A Retrospective Analysis of the Acquisition of Target's Pharmacy Business by CVS Health: Labor Market Perspective

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Abstract

There has been recent compelling empirical evidence of the rising employer monopsony power. A robust negative relationship between employer concentration and wages has been established in the literature. One significant source of higher labor market concentration is merger and acquisition activity between employers in the same labor market. In this paper, using difference-in-differences regression, we analyze the effect of the acquisition of Target's pharmacy business by CVS Health that took place in December 2015 on annual salary. In addition, we test for heterogeneous merger effects based on occupation, namely pharmacists, pharmacy technicians, and retail salespeople. We also test for differential wage effects for employees working at the merging firms compared to those working for other employers in the same labor market. We estimate the average merger effect to be a salary reduction of 5.5%. Out of the three occupations of interest, pharmacy technicians are the most harmed by the merger as they face a wage reduction of 12%. Employees working for merging firms experienced an additional decline in their wages by 7.4% compared to those who work for other firms in the same market.

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1 INTRODUCTION

Until recently, the starting point for labor economics was the assumption of perfect competition, hence wages are equal to the marginal product of labor. Even if firms have market power in the product market, the analysis would follow the assumption that they face perfect competition in the labor market. It was not until the 1990s that scholars provided both theoretical and empirical evidence supporting the existence of labor market monopsony (Card, 2022). For instance, a key implication of the dynamic monopsony model with a job ladder presented by Manning (2003) is that all workers experience a wage markdown in equilibrium. He showed that only in a special case, workers could receive their full marginal product, implying that perfect competition in the labor market is an exception rather than the norm. Further, several empirical studies showed that labor supply elasticity is finite and low in some markets —another clear contradiction to the perfect labor markets assumption (Azar, Berry, & Marinescu, 2022; Azar et al., 2019; Sokolova & Sorensen, 2021).

The prevalence of micro-level data covering labor markets, such as survey data, online job vacancies data, and employer-employee matched datasets, aided the development of the empirical literature studying employers' monopsony power in the labor market. In 2016, using data covering the near-universe of online job postings in the U.S. economy, the average local labor market had a Herfindahl Hirschman Index (HHI) of 4378, equivalent to a market where only 2.3 recruiting firms with equal market shares of the total number of vacancies.[1] In addition, around two-thirds of the local labor markets were found to be highly concentrated (Azar et al., 2020).[2] Another paper showed that one in every six

[1]Local labor market was defined as the intersection between the occupation by six-digit SOC code and commuting zone at the quarterly level of 2016 data.

[2]These local labor markets had an HHI level of above 2500. According to the Department of Justice / Federal Trade Commission 2010 horizontal merger guidelines, a market with an HHI level above 2500 is considered a *highly* concentrated market.

workers in the U.S. economy in 2019 faced a wage reduction of at least 2% due to high labor market concentration (Schubert et al., 2021). Several other papers found evidence of a robust negative relationship between labor market concentration, measured by HHI, and wages (Arnold, 2019; Azar, Marinescu, & Steinbaum, 2022; Azar et al., 2020; Benmelech et al., 2022; Macaluso et al., 2019; Prager & Schmitt, 2021; Rinz, 2022; Schubert et al., 2021; Thoresson, 2021).

One source of higher labor market concentration is mergers and acquisitions between employers in the same labor market. Yearly, nearly 2% of workers work in establishments that engage in merger or acquisition activity (Arnold, 2019). The retail pharmacy industry is one of the sectors that experienced significant consolidation over the past decades. Since the 1980s, there has been a wave of mergers and acquisitions between several chain pharmacies in the United States that changed the market structure of the industry significantly (Zhu, Hilsenrath, et al., 2015). Recent merger retrospective studies provided evidence of wage reduction following merger as a result of higher employer concentration (Arnold, 2019; Benmelech et al., 2022; Guanziroli, 2022; Prager & Schmitt, 2021; Thoresson, 2021). Specifically, Prager and Schmitt (2021) found that nurses and pharmacists working in hospitals located in commuting zones that experienced a merger or acquisition between hospitals during 2000–2010 experienced wage reductions of about 6.8%.

To the best of our knowledge, there has not been any formal study that estimates the effect of mergers in the retail pharmacy industry in the United States on wages. In this paper, we aim to fill this gap by studying a single merger between two large national retail pharmacy chains in the United States. Our research objective is, first, to test if the merger has resulted in wage reductions. Second, whether the wage reductions, if any, differ by occupation —namely the pharmacists, pharmacy technicians, and salespeople occupations— and by employer affiliation to the merger —merging firms versus other firms.

We specifically study the acquisition of Target's pharmacy business by CVS Health that took place in 2015.[3] On December 16, 2015, CVS Health Corporation and Target Corporation announced the conclusion of CVS' acquisition of 1672 Target pharmacies existing in 47 states. Those 1672 pharmacies were accordingly operated through a store-within-a-store format, branded as CVS/pharmacy (Target, 2015). CVS Health and Target's pharmacies are two large national retail pharmacy chains in the United States. The acquisition deal took place at the national level, but it affected local areas differently. There are some commuting zones where both chains existed before the merger and others where only one of the chains had at least one establishment. We exploit this feature to give rise to a quasi-experiment research design.

We use vacancy-level data by Burning Glass Technologies (BGT) covering the near-universe of online job postings in the U.S. economy.[4] Our dataset covers the period 2010–2022, which covers a period of 5 years preceding the merger and 7 years following it. Using the BGT data, we construct a difference-in-differences (DiD) model in which the treated commuting zones are defined as those where we could observe at least one vacancy for both Target and CVS during the period January 1, 2015 – December 15, 2015, and other commuting zones are defined as the control group. By this criterion, there are 234 commuting zones in the treatment group and 475 commuting zones in the control group.

The DiD model allows the comparison of annual salary before and after the merger between the treated and control commuting zones. This should help identify the average merger effect on annual salaries. In addition, we test for heterogeneous merger effects based on two aspects. The first aspect is occupation; whether the merger effect, if any, varies by occupation. For this purpose, we define a specification that includes interaction

[3]We sometimes refer to this acquisition as a "merger" throughout the paper even though we are aware of the significant legal difference between a merger and an acquisition. In the industrial organization literature, the word "merger" is used in a broader sense to refer to merger and acquisition activity.

[4]Using online vacancies data has become more prevalent recently in studying monopsony in labor markets (e.g., Acemoglu et al., 2022; Callaci et al., 2023; Clemens et al., 2021; Forsythe et al., 2020; Hershbein & Kahn, 2018; Macaluso et al., 2019). To the best of our knowledge, this is the first paper to employ BGT vacancy-level data to conduct a merger retrospective analysis.

terms between the post-treatment indicator and three dummy variables for three specific occupations, namely pharmacists, pharmacy technicians, and retail salespeople occupations. The rationale behind choosing those three occupations is to have a mix of high-wage and low-wage occupations with varying skill and education requirements. There is no consensus in the literature as to whether high-paid or low-paid workers are more susceptible to wage reductions by employers with monopsony power. It is reasonable to argue that low-paid occupations usually require a low occupation-specific skill set. Thus, low-paid workers have wider labor markets since they have more outside job options, which implies that they are less prone to employer monopsony power.

Prager and Schmitt (2021) found evidence supporting this argument, but Azar et al. (2020) and Macaluso et al. (2019) could not identify a relationship between an occupation's skill level and degree of labor market concentration. Surprisingly, Guanziroli (2022) found that salespeople experienced a larger wage reduction than pharmacists after a merger of two large retail pharmacy chains in Brazil. Other research demonstrated that the effect of higher labor market concentration on wages depends on the availability of equally comparable outside job options. The authors showed that both pharmacists (a high-wage occupation) and pharmacy technicians (a low-wage occupation) are at the top of the list of occupations with most workers adversely affected by higher employer concentration since both occupations have limited outside job options (Schubert et al., 2021). To contribute to this unresolved question, we estimate the employer consolidation effect through merger on the annual salary of pharmacists (a high-wage occupation with limited outside job options), pharmacy technicians (a low-wage occupation with limited outside job options), and salespeople (a low-wage occupation with broad outside job options).

The second aspect explores heterogeneity based on employer affiliation to the merger. In other words, whether the merger effect on the annual salary of those who work for Target or CVS differs from that of those who work for other employers in the same local labor market. To test this claim, we define another specification that interacts the post-treatment indicator with a dummy variable that takes the value one if the vacancy is posted by CVS or Target and the value zero otherwise. Additionally, we study the interaction between the two aspects of merger effect heterogeneity, aiming to investigate whether the salaries of pharmacists, pharmacy technicians, and retail salespeople working for the merging firms were affected differently compared to other workers employed at the merging firms as well.

Our estimates indicate a negative effect of the merger on annual salary. Employees working in treated commuting zones experienced a decline in their annual salary over the 7 years following the merger by 5.5%, compared to those who work in commuting zones unaffected by the merger. In addition, results show that the average treatment effect of the merger differs by occupation. Of the three analyzed occupations, pharmacy technicians were the most harmed by the merger compared to pharmacists and retail salespeople. Pharmacy technicians experienced an additional salary reduction of nearly 6.5%, resulting in a total salary reduction of 12%. This suggests that employees working in a low-wage occupation with limited outside job options are the most likely to experience a decline in their salaries as a result of employer consolidation. Moreover, the average merger effect on annual salary also differs by employer affiliation to the merger. The wages of employees working for Target or CVS in the treated commuting zones declined by an additional 7.4% over the 7 years following the merger, compared to those who work for other firms. Thus, employees working for the merging firms had their wages reduced by an average of 12%.

