Does Labor Market Concentration Decrease Wages? Evidence from a Retail Pharmacy Merger

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Abstract

Increasing labor market concentration is a topic of recent policy concern. However, there is a lack of direct evidence showing how concentration affects workers. In this paper, I study the effects of a large merger in the retail pharmacy sector in Brazil on workers' wages. I use a matched employer-employee dataset from 2007 to 2018 that allows me to follow workers and establishments over time. The effect of concentration, or market power, is estimated using a difference-in-differences (DiD) approach that compares the labor market effects of the merger in regions in which firms overlapped and concentration increased relative to those in which it didn't. To account for composition effects, I implement a DiD estimator that includes establishment and worker fixed effects. I find that increasing market power lowers wages, but less than previously thought, for two reasons. First, failing to account for composition effects biases estimates of the effects of concentration. Second, the negative labor market effects of a merger are offset by competitors' responses. The results show a great deal of heterogeneity across occupations and worker tenure, which highlights the benefits of studying a particular industry.

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1 Introduction

The fall in the labor share and recent increase in inequality (Elsby et al., 2013; Karabarbounis and Neiman, 2013; Piketty and Zucman, 2014) have led economists and policymakers to consider whether increasing labor market concentration has decreased wages.[1] This argument is supported by indirect evidence of the monopsonistic behavior of firms, such as the small employment effects of minimum wage increases (Card and Krueger, 1994; Manning, 2011; Azar et al., 2019); the small labor supply elasticity estimates (Falch, 2010; Staiger et al., 2010; Matsudaira, 2014); and the presence of anticompetitive agreements (Krueger and Ashenfelter, 2018; Starr et al., 2021). However, direct evidence that increased concentration reduces wages is scarce.

This paper estimates the effect of labor market concentration on wages by analyzing the effect of a large merger in Brazil's retail pharmacy sector on labor market outcomes. Mergers can lead to greater concentration but can also generate synergies. To separately identify the effect of concentration, I take advantage of the fact that the merger I analyze increased concentration in some markets but not others. The effect of market power is estimated using a difference-in-differences (DiD) approach that compares the labor market effects of the merger in regions in which concentration increased relative to those in which it didn't. To account for composition effects, I implement a DiD estimator that includes establishment and worker fixed effects and estimate the effects of the merger separately by occupation. Finally, I analyze how competitors respond to the merger, which is another important determinant of overall labor market outcomes.

The data used in this analysis have many advantages. I use a matched employer-employee database that covers the universe of formal employees in Brazilian companies. The data are structured as a panel between 2007 and 2018, which allows me to follow establishments and workers over time. With this data, therefore, it is possible to assess how the merger affected the wages of workers in the firms that merged, including both incumbents and new entrants. I can also investigate the responses of competing firms. In all cases, the effects can be estimated including fixed effects to rule out composition effects. Lastly, because informality levels in the retail pharmacy level are much lower than in other sectors (partly because it is a highly regulated sector), the results capture the relevant population effects.

[1]For example, the following passages from the Executive Order by President Joseph Biden of July 2021 state: "It is the policy of my Administration to enforce the antitrust laws to combat the excessive concentration of industry, the abuses of market power, and the harmful effects of monopoly and monopsony—especially as these issues arise in labor markets" and "Consolidation has increased the power of corporate employers, making it harder for workers to bargain for higher wages and better work conditions."

I find that labor market concentration lowers wages, but the effects are smaller than prior estimates in the literature. This difference is due to composition effects. Without accounting for composition, I find that the wages of pharmacists fell by 7.9% and those of salespeople by 0.7%. These estimates are very similar to those of Prager and Schmitt (2021) in the US, despite the differences in setting and identification strategy.[2] However, when firm and worker fixed effects are included, the wages of pharmacists only fall by 2.6%. This suggests that the overall decline in wages was partly due to churning among pharmacists: After the merger, newly hired pharmacists were of lower quality, as measured by their estimated fixed effects. In contrast, the wages of salespeople fall by 3.7%. These results show that failing to account for composition effects biases estimates of the effects of concentration. Overall, my estimates suggest that the elasticity of wages with respect to labor market concentration ranges from -0.34 to -0.1 for pharmacists and from -17.5 and -0.13 for salespeople, depending on the labor market definition used to compute concentration measures. The large variation in elasticity for salespeople demonstrates the relevance of properly defining labor markets. The effects of the merger on employment are not precisely estimated, and I cannot rule out a significant decline in employment for both occupations.

The effects of mergers on competing firms are theoretically ambiguous and depend on whether firms are strategic complements or substitutes. I find that in markets in which concentration increased and wages in merging firms decreased, competing firms responded by increasing employment. Even though competing firms hire one more salesperson after the merger, the wages of salespeople at competing firms still fall by 3.3%. This suggests that firms are strategic substitutes—i.e., they respond to other firms' hiring strategies by doing the opposite of what their competitors do. This new finding helps explain why the estimated effects are modest, despite the fact that concentration increased: the labor market effects of a merger are limited by competitors' responses. If firms were instead strategic complements, we would likely observe a more sizeable wage decrease for all workers after the merger. This result implies that to make predictions about the effects of mergers, knowledge of how firms compete is necessary.

The results are counterintuitive, in that ex ante we expect pharmacists to have fewer outside options relative to salespeople, and thus face larger wage declines when concentration increases. To explain this, I estimate the effects of the merger on incumbents and newly hired workers separately. I find that the wages of incumbent pharmacists do not fall, but the wages of newly hired pharmacists do. These results are consistent with the higher unionization rate

[2] Prager and Schmitt (2021) analyze the effects of many mergers, not just one. They analyze hospital mergers and find that the wages of nursing and pharmacy workers fall by 6.8% in locations in which mergers increased concentration. They do not find evidence of differences in wage growth for unskilled workers.

of pharmacists in Brazil.[3] Unions can protect incumbent workers by negotiating wage floors and annual wage increases. However, in a scenario in which wage floors are not binding—as is the case for merging firms—unions do less for newly hired workers. This explains why the wages of pharmacists don't fall as much as expected.

The finding that wages of salespeople fall suggests that the preconceived idea that low-skill workers have greater outside options is inaccurate; salespeople working at drugstores may have some degree of firm- or industry-specific human capital or stronger preferences to work at a drugstore.[4] To assess this, I analyze data on mobility and show that 24% of salespeople employed in drugstores move to another drugstore when they switch jobs. If they were moving randomly across jobs with the same occupation title, this number should be closer to 5%. These preferences might be even more specific: Individuals working at large pharmacy chains are 20% more likely to move to another pharmacy chain than individuals working at small pharmacies. Hence, labor markets for low-skilled workers might be finer and more concentrated than they appear to be.

This paper makes two contributions to the literature. The first and most important contribution is implementing an estimator that accounts for composition effects. To my knowledge, I am the first to combine two widely used empirical approaches: DiD regression and two-way fixed-effects regressions (usually referred to as AKM to reflect the work of Abowd, Kramarz, and Margolis, 1999). The AKM approach consists of estimating a log wage regression with worker and firm fixed effects and is generally used to measure the contributions of workers and firms to earnings dispersion (Card, Heining, and Kline, 2013; Lamadon et al., 2021; Song et al., 2018, among many others). The method requires the use of large datasets in which we observe the same workers across firms, since this is the only consistent way to separately identify worker and firm fixed effects. I show that it is feasible to use this method to control for changes in the unobserved characteristics of workers in a DD empirical strategy.

A growing literature examines the overall labor market effects of mergers and acquisitions (Brown and Medoff, 1988; McGuckin and Nguyen, 2001; Li, 2012; DePasquale, 2018; Todd and Heining, 2020; He and Maire, 2020; Gehrke et al., 2021; Lagaras, 2020). While these papers find mixed results on wages, they mostly agree that merging firms restructure their

[3]In the 2013 Brazilian household survey, 34% of pharmacists working in drugstores reported being unionized. Only 9% of salespeople working in drugstores reported being unionized (PNAD, 2013).

[4]Neal (1995); Parent (2000); Poletaev and Robinson (2008); Kambourov and Manovskii (2009); Sullivan (2010) and many others present evidence that workers accumulate firm-, occupation-, and industry specific human capital. With upward an slopping firm-specific labor supply, firms may act as monopsonists. The models in Card et al. (2018) and Lamadon et al. (2021) have workers with heterogeneous preferences over non-wage job characteristics that view firms as imperfect substitutes.

labor force. For example, Gehrke et al. (2021) show that acquiring firms hire younger workers after the merger. I contribute to this literature by showing that merging firms may also restructure their labor force in unobservable ways, as captured by workers' fixed effects. I find that accounting for these changes is important in understanding the effects of mergers and concentration on wages.

Second, I provide causal evidence that concentration affects wages by studying a single merger and comparing establishments from the same firms. I identify the market power effect with the assumption that productivity gains are the same for establishments within merging firms regardless of whether they are in a market in which concentration increases. Azar et al. (2020); Qiu and Sojourner (2019); and Rinz (2020) show that labor market concentration is associated with lower wages. A few studies have used mergers as an instrument for the increase in concentration. Arnold (2021); Benmelech et al. (2020); and Prager and Schmitt (2021) use variation from many mergers and highlight the market power mechanism that allows firms to reduce wages.[5] These papers rely on the assumption that mergers between different companies will have similar productivity gains, regardless of their effect on labor market concentration. I add to this literature by studying a single merger, in which the assumption that synergies are identical across establishments within the merging firm is more likely to hold.[6]

The paper is organized as follows. First, in Section 2 I present a simple model of mergers to understand the theoretical predictions on employment and wages. Section 3 describes the RAIS data and describes the institutional background of the retail pharmacy sector and the merger. Section 4 presents the DiD identification strategy and empirical specifications. Section 5 presents the results of the effect of the merger on the labor market outcomes of merging firms. Section 6 presents the results of the effect of the merger on the labor market outcomes of competing firms. Section 7 presents supporting evidence and discusses the results, and Section 8 concludes.

[5] A recent literature also shows that firms with market power pay lower prices for agricultural inputs (Giroldo and Hollenbeck, 2021; Rubens, 2021) and manufacturing inputs (Morlacco, 2020).

[6] Nevo and Whinston (2010) and Berry et al. (2019) review the empirical industrial organization literature in the last 30 years and highlight the benefits of studying a single industry or even a single event.

2 Oligopsony Model and Merger Predictions

I present a general framework that helps in understanding the potential effects of a merger on the labor market outcomes of merging firms and competing firms. The purpose of this model is to explain why, as a matter of theory, the impact of a merger on wages is ambiguous, even in the absence of efficiency effects.[7]

In the model, firms compete in the product and labor markets. Firm j maximizes profit by choosing labor input l_j. This choice will also define the firm's production through a production function (which I omit). The equation below presents firm j's problem.

$$\max_{l_j} R_j(l_j, l_{-j}) - w_j(l_j, l_{-j})l_j. \tag{1}$$

Firms face a revenue function $R_j(l_j, l_{-j})$ and an inverse labor supply $w_j(l_j, l_{-j})$ curve, where $l_{-j} = \{l_1, ..., l_{j-1}, l_{j+1}, ..., l_J\}$ describe the labor quantities of competing firms. The labor supply curve is specific to firm j and aggregates workers' preferences toward firms and their consumption-leisure trade-off. Unlike the Cournot model, labor supply does not depend on an unidimensional aggregator, such that this is not an *aggregative game* (Asker and Nocke, 2021).

The firm's labor choice decision is at the margin, where the marginal revenue product of labor equals the marginal cost. This condition is presented by the first-order condition with respect to l_j:

$$\underbrace{\frac{\partial R_j(l_j, l_{-j})}{\partial l_j}}_{mrpl_j(l_j, l_{-j})} = \underbrace{\frac{\partial w_j(l_j, l_{-j})}{\partial l_j} l_j + w_j(l_j, l_{-j})}_{mc_j(l_j, l_{-j})}. \tag{2}$$

The equation implicitly defines firm j's best response function, $BR_j(l_{-j})$—i.e., how much labor firm j wishes to demand given the labor demands of its competitors. This equation determines whether firms are strategic substitutes or complements. When firms are predominantly strategic substitutes, they respond to a decrease in the labor demand of a competitor by increasing their own labor demand. This is always the case in Cournot games. Strategic complementarity is more intuitively described in terms of prices: Firms respond to a decrease in wages paid by competitors by decreasing their own wages. The model is solved by using

[7]There are a number of modifications to this model that can produce relevant changes in the merger predictions. For example, the production function may depend on capital and different types of labor; firms might have capacity constraints; firms may produce different products using the same labor inputs

all firms' best response functions to find the equilibrium quantities.

In the imperfectly competitive labor markets literature, it is common to present Equation 2 using equilibrium outcomes:[8]

$$w_j^* = \left(\frac{\eta_j^*}{\eta_j^* + 1}\right) MRPL_j^*, \tag{3}$$

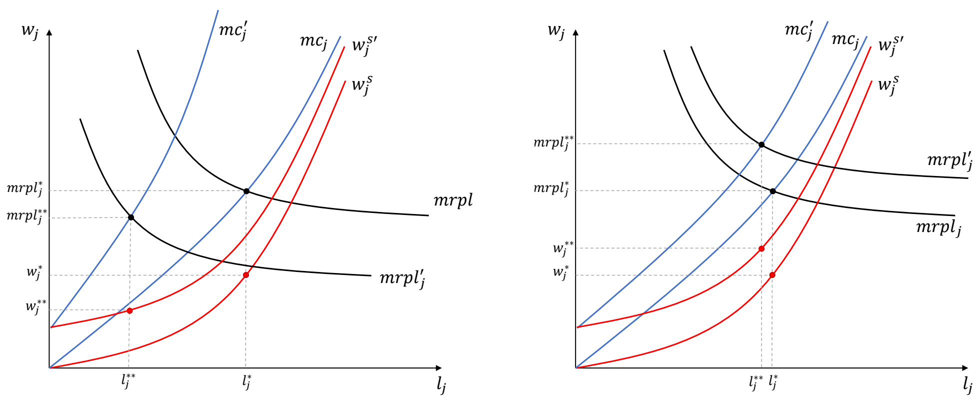
where w_j^* is the amount paid by firm j for a unit of work (hereafter, market wages); $MRPL_j^*$ is the marginal revenue product of labor of firm j; and η_j^* is the firm-specific elasticity of the labor supply, all measured in equilibrium. Equation 3 and Figure 1 show that when the labor supply is upward sloping and firms act in a monopsonistic way, workers will only receive a fraction of the marginal revenue product of labor. Figure 1 presents a visual overview of firm's problem. In the figure, I plot firm j's residualized labor supply curve, i.e, the labor supply evaluated at the competitors' equilibrium quantities, $w_j(l_j, l_{-j}^*)$; the residualized marginal cost curve, $mc_j(l_j, l_{-j}^*)$; and the residualized marginal revenue curve, $mrpl_j(l_j, l_{-j}^*)$.

It is tempting to infer the effects of a merger, increasing concentration in the labor market, or productivity shocks based on Equation 3. However, one must be cautious. All of the components of Equation 3 (w_j, η_j, and $MRPL_j$) are endogenously defined. Any event that changes the structural equations of the model ($p_j(q_j, q_{-j})$, $w_j(l_j, l_{-j})$ and $f_j(l_j)$) will change the best response functions of all firms in the market. For example, a productivity increase faced by firm j will first affect $MRPL_j = \frac{\partial R_j(l_j, l_{-j})}{\partial l_j}$. However, this will change the relation represented by Equation 3 and, consequently, it will change all firms' best response functions. The complex interactions between the new best response functions will define equilibrium outcomes. Note that the equilibrium outcomes l_j^*, l_{-j}^* will be such that the labor supply elasticity η_j and the $MRPL_j$ will also adjust. Hence, the original change in productivity does not predict the equilibrium change in $MRPL_j$ through its derivative.

2.1 The effect of a merger on market wages

A merger between two firms will have ambiguous effects on employment and wages in the merging firms and in competing firms. Suppose that firms j and k merge. The combined

[8] See, for example, Barth and Dale-Olsen (2009); Ashenfelter et al. (2010); Manning (2011); Berger et al. (2019); and Arnold (2021).



