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

Enas Farag † Alaa Abdelfattah‡ Chris Compton§

Anna Stansbury ¶ Marshall Steinbaum ‖

August 23, 2024

We analyze the labor market impact of CVS Health's acquisition of Target's pharmacy business in December 2015, using Lightcast job posting data. 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 4% in our preferred specification. We test for heterogeneous merger effects by treatment intensity and by occupational characteristics. Commuting zones where both CVS and Target had significant presence pre-merger experience worse pay outcomes following the merger. Further, occupations with lower pay, lower outward mobility, and whose outside job options were impacted the most by the merger exhibit more pronounced negative effects on pay.

*We have departed from the alphabetic norm for author order in acknowledgment of Enas Farag's effort on this project, which have unanimously earned her priority of authorship. We thank Wei Xiong and session participants at the International Industrial Organization Conference in 2024 for their feedback on this paper.

†University of Illinois Urbana-Champaign and Faculty of Economics and Political Science, Cairo University.

‡NYU Abu Dhabi and Occidental College.

§Lightcast.

¶MIT Sloan School of Management.

‖University of Utah Department of Economics.

1 Introduction

For a long time, a standard assumption of many labor market models was perfect competition, from which we derive that wages equal the marginal product of labor. In the 1990s, scholars began to challenge this assumption by building theoretical models and providing empirical evidence that support the existence of labor market monopsony. A seminal theoretical framework was Manning (2003)'s dynamic monopsony model with a job ladder, which shows that all workers experience a wage markdown save the special case where workers receive infinitely-frequent outside job offers and hence wages equal to their full marginal product of labor in equilibrium. Manning's groundbreaking theoretical work was later supported by several empirical studies that estimate a finite, low labor supply elasticity to individual firms—contradicting 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 and acquisitions (M&A) between employers in the same labor market, which reduces the number of employers and potentially increases the market power of remaining ones (Azar & Marinescu, 2024). Yearly, about 2% of workers work in establishments that engage in M&A activity (Arnold, 2021). Since the 1980s, the retail pharmacy industry has been one of the sectors to experience a series of consolidations that changed the industry market structure significantly (Zhu & Hilsenrath, 2015). To the best of our knowledge, this is the first formal study that estimates the effect of a single large merger on labor market outcomes, as well as the first study evaluating the effect of retail pharmacy consolidation on workers in the United States.

In this paper, we aim to fill this gap in the literature by using Lightcast online job posting data to study the effect of a single merger of two large national retail pharmacy chains in the United States, CVS Health and Target, on posted pay.[1] We evaluate CVS Health's

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

acquisition of 1,672 Target in-store pharmacies in 47 states across the US on December 16, 2015 (Target, 2015). While the acquisition took place nationally, it affected local labor markets differently since some commuting zones had both chains before the merger while others had only one of the chains or neither. Using this variation, we construct a difference-in-differences (DiD) model in which treated commuting zones are those where we observe at least one pharmacy-related vacancy for both CVS and Target during the year leading up the merger (January 1, 2015 – December 15, 2015), and all other commuting zones serve as controls. This leaves us with 234 treated commuting zones and 475 control commuting zones.

Using Lightcast data, which covers the near-universe of online job postings in the U.S. from 2010–2022, [2] we first test if the merger resulted in reductions in posted pay, thus providing evidence that employer consolidation diminishes labor market competition. Second, we investigate whether the merger's effect on posted pay differed by treatment intensity. Third, we test for heterogeneous merger effects by occupation. The dimensions of occupational heterogeneity we consider are average pay (high-wage versus low-wage occupations, ranked using Occupational Employment Statistics from BLS), mobility (defined as the share of workers who leave the occupation when they leave a job in the occupation, using Schubert et al. (2024)'s data built from resumes/job histories), and outside job options (defined as varying degree of treatment intensity within workers' current focal occupation and potential destination occupations).

We find that posted pay for new hires in the retail sector dropped by 4% as a result of the merger in the affected commuting zones. The merger effect was more pronounced (i.e., larger reductions in posted pay) in commuting zones where both merging parties had sig-

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., 2024; 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.

nificant presence pre-merger. Our event study analysis with alternative fixed effects shows no evidence of differential pre-merger pay trends in any of the specifications. Further, we show that the average effect of the merger on posted pay masks considerable differences based on occupational characteristics. First, pay for occupations that belong to the first and second quartiles of pay distribution decrease by 4.5% and 6.4% respectively, whereas the merger effect on high-wage occupations is not significantly different from zero. Second, the estimated merger effect for occupations with the lowest outward occupational mobility is a 7% pay reduction. Lastly, occupations whose outside job options are the most affected by the merger, because the merging parties are large employers in those outside job options, experience larger pay reductions. Overall, this paper demonstrates the role of occupational heterogeneity in understanding the impact of increasing labor market concentration through mergers on posted pay.

The contribution of this paper is threefold. First, it adds to the merger retrospective literature, as one of few papers to focus on the labor market effects of mergers. Second, it provides well-identified estimates of the effects of a change in labor market concentration on wages (in a context where compelling causal identification is rare). Third, it contributes to the literature on which occupations are most adversely affected by employer monopsony power.

The rest of the paper is organized as follows. Section 2 reviews 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 and event study results for our baseline model and treatment intensity specifications, and Section 5 tackles the dimensions of occupational heterogeneity. Section 6 discusses the implications of our findings on labor markets. Section 7 concludes.

2 Literature Review

The recent availability of micro-level data covering labor markets, such as online job vacancies data and matched employer-employee data, catalyzed 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 only 2.3 recruiting firms with equal market shares.[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. Using Longitudinal Employer-Household Dynamics (LEHD) data, Arnold (2021) employs a matched difference-in-differences strategy to analyze the wage and employment effects of (all) mergers taking place between 1999 and 2009 in the United States. He finds that the extent to which wages are affected by mergers depends on the change in the level of concentration in the labor market. This finding motivates our treatment intensity specification.

Focusing on mergers in the healthcare sector, 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:

[3] That paper defines labor markets by 6-digit SOC occupation, commuting zone, and quarter.

[4] According to the Department of Justice / Federal Trade Commission 2023 merger guidelines (U.S. Department of Justice & Federal Trade Commission, 2023), a market with an HHI level above 1800 is considered a *highly* concentrated market.

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, 2021). 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.

While to our knowledge ours is the only paper that studies the effect of retail pharmacy mergers in the US, there are two papers that study retail pharmacy consolidation (and de-consolidation) in Sweden and Brazil, respectively. Thoresson (2024) exploits the regulatory reform of the Swedish pharmacy market in 2009 that ended the government monopoly 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]

Guanziroli (2022) estimates the labor market effect of a merger between two large retail pharmacy chains in Brazil. He adopts 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 paper utilizes 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. The author finds that the wages of pharmacists dropped by 2.6% and that of salespeople decreased by 3.5%.

[5]Thoresson (2024) defines local labor markets as the intersection between the industry of dispensing chemists and commuting zones in Sweden.