The contribution of this paper is threefold. First, it adds to the merger retrospective analysis literature focusing on labor markets. Second, it provides supporting evidence to the strand of literature that investigates the relationship between labor market concentration and wages. Third, it contributes to the inconclusive literature on the differential wage effect of higher employer monopsony power; more specifically, whether the wage effects depend on an occupation's wage rank or probability of job mobility.

The rest of the paper is organized as follows. Section 2 presents the relevant literature. Section 3 describes the data used for the empirical analysis and shows some summary statistics. Section 4 explains the methodology we used to answer our research questions. Section 5 presents the difference-in-differences regression results. Section 6 discusses the implications of the findings on labor markets. Finally, Section 7 concludes.

2 RELATED LITERATURE

Our interest in studying the effects of merger-induced employer concentration on wages stems from the literature highlighting employers' monopsony power and lack of competition in local labor markets. In a perfectly competitive labor market, the elasticity of the labor supply curve facing an individual firm should be infinite. However, recent empirical literature provides evidence of low levels of residual labor supply elasticity, which implies a wage-setting power for employers. Azar et al. (2019) used job posting data from CareerBuilder.com to estimate the firm-level elasticity of job applications to changes in posted wages. Although labor supply elasticity is expected to be higher in populated areas, the authors estimated firm-level application elasticity to be only 4 in the most densely-populated labor markets. Manning (2003) illustrated that in a dynamic monopsony model with a job ladder, the elasticity of quits and the elasticity of recruits are equal. Therefore, labor supply elasticity can be estimated as twice the value of either elasticity. Azar, Berry, and Marinescu (2022) estimated labor supply elasticity as twice the value of recruitment elasticity. According to their estimate, labor supply elasticity was 4.79, suggesting a little over 20% wage markdown below the competitive wage. Several other papers provided widely varying estimates for the labor supply elasticity. Sokolova and Sorensen (2021) conducted a meta-analysis of this literature by employing 1320 estimates from 53 different studies. They came up with a best-practice estimate of 7.1, implying a 12% wage markdown below the marginal product of labor.

This paper relates to three strands of the empirical literature. First, it adds to the recently-evolving literature that studies mergers retrospectively to estimate the effects of employer consolidation on labor market outcomes. Arnold (2019) used a matched difference-in-differences strategy to analyze the wage and employment effects of mergers taking place between 1999 and 2009 in the United States. He found that the extent to which wages are affected by mergers depended on the change in the level of concentration in the labor market. Employees working in firms that are directly affected by the merger or acquisition experience a 2% reduction in their wage if the merger significantly increases labor market concentration. The effects are larger when mergers take place in already concentrated markets. On the market level, wages declined postmerger only for the top ventile merger-induced concentration changes. When concentration increased by 10%, market-level wages dropped by 2.2%, using the top ventile merger-induced concentration change as an instrument for concentration. As for employment levels, there is evidence that employment level falls by 13.4% in merging entities.

Thoresson (2021) exploited the regulatory reform of the Swedish pharmacy market in 2009 that ended the government monopoly of the retail pharmacy market to study the impact of changes in labor market concentration on wages. Following deregulation, around two-thirds of the monopoly pharmacies were privatized and private firms were allowed to open new pharmacies. As a result, the average HHI in the pharmacy market, weighted by employment in each local labor market, dropped from 1 (pre-regulation) to a little over 0.25 in 2016. This decline in HHI varied across commuting zones in Sweden, which enabled the calculation of the elasticity of wages to changes in HHI using a difference-in-differences model with varying treatment intensities. The author focused only on the concentration change caused by the privatization of already existing pharmacies to define her treatment. Wages increased by 2.5% to 6% for a local labor market that moved from the 75^{th} to the 25^{th} percentile of the labor market concentration distribution.[5] There was also a positive employment effect such that employment increased by 10%. Using the employer-employee matched dataset, the author estimated differential wage effects for incumbent workers, who

[5] Thoresson (2021) defines local labor markets as the intersection between the industry of dispensing chemists and commuting zones in Sweden.

she called *stayers*, versus newly hired workers. Stayers experienced positive significant wage gains following a reduction in labor market concentration, whereas the effect for the newly hired was not statistically significant. Further, the positive wage effects were larger for more experienced workers.

Prager and Schmitt (2021) employed a difference-in-differences methodology to study the impact of 84 mergers among hospitals between 2000 and 2010 in the United States on the wages of three sets of employees: pharmacists and nurses, skilled workers, and unskilled workers. The authors compared wages in commuting zones that experienced hospital mergers between 2000 and 2010 to commuting zones with no hospital merger activity within the same time frame. They found no evidence of wage reductions for unskilled workers. However, for the other two labor categories, wages declined only when the concentration increase induced by the merger was large (as in Arnold, 2019). For the top quartile of concentration-increasing mergers, wages decreased by 4% for skilled non-health professionals and 6.8% for nurses and pharmacists, over the 4 years post-merger. Further, they found that high levels of unionization and a pro-union environment seem to mitigate wage reductions.

Guanziroli (2022) also conducted a merger retrospective study, in which he estimated the labor market effects of a merger between two large retail pharmacy chains in Brazil. This paper adopted a difference-in-differences methodology to compare the wages and labor composition of pharmacists and salespeople in counties where both chains existed to counties where only one chain existed. The author utilized the Brazilian employer-employee matched dataset to add worker and establishment fixed effects to the DiD model. Thus, the post-treatment indicator coefficient would capture the wage effect of the change in labor market concentration induced by the merger, excluding observable and unobservable changes in labor force composition. This specification produced interesting findings. Without accounting for labor composition, the pharmacists' wage dropped by 7.9%, while that of salespeople only fell by 0.7%. When firm and worker fixed effects were included, the wages of pharmacists only dropped by 2.6% and that of salespeople fell by 3.5%. Similar to Prager and Schmitt (2021), it was found that pharmacists' wage reductions were small owing to high unionization rates among pharmacists. However, wages of newly hired pharmacists were lower, consistent with Thoresson (2021). A possible explanation is that newly hired workers are not covered by union agreements. Moreover, the merger resulted in lower worker fixed effects. In other words, following the merger, the merged entity hired pharmacists of lower fixed effects, who tend to accept lower wages.

Second, this paper relates to the rich literature that studies the relationship between changes in local labor market concentration and wages. Numerous researchers investigated this relationship by regressing market wages on the local level of HHI, as an indicator for labor market concentration (e.g., Arnold, 2019; Azar, Marinescu, & Steinbaum, 2022; Azar et al., 2020; Benmelech et al., 2022; Macaluso et al., 2019; Prager & Schmitt, 2021; Rinz, 2022; Schubert et al., 2021; Thoresson, 2021). A large share of this literature relies on online job vacancies data, such as CareerBuilder.com and BGT datasets, to compute HHI based on vacancy shares as a proxy for employment shares (Azar, Marinescu, & Steinbaum, 2022; Azar et al., 2020; Macaluso et al., 2019; Schubert et al., 2021). A robust negative relationship between local labor market concentration and the posted vacancy-level salary or average hourly earnings has been documented (see Azar, Marinescu, & Steinbaum, 2022; Azar et al., 2020; Macaluso et al., 2019; Rinz, 2022; Schubert et al., 2021; Thoresson, 2021). [6]

A key threat to identification for these studies is the possible endogeneity of variations in local labor market concentration. Hence, some papers used mergers as an instrument for variations in concentration (Arnold, 2019; Benmelech et al., 2022). Arnold (2019)

[6]The local labor market definition slightly differs between those papers. Macaluso et al. (2019) defines the labor market as the pair of four-digit SOC occupation by metro area for each year. Azar et al. (2020) and Azar, Marinescu, and Steinbaum(2022) use the intersection between six-digit SOC occupation and commuting zone for each year-quarter. Schubert et al.(2021) uses the six-digit SOC occupation by metro area for each year. Rinz(2022) defined labor market as the intersection of four-digit NAICS industry code and commuting zones.

found that when labor market concentration increases by 10%, market-level wages decline by 2.2%, using data covering all industries, but it declines by 3% to 6%, according to Benmelech et al. (2022), using data for the manufacturing sector. Consistent with Prager and Schmitt (2021) and Guanziroli (2022), Benmelech et al. (2022) provided evidence of the role of unions in countervailing employer monopsony power. In industries with almost zero unionization rates, the elasticity between wages and local HHI was nearly -0.015. In contrast, the elasticity in industries with the average unionization rate (19.8%) drops by a range of 29% and 45%.

To address the endogeneity problem, Azar, Marinescu, and Steinbaum (2022) used the average of the log of the inverse of the number of firms in the market in other commuting zones for the same occupation and time period as an instrument for HHI. They argued that this instrument is less likely to be endogenous since it does not depend on vacancy shares. Such instrument captures changes in market concentration caused by national changes in occupation rather than variations in a particular local labor market. The authors also control for labor market tightness. The estimated elasticity of market-level wage to changes in HHI was -0.14. Analogously, Rinz (2022) used the average HHI across other commuting zones for the same industry in the same year as an instrument for the HHI in each labor market. The estimated elasticity was between -0.03 and -0.05, depending on the earnings variable used. Schubert et al. (2021) tackled the endogeneity concern differently. They started with the fact that the level of local labor market concentration in a given market depends on the growth rate of firm-level vacancies relative to the overall vacancy growth in that labor market. Thus, they instrumented for the market-specific firm-level vacancy growth rate with the firm-level vacancy growth rate for each occupation at the national level, leaving out the geographic area in question. This empirical strategy yields a wage elasticity to changes in local labor market concentration ranging between -0.015 and -0.02, depending on the specification.

