The Role of Composition in Assessing Labor Market Power: Evidence from a Retail Pharmacy Merger

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.

Abstract

I use a merger event between two large retail pharmacy chains to detect labor market power within firms. Initially, I establish that variations in labor force composition within establishments are common across the economy over time. This motivates an empirical strategy that controls for changes in composition by considering both observable and unobservable worker characteristics. The results indicate that increased concentration reduces wages for salespeople (low-skill) but has no significant impact on pharmacists (high-skill). Notably, these findings change considerably if changes in composition are not accounted for, underscoring the importance of such considerations in studies tracking establishments over time and serving as a cautionary note for other researchers in the field.

I. Introduction

There is a renewed policy concern that firms may be building market power in the labor market, potentially reducing wages, and increasing income inequality. For example, in July 2021, a U.S. presidential executive order stated that:

"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."

In this context, it is relevant to ask whether firms actually have labor market power, and, if so, identify the specific labor markets where this power is exerted. In this paper, I infer the presence of labor market power by studying a large merger in Brazil's retail pharmacy sector, with a particular focus on low and high-skill labor markets, represented by salespeople and pharmacists. Notably, a growing body of literature uses merger and acquisition events to identify whether a hypothetical increase in labor market power translates into lower wages per worker. This approach faces the well-known challenge that M&A activities can both increase market power and lead to productivity enhancements. This paper highlights an additional difficulty that is pertinent to any study on establishment-level wage setting: over time, most establishments undergo changes in their workforce composition, including variations in worker characteristics, which can result in significant wage disparities and need not be associated with market power.

To assess the impact of market power, I analyze a merger where productivity gains likely occur at the national level, while changes in market concentration vary across municipalities. I employ a difference-in-differences (DiD) approach to estimate the effect of market power, comparing labor market outcomes in establishments from regions where concentration increased with establishments from regions where concentration remained stable. To address potential composition effects, I introduce a DiD estimator that incorporates establishment and worker fixed effects as in the AKM approach.[1] These controls help isolate wage changes unrelated to shifts in workforce composition based on observable and unobservable characteristics, thus allowing us to properly attribute the changes in wages to market power. Throughout the paper, I distinguish between market wages—the wage rate set at the establishment level, which should be the same for identical workers—and average wages, which simply reflect the mean wage across workers with different characteristics and are prone to changes in composition.

I follow establishments over time using RAIS, the Brazilian matched employer-employee database. The data are structured as a panel between 2007 and 2018, and covers the universe of formal employees in Brazilian companies. There are a few advantages of using

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[1]The AKM approach involves estimating a log wage equation with both worker and firm or establishment fixed effects. This method was initially introduced by Abowd, Kramarz, and Margolis (1999).

this data. First, the effects of the merger can be analyzed by occupation, as the data includes detailed occupational information—something rarely available in similar datasets from other countries. Second, the data includes hiring and separation dates, allowing me to study the effects on both incumbents and new entrants. Third, it covers all firms in Brazil, enabling an assessment of competing firms' responses. Additionally, I use data from all other firms in the labor market to estimate establishment and worker fixed effects. Lastly, since informality in the retail pharmacy sector is significantly lower than in other industries—partly due to its high level of regulation—the results capture the relevant population effects more accurately.

I first show that there are significant changes in the composition of establishments' labor force over time. For example, within two consecutive years, 20% of all establishments in Brazil altered the average age of their employees by more than two years. In addition, 55% experienced composition changes over a 5-year period leading to a shift in average wages exceeding 10%. These composition changes are just as frequent in the retail pharmacy sector. Changes in composition or restructuring are particularly relevant following mergers and acquisitions (Brown and Medoff, 1988; McGuckin and Nguyen, 2001; Li, 2012; DePasquale, 2018; Todd and Heining, 2022; He and Maire, 2020; Gehrke et al., 2021; Lagaras, 2023). For instance, Gehrke et al. (2021) show that acquiring firms tend to hire younger workers post-merger. The results in this paper also suggest that establishments modify the composition of their workforce concerning unobservable worker characteristics, such as skill levels. In sum, these composition changes represent a primary challenge when attempting to identify the effects of labor market power.

The main results suggest that labor market power varies by skill level. First, the findings indicate the presence of firm market power in the labor market of low-skill workers (salespeople). The market wages of salespeople at *merging firms* decrease by 3.7% in the treated group compared to the control group after the merger.[2] Additionally, I examined data on job mobility and found that 24% of salespeople employed in drugstores switch to another drugstore when they change jobs, which is much higher than the expected percentage of 5% if they were moving randomly across jobs with the same occupation title. This finding suggests that salespeople working at drugstores may have some degree of firm- or industry-specific human capital or stronger preferences for working at a drugstore.[3] The results could also be consistent with labor supply models based on

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[2]Note, however, that real wages are actually increasing in the period for both groups. The employment effects are not precisely estimated and we cannot reject large decreases (or increases) in employment

[3]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.

idiosyncratic preferences.[4] In summary, labor markets for low-skill workers may be more concentrated and finer than they initially appear.

Secondly, the merger did not affect the market wages of high-skill workers (pharmacists). The DiD effect on pharmacists' market wages at *merging firms* is statistically insignificant, with a point estimate of -2.7%. This result is surprising, as high-skilled workers are generally thought to be more affected by labor market power than low-skilled workers (Prager and Schmitt, 2021). There are two potential explanations for this unexpected finding. First, firms may already impose high markdowns on pharmacists' wages, so the increase in market power from the merger does not significantly reduce wages. A second, more plausible explanation is that wage-setting in the pharmacist labor market is constrained by collective bargaining with unions.[5] Wage negotiations occur at the regional level, meaning that wages for pharmacists rose at similar rates in both treated and control groups.

The main contribution of this paper is to show that accounting for changes in composition has a significant impact on the estimated effects of mergers on wages. Without accounting for composition, I find that the average wages of pharmacists fell by 7.9% and those of salespeople by 0.7%.[6] However, when firm and worker fixed effects are included, the effect on the market wages of pharmacists is smaller and not statistically significant (point estimate of -2.7%). The overall decline in average wages was partly due to new entrants. Following the merger, treated establishments hired pharmacists who were willing to accept lower wages and who had accepted lower wages in previous or future jobs. These are "lower fixed-effect" workers, who may have graduated from lower-quality universities or possess fewer specializations and skills. This change in hiring practices may have been part of the company's broader restructuring strategy aimed at reducing payroll expenses, and it does not necessarily imply a gain in labor market power. The difference in results between market wages and average wages emphasizes that, without accounting for composition, the impact of a firm's labor force restructuring on payroll may be mistakenly attributed to market power.[7]

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[4]For instance, studies by Card et al. (2018), Berger, Herkenhoff, and Mongey (2022), and Lamadon, Mogstad, and Setzler (2022) present labor supply models that illustrate how workers' idiosyncratic preferences for non-wage job characteristics result in firms being viewed as imperfect substitutes. These features may produce an upward-sloping firm-specific labor supply curve that ultimately leads to firm market power in the labor market for low-skilled workers.

[5]Notably, pharmacists have unionization rates nearly four times higher than salespeople. In the 2013 Brazilian household survey, 34% of pharmacists working in drugstores reported being unionized, compared to only 9% of salespeople (PNAD, 2013).

[6]These estimates are very similar to those of Prager and Schmitt (2021) in the US, despite the differences in setting and identification strategy. The authors 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.

[7]Note that while the regressions in Arnold (2021) include worker fixed-effects, the paper's main specification is on the wages of stayers, in which the inclusion of worker-fixed effects should not have any practical purpose. I argue that following stayers is the second-best approach. Over long periods of time, the workers that stay in the same establishment may be a very selected group.

The market 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 and the need for a standard definition across the literature. Unfortunately, the effects of the merger on employment are not precisely estimated, and I cannot rule out a significant decline in employment at merging-firms for both occupations.

I present additional findings regarding the impact of the merger on wages at *competing firms*. Following the merger, the wages of salespeople at *competing firms* decreased by 3.3%, despite them hiring an additional salesperson. This finding is consistent with firms competing by choosing quantities, as in Cournot competition. In this scenario, the merger increases merging and competing firm's labor market power, which can now pay lower wages. As a result of these lower wages, salespeople become a less costly option, leading competing firms to hire more of them.

Within the growing literature on labor market power—where some influential studies associate concentration with lower wages (Azar et al., 2020; Benmelech et al., 2022; Bassanini et al., 2023, Qiu and Sojourner, 2023, and many others), and some other studies use general equilibrium models to derive a more precise understanding of market power and predict its consequences (Berger et al., 2022; Amodio et al., 2023; Rubens, 2023)—this paper is closely related to a third group of studies that use mergers or concentration events to identify the causal effect of labor market power on wages (Prager and Schmitt, 2021; Arnold, 2021; Lagaras, 2023; Thoresson, 2023). This paper and the latter group of papers have two main distinctions.

First, the productivity assumption in this paper is less restrictive than in the other studies. Mergers and acquisitions can lead to increased productivity, which, in turn, can result in higher wages. To separately identify this effect from labor market power, we need to assume that productivity gains are, on average, the same for merging firms in treated and control groups. I argue that, in the current analysis, productivity gains occur at the national level, such that a difference-in-difference estimate will capture the gain in labor market power. Expected gains in the retail pharmacy merger include better bargaining with suppliers, improvements in logistics and optimization of distribution centers, and the consolidation of headquarters and administrative departments; all of these factors should affect establishments in both treated and control groups in the same way.[8] In

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[8]In the context of this paper, one might also be concerned that establishments in treated municipalities, where firms overlapped, may achieve synergies by reducing redundant workers. I argue that this type of strategy would only reduce market wages if firms have market power. Labor-saving synergies **that reduce wages** should be viewed as evidence that firms have wage-setting power in the labor market and do not invalidate the identifying assumptions in this paper.

contrast, Arnold (2021) and Prager and Schmitt (2021) compare mergers that increased concentration with mergers that did not increase concentration, potentially from different industries. The implicit assumption is that mergers in different industries, that induce different changes in concentration, have similar productivity gains.[9]

Secondly, we target a different measure—or statistical moment. Arnold (2021) and Lagaras (2023) focus on changes in the wages of two populations: (i) workers employed at the merging firms before the merger and (ii) workers who remain with the firm (stayers). The first measure captures a mix of effects, including those arising from the displacement of workers who left the merging firms, which is undesired in a study of labor market power. The second measure—tracking stayers—can reveal labor market power effects, provided that the panel of stayers remains balanced over time. This is rarely the case, with worker mobility typically leading to a small, selected sample or a short time window.

In contrast, Prager and Schmitt (2021) and the present study adopt a different approach by targeting changes in wages at the establishment level.[10] This perspective is natural, as market wages are theoretically determined at the establishment level (Card et al., 2018; Berger et al., 2022). It also captures the entire affected population, not only stayers. Yet, this approach has an important limitation: worker turnover can change the composition of the labor force within an establishment. Just as we cannot directly compare the price of a 300ml can of soda with that of a 1-liter bottle, we cannot compare wages across very different workers, even within the same firm and occupation. This study contributes to the literature by emphasizing the importance of accounting for such compositional changes when tracking establishments over time.

The remaining sections of the paper are structured as follows. In Section II, I provide an overview of the Brazilian linked employer-employee data, detailing how establishments in the data undergo composition changes over time. Additionally, I discuss the institutional setting related to the retail pharmacy sector and describe the merger event. Section III outlines the empirical strategy and the underlying identifying assumptions. Sections IV and V present the effects of the merger on the merging firms and competing firms, respectively. In Section VI, I discuss the magnitudes of these effects and explore their implications for the conceptualization of labor market definitions. Finally, Section VII provides the conclusion.

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[9]In Arnold (2021), mergers with a high change in labor market concentration are assumed to have the same productivity gains as mergers with low changes in labor market concentration, irrespective of the industry. Prager and Schmitt (2021) assumes that productivity gains are the same across different hospital mergers.

[10]This measure is equivalent to average payroll per worker.

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.[11]

The dataset includes information on firms (legal form of the company); establishments (industry sector and municipality); 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.[12] 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 municipalities and contains 392 establishments. Between 2007 and 2018, 51,380 individuals worked in one of these establishments, resulting in 134,216 person-year observations. The second column of Table 1 presents the sample size for competing firms in the same municipalities: 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. *Changes in composition in Brazilian establishments*

Changes in worker composition at the establishment level are a common phenomenon. Firms frequently hire and terminate workers with different characteristics, leading to substantial shifts in an establishment's workforce composition over short periods. As a result, studies evaluating changes in average wages at the establishment or firm level

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[11]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.

