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

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Abstract

We analyze the labor market impact of the acquisition of Target's pharmacy business by CVS Health in December 2015, using job posting data from Lightcast. Employing difference-in-differences and event study specifications with treatment assigned by geography, we find that the acquisition reduced pay in affected labor markets by 5.5% in our preferred specification. We test for heterogeneous merger effects by occupational pay rank and by outward occupational mobility. Lower-pay and lower-mobility occupations exhibit more pronounced negative effects from the merger.

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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. In Industrial Organization, researchers typically assume firms have some form of market power in the product market, but the analysis would follow the assumption that they hire in labor markets that are perfectly competitive. However, in the 1990s, scholars began to provide both theoretical models 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 the special case of infinitely-frequent outside job offers would workers 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 to the individual employer is finite and low in some markets—another clear contradiction to the assumption of perfect competition in labor markets (Azar, Berry, & Marinescu, 2022; Azar et al., 2019; Sokolova & Sorensen, 2021).

One potential source of imperfect labor market competition is mergers between employers in the same labor market, reducing the number of employers and hence potentially increasing the market power of the remaining ones (Azar & Marinescu, 2024). 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 in recent 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, 2015).

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 labor market outcomes. 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 resulted in reductions in posted pay, indicating that employer consolidation diminished labor market competition. Second, we test whether the posted pay reductions, if any, differ by occupational characteristics including average pay and job mobility, as well as pay frequency (hourly wage versus annual salary).

We study the acquisition of Target's pharmacy business by CVS Health that took place in 2015.[1] On December 16, 2015, CVS Health Corporation closed its acquisition of 1672 Target in-store pharmacies in 47 states. Those 1672 pharmacies were accordingly operated through a store-within-a-store format, branded as CVS/pharmacy (Target, 2015). Prior to the acquisition, CVS Health and Target's pharmacies were two large national retail pharmacy chains in the United States. The acquisition 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 in our quasi-experimental research design.

We use online job vacancy data from Lightcast covering the near-universe of online job postings in the U.S. economy.[2] Our dataset covers the period 2010–2022, which is five years preceding the merger and seven years following it. Using the Lightcast data, we construct a difference-in-differences (DiD) model in which the treated commuting zones are defined as those where we 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

[1]We sometimes refer to this acquisition as a "merger" throughout the paper despite the distinction that can be made 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.

[2]Using online vacancies data has become more prevalent recently in studying monopsony in labor markets (e.g., Acemoglu et al., 2022; Azar, Berry, & Marinescu, 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 vacancy data to conduct a merger retrospective analysis.

group and 475 commuting zones in the control group.

The DiD model allows us to compare the pay posted in job ads before and after the merger, in the treated versus control commuting zones. This identifies the treatment effect of the merger on posted pay under the assumptions of parallel trends and Single Unit Treatment Value (SUTVA). From there, we test for heterogeneous merger effects by occupation and pay frequency. The dimensions of occupational heterogeneity we consider are average pay (high-wage versus low-wage occupations, ranked using Occupational Employment Statistics from BLS) and mobility (defined in two different ways as discussed below, and measured using Schubert et al. (2024)'s data built from resumes/job histories). Pay frequency is reported directly in the Lightcast job vacancy data. Because our dataset is entirely comprised of job vacancies, the effect of the CVS–Target merger on the posted annual salary estimated in this paper is primarily about newly hired workers. If the purpose is to distinguish between the merger effect on the earnings of existing workers and new hires, one should use matched employer-employee datasets, similar to Guanziroli (2022) and Thoresson (2021).

Our findings speak directly to the literature on which workers are most adversely affected by employer monopsony power in labor markets, and specifically which workers are most adversely impacted by mergers of employers in labor markets. Prager and Schmitt (2021) directly addresses this question with respect to a wave of hospital mergers in the 2000s that impacted local labor markets differentially. Those authors find that more merger exposure leads to earnings losses for the occupations with few options outside of hospital employers: skilled medical personnel. By contrast, both white-collar administrators and low-wage service workers were not harmed by those hospital mergers, which the authors interpret to be because they have abundant employment options outside the hospital sector.

We borrow our empirical strategy from those authors, and our heterogeneity analysis is designed to disentangle two relevant sources of occupational heterogeneity: mobility as measured by occupation transition data, and status as measured by occupation average pay.

Our findings also extend those of Schubert et al. (2024), who estimate the effective labor market concentration in different occupations, taking into consideration whether workers in each occupation have the option to take jobs in adjacent occupations. Our findings about the CVS–Target merger's effect on occupations with different degrees of labor market mobility validate those authors' interpretation: the easier it is for workers to take a job in a different occupation, the less harmed they were by the merger.

The contribution of this paper is threefold. First, it adds to the merger retrospective literature focusing on labor markets. Second, it provides supporting and arguably better-identified findings to the literature that investigates the relationship between labor market concentration and wages. Third, it contributes to the inconclusive literature on which occupations are most adversely affected by employer monopsony power.

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 explains the methodology we used to answer our research questions. Section 4 presents the difference-in-differences regression results. Section 5 discusses the implications of our findings on labor markets. Finally, Section 6 concludes.

2 Literature Review

Our interest in studying the effect of a large retail acquisition on pay stems from the literature highlighting lack of competition in local labor markets. The prevalence of micro-level data covering labor markets, such as survey data, online job vacancies data, and matched employer-employee data, aided the development of the empirical literature studying employers' monopsony power in the labor market. Using data covering the near-universe of online job postings in the US economy, Azar et al. (2020) estimated that in 2016, 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.[3] [4] Schubert et al. (2024) showed that one in every six workers in the U.S. economy in 2019 faced a wage reduction of at least 2% due to high labor market concentration. Further, recent empirical literature provides evidence of low levels of residual labor supply elasticity, implying wage-setting power on the part of employers (Azar, Berry, & Marinescu, 2022; Azar et al., 2019; Sokolova & Sorensen, 2021).

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. 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, the average HHI in the pharmacy market dropped from 1 to a little over 0.25 in 2016. This decline in HHI varied across commuting zones, enabling the calculation of the elasticity of wages to changes in HHI using a difference-in-differences model with varying treatment intensities. 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]

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

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[3] 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.

[4] 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.

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

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.

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 matched employer-employee data to add worker and establishment fixed effects to capture the wage effect of the change in labor market concentration induced by the merger, excluding observable and unobservable changes in labor force composition. The author found that the wages of pharmacists dropped by 2.6% and that of salespeople decreased by 3.5%.

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., 2024; Thoresson, 2021). A large share of this literature relies on online job vacancies data, such as CareerBuilder.com and Lightcast 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., 2024). 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., 2024; 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). Others used different concentration measures at the national level to instrument for local labor market concentration (Azar, Marinescu, & Steinbaum, 2022; Rinz, 2022; Schubert et al., 2024).

Third, the literature does not provide a clear understanding of which type of occupations suffer the most from the effects of increasing employer concentration on their workforce. Some studies focused on the relationship between labor market concentration and occupations' skill requirements, whereas other focused on occupational pay and mobility ranks. 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.[7] 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 contrast, Prager and Schmitt (2021) provided evidence of wage growth differentials based on the workers' skill level and the ease of mobility across industries. The wages of unskilled workers whose job tasks are not specific to the hospital industry were not affected by the merger. However, skilled workers and more specialized workers, namely nurses and pharmacists, experienced significant wage reductions. Guanziroli (2022) showed that following a merger between two large pharmacy chains in Brazil, the wages of salespeople

[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.(2024) 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.