Third, the literature is not quite clear whether low-wage workers are less prone to employer monopsony power compared to high-wage workers. Macaluso et al. (2019) found a low correlation coefficient, nearly 0.06, between the average skill level of an occupation and the average labor market concentration, measured by the HHI. The authors ran a set of unconditional regressions, where they regressed the firm-market-year level of HHI on 22 occupation dummies defined as per the two-digit Standard Occupational Classification (SOC) codes. There was no evidence of systematic patterns between an occupation's skill level and average labor market concentration. Similarly, Azar et al. (2020) showed that there is a weak to no relationship between the local labor market concentration and occupations' rank, whether ranked by level of earnings or education. In other words, there was no evidence suggesting that workers in low-wage occupations or in occupations requiring less education face low levels of employer concentration, and vice versa.

In contrast, Prager and Schmitt (2021) provided evidence of wage growth differentials based on the skill level of workers in the hospital industry. Wages did not decline post-merger for unskilled workers, such as cafeteria workers and janitors, implying that this set of workers does face a broader labor market. However, wages did drop for skilled workers and more specialized workers, namely nurses and pharmacists. Guanziroli (2022) showed that following a merger between two large pharmacy chains in Brazil, the wages of salespeople declined more than that of pharmacists. It was hypothesized that salespeople should not be affected by a pharmacy merger since they can simply move to the retail industry, for instance. Using the employer-employee matched dataset, the author demonstrated that one in every four salespeople who was working in a pharmacy and switched jobs still worked in a pharmacy the following year. Thus, the labor market for salespeople working in a drugstore could be narrower than previously thought.

Schubert et al. (2021) studied the issue of labor market definition more formally using highly granular occupation mobility data covering 16 million U.S. workers' resumes. The paper concluded that the extent to which wages respond to changes in labor market concentration depends on the availability of outside job options. Workers who are more likely to find comparably good jobs in other occupations are less prone to wage reductions resulting from employer monopsony power, regardless of the occupation's skill level or average wage rank. The authors presented a list of the 25 occupations that are the most negatively affected by employer consolidation. At the top of the list, there are high-wage occupations such as registered nurses and pharmacists, and low-wage occupations such as security guards, hair stylists, and pharmacy technicians.

3 DATA

We use micro-data from Burning Glass Technologies (BGT). BGT uses artificial intelligence methods to sweep online job postings from nearly 40,000 websites, hence, capturing the near-universe of online job vacancies. Because a job ad can be posted on multiple online platforms, BGT cleans the vacancy data to remove duplicates to have a unique observation for each vacancy. This dataset covers almost 80% of total job advertisements in the U.S. economy (Macaluso et al., 2019). In addition, the occupational distribution of vacancies in the BGT data was found indistinguishable from that in the Occupational Employment Statistics (Hershbein & Kahn, 2018).

A recent study conducted by the Organization for Economic Cooperation and Development (OECD) showed that BGT data is statistically representative of the labor market in the United States during the period 2010–2019. The authors found that BGT data is a reliable source of up-to-date insights about labor market demand. This finding is especially true for jobs where the hiring procedures typically occur in an online setting. Accordingly, occupations that tend to be recruited mainly offline are often underrepresented in the BGT dataset. Further, the study revealed that the representation of some occupations changes over time when compared to official employment statistics (Cammeraat & Squicciarini, 2021).

The unit of observation in BGT data is an online vacancy. We have access to approximately 374 million job vacancies from 2010 to 2022. The data contain multiple variables summarizing most of the information mentioned in a job listing, such as listing date, occupation, job title, location, employer name, four-digit and six-digit North American Industry Classification System (NAICS) codes, education requirements, and salary. Our variables of interest are job posting date, six-digit SOC code, Federal Information Processing System (FIPS) code —used to match each vacancy to the respective commuting zone based on the United States Department of Agriculture (USDA) commuting zone delineation in 2000— employer's name, and posted vacancy-level salary. The BGT dataset reports salaries in the form of a range. For each vacancy with salary information, lower and upper salary bounds are given. We use the midpoint between those bounds to define our salary variable.[7]

One downside of the BGT dataset is that only 24% of the vacancies between 2010 and 2022 have salary information. As a result, our sample size is reduced to approximately 90 million vacancies. Our empirical strategy –explained thoroughly in Section 4– relies primarily on the location of job ads. Hence, we drop vacancies with missing FIPS codes since we cannot accurately determine their respective commuting zones. The merger occurred in December 2015. Thus, we have data covering 5 years pre-merger and 7 years post-merger. We use the employer name variable to tag large retail pharmacy chains, such as CVS, Target, Walgreens, Rite Aid, Albertsons, Kroger, Walmart, Costco, and Publix. It is worth noting that some of those chains carry different store banners around the United States.[8]

The primary goal of this paper is to investigate the effects of a merger between two large retail pharmacy chains on the annual salary in the United States. Therefore, we use

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[7] That salary variable is winsorized at the 1^{st} and the 99^{th} percentile by year and 6-digit SOC code to remove outliers.

[8] For instance, the Albertsons chain carries the following banners: Acme, Albertsons, Amigos, Andronico's, Balducci's, Carrs, Haggen, Jewel Osco, King's, Market Street, Pavilions, Randalls, Safeway, Shaw's, Star Market, Tom Thumb, United Supermarkets, and Vons. The Kroger chain carries several store banners as well, such as Bakers, City Market, Dillons, Food4Less, Fred Meyer, Fry's, Harris Teeter, JayC, King Soopers, Kroger, Mariano's, Metro Market, Payless Super Markets, Pick n Save, QFC, Ralphs, Ruler Foods, and Smith's.

the four-digit NAICS code variable in our dataset to restrict the sample to only include vacancies from specific retail industries, namely the food and beverage retailers, health and personal care retailers, and other general merchandise stores including department stores and warehouse clubs. The motivation behind this restriction is the fact that retail pharmacists cannot easily move to the general medical and surgical hospital industry. In other words, retail pharmacists facing rising employer monopsony power cannot easily switch jobs to become hospital pharmacists, also known as clinical pharmacists.

One of the main barriers to the switch from being a retail pharmacist to a hospital pharmacist is the residency training requirement. Hospitals usually require at least one year of residency training before hiring clinical pharmacists. To overcome this barrier, a retail pharmacist should seek board certifications, some of which require a minimum of 4 years of applicable experience to be eligible to sit for a board exam (Phan, 2021). Furthermore, the day-to-day duties of a retail pharmacist differ from those of a clinical pharmacist. According to the 2019 national pharmacist workforce study, the most common tasks conducted by community pharmacists –pharmacists who work in independent pharmacies, chain pharmacies, mass merchandisers, supermarkets, or health system retail– were administering vaccines, providing patient medication assistance, dispensing Naloxone, and providing medication therapy management. On the other hand, the three most common services provided by pharmacists working in hospitals were drug level monitoring, therapeutic drug interchange, and ordering laboratory tests (Arya et al., 2020).

The Occupational Employment and Wage Statistics (OEWS) published by the Bureau of Labor Statistics (BLS) show that there are considerable variations in the occupational wage trends across industries for the pharmacy technicians and retail salespeople occupations. Figures 1 and 2 show that the average annual salary of pharmacy technicians and retail salespeople working in the general medical and surgical hospitals industry is consistently higher than the average annual salary of those employed in other retail industries. If labor mobility is easy and feasible across industries for pharmacy technicians and retail salespeople, we should not observe this deviation between the hospital industry and the other retail industries presented in the figures. Therefore, Figures 1 and 2 provide circumstantial evidence suggesting that pharmacy technicians and retail salespeople job vacancies in the hospital industry are not substitutes for similar vacancies posted by food and beverage retailers, health and personal care retailers, and general merchandise retailers.

Henceforth, we choose to restrict our sample to only include the job vacancies posted within the following industries: food and beverage retailers, health and personal care retailers, and general merchandise retailers. This gives rise to a sample size of approximately 1.4 million vacancies between 2010 and 2022. Because our dataset is entirely comprised of job vacancies, it goes without saying that the effect of the CVS–Target merger on annual salary estimated in this paper is primarily about newly hired workers. If the purpose is to distinguish between the merger effect on the salaries of existing workers and new hires, one should use employer-employee matched datasets, similar to Guanziroli (2022) and Thoresson (2021). Throughout the rest of this section, we present the relevant statistics related to our outcome variable, the posted annual salary, to get a better sense of the data. Then, we lay out the structure of the local labor markets for pharmacists, pharmacy technicians, and retail salespeople during the period 2015Q1–Q3.

3.1 Annual Salary by Occupation

Figure 3 presents the average annual salary for the three occupations of interest over the sample period using the BGT dataset and the annual occupational wages published by the BLS.[9] As per Figure 3a, the BGT data series shows a downward trend for the pharmacists' annual salary, whereas the BLS series depicts a slightly upward trend. The BGT series shows that the average annual salary for pharmacists is exceptionally low in 2020 and

[9]For each of the three occupations, the BLS data series is estimated by computing the average of the annual mean wage for the following industries: health and personal care retailers, food and beverage retailers, and general merchandise retailers.

2022. The averages are \$70,482 and \$51,713, respectively. In 2020, almost 50% of the job vacancies whose posted salary is below \$70,482 have unrecognizable or missing employer names. However, in 2022, 89% of pharmacists' job ads in which the posted salary is below average are posted by CVS Health.