Second, this paper relates to the rich literature that studies the effect of local labor market concentration on wages (e.g. Azar, Marinescu, & Steinbaum, 2022; Benmelech et al., 2022; Macaluso et al., 2019; Rinz, 2022; Schubert et al., 2024).[6] A key threat to identification for these studies is the possible endogeneity of the determinants of local labor market concentration and pay. Three primary strategies have been used in the empirical literature to date: using mergers as an instrument for variation in concentration (Arnold, 2021; Benmelech et al., 2022); using large employers' national hiring growth as an instrument for variation in local employer concentration (Schubert et al., 2024); and using changes in national occupation- or industry-level concentration to instrument for local labor market concentration (Azar, Marinescu, & Steinbaum, 2022; Rinz, 2022). Our paper thus falls in the first category: we address endogeneity concerns by studying a merger-induced change in concentration.

Third, the literature does not provide a clear understanding of which type of occupations suffer the most from employer concentration. 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. Those authors find that greater merger exposure leads to earnings losses for the occupations with few options outside of hospital employers: skilled medical personnel and skilled non-health pro-

[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.

fessionals. 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. But Guanziroli (2022) showed that the wages of salespeople declined more than that of pharmacists following the merger he studied.

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.

Our heterogeneity analysis is designed to disentangle three relevant sources of occupational heterogeneity: status as measured by occupational salary rank, outward occupational mobility using Schubert et al. (2024)'s leave share data (share of workers who leave the occupation when they leave a job in the occupation), and treatment intensity in outside job options, taking into consideration whether workers in each occupation have the option to take other jobs within the same occupation or in other adjacent occupations. Our findings about the CVS–Target merger's effect on occupations with different degrees of labor market mobility validate the interpretation provided by Guanziroli (2022), Prager and Schmitt (2021), and Schubert et al. (2024): the easier it is for workers to take a job in a different occupation, the less harmed they were by the merger. Further, workers whose outside options were most impacted by the merger were harmed more. Where our findings differ from Prager and Schmitt (2021) is that low-wage workers were significantly harmed by the CVS-Target merger.

3 Empirical Strategy

3.1 Data

Our main data source is online job posting data from Lightcast,[8] henceforth LC, an employment analytics and labor market information company. LC collects data from roughly 51,000 websites, including job boards and company pages such that it covers the near-universe of online job postings from 2010 to 2022 for all areas in the United States. For each job posting, we have information you would expect to find in a job ad such as education level, experience expected as well as a set of required and preferred skills. More relevant to our analysis, LC standardizes posting information to provide us with listing date, employer name, job title codified into six-digit SOC, location linked to Federal Information Processing System (FIPS) code, four-and six-digit North American Industry Classification System (NAICS) codes, and posted salary, annualized under the assumption of full-time work.[9] This posted salary variable is our main outcome variable.

The nature of Lightcast job posting data comes with two advantages and drawbacks. The first advantage is that on average, LC data captures 92.6% of the monthly job openings reported by the Job Openings and Labor Turnover Survey (JOLTS) (Lightcast, 2024) and is statistically representative of the labor market in the United States during the period 2010–2019 (Cammeraat & Squicciarini, 2021). But unlike JOLTS, which is typically available only at aggregate levels (like occupations, industries or states), LC is available at the vacancy level with information on each opening's date, industry (6-digit NAICS), county, occupation, and pay. The latter's granular geographical level is essential for our identification strategy since our "treatment" happens at commuting zone level.

[8] Previously known as Burning Glass Technologies (BGT).

[9] LC reports posted annual salaries in the form of a lower and upper bound salary range. When 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.

The second advantage of using LC data is that the posted wage is often a more elastic indicator of variation in labor market competition, compared to realized earnings of incumbent workers. Callaci et al. (2024) show that immediately following the removal of no-poaching restrictions in franchise contracts, franchisee-employers raised pay posted in job ads for exactly workers they were most likely prohibited from hiring ex-ante, store managers. Furthermore, Lightcast's posted salary data closely reflects changes in the salaries of new hires identified in the Current Population Survey (CPS) data and the Quarterly Workforce Indicators (QWI) with a correlation close to one (Hazell & Taska, 2020).[10] Thus, Lightcast posted salary data aligns well with the realized pay of new hires, capturing exactly our outcome of interest.

On the other hand, a well documented flaw of LC data is that it tends to under-represent industries where offline postings and word-of-mouth are still common in hiring (e.g., construction) and over-represent white collar jobs (e.g., Professional and Business Services). While this is a valid concern, Hershbein and Kahn (2018) show that compared to CPS data, LC data representativeness seems to be time-invariant at the occupational level and hence not a likely explanation for our estimated results (since our identifying variation is across commuting zones and over time, but within occupations).

A second and more central criticism of the LC data is that only 24% of the vacancies between 2010 and 2022 have posted pay information. The sparsity of wage/salary information is particularly concerning because the proportion of vacancies with salary information significantly increases after 2018 (Batra et al., 2023). This increase in the prevalence of posting pay in job ads coincides with some states adopting laws mandating salary transparency, as well as tight labor markets generally contributing to a growing norm of posting pay in job ads (Stahle, 2023). However, it also coincides with the addition of *LinkedIn* and *Indeed* to Lightcast source material; two job boards with a high share of imputed wages.[11]

[10] 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.

[11] Callaci et al. (2024) analysis of the posted salary in comparison to the body of the job ad for a small

Therefore, to assuage concerns about imputed wages, we adopt Callaci et al. (2024)'s solution and drop vacancies sourced from either LinkedIn or Indeed. Figure 1 shows the number of online vacancies with posted salary information in the retail sector (see Section 3.2 for more details) before and after dropping the vacancies sourced from either LinkedIn or Indeed over the sample period. The gap between the two series reflects the number of vacancies retrieved from either of those two job boards, which increases significantly after 2018. Table 1 reports the number of vacancies with posted salary information that are retrieved from either LinkedIn or Indeed for large retail pharmacy chains.

The wide variation in the frequency of pay-posting at the employer level could bias estimates that depend on firm-level posted pay to assign treatment (Batra et al., 2023). This is a concern that pertains more closely to the minimum wage literature, where the bite of the minimum wage is more severe for ex-ante lower-wage firms, so posted pay is used to assign treatment intensity. Batra et al. (2023) demonstrate that such a specification could bias estimates of minimum wage effects because units assigned to treatment would show increasing pay post-treatment due to mean reversion, rather than an increase in the minimum wage. This concern does not apply to our setting since treatment is assigned based on geography, not on posted pay at the firm level, and we do not estimate any employer-specific treatment effects. In fact, our preferred specification includes employer fixed effects, such that treatment effects are estimated within employers, leveraging geographic variation in exposure to the merger.

3.2 Sample Restriction

The primary goal of this paper is to evaluate the effect of CVS Health's acquisition of Target's pharmacy business on the posted pay of new local hires. Our research design uses geographical variation in exposure to this merger based on ex-ante existence of both CVS-

sample of the post-2018 data indicates these two job boards are responsible for nearly every case of posted salaries that are inconsistent with the job ad text.