[7] Their empirical strategy involved running 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 SOC codes.

declined more than that of pharmacists because retail salespeople working in drugstores have fewer outside options. One in every four salespeople working in a pharmacy who switched jobs still worked in a pharmacy the following year.

Schubert et al. (2024) focused on the degree of occupational mobility. The authors used highly granular occupation mobility data covering 16 million US workers' resumes to study mobility patterns across occupations (six-digit SOC). They found that 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 paper lists the twenty occupations with the largest number of workers who experience a decline in their wages by at least 2% due to above-median employer concentration in 2019. At the top of the list, there are high-wage occupations such as registered nurses and pharmacists, and low-wage occupations such as hairdressers, secretaries, and administrative assistants.

3 Empirical Strategy

3.1 Data

We use proprietary job posting data from Lightcast (formerly known as Burning Glass Technologies, abbreviated BGT). Lightcast sweeps over 51,000 sources daily to collect job vacancies posted on online job boards and company websites, capturing the near-universe of online job vacancies. Because a job ad can be posted on multiple online platforms, Lightcast employs de-duplication methods to remove duplicates and have a putatively-unique observation for each vacancy.

According to Lightcast, on average, 92.6% of the monthly job openings reported by the Job Openings and Labor Turnover Survey (JOLTS) are captured by Lightcast data during the period 2013–2024 (Lightcast, 2024). Industries that tend to be underrepresented are industries where offline postings and word-of-mouth are still common in hiring, such as the Accommodation, Food Services and Construction industries. On the other hand, Professional and Business Services industries tend to be overrepresented.

Other studies have tested the representativeness of Lightcast data. A study conducted by the Organization for Economic Cooperation and Development (OECD) showed that Lightcast data is statistically representative of the labor market in the United States during the period 2010–2019 (Cammeraat & Squicciarini, 2021). Hershbein and Kahn (2018) showed that although some occupations are overrepresented while others are underrepresented when compared to the current population survery (CPS) data, the representativeness of Lightcast data seems to be time-invariant at the occupational level. Macaluso et al. (2019) found that Lightcast data cover almost 80% of total job advertisements in the U.S. economy.

The unit of observation in Lightcast 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 ad, such as listing date, occupation (six-digit SOC), job title, location, employer name, four-digit and six-digit North American Industry Classification System (NAICS) codes, education and skill requirements, posted salary (which is annualized under the assumption of full-time work), and pay frequency (reflecting the unit of pay given in the body of the ad). Our variables of interest are job posting date, six-digit SOC code, four-digit NAICS 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, posted vacancy-level annual salary, and pay frequency. The Lightcast data report posted annual salaries in the form of a range. For each vacancy with salary information, lower and upper salary bounds are given. Where those are not identical, we use the midpoint between those bounds to compute our posted pay variable. That posted salary variable is then winsorized at the 1^{st} and the 99^{th} percentile by year and six-digit SOC code to remove outliers.

One downside of the Lightcast dataset is that only 24% of the vacancies between 2010 and 2022 have posted salary information. As a result, our sample size is reduced to approximately 90 million vacancies. One major concern when using Lightcast posted salary information is that the posted salaries do not necessarily reflect the realized salaries for new hires, let alone what incumbent workers are paid. However, Hazell and Taska (2020) found that Lightcast posted salary data closely reflect changes in the salaries of new hires using CPS data. The coefficient from regressing the log of salaries estimated using CPS data on the log of salaries reported by Lightcast at the state-quarter level over the period 2010–2016 is nearly one. Those authors also compared Lightcast posted salary to the average earnings for new hires from the Quarterly Workforce Indicators (QWI) at the state quarter-level over the period 2010–2016. The elasticity of QWI earnings with respect to Lightcast salary is estimated to be close to one. Thus, Lightcast salary data align with the realized wages of new hires, whether compared to survey or administrative data.

Aside from Hazell and Taska (2020)'s comparison of Lightcast data to QWI, there are good reasons to think that posted pay in job ads is an accurate estimate of what newly-hired workers are actually paid. Hall and Krueger (2012) surveyed a representative sample of US workers to study the probability of wage posting and bargaining. Around 63% of the respondents did not bargain over pay before accepting their current or most recent job. In addition, 30% of respondents reported that they knew exactly their expected salary before being hired, implying probability of wage posting of nearly one-third. Further, Hazell and Taska (2020) highlighted that online vacancy posting is often expensive when posted to online job boards, and job boards usually charge additional fee to keep vacancies open if not filled after a month. Accordingly, firms that post wages to their online vacancies have an incentive to post up-to-date wage information.

Batra et al. (2023) raise concerns about the accuracy of Lightcast's salary data, partly due to the infrequency with which employers post pay before 2017, and partly because the proportion of vacancies with salary information significantly increases after 2018. One likely contributor to the increasing share of job ads that include posted salary in the Lightcast data is because Lightcast may import imputed salary information from major job boards, such as LinkedIn and Indeed. However, it is also the case that the prevalence of including salary information in job advertisements has increased significantly in recent years, both due to some states adopting laws mandating salary transparency, as well as the tight labor market (Stahle, 2023). We adopt Callaci et al. (2023)'s technique to address the issue of imputed salaries by dropping vacancies that are most likely to have imputed salaries.[8]

The other major concern raised by Batra et al. (2023) is that the wide variation in the frequency of pay-posting at the employer level would bias estimates that depend on firm-level posted pay to assign treatment. That is a particular issue in the minimum wage literature, in which the bite of the minimum wage is more severe for ex-ante lower-wage firms. Implementing a specification that makes use of that fact in the Lightcast data would mean that posted pay assigns treatment intensity. The authors demonstrate that this could bias estimates of the effect of a minimum wage increase estimated from sparse data on firm-level pay, because units assigned to treatment would show increasing pay post-treatment due to mean reversion, rather than an increase in the minimum wage. We emphasize that this concern does not apply in our setting: treatment is assigned based on geography, not on posted pay, and we do not estimate any employer-specific treatment effects. Our preferred specification employs employer fixed effects, which means that treatment effects are estimated within employers, leveraging geographic variation.

As against these (valid) concerns about the data quality of online job ads, and especially the pay information they contain, we point out the following benefit: posted pay is evidently an elastic indicator of variation in labor market competition, in contrast with the salaries of

[8]If the job ad text includes a sentence with the word "estimated" and the "$" symbol, or it includes the phrase "similar jobs pay," that job ad is tagged as having an imputed salary and accordingly dropped from our sample.

incumbent workers. Callaci et al. (2023) show that immediately upon being informed about the removal of no-poaching restrictions in franchise contracts, franchisee-employers raised pay posted in job ads for exactly the workers (store managers) they would be most likely to have been prohibited from hiring ex-ante. By contrast, administrative earnings data, e.g. from unemployment insurance systems, reflects job matches entered into under different competitive conditions and is therefore likely to lag changes in those conditions. During the period of increased labor market tightness that followed the COVID-19 pandemic, there is a significantly increased volume of job ads in Lightcast data posted by employers facing labor shortages, followed by an upward trend in posted pay (Steinbaum, 2023). Finally, published peer-reviewed studies that use the Lightcast data include Acemoglu et al. (2022), Azar et al. (2020), Clemens et al. (2021), Forsythe et al. (2020), and Hershbein and Kahn (2018).

3.2 Sample Restriction

The primary goal of this paper is to investigate the effects of a merger between two large retail pharmacy chains on the posted pay of new hires. Therefore, we use 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 health professionals cannot easily move to the general medical and surgical hospital industry, the other sector in which pharmacists are predominantly employed. In other words, retail pharmacists facing 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 four 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).