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

may misattribute the effects of compositional changes to the impact of the event under study. This important pattern has received little attention in the labor literature, and data limitations are partially to blame. For example, studies on the effects of mergers on labor markets, such as Prager and Schmitt (2021) and Arnold (2021), lack detailed worker characteristics and therefore overlook changes in composition.

I describe changes in compositions in terms of observable characteristics for the universe of formal establishments in Brazil, for the period between 2010 and 2015. This covers approximately 400 thousand establishments from all industry sectors. In Appendix Section C, I specifically examine composition changes in the retail pharmacy sector. The results for pharmacies are similar to those for establishments in other sectors and show a high degree of compositional change.

Table 2 illustrates how establishment composition changes, in terms of worker's age, tenure, the proportion of male workers, the presence of college graduates, and the distribution of professionals and technicians within one-digit occupations. The first three columns of the table highlight substantial standard deviations in the one, three, and five-year differences in these variables.

In order to provide economic context to these figures, we calculate the percentage of establishments where a one-year predicted wage change exceeding 3% can be attributed solely to changes in workforce composition. This information is presented in columns 4 to 6 of Table 2. For instance, 21.2% of establishments had a predicted wage change surpassing 3% within a year due to alterations in the average worker age. Column 7 further reveals that this change in age exceeds 2 years in absolute terms, derived from a regression analysis linking log-wages to the respective variable in that row. In another example, the table shows that 40% of establishments experienced changes in the proportion of college-educated workers that would result in wage shifts greater than 3%. Given that college-educated workers typically receive higher wages, it's noteworthy that an establishment need only replace 3 non-college workers with college degree holders out of every 100 workers to induce a 3% change in the firm's payroll per worker. This emphasizes the significant influence that shifts in composition can exert on economic outcomes.

Figure 1 expands the analysis to examine the cumulative impact of all changes in composition within establishments over a five-year period on predicted wages per worker. The figure depicts the cumulative distribution of establishments based on the magnitude of their predicted change in payroll—predictions are derived from a log-wage regression that includes age and schooling interacted fixed effects, 3-digit occupation fixed effects, a cubic function of tenure, and a gender indicator. The findings reveal significant wage fluctuations attributable to changes in composition. For instance, more than 90% of establishments experienced changes in composition that would predict wage shifts exceeding 3%, and approximately 30% of establishments with more than 10 employees witnessed changes that would predict wage shifts exceeding 10%. The figure indicates substantial

magnitudes across establishments, irrespective of their size.

Given the frequent shifts in composition, one might naturally wonder why establishments choose to alter their labor force composition. There are numerous scenarios that might prompt this adjustment. One plausible scenario arises when minor price or productivity fluctuations prompt firms to consider slight modifications to their workforce. For instance, a drugstore seeking a modest expansion may opt to hire a 20-year-old pharmacist over a more experienced and consequently pricier counterpart. Alternatively, a firm housing both a senior and a junior pharmacist may lean towards replacing the junior pharmacist with a more experienced one when aiming to expand. Remarkably, when confronted with similar shocks, two firms can exhibit contrasting shifts in composition. These changes in labor force composition often serve as a means for firms to navigate the inherent divisibility of work.

C. 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 municipalities or within the state (Menezes-Filho et al., 2011). In contrast, U.S. workers tend to organize at the firm or plant level.[13] 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 agreements within a year.

The Brazilian analytical process for mergers and anti-competitive agreements is closely

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[13]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).

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.[14]

D. The merger between two major retail pharmacy chains

I study a merger between two large retail pharmacy chains in Brazil.[15] 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 municipalities 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 municipalities. Figure 2 presents a map of the nine southern states from Brazil to show the presence of the merging firms in each municipality before the merger. Municipalities in which both merging firms had an establishment in 2010 are drawn in red. Municipalities in which only one of the merging firms had an establishment in 2010 are drawn in yellow and orange. The white area represents municipalities in which none of the firms had an establishment in 2010.

The merger changed labor market concentration in some regions. Panel A of Figure 3 shows that the merger increases the pharmacists' labor market Herfindal-Hirschman index (HHI) in many municipalities.[16] Panel B shows that the merger did not change concentration in the labor market of salespeople, where the market is also defined at the

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[14]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

[15]For confidentiality reasons, I do not disclose the names of the firms.

[16]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 municipality 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.

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

municipality-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 3 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 3 presents some descriptive statistics of municipalities and workers from merging firms in the treated and control groups. On average, municipalities in the control group are smaller in population and have lower per capita government revenue than municipalities in the treated group. Municipalities 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 municipalities 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.

III.   Identification Strategy and Empirical Specification

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 municipalities. 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 municipalities in which both merging firms had at least one establishment in 2010, one year before the merger was announced. I denote as control municipalities 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

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

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 municipality c; and ε_{jt} is an error term. Additionally, I estimate a leads and lags equation, presented below. In the equation, $\mathbb{1}[t = k]$ indicates the year relative to the merger; λ_t are year fixed effects; and ε_{jt} is an error term.

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

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.

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\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; 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 municipalities. 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.

It is important to note that this approach requires the extension of 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 mobility.

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

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

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.

The greatest advantage of this method lies in its ability to estimate the effect of the merger for all workers, after controlling for changes in unobserved composition. Alternative strategies, such as estimating effects for stayers (which I also employ) or estimating the worker fixed effect prior to the merger, have additional limitations. Firstly, in contexts of high turnover, few workers remain in the same establishment for extended periods. The pool of workers potentially affected by the merger mainly comprises individuals entering the firm post-merger. Secondly, many individuals in this sample are young and lack a substantial period of labor market participation necessary for estimating worker fixed effects before the merger. The method presented above avoids disregarding this significant pool of individuals most likely to be influenced by the merger. Moreover, it provides an estimate of the worker fixed effect unaffected by the merger, as this effect is captured by the DiD estimators.

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, municipalities may have different trends associated with characteristics that are not balanced between treated and control groups (see Table 3), which violates the parallel trends assumption. To address this concern, I control for interactions between municipality 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 homogeneous across individual characteristics. Exogenous mobility means that workers do not select into firms based on the idiosyncratic error term, ε_{it}. The Brazilian data has been increasingly used for AKM estimation, with careful robustness checks presented by Alvarez et al. (2018) and Gerard et al. (2021) that answer to the concerns in Bonhomme, Lamadon and Manresa (2019), and Bonhomme (2020).

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 municipalities, in which firms overlapped, but it did not change concentration in control municipalities, 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 municipality level.[18] The key identifying assumption is that synergies are realized at the national level and are the same for treated and control municipalities. Examples of synergies at the national level are better bargaining with suppliers, improvements in logistics and optimization of distribution centers, and combining headquarters and administrative departments. Under this assumption, the difference between treated and control groups yields the market power effect, excluding the effects from synergies.

Other papers have used a stronger assumption to address the same challenge. To identify the market power effect, Arnold (2021) assumes that the effect of the mergers on productivity (synergies) is independent of the change in local labor market concentration. For example, mergers with high change in labor market concentration will have the same synergies as mergers with low change in labor market concentration. The difference 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 4 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 5). 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

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

gap. Figure 5 shows a statistically significant effect at the 5% significance level.

Panel A of Figure 6 presents the difference in trends in the average log wages of pharmacists (see Appendix Figure A.4 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.3). Columns 2 to 4 of Table A.3 use a continuous treatment measure—projected changes in HHI at the municipality level—instead of the binary treatment. The results show that firms spend less per pharmacist in municipalities 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.4, I show that the results for pharmacists do not change if we use average wages instead of wages from December.[19] 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.5 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 6 shows that the wages of salespeople do not exhibit the same decline as for pharmacists (see Appendix Figure A.4 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

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[19]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).

into account. In the next section, I show that changes in labor force composition play a large role in the merger I study.

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

Table 4 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 municipality level. The estimate is still not statistically different than zero if I cluster the standard errors at the municipality-year level or at the establishment level.

[**Table 4 here**]

Panel B of Table 4 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 municipality $\times$ year or establishment level, the effect is statistically significant at the 1% level.[20] 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 3 shows that municipalities in the treated group are more populous than countries in the control group. The concern is that, for being different, these municipalities have different trends in the wages of pharmacists and salespeople. I address this concern by including interactions between year indicators and pre-merger municipality characteristics in column 5 of Table 4. Municipality characteristics are indicators for each quartile of

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

the following variables: 2010 population, 2003 HDI, 2010 informality levels, and 2006 municipality government revenue per capita. The comparison of estimates in columns 4 and 5 of Table 4 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 municipalities 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 (Engbom and Moser, 2022), these effects should not differ across treatment and control groups.

Figure 7 presents lead and lag estimates following the specification from column 4, which includes establishment and person fixed effects. The omitted category represents the difference between the treated and control groups in 2011. Panels A and B of Figure 7 show the relative trends in residualized log wages between treated and control groups for pharmacists and salespeople at merging firms, respectively.[21] It's important to exercise caution when interpreting these results, as the sample used to identify such trends may differ from the sample in the estimates presented in Table 4. This discrepancy arises because the estimation in Figure 7 requires firm movers for each occupation within merging firms for every single year. Consequently, these estimates can be more susceptible to outliers. Panels C and D of Figure 7 present lead and lag estimates for pharmacists and salespeople in competing firms. The mean results are discussed in section V..

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.

C. Effect of the merger on other labor market outcomes

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

Column 2 of Table 5 presents the effects on average age. Changes in labor force

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[21]Figure A.5 presents the residualized trends for both treatment and control groups for pharmacists.

composition in terms of age explain the difference between the estimates in columns 1 and 2 of Table 4. 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 4 by 2%. This occurs because older workers usually receive higher wages. Therefore, the estimate of -5.6% is masking a larger decrease in wages, compensated by a change in labor force composition. The inverse occurs for salespeople, where the wage effect of -2.4% partially captures the reduction in the average age of workers.

Column 3 of Table 5 presents the changes in labor force composition using worker fixed effects as an outcome. These changes explain the main result of the paper: The difference between estimates in columns 3 and 4 of Table 4. 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 4 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 6 shows that this difference in estimates arises from a change in composition: Average age decreases by 1.7% and the average of pharmacists' fixed effects decreases by 4.8% (SE=0.012).

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

Column 1 of Table 6 presents the effects of the merger on employment in competing firms. There is a small increase in the number of pharmacists, but the effect is small, 0.187 (SE=0.098). Small effects on the employment of pharmacists might be explained by the production technology and the legal requirements drugstores entail: A pharmacist

always has to be working when the facility is open. The effect on the employment of salespeople is larger: an increase of 1.08 workers, which is around 10% of the number of salespeople in competing firms.

The fall in wages and increase in the employment of salespeople in competing firms is consistent with the hypothesis that firms act as strategic substitutes 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 8 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]

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

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 municipalities. This explains the null results of panel A of Figure 8. 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 3 shows that the merger does not change the HHI of salespeople when we define a labor market at the municipality×year×1-digit occupation.

However, salespeople working in drugstores might have smaller labor markets than previously thought. In Appendix Table A.9, I show that 74% of salespeople working in a drugstore continue as salespeople in the following year and that 73% still work in a drugstore in the following year. Table 7 shows that 24% of the salespeople who were working in a drugstore and switched jobs were still working in a drugstore the following year. The share of salespeople moving to drugstores should be closer to 5% if they had switched randomly to other salespeople jobs. I interpret these results as evidence that salespeople working in drugstores have higher preference to remain in drugstores (as in ?), face some type of information friction that prevents them to finding a job in another sector, or accumulate industry specific human capital such that an industry switch would imply into lower wages. In any of these cases, the evidence points that salespeople working in drugstores do not consider all salespeople jobs equally as their labor market.

Perhaps more strikingly, I show that pharmacists and salespeople may have preferences/information frictions/specific human capital over the type of drugstore they wish to work at. In Figure 9, 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 municipality as percentage of total drugstores. Pharmacy chains are defined as firms that have more than five establishments in one year in the entire country. I present results for two groups of workers based on their employer of origin: pharmacy chain or independent drugstore. Panel B of Figure 9 shows that salespeople who were working in a pharmacy chain are more likely to switch jobs to another pharmacy chain than salespeople who where originally working in an independent drugstore. Panel A of Figure 9 shows a similar pattern for pharmacists. These results provide additional evidence that labor markets

are significantly more specific than standard definitions imply. Thus, firms can exert market power even to low skill workers.

