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

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Abstract

I use a large merger to study the effects of a concentration on labor market outcomes. I find that concentration lowers wages, but the effects are different than prior estimates in the literature. I observe a larger reduction in wages of salespeople (low skill) than pharmacists (high skill). Two hypotheses can explain the results: (i) salespeople have strong preferences for jobs or accumulate some industry-specific human capital, and (ii) pharmacists are better organized into unions. Previous studies do not account for changes in labor force composition that are relevant after a merger, which could explain the difference in estimates.

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;

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[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."

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 concentration on labor market outcomes by analyzing a large merger in Brazil's retail pharmacy sector. Mergers can lead to greater concentration but can also generate synergies. To separately identify the effect of concentration, I chose to study a merger that 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 novel DiD estimator that includes establishment and worker fixed effects. 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 over time. Furthermore, it is possible to assess how the merger affected the wages of workers in the firms that merged, including both incumbents and new entrants. The data covers all firms in Brazil, such that I can also assess the responses of competing firms and use all the other firms to identify and estimate worker fixed 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.

I find that labor market concentration lowers wages, but the effects are different 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.

These wage results are not negligible when considering merging firm's market share. 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.

The results are counterintuitive, in that ex ante we expect pharmacists to have fewer outside options relative to salespeople, and thus face larger

[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.

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 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%. 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

[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.

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 DiD 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 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. This is true when following establishments over time (as in this paper and in Prager and Schmitt, 2021) but also when following a cohort of workers over time (as in Arnold, 2021).

As a second contribution, 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

[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).

is more likely to hold.[6]

I. Merger Predictions in Oligopsony Models

The purpose of this section is to explain why, as a matter of theory, the impact of a merger on labor market outcomes is ambiguous. Mergers may change the structure of product and labor markets, and changes in both markets can spillover to workers.[7] In this paper, I refer to market power as the combined effect of product and labor market power. Below, I explain how the prediction on wages and employment, at merging firms and competing firms may differ between (i) monopsony, (ii) search friction and bargaining, and (iii) collective bargaining models.[8] Which model better represents the reality is an empirical question.

In **modern monopsony models** (Card et al., 2018;Berger et al., 2019), a merger between two firms will decrease wages and employment at the **merging firms**. This can occur both due to labor market power and product market power. In the labor market, merging firms face upward slopping labor supply curves and internalize that their hiring decisions will affect both firms' labor supply. Therefore, each firm hires less workers such that both firms can pay lower wages. In the product market, merging firms face downward slopping product demand and internalize that by decreasing their production both firms can receive higher prices. The decrease in production means that firms will have to decrease their labor inputs, which leads to a decrease in wages.[9]

In this model, a merger will reduce wages at **competing firms** but the effect on employment is ambiguous. There are two mechanisms at work: income and competition. The income mechanism states that labor is now cheaper, so competing firms hire more workers. The competition mechanism depends on whether firms are strategic substitutes (Cournot)

[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.

[7]Mergers may also have synergies that affect productivity, wages, and employment. The theoretical framework and the empirical analysis in this paper exclude such effects because synergies might be specific to each merger.

[8]See Card (2022) for a discussion of the evolution of these models.

[9]Note that this is a partial equilibrium effect since it does not take competing firms' response into account.

or strategic complements (Bertrand). Firms that are strategic substitutes will hire more workers when their competitors hire less workers. Firms that are strategic complements will lower their wage offer and hire less workers when competing firms lower their wage offer. In equilibrium, employment could either increase or decrease at competing firms.

In **search models**, a merger is predicted to reduce wages and keep employment unchanged. In Jarosch et al. (2019), a merger reduces the number of firms but not the number of job vacancies. As a consequence, workers are matched to the same number of jobs but to a lower number of firms. By having fewer outside options, workers have a weaker bargaining position. In this scenario, a merger reduces wages paid by merging and competing firms, but unlike the monopsony model, employment remains unchanged.

When considering product market concentration within a search model, a merger could potentially increase wages. For example, suppose that product demand is perfectly inelastic. In that case, merging firms can increase prices without decreasing production. Employment does not change, and the surplus created by the job match increases. Even though workers have lower bargaining power, total surplus may increase to the point that wages also increase.

In **collective bargaining models**, a merger might not have any effect on the labor market. This could happen when workers at both merging firms are represented by the same union before the merger and wages were set jointly—like it is for some unions in Brazil. In this case, a merger would not change the bargaining relation between unions and employers and would not change wages nor employment. In other countries and institutional settings, the model can have different predictions.

The results of this paper suggest that the labor market of salespeople is consistent with modern monopsony models and the labor market of pharmacists is consistent with collective bargaining models. Such heterogeneity highlights the benefits of studying a particular industry.

II. Data and context

A. *Employer-employee matched dataset*

I use the 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.[10]

The dataset includes information on firms (legal form of the company); establishments (industry sector and county); workers (age, gender, race, and education); and job characteristics (occupation, date of hiring, date of separation, hours worked, 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.