Health and a Target (with a pharmacy) in a given commuting zone pre-merger. To this end, our data building process is done in two steps. Starting with 374 million vacancies from 2010 to 2022, we first restrict our sample to postings with populated FIPS code, employer name, and pay.[12] These restrictions allow us to correctly assign postings to treatment and control, include employer fixed effects, and have a populated outcome variable respectively. We further limit our sample to job ads with occupation (six-digit SOC) information such that we can control for any effects of occupation on posted wages.

Since we are studying the effect of a merger between two large retail pharmacies on posted pay, our second data-building step is using the four-digit NAICS code to restrict our sample to include vacancies from the following retail industries: food and beverage retailers, health and personal care retailers, and other general merchandise stores including department stores and warehouse clubs. This leaves us with approximately 740,000 online vacancies posted between 2010 and 2022. We limit our sample only to retail industries—excluding vacancies from pharmacies in hospitals—because retail health professionals cannot easily move to the general medical and surgical hospital industry. The remainder of this section will discuss in detail barriers to entry by pharmacists and pharmacy technicians into the hospital industry.

For pharmacists, there are two key barriers to switching from being a retail pharmacist to a hospital pharmacist (also known as a clinical pharmacist): a residency training requirement and task differences. For the former, hospitals usually require at least one year of residency training before hiring clinical pharmacists. To overcome this barrier, a retail pharmacist must 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). As for task-based challenges, the day-to-day duties of a retail pharmacist differ from those of a clinical pharmacist. While the most common tasks conducted by community pharmacists[13] are ad-

[12] Dropping vacancies sourced from LinkedIn and Indeed to assuage concerns of imputed wages as discussed in section 3.1

[13] This is the term used to refer to pharmacists who work in independent pharmacies, chain pharmacies,

ministering vaccines, providing patient medication assistance, dispensing Naloxone, and providing medication therapy management, the three most common services provided by pharmacists working in hospitals are drug level monitoring, therapeutic drug interchange, and ordering laboratory tests (Arya et al., 2020). In short, retail pharmacists facing an increase in employer monopsony power cannot easily switch jobs to become hospital pharmacists.

The second largest pharmacy occupation is pharmacy technician. It can also be difficult for retail pharmacy technicians to switch to work as hospital pharmacy technicians. Hospital pharmacy technician jobs require additional responsibilities beyond a retail pharmacy technician's main responsibility of filling prescriptions for patients. According to the American Society of Health System Pharmacists, these can include preparing sterile medications, operating pharmacy automation systems, obtaining medication histories, facilitating transitions of care, diversion prevention, and supply chain management (American Society of Health-System Pharmacists, 2024).

These differences are reflected in large differences in earnings for workers across the two different sectors. Looking at Figure 2, from the BLS Occupational Employment and Wage Statistics, shows that the average annual salary of pharmacy technicians working in the general medical and surgical hospitals industry is substantially higher than the average annual salary of those employed in retail industries. If labor mobility is easy and feasible across industries for pharmacy technicians, we should not observe this persistent pay disparity between the hospital industry and retail industries. Therefore, Figure 2 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.

---

mass merchandisers, supermarkets, or health system retail.

3.3 Methodology

To estimate the effect of CVS's acquisition of Target's pharmacy business, we employ a difference-in-differences research design using commuting-zone-based treatment, applying the empirical strategy implemented by Prager and Schmitt (2021). We adapt their methodology 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 commuting zones.

Prior to the CVS–Target merger, both parties existed in some commuting zones but not in others.[14] Accordingly, commuting zones where both Target and CVS had at least one establishment before the merger experienced an increase in employer concentration post merger, whereas commuting zones where only one party or neither existed did not experience a change in employer concentration. Using the employer's name and geographic identifiers available in the LC dataset, we define our treatment as commuting zones where both CVS and Target posted at least one pharmacy-related[15] vacancy during the period January 1, 2015–December 15, 2015, indicating that both chains had at least one establishment in that geographic labor market in the year preceding the acquisition. All other commuting zones form our control group. Our treatment group consists of 234 commuting zones and our control group consists of 475 commuting zones.

Table 2 reports average posted pay pre- and post-merger for the treatment and control group. Figure 3 depicts the trends of the posted annual salary for both the treated and control commuting zones over the period 2010–2022. Before the merger, the treated observations have higher average salaries but followed the same trend as the control observations. However, we see substantial convergence in salaries after the merger for the

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

[15] The pharmacy-related occupations we consider are pharmacists (SOC: 29-1051), pharmacy technicians (SOC: 29-2052), and pharmacy aides (SOC: 31-9095). We restrict to pharmacy-related postings since not all Target locations have an in-store pharmacy.

treated and control commuting zones. Our empirical strategy outlined below is designed to estimate the degree to which this relative decline in pay for treated observations can be explained by the merger.

The procedure above categorizes commuting zones as either treated or not-treated by the merger. In addition, we are interested in estimating the effect of the merger based on the degree to which commuting zones are affected. Therefore, we calculate the share of vacancies posted by both CVS and Target out of all the vacancies posted in a given commuting zone in 2015,[16] then compute the treatment intensity as Δ HHI, which is twice the product of the ex-ante vacancy shares of each party. This measure, one of the screens for prospective competitive effects of mergers evaluated under the Merger Guidelines,[17] is higher whenever each of the merging parties' vacancy shares is high; importantly, it also has the property that it increases in the symmetry of the merging parties' pre-merger vacancy shares.[18] We use the median of this treatment intensity measure to divide the treated commuting zones into two groups reflecting varying degrees of treatment intensity (i.e., below-median and above-median). Commuting zones in which the treatment intensity measure is above the median value are those in which the merger caused significant consolidation in the retail labor market because both CVS and Target had significant presence before the merger. Table 3 reports summary statistics for both treatment intensity groups.

The core identifying assumption underlying our difference-in-difference estimation is that posted wages in treated commuting zones would not have evolved differently than the posted wages in control commuting zones in the absence of the merger, conditional on our fixed effects. This could happen if places which had both Target and CVS pharmacies

[16]Vacancy shares are calculated as the share of vacancies posted by each merging party separately out of all the total retail-industry job vacancies (the retail industries mentioned in Section 3.2) posted in a given commuting zone for each quarter of 2015, where the last quarter is only considered until December 15, 2015. Then, the each party's merging share is averaged over the 4 quarters of 2015.

[17]See pages 5 and 6 of U.S. Department of Justice and Federal Trade Commission (2023).

[18]This is important because, for example, a merger where CVS and Target both have vacancy shares of 10% pre-merger, leading to a combined vacancy share of 20% post-merger, reflects a much larger change in the competitive environment than a merger where Target had 1% and CVS had 19% ex ante.

in 2015 were different on many dimensions to places that did not have both. To address this concern, we employ commuting zone fixed effects in all specifications, thus holding constant any time invariant differences across commuting zones. Nonetheless, commuting-zone fixed effects don't resolve the threat if treated commuting zones were on differential economic trends relative to the control group. Specifically, to invalidate a finding of a negative wage effect of the merger, it would need to be the case that retail wages in untreated commuting zones were on a faster growth trend than retail wages in treated commuting zones in 2016-2022 after the merger took place, but not in 2010-2015 before the merger.

