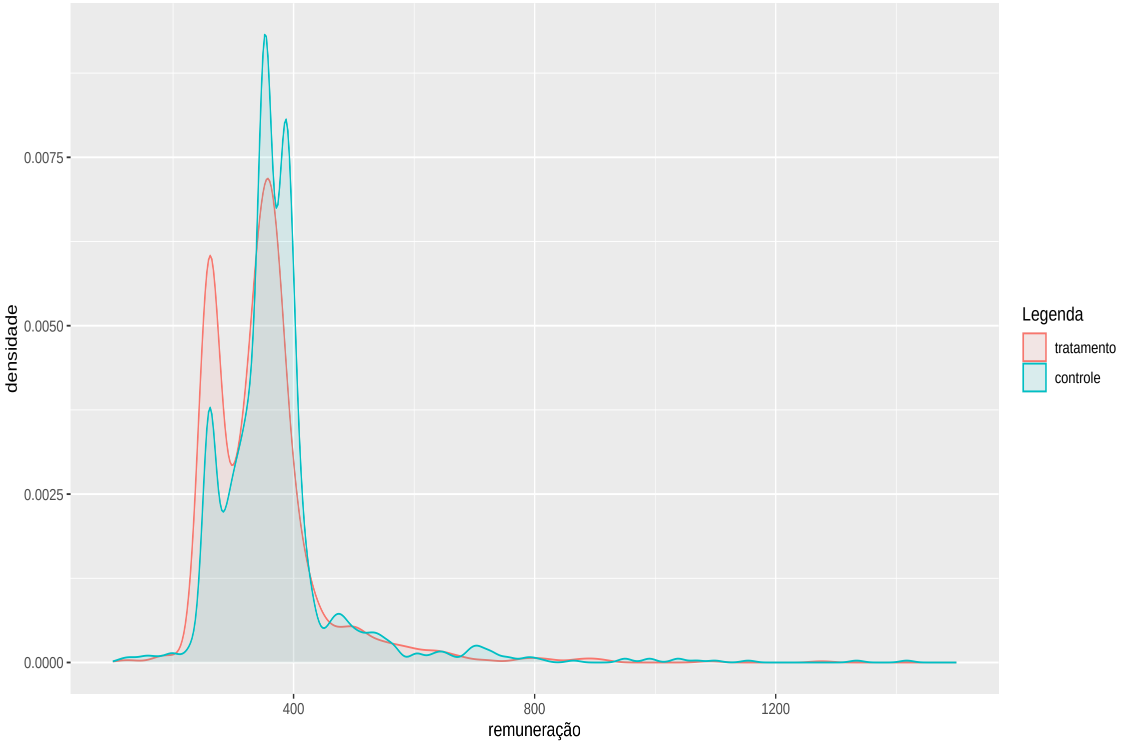
APPENDIX C – ADDITIONAL FIGURES

Figure 17 – Distribution of December Earnings Between Treated and Control Groups in 2003



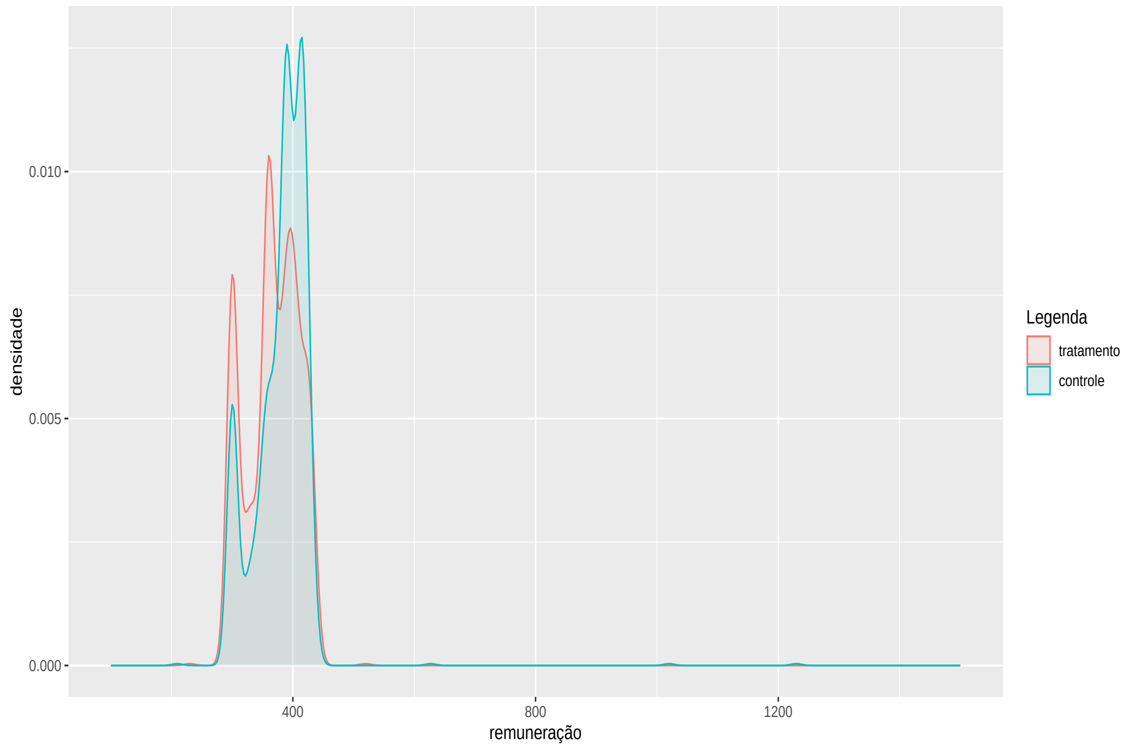
Source: Prepared by the author (2023)