((a)) $w_j^{**} < w_j^*$ ((b)) $w_j^{**} > w_j^*$

Figure 1: Labor choice of merging firm j

The graph shows firm j's residualized inverse labor supply, $w_j^s = w_j(l_j, l_{-j}^*)$, marginal cost, $mc_j(l_j, l_{-j}^*)$, and marginal revenue curves, $mrpl_j(l_j, l_{-j}^*)$. Residualized curves are evaluated at the competitors' equilibrium quantities. Panel A simulates the effect of a merger in which firms are strategic substitutes in the labor and product markets. Panel B simulates the case in which the merger comes with a strong productivity increase. The curves $w_j^{s\prime}$,mc_j' and $mrpl_j'$ are evaluated at the new equilibrium quantities of competing firms. Firm j's equilibrium outcomes are defined by the intersection between the marginal cost and marginal revenue curves.

firm maximizes the joint profit function:

$$\max_{l_j, l_k} R_{jk}(l_j, l_k, l_{-jk}) - w_j(l_j, l_{-j})l_j - w_k(l_k, l_{-k})l_k, \tag{4}$$

where I simplify the revenue functions with $R_{jk}(l_j, l_k, l_{-jk}) = R_j(l_j, l_{-j}) + R_k(l_k, l_{-k})$. The first-order condition with respect to l_j gives:

$$\underbrace{\frac{\partial R_{jk}(l_j, l_k, l_{-jk})}{\partial l_j}}_{mrpl_j(l_j, l_{-j})} = \underbrace{\frac{\partial w_j(l_j, l_{-j})}{\partial l_j} l_j + w_j(l_j, l_{-j}) + \frac{\partial w_k(l_k, l_{-k})}{\partial l_j} l_k}_{mc_j(l_j, l_{-j})}. \tag{5}$$

In equilibrium:

$$w_j^{**} = \left(\frac{\eta_j^{**}}{\eta_j^{**} + 1}\right)\left(MRPL_j^{**} - \frac{\partial w_k}{\partial l_j} l_k^{**}\right). \tag{6}$$

We cannot compare Equations 4 and 7 to get exact predictions of the effects of mergers. This happens because all equilibrium outcomes will be different, such that an increase in the MRPL will change the elasticity of labor supply. The size and sign of each effect depends on a series of factors in both product and labor markets: whether merging firms are strategic substitutes or complements, whether merging firms and competing firms are strategic substitutes or complements, on the format of the production function, and on the elasticity of the labor supply.

Figure 1 shows, with two examples, that a merger has ambiguous effects on wages. In Panel A, I present an example in which wages paid by the merging firm j fall with the merger. The figure plots firm j's residualized problem when it merges with a strong strategic substitute in both the labor and product markets. When merging, firm j internalizes that her labor decisions affect the costs of the target firm k. Consequently, her marginal cost curve shifts upward, as shown on the right-hand side of Equations 2 and 5. The same logic applies to the product market: The merging firm internalizes that her production decision affects prices received by the target firm k. Thus, the marginal revenue curve shifts downward. In the following step, strategic substitute competitors will increase their labor demand, which shifts the labor supply curve of firm j to the left. As a result, equilibrium wages paid by firm j fall. Note that the magnitude of the wage decrease depends on the strength of competitors' response.

Panel B of Figure 1 shows an example in which wages paid by the merging firm j increase with the merger. First, as in Panel A, firm j internalizes that her labor decisions affect the costs of the target firm k, which shifts the marginal cost curve upward. In this example, the merger has strong synergies. These synergies are included in the model as a positive productivity shock in the production function of merging firms, which shifts the marginal revenue curve upward. In the example, the effect of synergies is smaller than the combined effect of labor and market power, such that the labor demand of firm j still decreases. Strategic substitute competitors will respond by increasing their labor demand, which shifts the labor supply curve of firm j to the left. In the example, equilibrium wages paid by firm j increase, even though employment decreases.

2.2 The effect of a merger on labor force composition

In this section, I include another labor input, s_j, in the firm's production function to show that a merger may also affect its labor force composition. We can interpret s_j and l_j as labor inputs from different occupations, as workers with different characteristics (age, experience, ability, etc.), or as workers with different tenure at the firm (incumbent workers or new hires).

The aspect I wish to highlight is that merging firms may have different market power over each labor input. If that is the case, the merger will change the relative prices and relative quantities of each input. Without loss of generality for the argument, suppose that the labor market for this additional labor input and the product market are perfectly competitive. Firm j's problem is

$$\max_{l_j, s_j} f(l_j, s_j) - w_j^l(l_j, l_{-j})l_j - w^s s_j, \tag{7}$$

where $f(l_j, s_j)$ is the production function, w_j^l are wages paid by firm j in the imperfect labor market, w^s are the wages paid in the perfectly competitive labor market, and the product's price is set to one.

Proposition 1: Following a merger, (i) the labor force composition at the merged firms, represented by the ratio l_j/s_j, changes, and (ii) the percentage change in the per worker payroll at the merged firms is not equivalent to the weighted average effect of the merger on wages.

The proof is in Appendix E. Proposition 1 shows the importance of using detailed data on each worker's occupation, individual characteristics, and work experience to analyze the effects of a merger on wages.

3 Data and context

3.1 Employer-employee matched dataset

The data in this paper is a Brazilian employer-employee matched dataset (RAIS—Relação Anual de Informações Sociais). RAIS is a restricted-access longitudinal dataset of administrative records collected by the Brazilian Ministry of Employment and Labor that covers all formal workers and firms in Brazil.[9]

The dataset includes information on firms (legal form of the company and identification number); establishments (industry sector, county, and identification number); workers (age, gender, race, education, and identification number); and job characteristics (occupation, date of hiring, date of separation, reason for separation, hours worked, employment status on December 31, tenure, average wage, and wage in December). A unit of observation in the data is a job relation between a worker and an establishment in a given year. Using this information, I am able to track workers throughout their formal job history. If workers have

[9]The main purpose of RAIS is to administer a federal wage supplement (Abono Salarial) to formal employees. There are incentives for truthful reporting. In principle, an employer's failure to report the information can result in fines proportional to the firm size.

multiple jobs during a year, they will appear several times in the data. That said, in my analysis I restrict attention to individuals with a formal employment link on December 31.

The empirical strategy used in this paper requires two different data samples, which I call the drugstores sample and the fully connected sample. The focus of the paper is on drugstores; therefore, the drugstores sample is restricted to firms in the retail pharmacy sector that had at least one pharmacist working between 2007 and 2018. However, it is possible that establishments such as distribution centers, headquarters, and warehouses are wrongly classified as drugstores. To prevent that, I drop from the sample establishments that are sorted in one of following criteria: (i) establishments with more than 100 full-time employees in any year (ii) establishments with more than 50 full-time employees and no pharmacist in any year, and (iii) establishments with more than 10 full-time employees and more than 60% of employees in warehouse occupations in any year. I further restrict the sample to a balanced panel of establishments—i.e, establishments that had at least one employee every year between 2007 and 2018.

Table 1 presents sample size numbers by merging firms and competitors. The balanced panel of drugstores is spread among 91 counties and contains 392 establishments. Between 2007 and 2018, 51,380 individuals worked in one of these establishments, resulting in 134,216 person-year observations. The second column of Table 1 presents the sample size for competing firms in the same counties: 9,798 firms that employed 497,658 different individuals during the period. In Appendix Table B.1, I present additional statistics on the drugstore sample and on less restrictive samples. Around 16% of observations in merging firms are for workers who were employed as pharmacists, and around 62% were employed as salespeople.

In the second sample—the fully connected sample—I keep the set of establishments from all sectors in the economy that are connected to establishments in the drugstore sample through worker mobility. The fully connected sample includes information on all workers who switched jobs at least once between 2007 and 2018 and have worked in one of these establishments. With this data, I can properly estimate worker and establishment fixed effects. I further explain this problem in Section 4.2. I restrict the data to workers between 18 and 60 years old, who have 40 or more contractual weekly hours of work, and who work in the states in which the merging firms had an establishment in 2010. If workers have more than two observations in a year, I keep the two observations with the earliest dates of admission. Column 3 of Table 1 shows that the fully connected set consists of 189 million observations from 29 million individuals working in 4.2 million establishments. Note that the drugstore sample is not a subset of the fully connected sample, since it includes workers that have never switched establishments.

3.2 Institutional setting

In this section, I present some key similarities and differences between the Brazilian and U.S. institutional settings that are relevant to this study of the retail pharmacy sector.

Retail pharmacies, or drugstores, are facilities that sell medication, cosmetics, and pharmaceutical products. Pharmacies may also administer vaccines and compound medication, and can sell some food products for special purposes (Sebrae-SP, 2015). While drugstores in Brazil are similar to U.S. drugstores in terms of the products sold, they tend to differ in size. In Brazil, drugstores are smaller in size and number of employees. An average Brazilian drugstore has around 8 employees, with one or two pharmacists, while an average American drugstore has around 14 employees. In Appendix C, I present more detailed information on the retail pharmacy sector in Brazil.

In Brazil, the relation between employers and employees is mediated through unions, with some key differences with the rest of the world. Workers are automatically associated with a union that represents their category. The smallest representing unit is the city, with most unions representing workers in a group of counties or within the state (Menezes-Filho et al., 2011). In contrast, U.S. workers tend to organize at the firm or plant level.[10] Another singularity of the Brazilian case is the existence of employer unions that represent groups of employers in a region. Employees' unions negotiate collective agreements with employers' unions in overlapping regions. In the context of retail pharmacies, employees' unions are organized at the state level, but employer unions are organized at the sub-state level. Most states have an acting pharmacist union and a union representing other workers at drugstores. Hence, unions might have to sign multiple collective agreements within a year. In these agreements, unions bargain for wage floors and wage increases, among other conditions of employment. In summary, employer and employee unions bargain with each other and then sign collective agreements that are valid for all formal firms and workers in the region.

The Brazilian analytical process for mergers and anti-competitive agreements is closely related to the conduct of the U.S. Department of Justice and Federal Trade Commission. Firms above a certain revenue threshold that wish to merge must pass through the scrutiny of CADE (Conselho Administrativo de Defesa Economica), the antitrust agency. CADE evaluates whether the merger significantly increases concentration in the relevant market and decides whether to approve the merger, propose remedies or deny the merger. The law

[10] In the U.S., union membership is voluntary, with unions being able to represent workers at the plant level. Membership is also free in signatory countries of Convention 87 of the ILO (Freedom of Association and Protection of the Right to Organise Convention, 1948).

that regulates CADE is ambiguous regarding whether the agency can stop a merger based on labor markets concerns.[11] That said, in 2021, for the first time ever, CADE opened a case to investigate a cartel between human resource departments from the pharmaceutical industry and medical product suppliers.[12] This implies that CADE has the authority to maintain competition in labor markets. In the next section, I present the merger in the retail pharmacy sector that I study.

3.3 The merger between two major retail pharmacy chains

I study a merger between two large retail pharmacy chains in Brazil.[13] This was a horizontal merger in the sense that the firms operated in the same markets and sold substitute goods. Firms announced the merger in 2011 and the Brazilian antitrust agency unanimously approved the merger in 2012, nine months after the announcement. I expect a delay between the juridical implementation of the merger and the de facto integration of the companies, which is common to mergers and acquisitions. A local news magazine reported that until mid-2013 the management of each company was still separate, even though all administrative employees were working in the same building.

In 2012, the merging firms had to change the establishments' identification numbers. I re-code these establishments' IDs so that they have the same IDs throughout all of the time series. I also re-code workers' tenure. I assume that two establishments with different IDs are actually the same establishment if (i) they are in the same county, (ii) they share 50% or more of their labor force, and (iii) one of the establishments exits the data when the other enters. In addition to changing IDs, firms had to report their workers' information to RAIS twice in 2012. Although I can identify this in the data, I exclude the year 2012 from the main specification to prevent reporting mistakes. That said, including 2012 does not significantly change the results.

An attractive feature of the merger is that firms overlapped in some counties and did not overlap in others, which provides plausible exogenous changes in concentration. Although other mergers and acquisitions occurred during the time frame of the analysis, the merger I study is the only one in which firms' operations overlapped in many counties. Figure 2 presents a map of the nine southern states from Brazil to show the presence of the merging

[11] Law number 8,884, from 1994, and law number 12,529, from 2011, establish that the agency must intervene when companies harm competition in the "relevant market."

[12] http://valor.globo.com/legislacao/noticia/2021/03/24/cade-investiga-formacao-de-cartel-entre-departamentos-de-recursos-humanos.ghtml
Note: The cartel did not operate in the retail pharmacy sector.

[13] For confidentiality reasons, I do not reveal the firms' names.

firms in each county before the merger. Counties in which both merging firms had an establishment in 2010 are drawn in red. Counties in which only one of the merging firms had an establishment in 2010 are drawn in yellow and orange. The white area represents counties in which none of the firms had an establishment in 2010. For future reference, the empirical strategy in this paper terms counties in which firms overlapped the treatment group and counties in which firms did not overlap the control group.

The merger changed labor market concentration in some regions. Panel A of Figure 3 shows that the merger increases the pharmacists' labor market Herfindal-Hirschman index (HHI) in many counties.[14] Panel B shows that the merger did not change concentration in the labor market of salespeople, where the market is also defined at the county-occupation intersection. Given these pictures, it is reasonable to assume that a merger between two retail pharmacy chains will affect pharmacists and salespeople in different ways. In theory, pharmacists have less outside options then salespeople, with the job opportunities of pharmacists being restricted to drugstores, hospitals and pharmaceutical companies. Salespeople, on the other hand, may work in a similar position at drugstores, supermarkets, general stores, and any other retail shop. That said, Figure 3 uses ad-hoc labor market definitions. In section 7.1, I discuss how finer labor market definitions change our ex-ante hypothesis regarding the effect of the merger.

Table 2 presents some descriptive statistics of counties and workers from merging firms in the treated and control groups. On average, counties in the control group are smaller in population and have lower per capita government revenue than counties in the treated group. Counties in the overall sample had lower informality rates than the nationwide average (27% versus 40%) and HDI levels comparable to countries such as Spain or Greece (measured in 2000). There are no significant differences in informality or HDI between treated and control groups. Panel B of Table 2 presents the characteristics of individuals working in a merging firm within the balanced sample of establishments. In 2011, there were 2,374 and 5,665 people working in establishments in the control and treated groups, respectively. The retail pharmacy sector employs more women than men, with males representing only 31% of

[14] I measure concentration in the labor market using the HHI, as described in the equation below. There is an HHI index for each occupation O in county C at time t. The index is constructed by summing the square of firm f's occupation shares. Note that the set of firms $f \in F$ is restricted to firms that had at least one employee from occupation o in the years previous to the merger. Hence, the number of firms that employed pharmacists is smaller than the number of firms that employed salespeople. The HHI varies between zero and 10,000, with higher values indicating a more concentrated labor market.

$$HHI^{o}_{ct} = \sum_{f} (\text{Share of occupation o workers})^2_{fct}.$$

workers. The average worker is around 24 years old, with workers in the treated group being on average 1 year older than workers in the control group. Workers in the treated group also receive higher wages.

While baseline characteristics or workers and counties in treated and control groups may differ, this does not trigger great concern about identification. The empirical strategy in this paper adopts a differences-in-differences approach and the identifying assumption requires that the outcomes of interest in both groups follow similar trends (and not levels). In the main empirical specification, however, I include age controls and county characteristics interacted with year fixed effects.

4 Identification Strategy and Empirical Specification

Section 2 shows that theory yields ambiguous predictions regarding the effects of a merger. Hence, whether merger-induced concentration reduces wages is an empirical question. To identify the effects of concentration on labor market outcomes, I use a difference-in-differences (DiD) design applied to the study of a single merger in the retail pharmacy sector in Brazil.

The DiD compares labor market outcomes before and after the merger in treated versus control counties. Merging firms are present in many locations, so I use the variation in the firms' overlap within locations to define the treatment and control group. I denote as treated counties in which both merging firms had at least one establishment in 2010, one year before the merger was announced. I denote as control counties in which only one of the firms had an establishment in 2010. I will later discuss the advantages of this setting.

I separate the analysis into two samples: establishments of merging firms and establishments of competing firms. In both cases, I restrict the sample to establishments that had at least one employee in all years between 2007 and 2018. Besides separating the sample by type of firm, I estimate the regressions for pharmacists and salespeople separately. The empirical specification, at the establishment level, is of the form

$$y_{jt} = \delta_0 + \delta_1 post_t + \delta_2 treat_{c(j)} + \delta_3 post_t \times treat_{c(j)} + \varepsilon_{jt}, \tag{8}$$

where y_{jt} is an outcome such as average wage or employment in establishment j in year t. The indicator $post_t$ equals one for observations after 2011; $treat_{c(j)}$ is a variable that indicates whether establishment j is located in a treated county c; and ε_{jt} is an error term. Additionally, I estimate a leads and lags equation, presented below. In the equation, $\mathbb{1}[t = k]$

indicates the year relative to the merger; λ_t are year fixed effects; and ε_{jt} is an error term.

$$y_{jt} = \beta_1 treat_{c(j)} + \sum_{\substack{k=-4 \\ k\neq 0}}^{7} \delta_k \mathbb{1}[t=k] \times treat_{c(j)} + \lambda_t + \varepsilon_{jt}. \tag{9}$$

Using establishment-level data to analyze changes in wages has a major limitation: As establishments hire and fire workers, their labor force composition changes over time. Proposition 1 states that the merger may also induce changes in labor force composition. If, for example, firms hired younger employees after the merger, we would naturally observe a fall in average wages. In this case, the DiD estimator above is not informative regarding how the merger changes market wages—i.e., we cannot use this estimator to infer wages in the counterfactual scenario in which the merger does not occur. To identify this effect, I present the identification strategy at the individual level, which accounts for changes in labor force composition in the next two sections. I discuss the use and value of the DiD estimator without controls in Section 5.1.