A likely possibility that could prompt treated and control commuting zones to trend differently is that places with both Target and CVS stores are more urban since 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 lack 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). To address this concern, we estimate quarterly event studies, showing that treated and control commuting zones had parallel pre-trends in posted pay for the five years prior to the merger. Moreover, in Appendix A, we allow for pay to differ more flexibly across urban and rural areas to account for the possibility of divergent pay trends in treatment and control commuting zones.

Similarly, for our treatment intensity results, the core identifying assumption is that posted salaries in commuting zones with above median HHI would not have evolved differently than the posted wages in commuting zones with below median HHI, conditional on our fixed effects. In Figure 4, we look at the trend in the average annual salary for the two groups of treated commuting zones based on treatment intensity. More-treated commuting zones had higher posted pay before the merger, as compared to less-treated commuting zones. Visual inspection of raw data suggests parallel trends prior to the merger. This is confirmed by our event-study estimates in the results section.

3.4 Specification

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

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

The dependent variable is the log of the posted pay for vacancy *i* posted by employer *e* that belongs to occupation *o* located in commuting zone *c* at time *t*, which is defined on a quarterly basis. $Post_t$ is an indicator variable that takes the value one for observations after December 16, 2015. $Treat_c$ indicates whether the commuting zone for each observation is treated or not. β is our coefficient of interest. α_c represents commuting zone fixed effects, which control for static wage differentials across commuting zones due to factors unrelated to their exposure to the merger. γ_{ot} represents occupation-by-year-quarter fixed effects, which control for differential occupational wage trends over time. μ_e represents employer fixed effects, which control for employer-specific wage policies. Standard errors are clustered at the commuting-zone level.

To test the parallel trends assumption, we expand our DiD model into an event study specification that estimates a separate treatment effect for each quarter leading up to and following the merger. The event study specification takes the following form:

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

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.

Finally, we construct a treatment-intensity specification, where we interact the post-treatment indicator with a below-median and an above-median indicator of our treatment intensity measure.

$$ln(Salary_{ioect}) = \beta_1 \textit{Below Median}_c \times Post_t + \beta_2 \textit{Above Median}_c \times Post_t + \alpha_c + \gamma_{ot} + \mu_e + \epsilon_{ioect} \quad (3)$$

Below Median$_c$ is an indicator for vacancies in commuting zone c whose Δ HHI is below sample median and *Above Median*$_c$ is an indicator for vacancies in commuting zone c whose Δ HHI is above the sample median. Therefore, β_1 and β_2 capture the effect of the merger on commuting zones that did not experience significant concentration change and commuting zones in which the merger caused significant consolidation respectively. We further run this specification in event study format by expanding the pre-and post- period into year-quarters (similar to equation 2) to track parallel trends before the merger and dynamic treatment effects after.

4 Results

4.1 Baseline Model

Table 4 presents the estimation results of the baseline model (equation 1) with alternative fixed effects specifications. All specifications show large, negative, and statistically significant relative wage declines in treated vs. control commuting zones after the merger. The specification reported in the 4^{th} column is our preferred specification 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 on the post-treatment indicator for this specification suggests that the posted annual salary decreased as a result of the merger by approximately 4%, on average, over the 7 years following the merger.[19] Put differently, if the identifying assumptions for causal inference from a difference-in-differences methodology hold so that the control commuting zones constitute a valid counterfactual for what

[19] The dependent variable is in log form, so we exponentiate the coefficient for interpretation. Precisely, posted annual salary declined by $[e^{-0.0408} - 1] * 100 \approx -4\%$.

would have occurred in the treated commuting zones absent the merger, the merger reduced pay for new hires in the retail sector by 4%.

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 relative 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 by plotting the event study coefficients of equation 2 in Figure 5. The reference quarter for this estimation is the quarter preceding the merger, the third quarter of 2015. Each of the four sub-figures corresponds to the different specifications reported in the four columns of Table 4.

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 relative 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): wages in treated commuting zones rose more slowly than in control commuting zones as the economy recovered and the labor market tightened over the following years.[20] 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.

To address the concern that the merger effect we estimate during and after the pandemic could be driven by differential effects of either the pandemic itself or efforts to remediate it that happen to be correlated with the geography of the CVS-Target merger, we estimate

[20]Theory suggests that the more monopsonistic the employer, the larger the markdown of the wage from the marginal product; thus, that there is less passthrough of positive demand shocks to wages. To the extent that the post-COVID recovery period reflected rapid growth in aggregate demand (and thus in the marginal product of labor), our results would be consistent with treated commuting zones having become more monopsonistic, and thus workers in these commuting zones seeing less of a benefit from rising aggregate demand in terms of higher wages. Caldwell et al. (2024) contains a useful discussion.

a specification that controls for a proxy for pandemic-era geographic variation in labor market outcomes in Appendix B. Our results are still robust after adding stay-at-home-orders-by-time fixed effects, which control for the variation in the length of mandatory stay-at-home orders in March-May 2020 across commuting zones. In fact, the estimated merger effect increases to nearly 5% as shown in Table B1 and the event study plots in Figure B1 shows a more pronounced reduction in annual salary. We conclude that if anything, geographic variation in pandemic effects attenuates the estimated effect of the merger in our main specification.

4.2 Treatment Intensity Specification

We report estimates of equation 3 in Table 5 for the same four fixed effects specifications, and event study plots in Figure 6. Across all four specifications, the merger effect is larger for more-treated commuting zones. Specifically, in our preferred specification (column 4), the merger effect is larger (-4.3%) in the commuting zones in the top half of the treatment-intensity distribution than in the bottom half (-3.7%) – indicating that where the merger reduced labor market opportunities for retail workers the most, their pay correspondingly suffered more. Moreover, the event study estimates for the pre-treatment coefficients are consistent with the parallel trends assumption, indicating that more- and less-treated commuting zones were not on different pay trajectories prior to the merger.

5 Heterogeneous Effects of the Merger by Occupation

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 outside job options.

We test for heterogeneous merger effects by occupational salary rank to identify whether low-pay or high-pay occupations are more adversely affected by the merger. To answer this question, first we rank occupations (six-digit SOC) into four quartiles based on the occupation's average annual salary in 2015 using the OEWS wage estimates published by the BLS.

Second, we examine heterogeneity of the merger's effects by occupations' degree of outward mobility, using outward occupational mobility data from Schubert et al. (2024). Specifically, we segment occupations into four quartiles based on their *leave share*, estimated from resume data as the share of observations in year t that are observed in a different occupation in year $t + 1$.[21] To the extent that the merger increases CVS and Target's labor market power, we expect that the effects of the merger on posted annual salary will be greater for workers who had limited ability to leave their occupation (to avoid any potential wage-suppressive effects of the merger). Table 6 lists the top five occupations (six-digit SOC) for each quartile of the outward mobility (leave share) distribution.