The empirical strategy used in this paper requires two different data samples. The first sample—the drugstores sample —is restricted to firms in the retail pharmacy sector that had at least one pharmacist working between 2007 and 2018. 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. In the second sample—the fully connected sample—I keep the establishments from all sectors in the economy that are connected to establishments in the drugstore sample through worker mobility. The sample is restricted to workers who switched jobs at least once between 2007 and 2018.[11] See Appendix A for a more detailed description of the data restrictions.

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

[10]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.

[11]Due to this restriction, the drugstore sample is not a subset of the fully connected sample.

competing firms in the same counties: 9,798 firms that employed 497,658 different individuals during the period. The fully connected set consists of 189 million observations from 29 million individuals working in 4.2 million establishments.

B. Institutional setting

In this section, I present some key similarities and differences between the Brazilian and international 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 B, 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. For example, most states have a pharmacists' union and a union representing other workers at drugstores. 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.[12] Another singularity of the Brazilian case is the existence of employer unions that represent groups of employers in a region. The unions of employers and employees bargain for wage floors and wage increases, among other conditions of employment, in regions in which they overlap. Hence, unions might have to sign multiple collective

[12] 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).

agreements within a year.

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 that regulates CADE is ambiguous regarding whether the agency can stop a merger based on labor markets concerns.[13]

C. The merger between two major retail pharmacy chains

I study a merger between two large retail pharmacy chains in Brazil.[14] 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.

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 1 presents a map of the nine southern states from Brazil to show the presence of the merging 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.

The merger changed labor market concentration in some regions. Panel

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

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

A of Figure 2 shows that the merger increases the pharmacists' labor market Herfindal-Hirschman index (HHI) in many counties.[15] 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 fewer outside options than 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 2 uses ad-hoc labor market definitions. In section VI., 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.

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—which I discuss now in detail.

[15] 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.

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

III. Identification Strategy and Empirical Specification

Section I. argues 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 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

$$y_{jt} = \delta_0 + \delta_1 post_t + \delta_2 treat_{c(j)} + \delta_3 post_t \times treat_{c(j)} + \varepsilon_{jt}, \quad (1)$$

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.

$$y_{jt} = \beta_1 treat_{c(j)} + \sum_{\substack{k=-4 \\ k\neq 0}}^{7} \delta_k \mathbb{1}[t = k] \times treat_{c(j)} + \lambda_t + \varepsilon_{jt}. \quad (2)$$

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. Furthermore, 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 develop the identification strategy at the individual level, which accounts for changes in labor force composition in observable and unobservable characteristics, next.

A. 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]

$$
\begin{aligned}
\ln(wage)_{it} = \theta_i + \psi_{J(i,t)} &+ \sum_{k \neq o,o'} \delta_0^k \mathbb{1}[Group_k] \\
&+ \sum_{k} \delta_1^k \mathbb{1}[Group_k] \times Post_t \\
&+ \sum_{k} \delta_2^k \mathbb{1}[Group_k] \times Treat_c \\
&+ \sum_{k} \delta_3^k \mathbb{1}[Group_k] \times Treat_c \times Post_t \\
&+ X_{it}'\beta + \lambda_t + \varepsilon_{it},
\end{aligned} \tag{3}
$$

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;

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

time-varying individual characteristics, X_{it}; 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 1. 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 effect of the merger on wages for each group k, excluding composition effects.

The purpose of including establishment fixed effects in Equation 3 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—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 mo-

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

bility.

Equation 3 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.

B. 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 underlying assumption 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 had been treated. In the results section, I provide suggesting evidence that this assumption holds by showing that the pre-trends are parallel. Yet, counties may have different trends associated with characteristics that are not balanced between treated and control groups (see Table 2), 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—is addressed with Equation 3. Identification relies on three assumptions: The log-linearity of wages, additivity, and exogenous mobility. 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. Exogenous mobility implies that workers do not select into firms based on the idiosyncratic error term, ε_{it}. In Appendix Figure A.1, 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

[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.

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 with my setting is that I use variation within a merger and Arnold compares the effects of different mergers. 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.

IV. Effect of the Merger in Merging Firms

A. Effect of the merger on overall wages

Figure 3 presents trends in average log wages for all workers in merging firms and 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 (see Figure 4). This gives supportive evidence on the parallel trends assumption and the assertion that establishments in treated and control groups are comparable. Third, wages in the treated and control groups seem to converge after the treatment, covering a large gap. Figure 4 shows a statistically significant effect at the 5% significance level.

Panel A of Figure 5 presents the difference in trends in the average log wages of pharmacists (see Appendix Figure A.2 for trends). The relative wages of pharmacists start with a slow decline after the merger and reach a strong decrease of 12% after 6 years, with an average effect of -7.3% (Appendix Table A.1). Columns 2 to 4 of Table A.1 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. Note that these results do not reflect the causal effect of market power on wages since they do not account for changes 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 A.2, I show that the results for pharmacists do not change if we use average wages instead of wages from December.[20] The 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 A.3 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 Figure 5 shows that the wages of salespeople do not exhibit the same decline as for pharmacists (see Appendix Figure A.2 for trends). 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, the results do not imply that concentration did not reduce the wages of salespeople since they do not account for changes labor force composition.

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.

[20]Note that these regressions follow Equation 1 but are run at the individual level and use the drugstores sample (and not the fully connected sample).