Further, based on Occupational Employment and Wage Statistics (OEWS) occupational wage trends, there are large differences in earnings reported for workers in the two different sectors. Figure 1 shows that the average annual salary of pharmacy technicians 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, we should not observe this deviation between the hospital industry and retail industries. Therefore, Figure 1 provides circumstantial evidence suggesting that pharmacy technicians 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.

Hence, we 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.

3.3 Methodology

To estimate the effect of CVS's acquisition of Target's pharmacy business, we employ a difference-in-differences research design. We adapt the empirical strategy implemented by Prager and Schmitt (2021) to a setting of one national-level mega-merger, as opposed to the series of smaller, regional mergers that those authors focus on. We compare posted pay before and after the merger between treated and control labor markets.

Prior to the CVS–Target merger, both parties existed in some commuting zones but not in others.[9] 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 geographic identifiers available in the Lightcast dataset, we construct a list of commuting zones where either Target or CVS posted a vacancy during the period January 1, 2015–December 15, 2015.[10] [11] The commuting zones for which we observe at least one vacancy for both Target and CVS (indicating that both chains had at least one establishment in that geographic labor market in the year preceding the acquisition) were defined as treated commuting zones and the others form our control group. Using this measure, the treatment group consists of 234 commuting zones and the control group consists of 475 commuting zones.

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[9]We use commuting zones as the geographic analog of local labor markets, following Azar, Berry, and Marinescu (2022) and Azar et al. (2020).

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

[11]Because our treatment relies primarily on the location of job ads, we drop vacancies with missing FIPS code.

3.4 Summary Statistics

Table 1 reports the average posted pay of all the vacancies in our sample stratified by commuting zone-based treatment. The standard deviation of posted annual salary is the value in parentheses. Before the merger, the average posted annual salary for the treated observations was $50,976 compared to $42,468 for the control observations. After the merger, the average salary among observations in the treatment commuting zones dropped to $40,563—a 20% decline—while that of control observations fell to $42,098, only a 1% decrease. Whether this significant wage reduction can be explained by the merger is what we aim to answer in this paper. Figure 2 depicts the trends of the posted annual salary for both the treated and control commuting zones over the period 2010–2022.

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 per a survey conducted in 2014 by Morning Consult, a business intelligence firm (Evans, 2014). Hence, one possible explanation for post-merger pay convergence observed in Figure 2 is urban-rural pay convergence that is not itself the result of the merger. Our specification is designed to rule out this and similar possibilities, and thus to isolate the merger effect.

As discussed in Section 2, the literature has been inconclusive regarding which class of workers is more adversely affected by employer monopsony power. In this paper, we contribute to this literature by studying whether the merger effect differs along three dimensions: occupational salary, outward occupational mobility, and pay frequency. In order to test for heterogeneous effects by pay, we rank occupations (six-digit SOC) into four quartiles based on the occupation's average annual salary for the year 2015 using the OEWS wage estimates published by the BLS.

We define the degree of outward occupational mobility in two different ways. For both, we rely on the findings of Schubert et al. (2024). Those authors constructed a dataset of mobility patterns within and across occupations using 16 million unique US resumes collected by Lightcast. Using this granular data that tracks workers' job histories over 2002–2018, the authors calculated 'occupation *leave* share' and 'occupation *transition* share'. The authors define the 'occupation *leave* share' as "the share of people who leave their occupation when they leave their job," whereas the 'occupation *transition* share' is defined as the probability that a worker move from one occupation to another conditional on leaving their job. In other words, the leave share tells us the probability that a worker leaves their current occupation when changing jobs, without restricting the occupation that the worker moves to. The transition shares focuses on the probability that a worker moves to a particular destination occupation when changing jobs.

Schubert et al. (2024) presents a table that includes the twenty large occupations with lowest and highest leave shares. Table 2 lists those occupations and their corresponding leave shares as reported by Schubert et al. (2024). We use this information as one proxy for outward occupational mobility. For example, an occupation with a high leave share implies a high degree of occupational mobility, meaning that workers have high chances of finding equally comparable or better jobs in other occupations. Figure 3 visualizes the posted pay from Lightcast job ads in each of the three leave-share bins (low leave share, high leave share, and occupations not reported in either table in Schubert et al. (2024)). Occupations with the lowest leave share have the highest average posted salary.

Our other proxy for outward occupational mobility uses the occupational transition matrix computed by those authors, which they made publicly available. Analogous to the four-firm concentration ratio (CR4) that is used in the Industrial Organization literature, we take the sum of the transition shares to the top four destination occupations for each row (initial occupation) of the transition matrix. In other words, the occupational mobility dataset constructed by Schubert et al. (2024) computes the probability of workers moving from one occupation (initial occupation) to another specific occupation (destination occupation) conditional on leaving their jobs. We rank these probabilities for each initial occupation and take the sum of the highest four destination occupations. The rationale behind this measure is that the transition shares for workers in occupations with a high degree of outward occupational mobility will be high, indicating that they have the option to take jobs in other occupations in response to worsening terms and conditions of work in their current job. Therefore, the sum of the top four transition shares will be relatively high for occupations whose workers enjoy high degree of mobility and relatively low for occupations whose workers find it difficult to change their occupations. After calculating the sum of the top-four transition shares for each initial occupation, we rank initial occupations into four quartiles. Figure 4 visualizes average pay in each of the four mobility quartiles during the study period.

Comparing the two measures of outward occupational mobility, occupations with a high leave share tend to be lower-pay occupations, while those with low leave share correspond to high pay. Perhaps unsurprisingly, high-pay occupations also reflect high mobility rates (as measured by the sum of the top four transition rates). Another way of saying that is that workers in lower-pay occupations are more likely to leave their occupation when they leave their job and diffuse across many different destination occupations when they leave their job. Accordingly, the sum of the top four transition shares is relatively low for those occupations, indicating a low mobility rank. On the other hand, higher-pay workers are less likely to leave their occupation when they change jobs and they transition to fewer destination occupations, resulting in a relatively higher mobility measure based on the sum of the top four transition shares. Figure 5 depicts this positive pay-mobility correlation at the occupation level. Altogether, the occupations with low leave shares and high transition rates to the top four destination occupations are those with more training requirements, longer job tenure, and more occupation-specific skills, but the relationship between those measures of mobility and pay is noisy. Hence, a key contribution of this paper is to examine each dimension of occupational heterogeneity separately.

Finally, we are also interested in investigating whether the merger's effect on pay differs according to pay frequency. We rely on the pay frequency variable available in the Lightcast dataset to identify vacancies with annual versus hourly pay. The average posted annualized pay over time for hourly-wage versus annual-salary vacancies is shown in Figure 6.

3.5 Specification

Our baseline specification in this paper is the DiD model represented by 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 pay 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 differential occupational wage trends over time. We also add employer fixed effects, represented by μ_e, to control for employer-specific wage policies. $Post_t$ 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.

To test the parallel trends assumption necessary to attach a causal interpretation to a DiD estimator, we construct an event study specification that estimates a separate treatment effect for each quarter leading up to and following the merger. The event study specification is represented by equation 2.

$$ln(Salary_{ioect}) = \alpha_c + \gamma_{ot} + \mu_e + \sum_{\substack{t=-23 \\ t\neq -1}}^{28} \beta_t \mathbb{1}[t = quarter] \times Treat_c + \epsilon_{ioect} \quad (2)$$

where $\mathbb{1}[t = quarter]$ indicates the quarter relative to the third quarter of 2015, one quarter before the merger. We have data covering 23 quarters pre-merger and 28 quarters post-merger.