Figure 7 visualizes the posted pay from LC job ads for each of the four quartiles of the leave share distribution. Occupations with the least degree of outward occupational mobility (i.e., lowest leave share) have the highest pay over the sample period. We observe a negative relationship between the posted pay and outward occupational mobility in figure 8, which is a binned scatterplot between the log of posted annual pay and occupational leave share for all the occupations in our sample. Occupations with low leave share tend to be those requiring greater experience and/or education, which workers are less likely to leave after they become qualified.

Finally, we calculate a version of our treatment intensity measure which varies by occupation *within* commuting zone. This measure, which we call the outside job option index (OJOI), estimates the extent to which the merger affects workers' outside options,

[21] Specifically, Schubert et al. (2024) use 16 million unique US workers' resumes collected by LC to construct a matrix of six-digit SOC by six-digit SOC observations in consecutive years. Our measure of outward mobility is defined by taking all consecutive-year pairs of person-occupation observations, and taking the share of all these observations which start in occupation o in year t for which there is an observation in a different occupation p in year $t + 1$. For more details on the LC resume data and the construction of these occupational mobility measures, see Schubert et al. (2024).

separately for each occupation in each treated commuting zone.[22] We construct the OJOI in two steps. First, we construct the direct analog of our treatment intensity measure explained in Section 3.3, separately for each occupation-by-commuting zone cell, computing occupation-specific treatment intensity $\Delta HHI_{p,c} = 2 \cdot \text{CVS}_{p,c,2015Q1-Q3} \cdot \text{Target}_{p,c,2015Q1-Q3}$ as twice the product of the share of vacancies posted by each of the merging parties for each destination occupation p in commuting zone c during 2015.[23] Next, we construct the OJOI for each focal occupation o and commuting zone c as a weighted average across these occupation-CZ-specific treatment intensity estimates, where the weights are workers' mobility flows from focal occupation o to each possible destination occupation p, denoted $\theta_{o \to p}$. We note that the inclusion of non-zero diagonal elements in this transition matrix, each of whose elements is used as a weight in computing $OJOI$, means that 'outside' refers to job options outside a notional pre-merger job, as opposed to outside a worker's occupation. These flows are once again from the six-digit SOC level occupational mobility data constructed by Schubert et al. (2024).[24] The OJOI is thus:

$$OJOI_{o,c,2015Q1-Q3} = \sum_{p}^{N_{occupations}} \theta_{o \to p} \times 2 \cdot \text{CVS}_{p,c,2015Q1-Q3} \cdot \text{Target}_{p,c,2015Q1-Q3} \tag{4}$$

Intuitively, we are trying to test for differential merger effects depending on the degree of monopsony power the merging parties enjoy as employers in the possible outside job options for workers affected by the merger. For instance, we know from the occupational mobility data constructed by Schubert et al. (2024) is that when pharmacists change jobs, the probability that they take another job in the Pharmacists occupation is 82%, followed

[22]Non-treated commuting zones do not have an OJOI since they lack pharmacy-related vacancies from either CVS or Target.

[23]We calculate the treatment intensity measure for each possible destination occupation p within each commuting zone c to capture the effect of the merger on the competitive landscape in all other possible outside job options in occupations that workers might move to (destination occupation p) if they leave their jobs in their current focal occupation o.

[24]Specifically, $\theta_{o \to p}$ is defined by taking all consecutive-year pairs of person-occupation observations, and calculating the share of all these observations which start in occupation o in year t for which there is an observation in specific occupation p in year $t+1$. In other words, $\theta_{o \to p}$ reflects the probability that an individual working in a focal occupation o in year t moves to a destination occupation p in year $t+1$, including movements within the same occupation (i.e., $o = p$).

by the Medical and Health Services Managers occupation with a corresponding probability of 1%. There are other numerous destination occupations that pharmacists move to but the probability of moving to any of them is less than 1%. Accordingly, we hypothesize that an individual whose focal occupation o is Pharmacists is likely to be more adversely affected by the merger because their potential outside jobs lie within destination occupations p where both CVS and Target are likely to be large employers. In contrast, the probability that a cashier stays in the same occupation when switching jobs is 62%, followed by plenty of other destination occupations where the merging parties are not likely to be significantly large employers.

In other words, higher values of OJOI means that the merging parties are relatively large employers in workers' relevant labor market, including both workers' current focal occupation and the other destination occupations that they tend to transition to. Accordingly, we expect the merger effect on pay to be larger (more negative) for the group of vacancies whose combination of occupation by commuting zones have values of OJOI higher than the sample median, signifying that the outside options most relevant to workers in occupation o in commuting zone c were worsened by the merger. Figure 9 depicts the pay trends over time for the combination of occupations by treated commuting zones with OJOI below and above median. On average, the average posted annual salary is lower for vacancies posted in occupations where both CVS and Target had significant presence before the merger.

For each dimension of heterogeneity discussed above, 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 outside job option index ranking.

5.1 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 7 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 in the retail industry is reduced by 4.5% for the quartile of occupations with the lowest average annual salary—occupations whose average annual salary were below $35,140 in 2015 according to the OEWS. As for the second quartile of occupations, where the average annual salary ranged between $35,140 and $48,150 in 2015 according to the OEWS, workers in the retail sector experienced a decline in average annual salary by 6.4%. Treatment effect estimates for the top two occupation salary quartiles are not significantly different from zero.

5.2 Heterogeneity by Outward Occupational Mobility

Table 8 presents the estimated DiD coefficients for each quartile of the occupational leave share distribution with alternative fixed effects specifications. Workers in occupations with the lowest probability of leaving their occupations when leaving their jobs are the most adversely affected by the merger. Column 4 shows that for occupations with the least degree of outward mobility, posted annual salary in the retail industries mentioned in Section 3.2 fall by nearly 7% in treated commuting zones relative to control commuting zones; the reductions in posted pay in more outwardly mobile occupations are around half that size.

5.3 Heterogeneity by Outside Job Option Index

The results of our heterogeneity analysis by the OJOI we construct in section 5 are presented in Table 9. In occupations for which the empirical analog to an occupation-specific labor market was more adversely affected by the merger, we see larger pay declines (-4.1% for occupations with above-median $OJOI$, versus -3.3% for occupations in the lower half of the $OJOI$ distribution). In other words, workers in occupations for which the outside job options are most affected by the merger because the merging parties enjoy a degree of employer concentration in the potential destination occupations experience worse pay outcomes.

Figure 10 visualizes the estimated merger effects for the baseline model along with the three dimensions of occupational heterogeneity. It plots the estimated coefficients from specification (4) of Tables 4, 7, 8, and 9. It shows that both lower-wage and lower-mobility occupations suffered disproportionate pay reductions from the merger. Further, workers in occupations where the merger limits outside job options due to the merging parties' employer power in those potential outside options face poorer pay outcomes.