B. 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 3, 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 different than zero at the 5% significance level with standard errors clustered at the county level. The estimate is still not statistically different than zero if I cluster the standard errors at the county-year level or at the establishment level.

I use leads and lags graph to show evidence that treated and control groups are also comparable when I include worker and establishment fixed effects. Figure 6 presents leads and lags estimates following the specification from column 4. The omitted category is the difference between treated and control groups in 2011. The figure shows that trends in residualized log wages are parallel before the treatment, which is evidence in favor of the parallel trends assumption. After the merger, there is a slow decline in market wages, but this effect is not statistically significant. Figure A.3 present the residualized trends for both treatment and control groups and shows the same facts. In summary, market wages of pharmacists do not fall once we take changes in composition into account. Notably, this 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 column 4, which includes 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, column 4 shows 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.[21] The results do not support the assumption that salespeople are less affected by the merger.

One might be concerned that other events might be driving these results. For example, Table 2 shows that counties in the treated group are more populous than countries in the control group. The concern is that, for being different, these counties have different trends in the wages of pharmacists and salespeople. I address this concern by including interactions between year indicators and pre-merger county characteristics in column 5 of Table 3. County characteristics are indicators for each quartile of the following variables: 2010 population, 2003 HDI, 2010 informality levels, and 2006 county government revenue per capita. The comparison of estimates in columns 4 and 5 of Table 3 shows that the inclusion of these variables does not significantly change the magnitude of estimates. This provides additional evidence that results are robust and that control counties are comparable to treated ones.

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 VI., 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]Standard errors are estimated using the Frisch–Waugh–Lovell method.

C. *Effect of the merger on other labor market outcomes*

Table 4 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 4 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 4 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.

V. 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 3 presents estimates of selected parameters from Equation 3 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 5 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 3 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 5 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 5 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 in a monopsony model. 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.

VI. 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 IV., 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 would be more affected by the merger because they 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. Pharmacists are both more specialized and more organized than salespeople. Survey data show that 34% of pharmacists are unionized, while only 9% of salespeople working at pharmacies are (PNAD, 2011). One of the unions' functions is to negotiate wage floors and annual wage increases with employers. The results from this paper indicate that a merger between two firms might not weaken the bargaining power of employees' unions. This is consistent with the centralized nature of negotiations and with a model of collective bargaining.

I present additional evidence that supports the efficacy of pharmacists unions by separately analyzing the effect of the merger on incumbent and newly hired workers. Figure 7 presents the leads and lags results for pharmacists in these two 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. Yet, panel B shows that the wages of pharmacists hired in each year—i.e, entry-level wages for pharmacists—decrease by up to 20%.[22]

A possible explanation for these results is that incumbent workers are shielded by union agreements and new workers are not. First, firms have little room to differentially change incumbent workers' wages across counties.

[22]These regressions do not include individual characteristics and worker fixed effects, not accounting for changes in composition.

This explains the null results of panel A of Figure 7. Second, wage floors are the only instrument that unions have to protect newly hired workers. It turns out that wage floors are not binding for pharmacy chains, which usually pay higher wages than independent drugstores and than the wage floor. The results suggest that the merger changed firm's hiring strategy: merging firms decide to hire pharmacists that accept lower wages, which tend to be of lower worker fixed-effect.

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 2 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 Appendix Table A.4, 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. Appendix Table A.5 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.

I show that pharmacists and salespeople may have preferences over the type of drugstore they wish to work at. In Appendix Figure A.4, 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 Appendix Figure A.4 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 Appendix Figure A.4 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).

A. 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. Appendix Table A.6 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 Appendix Table A.6 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.

VII. Conclusion

This paper studies the effects of market power on wages by analysing a merger between two large retail pharmacies in Brazil. I find that the wages of pharmacists do not decrease after the merger and that the wages of salespeople do. The results for pharmacists are consistent with collective bargaining models. The results for salespeople are consistent with modern versions of the monopsony model.

The literature on the labor market effects of mergers is still nascent compared with the industrial organization (IO) literature on the product market effects of mergers. Two features of 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, by including individual controls such as age and worker fixed effects, this paper shows the importance of standardizing a unit of labor when following establishments 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.

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, possibly preventing wages from falling even more. That said, how firms compete in other labor markets is an interesting avenue for future research.

Finally, this paper studied the effects of a merger through the mechanism of market power, excluding synergies. Regulatory agencies are 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