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.7 presents measures of concentration using the HHI. In the table, I report the average HHI and average merger-induced change in HHI across municipalities in the treated group, in which the merging firms overlapped and concentration increased. The merger-induced change in HHI in municipalities 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.7 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 the presence of strong worker unions. The results for salespeople are consistent with monopsony models as in **?** or Berger et al. (2022).

While the retail pharmacy sector may not be of general interest, the key message of this paper, which emphasizes the importance of accounting for changes in composition, applies to all sectors and studies that track establishments over time. If we did not

account for changes in composition in terms of unobservable worker characteristics, we would have incorrectly concluded that firms significantly reduced pharmacists' wages. This is something we have demonstrated to be untrue.

Retrospective merger analysis provides valuable insights into how labor markets can be defined. I present evidence that labor markets can be finer than the year-municipality intersection. Researchers examining labor markets typically use the intersection between year and regional units such as states, metropolitan areas, commuting zones, or municipalities 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 (2023); and Schubert et al. (2022), 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 municipality level. However, the merger reduced the wages of salespeople, which suggests that labor markets may be defined by the intersection of months, occupation, industry, municipality, and firm characteristic, such as firm size.

Lastly, this paper studied the effects of a merger through the mechanism of market power, excluding productivity gains (or synergies). Regulatory agencies are interested in the total effects of merger on wages, including these gains. The empirical strategy in this paper does not address these outcomes, since productivity gains 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 productivity gains that actually increased wages.

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Figures
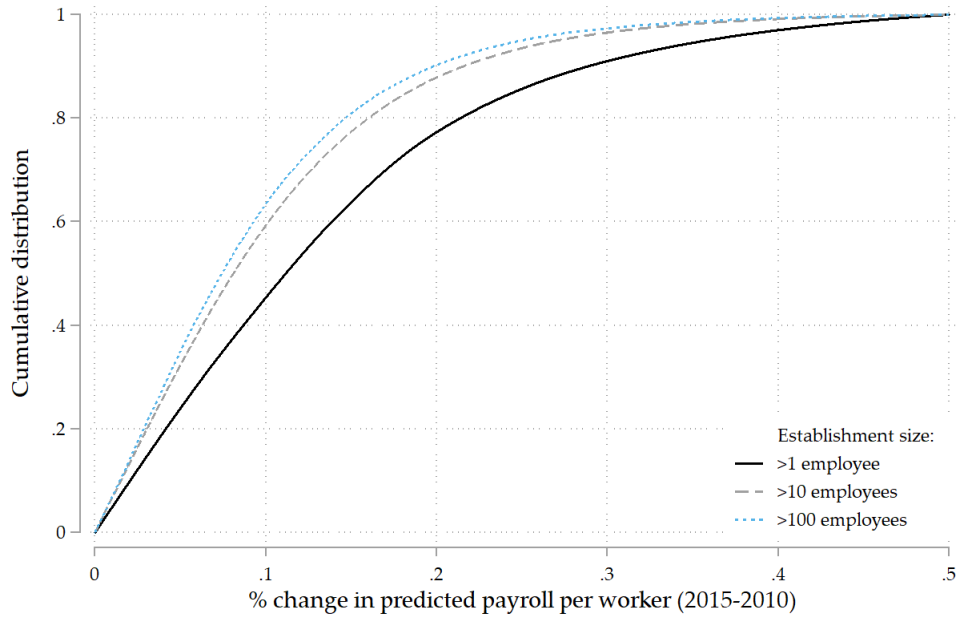
Figure 1: Cumulative distribution of changes in predicted payroll per worker due to changes in composition

Note: The figure shows the fraction of establishments (y-axis) by their changes in predicted payroll per worker between 2015 and 2010 (x-axis) in absolute value. Changes in payroll per worker only depend on changes in the establishment's workforce composition in terms of observable characteristics. To map worker characteristics to wages we use the estimates of a regression of log-wages on age and schooling interacted fixed effects, 3-digit occupation fixed effects, a cubic function of tenure, and a gender indicator. The wage regression uses data from 2015 and includes 59,264,277 observations from workers between ages 18 and 65 who worked 40 hours or more. Data: RAIS 2010 and 2015.
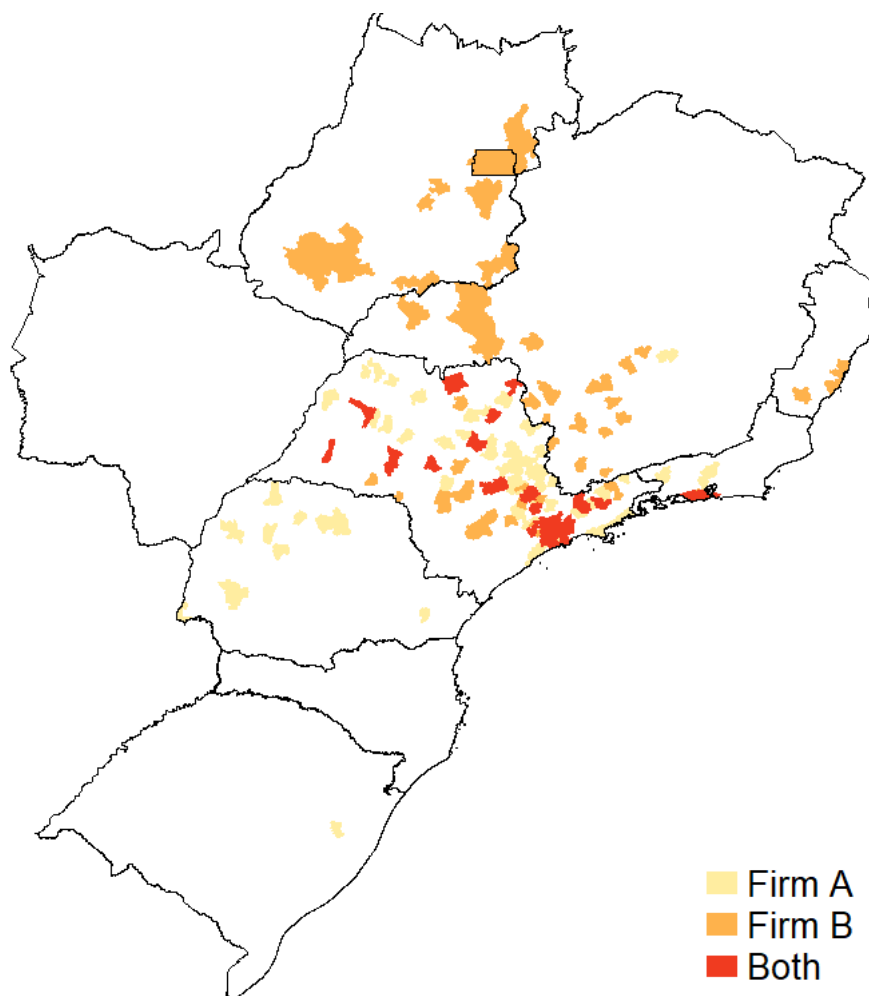
Figure 2: Presence of merging firms before the merger (2010)

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

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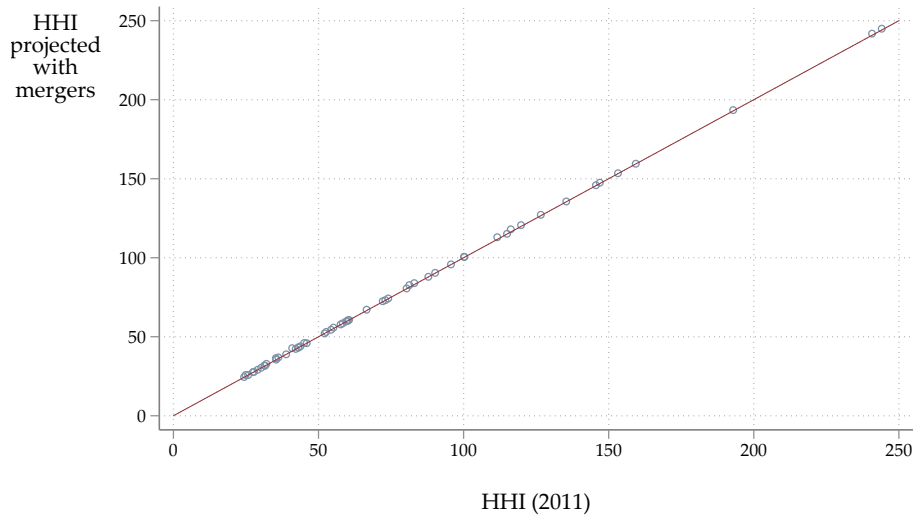
((b)) Salespeople

Figure 3: HHI projected with mergers versus 2011 HHI

Note: Each circle in the figure represents a municipality 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 municipality that hired workers in these occupations. The vertical axis displays the 2011 HHI projected with the merger. The projection serves to calculate a change in HHI that is due to the merger and not to the entrance and exit of firms or changes in firms' employment shares.
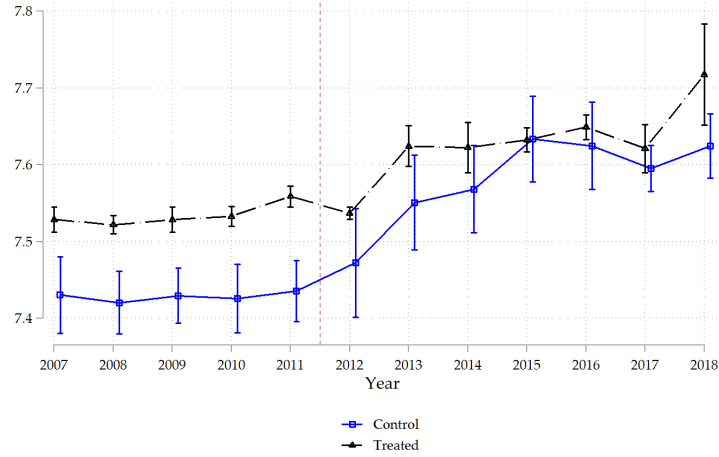
Figure 4: Trends - Ln(wage) of all workers in merging firms

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

Figure 5: 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 municipality level.

Number of observations: 12925

((a)) Pharmacists

Number of observations: 53320
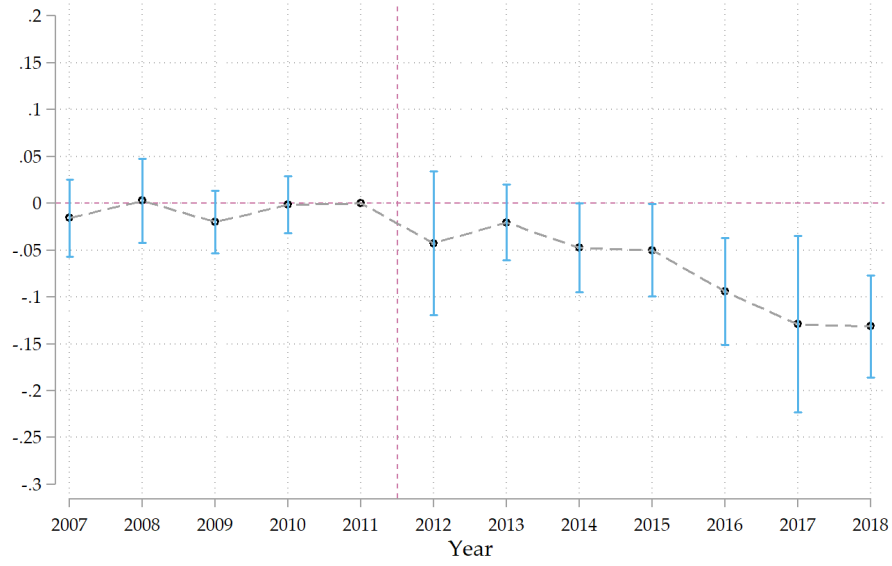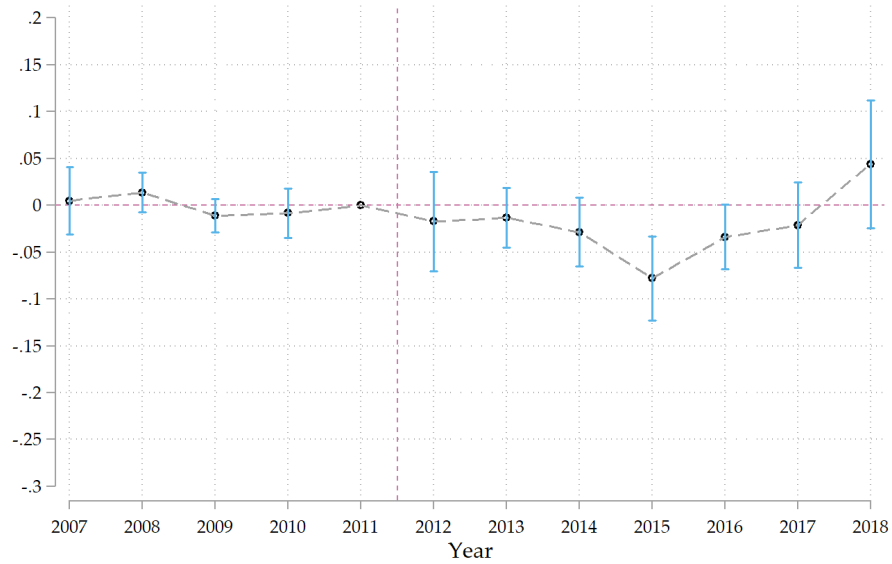
((b)) Salespeople

Figure 6: 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 municipality level.