4.1 Basic difference-in-differences with individual characteristics

To control for observable changes in the labor force composition, I estimate the DiD over the balanced panel of establishments using individual-level observations and individual controls:

$$ln(wage)_{it(j)} = \delta_0 + \delta_1 post_t + \delta_2 treat_{c(j)} + \delta_3 post_t \times treat_{c(j)} + X'_{it}\beta + \varepsilon_{it}, \tag{10}$$

where, $it(j)$ represents individual i who was working at establishment j during year t and $c(j)$ is the county in which establishment j is located. As in the previous section, the indicator $post_t$ equals one for observations after 2011; $treat_{c(j)}$ is an indicator for observations in treated counties; and ε_{it} is an error term. X_{it} is a vector of individual controls: age, education, gender, and race. Although I use the same balanced panel of establishments as in the previous section, the population of workers within each establishment changes every year. Therefore, I estimate the effect of the merger on the wages of all individuals working in an establishment in the sample.[15]

Equation 10 controls for observable changes in composition. However, firms might change the composition of their labor force in ways the econometrician does not observe, such as workers' ability or other qualifications. In the next section, I present an extension of the DiD model in which I include worker fixed effects to control for these unobserved changes in

[15] Alternative approaches study the effects of an event or policy on the wages of incumbent workers only.

composition.

4.2 Difference-in-differences with worker and establishment fixed effects

The main empirical specification in this paper is an extension of the DiD model that adds worker and establishment fixed effects and uses the fully connected sample. The inclusion of worker fixed effects allows me to control for all of time-invariant characteristics of workers, including unobserved skill. In this way, I can estimate the actual effect of the merger on market wages, net of observable and unobservable changes in labor force composition.

The equation below incorporates worker and establishment fixed effects in the DiD model.[16]

$$\begin{aligned} \ln(wage)_{it} = \theta_i + \psi_{J(i,t)} &+ \sum_{k\neq o,o'} \delta_0^k \mathbb{1}[Group_k] \\ &+ \sum_{k} \delta_1^k \mathbb{1}[Group_k] \times Post_t \\ &+ \sum_{k} \delta_2^k \mathbb{1}[Group_k] \times Treat_c \\ &+ \sum_{k} \delta_3^k \mathbb{1}[Group_k] \times Treat_c \times Post_t \\ &+ X_{it}'\beta + \lambda_t + \varepsilon_{it}, \end{aligned} \tag{11}$$

where log wages of individual i at time t are separable into worker fixed effects, θ_i; establishment fixed effects, $\psi_{J(i,t)}$, with the subscript $J(i,t)$ referring to establishment J in which individual i was working at time t; time-varying individual characteristics, $X_{it}\beta$; and year fixed effects λ_t. The equation also includes four sums over the groups k. The terms inside the sums correspond to the DiD terms for each group. For example, the parameter δ_0^1 is analogous to the parameter δ_0 from Equation 10. There are six groups from the interactions between two types of firms (merging firms and competing drugstores) and three occupation categories (pharmacists, salespeople, and other occupations). I omit δ_0^k for groups o and o' to avoid collinearity with establishment fixed effects, where o and o' refer to the groups "other occupations in merging firms" and "other occupations in competitors" As in the previous section, the indicator $Post_t$ equals one for observations after 2011, and $Treat_c$ is an indicator for observations in treated counties. I am interested in the parameters δ_3^k, which recover the

[16] In Appendix D, I rewrite Equation 15 in its extensive form for easier exposition.

effect of the merger on wages for each group k, excluding composition effects.

The purpose of including establishment fixed effects in Equation 11 is to prevent other parameters from being biased. There are several theories regarding why different establishments have different levels of $\psi_{J(i,t)}$: Establishments may differ in their productivity levels, in amenities, or in market power in the labor market. Given that, individual fixed effects and other parameters will be biased if we do not include establishment fixed effects. For example, a worker who switches from a low-productivity establishment to a high-productivity establishment is likely to receive a wage increase. I would mistakenly assign the establishment productivity effect to that individual (or to individual time-varying characteristics) if I did not include an establishment fixed effect.

As a consequence, I have to extend the drugstore estimation sample to the fully connected sample. The fully connected sample is necessary in order to jointly identify worker and establishment fixed effects. This is a well-known problem in the literature.[17] For example, I cannot separately identify worker and establishment effects if an establishment is composed of workers who never switched establishments. In my setting, workers in the retail pharmacy sector have also worked in firms from other sectors. Hence, I keep the set of establishments from all sectors in the economy that are connected to establishments in the drugstore sample through worker mobility. In summary, accounting for changes in composition in terms of worker fixed effects creates a great data requirement. Fortunately, the RAIS dataset allows me to solve this problem.

Equation 11 includes all six groups, such that I must estimate 22 δ^k parameters jointly. I explain why this is the case with the following example. Suppose the merger reduces the wages of pharmacists at merging firms and competing firms by the same amount. Then suppose that a pharmacist working at a merging firm switches firms after the merger and starts working at a competing drugstore, still as a pharmacist. We would expect a reduction in the wage of this pharmacist that is attributable to the merger and not the worker. However, if I did not include the DiD parameters for the competing firms, I would estimate a lower establishment fixed effect for the competing firm, which in turn would bias the estimate of that worker's fixed effect, which in turn would bias the δ_3^k parameter for pharmacists in merging firms. Thus, all DiD parameters must be jointly estimated.

An alternative way to incorporate all DiD effects would have been to include establishment fixed effects interacted with each occupation group in the log wage equation. In practice, the interaction between establishment fixed effects and occupation creates a "sub-establishment." For example, a drugstore that hires pharmacists, salespeople, and managers would be divided

[17] For example, see Abowd et al. (1999) and Card et al. (2013).

into three "sub-establishments": the pharmacists' drugstore, the salespeople's drugstore, and the managers' drugstore. Identification of each sub-establishment fixed effect requires a sample of sub-establishment switchers, whereas estimation requires that the sample is sufficiently large. These requirements might not be met. Instead of introducing a full set of sub-establishment fixed effects in the log wage equation, I interact the group dummy with pre- and post-merger dummies. This reduces the dimensionality of the problem, solves the data requirement problem, and still provides the parameter of interest.

4.3 Identifying assumptions

There are three main challenges when trying to identify the effects of a merger on wages through market power: (i) other events might occur at the same time as the merger; (ii) firms may change their labor force composition after the merger; and (iii) merger-induced synergies may also affect wages, which makes it difficult to identify the market power effect. Note that antitrust agencies such as the DOJ, FTC, and CADE are interested in the total effect of mergers on prices and wages, which includes the effect through synergies. I am only interested in identifying the effects through market power, excluding synergies.

The DiD approach solves the first challenge by taking differences with a control group. The idea is that any event, other than concentration, that affects workers in the treated group also affects workers in the control group. Hence, the difference between treated and control groups will exclude the effect from those events. The underlying assumption in the DiD is the parallel trends assumption: Conditional on controls, wages in the control group would have followed the same trends as wages in the treated group, if they were treated. In the results section, I provide evidence that the parallel trends assumption holds by showing that the pre-trends are parallel. Yet, as shown in Section 3.3, treated and control groups have different characteristics. For example, counties in the treated group are more populous. A potential concern is that counties have different trends associated with these characteristics, which violates the parallel trends assumption. To address this concern, I control for interactions between county characteristics and year fixed effects in the main specification. Results do not change when I do this.

The second challenge—accounting for changes in the labor force composition—was addressed using Equations 10 and 11. Identification relies on two common assumptions: The log-linearity of wages and additivity. The log-linearity of wages is a widely accepted assumption in labor economics. The additional additivity assumption implies that the effect of the treatment is not heterogeneous by individual characteristics. Identification in Equation 11 requires the additional assumption of exogenous mobility. Exogenous mobility implies that workers do not select into firms based on the idiosyncratic error term, ε_{it}. In Appendix Figure A.8, I carry out the test proposed by Card, Heining, and Kline (2013). The figure shows that workers moving from low-paying establishments to high-paying establishments have a wage increase and that workers moving from high-paying establishments to low-paying establishments have a symmetric wage decrease. If variation in wages across establishments were mainly due to sorting, we would expect wage increases in the latter case.[18]

The DiD also addresses the third challenge—separately identifying the effects of market power from the effects of synergy. In my setting, the merger increased concentration in treated counties, in which firms overlapped, but it did not change concentration in control counties, in which firms did not overlap. Considering that the merger was decided at the national level, increases in concentration due to the merger are arguably exogenous at the county level.[19] The key identifying assumption is that synergies are realized at the national level and are the same for treated and control counties. Examples of synergies at the national level are better bargaining with suppliers, improvements in logistics and optimization of distribution centers, and combining headquarters and administrative departments. Under this assumption, the difference between treated and control groups yields the market power effect, excluding the effects from synergies.

Other papers have used a stronger assumption to address the same challenge. To identify the market power effect, Arnold (2021) assumes that the effect of the mergers on productivity (synergies) is independent of the change in local labor market concentration. For example, mergers with high change in labor market concentration will have the same synergies as mergers with low change in labor market concentration. The difference in my setting is that Arnold compares the effects of different mergers and I use the same merger. Similarly, Prager and Schmitt (2021) compare the effects of hospital mergers in which there is an increase in labor market concentration with other hospital mergers in which there is no increase in labor market concentration.

[18] Gerard et al. (2021) use the same matched employer-employee dataset and present additional tests that suggest the assumption holds.

[19] Dafny et al. (2012) use a similar assumption to analyze the effects of the Aetna-Prudential merger in the health insurance market.

5 Effect of the Merger in Merging Firms

5.1 Effect of the merger on overall wages

Figure 4 presents trends in average log wages for all workers in merging firms. The figure includes observations from all individuals from the balanced panel of merging establishments. Analysis of these trends yields three takeaways. First, wages increase after the merger for both treated and control groups. We cannot attribute this increase to synergies from the merger, since other events during that period might have also affected wages; minimum wages and average wages for all workers in Brazil were also increasing in that period. Second, the pre-trends are parallel. Figure 6 presents the difference in log wages between treatment and control groups and confirms this. This gives supportive evidence on the parallel trends assumption and the assertion that establishments in treated and control groups are comparable. Third, although wages in the treated and control groups seem to converge after the treatment, the overall effect of the merger is not statistically different from zero for most years and has small magnitude. Figure 6 shows a statistically significant effect at the 5% significance level in 2017.

Panel A of Figures 5 and 7 present trends and difference in trends in the average log wages of pharmacists. Compared with the previous results, the figures show a different picture for pharmacists; this highlights the benefits of using data on specific occupations. The trends in Panel A of Figure 5 show that the average wage of pharmacists in the control group has a small increase after the merger. However, the average wage of pharmacists in the treated group, where concentration increases, falls after the merger. Panel A of Figure 7 shows that the relative wages of pharmacists start with a slow decline after the merger and reach a strong decrease of 12% after 6 years. Table B.5 shows that the average effect of concentration on the wages of pharmacists is -7.3%. Columns 2 to 4 of Table B.5 use a continuous treatment measure—projected changes in HHI at the county level—instead of the binary treatment. The results show that firms spend less per pharmacist in counties in which concentration increases more. Remember that the pool of workers in each establishment changes every year. Hence, without further information we cannot separate the effect of the merger on wages from changes in labor force composition.

Results are robust to less restrictive samples and other measures of wages. By comparing column 1 to 4 with columns 5 to 8 of Appendix Table B.3, I show that the results for pharmacists do not change if we use average wages instead of wages from December.[20] The

[20]Note that these regressions follow Equation 10 and use the pharmacists sample (and not the fully connected sample).

sample increases when using average wages because it includes individuals who worked in a merging firm for only part of the year and, for individuals who worked in two different establishments in the same year, both observations. Table B.4 relaxes some sample restrictions. I show that the effect on average wages does not significantly change when I (i) weight for the inverse of the number of pharmacists in each establishment (column 2), (ii) include workers with the top 1% of wages (column 3), (iii) include the year 2012 (column 4) or (iv) include all establishments from merging firms and not just the balanced panel of establishments (column 5).

Panel B of Figures 5 and 7 shows that the wages of salespeople do not have the same decline as for pharmacists. Figure 5 shows that the trends in the average wages of salespeople are always parallel between treated and control groups. Panel B of Figure 7 shows that the relative wages of salespeople have a small decrease after the merger (-6% in 2015) with a small increase after 2015. The average effect post-merger is not statistically different from zero and is small in magnitude. Once again, these results do not imply that concentration did not reduce the wages of salespeople.

A recent study by Prager and Schmitt (2021) finds notably similar results. Using a different empirical strategy, the authors study many hospital mergers in the US. They show that after a high-concentration-inducing merger, the average wages of more specialized workers, such as pharmacists and nurses, decrease by 7%, while the average wages of unskilled workers remain stable. These results are consistent with the hypothesis that specialized workers have fewer outside options and are more exposed to the negative effects of concentration. The authors then conclude that mergers may affect wages through market power, but these effects only apply in relatively narrow circumstances and do not affect low-skill workers. As with the results in this section, the authors observe the average wages of a pool of workers in each establishment and do not take changes in composition into account. In the next section, I show that changes in labor force composition play a large role in the merger I study.

5.2 Effect of the merger on wages, accounting for changes in composition

Table 3 presents the main results of the paper. The table shows estimates of selected parameters from Equation 11, which are estimated over the fully connected set. Panel A shows that the results from the previous section on the wages of pharmacists were mostly driven by a change in worker composition within each establishment and not by a change in wages due to market power. The panel presents estimates of δ_3 for pharmacists in merging firms.

First, without controls, the wages of pharmacists fall by -5.6% (column 1). The effect of the merger gets stronger when I include age controls (age squared and cubic), decreasing to -7.9% (column 2). The inclusion of establishment fixed effects does not significantly change the estimate (column 3). However, the inclusion of individual fixed effects reduces the magnitude of the effect, with the estimate decreasing to -2.6% (column 4). The latter result is not statistically significant even if we cluster standard errors at the county-year level or at the establishment level, and is robust to the inclusion of time dummies interacted with county characteristics (column 5).[21] Figure A.4 presents residualized trends in log wages of pharmacists. I interpret the figure as evidence that pre-trends are parallel when I include worker and establishment fixed effects. In summary, the wages of pharmacists do not fall as much as previously thought, which contradicts the hypothesis that more specialized workers have fewer outside options and suffer greatly from events that increase concentration.

Panel B of Table 3 shows a decrease in the relative wages of salespeople, which also contrasts with the results from the previous section. First, note that the point estimate is negative in all specifications, but it is only statistically significant in columns 4 and 5, which include worker fixed effects. Second, the point estimate increases when I include age controls, which suggests that the average age of salespeople decreases in the treatment group relative to the control group. Third, columns 4 and 5 show that the relative wages of salespeople reduce by -3.7%, which is statistically significant at the 10% confidence level. When clustering at the county $\times$ year or establishment level, the effect is statistically significant at the 1% level.[22] The results do not support the assumption that salespeople are less affected by the merger.

The increase in minimum wages is not likely to drive these results. Although salespeople in the control group receive lower wages than salespeople in the treated group, more than 95% of the salespeople receive wages higher than the minimum wage. While there is evidence that minimum wages have spillover effects to the rest of the wage distribution, these effects should not differ across treatment and control groups.

In section 7, I discuss potential explanations for why the merger has a negative effect on the wages of salespeople and a small effect on the wages of pharmacists.

[21] I include dummies for each quartile of the following county characteristics: population from the 2010 census, HDI constructed by Ipeadata in 2003, informality from 2010 census, and county government revenue per capita from 2006.

[22] Standard errors are estimated using the Frisch–Waugh–Lovell method.

5.3 Effect of the merger on other labor market outcomes

Table 5 presents DiD results for other outcomes of merging firms. Column 1 shows employment effects. Establishments in treated counties have a statistically not different from zero reduction in the employment of pharmacists when compared with establishments in control counties. The magnitude of -0.18 (SE=0.24) represents a decrease of 6% in the employment of pharmacists. Given the large standard errors, I cannot rule out larger declines in employment. The employment of salespeople has an even smaller effect in magnitude which is also not statistically significant. Employment increases by 0.10 (SE=0.57), which represents an increase of only 1% in the number of salespeople. Again, given the large standard errors, I cannot rule out larger declines in employment.