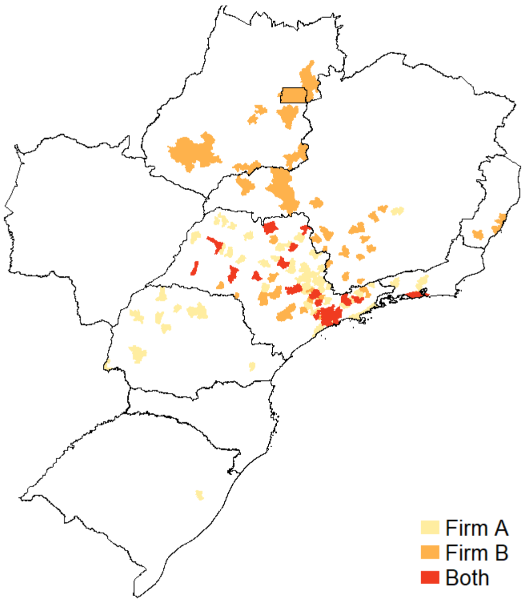
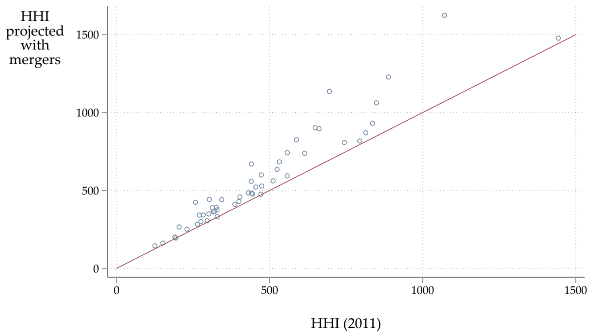
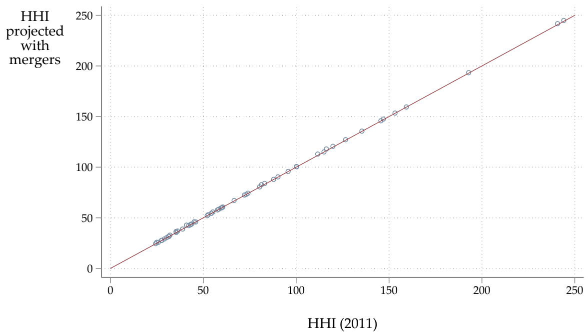


Figure 1: Presence of merging firms before the merger (2010)

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 2: 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.

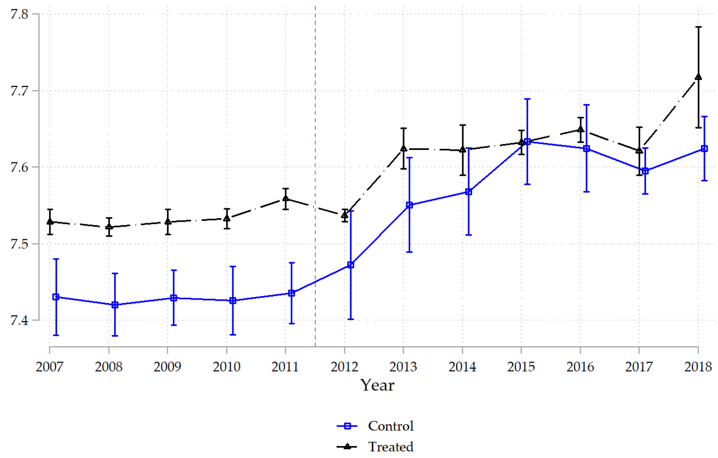


Figure 3: Trends - Ln(wage) of all workers in merging firms

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.

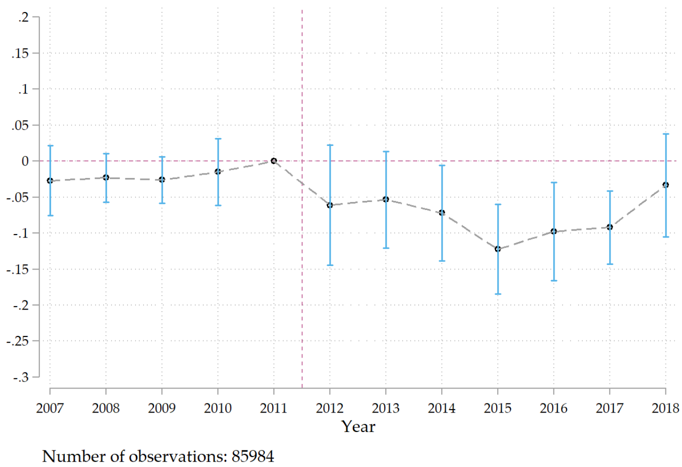
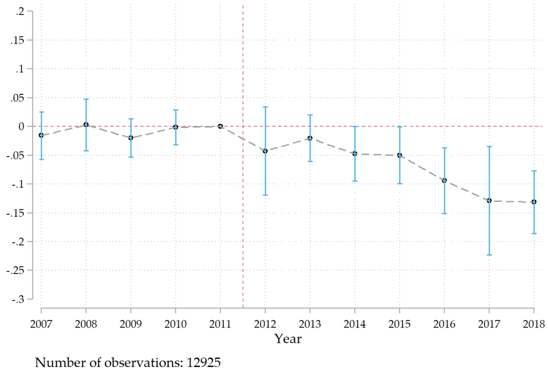
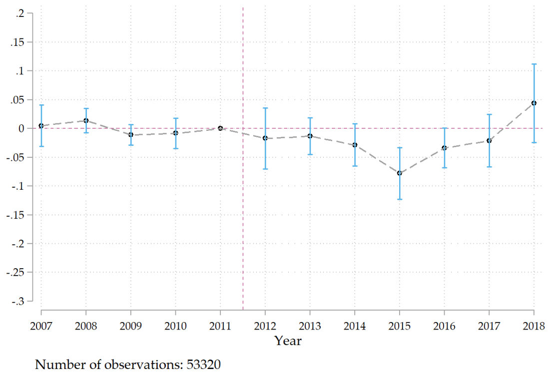


Figure 4: Leads and lags - Ln(wage) of all workers in merging firms

Note: The figure presents estimates of the parameters in Equation 2. 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 5: Leads and lags - Ln(wage) of workers in merging firms

Note: The figure presents estimates of the parameters in Equation 2. 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.