As for pharmacy technicians, Figure 3b depicts an upward trend for both series. Over the sample period, the average annual growth rate of the annual salary of pharmacy technicians is 3% for both series. Figure 3c reflects a significant deviation between the average annual wage estimated from the BGT dataset and that reported by BLS for retail salespeople during the period 2010–2015. As previously mentioned, the BGT dataset under-represents occupations that are not usually recruited in an online setting. A possible explanation for the exceptionally high average annual salary of retail salespeople before 2016 is that high-ranked job positions within the retail salespeople occupation were more likely to be posted online. That is why the average annual salary is higher than the average published by BLS. However, as time goes by, the online job vacancies pool becomes more representative of the occupation, and thus both series become closer. This is consistent with Cammeraat and Squicciarini (2021) who found that the representativeness of the occupational group that includes service and sales workers gets better over time in the BGT dataset covering the United States.

Over the sample period, the average annual salary varies greatly by employer. Table 1 lists the average annual salary in USD by occupation for nine of the largest retail pharmacy chains in the United States. For pharmacists, the highest paying chain is Costco, which pays around \$135,000 per year on average, and the lowest is CVS, paying \$27,345 per year on average. The latter anomalous average raises a lot of questions. This unusually low average annual salary for pharmacists hired by CVS is influenced by the averages in 2015, 2020, and 2022. The average annual salary for pharmacists hired by CVS excluding those years is \$118,355. In 2015, there were only two pharmacist vacancies posted by CVS that had salary information in which the average salary was \$22,880. It is highly likely that there is some sort of data collection error. There were 55 pharmacist job ads posted by CVS with wage information in 2020 where the average salary was $63,496 and the median salary was $52,250. CVS posted 8470 pharmacist job vacancies with salary information in 2022. The average salary for those vacancies was $24,833 and the 90^{th} percentile was $17,914.

To explore that aberration further, we look at the distribution of the 2022 pharmacist vacancies posted by CVS by job title in Table 2. The distribution of the annual salary looks abnormally low for the pharmacist, pharmacy manager, and staff pharmacist job titles. Those three job titles comprise 96% of the observations in 2022, hence driving the mean salary to an exceptionally low level. There are two possible explanations for the distributions presented in Table 2. First, some non-pharmacist CVS vacancies are being misassigned into the pharmacist occupation, especially that of staff pharmacist. Second, the BGT team could be using imprecise methods for imputing the salaries of vacancies without salary information. The rationale behind the latter explanation is that over the years, the maximum proportion of CVS pharmacists' job ads that include salary information was 3%. In 2022, that proportion soared to 50%.

The second column of Table 1 lists the average annual salary of pharmacy technicians by chain. The highest-paying chain is Rite Aid and the lowest is Publix. Rite Aid's high average annual salary is mainly driven by high salaries in 2014, 2015, and 2022. There was only one job vacancy in 2014 with an annual salary of $87,500; four vacancies in 2015 where the average annual salary was $49,510; and 134 vacancies with an average of $72,934. Costco pays the most for retail salespeople, with an average annual salary of $58,817 and Kroger pays the least, with an average of $27,791 per year.

One of the main characteristics of the CVS–Target merger is that both chains had pharmacies competing together in some commuting zones before the merger, whereas other commuting zones had only one chain. The commuting zones where both chains existed in 2015 are the treated commuting zones and other commuting zones are control or untreated commuting zones –this is explained in detail in Section 4. Figure 4 explores the wage trends for each of the three occupations of interest in both the treated and untreated commuting zones.

At first glance, Figure 4a shows that pharmacists employed in untreated commuting zones suffered tremendous wage reductions in 2020. Table 3 thoroughly looks into the distribution of the pharmacists' annual salary in 2020 working in both treated and untreated commuting zones. The table shows that the salaries for the pharmacists' vacancies posted in untreated commuting zones are questionable. The recorded annual salary was $37,752 for 1,191 out of the 1,353 vacancies. Half of those 1,191 observations were vacancies with unrecognizable or missing employer names and none of those vacancies was posted by any of the well-known retail pharmacy chains. As for the 162 vacancies whose recorded salary was not $37,752, their average salary is $95,148 —7.4% higher than the average salary of pharmacists employed in treated commuting zones. Of those 162 vacancies, 25 were posted by Walgreens, 10 were posted by CVS, and 4 were posted by Walmart. Therefore, the average annual salary of pharmacists working in untreated commuting zones in 2020 should be cautiously considered.

The average annual salary of pharmacy technicians employed in treated commuting zones exceeded that of untreated commuting zones, but both series exhibited similar trends before 2015 as per Figure 4b. After the merger took place, the wages increased for both series until 2017. During the period 2018–2020, the wages for pharmacy technicians employed in untreated commuting zones rose above that of those working in treated commuting zones. Figure 4c shows that the average annual salary of retail salespeople working in treated commuting zones was generally at least as high as that of those working in untreated commuting zones over the sample period, except for 2010. The average annual salary of retail salespeople working in treated commuting zones in 2010 is astonishingly high, suggesting that it is greatly affected by large outliers. Table 4 presents the distribution of salaries posted in the 2010 retail salespeople's vacancies classified by commuting zone-based treatment. It is apparent that the above median salaries for the untreated commuting zones are extremely inflated. The most reasonable explanation for this distribution is assigning vacancies to the wrong six-digit SOC code.

3.2 Labor Market Concentration

There has been a lot of consolidation in the retail pharmacy industry in the United States, especially among large retail pharmacy chains. To get a better sense of the market structure before the CVS–Target merger, we calculate the market shares of both parties in addition to some other large retail pharmacy chains. For the purpose of calculating market shares, we define local labor markets as the combination of the six-digit SOC occupation code and commuting zone for each quarter in our sample period, similar to Azar, Marinescu, and Steinbaum (2022) and Azar et al. (2019, 2020). When an individual considers changing their job, they usually consider other jobs within the same occupation. Some might even argue that individuals would not view all jobs within an occupation as substitutes but rather within the same job title, which implies using job titles instead of the broader occupation to define a labor market. Azar et al. (2020) formally proved that a labor market defined at the six-digit SOC occupation code is well-defined based on the "*small but significant non-transitory reduction in wages*" (SSNRW), and it is actually a conservative way of defining labor markets in the United States.[10]

The geography of a labor market is bound by the location of work and employees' place of residence. Hence, a local labor market should be defined based on the commuting patterns of employees (Tolbert & Sizer, 1996). That is why commuting zones are the best way to define the geographical aspect of a local labor market. Counties and Metropolitan Statistical Areas (MSAs) were previously used to delineate a local labor market, but both

[10]The small but significant non-transitory reduction in wages (SSNRW) test is analogous to the small but significant non-transitory increase in prices (SSNIP) test that is commonly used in the antitrust literature and practice to define a market.

options are not quite representative of a labor market. By definition, labor markets defined by counties are restricted by a county's boundaries, failing to account for labor movement between counties and even between states in some cases. As for MSAs, the main drawback is the exclusion of rural areas.

Using the aforementioned labor market definition, we calculate the vacancy shares as a proxy for market shares for nine of the largest national retail pharmacy chains in the United States. Market shares are calculated based on the number of posted vacancies by each chain relative to the total number of vacancies in each local labor market. Table 5 presents the average market shares of those chains for the labor markets for pharmacists, pharmacy technicians, and retail salespeople. The shares are averaged across the first three quarters of 2015 —i.e., before the acquisition of Target's pharmacies by CVS in December 2015. Among those retail pharmacy chains, in 2015, CVS was the second largest employer of pharmacists, after Walmart, and the largest employer of pharmacy technicians, with a market share of nearly 40%. On average, Target held a 3.4% share of the pharmacists' labor markets, a 2.7% share of the pharmacy technicians' labor markets, and nearly 3% of the retail salespeople's labor markets. Those 9 chains collectively controlled around 53% of the pharmacists' labor markets, 60% of the pharmacy technicians' markets, and 13% of the retail salespeople markets.

According to the U.S. Department of Justice and the Federal Trade Commission horizontal merger guidelines, mergers that result in an HHI increase of 100 points or more are likely to cause adverse competitive effects and often require scrutiny (U.S. Department of Justice and the Federal Trade Commission, 2010). The change in HHI as a result of the merger is referred to as "delta". Delta is calculated as the change between the post-merger HHI and pre-merger HHI. The CVS–Target merger increased HHI by more than 100 points in 1,829 local labor markets, of which 313 markets are labor markets for pharmacists, 351 markets for pharmacy technicians, and 123 markets for retail salespeople.

4 METHODOLOGY

To estimate the effect of CVS acquisition of Target's pharmacy business, we employ a difference-in-differences research design. We aim to compare the average annual salary before and after the merger between treated and control labor markets. We adapt the empirical strategy followed by Prager and Schmitt (2021) to answer our research question given the data at our disposal.

An interesting feature of the CVS–Target merger is that both parties already existed in some commuting zones but not in others. Accordingly, those commuting zones where both Target and CVS had at least one establishment before the merger experienced an increase in employer concentration, whereas the commuting zones where only one party or neither existed did not experience a change in employer concentration as a result of the merger. Using the employer's name and FIPS code variables available in the BGT dataset, we were able to get a list of commuting zones where either Target or CVS posted a vacancy during the period January 1, 2015–December 15, 2015.[11] The commuting zones for which we could observe at least one vacancy for both Target and CVS were defined as treated commuting zones and the others were considered control commuting zones. Using this measure, the treatment group included 234 commuting zones and the control group included 475 commuting zones.