For each dimension of heterogeneity discussed in subsection 3.4, we augment the specification in equation 1 with an interaction term signifying the quantile of either the occupational pay ranking, the occupational leave share ranking, or the occupational mobility ranking. For the pay frequency dimension, we simply re-run the specification separately on each set of observations: those reporting that the job pays an annual salary, versus those reporting the job pays an hourly wage.

4 Results

4.1 Baseline Model

Table 3 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 posted 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 posted 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 for new hires by 5.5%.

4.2 Event Study

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 wages even without the occurrence of the merger. To make sure this is not the case, we examine the differential pay trends between treated and control commuting zones before and after the merger. Figure 7 plots the coefficients estimated using equation 2. The reference quarter for this estimation is the quarter preceding the merger, the third quarter of 2015. Each of the four sub-figures corresponds the different specifications reported in the four columns of Table 3.

There is no evidence of differential pre-merger salary trends in any of the specifications. Compared to control commuting zones, the average posted pay in the treated commuting zones started to steadily decline following the merger, and the negative effect magnified over time. This negative trend in the posted pay persists and intensifies during the COVID-19 pandemic, when retailers were hiring aggressively and labor market churn was generally high (Autor et al., 2023). The implication of these findings is that labor market competition for workers was adversely affected by a merger of major retail employers that had happened five years earlier.

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

4.3 Heterogeneity by Occupational Salary

We re-run the baseline model allowing the coefficient of the post-treatment indicator to vary based on occupational salary rank to investigate which class of workers are the most affected by the merger. Table 4 reports the estimated coefficients for each of the four salary quartiles. According to the preferred specification reported in column 4, workers in low-wage occupations are the most adversely affected by the merger. The posted annual salary for the new hires is reduced by 5.9% for the quartile of occupations with the lowest average annual salary and by 7.3% for the second quartile of occupations. Treatment effect estimates for the top two occupation salary quartiles are not significantly different from zero.

4.4 Heterogeneity by Outward Occupational Mobility

The second aspect of heterogeneous effects we are interested in investigating is outward occupational mobility. As discussed in Section 3.4, we measure outward occupational mobility in two different ways. First, using indicators for the occupations with the highest and lowest leave shares, Table 5 presents the estimated DiD coefficients for each set of occupations with alternative fixed effects specifications. Column 4 shows that compared to other occupations, new hires seeking jobs at any of the twenty occupations with the lowest leave shares experience an additional reduction in their posted annual salary by 5.8%. Hence, the total reduction in their pay is nearly 10.4%, on average.[13] Workers seeking jobs at any of the twenty occupations with the highest leave shares also experience additional reduction in pay but by a lesser amount compared to low leave share occupations. On average, newly hired workers at occupations with the highest leave share face a decline of 6.4%.

[13] The total effect on the posted annual salary of new hires seeking jobs at any of the twenty occupations with the lowest leave shares is $[e^{-0.0498-0.0598} - 1] * 100 \approx -10.4\%$.

As for the second measure of outward occupational mobility, Table 6 reports the estimated coefficients for each quartile of occupations ranked by the sum of the top four transition shares. Based on the fourth specification, we observe a direct relationship between the degree of outward occupational mobility and estimated effect of the merger on posted annual salary of new hires. For occupations with the lowest concentration of transition shares (i.e., low outward occupational mobility), the annual posted salary of new hires decreases by 6.5%. The magnitude of the pay reduction decreases as the degree of outward occupational mobility increases. The estimated reduction in posted annual salary is 5.2% for the second quartile of occupations and 4.6% for the third quartile of occupations. For occupations that belong to the top quartile of transition-share concentration, meaning that workers in these occupations enjoy the highest degree of occupational mobility compared to other occupations, we estimate a 4.7% increase in the annual posted salary.

Figure 8 visualizes specification (4) from each of Tables 4 and 6. It shows that both lower-wage and lower-mobility occupations suffered disproportionate pay reductions from the merger. We turn next to disentangling the two dimensions of occupational heterogeneity.

4.5 Heterogeneity by the Interaction of Occupational Salary and Outward Mobility

The results reported above indicate that workers in lower-pay occupations and in lower-mobility occupations suffer particularly large negative pay effects from the merger. In order to disentangle these two dimensions of heterogeneous treatment effects, we estimate a specification that interacts occupational salary ranks and occupational mobility ranks together at once. We aim to answer the following question: For each occupational salary quartile, does the merger effect depend on the degree of outward occupational mobility? To answer this question, we interact each salary quartile with the four transition-share concentration quartiles and estimate the post-treatment indicator for the resulting 16 interaction terms (4 pay quartiles x 4 mobility quartiles). Table 7 lists the top five occupations by observation count in each of the 16 cells of this interaction.

To visualize the regression results, we report the estimated coefficients in matrix form in Table 8. Regardless of the degree of outward occupational mobility, occupations in the two lowest salary quartiles experience a significant reduction in pay following the merger. If anything, the negative effect is larger for occupations in the second-lowest salary quartile relative to the lowest, but both exhibit substantial negative pay effects regardless of outward mobility. By contrast, the estimated merger effect for occupations with the lowest degree of outward occupational mobility (Q1 mobility) depends on the occupational salary rank. Low-wage occupations with the lowest occupational mobility experience a reduction in pay, whereas high-wage occupations with the lowest degree of mobility do not suffer a reduction in pay. These findings suggest that between the occupational salary and mobility ranks, salary rank matters more. However, mobility does matter, particularly in the third salary quartile: occupations with low outward mobility suffer pay reductions from the merger, but not occupations with high mobility.

4.6 Heterogeneity by Pay Frequency

The third dimension of heterogeneity that we investigate in this paper is pay frequency. We estimated the baseline specification for the sub-sample of annual-pay vacancies and report the coefficients in Table 9. The pay of annual-salary vacancies declined by an average of 3.4% following the merger. Table 10 shows that the the pay of hourly-pay vacancies decreased by 2.9%.[14] These results are similar to Callaci et al. (2023) in that annual-salary workers' pay is seemingly more sensitive to variation in competitive conditions than hourly wage workers, but the differences in estimated effect sizes on the pay frequency dimension are nowhere near as large as the heterogeneity we report by occupational pay rank.

[14]That both coefficients are smaller in magnitude than the baseline estimates from Table 3 is due to the fact that many if not most observations that report pay do not report a pay frequency.

5 Discussion

The results presented in Section 4 contribute to the labor market monopsony literature in multiple ways. First, in line with Arnold (2019), we find evidence of reduction in the posted pay as a result of the merger. The posted annual salary in the commuting zones affected by the merger declined by 5.5% over the 7 years following the merger, relative to unaffected commuting zones. This estimated effect on posted pay 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 pay policies. Further, our finding corroborates the negative relationship between higher labor market concentration and wages established in the literature.

Second, we showed that the effect of the merger on posted pay depends on other occupational characteristics, such as occupational pay rank and occupational mobility rank. Figure 8 compares the estimated merger effect for the full sample to the estimated effect for each occupational salary quartile and mobility quartile. New hires seeking jobs at low-wage occupations —where the average annual salary in 2015 was less than \$48,150— are the most adversely affected by the merger. This finding provides a clear answer to the question raised by Azar et al. (2020) regarding which class of workers (low-paid versus high-paid) suffer the most when employer concentration increases in a labor market.