Column 2 of Table 5 presents the effects on average age. Changes in labor force composition in terms of age explain the difference between the estimates in columns 1 and 2 of Table 3. The average age of pharmacists increases by 2.5%, which is around 9 months, relative to the control group. Although the point estimate is not statistically significant, its magnitude is sufficient to change the estimates in the wage equation of Table 3 by 2%. This occurs because older workers usually receive higher wages. Therefore, the estimate of -5.6% is masking a larger decrease in wages, compensated by a change in labor force composition. The inverse occurs for salespeople, where the wage effect of -2.4% partially captures the reduction in the average age of workers.

Column 3 of Table 5 presents the changes in labor force composition using worker fixed effects as an outcome. These changes explain the main result of the paper: The difference between estimates in columns 3 and 4 of Table 3. There is a relative decrease in worker fixed effects of 3.6% (Panel A). This explains why we see a large negative effect in the wages of pharmacists. What is actually happening is that establishments in the treated group are hiring pharmacists of lower fixed effects after the merger, relative to establishments in the control group. Panel B shows the opposite effect for salespeople (1.6% increase, SE=0.01). Establishments in the treated group are hiring salespeople of higher fixed effects after the merger, relative to the control group.

6 Effect of the Merger in Competing Firms

The effect of the merger on the wages of pharmacists working at competing firms is not statistically significant and varies with each estimation specification. Table 4 presents estimates of selected parameters from Equation 11 for competing firms. Column 1 shows that the average wages of pharmacists fall by 3.2%. However, when including age controls and worker and establishment fixed effects, we observe a statistically not significant increase in wages of 2.5%. Table 6 shows that this difference in estimates arises from a change in composition: Average age decreases by 1.7% and the average of pharmacists' fixed effects decreases by 4.8% (SE=0.012).

Competing firms reduce the relative wages of salespeople in treated counties. Panel B of Table 4 shows that the relative wages of salespeople fall by 3.3% after the merger. This effect is statistically significant and only varies by 1% with the removal of controls. Table 6 confirms that changes in age and worker fixed effects have low magnitude, such that they are not that relevant for the results. Note that the effect on the wages of salespeople in competing firms has magnitude similar to the effect in merging firms.

Column 1 of Table 6 presents the effects of the merger on employment in competing firms. There is a small increase in the number of pharmacists, but the effect is small, 0.187 (SE=0.098). Small effects on the employment of pharmacists might be explained by the production technology and the legal requirements drugstores entail: A pharmacist always has to be working when the facility is open. The effect on the employment of salespeople is larger: an increase of 1.08 workers, which is around 10% of the number of salespeople in competing firms.

The fall in wages and increase in the employment of salespeople in competing firms is consistent with the hypothesis that firms act as strategic substitutes. As previously discussed, strategic substitutes respond to other firms' hiring strategies by doing the opposite of what their competitors do. In this case, competing firms responded to a decrease in merging firms' employment of salespeople by hiring more salespeople. This possibly prevented the wages of salespeople to fall even more. This new finding helps explain why the estimated effects are modest, despite the fact that concentration increases: The labor market effects of a merger are limited by competitors' responses.

7 Discussion and Magnitudes

In this section, I discuss two questions that emerged from the previous results: Why didn't pharmacists' wages in merging firms fall? And why did the wages of salespeople fall? In Section 5.2, I showed that the relative wages of pharmacists in merging firms fell by only 2.6% after the merger. On the other hand, the wages of salespeople fell by 3.5%. This evidence is contrary to the ex ante hypothesis that pharmacists are more specialized and have fewer outside options than salespeople.

The small effect of the merger on pharmacists' wages is consistent with the higher unionization rate of pharmacists. At the same time that pharmacists are more specialized, they are also more organized. Survey data show that 34% of pharmacists are unionized, while only 9% of salespeople working at pharmacies are (PNAD, 2011). In Brazil, employees' unions function at the state-occupational level, and even though membership is not mandatory, union decisions are valid for all workers from that state-occupation. As in many countries, one of unions' functions is to negotiate wage floors and annual wage increases with employers. These agreements are settled with the employers' union in each region. The results from this paper and the centralized nature of negotiations indicate that a merger between two firms might not weaken employees' unions' bargaining power.

I present additional evidence to support this argument. Figure 8 presents the leads and lags results for pharmacists in two different samples: incumbent workers and newly hired workers. Panel A shows that the average wages of incumbent pharmacists do not change after the merger, compared with a control group. On the other hand, panel B shows that the wages of pharmacists hired in each year—i.e, entry-level wages for pharmacists—decrease by up to 20%. These regressions do not include individual characteristics and worker fixed effects. I argue that incumbent workers are shielded by union agreements, such that firms have little room to differentially change incumbent workers' wages across counties. At the same time, pharmacy chains pay higher wages than independent drugstores, so the wage floors might not be binding for them. Consequently, these firms face less rigidity when setting the wages of new hires.

The second surprising result is that the wages of salespeople decrease by 3.5%. Ex ante, we hypothesized that salespeople working in drugstores are fully mobile with respect to other industries, such as supermarkets, general stores, etc. In this scenario, a merger would not increase concentration in the salespeople labor market, and thus it should not affect the wages of salespeople. Accordingly, Panel B in Figure 3 shows that the merger does not change the HHI of salespeople when we define a labor market at the county$\times$year$\times$1-digit occupation.

However, salespeople working in drugstores might have smaller labor markets than previously thought. In Table B.6, I show that 74% of salespeople working in a drugstore continue as salespeople in the following year and that 73% still work in a drugstore in the following year. Table B.7 shows that 24% of the salespeople who were working in a drugstore and switched jobs were still working in a drugstore the following year. The share of salespeople moving to drugstores should be closer to 5% if they had switched randomly to other salespeople jobs. I interpret these results as evidence that salespeople working in drugstores have higher preference to remain in drugstores and do not consider all salespeople jobs equally as their labor market.

In a new finding, I show that pharmacists and salespeople may have preferences over the type of drugstore they wish to work at. In Figure A.11, I plot a nonparametric regression of the share of workers who move to a pharmacy chain over the share of establishments from pharmacy chains in each county as percentage of total drugstores. Pharmacy chains are defined as firms that have more than five establishments in one year in the entire country. I present results for two groups of workers based on their employer of origin: pharmacy chain or independent drugstore. Panel B of Figure A.11 shows that salespeople who were working in a pharmacy chain are more likely to switch jobs to another pharmacy chain than salespeople who where originally working in an independent drugstore. Panel A of Figure A.11 shows a similar pattern for pharmacists. These results provide additional evidence that workers' preferences and/or specific human capital are capable of defining upward-sloping labor supply curves that are specific to each firm—or at least for each type of firm, as in Card et al. (2018).

7.1 Magnitudes

So far, we have seen that the merger increased concentration and decreased wages in some markets. Yet how much did wages decrease after a certain increase in concentration?

Elasticities vary significantly with labor market definitions. To show that, I use the DiD wage estimates for pharmacists and salespeople in merging firms (-2.6% and -3.5%, respectively) and different measures of increases in concentration. Table B.8 presents measures of concentration using the HHI. In the table, I report the average HHI and average merger-induced change in HHI across counties in the treated group, in which the merging firms overlapped and concentration increased. The merger-induced change in HHI in counties in the control group is always zero by definition.

Wage elasticities with respect to concentration increases are small in the case of pharmacists. The HHI in 2011 for the pharmacists' labor market varies between 457 and 2,316. These measures depend on the labor market definition: all establishments that hired a pharmacist versus only pharmacy chains. Similarly, merger-induced changes in HHI vary from 37 to 651 points. An average increase of 37 points is considered small for DOJ and FTC merger guidelines. However, an increase of 651 points is large enough to receive antitrust attention. The elasticity of wages with respect to labor market concentration ranges from -0.34 to -0.1 for pharmacists.

Panel B of Table B.8 presents measures of concentration for salespeople. First, using a broad labor market definition, we get that concentration increases by 0.1 points, or 0.2%. This generates the implausibly large elasticity of wages with respect to concentration of -17.5. When restricting the labor market to salespeople in pharmacy chains, the elasticity is -0.13. Hence, the results show that finer labor market definitions give a better description of the salespeople's labor market. The development of a method to define labor markets is left for future research.

8 Conclusion

Using a matched employer-employee dataset from Brazil, I study the effects of a merger between two large retail pharmacies on wages, through the mechanism of market power. With this data and setting, I can implement a difference-in-differences approach that includes worker characteristics and worker and establishment fixed effects.

The main result of the paper shows the importance of including controls in the DiD approach. I find that the average wages of pharmacists do not decrease after the merger, when comparing treated and control groups, and that the wages of salespeople do. In a secondary analysis, I show that the entry-level wages of new pharmacists decrease after the merger and the wages of incumbent pharmacists do not decrease. We would have gotten a different conclusion in a regression without individual controls: The wages of pharmacists decrease and the wages of salespeople do not. I interpret the first findings as suggestive that the merger increased firms' market power regarding both pharmacists and salespeople. I contemplate two possible reasons why the relative wages of incumbent pharmacists do not fall: (i) pharmacists have a strong union that sets wage floors and annual increases at a regional level for incumbent workers and (ii) drugstores are required to have a pharmacist working at all times, which constrains firms' maximization problem.

The literature on the labor market effects of mergers is still nascent compared with the extensive industrial organization (IO) literature on the product market effects of mergers. Two characteristics in this paper may sound obvious to IO economists but are still incipient in labor economics. First, the analysis of a single merger in a specific sector provides a narrative that might be useful to other mergers and sectors. In contrast, the aggregate analysis of multiple mergers may underscore important differences across industries and markets. Second, in IO, comparison of the prices of a product over time will necessarily use the price per unit of a product, where the unit is standardized across all observations. This standardization is more difficult in labor economics, since people are harder to compare than products. By including individual controls such as age and worker fixed effects, this paper shows the importance of standardizing a unit of labor when comparing wages over time.

I present evidence that labor markets can be finer than the year-county intersection. Researchers examining labor markets typically use the intersection between year and regional units such as states, metropolitan areas, commuting zones, or counties to approximate labor markets. In many cases, these choices are guided by data availability. The results in this paper give empirical support to recent work by Manning and Petrongolo (2017); Nimczik (2020); and Schubert et al. (2021), who use data on vacancies and worker flows to show that labor markets are more local than previously thought. In my setting, the merger should not have reduced wages if the labor market had been defined at the county level. However, the merger reduced the wages of salespeople, which suggests that labor markets may be defined by the intersection of months, occupation, industry, county, and firm characteristic, such as firm size. Workers might have strong preferences for one types of drugstore: I show that salespeople from pharmacy chains are more likely to switch to another pharmacy chain than to an independent drugstore. The use of job mobility and firm characteristics to define labor markets is an interesting avenue for future research.

This paper also investigates how firms strategically interact in the labor market. I show that, following a wage reduction for salespeople, competing firms increased their labor demand. The theoretical framework shows that the effects of the merger on wages greatly depend on the type of competition between firms. For example, if firms acted as strategic complements, the wage decrease for salespeople could be even greater. That said, how firms compete in other labor markets remains an open question in labor economics. Hence future research should be careful when defining firm competition in the labor market.

Finally, this paper studied the effects of a merger through the mechanism of market power, net of synergies. Regulatory agencies such as the DOJ, FTC, and CADE might be more interested in the total effects of merger on wages, including synergies. The empirical strategy in this paper does not address these outcomes, since synergies and other shocks to the labor market are not separately identified. As far as I know, the merger in this paper could have had strong synergies that actually increased wages.

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Figures

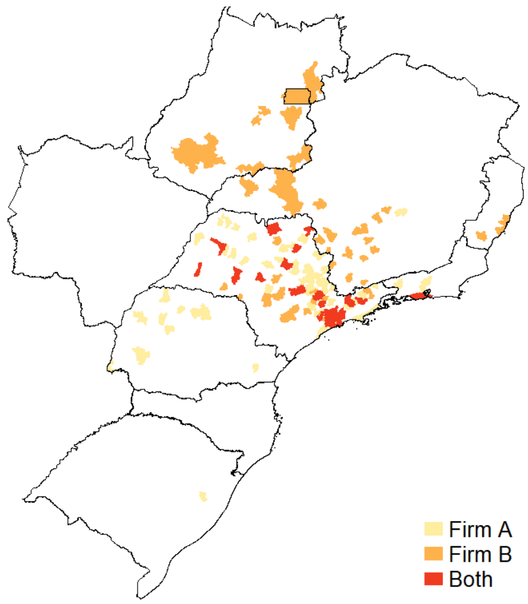
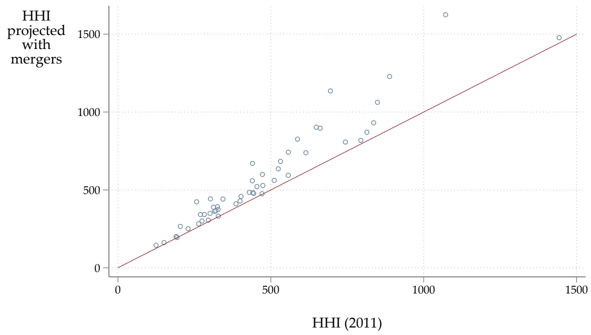
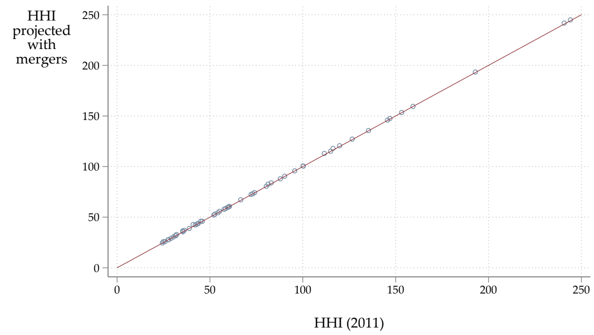


Figure 2: Presence of merging firms before the merger (2010)

Note: The figure presents a map of the south of Brazil and highlights counties in which only one of the merging firms had an establishment (69 in light yellow and 54 in orange) and counties in which both firms had establishments prior to the merger (31 in red). The area in white denotes counties in which firms were not present in 2010.



((a)) Pharmacists



((b)) Salespeople

Figure 3: HHI projected with mergers versus 2011 HHI

Note: Each circle in the figure represents a county in which merging firms overlapped. The horizontal axis displays the 2011 Herfindahl–Hirschman Index (HHI) in the labor market of pharmacists (panel A) and salespeople (panel B). The HHI is computed using the shares of all firms within a county that hired workers in these occupations. The vertical axis displays the 2011 HHI projected with the merger. The projection serves to calculate a change in HHI that is due to the merger and not to the entrance and exit of firms or changes in firms' employment shares.