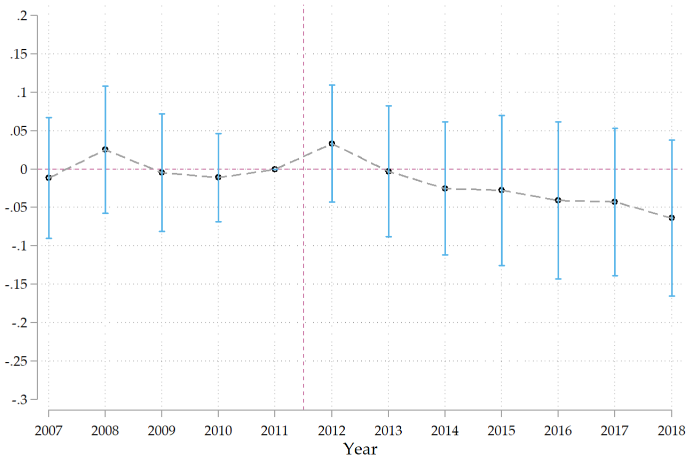
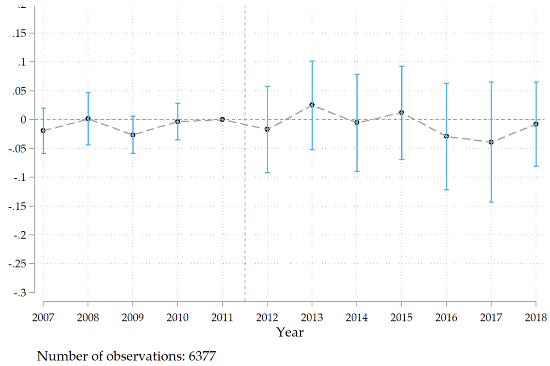
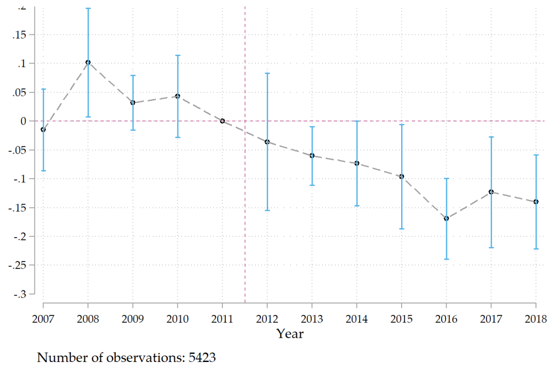


Figure 6: Leads and lags - Ln(wage) of pharmacists in merging firms with two-way fixed effects

Note: The figure presents leads and lags estimates. The omitted category is the difference between treated and control groups in 2011. Estimates come from a regression that uses the fully connected set and includes individual characteristics and worker and establishment fixed effects, similar to Equation 3. Log wages are measured in December. The sample only includes workers employed on December 31. Standard errors are clustered at the county level.



((a)) Incumbent workers



((b)) Entry-level wage

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

Note: The figure presents estimates of the parameters in Equation 2. 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.

Tables

Table 1: Sample size

Number of...	Drugstores Sample: Merging firms	Drugstores Sample: Competitors	Fully Connected 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)
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

Note: Selected coefficients from the difference-in-differences estimate as in Equation 3, where the unit of observation is at the individual level. Each column represents a regression with 189,072,473 observations from 29,260,227 workers and 4,273,843 establishments. 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. 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 Equation 1. 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 5: 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 Equation 1. 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.

[For Online Publication]

A Data Appendix

In this section I describe data restrictions to the drugstore and the fully connected samples.

- Workers with multiple jobs during a year will appear several times in the data. I restrict the sample to individuals with a formal employment link on December 31.
- 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:
  1. establishments with more than 100 full-time employees in any year
  2. establishments with more than 50 full-time employees and no pharmacist in any year, and
  3. establishments with more than 10 full-time employees and more than 60% of employees in warehouse occupations in any year.
- In the fully connected sample, 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.
- 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.

In Appendix Table A.7, I present additional statistics on the drugstore sample and on less restrictive samples.

B 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

[23]CMED Resolution n°2, March 5, 2004.
[24]Law number 5,991 from 1973.

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 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 A.8 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 A.8 shows that the number of individuals working in pharmacies has almost doubled. This is true for both pharmacists and salespeople. Figure A.5 shows that the number of pharmacists per 10,000 residents increased from 3.3 to 6.6 between 2007 and 2018. While

[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).

this increase did not significantly change the share of female pharmacists (69% to 73%, Figure A.6), it did increase racial diversity in the occupation. Figure A.7 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.8). Last, the share of workers with a high school degree increased from 78% to 91% (Table A.8).

Despite the increase in employment, the wages of pharmacists also increased in the period. Appendix Table A.8 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.9 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).