((a)) Pharmacists - Merging firms
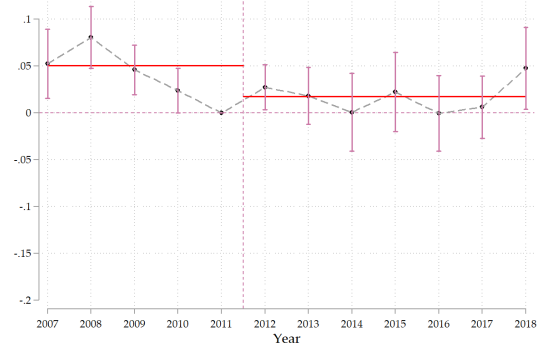
((b)) Salespeople - Merging firms
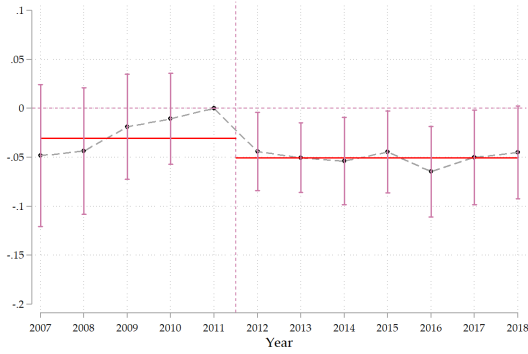
((c)) Pharmacists - Competing firms
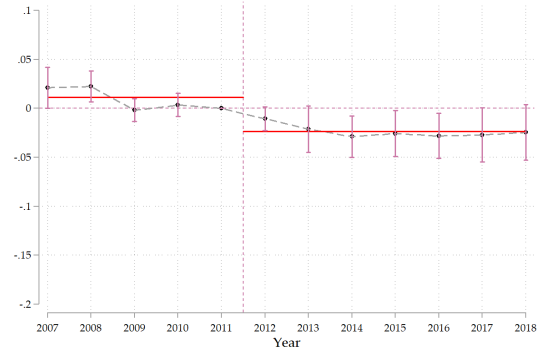
((d)) Salespeople - Competing firms

Figure 7: Leads and lags - Ln(wage) regressions 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, as presented in Equation 3. Log wages are measured in December. The sample only includes workers employed on December 31. Standard errors are clustered at the municipality level. Horizontal lines represent the mean of pre- and post-treatment coefficients.

Number of observations: 6377

((a)) Incumbent workers

Number of observations: 5423
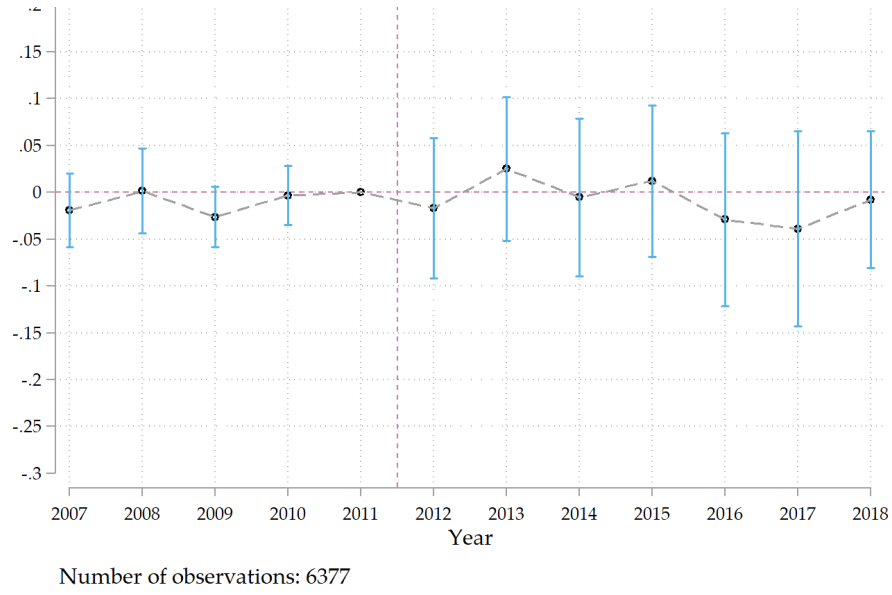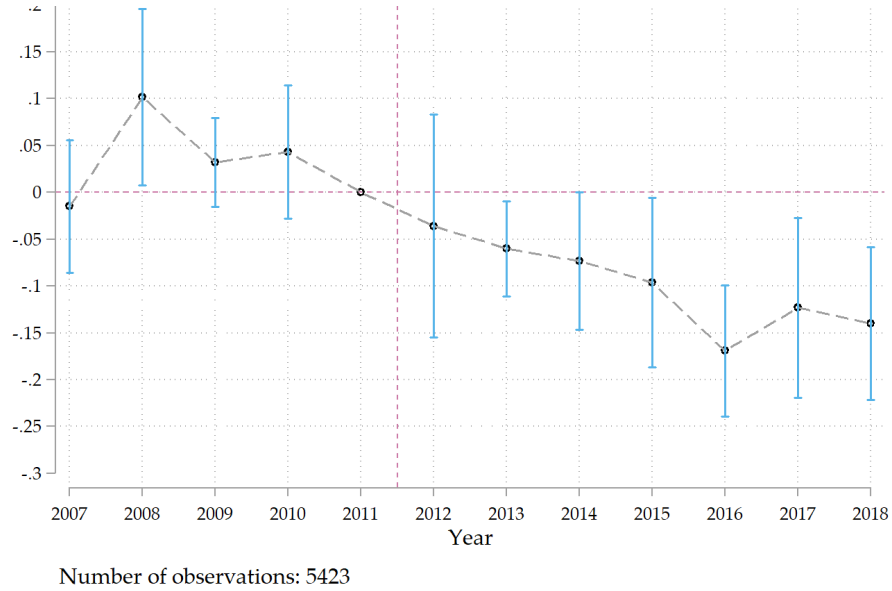
((b)) Entry-level wage

Figure 8: 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 municipality level.
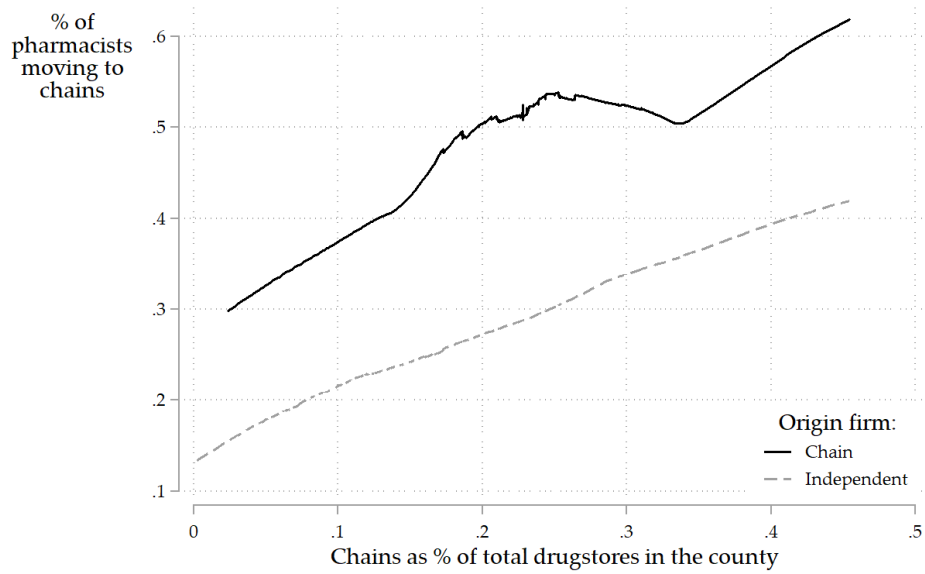
((a)) Pharmacists
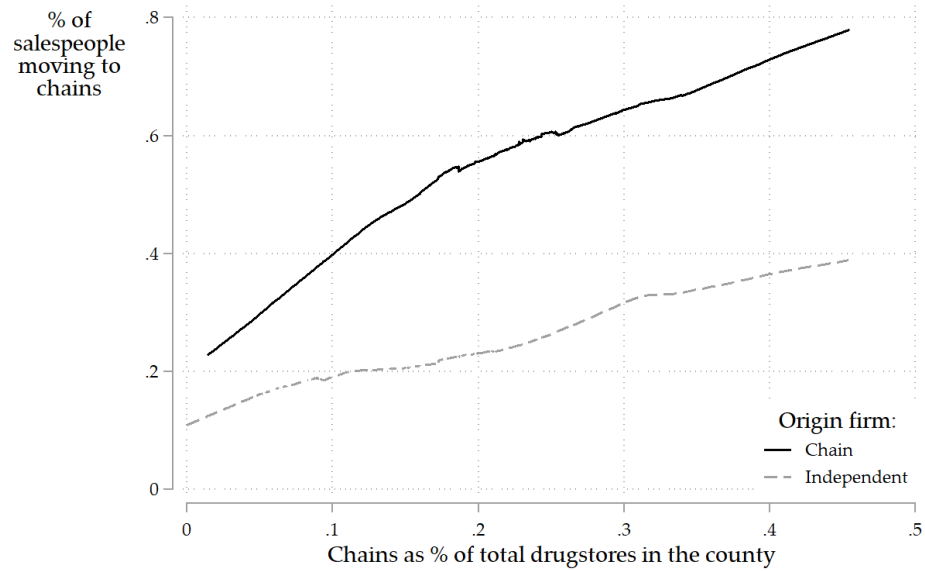
((b)) Salespeople

Figure 9: Job transition probabilities conditional on moving to another drugstore

Note: The figures plot nonparametric regressions at the municipality 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 municipality. 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.

Tables

Table 1: Sample size

Number of...	Drugstores Sample		Fully Connected Sample
	Merging firms	Competitors	
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 municipality 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: Share of establishments with changes in composition that imply a wage effect greater than 3% in absolute value

Variable	Standard Deviation			Share of establishments			3% wage effect
	1 year	3 years	5 years	1 year	3 years	5 years	
Δ Average age	1.7	2.6	3.1	21.2%	43.7%	60.2%	2.0
Δ Average tenure	8.9	14.0	18.4	20.0%	49.3%	67.3%	7.9
Δ Share male	7.5%	10.7%	12.1%	2.6%	7.8%	10.5%	19%
Δ Share with college	5.2%	7.5%	9.2%	22.1%	34.0%	40.1%	3%
Δ Share of professionals	4.4%	6.4%	7.4%	12.3%	18.5%	21.6%	3%
Δ Share of technicians	5.7%	8.4%	9.8%	5.3%	10.2%	12.7%	10%

Note: The first three columns present the standard deviation of the change in the variable for that row, between 2010 and 2011, 2013, and 2015, where a unit of observation is an establishment. The columns present differences between 2011, 2013, and 2015 relative to 2010 (one, three, and five-year differences, respectively). The sample includes all establishments in Brazil that had, in each sample year, at least 10 employees between ages 18 and 65 who worked 40 hours or more. After restrictions, the sample has 432,720 establishments. Columns 4 to 6 present the share of establishments for which the change in composition is greater than the value in Column 7, in absolute value. Column 7 presents the change in the variable that would imply a 3% change in wages, according to the estimates from a regression between log-wages and the variable in that row. Each wage regression uses data from 2015 and includes 59,264,277 observations. Data: RAIS 2010, 2011, 2013, and 2015.