As for occupational mobility rank, occupations with the lowest degree of occupational mobility, measured by the sum of the top four transition shares, experience the largest reduction in posted pay. This estimated pay reduction decreases as the degree of outward occupational mobility increases and posted pay is estimated to increase following the merger for occupations with the highest degree of mobility. Our findings are consistent with Prager and Schmitt (2021) in the sense that workers with the most industry- and job-specific skills (i.e., lower degree of outward mobility) are the most harmed by employer consolidation. In addition, similar to Schubert et al. (2024), we found that the average effect of the merger on posted pay obscures significant occupational heterogeneity. Whether we rank occupations based on the leave share estimates or the sum of the top four transition shares, we find evidence of heterogeneous merger effects for each class of occupations.

Third, when we interacted occupational pay and mobility ranks to study joint effects, we concluded that pay rank matters more. Prager and Schmitt (2021) found that unskilled workers whose job tasks are not exclusive to the hospital industry —mostly blue-collar workers where the top occupation is Housekeeping— are not affected by employer consolidation in the hospital sector. The authors suggested that this set of workers enjoy higher degree of outward occupational mobility and accordingly not affected by employer consolidation in the hospital industry. However, our findings showed that irrespective of the degree of outward occupational mobility, low-wage occupations experience significant reductions in their posted annual salary following the merger.

Fourth, Guanziroli (2022) found that the wages of pharmacists dropped by 2.6% and that of salespeople fell by 3.5% as a result of a merger between two large national retail pharmacy chains in Brazil. Similar to Guanziroli (2022), we estimate that the effect of the merger on pay is worse for retail salespeople. However, our results show that pharmacists' posted annual salary is not affected by the merger. As shown in Table 7, retail salespeople is the top occupation in terms of observation count that belongs to the first quartile of both pay and mobility distributions, and pharmacists is the top occupation that belongs to the highest salary quartile and second mobility quartile. Based on the estimated coefficients in Table 8, we estimate a 5.7% reduction in the posted annual salary for retail salespeople in the labor markets affected by the merger, whereas the merger effect on the annual posted salary of pharmacists is not significantly different from zero.

Fifth, any heterogeneity in the merger effect by pay frequency is not as large as the other two heterogeneity dimensions: occupational pay rank and outward occupational mobility rank. We found that the posted pay for annual-pay vacancies dropped by an average of 3.4%, whereas that of hourly-pay vacancies declined by an average of 2.5%.

Finally, the key contribution of this paper is to provide a comprehensive answer to the following question: Does the effect of employer consolidation on pay systematically differ by occupational pay rank and/or the degree of outward occupational mobility? This paper provides a novel answer by, first, disentangling the effect of each dimension and, second, studying the interaction between these two dimensions of occupational heterogeneity. Lower-pay and lower-mobility occupations are the most harmed by mergers between employers in the same local labor markets. Furthermore, the occupational pay rank has more pronounced effects irrespective of degree of outward occupational mobility, especially for low-wage occupations.

6 Conclusion

This paper analyzes the effect of the acquisition of Target's pharmacy business by CVS Health in 2015 on the posted annual salary of new hires in the affected local labor markets. We employed a difference-in-differences model to measure the average treatment effect of the merger using online vacancies data covering the period 2010–2022. In addition, we test for heterogeneous effects based on occupational characteristics: occupational pay rank, outward occupational mobility rank, and pay frequency.

We found evidence of reduction in posted pay by 5.5%, on average, following the merger. The average merger effect on the posted annual salary conceals considerable variations depending on occupational characteristics. New hires seeking jobs at low-pay occupations face disproportionate pay reductions, compared to higher-pay occupations whose workers' posted annual salary is not affected by the merger. As the degree of outward occupational mobility increases, the effect of the merger on average posted annual salary becomes less pronounced. This paper contributes to the scarce yet evolving literature that focuses on the labor market repercussions of mergers. Further, it provides clarity as to whether employer monopsony power differs depending on the occupational pay and mobility ranks.

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Figures and Tables

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

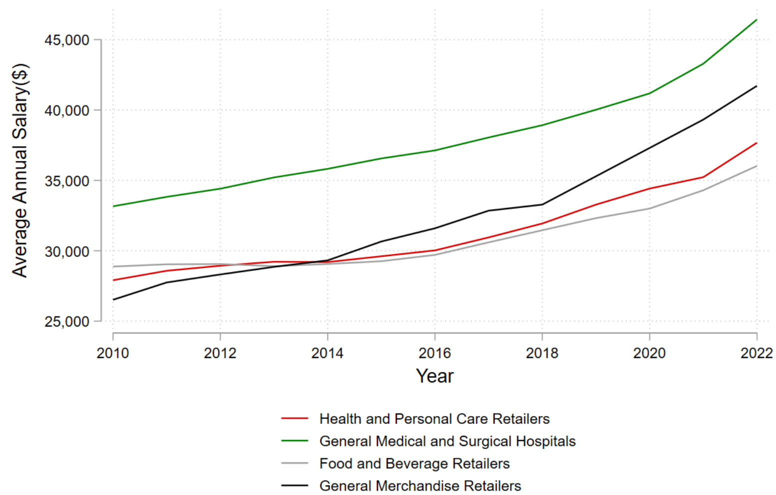


Figure 2: Average posted annual salary by commuting zone-based treatment, 2010–2022.

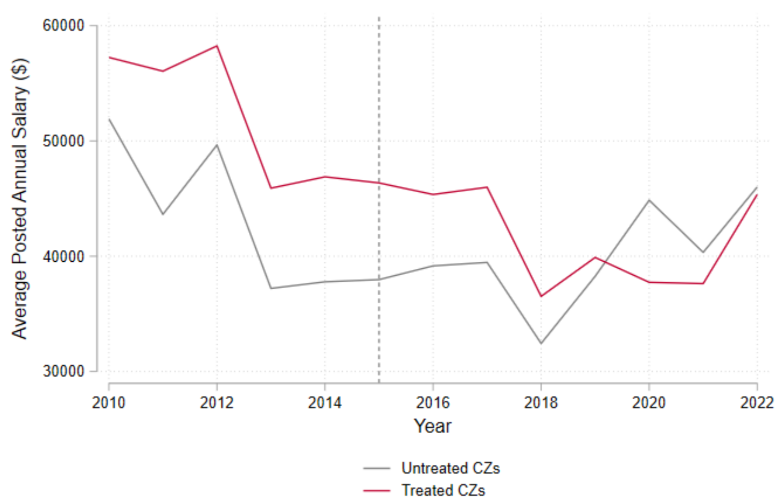
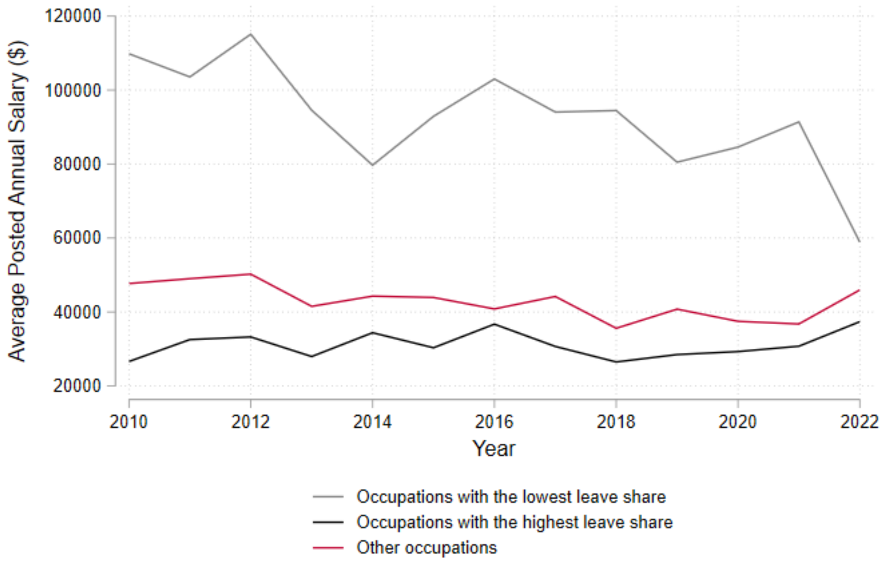


Figure 3: Average posted annual salary for each of the three leave-share bins, classified based on the leave share estimates reported by Schubert et al. (2024), 2010–2022.