Figure 18 – Distribution of December Earnings Between Treated and Control Groups in 2004



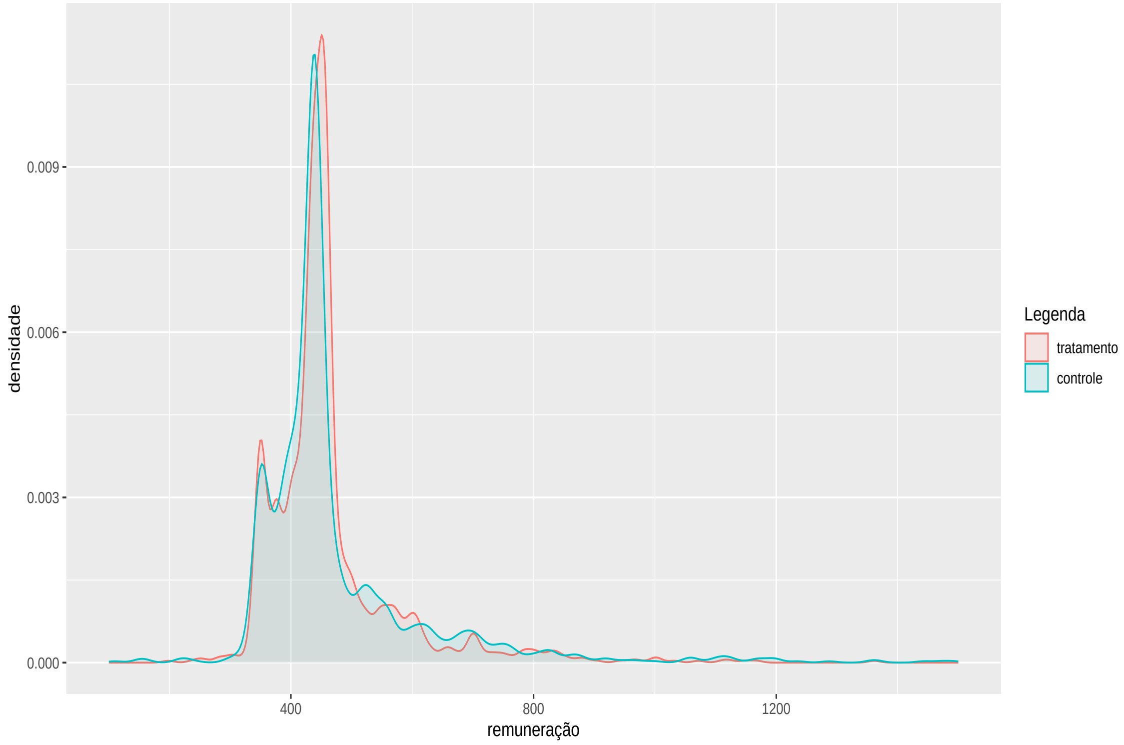
Source: Prepared by the author (2023)

Figure 19 – Distribution of December Earnings Between Treated and Control Groups in 2005