C 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}
$$

Appendix Figures

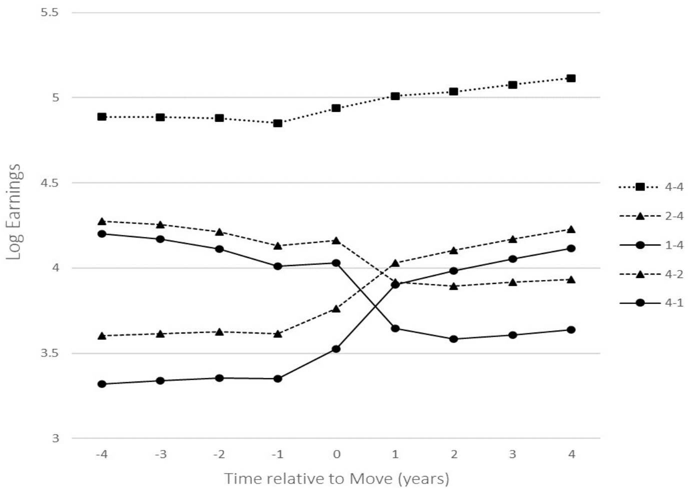
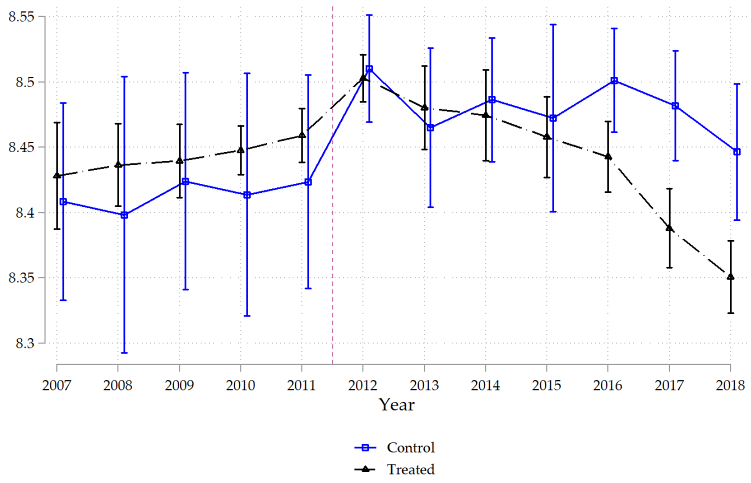
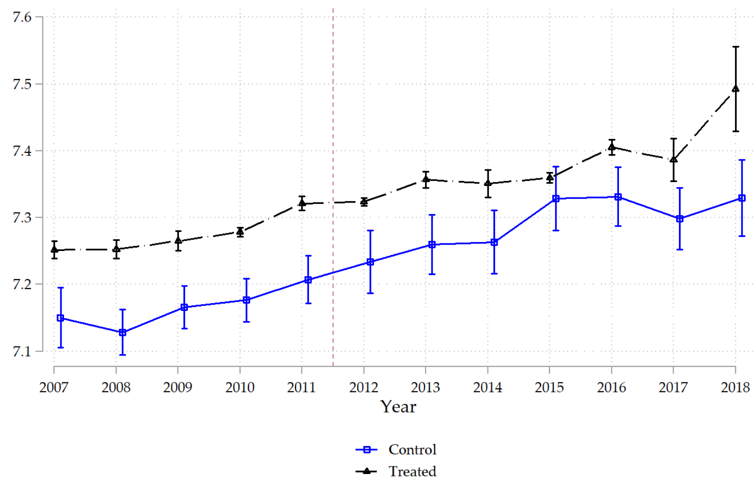


Figure A.1: Event Study of Changes in Earnings when Workers Move Between Firms

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.2: 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.