Note: Table 2 lists the twenty occupations with the highest and lowest leave shares.

Figure 4: Average posted annual salary by mobility rank, measured based on the sum of the top-four transition shares, 2010–2022.

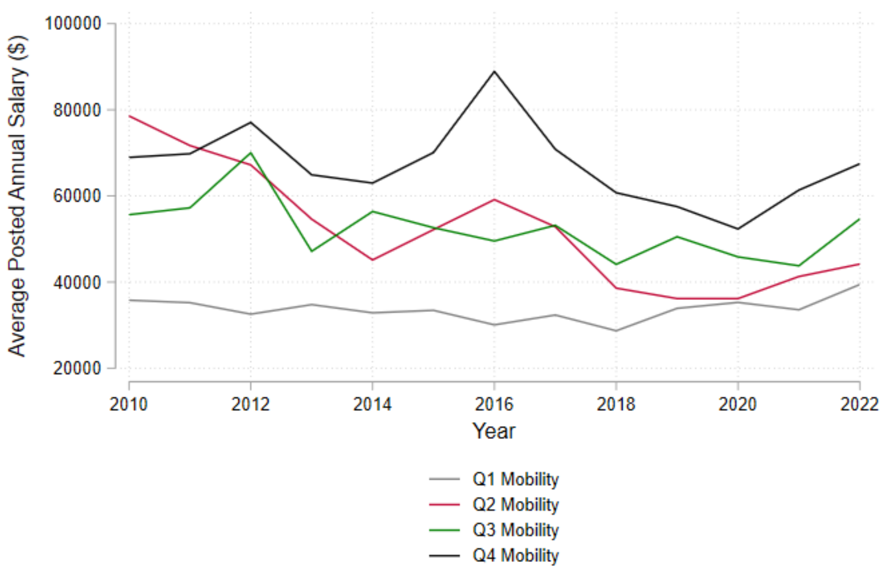


Figure 5: Correlation between posted pay and occupational mobility.

Figure 6: Average posted annual salary by pay frequency, 2010–2022.

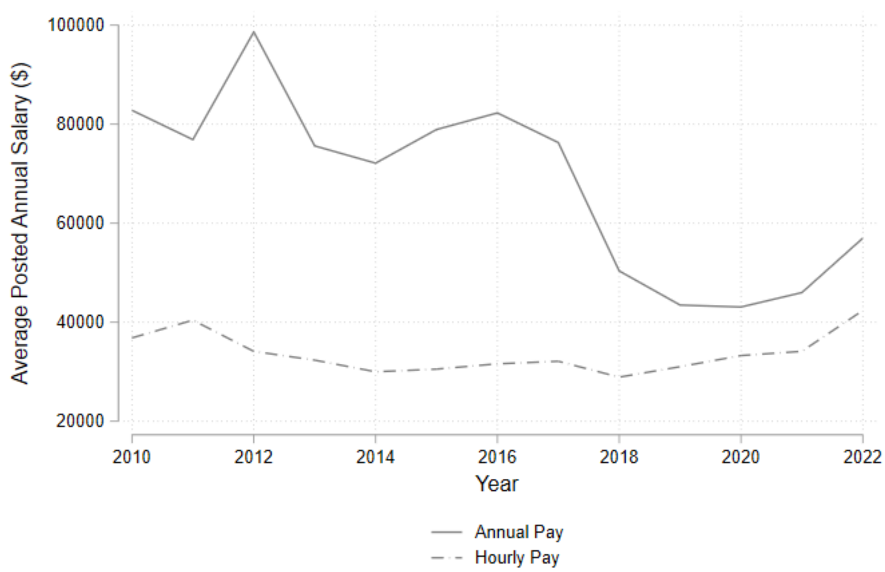
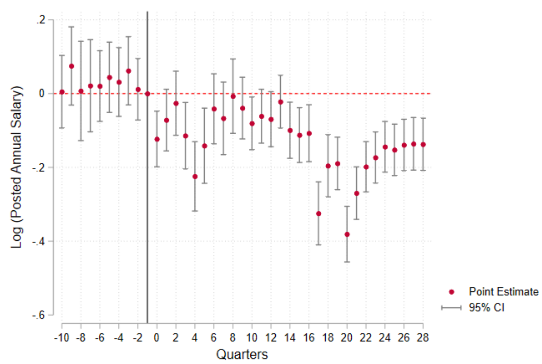
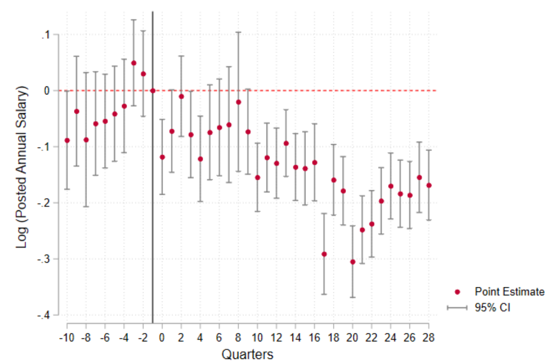


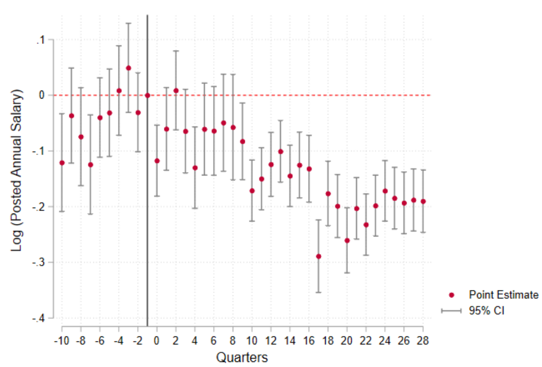
Figure 7: Differential trends of posted annual salary between treated and control commuting zones before and after the merger.



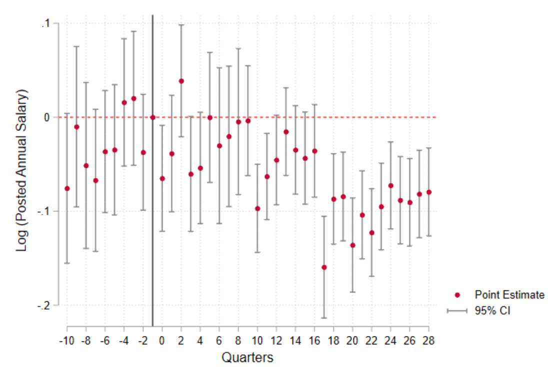
(a) Specification (1)



(b) Specification (2)



(c) Specification (3)



(d) Specification (4)

Figure 8: DiD coefficient estimates for the baseline model and heterogenous effects by occupational salary and mobility ranks.