Table 3: Summary Statistics of treated and control groups

	Control		Treated	
	Mean	Obs.	Mean	Obs.
Panel A: Municipality 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 municipality 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 4: 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 × Municipality 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 municipality level and presented in parentheses.

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

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

Note: Each cell represents the estimate of the difference-in-differences parameter from 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 municipality fixed effects. Standard errors are clustered at the municipality level and presented in parentheses.

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

Outcome:	Employment	Ln(Age)	Worker FE
	(1)	(2)	(3)

Panel C: Pharmacists in Competitors

Treat × Post	0.187	-0.017	-0.048
	(0.098)	(0.006)	(0.012)
Observations	127,710	82,421	72,450
Number of workers	-	26,443	23,107
Number of establishments	10,839	6,090	5,800

Panel D: Salespeople in Competitors

Treat × Post	1.078	-0.015	-0.016
	(0.522)	(0.005)	(0.016)
Observations	127,710	500,865	466,628
Number of workers	-	202,291	181,171
Number of establishments	10,839	10,173	9,920
Year FE	X	X	X

Note: Each cell represents the estimate of the difference-in-differences parameter from 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 municipality fixed effects. Standard errors are clustered at the municipality level and presented in parentheses.

Table 7: Job transition for movers

	Status in 2010				
Occupation in 2009	Same occupation as in 2009	Same municipality 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, municipality, micro-region and pharmacy in 2010, respectively. Column 5 shows the sample size for each occupation. Source: RAIS 2009 and 2010.

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 municipality, (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.8, 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, a typical drugstore has 8.5 employees, where 1 or 2 of these employees are a pharmacist and 4 to 5 are salespeople (including cashiers). Other employees can be working in managerial tasks, cleaning, product delivery, or organizing inventory.

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

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

Table B.1 shows that the retail pharmacy sector in Brazil was growing during the pe-

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[23]CMED Resolution n°2, March 5, 2004.

[24]Law number 5,991 from 1973.

[25]Law number 13,021, from 2014 and Resolução – RDC n° 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).

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

The growth in the sector promoted changes in the composition of pharmacists and other workers. Table B.1 shows that the number of individuals working in pharmacies has almost doubled. This is true for both pharmacists and salespeople. Panel A of Figure B.1 shows that the number of pharmacists per 10,000 residents increased from 3.3 to 6.6 between 2007 and 2018. While this increase did not significantly change the share of female pharmacists (69% to 73%, Panel B, Figure B.1), it did increase racial diversity in the occupation. Panel C 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 (Panel D, Figure B.1). Last, the share of workers with a high school degree increased from 78% to 91% (Table B.1).

Despite the increase in employment, the wages of pharmacists also increased in the period. Appendix Table B.1 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 B.2 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.

A. Consolidation in the retail pharmacy sector

During the 2000s and 2010s, the Brazilian retail pharmacy sector underwent a gradual process of market consolidation. In 2004, pharmacy chains accounted for only 34% of to-

tal sales, but by 2014 their market share had risen to 53% (Fernandes et al., 2020).[28] This consolidation process was driven both by mergers and acquisitions and by the organic expansion of individual groups. While the main players differed in their strategies—for instance, in the customer segments they targeted—there were two common features in the growth strategies of the largest chains: (i) increasing the number of stores in markets where they were already established, and (ii) expanding into new regions. The first strategy was driven by demographic trends, particularly population aging in Brazil, as older individuals tend to consume more pharmaceutical products; the second was supported by expectations of regional economic growth across the country.

Table B.1 and Figure B.3 corroborate this pattern, showing that the number of drugstores increased substantially between 2007 and 2018. Most of this expansion was driven by pharmacy chains—drugstores belonging to firms that operated at least five establishments in a given year—as illustrated in Figure B.3.[29] The firms involved in the merger analyzed in this paper experienced a much faster expansion. This growth occurred along two dimensions: by 2018, the merging firms operated in three times as many municipalities as in 2007 (Figure B.4) and owned four times as many establishments (Figure B.5). Importantly for the analysis in this paper, this expansion was primarily driven by store openings, with closures being relatively rare, as shown in Figure B.5.[30]

Besides organic growth, the largest pharmacy chains engaged in a series of mergers and acquisitions during the 2000s and 2010s. These mergers were justified in reports to investors as a means of creating synergies that would enable firms to reduce costs and increase market share. Brazil's economic growth and the development of its financial market likely contributed to the timing of these mergers. Several groups completed their IPOs in the mid-2000s. In 2007, firms with more than 100 establishments employed only 14% of all pharmacy workers. At the time, financial analysts expected the sector to undergo a consolidation process similar to that observed in the U.S. retail pharmaceutical industry. Indeed, many industry reports referred to the American pharmaceutical sector as a benchmark for the future of the Brazilian market. By 2016, firms with more than 100 establishments employed 33% of all pharmacy workers, and the share of revenue from large chains also increased, reaching around 53% of the sector's total revenue.

Another concern in this study is that the merger under analysis could overlap with other mergers, potentially confounding the treatment effect or even affecting the control

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[28]Fernandes et al. (2020) describe the main characteristics of the Brazilian pharmaceutical retail market, the sector's outlook at the time of the study, and an analysis of the main players during the 2010s. The authors draw on information from industry reports, media sources, and an interview with an experienced pharmaceutical specialist who held relevant positions in leading firms.

[29]We define a pharmacy chain as a firm operating five or more establishments in a single year. This approach likely underestimates the true importance of pharmacy chains, as some groups may operate under multiple firm identifiers (CNPJ).

[30]The data for 2012 are duplicated due to the merger, which caused establishments to be reported twice in RAIS.

group. I examine the eight largest pharmacy chains operating in Brazil in 2011 and argue that this is not the case.[31]

First, only four of these chains had establishments in the municipalities included in my sample in 2011 (before the merger analyzed here). This is not problematic, since the analysis also accounts for effects on competing firms. Moreover, the major retail pharmacy groups were largely concentrated in different states or regions of Brazil at the time.

Second, one of the other large chains underwent a merger during the same two-year period as the merger studied here. This could be problematic if there were overlaps with the municipalities in my sample. Fortunately, these merging firms had very limited geographic overlap—one was primarily concentrated in Rio de Janeiro and the other in São Paulo. Out of the 91 municipalities in the sample, overlap occurred in only five, and the difference in market presence between the two firms was such that the merger did not significantly affect labor market concentration in any of these cases.

Naturally, these chains continued to expand and opened new drugstores in many municipalities, including those in my sample. In such cases, changes in concentration may arise from entry rather than consolidation. I argue that, as long as such entry occurred similarly across treatment and control groups, it should not bias the analysis. In the next section, I show that the merger analyzed in this paper shifts the distribution of labor market concentration in treated municipalities relative to control municipalities.

Despite the initial expansion of pharmacy chains, the growth of pharmacy chains in terms of market share has slowed in recent years and fell short of analysts' expectations. This outcome is often attributed to the sustained market share of independent pharmacies, supported by the sale of generic medications through a governmental program (RD, 2018).

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[31]By 2011, there were eight large pharmacy groups with more than 100 establishments each: RaiaDrogasil (merger between Droga Raia and Drogasil), DPSP Group (merger between Drogaria São Paulo and Drogarias Pacheco), BR Pharma (acquisition of Farmais, Rosário, and Big Ben), Profarma (Drogasmil, Farmalife, Drogarias Tamoio), Pague Menos, Panvel, Extrafarma, and Araujo. In later years, other groups also grew and became relevant players, such as Drogal, Rede Maxxi Econômica, Farmácia Preço Popular, Drogaria Nissei, São João Farmácias, and Drogaria Globo, as well as some supermarket chains like Grupo Pão de Açúcar and Carrefour.

Table B.1: 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.
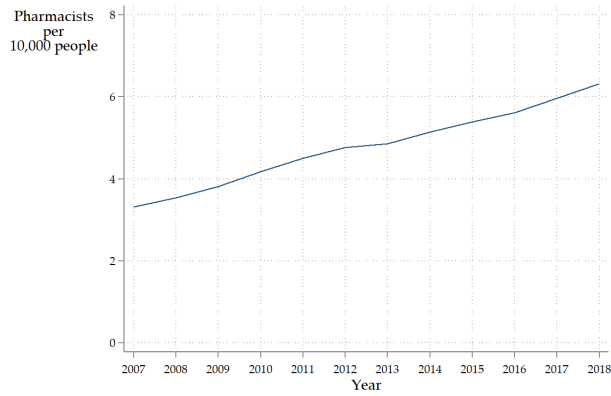
((a)) Pharmacists per capita
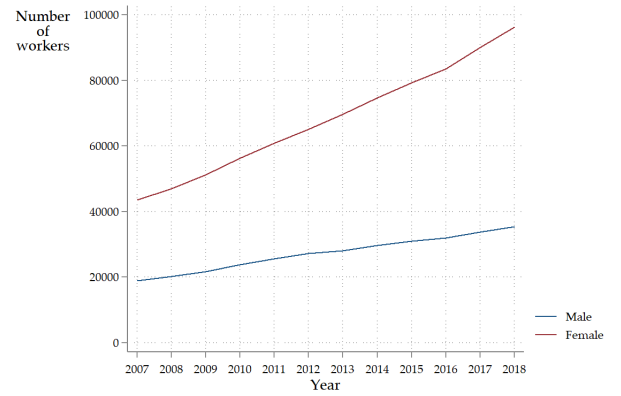
((b)) Pharmacists' gender composition
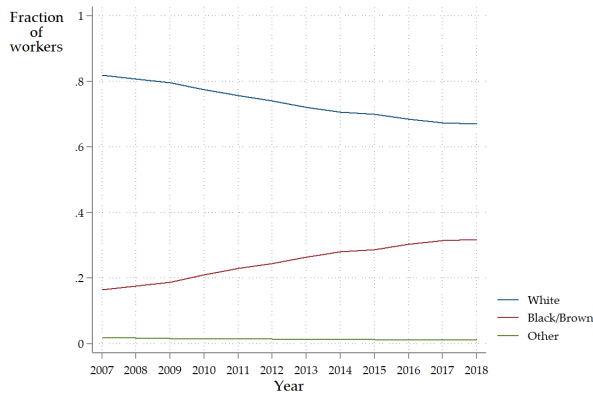
((c)) Pharmacists' racial composition
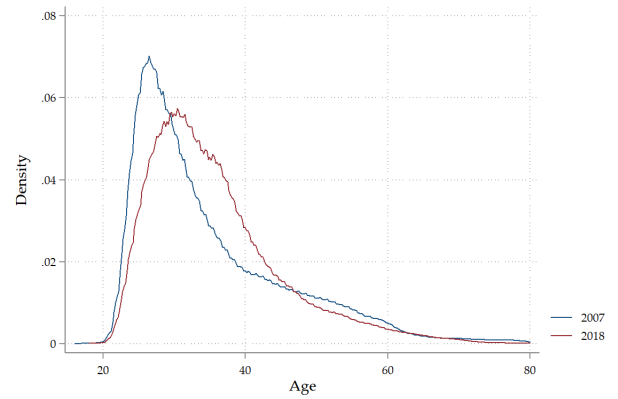
((d)) Pharmacists' age distribution