6 Discussion

The results presented in Sections 4 and 5 contribute to the labor market monopsony literature in multiple ways. First, in line with Arnold (2021), we find evidence of a reduction in posted pay as a result of the merger. The posted annual salary for new hires in the retail sector in the commuting zones affected by the merger declined by 4% over the 7 years following the merger, relative to unaffected commuting zones. This estimated effect on posted pay excludes any variation in wage levels 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. Our estimate of the treatment-intensity specification (equation 3) shows larger pay reductions in commuting zones where the merging parties' pre-merger vacancy shares indicate the merger would have had the greatest effect on local labor market concentration. That corroborates the negative relationship between labor market concentration and wages established in the literature, using an identification strategy and associated specification that does not suffer from the endogeneity concerns that pervade static wage-concentration regressions, since the merger constitutes plausibly-quasi-exogenous variation in labor market concentration, and we control for unobserved employer heterogeneity in wage-setting. It also partly addresses identification concerns arising from geographic pay trends that may coincide with the geography of the merger, since we observe no differential pre-treatment pay trends between commuting zones that were differentially treated by the merger.

Second, we showed that the effect of the merger on posted pay depends on other occupational characteristics, such as occupational pay rank, occupational mobility rank, and outside job options. Figure 10 compares the estimated merger effect for the full sample to the estimated effects for those three dimensions of occupational heterogeneity. 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 share of workers in that occupation who move to a different occupation when they leave their job, experience the largest reduction in posted pay. This estimated pay reduction decreases as the degree of outward occupational mobility increases. Our findings are partly 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. 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 CVS-Target merger. Hence, our findings are not consistent with the view that because low-wage workers have many alternative employers, mergers between two of those many employers are unlikely to worsen their pay.

Third, the occupations whose outside options were most impacted by the merger show larger pay declines than occupations where workers could have moved more easily to other jobs across occupation lines, where the merger would have had less impact on employer concentration. This validates the approach to market definition taken by Schubert et al. (2024) in the sense that defining an occupation-specific labor market consisting of own-occupation jobs/employers and those in proximate occupations (as measured by cross-occupation worker flows) improves our understanding of the particular circumstances faced by differently-situated workers in response to a change in the market power of employers in the occupation where they work.

As Schubert et al. (2024) emphasize, workers most harmed by labor market concentration are likely those who i. work in occupations with a high degree of employer concentration, ii. are unlikely to leave those occupations if and when they leave their jobs, and iii. if they did leave, would move primarily to occupations that also feature high employer concentration. This paper can be interpreted as lending support to all three channels in a dynamic sense, using a retail mega-merger to proxy for a change in labor market concentration and therefore in labor market power. This paper provides a novel answer by disentangling the effect of each dimension of occupational heterogeneity.

7 Conclusion

In this paper, we analyze the effect of the acquisition of Target's retail pharmacy business by CVS Health in 2015 on the posted annual salary of new hires in the affected local labor markets. We employ 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, outward occupational mobility, and outside job options.

We find evidence of reduction in posted pay in the retail sector by 4%, on average, following the merger. The average merger effect on the posted annual salary conceals considerable variation 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. In addition, pay for workers in the top half of the distribution of outside job option exposure declined more than in the lower half. 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 occupational characteristics, including the accessibility of jobs outside a worker's current occupation.

References

Acemoglu, D., Autor, D., Hazell, J., & Restrepo, P. (2022). Artificial intelligence and jobs: Evidence from online vacancies. *Journal of Labor Economics*, *40*(S1), S293–S340.

American Society of Health-System Pharmacists. (2024). *Pharmacy technician career overview*. https://www.ashp.org/pharmacy-technician/about-pharmacy-technicians/pharmacy-technician-career-overview?loginreturnUrl=SSOCheckOnly

Arnold, D. (2021). *Mergers and Acquisitions, Local Labor Market Concentration, and Worker Outcomes* (Working Paper). https://darnold199.github.io/jmp.pdf

Arya, V., Bakken, B. K., Doucette, W. R., Gaither, C. A., Kreling, D. H., Mott, D. A., Schommer, J. C., & Witry, M. J. (2020). *National pharmacist workforce study 2019* (tech. rep.). American Association of Colleges of Pharmacy. https://www.aacp.org/sites/default/files/2020-03/2019_NPWS_Final_Report.pdf

Autor, D., Dube, A., & McGrew, A. (2023, March). *The Unexpected Compression: Competition at Work in the Low Wage Labor Market* (Working Paper No. 31010). National Bureau of Economic Research. Retrieved June 17, 2023, from https://www.nber.org/papers/w31010

Azar, J., Berry, S., & Marinescu, I. E. (2022). *Estimating Labor Market Power* (Working Paper No. 30365). National Bureau of Economic Research. Retrieved August 22, 2022, from https://www.nber.org/papers/w30365?utm_campaign=ntwh&utm_medium=email&utm_source=ntwg20

Azar, J., & Marinescu, I. (2024). Monopsony Power in the Labor Market [Forthcoming]. In *Handbook of Labor Economics*.

Azar, J., Marinescu, I., & Steinbaum, M. (2019). Measuring Labor Market Power Two Ways. *American Economic Association Papers & Proceedings*, *109*, 317–21.

Azar, J., Marinescu, I., & Steinbaum, M. (2022). Labor Market Concentration. *Journal of Human Resources*, *57*(S), S167–S199. https://doi.org/10.3368/jhr.monopsony.1218-9914R1

Azar, J., Marinescu, I., Steinbaum, M., & Taska, B. (2020). Concentration in US Labor Markets: Evidence from Online Vacancy Data. *Labour Economics*, *66*(101886). https://www.sciencedirect.com/science/article/pii/S0927537120300907

Batra, H., Michaud, A., & Mongey, S. (2023). *Online job posts contain very little wage information* (Working Paper No. 31984). National Bureau of Economic Research. https://doi.org/10.3386/w31984

Bean, J. (2021, December 13). *Comparing location strategies: Walmart vs. Target*. Towards Data Science. Retrieved March 16, 2023, from https://towardsdatascience.com/comparing-location-strategies-walmart-vs-target-d2bb00c9c7b3

Benmelech, E., Bergman, N. K., & Kim, H. (2022). Strong employers and weak employees: How does employer concentration affect wages? *Journal of Human Resources*, *57*(S), S200–S250.

Bounthavong, M. (2024). Despair and hope: Is the retail community pharmacy workforce in danger of becoming a monopsony labor market? *Journal of the American Pharmacists Association*, *64*(3), 102039. https://doi.org/10.1016/j.japh.2024.02.012

Caldwell, S., Dube, A., & Naidu, S. (2024). *Monopsony Makes It Big* (tech. rep.).