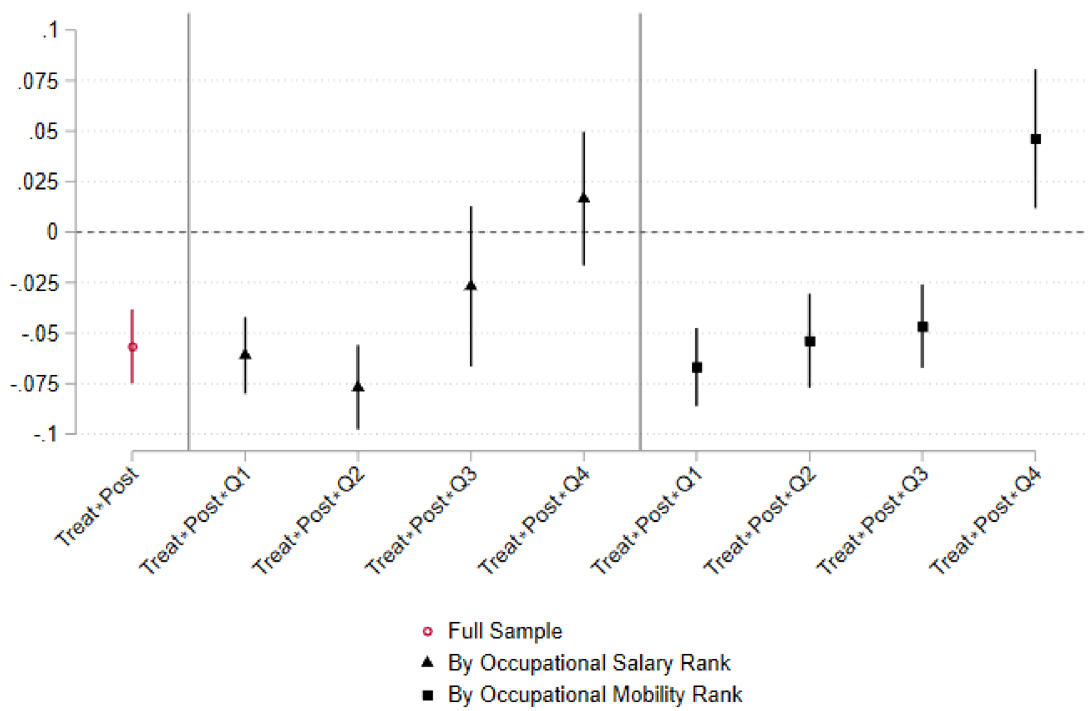


Table 1: Average of posted nominal annual salary by commuting zone-based treatment and time of treatment, 2010-2022.

	Pre-treatment		Post-treatment	
	Untreated CZs	Treated CZs	Untreated CZs	Treated CZs
Posted annual salary	42,468	50,976	42,098	40,563
	(38,727)	(41,880)	(28,575)	(24,880)
N	14,099	82,070	91,797	1,217,493

SD in parentheses

Table 2: Occupations with the lowest and highest leave shares as reported by Schubert et al. (2024)

Twenty Occupations with the lowest leave share		Twenty occupations with the highest leave share	
Occupation title	Leave share	Occupation title	Leave share
Dental hygienists	0.062	Installation, maintenance, and repair workers, all other	0.29
Nurse practitioners	0.088	Parts salespersons	0.29
Pharmacists	0.09	Billing and posting clerks	0.29
Firefighters	0.098	Data entry keyers	0.29
Self-enrichment education teachers	0.1	Cashiers	0.29
Physical therapists	0.11	Insurance claims and policy processing clerks	0.3
Postsecondary teachers, all other	0.11	Stock clerks and order fillers	0.3
Graphic designers	0.12	Packers and packagers, hand	0.3
Emergency medical technicians and paramedics	0.12	Cooks, institution and cafeteria	0.3
Fitness trainers and aerobics instructors	0.13	Helpers production workers	0.31
Licensed practical and licensed vocational nurses	0.13	Sales rep., wholesale mfg., tech. scient. products	0.31
Lawyers	0.13	Hosts and hostesses, restaurant, lounge, and coffee shop	0.31
Registered nurses	0.13	Shipping, receiving, and traffic clerks	0.31
Health specialties teachers, postsecondary	0.13	Loan interviewers and clerks	0.32
Physicians and surgeons, all other	0.14	Counter attendants, cafeteria, food concession, and coffee shop	0.32
Heavy and tractor-trailer truck drivers	0.14	Bill and account collectors	0.32
Radiologic technologists	0.14	Tellers	0.32
Hairdressers, hairstylists, and cosmetologists	0.14	Machine setters, operators, and tenders	0.32
Coaches and scouts	0.14	Telemarketers	0.36
Chief executives	0.15	Food servers, nonrestaurant	0.45

Note: Adapted from "Employer Concentration and Outside Options," by G. Schubert, A. Stransbury, and B. Taska, 2024, p.90 (http://dx.doi.org/10.2139/ssrn.3599454)

Table 3: 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 × 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	1,244,816	1,244,773	1,240,526	1,217,703
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

Note: Robust standard errors clustered at the commuting zone-by-occupation (6-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 4: DiD estimates – Heterogeneous effects by occupational salary rank

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post × Q1 salary	-0.318***	-0.107***	-0.126***	-0.0610***
	(0.0172)	(0.0150)	(0.0116)	(0.00969)
Treat × Post × Q2 salary	0.00367	-0.147***	-0.137***	-0.0768***
	(0.0176)	(0.0160)	(0.0124)	(0.0106)
Treat × Post × Q3 salary	0.0843***	-0.117***	-0.0850***	-0.0269
	(0.0189)	(0.0187)	(0.0243)	(0.0202)
Treat × Post × Q4 salary	0.186***	-0.258***	-0.0403**	0.0165
	(0.0198)	(0.0204)	(0.0192)	(0.0169)
Constant	10.65***	10.60***	10.59***	10.54***
	(0.0153)	(0.0127)	(0.00998)	(0.00828)
Observations	1,244,816	1,244,773	1,240,526	1,217,703
R-squared	0.231	0.341	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

Notes: Occupations (six-digit SOC) are ranked into four quartiles based on the occupation's average annual salary for the year 2015 –the year during which the merger took place– using the OEWS wage estimates. Based on the OEWS national wage estimates, the 25^{th}, 50^{th}, and 75^{th} percentiles of the 2015 annual wage mean are $35,140, $48,150, and $69,060, respectively. Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 5: DiD estimates – Heterogeneous effects by outward occupational mobility rank, measured based on the leave share estimates reported by Schubert et al. (2024)

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.188***	-0.121***	-0.112***	-0.0498***
	(0.0175)	(0.0143)	(0.0113)	(0.00943)
Treat × Post × Low leave share	0.349***	-0.265***	-0.0499**	-0.0598***
	(0.0224)	(0.0287)	(0.0218)	(0.0192)
Treat × Post × High leave share	-0.184***	0.0232***	-0.0237***	-0.0159**
	(0.00793)	(0.00891)	(0.00744)	(0.00738)
Constant	10.67***	10.60***	10.59***	10.54***
	(0.0163)	(0.0126)	(0.00999)	(0.00830)
Observations	1,244,816	1,244,773	1,240,526	1,217,703
R-squared	0.119	0.342	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