Figure B.1: Trends in pharmacists' occupation

Note: Panel A shows the evolution of the number of pharmacists per capita in Brazil. Pharmacist data is sourced from RAIS (2007–2018), while yearly population estimates are obtained from the Census and Ipeadata. Panel B presents the evolution of the gender composition of pharmacists. Panel C displays the racial composition. Panel D compares the age distribution of pharmacists in 2007 and 2018. Source: RAIS (2007–2018).
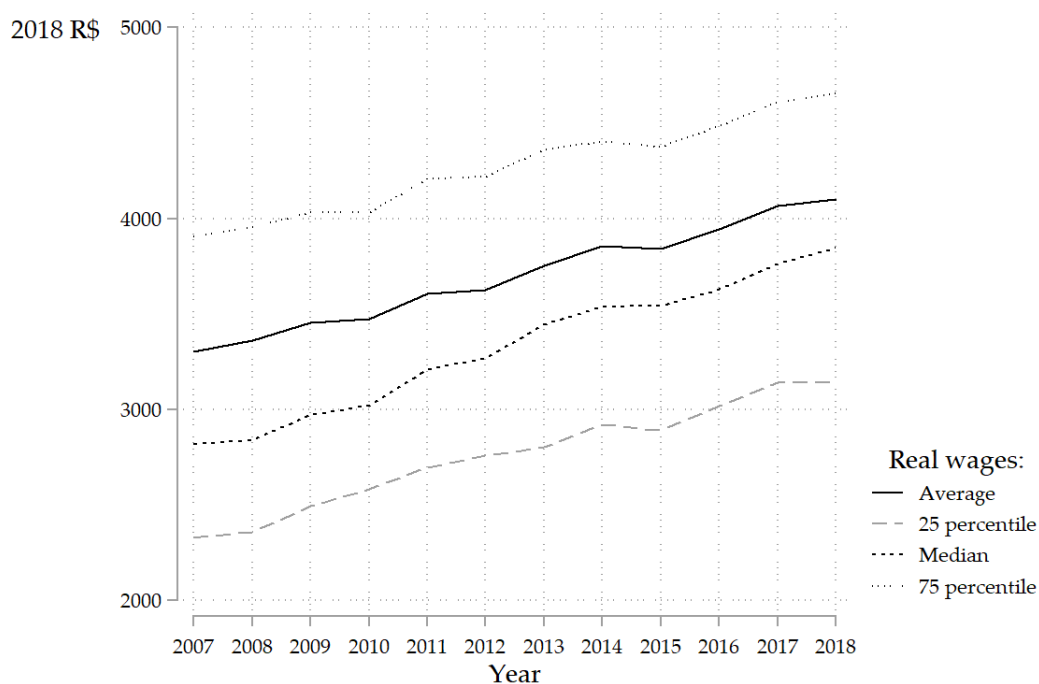
Figure B.2: Trends in pharmacists' wage distribution - Brazil

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.
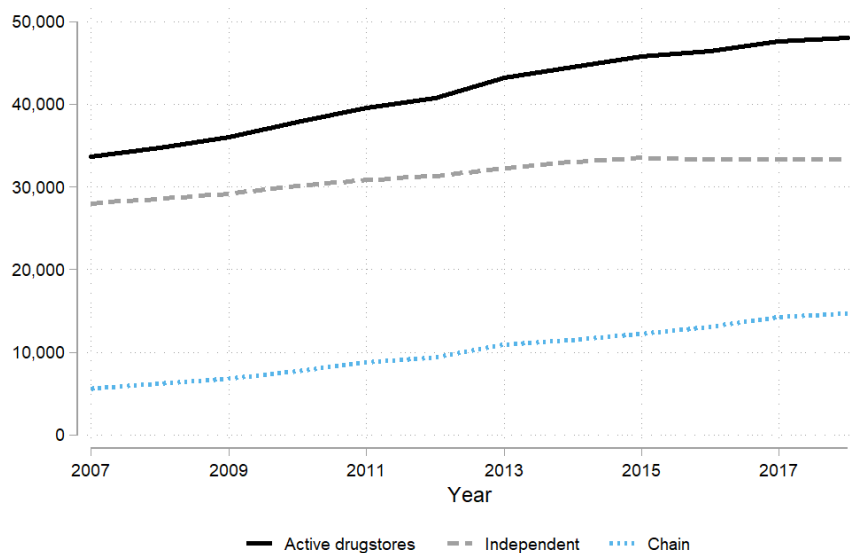
Figure B.3: National expansion - Number of Establishments

Note: The figure shows the number of drugstores in Brazil with at least one pharmacist, distinguishing between independent establishments (firms with fewer than five establishments in a given year) and pharmacy chains (firms with five or more establishments in a given year). Source: RAIS 2007-2018.
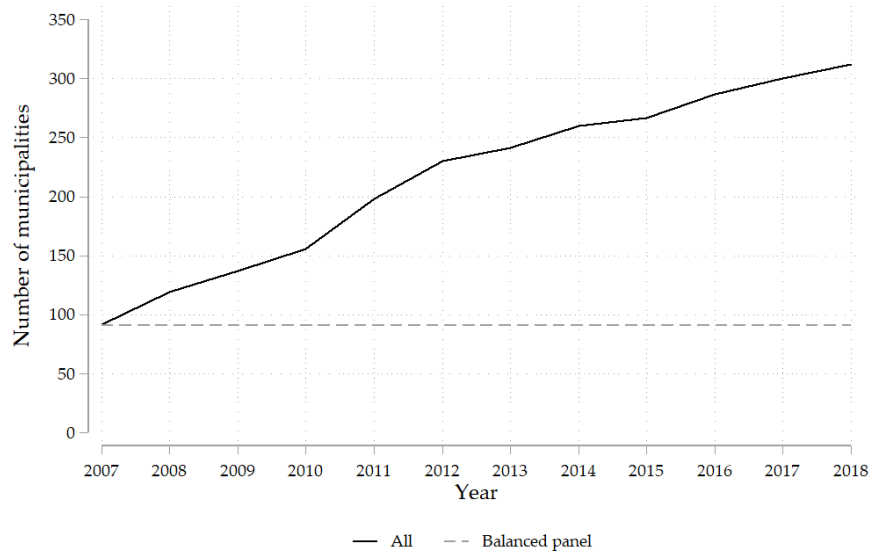
Figure B.4: Merging firm's expansion - Number of Municipalities

Note: The figure shows the number of municipalities where merging firms had at least one establishment each year.
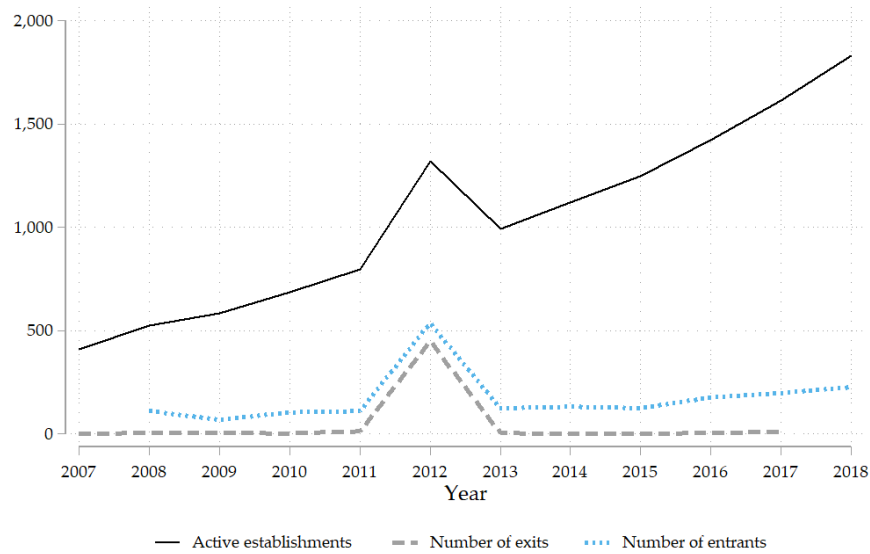
Figure B.5: Merging firm's expansion - Number of Establishments

Note: The figure shows the number of active establishments, exits, and new entries from merging firms.

# C Changes in composition in the retail pharmacy sector

Changes in the composition of workers at pharmacies are as prevalent as those observed across establishments in all sectors, as reported in Section B..

Table C.1 presents an analysis of compositional changes in drugstores, comparable to the analysis in Table 2 for all sectors. The sample is restricted to establishments operating as drugstores with at least five employees in 2010, 2011, 2013, and 2015, resulting in a final sample of 16,222 establishments.

There is substantial variation in changes to establishments' composition across all observable variables, with slightly greater variance than that observed in establishments from all sectors. Columns 1, 2, and 3 of Table C.1 present the standard deviation of changes in average age, average tenure, share of male employees, share with a college degree, share of pharmacists, and share of salespeople for 1-, 3-, and 5-year periods, respectively. On average, establishments change the composition of their workforce in terms of average age by more than two years between 2010 and 2011, compared to 1.7 years for establishments across all sectors.

To illustrate the frequency and commonality of such changes, Appendix Figure C.1 plots the average age of workers at 46 establishments where the 2012 average age was between 30 and 30.1 years. The sample is restricted to establishments with a median size of 10 to 20 employees to minimize variance from outliers. Even within this small sample, it is striking that establishments exhibit large annual fluctuations in average age, and this pattern persists across multiple years of the data.

Column 7 of Table C.1 presents the change in composition required to produce a 3% change in wages, as predicted by a regression of log-wages on each variable in levels. Interestingly, these values are higher than those in the corresponding column of Table 2. In other words, age and tenure appear to be less predictive of wages in the retail pharmacy sector than in the overall economy. Notably, gender has an almost negligible effect on wages. This aligns with Goldin and Katz (2016)'s prediction of pharmacy as an egalitarian profession and extends this perspective to the entire retail pharmacy sector.

Columns 4, 5, and 6 report the share of establishments that experienced a change in composition substantial enough to result in a 3% change in wages (as predicted by Column 7). Nearly 20% of drugstores underwent shifts in their worker age composition that could lead to a 3% change in average wages. This percentage is lower than the corresponding value in Table 2, not due to lower variance but because age has a smaller impact on wages in the retail pharmacy sector. This pattern is even more pronounced for gender. While establishments exhibit significant fluctuations in their gender composition, these changes have virtually no effect on wages. As a result, no establishment experienced a shift in gender composition that would translate into a 3% wage change.

The most critical factor to consider is occupation. There is substantial variation in the

share of employees working as pharmacists or salespeople, and since these occupations have significantly different salaries, even minor shifts in composition can lead to major changes in predicted wages. Consequently, more than 50% of establishments experienced changes in their occupational composition that could result in wage fluctuations exceeding 3%. Notably, many establishments exhibited even larger implied wage changes.

These findings highlight the importance of having detailed occupational data when analyzing wage dynamics within establishments over time.

Table C.1: Share of drugstores with changes in composition that imply a wage effect greater than 3% in absolute value

Variable	Standard Deviation			Share of establishments			3% wage effect
	1 year	3 years	5 years	1 year	3 years	5 years	
Δ Average age	2.1	3.0	3.5	7.3%	19.0%	29.8%	3.8
Δ Average tenure	8.4	13.3	17.1	2.7%	12.2%	24.8%	19.9
Δ Share male	11.2%	15.0%	16.7%	0.0%	0.0%	0.0%	286.6%
Δ Share with college	9.2%	10.9%	12.4%	46.0%	58.7%	65.1%	3.8%
Δ Share of pharmacists	7.3%	9.6%	10.8%	40.3%	52.3%	57.4%	3.5%
Δ Share of salespeople	13.1%	18.0%	20.0%	44.7%	59.0%	64.2%	7.5%