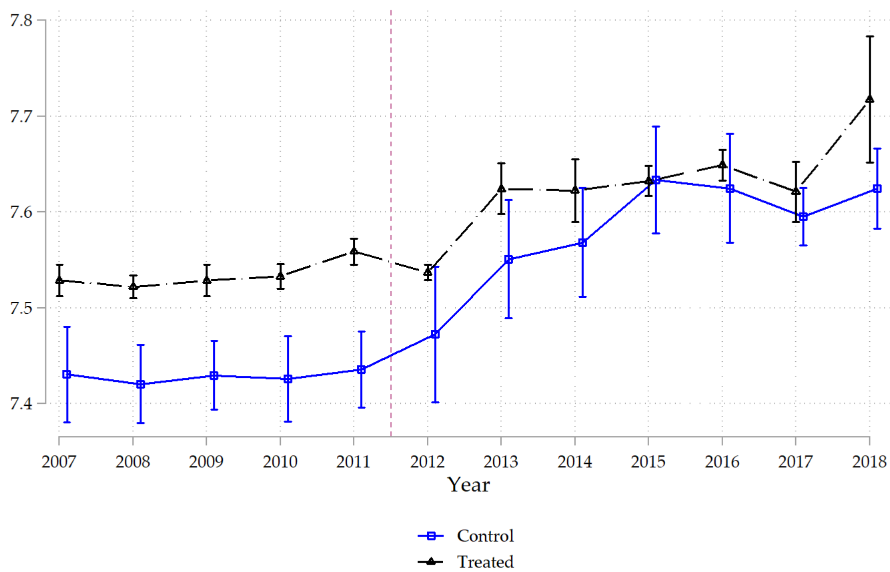
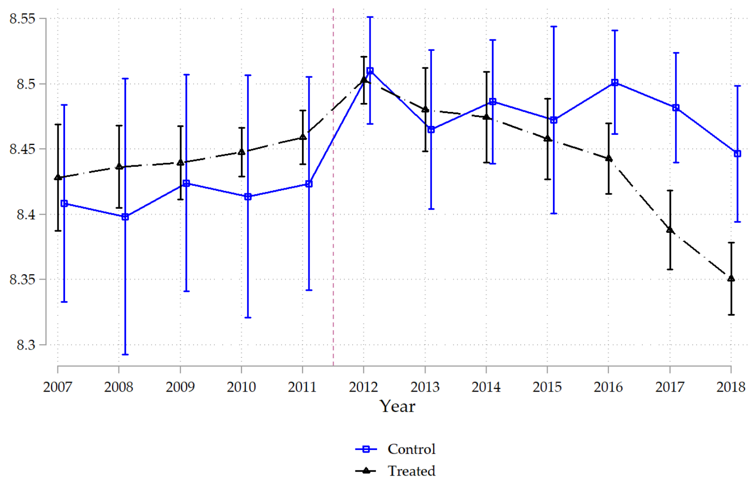
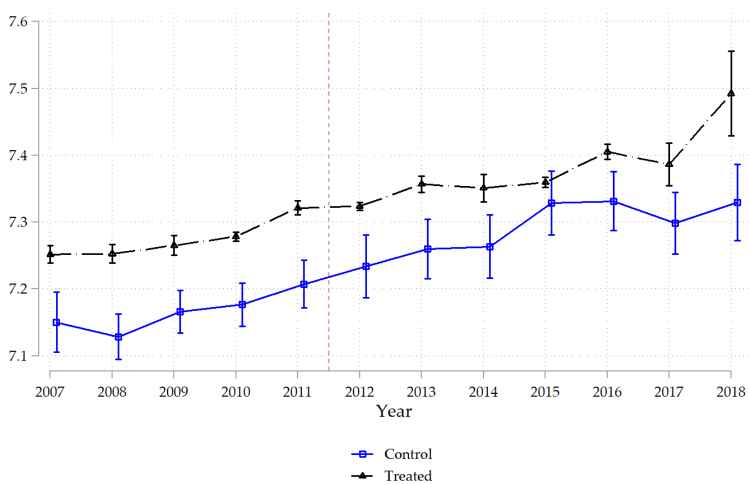


Figure 4: Trends - Ln(wage) of all workers in merging firms

Note: The figure presents the average ln(wage) in treated and control groups. Estimates come from a regression that includes all individuals working in merging firms, from 2007 to 2018. Regressions do not include any additional controls. Log wages are measured in December. The sample only includes workers employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.



((a)) Pharmacists



((b)) Salespeople

Figure 5: Trends - Ln(wage) of workers in merging firms

Note: The figure presents the average ln(wage) in treated and control groups. Estimates come from a regression that includes pharmacists (panel A) or salespeople (panel B) working in merging firms, from 2007 to 2018. Regressions do not include any additional controls. Log wages are measured in December. The sample only includes workers employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.

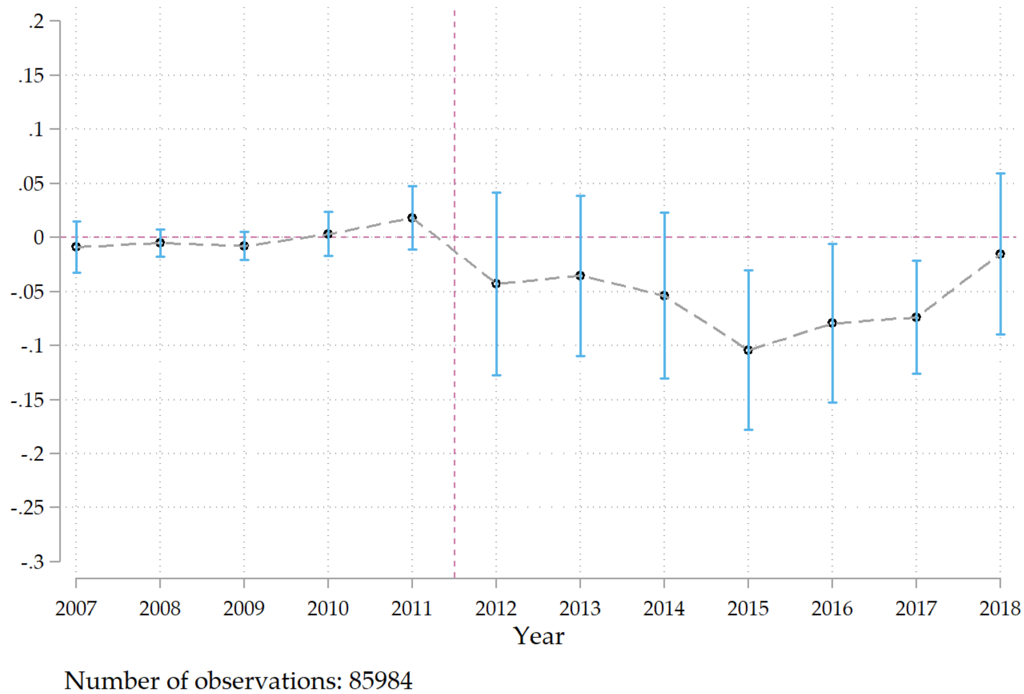
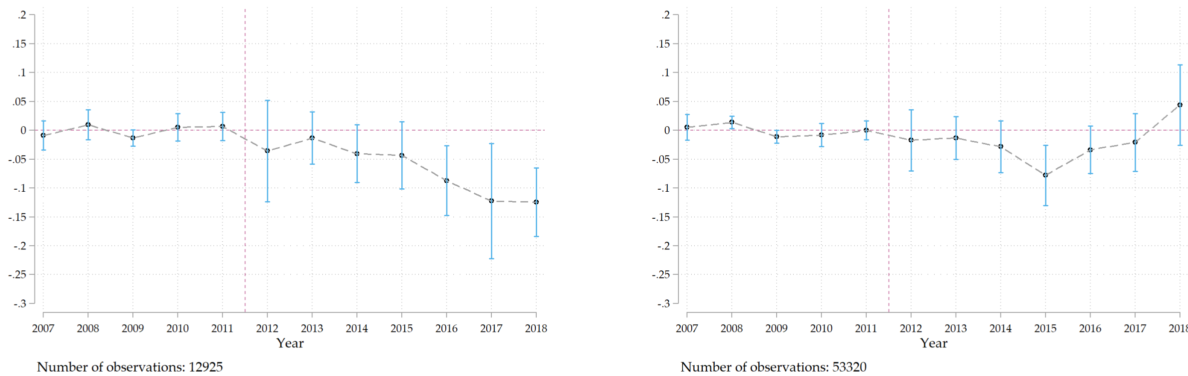


Figure 6: Leads and lags - Ln(wage) of all workers in merging firms

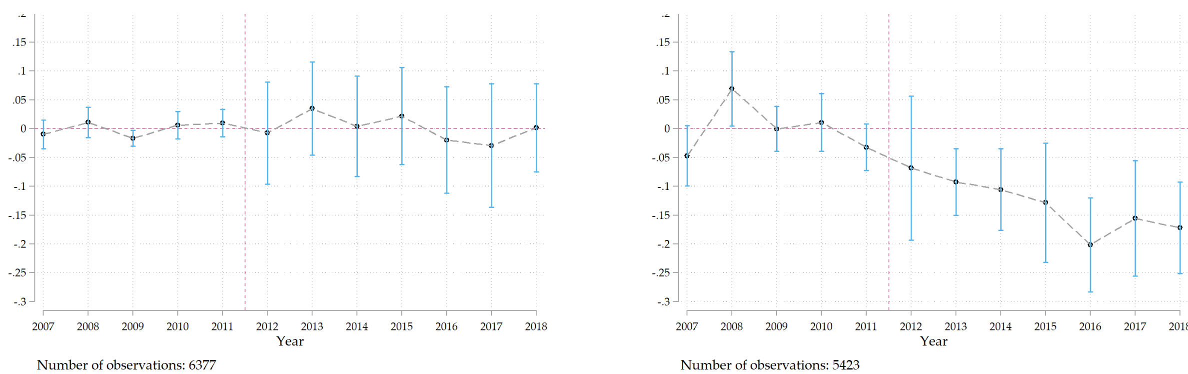
Note: The figure presents leads and lags estimates. The baseline group is the mean of pre-treatment differences between treated and control groups. Point estimates and standard errors are obtained with a two-step approach. First, I regress log wages on the interaction between year and treatment indicators. The regression is run at the individual level and does not include any additional controls. Second, I use a linear transformation to calculate demeaned point estimates and their respective standard errors. Log wages are measured in December. The sample only includes workers employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.



((a)) Pharmacists ((b)) Salespeople

Figure 7: Leads and lags - Ln(wage) of workers in merging firms

Note: The figures present leads and lags estimates. The baseline group is the mean of pre-treatment differences between treated and control groups. Point estimates and standard errors are obtained with a two-step approach. First, I regress log wages on the interaction between year and treatment indicators. The regressions are run at the individual level and do not include any additional controls. Second, I use a linear transformation to calculate demeaned point estimates and their respective standard errors. Log wages are measured in December. The sample only includes pharmacists (Panel A) or salespeople (Panel B) employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.



((a)) Incumbent workers ((b)) Entry-level wage

Figure 8: Leads and lags - Ln(wage) of pharmacists in merging firms

Note: The figures present leads and lags estimates. The baseline group is the mean of pre-treatment differences between treated and control groups. Point estimates and standard errors are obtained with a two-step approach. First, I regress log wages on the interaction between year and treatment indicators. The regressions are run at the individual level and do not include any additional controls. Second, I use a linear transformation to calculate demeaned point estimates and their respective standard errors. Log wages are measured in December. The sample only includes pharmacists that are incumbent (Panel A) or newly hired (Panel B) and are employed on December 31. Incumbent workers are individuals who were hired before the merger. Entry-level, or newly hired, workers are individuals who were hired in the current year. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.

Tables

Table 1: Sample size

	Drugstores Sample		Fully Connected
Number of...	Merging firms	Competitors	Sample
Counties	91	91	2,812
Firms	2	9,798	3,328,498
Establishments	392	11,212	4,273,843
Workers	51,380	497,658	29,260,227
Observations	134,216	1,586,235	189,072,473

Note: "Drugstores sample" refers to the balanced panel of establishments in the retail pharmacy sector, where establishments belong to a county in which merging firms were present in 2010. The fully connected sample includes establishments from all sectors that are connected through worker mobility to establishments in the drugstore sample. RAIS 2007—2018.

Table 2: Summary Statistics of treated and control groups

	Control		Treated	
	Mean	Obs.	Mean	Obs.
Panel A: County level				
Population (2010) (thousands)	232.6	61	914.8	30
Government revenue (2006) (R$ pc)	2,443.6	61	2,925.4	30
HDI (2000)	0.81	61	0.83	30
% Informality (2010)	28.1	61	25.7	30
Pharmacists HHI (2011)	669	61	478	30
Panel B: Individual Level (2011)				
% Male	32.8	2374	30.5	5660
Age	23.55	2374	24.43	5660
Ln(wage) of all workers	7.44	2374	7.57	5665
Ln(wage) of pharmacists	8.43	286	8.47	738
Ln(wage) of salespeople	7.21	1608	7.33	3519

Note: Population data are from the 2010 census. Government revenue data are from the year 2006 and were accessed through Ipeadata. HDI refers to the human development index of 2000, also acessed through Ipeadata. Informality is accessed from the 2010 Census and defined as the share of workers who do not contribute to the social security system. Pharmacists' HHI refers to the Herfindahl–Hirschman Index for the labor market of pharmacists, where a market is defined by the county and occupation intersection. HHI is computed using 2011 RAIS data. All individual-level characteristics are computed for individuals working in a balanced panel of establishments of merging firms in 2011 using RAIS data.

Table 3: Difference-in-differences estimates. Dependent variable is Log(wage)

	(1)	(2)	(3)	(4)	(5)
Panel A: Pharmacists in Merging Firms					
Treat × Post	-0.056*	-0.079**	-0.069**	-0.026	-0.028
	(0.032)	(0.034)	(0.032)	(0.035)	(0.035)
Panel B: Salespeople in Merging Firms					
Treat × Post	-0.024	-0.007	-0.021	-0.037*	-0.035*
	(0.018)	(0.025)	(0.017)	(0.019)	(0.018)
Age controls		X	X	X	X
Individual FE				X	X
Establishment FE			X	X	X
Year FE	X	X	X	X	
Year FE × County chars					X
Observations	189,072,473	189,072,473	189,072,473	189,072,473	189,072,473
Number of workers	29,260,227	29,260,227	29,260,227	29,260,227	29,260,227
Number of establishments	4,273,843	4,273,843	4,273,843	4,273,843	4,273,843

Note: Selected coefficients from the difference-in-differences estimate as in Equation 11, where the unit of observation is at the individual level. Log wages are measured in December and the sample only includes workers employed on December 31. Standard errors are clustered at the county level and presented in parentheses.

Table 4: Difference-in-differences estimates. Dependent variable is Log(wage)

	(1)	(2)	(3)	(4)	(5)
Panel C: Pharmacists in Competing Firms					
Treat × Post	-0.032*	-0.026	-0.016	0.027	0.025
	(0.016)	(0.019)	(0.018)	(0.017)	(0.018)
Panel D: Salespeople in Competing Firms					
Treat × Post	-0.046**	-0.042*	-0.035	-0.033**	-0.032**
	(0.019)	(0.022)	(0.025)	(0.014)	(0.015)
Age controls		X	X	X	X
Individual FE				X	X
Establishment FE			X	X	X
Year FE	X	X	X	X	
Year FE × County chars					X
Observations	189,072,473	189,072,473	189,072,473	189,072,473	189,072,473
Number of workers	29,260,227	29,260,227	29,260,227	29,260,227	29,260,227
Number of establishments	4,273,843	4,273,843	4,273,843	4,273,843	4,273,843

Note: Selected coefficients from the difference-in-differences estimate as in Equation 11, where the unit of observation is at the individual level. Log wages are measured in December and the sample only includes workers employed on December 31. Standard errors are clustered at the county level and presented in parentheses.

Table 5: Difference-in-differences estimates. Other outcomes

Outcome:	Employment	Ln(Age)	Worker FE
	(1)	(2)	(3)
Panel A: Pharmacists in Merging firms			
Treat × Post	-0.179	0.025	-0.036
	(0.238)	(0.015)	(0.024)
Observations	4,690	12,449	12,157
Number of workers	-	5,158	4,965
Number of establishments	392	392	392
Panel B: Salespeople in Merging firms			
Treat × Post	0.103	-0.012	0.016
	(0.566)	(0.014)	(0.011)
Observations	4,690	50,869	48,293
Number of workers	-	28,826	26,582
Number of establishments	392	392	392
Year FE	X	X	X

Note: Each cell represents the estimate of the difference-in-differences parameter from Equations 8 or 10, depending on the unit level. Regressions in column 1 are at the establishment level and regressions in columns 2 and 3 are at the individual level. Regressions include year and county fixed effects. Standard errors are clustered at the county level and presented in parentheses.

Table 6: Difference-in-differences estimates. Other outcomes - Competitors

Outcome:	Employment	Ln(Age)	Worker FE
	(1)	(2)	(3)
Panel C: Pharmacists in Competitors			
Treat × Post	0.187*	-0.017***	-0.048***
	(0.098)	(0.006)	(0.012)
Observations	127,710	82,421	72,450
Number of workers	-	26,443	23,107
Number of establishments	10,839	6,090	5,800
Panel D: Salespeople in Competitors			
Treat × Post	1.078**	-0.015***	-0.016
	(0.522)	(0.005)	(0.016)
Observations	127,710	500,865	466,628
Number of workers	-	202,291	181,171
Number of establishments	10,839	10,173	9,920
Year FE	X	X	X

Note: Each cell represents the estimate of the difference-in-differences parameter from Equations 8 or 10, depending on the unit level. Regressions in column 1 are at the establishment level and regressions in columns 2 and 3 are at the individual level. Regressions include year and county fixed effects. Standard errors are clustered at the county level and presented in parentheses.