We are also interested in studying the differential merger effects based on two aspects. First, whether the merger's wage effects differ by occupation, namely the pharmacists, pharmacy technicians, and retail salespeople occupations. Second, whether the merger's wage effect varies between the merging and non-merging parties in the same local labor market. To measure those differential effects, we interact the post-treatment indicator, first, with a dummy variable for each occupation and, second, with a dummy variable for being

[11] The acquisition was completed on December 16, 2015.

a merging party (Greenfield, 2014; Guanziroli, 2022).

Our baseline specification in this paper is the DiD model presented in equation 1.

$$ln(Salary_{ioect}) = \alpha_c + \gamma_{ot} + \mu_e + \beta Treat_c \times Post_t + \epsilon_{ioect} \tag{1}$$

The dependent variable is the log of the posted annual salary for vacancy i posted by the employer e that belongs to occupation o located in commuting zone c at time t, which is defined on a quarterly basis. We include two-way fixed effects, where α_c is the commuting zone fixed effects and γ_{ot} is the occupation-by-year-quarter fixed effects. The commuting zones' fixed effects are essential to control for wage variation across commuting zones due to factors unrelated to the merger. Occupation-by-time fixed effects control for occupational wage variation over time. We also add employer fixed effects, measured by μ_e, to control for employer-specific wage policies that could affect the observed salary. $Post_t$ is a dummy variable that takes the value one for observations after December 16, 2015. $Treat_c$ is another dummy variable that indicates whether the commuting zone for each observation is treated or not. Standard errors are two-way clustered by occupation and commuting zones.

We run another specification, see equation 2, that allows for differential merger effects by occupation through interacting the post-treatment indicator with three dummy variables, each representing either the pharmacist, pharmacy technician, or retail salespeople occupations.

$$\begin{aligned} ln(Salary_{ioect}) = \alpha_c + \gamma_{ot} + \mu_e + \beta Treat_c \times Post_t \\ +\delta_1 Treat_c \times Post_t \times Pharm_o + \delta_2 Treat_c \times Post_t \times Tech_o \\ +\delta_3 Treat_c \times Post_t \times Retail_o + \epsilon_{ioect} \end{aligned} \tag{2}$$

The dependent variable and fixed effects are the same as equation 1. $Pharm_o$ takes the value one for all the pharmacists' vacancies and zero otherwise. Similarly, $Tech_o$ takes the value one for pharmacy technicians' vacancies, and $Retail_o$ takes the value one for retail salespeople's vacancies. For instance, if δ_1 is negative and statistically significant, this means pharmacists' average annual salary is reduced more than that of other occupations.

The third specification is the one represented by equation 3. This specification is adapted from Guanziroli (2022). The purpose of this specification is to test for differential merger effects based on employer affiliation to the merger; whether they are one of the merging parties or not.

$$\begin{aligned} ln(Salary_{ioect}) = \alpha_c + \gamma_{ot} + \mu_e + \beta Treat_c \times Post_t \\ +\theta Treat_c \times Post_t \times Merging_e + \epsilon_{ioect} \end{aligned} \tag{3}$$

$Merging_e$ is a dummy variable that takes the value one if the vacancy *i* is posted by either Target or CVS and takes the value zero otherwise. If the coefficient θ is found to be statistically significant, then we would have evidence that the merger effect on the salaries of the employees working at the merging parties is different from those working for non-merging parties.

Lastly, we study the heterogeneous effects by occupation and merger affiliation together by interacting the post-treatment indicator with the dummy variable for employer affiliation to the merger and the occupation dummy variables, one at a time. This specification is presented in equation 4. If, for instance, λ_3 is negative and statistically significant, this means that retail salespeople working at either Target or CVS experience salary reductions post-merger compared to merging firms' workers employed in other occupations.

$$\begin{aligned} ln(Salary_{ioect}) = \alpha_c + \gamma_{ot} + \mu_e + \beta Treat_c \times Post_t \\ +\theta Treat_c \times Post_t \times Merging_e \\ +\lambda_1 Treat_c \times Post_t \times Merging_e \times Pharm_o \\ +\lambda_2 Treat_c \times Post_t \times Merging_e \times Tech_o \\ +\lambda_3 Treat_c \times Post_t \times Merging_e \times Retail_o + \epsilon_{ioect} \end{aligned} \tag{4}$$

5 RESULTS

To begin with, Table 6 shows the mean annual salary of all the vacancies in our sample stratified by commuting zone-based treatment and time of treatment. The standard deviation of annual salary is the value in parentheses. Before the merger, the average salary for the treated observations was $50,976 compared to $42,470 for the control observations. After the merger, the average salary of the treated observations dropped to $40,563 —a 20% decline— while that of control observations fell to $42,098 — only a 1% decrease. In other words, treated observations experienced 20% wage reduction post-merger, while wages of untreated observations declined by only 1%. Whether this significant wage reduction, 19 percentage points more, faced by treated observations post-treatment can be explained by the merger is what we aim to answer in the rest of this section.

It is worth highlighting that the number of observations in the treated group is much larger than that of the control group. This can be explained by the fact that Target stores are usually located in densely populated areas that tend to have strong economic activity, and hence more job postings (Bean, 2021). This is also true for CVS which lacked presence in rural areas as opposed to its significant presence in high-population areas as per a survey conducted in 2014 by Morning Consult — a business intelligence firm (Evans, 2014).

The rest of this section is divided into four subsections. The first subsection reveals the results of the baseline specification expressed by equation 1. The second subsection investigates whether the merger wage effect, if existing, varies by occupation. The third subsection tests for differential merger effect according to employer affiliation to the merger. The fourth and last subsection presents the results of the differential merger effect for pharmacists, pharmacy technicians, and retail salespeople based on their employer affiliation to the merger.

5.1 Baseline Model

Table 7 presents the estimation results of the baseline model with alternative fixed effects specifications. The specification reported in the 4^{th} column is the preferred one since it includes commuting-zone fixed effects, occupation-by-year-quarter fixed effects, and employer fixed effects. Accordingly, the estimated average treatment (merger) effect reflects the post-merger change in the average annual salary experienced by employees working in treated commuting zones after controlling for wage variation across commuting zones due to factors unrelated to the merger, quarterly wage trends for each occupation, and employer-specific wage policies. The estimated coefficient of the post-treatment indicator is negative and statistically significant at the 1% significance level. This coefficient indicates that the annual salary decreased as a result of the merger by approximately 5.5%, on average over the 7 years following the merger.[12] Put differently, assuming that the parallel trend assumption holds so that the control commuting zones constitute a valid counterfactual for what would have occurred in the treated commuting zones absent the merger, the merger reduced pay by 5.5%.

The aforementioned DiD estimate could be a biased estimate of the causal effect of the merger if the CVS–Target merger predominantly took place in markets that would have faced a decline in wage growth even without the occurrence of the merger. To make sure this is not the case, we examine the differential wage trends between treated and control commuting zones before and after the merger. Figure 5 plots the coefficients estimated using equation 1 after replacing the dummy variable $Post_t$ with lead dummies for the 10 quarters leading to the merger and lag dummies for the 28 quarters following the merger. The reference quarter for this estimation is the quarter preceding the merger —i.e., the third quarter of 2015. Each of the four sub-figures corresponds the different specifications

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[12]The dependent variable is in log form, so we exponentiate the coefficient for interpretation. Precisely, annual salary declined by $[e^{-0.0566} - 1] * 100 \approx -5.5\%$.

reported in the four columns of Table 7. There is no evidence of differential pre-merger wage trends regardless of the specification. Compared to control commuting zones, the average annual salary in the treated commuting zones started to steadily decline following the merger, and the negative effect magnified over time. This negative wage trend persists and intensifies during the COVID-19 pandemic.

5.2 Heterogeneous Effects by Occupation

As stated in Section 4, we interact the post-treatment indicator of the baseline specification with a dummy variable for pharmacists, pharmacy technicians, and salespeople to test for heterogeneous treatment effects for these three occupations. Table 8 reports the coefficients resulting from estimating equation 2 with alternative fixed effects specifications. According to the preferred specification depicted in column 4, the coefficient for the pharmacist occupation dummy is positive but statistically insignificant. The magnitude of the interaction coefficient for the pharmacist occupation is not large enough to completely counteract the average treatment effect estimated in Section 5.1. This means it is highly likely that pharmacists experienced a decline in their wages, but they were less negatively affected by the merger compared to other occupations. We would like to point out that there is a total of 475,059 pharmacist vacancies in our sample, of which only 34,696 observations had salary information. Further, as previously mentioned in Section 3.1, several pharmacists' observations had anomalous salary information. Therefore, this finding should be interpreted with some reservation.

The estimated coefficient of interacting the post-treatment indicator with the pharmacy technician occupation dummy is negative and statistically significant at the 1% significance level. Hence, there is evidence that pharmacy technicians are more harmed by this merger than other occupations. After the merger, technicians working in treated commuting zones experienced a salary reduction of nearly 12% — 5.5% due to the merger in general and an additional reduction of nearly 6.5% exclusive to the technicians' occupation.[13] This finding is consistent with Schubert et al. (2021), who concluded that the extent to which employees face wage reductions due to higher employer concentration depends on the availability of outside options. They found out that the pharmacy technician occupation is one of the top 25 occupations with most employees adversely affected by employer concentration due to the lack of comparably good outside job options, in other words, other occupations. As for retail salespeople, the interaction coefficient is positive and significant at the 1% significance level. However, its magnitude is not high enough to counteract the negative average treatment effect. This means the average annual salary for retail salespeople working in treated commuting zones decreased by 3%.[14]

5.3 Heterogeneous Effects by Employer Affiliation to the Merger

The second aspect of heterogeneous effects we are interested in investigating is employer affiliation to the merger. In this section, our objective is to test whether the merger wage effect for employees working for the merging parties differs from the effect for those who work for other firms. For this purpose, we are using the specification represented by equation 3, where the post-treatment indicator is interacted with a dummy variable that takes the value one if the vacancy is posted by either Target or CVS and takes the value zero otherwise.