Note: The first three columns present the standard deviation of the change in the variable for that row, between 2010 and 2011, 2013, and 2015, where a unit of observation is an establishment. The columns present differences between 2011, 2013, and 2015 relative to 2010 (one, three, and five-year differences, respectively). The sample includes all drugstores in Brazil that had, in each sample year, at least 5 employees between ages 18 and 65 who worked 40 hours or more. After restrictions, the sample has 16,222 establishments. Columns 4 to 6 present the share of establishments for which the change in composition is greater than the value in Column 7, in absolute value. Column 7 presents the change in the variable that would imply a 3% change in wages, according to the estimates from a regression between log-wages and the variable in that row. Each wage regression uses data from 2015 and includes 593,036 observations. Data: RAIS 2010, 2011, 2013, and 2015.
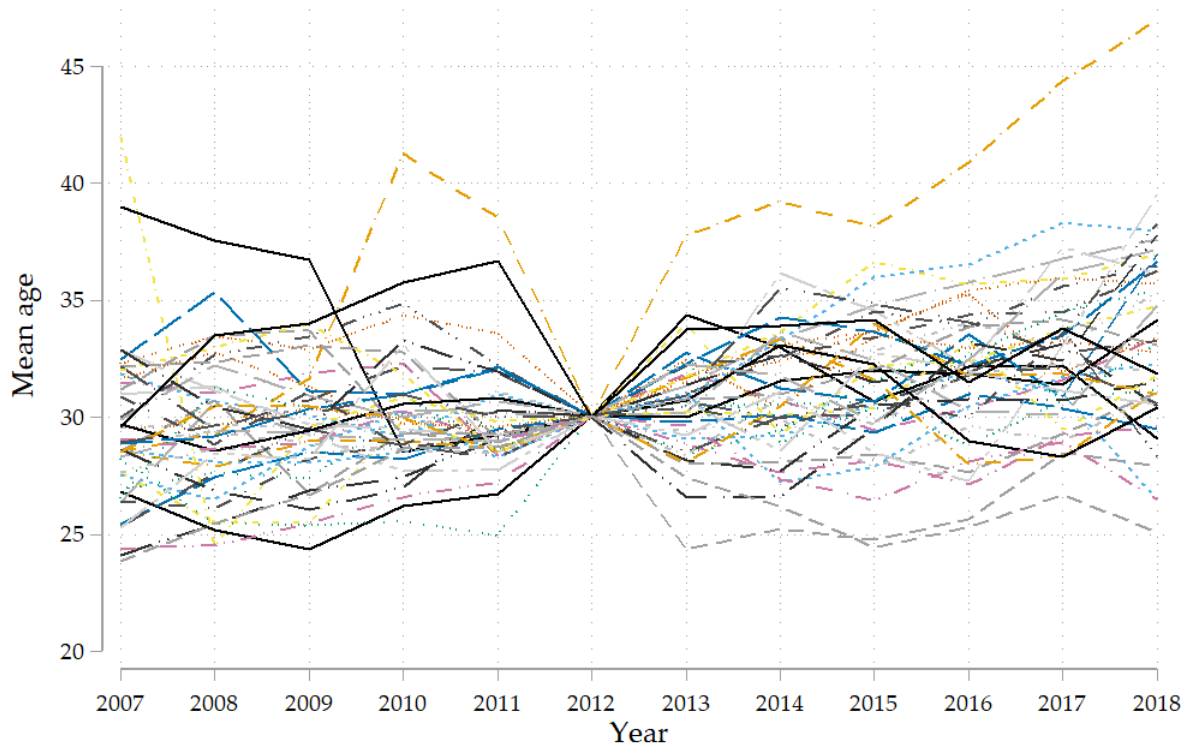
Figure C.1: Example: Change in drugstore's median workers' age

Note: The figure plots the evolution of the median worker's age at 46 establishments. The example restricts the sample to drugstores with a median age in 2012 between 30 and 30.1 years old and with a median size between 10 and 20 employees. Each line represents an establishment. Source: RAIS 2007-2018.

D SUTVA: distance between markets and worker mobility

In this section, I address concerns regarding the validity of the Stable Unit Treatment Value Assumption (SUTVA) in the empirical setting of this paper. SUTVA requires that: (i) the assignment to treatment does not change over time, and (ii) the treatment of one observation does not interfere with the treatment assignment or potential outcomes of another observation.

To understand potential violations of SUTVA and avoid confusion, it is first necessary to clarify the unit of treatment and the process of treatment assignment. In this context, labor market definitions play a key role.

The unit of treatment in this study is the establishment, not the worker. Assignment to treatment or control at the establishment level is determined by two factors: the national-level merger and the pre-existing overlaps between merging firms within the same labor market. Establishments—both of merging and non-merging firms—are assigned to the treatment group if they are located in a labor market "affected" by the merger. Theoretical models of monopsonistic competition (and related market structures) suggest that an "affected" labor market is one where merging firms had an overlapping presence prior to the merger, such that the merger induces gains in market power. In summary, establishments are the unit of treatment, and treatment assignment depends on how the labor market is defined.

(i) The assignment to treatment does not change over time

The first concern raised by SUTVA is that the assignment to treatment must be stable and not change over time. Defining establishments as the unit of analysis directly addresses this issue. Establishments do not change location, and during the study period, firms rarely closed establishments (Figure B.5). As a result, the pre-merger overlap of merging firms—which determines treatment assignment—remains consistent over time and reliably leads to increased market concentration.

One potential violation of SUTVA arises from the exogenous entry or exit of establishments belonging to other firms, which may alter market concentration. Figure 3 illustrates changes in concentration driven by the merger, while Figures D.1, D.2, and D.3 show actual changes in concentration for the periods 2011–2014, 2011–2016, and 2011–2018, respectively. Concentration increases in most treated municipalities but also rises in control municipalities. Importantly, the distribution of changes in the HHI in treated municipalities stochastically dominates that in control municipalities across all years. Nonetheless, results toward the end of the period should be interpreted with caution, as the excess increase in concentration in treated municipalities may partly reflect post-merger entry by locally dominant pharmacy groups and the exit of independent pharmacies.

(ii) The treatment of one observation does not interfere with the treatment assignment or potential outcomes of another observation.

The second concern of SUTVA is that treatment may spill over to control groups. A common concern is that worker mobility across municipalities could threaten identification by creating such spillovers. I argue that this is a second-order issue, because mobility patterns and the geographic distribution of treated and control municipalities make such spillovers unlikely.

First, treated and control municipalities share few borders, reducing the probability of spillovers. Table D.1 shows 660 total borders with neighboring municipalities, of which only 17 are treated–control borders. Control municipalities have on average 7.4 neighbors, including about 0.3 treated neighbors and 0.5 control neighbors. If neighbors exert equal influence on the local labor market, potential spillovers from treated municipalities would be weighted at roughly 10%.

Secondly, establishments are the unit of treatment, so worker mobility across municipalities could affect treatment only by inducing a labor-supply shock in control areas. I argue that this is second-order, since it would require substantial displacement caused by the merger—something that does not occur in the merger we study.

Lastly, mobility patterns across municipalities suggest that the municipality×occupation definition appropriately captures local labor markets, and that neighboring municipalities make only a minor contribution to them. Table D.2 presents data on worker mobility. Between 2007 and 2008, around 15% of workers switched jobs, and slightly fewer than half of them also changed municipality. Interestingly, the destination municipality is usually neither a neighbor nor within the same microregion.[32] To quantify this, only 2.4% of workers switch jobs to another municipality in the same microregion within a year. Table D.2 also reports mobility patterns between workers in treated and control municipalities. Within three years, only 0.3% of pharmacists and 0.2% of salespeople move from a treated municipality to a control municipality, and only 0.4% make the reverse move. I interpret these mobility patterns as evidence that treatment spillovers to other units are highly unlikely.

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[32]Microregion is a broader geographic unit that includes multiple municipalities within the same state.
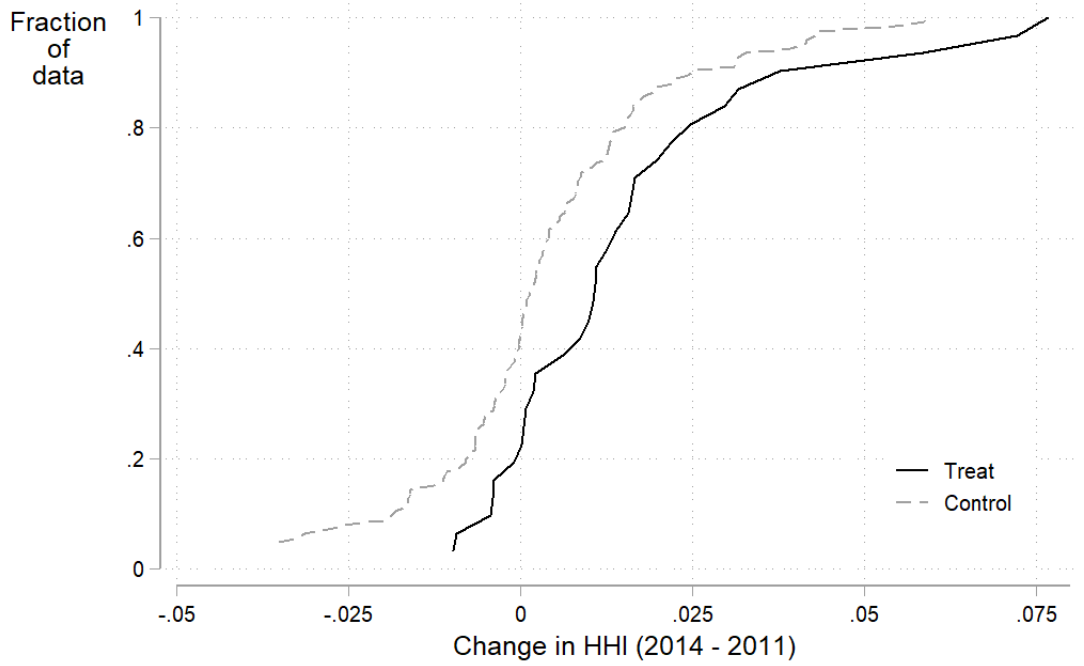
Figure D.1: Cumulative distribution function of changes in HHI between 2011 and 2014

Note: The figure presents the cumulative distribution of HHI changes across municipalities over the three-year period from 2011 to 2014, for municipalities in the treated group (solid line) and the control group (dashed line).
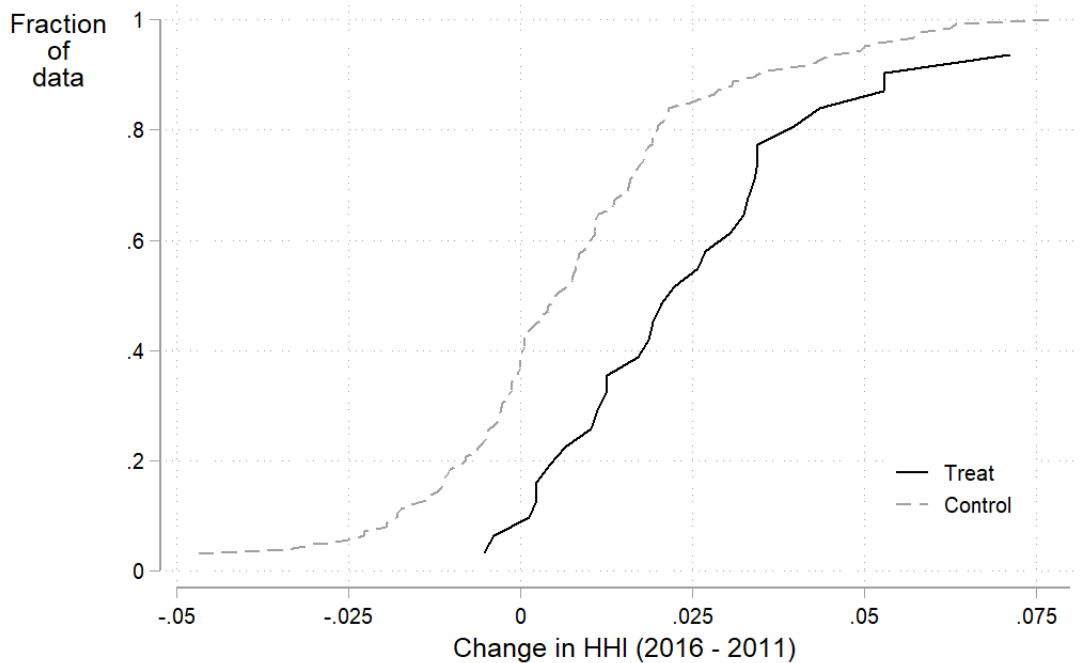
Figure D.2: Cumulative distribution function of changes in HHI between 2011 and 2016

Note: The figure presents the cumulative distribution of HHI changes across municipalities over the five-year period from 2011 to 2016, for municipalities in the treated group (solid line) and the control group (dashed line).
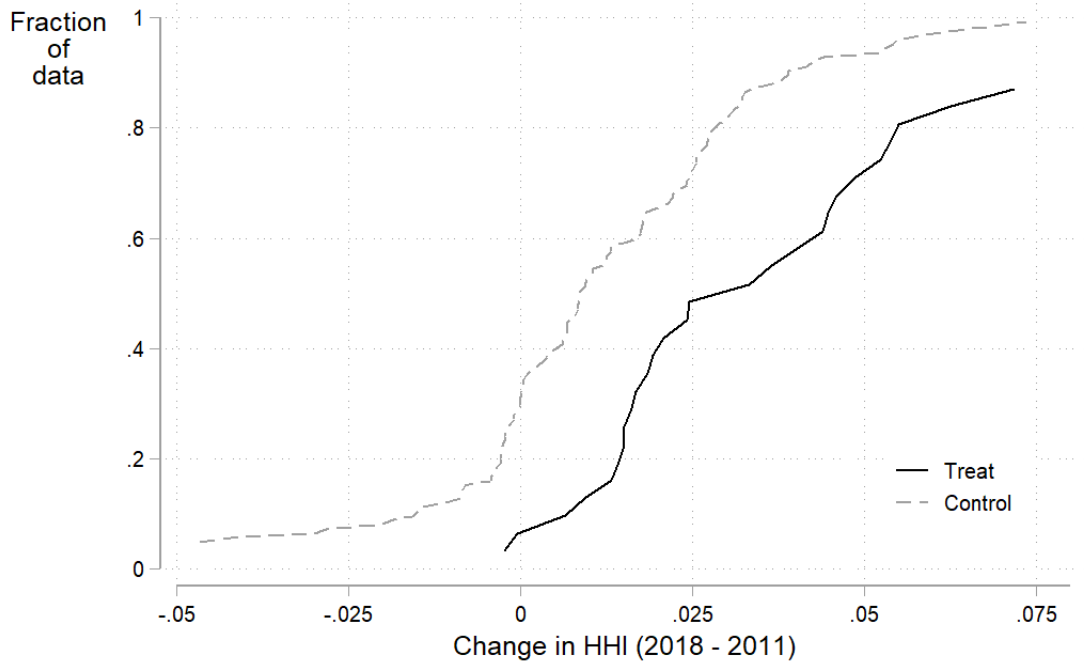
Figure D.3: Cumulative distribution function of changes in HHI between 2011 and 2018

Note: The figure presents the cumulative distribution of HHI changes across municipalities over the seven-year period from 2011 to 2018, for municipalities in the treated group (solid line) and the control group (dashed line).