A Appendix Figures

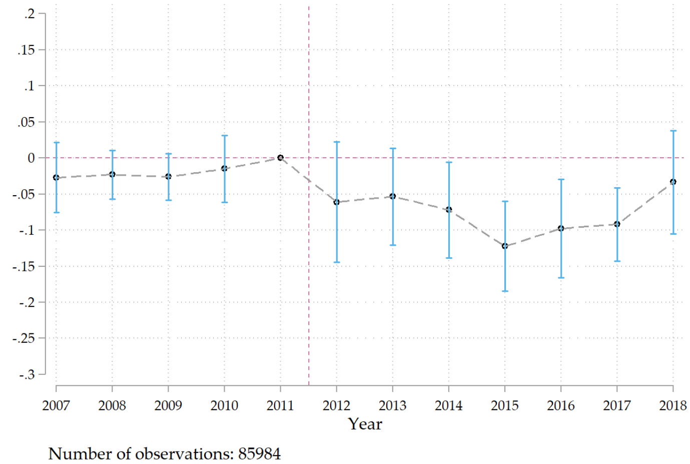
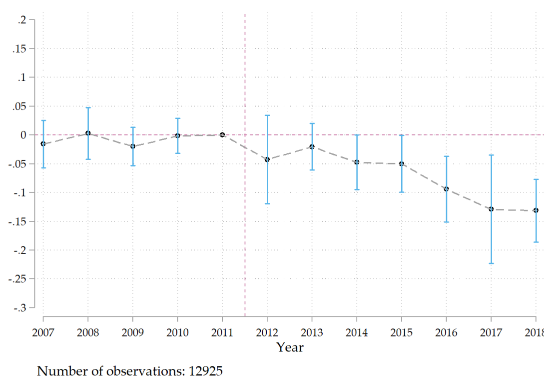
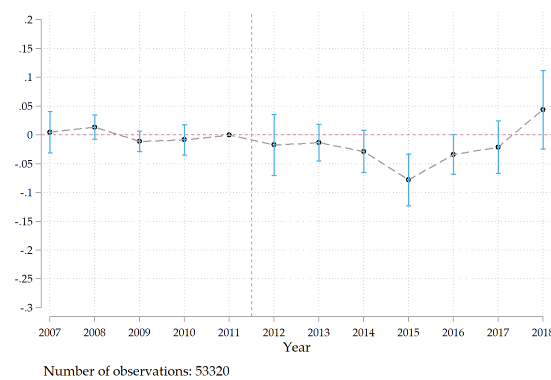


Figure A.1: Leads and lags - Ln(wage) of all workers in merging firms

Note: The figure presents estimates of the parameters in Equation 9. The omitted category is the difference between treated and control groups in 2011. The regression is run at the individual level and does not include any additional controls. Log wages are measured in December. The sample only includes workers employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.



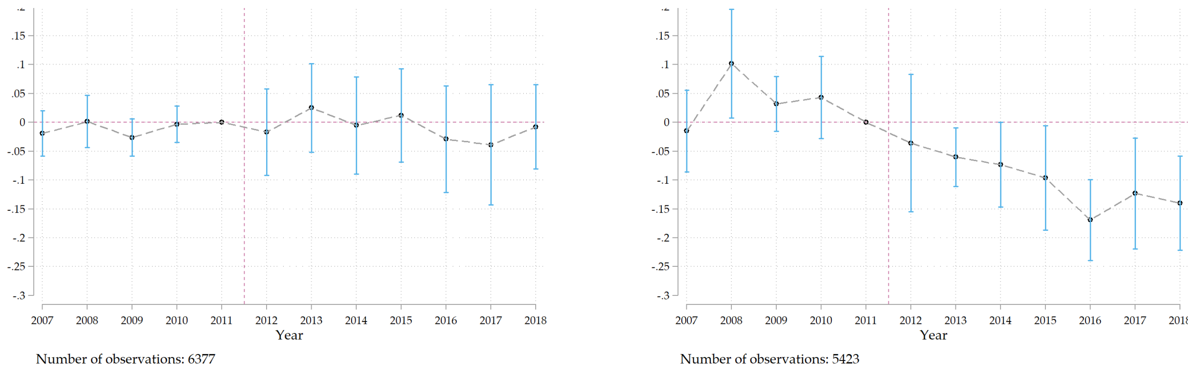
((a)) Pharmacists



((b)) Salespeople

Figure A.2: Leads and lags - Ln(wage) of workers in merging firms

Note: The figure presents estimates of the parameters in Equation 9. The omitted category is the difference between treated and control groups in 2011. The regression is run at the individual level and does not include any additional controls. Log wages are measured in December. The sample only includes pharmacists (Panel A) or salespeople (Panel B) employed on December 31. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.



((a)) Incumbent workers

((b)) Entry-level wage

Figure A.3: Leads and lags - Ln(wage) of pharmacists in merging firms

Note: The figure presents estimates of the parameters in Equation 9. The omitted category is the difference between treated and control groups in 2011. The regression is run at the individual level and does not include any additional controls. Log wages are measured in December. The sample only includes pharmacists who are incumbent (Panel A) or newly hired (Panel B) and are employed on December 31. Incumbent workers are individuals who were hired before the merger. Entry-level, or newly hired, workers are individuals who were hired in the current year. The sample is composed of a balanced panel of establishments. Standard errors are clustered at the county level.

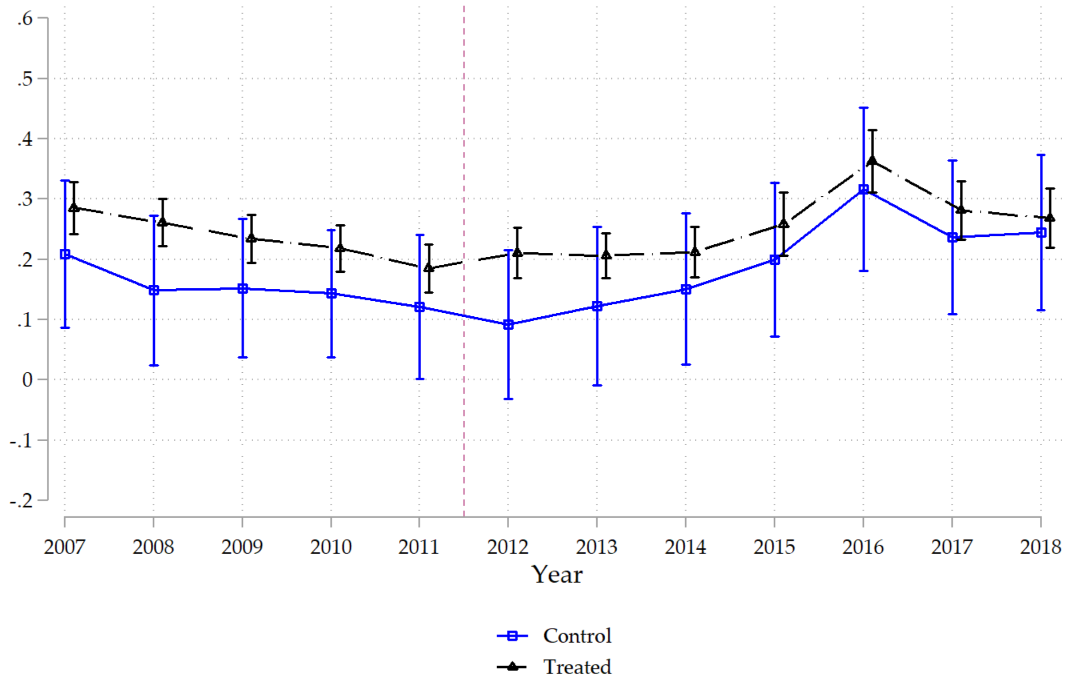
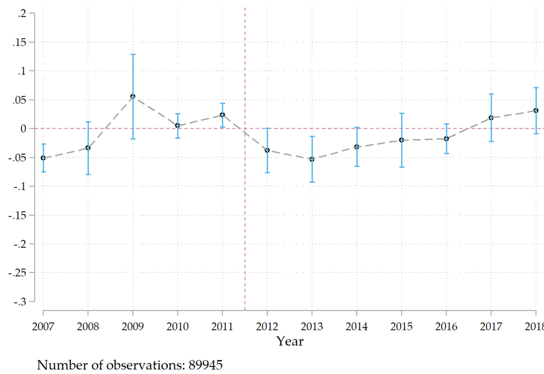


Figure A.4: Residual ln(wage) of pharmacists in merging firms with two-way fixed effects

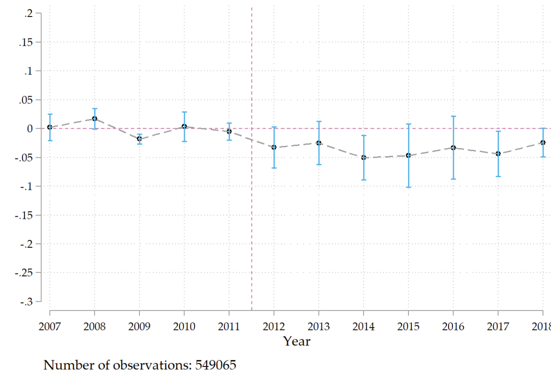
Note: The figure presents the residual ln(wage) in treated and control groups for pharmacists in merging firms. Estimates come from a regression that uses the fully connected set and includes individual characteristics and worker and establishment fixed effects. Unlike Equation 11, treatment and control indicators are interacted with all year indicators and not just the post indicator. The figure reports the estimate and standard errors of the parameters associated with these interactions. Log wages are measured in December. The sample only includes workers employed on December 31. Standard errors are clustered at the county level.

Figure A.5: Leads and lags - Ln(wage) of all workers in competing firms

Note: The figure presents leads and lags estimates. The baseline group is the mean of pre-treatment differences between treated and control groups. Point estimates and standard errors are obtained with a two-step approach. First, I regress log wages on the interaction between year and treatment indicators. The regression is run at the individual level and does not include any additional controls. Second, I use a linear transformation to calculate demeaned point estimates and their respective standard errors. Log wages are measured in December. The sample only includes workers employed on December 31. The sample is composed of a balanced panel of establishments of competing firms. Standard errors are clustered at the county level.



((a)) Pharmacists



((b)) Salespeople

Figure A.6: Leads and lags - Ln(wage) of workers in competing firms

Note: The figures present leads and lags estimates. The baseline group is the mean of pre-treatment differences between treated and control groups. Point estimates and standard errors are obtained with a two-step approach. First, I regress log wages on the interaction between year and treatment indicators. The regressions are run at the individual level and do not include any additional controls. Second, I use a linear transformation to calculate demeaned point estimates and their respective standard errors. Log wages are measured in December. The sample only includes pharmacists (Panel A) or salespeople (Panel B) employed on December 31. The sample is composed of a balanced panel of establishments of competing firms. Standard errors are clustered at the county level.

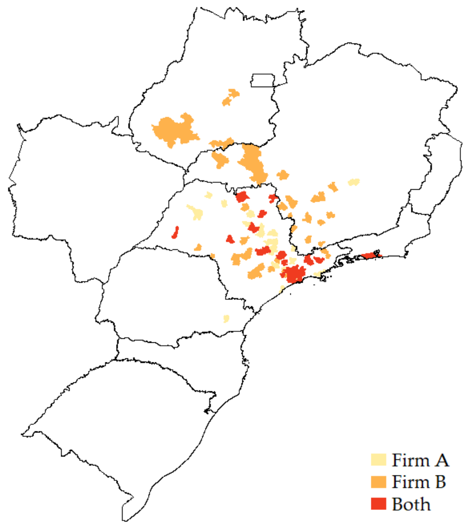


Figure A.7: Presence of merging firms before the merger - Balanced panel of establishments (2010)

Note: The figure presents a map of the south of Brazil and highlights counties in which only one of the merging firms had an establishment (24 in light yellow and 36 in orange) and counties in which both firms had establishments prior to the merger (30 in red). The area in white denotes counties in which firms were not present in 2010. Note: The map only includes establishments that were open uninterruptedly between 2007 and 2018 but the color definition uses all establishments.

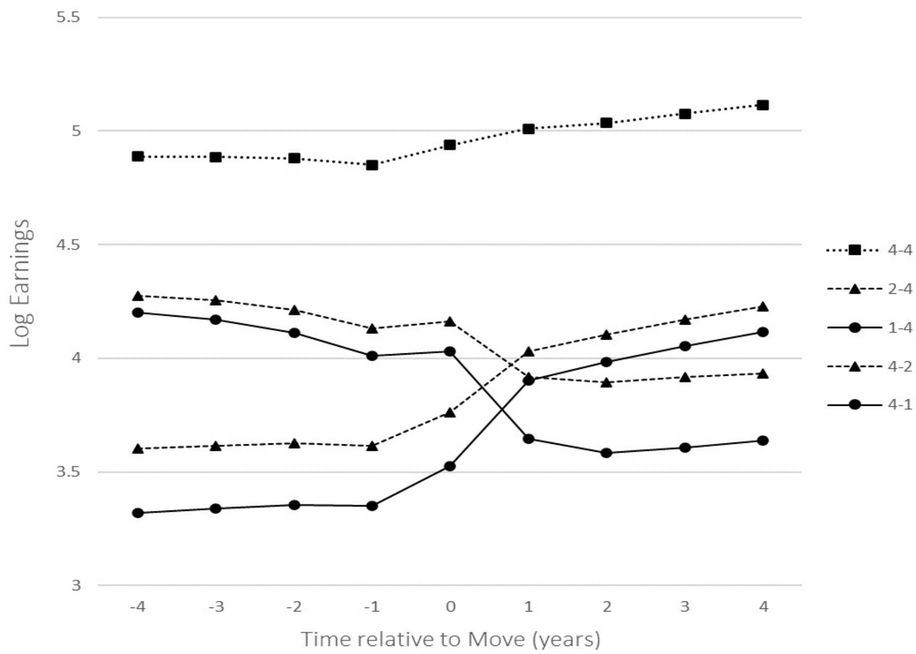


Figure A.8: Event Study of Changes in Earnings when Workers Move Between Firms

Note: In this figure, I classify firms into four equally sized groups based on the mean earnings of non-movers in the firm (with 1 and 4 being the group with the lowest and highest mean earnings, respectively). I then compute mean log earnings for the workers who move between these groups of firms in the years before and after the move. Note that the employer differs between event times -1 and 1, but we do not know exactly when the change in employer occurred. Thus, to avoid concerns over workers exiting and entering employment during these years, one might prefer to compare earnings in event years -2 and 2.

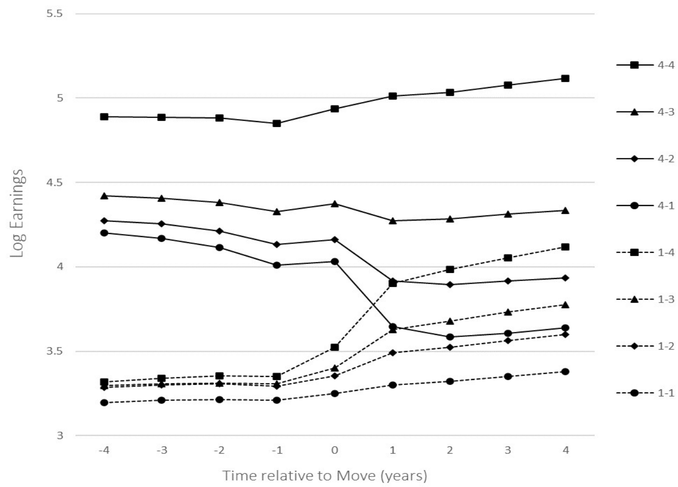


Figure A.9: Event Study of Changes in Earnings when Workers Move Between Firms

Note: In this figure, I classify firms into four equally sized groups based on the mean earnings of non-movers in the firm (with 1 and 4 being the group with the lowest and highest mean earnings, respectively). I then compute mean log earnings for the workers who move between these groups of firms in the years before and after the move. Note that the employer differs between event times -1 and 1, but we do not know exactly when the change in employer occurred. Thus, to avoid concerns over workers exiting and entering employment during these years, one might prefer to compare earnings in event years -2 and 2.