Table 9 reports the estimated coefficients with alternative fixed effects specifications. Column 4 shows that the post-treatment coefficient for merging firms is negative and significant at the 1% significance level. In absolute terms, the magnitude of the merger effect for merging firms is greater than the average treatment effect for all firms. This implies that the annual salary of the employees working for Target and CVS in the treated commuting

[13] The total salary reduction for pharmacy technicians is $[e^{-0.0591-0.0675} - 1] * 100 \approx -12\%$. The salary reduction for pharmacy technicians compared to all other occupations is $[e^{-0.0675} - 1] * 100 \approx -6.5\%$.

[14] The total effect on the annual salary of retail salespeople is $[e^{-0.0591+0.0279} - 1] * 100 \approx -3\%$.

zones declines by more than double the salary reduction experienced by other employees. Following the merger, employees who work in the treated commuting zones for firms other than Target and CVS face a salary reduction of 5%, whereas the salaries of those who work for either Target or CVS are reduced by 12%.[15] This finding reflects the wage-setting power that the merging parties enjoy and exercise against their newly hired workers.

5.4 Heterogeneous Effects by Occupation and Employer Affiliation to the Merger

We were also curious to study the interaction between the heterogeneous effects by occupation and employer affiliation together at once. In this section, we aim to answer the following question: Among all the employees working for the merging parties, is the average annual salary of pharmacists, pharmacy technicians, and retail salespeople affected differently? To answer this question, we estimate equation 4. The estimated coefficients are shown in Table 10 with alternative fixed effects specifications. The main finding from this table is that when we control for the three occupations of interest, the coefficient for the post-treatment indicator interacted with the $Merging$ dummy is positive and statistically significant for all four specifications. This means that the negative coefficients for the $Merging$ interaction term reported in Table 9 were mainly driven by the pharmacists, pharmacy technicians, and retail salespeople occupations. This does not mean that workers in other occupations who work for the merging parties are not adversely affected by the merger. In fact, their average annual salary declined by 2% as a result of the merger.[16]

Regarding the three occupations of interest, column 4 of Table 10 shows that the three interaction coefficients are all negative and statistically significant at the 1% significance level. The estimated coefficient for the pharmacists' occupation implies that pharmacists

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[15] The total effect on the annual salary of workers employed at the merging parties is $[e^{-0.0502-0.077} - 1] * 100 \approx -12\%$.

[16] The decline in salary salary experienced by employees working for the non-merging parties is $[e^{-0.0464+0.0251} - 1] * 100 \approx -2\%$.

working for the merging entity face a salary reduction of nearly 54%.[17] However, we believe that this coefficient greatly overestimates the salary reduction experienced by pharmacists due to the data concerns we previously mentioned in Section 3.1. By definition, estimating the coefficient of the interaction term between the post-treatment indicator, employer affiliation to the merger indicator, and the pharmacist occupation indicator compares the annual salary posted in the pharmacists' job ads by Target and CVS to the annual salary posted in other job ads by the same merging firms pre- and post-merger and across the treated and control commuting zones. The pharmacists' job ads posted by the merging parties post-merger are essentially CVS pharmacists' job ads. The average annual salary posted in CVS job ads for pharmacists was unduly low in the years 2020 and 2022, as explained earlier. These considerably low averages are highly likely to have greatly influenced the pharmacists' coefficient in Table 10.

As for pharmacy technicians, the coefficients in column 4 indicate that their annual salary exhibited a negative wage growth of 11%.[18] In Section 5.2, we found that belonging to the retail salespeople occupation mitigated the salary reduction by approximately 3%. Table 10 reveals that compared to other occupations hired by the merging firms, the annual salary of retail salespeople working for Target or CVS post-merger dropped by approximately 11%. Accordingly, the total salary reduction experienced by retail salespeople hired by either Target or CVS is nearly 13%.[19]

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[17] The merger effect on the annual salary of pharmacists working for the merging parties is $[e^{-0.0464+0.0251-0.758} - 1] * 100 \approx -54\%$.

[18] The merger effect on the annual salary of pharmacy technicians working for the merging parties is $[e^{-0.0464+0.0251-0.0986} - 1] * 100 \approx -11\%$.

[19] The merger effect on the annual salary of retail salespeople working for the merging parties is $[e^{-0.0464+0.0251-0.114} - 1] * 100 \approx -13\%$.

6 DISCUSSION

The results presented in Section 5 contribute to the labor market monopsony literature in multiple ways. First, the average annual salary in the commuting zones affected by the merger declined by 5.5% over the 7 years following the merger, in line with Arnold (2019). This finding is also consistent with the negative relationship between higher labor market concentration and wages. This estimated wage effect excludes any wage variation across commuting zones due to factors unrelated to the merger, changes in the quarterly wage growth rate for each occupation, and employer-specific wage policies.

Second, the average merger effect on annual salary differs by occupation as well. Pharmacy technicians —a low-wage occupation with limited outside job options— are worse off after the merger; the average wage for pharmacy technicians in the treated commuting zones is reduced by an additional 6.5% compared to other occupations. Accordingly, the pharmacy technicians' wages are reduced by a total of 12% over the 7 years post-merger. Table 5 shows that CVS is the largest employer of pharmacy technicians among retail pharmacy chains –it controls nearly 40% of the labor market for pharmacy technicians. Following the merger, employer monopsony power increased in the labor markets for pharmacy technicians, which resulted in additional wage reductions for pharmacy technicians. This paper corroborates the findings of Schubert et al. (2021) who estimated that 83% of pharmacy technicians experienced a drop in their wages by at least 2% due to employer concentration in 2019.

On the other hand, the annual salary of pharmacists —a high-wage occupation with limited outside job options— does seem to be less adversely affected by the merger compared to other occupations. As we highlighted earlier, having few pharmacist job ads with salary information and aberrant salary information for some of the observations makes me less certain about that finding. The differential merger effect on the wages of salespeople —a low-wage occupation with broad outside job options— was found to be positive but not high enough to counteract the average merger effect on annual salary. Compared to other occupations, the annual salary of retail salespeople increased by 2.8%, on average over the 7 years following the merger. However, the average annual salary reduction for all occupations is 5.5%, meaning that retail salespeople face a drop in their salary by approximately 3%. This result contradicts Prager and Schmitt (2021) who found no evidence of wage reductions for unskilled workers as a result of hospital mergers between 2000 and 2010 in the United States. On the other hand, this paper is consistent with Guanziroli (2022) who concluded that retail salespeople's wages dropped by 3.5% following a merger between two large retail pharmacy chains in Brazil.

Third, there is statistically significant evidence that the average merger effect on annual salary does vary based on employer affiliation to the merger. Over the 7 years following the merger, the annual salary of employees who work for either Target or CVS in the treated commuting zones experienced an additional decline of 7.4% compared to those who work for other firms in the same labor market. This finding in itself is a depiction of the imperfect competition in labor markets that results in a divergence between a firm's wage level and the market wage level. Further, it implies the higher monopsony power the merging parties gain and exercise following the merger.

Fourth, Section 5.4 showed that retail salespeople working for either Target or CVS are greatly harmed by the merger. The average salary reduction for retail salespeople working for the merging parties is estimated to be nearly 13%. This is also in line with Guanziroli (2022). Because the author had access to Brazil's employer-employee matched dataset, they were able to study the job mobility of retail salespeople working for the merging parties. They found that salespeople working for retail pharmacies do not necessarily consider other salespeople's jobs as comparable to their drugstore jobs. Further, salespeople who work for chain pharmacies tend to switch jobs to another chain pharmacy compared to those who work for independent pharmacies.

As previously mentioned in Section 2, the literature has been inconclusive about the relationship between the effect of labor market monopsony on wages and occupations' wage rank. The results of this paper provide a novel answer. Low-wage occupations with limited outside job options are the most likely to experience higher reductions in their salary in the event of increasing labor market concentration.

7 CONCLUSION

Recent theoretical and empirical literature proved that labor markets are not perfectly competitive. In fact, a perfectly competitive labor market is the exception rather than the norm. Workers get paid less than their marginal product in equilibrium. Labor supply elasticity of the individual firm is way less than infinity. A meta-analysis synthesizing 1320 labor supply elasticity estimates from 53 different studies showed that residual labor supply elasticity is around 7, which means that workers only receive 88% of their marginal product. Additionally, a robust negative relationship between labor market concentration and wages was established in the empirical monopsony literature.

This paper analyzes the effect of the acquisition of Target's pharmacy business by CVS Health in 2015 on the annual salary of new hires. we employed a difference-in-differences model to measure the average treatment effect of the merger. In addition, we test for heterogeneous effects based on occupation. We estimated the effect of the merger on the wages of pharmacists, pharmacy technicians, and salespeople. We specifically chose those three occupations to understand what type of occupations (high-wage versus low-wage and high-mobility versus low-mobility) face the highest employer monopsony power following a merger. We also studied whether the merger effect is different for those who work for the merging firms compared to those who work for other employers.