Callaci, B., Gibson, M., Pinto, S., Steinbaum, M., & Walsh, M. (2024). *The Effect of Franchise No-poaching Restrictions on Worker Earnings* (Working Paper). https://marshallsteinbaum.org/assets/franchise_no-poach_revised_6-2024.pdf

Cammeraat, E., & Squicciarini, M. (2021). Burning Glass Technologies' data use in policy-relevant analysis: An occupation-level assessment. *OECD Science, Technology and Industry Working Papers*, *No. 2021/05*. OECD Publishing, Paris. https://doi.org/https://doi.org/https://doi.org/10.1787/cd75c3e7-en

Clemens, J., Kahn, L. B., & Meer, J. (2021). Dropouts need not apply? the minimum wage and skill upgrading. *Journal of Labor Economics*, *39*(S1), S107–S149.

Evans, M. (2014, November 10). *Rural areas left behind mainstream drugstores*. Morning Consult. Retrieved March 16, 2023, from https://morningconsult.com/2014/11/10/rural-areas-left-behind-mainstream-drugstores/

Forsythe, E., Kahn, L. B., Lange, F., & Wiczer, D. (2020). Labor demand in the time of COVID-19: Evidence from vacancy postings and UI claims. *Journal of Public Economics*, *189*(104238). https://doi.org/10.1016/j.jpubeco.2020.104238

Guanziroli, T. (2022). Does labor market concentration decrease wages? Evidence from a retail pharmacy merger. https://conference.iza.org/conference_files/LaborMarkets_2022/guanziroli_t32516.pdf

Hazell, J., & Taska, B. (2020). Downward rigidity in the wage for new hires. *Available at SSRN 3728939*.

Hershbein, B., & Kahn, L. B. (2018). Do Recessions Accelerate Routine-Biased Technological Change? Evidence from Vacancy Postings. *American Economic Review*, *108*(7), 1737–1772. https://doi.org/10.1257/aer.20161570

Lightcast. (2024). *Representativeness Analysis of Lightcast Job Posting Data - U.S.* (tech. rep.).

Macaluso, C., Hershbein, B., & Yeh, C. (2019). *Concentration in U.S. local labor markets: Evidence from vacancy and employment data* (2019 Meeting Papers No. 1336). Society for Economic Dynamics. https://EconPapers.repec.org/RePEc:red:sed019:1336

Manning, A. (2003). *Monopsony in motion: Imperfect competition in labor markets*. Princeton University Press.

Phan, R. (2021, November). *Making the switch from retail to hospital pharmacist*. GoodRx Health. Retrieved November 1, 2023, from https://www.goodrx.com/hcp/pharmacists/retail-pharmacy-to-hospital-pharmacists

Prager, E., & Schmitt, M. (2021). Employer Consolidation and Wages: Evidence from Hospitals. *American Economic Review*, *111*(2), 397–427.

Rinz, K. (2022). Labor market concentration, earnings, and inequality. *Journal of Human Resources*, *57*(S), S251–S283.

Schubert, G., Stansbury, A., & Taska, B. (2024). Employer concentration and outside options. *Available at SSRN 3599454*.

Sokolova, A., & Sorensen, T. (2021). Monopsony in Labor Markets: A Meta-Analysis. *ILR Review*, *74*(1), 27–55. https://doi.org/10.1177/0019793920965562

Stahle, C. (2023, March). Pay Transparency in Job Postings Has More than Doubled Since 2020. Retrieved December 15, 2023, from https://www.hiringlab.org/2023/03/14/us-pay-transparency-march-2023/

Steinbaum, M. (2023). *Evaluating the Competitive Effect of the Proposed Kroger-Albertsons Merger in Labor Markets* (Working Paper). https://marshallsteinbaum.org/assets/kroger_albertsons_labor.pdf

Target. (2015, December 16). *CVS Health and Target announce completed acquisition of Target's pharmacy and clinic businesses* [Press Release]. https://corporate.target.com/article/2015/12/cvs-target-acquisition-complete

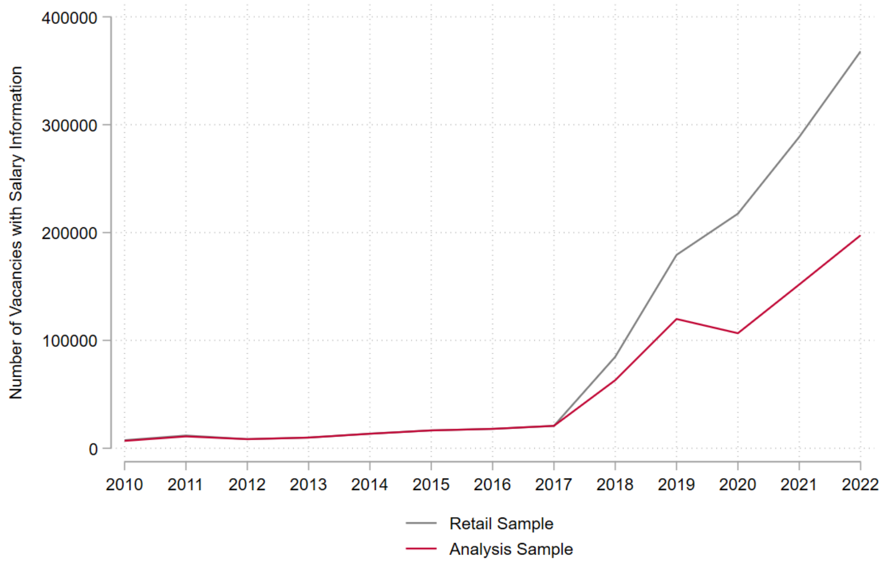
Thoresson, A. (2024). Employer concentration and wages for specialized workers. *American Economic Journal: Applied Economics*, *16*(1), 447–479. https://www.ifau.se/globalassets/pdf/se/2021/wp-2021-6-employer_concentration_wages-for-specialized-workers.pdf

U.S. Department of Justice & Federal Trade Commission. (2023, December). Merger Guidelines.

Zhu, P., & Hilsenrath, P. E. (2015). Mergers and Acquisitions in U.S. Retail Pharmacy. *Journal of Health Care Finance*, *41*(3).

Figures

Figure 1: Number of vacancies with posted salary information for the retail sample and analysis sample, 2010–2022.



The retail sample includes all the online vacancies with posted salary information and known occupations (6-digit SOC) and employers in the following retail industries: food and beverage retailers, health and personal care retailers, and other general merchandise stores including department stores and warehouse clubs as explained in Section 3.2. The analysis sample is a subset of the retail sample after dropping vacancies sourced from either LinkedIn or Indeed to eliminate vacancies with probable imputed salaries as per the discussion is Section 3.1.