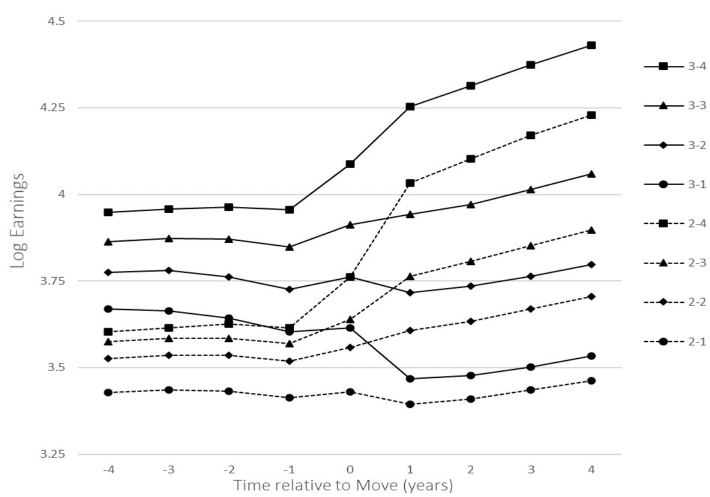
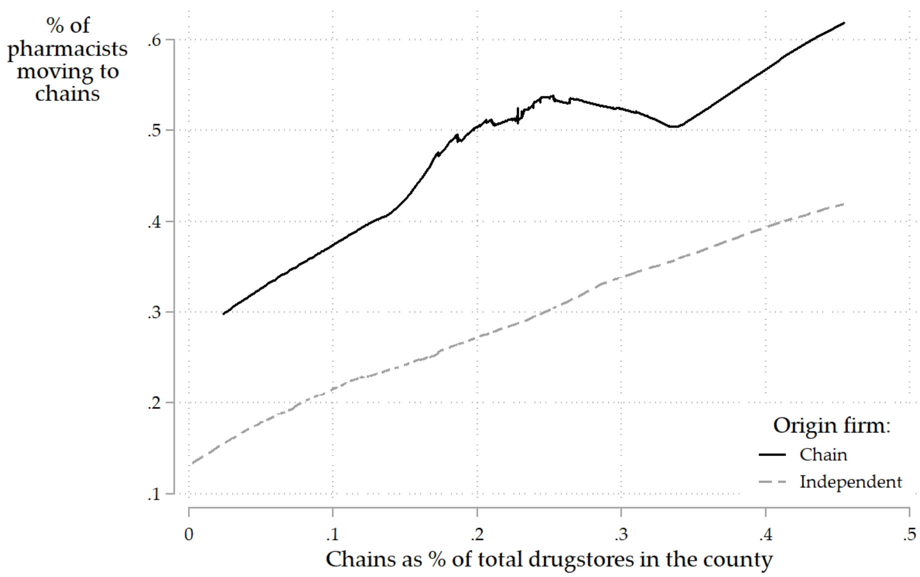
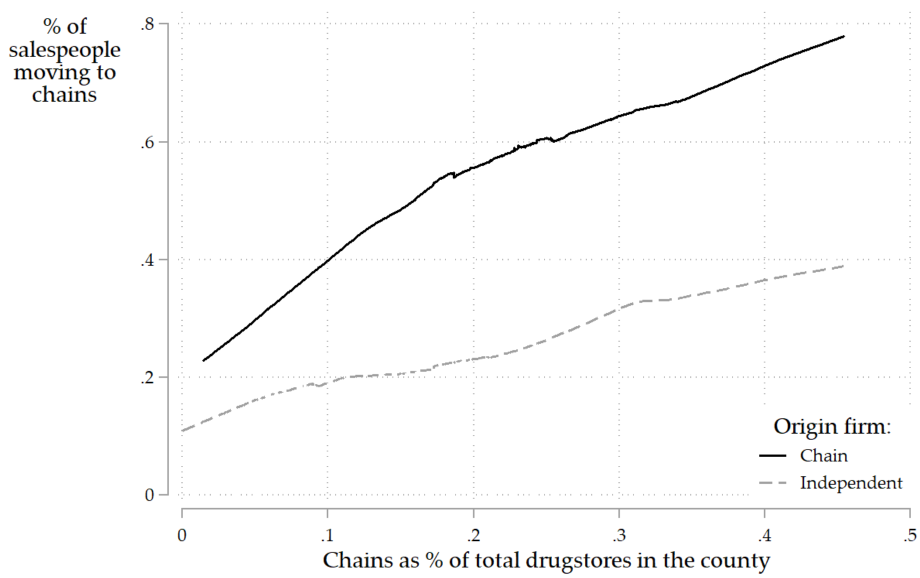


Figure A.10: Event Study of Changes in Earnings when Workers Move Between Firms

Note: In this figure, I classify firms into four equally sized groups based on the mean earnings of non-movers in the firm (with 1 and 4 being the group with the lowest and highest mean earnings, respectively). I then compute mean log earnings for the workers who move between these groups of firms in the years before and after the move. Note that the employer differs between event times -1 and 1, but we do not know exactly when the change in employer occurred. Thus, to avoid concerns over workers exiting and entering employment during these years, one might prefer to compare earnings in event years -2 and 2.



((a)) Pharmacists



((b)) Salespeople

Figure A.11: Job transition probabilities conditional on moving to another drugstore

Note: The figures plot nonparametric regressions at the county level. The sample includes individuals working in drugstores in 2009 who switched jobs between 2009 and 2010 and stayed at a drugstore. The dependent variable is the percentage of pharmacists (Panel A) or salespeople (Panel B) who were working in a pharmacy chain in 2010. This is regressed on the ratio between the number of establishments from pharmacy chains and the total number of pharmacies in the county. The figure separates individuals who were working in 2010 in a pharmacy chain (continuous line) and individuals who were working in an independent pharmacy (dashed line). Source: RAIS 2009-2010.

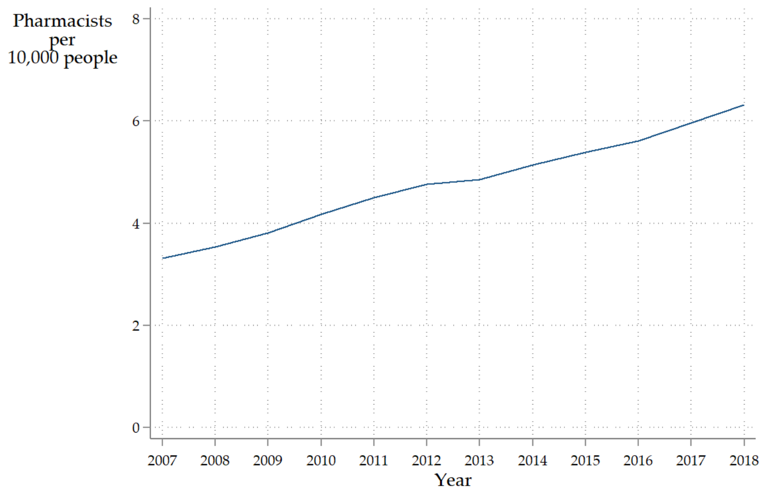


Figure A.12: Pharmacists per capita - Brazil

Note: The figure plots the evolution in the number of pharmacists per capita. Pharmacist data comes from RAIS 2007-2018. Yearly population estimates come from the Census and Ipeadata.

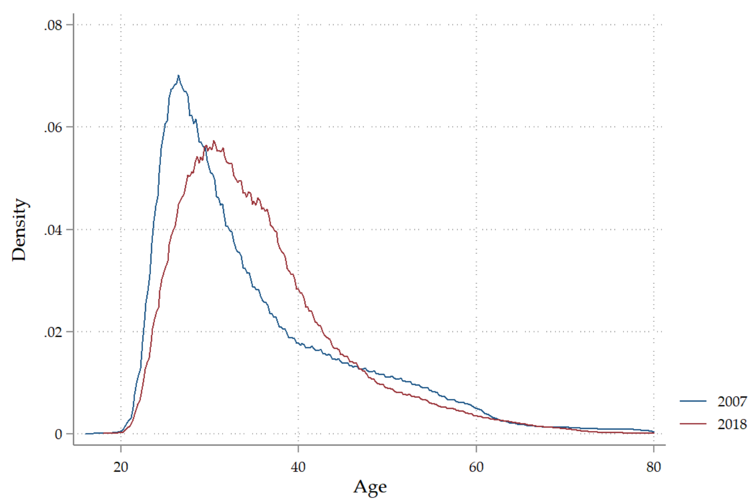


Figure A.13: Age distribution of pharmacists - Brazil

Note: The figure plots the age distribution of pharmacists in 2007 and 2018. The age distribution shifts to the right from 2007 to 2018. Source: RAIS 2007-2018.

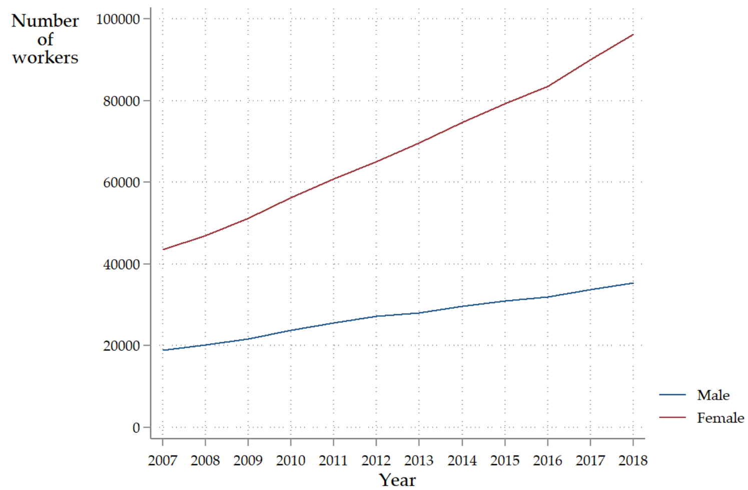


Figure A.14: Trends in pharmacists' gender composition - Brazil

Note: The figure plots the evolution in the gender composition of pharmacists from 2007 to 2018. The share of female pharmacists raises from 69% in 2007 to 73% in 2018. Source: RAIS 2007-2018.

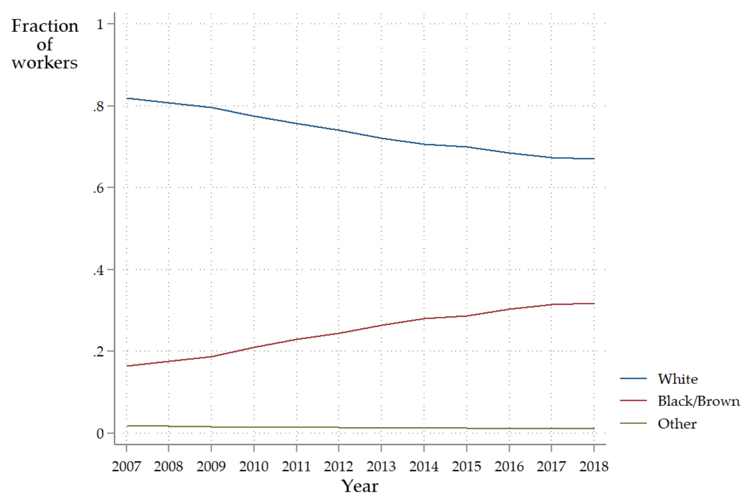


Figure A.15: Trends in pharmacists' race composition - Brazil

Note: The figure plots the evolution in the race composition of pharmacists from 2007 to 2018. The profession gets more representative of the Brazilian population over time, with the share of whites falling from 81% to 67% and the share of black and brown increasing from 18% to 32%. Source: RAIS 2007-2018.

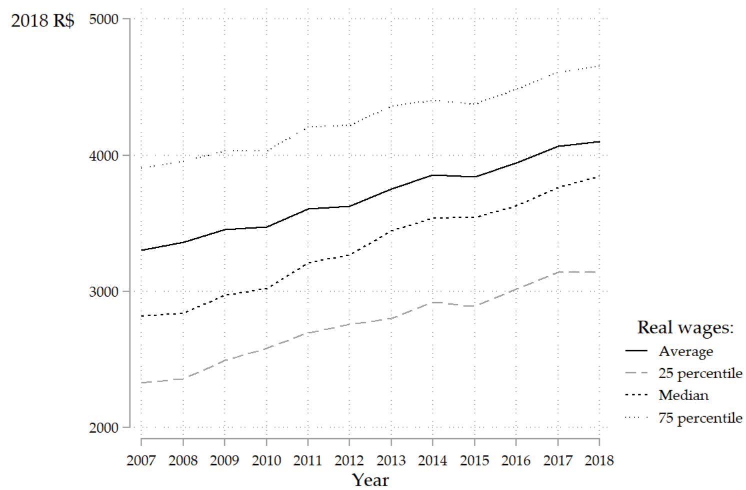


Figure A.16: Trends in pharmacists' wage distribution - Brazil

Note: The figure plots the evolution in the wage distribution of pharmacists from 2007 to 2018. Real wages are reported in 2018 reais. Source: RAIS 2007-2018.

B Appendix Tables

Table B.1: Sample size

	All		Relevant Counties		Balanced		
Number of...	Merging	Competitor	Merging	Competitor	Merging	Competitor	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Counties	319	3,698	154	154	91	153	91
Firms	2	91,379	2	63,439	2	12,766	9,798
Establishments	1,821	114,761	1,404	77,187	392	14,751	11,212
Workers	92,267	1,228,711	91,505	1,225,704	51,380	632,906	497,658
Observations	269,877	4,361,448	261,827	4,029,495	134,216	2,015,316	1,586,235
Workers:							
Pharmacists	8,024	74,358	7,722	62,326	3,190	27,177	20,919
Salespeople	64,597	741,511	63,918	712,101	33,579	323,486	256,218
% of Observations:							
Pharmacists	16.3	11.0	15.9	9.4	13.2	8.5	8.3
Salespeople	61.8	53.7	62.8	54.0	66.0	53.3	53.4

Note: The table presents sample sizes. The first two columns do not restrict the sample. They include all observations in merging firms and their competitors from 2007 to 2018. Columns 3 and 4 restrict the sample to counties in which merging firms had an establishment in 2010. Column 6 and 7 restrict the sample to establishments that had at least one employee in every year between 2007 and 2018. Column 8 restricts the sample of competitors to the same 91 counties from the sample of merging firms. Source: RAIS 2007-2018.

Table B.2: Descriptive statistics on the retail pharmacy sector

	2007	2018	Annual growth (%)
Number of...			
Firms	26,187	30,094	1.3
Establishments	33,216	47,400	3.3
Employees	233,400	403,228	5.1
Pharmacists	39,905	78,977	6.4
Salespeople	121,897	216,055	5.3
Total Revenue (2018 R$)(billions)	59.9	153.7	8.9
Minimum wage (2021 R$)	807	1086	2.7
Avg. wage/min. wage:			
All workers	2.3	2.3	0.0
Pharmacists	4.6	4.2	-0.8
Salespeople	1.7	1.65	-0.3
% Completed High school	78	91	1.4
% Female	58	64	0.9

Note: Revenue data come from the Brazilian annual retail trade survey (PAC, Pesquisa Anual de Comércio). Minimum wage data are extracted from Ipeadata and deflated using the consumer price index. The rest of the statistics are based on the RAIS dataset and include the full count of establishments in the retail pharmacy sector that had at least one pharmacist.

Table B.3: Robustness. Difference-in-differences estimates. Dependent variable is Log(wage)

	December Wage				Average Wage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pharmacists in Merging Firms								
Treat	0.029	0.027	0.031		0.026	0.025	0.030	
	(0.045)	(0.044)	(0.044)		(0.056)	(0.054)	(0.054)	
Treat × Post	-0.073	-0.077	-0.076	-0.064	-0.076	-0.082	-0.082	-0.066
Cluster: County	(0.029)**	(0.029)***	(0.028)***	(0.029)**	(0.033)**	(0.033)**	(0.032)**	(0.030)**
Cluster: County X Year	(0.025)**	(0.025)***	(0.025)***	(0.013)***	(0.030)**	(0.030)***	(0.030)***	(0.014)***
Cluster: Establishment	(0.013)***	(0.013)***	(0.013)***	(0.012)***	(0.013)***	(0.013)***	(0.013)***	(0.012)***
Robust	(0.008)***	(0.008)***	(0.008)***	(0.007)***	(0.008)***	(0.008)***	(0.008)***	(0.007)***
Age controls		X	X	X	X	X	X	X
Other individual characteristics			X	X			X	X
Individual FE								
Establishment FE								
County FE				X				X
Year FE	X	X	X	X	X	X	X	X
Observations	12,328	12,325	12,324	12,324	18,776	18,772	18,771	18,771
Number of workers	5,127	5,127	5,127	5,127	6,612	6,612	6,612	6,612
Number of establishments	392	392	392	392	392	392	392	392

Note: The table presents estimate of the difference-in-differences parameter from Equations 10 at the individual level. The dependent variable is either the log of December wage or log of average wages within a year. Columns 1 to 4 only include workers employed on December 31. Columns 5 to 8 include more observations, since workers can have more than one employer in a year.

Table B.4: Robustness. Difference-in-differences estimates. Dependent variable is Log(wage)

Sample restrictions	All restrictions (1)	All, with weights (2)	includes outliers (3)	includes 2012 (4)	includes all establishments (5)
Panel A: Pharmacists in Merging Firms					
Treat × Post	-0.066**	-0.061**	-0.062*	-0.056**	-0.096***
	(0.030)	(0.028)	(0.031)	(0.026)	(0.030)
Age controls	X	X	X	X	X
Other individual characteristics	X	X	X	X	X
Individual FE					
Establishment FE					
County FE	X	X	X	X	X
Year FE	X	X	X	X	X
Observations	18,771	18,771	18,960	20,499	43,969
Number of workers	6,612	6,612	6,637	6,746	13,309
Number of establishments	392	392	392	392	1,410

Note: The table presents estimates of the difference-in-differences parameter from Equation 10 at the individual level. Column 1 is similar to column 8 of Table B.3. The dependent variable is log of average wages. Columns 1 and 2 include all sample restrictions. Column 2 uses inverse employment weights, such that establishments have the same weight. Column 3 includes outliers: the top 1% of the observations in terms of wages. Column 4 includes observations from 2012. Column 5 includes all establishments from merging firms, not limiting it to the balanced panel of establishments. Standard errors are clustered at the county level and presented in parentheses.