There is evidence of rising employer monopsony power following the merger that resulted in a 5.5% decline in annual salary, on average. Pharmacy technicians —a low-wage occupation with limited outside job options— were the most adversely affected by the merger. They experienced an additional salary reduction of 6.5%. Furthermore, workers employed at the merging firms had their salaries reduced by an average of 12%. Lastly, retail salespeople working at the merging firms faced salary reduction of nearly 13%. This paper contributes to the scarce yet evolving literature that focuses on the labor market repercussions of mergers. Further, it provides some clarity as to whether employer monopsony power differs depending on the occupation's wage rank, skill level, and/or mobility.

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FIGURES

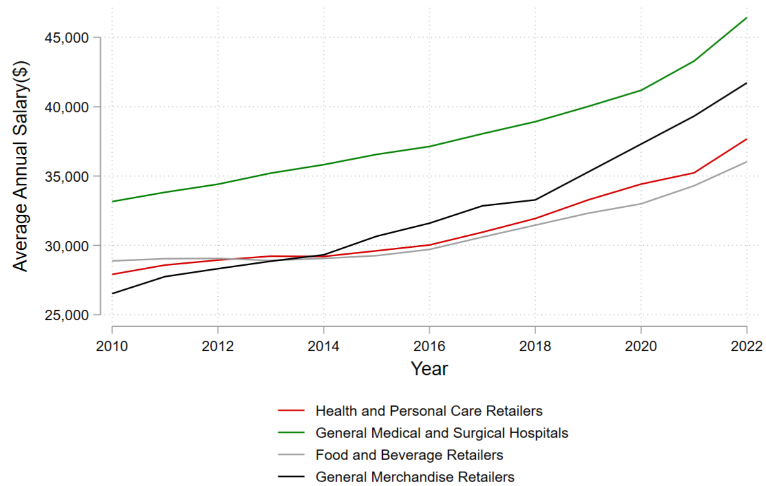


Figure 1: Average annual salary of pharmacy technicians by industry using BLS OEWS data, 2010–2022.

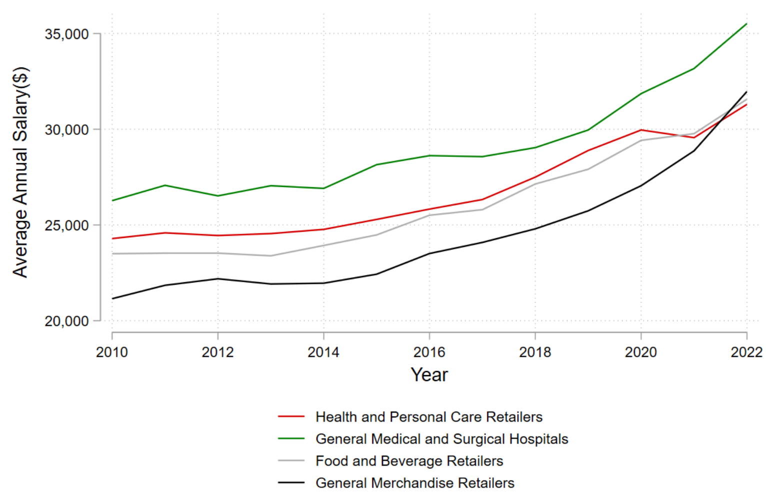
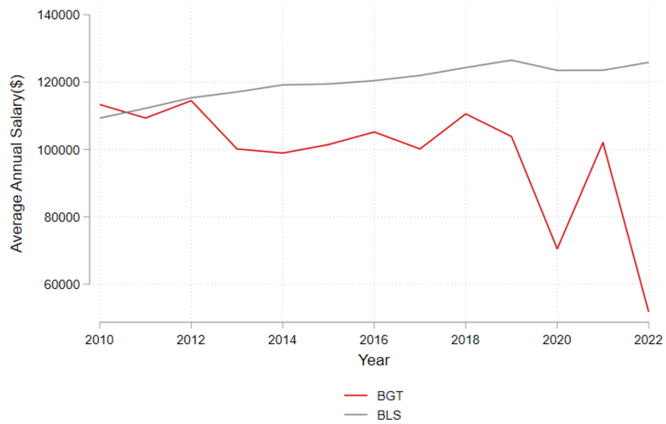
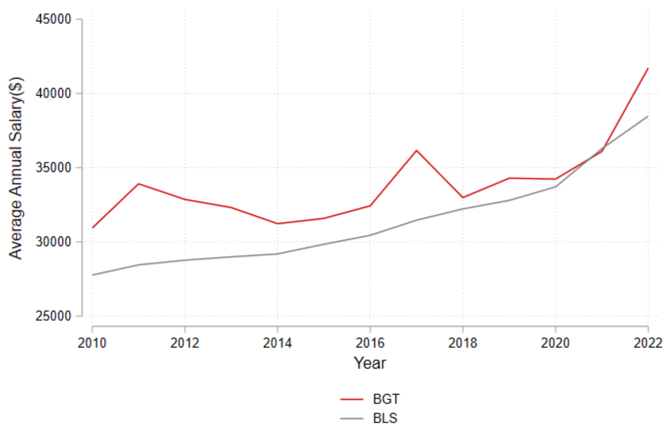


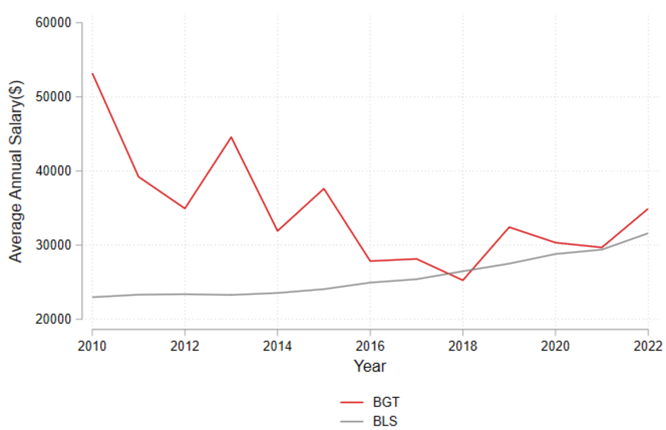
Figure 2: Average annual salary of retail salespeople by industry using BLS OEWS data, 2010–2022.



(a) Pharmacists

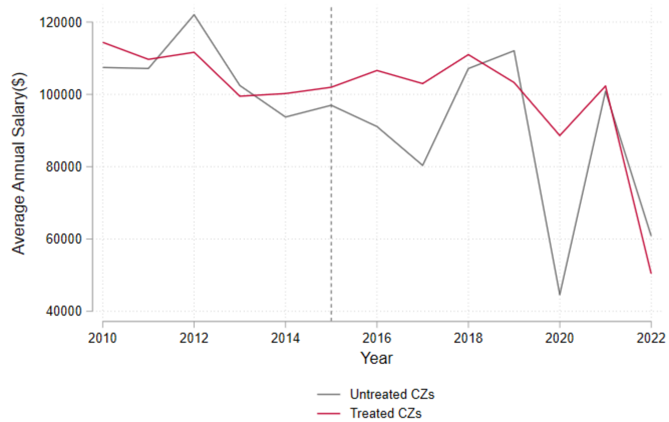


(b) Pharmacy Technicians

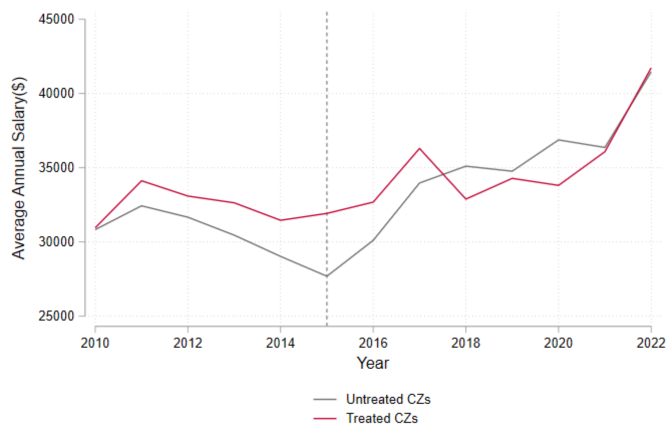


(c) Retail Salespeople

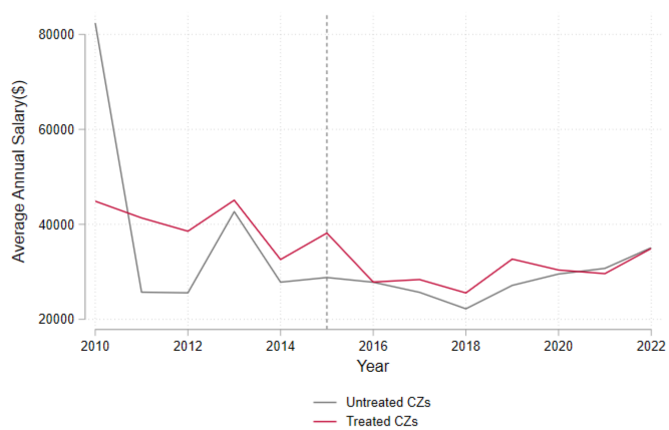
Figure 3: Average annual salary of pharmacists, pharmacy technicians, and retail salespeople using BGT and BLS data, 2010–2022.