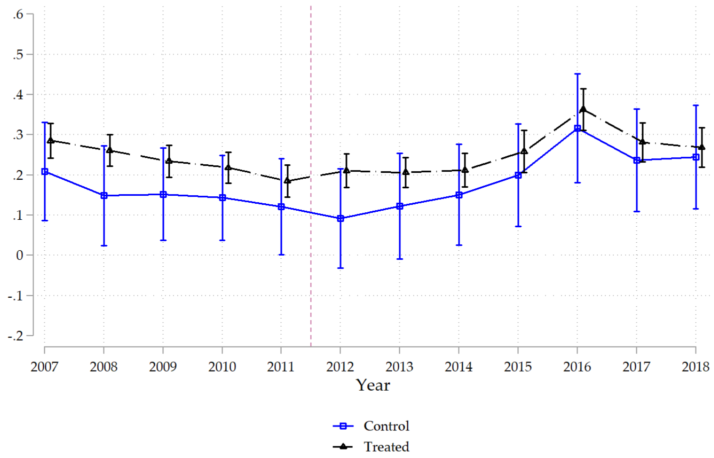
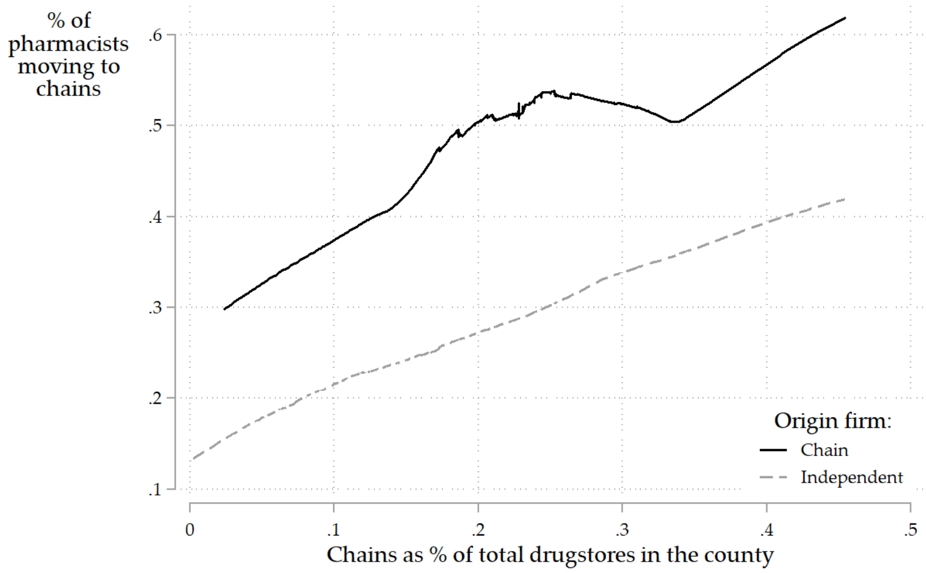
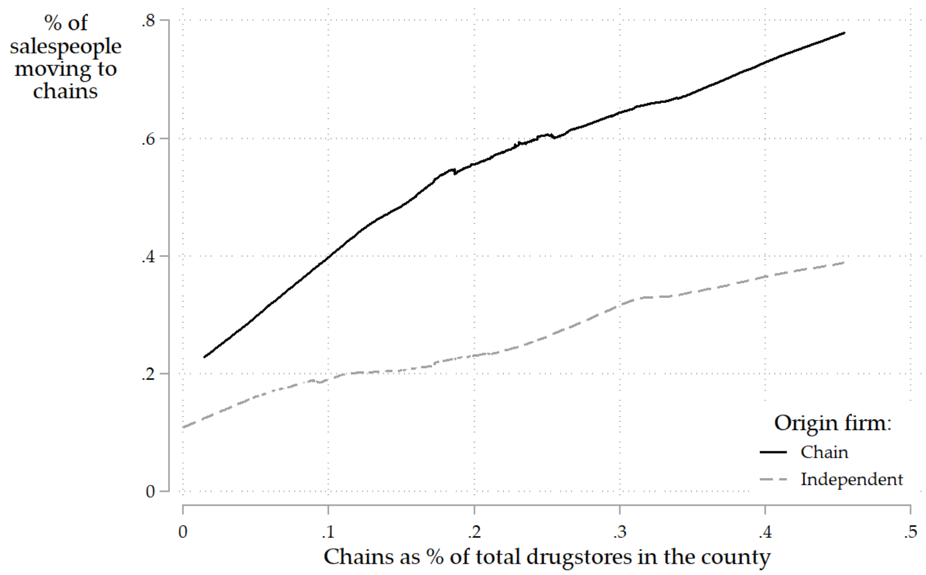


Figure A.3: Residual ln(wage) of pharmacists in merging firms with two-way fixed effects

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 3, 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.



((a)) Pharmacists



((b)) Salespeople

Figure A.4: 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.

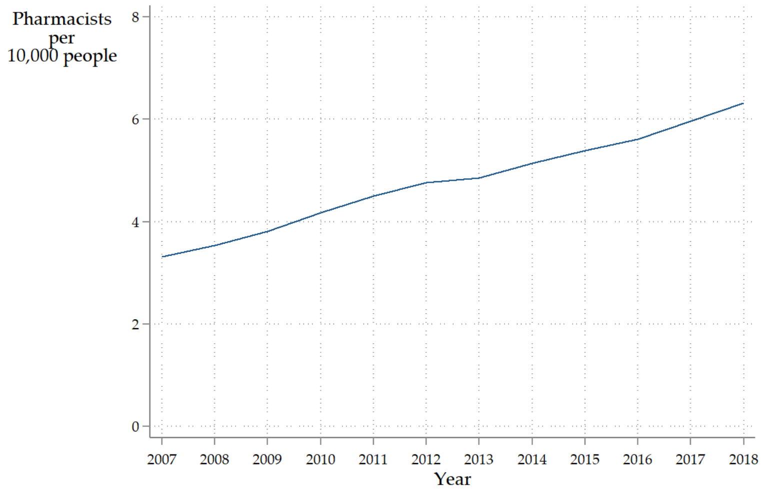


Figure A.5: Pharmacists per capita - Brazil

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.