Table D.1: Neighbors of Treated and Control municipalities

| | Treated | Control |
	N. obs.	N. obs.
Municipalities	30	61
Neighboring municipalities	208	452
Neighboring borders within the same group	48	30
Neighboring borders between treated and controls	17	
Municipalities with at least one treated neighbor	18	10
Municipalities with at least one control neighbor	11	24

	min.	avg.	max.	min.	avg.	max.
Number of neighbors	2	6.9	20	2	7.4	11
Number of treated neighbors	0	1.6	7	0	0.3	4
Number of control neighbors	0	0.6	4	0	0.5	2

Note: The table presents descriptive statistics on the number of neighboring municipalities of treated and control municipalities, as well as their treatment status.

Table D.2: Mobility patterns - 2007 to 2008/2010

| Sample:	Treated		Control		All	
Period within the course of...	1-year	3-years	1-year	3-years	1-year	3-years
a. All workers						
Number of workers	6,852,220	6,265,053	3,427,289	3,151,170	26,661,046	24,371,693
Share of workers switching establishments	16.6%	41.2%	16.3%	40.2%	14.7%	38.4%
Share of workers switching municipalities	7.5%	16.8%	7.5%	16.3%	7.2%	17.0%
Share of workers switching to a municipality within the same microregion	2.1%	4.9%	2.5%	5.8%	2.4%	5.8%
as % of municipality switchers	28.2%	29.2%	32.9%	35.6%	33.3%	33.9%
Share of workers switching to a neighboring municipality	2.4%	5.8%	2.5%	5.7%	2.4%	5.8%
as % of municipality switchers	32.3%	34.2%	33.9%	35.0%	33.3%	34.0%
Share of workers switching to a neighboring treated municipality	1.6%	3.8%	0.5%	0.9%	0.6%	1.5%
as % of municipality switchers	21.4%	22.3%	6.1%	5.6%	8.9%	8.8%
Share of workers switching to a neighboring control municipality	0.2%	0.4%	0.9%	2.0%	0.3%	0.7%
as % of municipality switchers	2.3%	2.3%	11.7%	12.1%	4.2%	4.1%
b. Pharmacists						
Number of workers	7,371	7,665	4,654	4,298	28,334	25,989
Share of workers switching establishments	29.2%	60.2%	28.1%	51.3%	26.4%	53.9%
Share of workers switching municipalities	9.7%	22.1%	9.9%	19.4%	11.4%	24.1%
Share of workers switching to a municipality within the same microregion	3.6%	8.1%	3.7%	6.3%	4.5%	9.2%
as % of municipality switchers	36.6%	36.9%	36.9%	32.7%	39.6%	38.2%
Share of workers switching to a neighboring municipality	4.2%	8.9%	3.5%	6.1%	4.3%	8.8%
as % of municipality switchers	43.4%	40.5%	34.8%	31.5%	38.0%	36.4%
Share of workers switching to a neighboring treated municipality	2.9%	6.1%	0.4%	0.7%	1.1%	2.7%
as % of municipality switchers	30.2%	27.5%	3.7%	3.8%	9.6%	11.0%
Share of workers switching to a neighboring control municipality	0.3%	0.5%	1.0%	1.5%	0.6%	1.0%
as % of municipality switchers	2.9%	2.4%	9.7%	7.9%	5.2%	4.3%
c. Cashiers						
Number of workers	102,187	88,573	41,077	34,149	336,371	289,641
Share of workers switching establishments	21.2%	53.4%	25.4%	59.4%	20.6%	53.9%
Share of workers switching municipalities	6.8%	14.7%	6.7%	14.8%	6.5%	15.3%
Share of workers switching to a municipality within the same microregion	2.4%	5.5%	3.2%	6.5%	2.8%	6.5%
as % of municipality switchers	35.2%	37.5%	47.1%	44.3%	43.9%	42.5%
Share of workers switching to a neighboring municipality	2.6%	6.2%	3.2%	6.8%	2.8%	6.6%
as % of municipality switchers	37.6%	42.4%	47.8%	46.2%	43.1%	43.1%
Share of workers switching to a neighboring treated municipality	1.5%	3.4%	0.4%	1.1%	0.7%	1.7%
as % of municipality switchers	21.7%	23.0%	6.4%	7.6%	10.9%	11.4%
Share of workers switching to a neighboring control municipality	0.2%	0.4%	1.2%	2.3%	0.4%	0.8%
as % of municipality switchers	2.2%	2.6%	17.2%	15.9%	5.7%	5.4%

Note: The table reports descriptive statistics on worker mobility patterns for all workers, pharmacists, and salespeople (Panels a, b, and c, respectively) across treated, control, and all municipalities. One-year mobility refers to worker transitions between 2007 and 2008, while three-year mobility refers to transitions between 2008 and 2010. Source: RAIS 2007, 2008, and 2010.

Appendix Figures
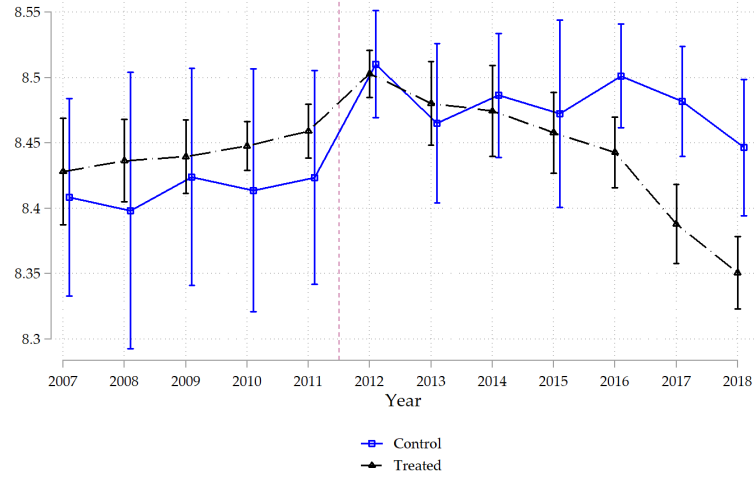
((a)) Pharmacists
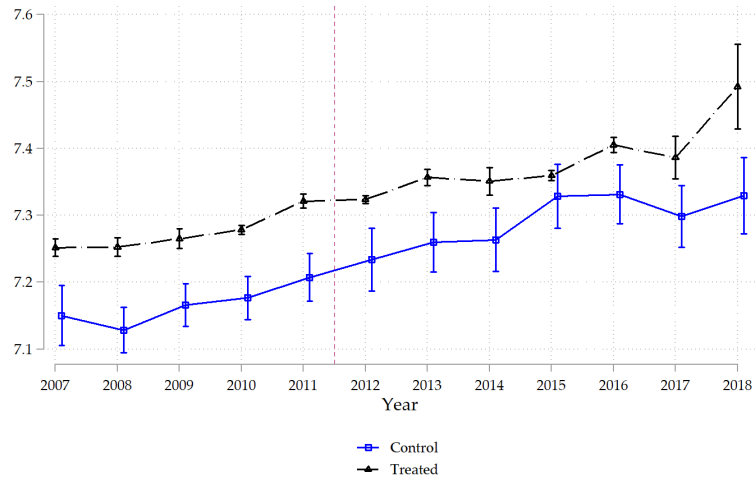
((b)) Salespeople

Figure A.4: 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 municipality level.
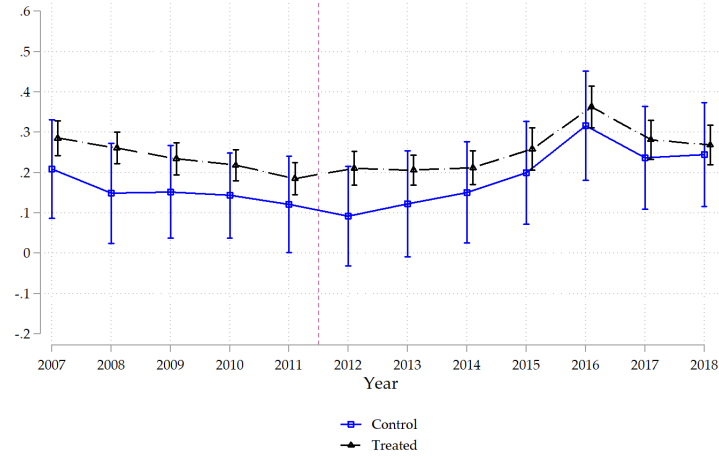
Figure A.5: Residual ln(wage) of pharmacists in merging firms with two-way fixed effects

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 municipality level.

Appendix Tables

Table A.3: 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 municipality 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 municipality level.

Table A.4: 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: Municipality	(0.029)	(0.029)	(0.028)	(0.029)	(0.033)	(0.033)	(0.032)	(0.030)
Cluster: Municipality 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								
Municipality 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.5: 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.4. 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 municipality level and presented in parentheses.

Table A.6: Job transition for all workers

Occupation in 2009	Status in 2010				
	Same occupation as in 2009	Same municipality 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, municipality, micro-region and pharmacy in 2010, respectively. Column 5 shows the sample size for each occupation. Source: RAIS 2009 and 2010.

Table A.7: Merger-induced changes in concentration

Labor market definition:	HHI in 2011	Projected HHI	ΔHHI	%ΔHHI
	(1)	(2)	(3)	(4)
Panel A: Pharmacists				
Municipality X Occupation X All workers X Year	457	494	37	8.1%
Municipality X Occupation X All workers X Year X Pharmacies	529	623	94	17.7%
Municipality X Occupation X All workers X Year X Pharmacy chain	2,316	2,967	651	28.1%
Panel B: Salespeople				
Municipality X Occupation X All workers X Year	77	77	0	0.2%
Municipality X Occupation X All workers X Year X Pharmacies	433	488	55	12.8%
Municipality X Occupation X All workers X Year X Pharmacy chain	2,269	2,870	601	26.5%
Municipality X Occupation X Newly hired workers X Year	91	91	0	0.3%
Municipality X Occupation X Newly hired workers X Year X Pharmacies	528	621	93	17.6%
Municipality X Occupation X Newly hired workers X Year X Pharmacy chain	2,364	2,927	563	23.8%
Municipality 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				
Municipality X Occupation X All workers X Year	632	755	123	19.5%
Municipality X Occupation X All workers X Year X Pharmacies	935	1,206	271	29.0%
Municipality 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 municipalities—i.e., municipalities in which merging firms overlapped in 2011. Projected changes in the HHI in control municipalities 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 municipality. 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.8: 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 municipalities 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 municipalities from the sample of merging firms. Source: RAIS 2007-2018.

Table A.9: Alternative specification: AKM 2007-2011

	Status in 2010				
Occupation in 2009	Same occupation as in 2009	Same municipality 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, municipality, micro-region and pharmacy in 2010, respectively. Column 5 shows the sample size for each occupation. Source: RAIS 2009 and 2010.