(a) Pharmacists

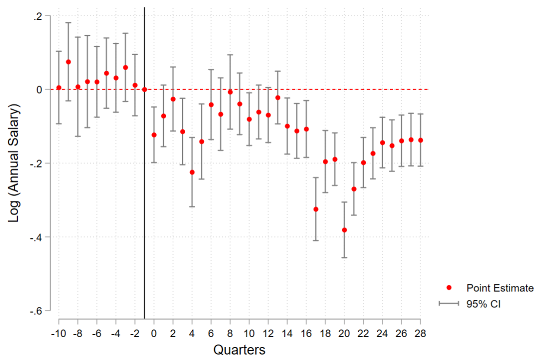


(b) Pharmacy Technicians

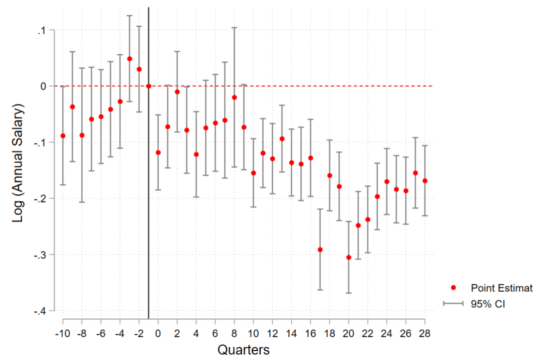


(c) Retail Salespeople

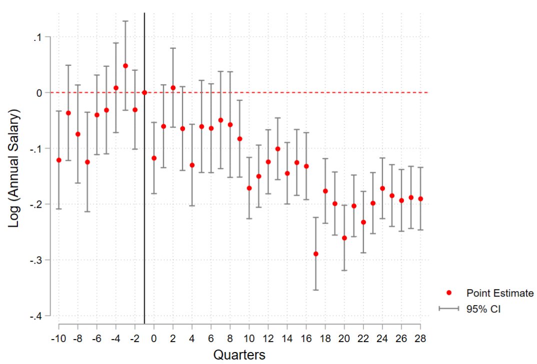
Figure 4: Average annual salary of pharmacists, pharmacy technicians, and retail salespeople by commuting zone-based treatment, 2010–2022.



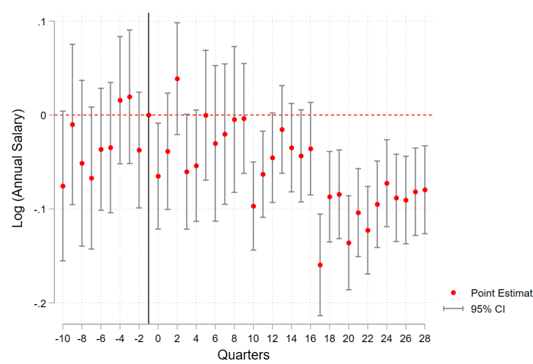
(a) Specification (1)



(b) Specification (2)



(c) Specification (3)



(d) Specification (4)

Figure 5: Differential wage trends between treated and control commuting zones before and after the merger.

TABLES

Table 1: Average annual salary (USD) of pharmacists, pharmacy technicians, and retail salespeople by chain, 2010–2022.

	Pharmacists	Pharmacy Technicians	Retail Salespeople
Target	84,862	32,944	30,852
CVS	27,345	35,823	32,506
Walgreens	100,341	34,442	35,782
Rite Aid	97,688	47,555	53,582
Albertsons	86,909	34,384	32,877
Kroger	98,777	31,306	27,791
Walmart	82,937	37,842	33,784
Costco	135,069	39,610	58,817
Publix	86,801	30,738	38,310
Obs.	12,867	27,681	28,135

Table 2: The distribution of the annual salary (USD) of pharmacists' vacancies posted by CVS in 2022 by job title.

	Mean	P10	P25	P50	P75	P90	Obs.
Clinical Pharmacist	137,160	117,650	129,400	142,450	142,450	153,750	310
Pharmacist	20,726	15,938	15,938	15,938	15,938	17,914	1,511
Pharmacy Director	160,500	160,500	160,500	160,500	160,500	160,500	1
Pharmacy Manager	23,693	17,914	17,914	17,914	17,914	17,914	1,640
Staff Pharmacist	19,299	15,938	15,938	15,938	15,938	15,938	4,999
NA	111,721	33,280	117,655	129,400	129,400	143,312	9
All years	24,833	15,938	15,938	15,938	17,914	17,914	8,470

Table 3: The distribution of the annual salary (USD) of pharmacists by commuting zone-based treatment, 2020.

	Untreated CZs	Treated CZs
Mean	44,624	88,600
SD	23,512	42,790
P10	37,752	34,320
P25	37,752	37,752
P50	37,752	102,333
P75	37,752	119,600
P90	37,752	135,200
P95	114,400	156,762
Obs.	1,353	1,931

Table 4: The distribution of the annual salary (USD) of retail salespeople by commuting zone-based treatment, 2010.

	Untreated CZs	Treated CZs
Mean	82,460	44,882
SD	86,467	49,755
P10	16,640	15,912
P25	17,500	17,472
P50	37,500	22,880
P75	150,000	52,000
P90	290,000	100,880
P95	290,000	150,000
Obs.	165	578

Table 5: Average market shares (%) by occupation: pharmacists, pharmacy technicians, and retail salespeople, 2015Q1:Q3.

	Pharmacists	Pharmacy Technicians	Retail Salespeople
CVS	19.59	39.42	3.42
Target	3.37	2.68	3.11
Walgreens	0.55	0.46	0.01
Rite Aid	3.88	6.85	1.12
Albertsons	2.50	3.31	1.01
Kroger	1.70	6.66	4.23
Walmart	20.93	0.18	0.54
Costco	0.10	0.12	0.01
Publix	0.28	0.20	0.00
Markets	1,659	1,640	1,948

Table 6: Average annual salary by commuting zone-based treatment and time of treatment, 2010-2022.

	Pre-treatment		Post-treatment	
	Untreated CZs	Treated CZs	Untreated CZs	Treated CZs
Salary	42,470	50,976	42,098	40,563
	(38,727)	(41,880)	(28,574)	(24880)
Obs.	14,100	82,070	91,799	1,217,524

SD in parentheses

Table 7: DiD estimates – Baseline specification (Estimating equation 1 with alternative fixed effects specifications, where the outcome of interest is the log of the annual salary posted in the job ad.)

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat $\times$ Post	-0.200***	-0.136***	-0.120***	-0.0566***
	(0.0173)	(0.0145)	(0.0112)	(0.00935)
Constant	10.67***	10.61***	10.60***	10.54***
	(0.0163)	(0.0129)	(0.0100)	(0.00830)
Observations	1,244,849	1,244,806	1,240,559	1,217,736
R-squared	0.081	0.340	0.459	0.563
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: DiD estimates – Heterogeneous effects by occupation (Estimating equation 2 with alternative fixed effects specifications, where the outcome of interest is the log of the annual salary posted in the job ad.)

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.159***	-0.113***	-0.122***	-0.0591***
	(0.0176)	(0.0140)	(0.0112)	(0.00943)
Treat × Post × Pharm	0.146***	-0.373***	0.0658*	0.0430
	(0.0338)	(0.0449)	(0.0353)	(0.0305)
Treat × Post × Tech	-0.0655***	-0.0805***	-0.0509***	-0.0675***
	(0.00928)	(0.0180)	(0.0145)	(0.0150)
Treat × Post × Retail	-0.225***	-0.0314**	0.0219***	0.0279***
	(0.00750)	(0.0123)	(0.00711)	(0.00657)
Constant	10.67***	10.60***	10.60***	10.54***
	(0.0162)	(0.0124)	(0.00992)	(0.00829)
Observations	1,244,849	1,244,806	1,240,559	1,217,736
R-squared	0.114	0.342	0.459	0.564
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: DiD estimates – Heterogeneous effects by employer affiliation to the merger (Estimating equation 3 with alternative fixed effects specifications, where the outcome of interest is the log of the annual salary posted in the job ad.)

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.199***	-0.127***	-0.120***	-0.0502***
	(0.0173)	(0.0143)	(0.0115)	(0.00957)
Treat × Post × Merging	-0.0130	-0.0891***	-0.00649	-0.0770***
	(0.0101)	(0.0121)	(0.00697)	(0.0107)
Constant	10.67***	10.61***	10.60***	10.54***
	(0.0163)	(0.0128)	(0.0102)	(0.00845)
Observations	1,244,849	1,244,806	1,240,559	1,217,736
R-squared	0.081	0.342	0.451	0.557
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: DiD estimates – Heterogeneous effects by occupation and employer affiliation to the merger (Estimating equation 4 with alternative fixed effects specifications, where the outcome of interest is the log of the annual salary posted in the job ad.)

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.198***	-0.117***	-0.111***	-0.0464***
	(0.0172)	(0.0139)	(0.0114)	(0.00945)
Treat × Post × Merging	0.115***	0.0297***	0.0525***	0.0251*
	(0.0105)	(0.00695)	(0.00703)	(0.0129)
Treat × Post × Merging × Pharm	-0.822***	-1.524***	-0.930***	-0.758***
	(0.0245)	(0.0342)	(0.0528)	(0.0471)
Treat × Post × Merging × Tech	-0.183***	-0.0910***	-0.102***	-0.0986***
	(0.0130)	(0.00947)	(0.0100)	(0.0118)
Treat × Post × Merging × Retail	-0.310***	-0.0509***	-0.0652***	-0.114***
	(0.0120)	(0.00926)	(0.00915)	(0.00810)
Constant	10.67***	10.60***	10.59***	10.54***
	(0.0163)	(0.0124)	(0.0101)	(0.00838)
Observations	1,244,849	1,244,806	1,240,559	1,217,736
R-squared	0.101	0.384	0.466	0.568
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1