Notes: Low leave share is a dummy variable that takes the value one for the 20 occupations with the lowest leave share. High leave share is a dummy variable that takes the value one for the 20 occupations with the highest leave share. A full list of those occupations is shown in Table 2. Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 6: DiD estimates – Heterogeneous effects by outward occupational mobility rank, measured by the concentration ratio of the top-four transition shares for each occupation

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post × Q1 mobility	-0.281***	-0.121***	-0.131***	-0.0668***
	(0.0172)	(0.0153)	(0.0118)	(0.00984)
Treat × Post × Q2 mobility	-0.250***	-0.173***	-0.115***	-0.0538***
	(0.0179)	(0.0175)	(0.0138)	(0.0119)
Treat × Post × Q3 mobility	0.0209	-0.147***	-0.108***	-0.0466***
	(0.0176)	(0.0155)	(0.0123)	(0.0105)
Treat × Post × Q4 mobility	0.142***	-0.125***	-0.0158	0.0462***
	(0.0247)	(0.0188)	(0.0184)	(0.0176)
Constant	10.65***	10.61***	10.59***	10.54***
	(0.0154)	(0.0127)	(0.00999)	(0.00831)
Observations	1,244,816	1,244,773	1,240,526	1,217,703
R-squared	0.171	0.340	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

Note: Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 7: Top five occupations in terms of observation count for each combination of salary and mobility quartiles

Salary Quartiles	Mobility Quartiles							
	Q1		Q2		Q3		Q4	
	Occupation Title	Obs.	Occupation Title	Obs.	Occupation Title	Obs.	Occupation Title	Obs.
Q1	Retail Salespersons	214,041	Cashiers	60,654	Receptionists and Information Clerks	4,276	Protective Service Workers, All Other	13,957
	Stock Clerks and Order Fillers	87,523	Merchandise Displayers and Window Trimmers	17,855	Industrial Truck and Tractor Operators	2,400	Meat, Poultry, and Fish Cutters and Trimmers	1,179
	Pharmacy Technicians	80,366	Bakers	17,439	Hosts and Hostesses, Restaurant, Lounge, and Coffee Shop	1,795	Food Batchmakers	381
	Customer Service Representatives	66,964	Driver-Sales Workers	10,780	Cooks, Short Order	1,245	Slaughterers and Meat Packers	152
	Combined Food Preparation and Serving Workers, Including Fast Food	33,240	Counter Attendants, Cafeteria, Food Concession, and Coffee Shop	8,725	Bartenders	1,144	Furniture Finishers	89
Q2	Heavy and Tractor-Trailer Truck Drivers	14,210	Sales and Related Workers, All Other	5,767	First-Line Supervisors of Retail Sales Workers	186,983	Chefs and Head Cooks	1,615
	Maintenance and Repair Workers, General	7,026	Installation, Maintenance, and Repair Workers, All Other	1,084	Bookkeeping, Accounting, and Auditing Clerks	3,833	Licensed Practical and Licensed Vocational Nurses	1,113
	Secretaries and Administrative Assistants	4,228	Dispatchers, Except Police, Fire, and Ambulance	740	Heating, Air Conditioning, and Refrigeration Mechanics and Installers	3,466	Human Resources Assistants, Except Payroll and Timekeeping	630
	Automotive Service Technicians and Mechanics	2,552	Bill and Account Collectors	728	Home Appliance Repairers	2,426	Procurement Clerks	392
	Automotive Body and Related Repairers	1,791	Construction Laborers	480	Opticians, Dispensing	1,158	Mechanical Door Repairers	80
Q3	Production, Planning, and Expediting Clerks	1,257	First-Line Supervisors of Office and Administrative Support Workers	6,430	Sales Representatives, Wholesale and Manufacturing, Except Technical and Scientific Products	29,911	Human Resources Specialists	4,361
	Career-Technical Education Teachers, Postsecondary	1,006	Food Service Managers	6,058	Sales Representatives, Services, All Other	5,768	First-Line Supervisors of Production and Operating Workers	1,493
	Meeting, Convention, and Event Planners	808	Computer User Support Specialists	3,648	Purchasing Agents, Except Wholesale, Retail, and Farm Products	1,849	First-Line Supervisors of Transportation and Material-Moving Machine and Vehicle Operators	661
	Public Relations Specialists	663	Industrial Engineering Technologists and Technicians	3,432	Advertising Sales Agents	1,714	First-Line Supervisors of Construction Trades and Extraction Workers	343
	Fine Artists, Including Painters, Sculptors, and Illustrators	646	First-Line Supervisors of Mechanics, Installers, and Repairers	1,594	First-Line Supervisors of Helpers, Laborers, and Material Movers, Hand	1,603	Lodging Managers	285
Q4	Managers, All Other	4,275	Pharmacists	23,055	General and Operations Managers	15,643	Marketing Managers	5,414
	Power Plant Operators	355	Administrative Services Managers	1,171	Business Operations Specialists, All Other	5,846	Computer Occupations, All Other	4,436
	Producers and Directors	300	Writers and Authors	615	Registered Nurses	3,473	Sales Managers	4,034
	Physician Assistants	281	Operations Research Analysts	480	Medical and Health Services Managers	2,719	Software Developers, Applications	3,453
	Social and Community Service Managers	74	Compliance Officers	458	Market Research Analysts and Marketing Specialists	2,666	Purchasing Managers	3,339

Table 8: DiD estimates – Heterogeneous effects by occupational salary rank and outward occupational mobility rank, measured based on the concentration ratio of the top-four transition shares for each occupation

	Mobility Quartiles			
Salary Quartiles	Q1	Q2	Q3	Q4
Q1	-0.0586*** (0.00980)	-0.0708*** (0.0123)	-0.0614*** (0.0189)	-0.0507** (0.0256)
Q2	-0.159*** (0.0215)	-0.0773*** (0.0242)	-0.0561*** (0.0105)	-0.122*** (0.0349)
Q3	-0.0471* (0.0283)	-0.0365** (0.0154)	-0.0213 (0.0374)	0.00815 (0.0278)
Q4	0.00819 (0.0478)	-0.00925 (0.0294)	-0.00135 (0.0196)	0.110*** (0.0231)
Observations	1,217,703			
R-squared	0.564			
CZ FE	YES			
Year-Quarter FE	NO			
Occupation FE	NO			
Occupation-by-YQ FE	YES			
Employer FE	YES			

Note: Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 9: DiD estimates for annual-pay vacancies (Estimating equation 1 for the sub-sample of annual-pay vacancies 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	-0.301***	-0.133***	-0.0922***	-0.0344**
	(0.0270)	(0.0230)	(0.0184)	(0.0166)
Constant	10.91***	10.76***	10.72***	10.66***
	(0.0245)	(0.0212)	(0.0170)	(0.0153)
Observations	458,941	458,882	455,869	445,625
R-squared	0.200	0.567	0.642	0.734
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

Note: Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 10: DiD estimates for hourly-pay vacancies (Estimating equation 1 for the sub-sample of hourly-pay vacancies 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	-0.118***	-0.0891***	-0.0819***	-0.0298***
	(0.0136)	(0.0116)	(0.00976)	(0.00849)
Constant	10.51***	10.48***	10.48***	10.43***
	(0.0126)	(0.0102)	(0.00844)	(0.00737)
Observations	758,522	758,460	754,620	737,260
R-squared	0.221	0.430	0.496	0.655
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

Note: Robust standard errors clustered at the commuting zone-by-occupation (six-digit SOC) level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1