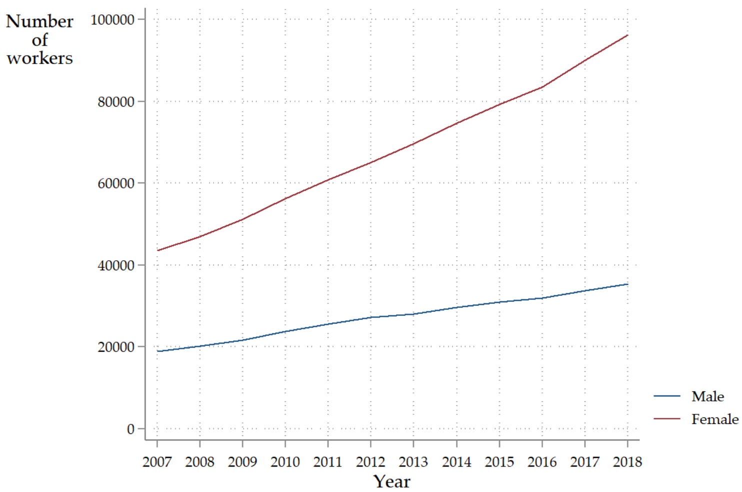


Figure A.6: Trends in pharmacists' gender composition - Brazil

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.

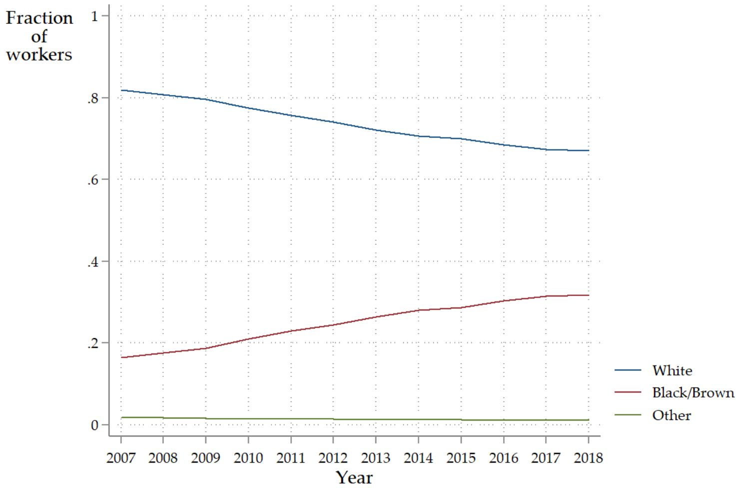


Figure A.7: Trends in pharmacists' race composition - Brazil

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.

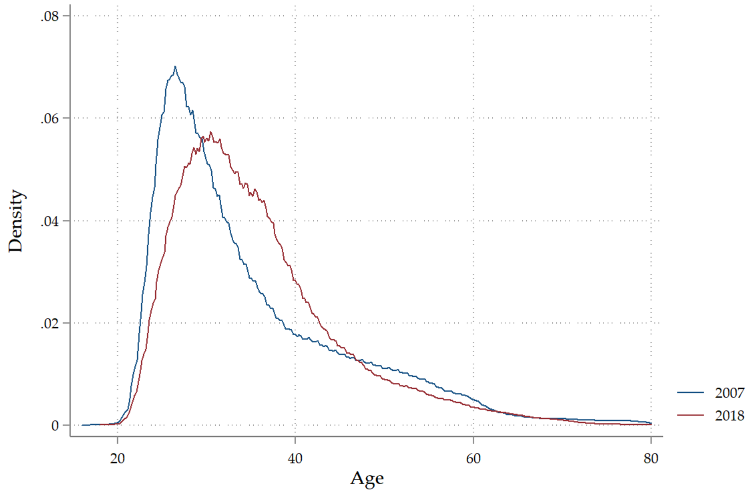


Figure A.8: Age distribution of pharmacists - Brazil

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.

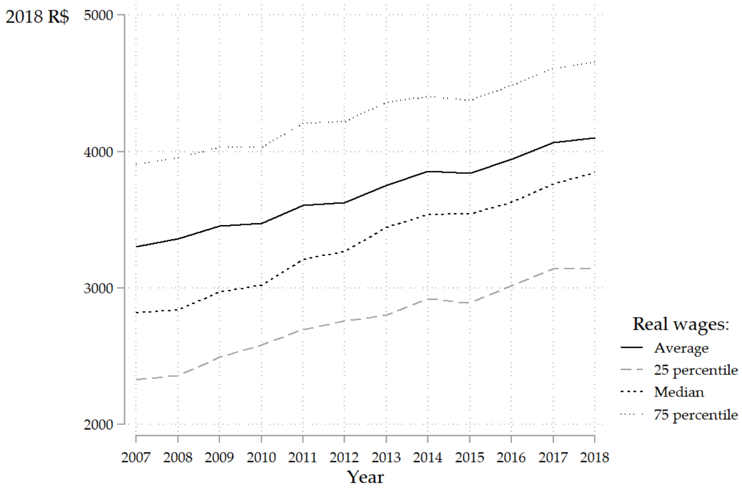


Figure A.9: Trends in pharmacists' wage distribution - Brazil

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.

Appendix Tables

Table A.1: 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 A.2: 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 1 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 A.3: 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 1 at the individual level. Column 1 is similar to column 8 of Table A.2. 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 A.4: 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 A.5: 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 A.6: 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.

Table A.7: Sample size

Number of...	All		Relevant Counties		Balanced		
	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 A.8: 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.