Table B.5: Difference-in-differences estimates. Dependent variable is ln(wage)

Sample:	Pharmacists in merging firms			
	(1)	(2)	(3)	(4)
Post × Treat	-0.0731**			-0.0628
	(0.0296)			(0.0416)
Post × ΔHHI		-3.878**	-3.818**	-0.870
		(1.876)	(1.908)	(2.345)
Treat	0.0310		-0.0672	-0.0266
	(0.0458)		(0.0434)	(0.0446)
ΔHHI		3.935*	7.070***	5.177***
		(2.215)	(2.002)	(1.180)
Constant	8.401***	8.391***	8.412***	8.399***
	(0.0421)	(0.0305)	(0.0382)	(0.0422)
Observations	12,452	12,452	12,452	12,452
R-squared	0.023	0.022	0.032	0.033

Note: All specifications include year fixed effects. ΔHHI refers to the projected change in the 2011 Herfindahl-Hirschman Index (HHI) due to the merger. A labor market is defined by the interesection between county and occupation. The HHI is divided by 10,000, such that ΔHHI varies between 0 and 1. Average projected change in the HHI is of 0.0111 (or 111 point in the standard HHI scale). Standard errors are clustered at the county level.

Table B.6: Job transition for all workers

	Status in 2010				
Occupation in 2009	Same occupation as in 2009	Same county as in 2009	Same microregion as in 2009	Still works in a pharmacy	N
Pharmacist	83%	73%	81%	73%	50,594
Salespeople	74%	82%	86%	73%	301,893
Manager	77%	83%	87%	82%	30,359

Note: The table shows the job transition probabilities of all workers employed in the retail pharmacy sector in 2009. Columns 1 to 4 show the share of workers who were working in the same occupation, county, micro-region and pharmacy in 2010, respectively. Column 5 shows the sample size for each occupation. Source: RAIS 2009 and 2010.

Table B.7: Job transition for movers

	Status in 2010				
Occupation in 2009	Same occupation as in 2009	Same county as in 2009	Same microregion as in 2009	Still works in a pharmacy	N
Pharmacist	65%	47%	63%	41%	30,098
Salespeople	36%	54%	63%	24%	122,336
Manager	25%	49%	58%	31%	9,008

Note: The table shows the job transition probabilities of movers—that is, workers employed in the retail pharmacy sector in 2009 who were working in a different establishment in 2010. Columns 1 to 4 show the share of workers who were working in the same occupation, county, micro-region and pharmacy in 2010, respectively. Column 5 shows the sample size for each occupation. Source: RAIS 2009 and 2010.

Table B.8: Merger-induced changes in concentration

Labor market definition:	HHI in 2011 (1)	Projected HHI (2)	ΔHHI (3)	%ΔHHI (4)
Panel A: Pharmacists				
County X Occupation X All workers X Year	457	494	37	8.1%
County X Occupation X All workers X Year X Pharmacies	529	623	94	17.7%
County X Occupation X All workers X Year X Pharmacy chain	2,316	2,967	651	28.1%
Panel B: Salespeople				
County X Occupation X All workers X Year	77	77	0	0.2%
County X Occupation X All workers X Year X Pharmacies	433	488	55	12.8%
County X Occupation X All workers X Year X Pharmacy chain	2,269	2,870	601	26.5%
County X Occupation X Newly hired workers X Year	91	91	0	0.3%
County X Occupation X Newly hired workers X Year X Pharmacies	528	621	93	17.6%
County X Occupation X Newly hired workers X Year X Pharmacy chain	2,364	2,927	563	23.8%
County X Occupation X Newly hired workers X Semester X Pharmacy chain	2,987	3,556	569	19.0%
Panel C: Salespeople, 6-digit occupation code				
County X Occupation X All workers X Year	632	755	123	19.5%
County X Occupation X All workers X Year X Pharmacies	935	1,206	271	29.0%
County X Occupation X All workers X Year X Pharmacy chain	3,149	4,543	1,394	44.3%

Note: The table shows the average Herfindahl–Hirschman Index (HHI), projected HHI, and projected changes in the HHI induced by the merger. Averages are taken over the 31 treated counties—i.e., counties in which merging firms overlapped in 2011. Projected changes in the HHI in control counties are equal to zero, and thus are not included. Each line of the table presents the HHI calculations using a different labor market definition. For example, the first line defines a labor market for pharmacists as all employed pharmacists in 2011 within a county. In the second line, I restrict the market to workers in pharmacies. In the third line. I further restrict the sample to pharmacists in pharmacy chains. Pharmacy chains are defined as firms that have more than 5 establishments in 2011, nationwide. I also present calculations in which I define the labor market only for workers who were hired in 2011, i.e. newly hired workers. Lastly, I define the labor market for workers hired in the first semester of 2011 (line 11). I use workers who were employed on December 31 of 2011 in all calculations.

C The retail pharmacy sector in Brazil

In this section, I present more detailed information on the retail pharmacy sector and the evolution of pharmacists' occupation.

What does a pharmacy look like in Brazil? Retail pharmacies, or drugstores, are facilities that sell medications, cosmetics, and pharmaceutical products. Pharmacies may also administer vaccines and compound medication, and can sell some food products for special purposes (Sebrae-SP, 2015). Pharmacies in Brazil are typically smaller than pharmacies in the US, both in terms of space and number of employees. In Brazil, an average drugstore has 8.5 employees, where 1 or 2 of these employees are a pharmacist and 4 to 5 are salespeople (including cashiers). Other employees can be working in managerial tasks, cleaning, product delivery, or organizing inventory.

Even though pharmacies are private companies, they are highly regulated and form part of the Brazilian health system. The list of products a pharmacy is allowed to sell is regulated by state laws and by Anvisa (Agencia Nacional de Vigilancia Sanitaria), the national health regulatory agency. In Brazil, there are also maximum "factory price" and "consumer price" restrictions on a list of essential medicaments. These restrictions are defined at the national level by Anvisa.[23] A pharmacy must also have an employee registered with the state's pharmacy council (Conselho Regional de Farmácia) available at all times when the store is open to the public.[24] In 2014, a new law specified that this professional should be a pharmacist, but this requirement had been binding since at least 2009 via decree.[25] Last, a requirement to work as a pharmacist in Brazil is having a university degree in pharmacy, which usually takes between four and five years.[26]

Informality is not a big issue in the retail pharmacy sector. As in most developing countries, Brazil has high levels of informality. However, probably due to the regulations and enforcement by Anvisa, the retail pharmacy sector has much lower levels of informality compared with other sectors. In 2012, 45% of workers in Brazil were in the informal sector. However, only 17.7% of people working in the sector that includes drugstores, fragrance shops, and shops for other medical and orthodontic products were working informally.[27] Since fragrance shops and shops for other medical and orthodontic products are not as regulated as pharmacies, I expect informality rates at pharmacies to be even lower. I use

[23] CMED Resolution nº2, March 5, 2004.

[24] Law number 5,991 from 1973.

[25] Law number 13,021, from 2014 and Resolução – RDC nº 44/09 da ANVISA.

[26] As a comparison, it usually takes 8 years of college study to earn a pharmacist degree in the U.S.

[27] Own calculations based on the Brazilian household survey (PNAD, Pesquisa Nacional por Amostra de Domicilios).

RAIS data in this paper, which only has information on workers formally employed. Thus, low informality rates are one of the attractive features of studying the retail pharmaceutical sector in Brazil.

Table B.2 shows that the retail pharmacy sector in Brazil was growing during the period of analysis. In 2018, there were around 30,000 firms and 47,000 establishments in the retail pharmacy sector in Brazil—42% more than in 2007. The sector employed around 400,000 workers, with 20% being pharmacists. The total revenue in the sector, of 154 billion reais, corresponded to 2.2% of Brazil's GDP. Between 2007 and 2018, the number of stores increased by 3.3% per year and total revenue in the sector increased by 8.9% per year. The substantial growth in the sector has been attributed to Brazil's economic growth and to higher demand for pharmaceutical products coming from a population that is aging.

The growth in the sector promoted changes in the composition of pharmacists and other workers. Table B.2 shows that the number of individuals working in pharmacies has almost doubled. This is true for both pharmacists and salespeople. Figure A.12 shows that the number of pharmacists per 10,000 residents increased from 3.3 to 6.6 between 2007 and 2018. While this increase did not significantly change the share of female pharmacists (69% to 73%, Figure A.14), it did increase racial diversity in the occupation. Figure A.15 shows that the share of whites decreased from 81% to 67% and the share of black and brown individuals increased from 18% to 32%. Age composition also changed, with an increase of 5 years in the modal age of pharmacists (Figure A.13). Last, the share of workers with a high school degree increased from 78% to 91% (Table B.2).

Despite the increase in employment, the wages of pharmacists also increased in the period. Appendix Table B.2 shows that pharmacists earn on average 4.2 times the minimum wage and salespeople employed in pharmacies earn 1.65 times the minimum wage. Although these ratios are constant or even decreasing over time, both the numerator—real wages—and the denominator—real minimum wage—have increased in the period. Between 2007 and 2018, real minimum wages had a strong increase of 2.7% per year. Figure A.16 shows that the real wages of pharmacists had a steady increase during the same period. Furthermore, wage increases seem to be similar throughout the wage distribution, with the 25th percentile, median wages, average wages, and the 75th percentile all growing by the same rate during the period. The fact that the employment and wages of pharmacists increased suggests that the demand for pharmaceutical services increased as well.

In the 2000s, some pharmacy chains started a consolidation and growth process. This was driven in part by Brazil's economic growth and the development of its financial market. A few groups concluded their IPOs in the mid-2000s. In 2007, firms with more than 100 establishments employed only 14% of those working in pharmacies. At the time, financial analysts expected the sector to go through a consolidation process similar to the U.S. retail pharmaceutical sector. By 2016, firms with more than 100 establishments employed 33% of those working in pharmacies. The share of revenue from larger chains also increased in the same period, reaching around 53% of the sector's revenue in 2016. However, the growth of pharmacy chains in terms of market share has been limited in the past few years and did not reach analysts' expectations. This is attributed to the steady market share of independent pharmacies, supported by the sales of generic medication through a governmental program (RD, 2018).

D Extensive form of Equation 1

$$
\begin{aligned}
\ln(wage)_{it} = \theta_i + \psi_{J(i,t)} &+ \delta_1\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbb{1}[Treat_c + Control_c] \\
&+ \delta_2\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_3\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times Treat_c \\
&+ \delta_4\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ \delta_5\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times \mathbb{1}[Treat_c + Control_c] \\
&+ \delta_6\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_7\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times Treat_c \\
&+ \delta_8\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ \delta_9\ \mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_{10}\mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times Treat_c \\
&+ \delta_{11}\mathbb{1}[J(i,t) \in \text{ Merging firms}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ \delta_{12}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbb{1}[Treat_c + Control_c] \\
&+ \delta_{13}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_{14}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times Treat_c \\
&+ \delta_{15}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Pharmacist}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ \delta_{16}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times \mathbb{1}[Treat_c + Control_c] \\
&+ \delta_{17}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_{18}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Salespeople}] \times Treat_c \\
&+ \delta_{19}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{SalesPeople}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ \delta_{20}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times \mathbb{1}[Treat_c + Control_c] \times Post_t \\
&+ \delta_{21}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times Treat_c \\
&+ \delta_{22}\mathbb{1}[J(i,t) \in \text{ Competitors}] \times \mathbb{1}[occ_{it} = \text{Other occ.}] \times \mathbf{Treat}_c \times \mathbf{Post}_t \\
&+ X_{it}\beta + \varepsilon_{it}
\end{aligned}
$$

E Proof of Proposition 1

Proposition 1: Following a merger, (i) the labor force composition at the merged firms, represented by the ratio l_j/s_j, changes, and (ii) the percentage change in the per worker payroll in the merged firms is not equivalent to the weighted average effect of the merger on wages.

- Throughout, I assume $N > 3$, where N is the number of firms in the market, and assign $i = j$ and $i = k$ to the two merging firms.
- I present a proof for the special case in which (i) the production function is additive on labor inputs, (ii) the linear labor supply for the labor input l: $w^l_j(l_j, l_{-j}) = (A + bL)$, and (iii) the market for the second labor input is perfectly competitive $w^s(S) = w^s$.
- The problem of firm j, before the merger, is

$$\max_{l_j, s_j} l_j^\alpha + s_j^\beta - w^l_j(l_j, l_{-j})l_j - w^s_j(s_j, s_{-j})s_j.$$

  - The first-order conditions for firm j with respect to l_j and s_j, respectively, are

$$\alpha l_j^{\alpha-1} = (A + bL) + bl_j$$
$$\beta s_j^{\beta-1} = w^s.$$

  - From symmetry: $l^*_j = l^*$ and $s^*_j = s^*$.
  - Using the FOCs, we get

$$\alpha l_j^{*\alpha-1} = A + (N + 1)bl^*.$$

  The above equation implicitly defines the equilibrium quantity of labor input l. The quantity of labor input s does not depend on the number of firms in the market and is presented below:

$$s^* = \left(\frac{w^s}{\beta}\right)^{\frac{1}{\beta-1}}$$

- The problem of firm j after the merger is

$$\max_{l_j, s_j, l_k, s_k} l_j^\alpha + s_j^\beta + l_k^\alpha + s_k^\beta - (A + bL)l_j - (A + bL)l_k - w^s s_j - w^s s_k.$$

  - The first-order conditions for firm j with respect to l_j and s_j, respectively, are

$$\begin{aligned} \alpha l_j^{\alpha-1} &= (A + bL) + 2bl_j \\ \beta s_j^{\beta-1} &= w^s. \end{aligned}$$

  - From symmetry: $l_j^{**} = l_k^{**}$ and $l_i^{**} = l^{**}\ \forall i \neq \{j, k\}$. Using the FOC for l:

$$\alpha l_j^{**\alpha-1} = A + (N - 2)bl^{**} + 4bl_j^{**}.$$

  - The competitors' problem does not change, such that we have the same equilibrium condition as before the merger:

$$\alpha l^{**\alpha-1} = A + (N - 1)bl^{**} + 2bl_j^{**}.$$

- Lemma A1: $l^{**} \neq l_j^{**}$
  - *Proof:* Suppose $l^{**} = l_j^{**}$. Then from the equilibrium conditions of competing firms and merging firms: $\alpha l_j^{**\alpha-1} = A + (N+1)bl^{**}$ and $\alpha l_j^{**\alpha-1} = A + (N+2)bl^{**}$. This is a contradiction.
- Lemma A2: $l_j^{**} \neq l^*$
  - *Proof:* Suppose $l_j^{**} = l^*$. Then competitors solve the same problem they were solving before the merger, such that $l^{**} = l^*$. This would imply that $l^{**} = l_j^{**} = l^*$. However, Lemma A1 says that $l^{**} \neq l_j^{**}$. Contradiction.
- Lemma A3: $s^* = s^{**}$
  - *Proof:* The FOC for labor input s does not depend on l and does not change after the merger.
- *Proof of item (i) of Proposition 1:* Directly from Lemmas A2 and A3:

$$\frac{l_j^{**}}{s_j^{**}} = \frac{l_j^{**}}{s^*} \neq \frac{l^*}{s^*} = \frac{l_j^*}{s_j^*}.$$

- Lemma A4: The merger changes relative wages. Given that the labor supply is aggregative on wages, it suffices to show that $L^* \neq L^{**}$.

  - The problem of competing firms before the merger can be simplified to

  $$l^* = \underset{l_i}{argmax}\ l_i^\alpha - (A + b(N-1)l^* + bl_i)l_i.$$

  - Using the fact that $(N-1)l^* = L^* - l^*$, we get

  $$l^* = \underset{l_i}{argmax}\ l_i^\alpha - (A + bL^* + bl_i - bl^*)l_i.$$

  - The problem of competing firms after the merger can be simplified to

  $$l^* = \underset{l_i}{argmax}\ l_i^\alpha - (A + b(N-3)l^* + 2bl_j^{**} + bl_i)l_i.$$

  - Using the fact that $(N-3)l^{**} + 2l_j^{**} = L^{**} - l^{**}$,

  $$l^{**} = \underset{l_i}{argmax}\ l_i^\alpha - (A + bL^{**} + bl_i - bl^{**})l_i.$$

  - Suppose $L^{**} = L^*$. Then from the simplified problems of the competing firms before and after the merger, we get that $l^{**} = l^*$. However, this implies that $l_j^{**} = l^*$, which contradicts Lemma A2:

  $$L^{**} = L^* \ ; \quad l^{**} = l^* \implies l_j^{**} = l^*$$

- *Proof of item (ii) of Proposition 1:* This comes directly from item (i) and different levels of w_L and w_S.