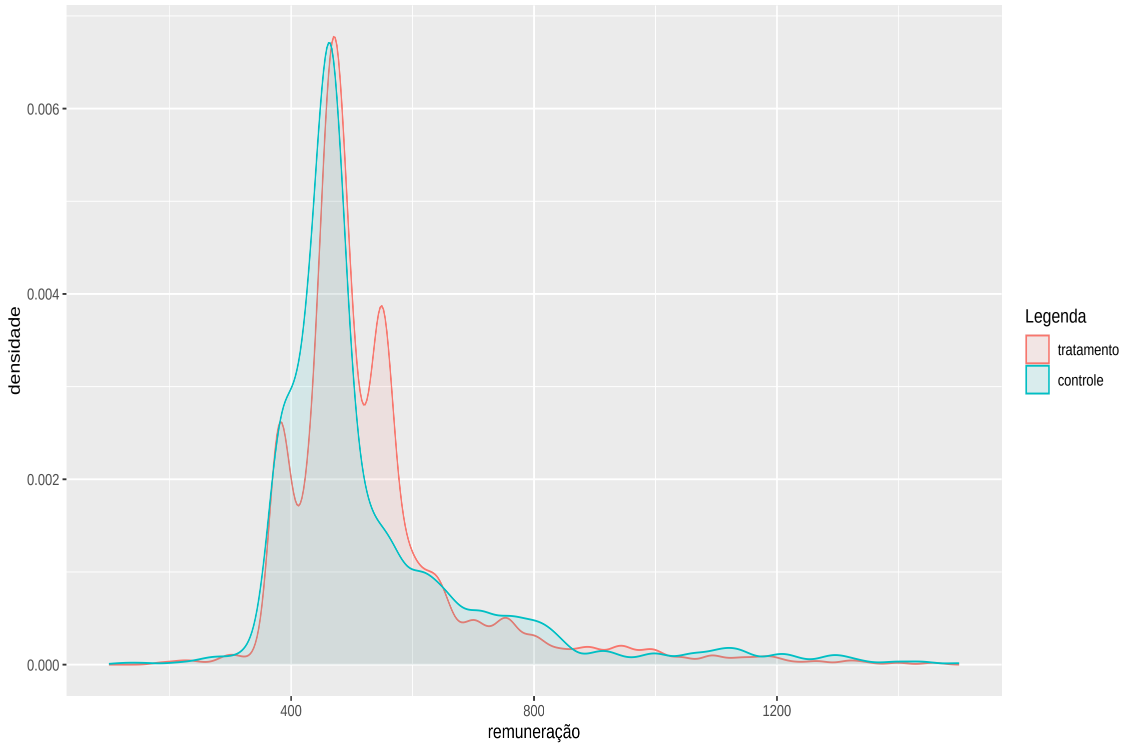
Source: Prepared by the author (2023)

Figure 20 – Distribution of December Earnings Between Treated and Control Groups in 2006



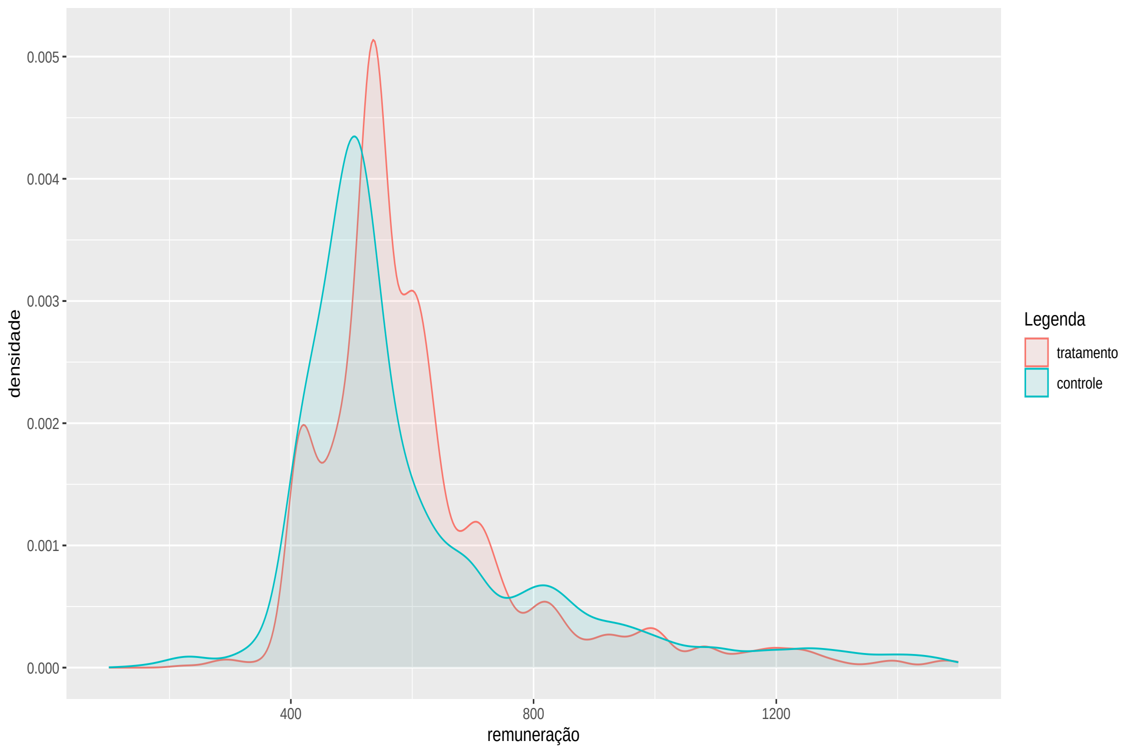
Source: Prepared by the author (2023)

Figure 21 – Distribution of December Earnings Between Treated and Control Groups in 2007



Source: Prepared by the author (2023)

Figure 22 – Distribution of December Earnings Between Treated and Control Groups in 2008



Source: Prepared by the author (2023)