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

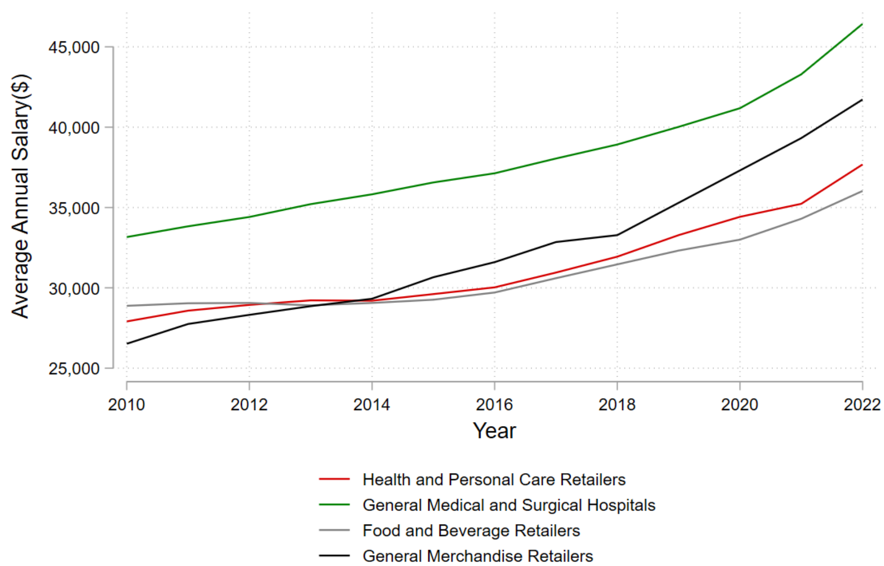


Figure 3: Average posted annual salary by commuting zone-based treatment, 2010–2022.

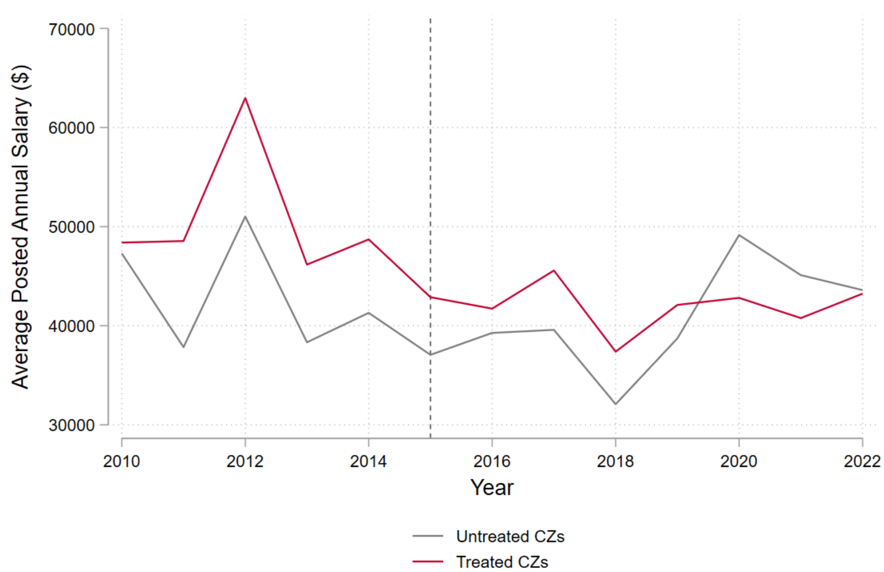


Figure 4: Average posted annual salary by commuting zone-based treatment intensity, 2010–2022.

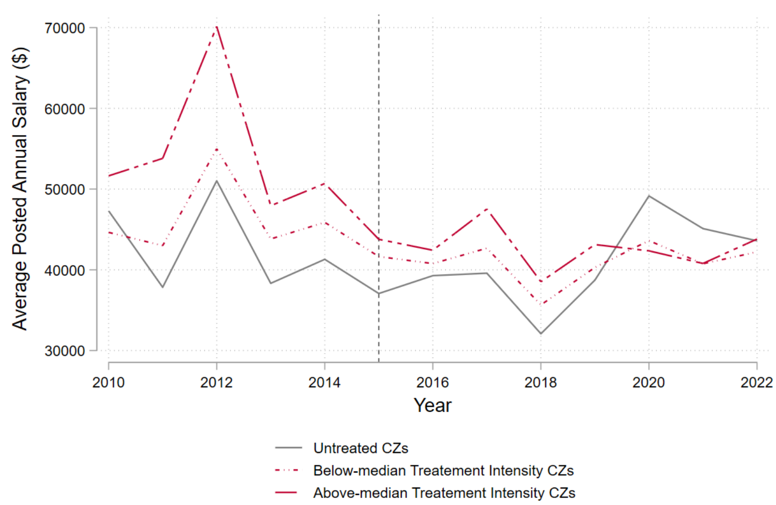
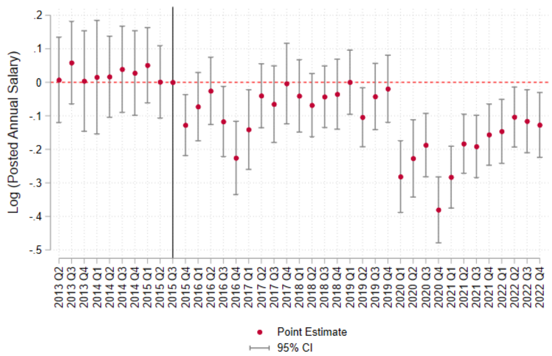
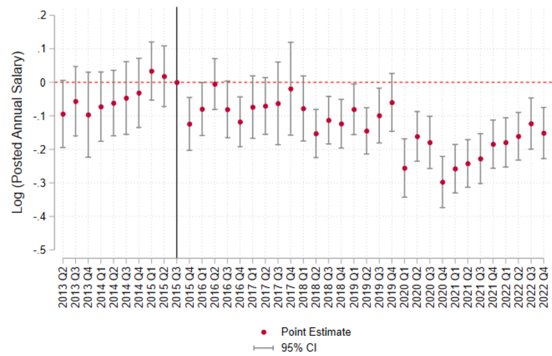


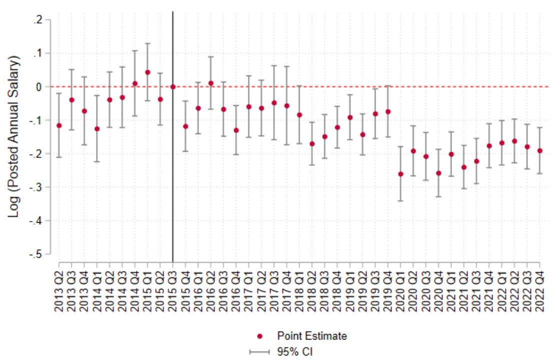
Figure 5: Event study estimates for the baseline specification.



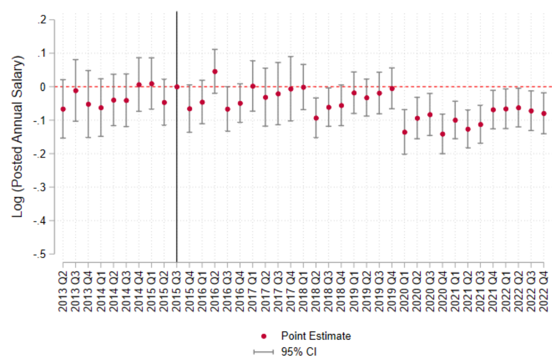
(a) Specification (1)



(b) Specification (2)



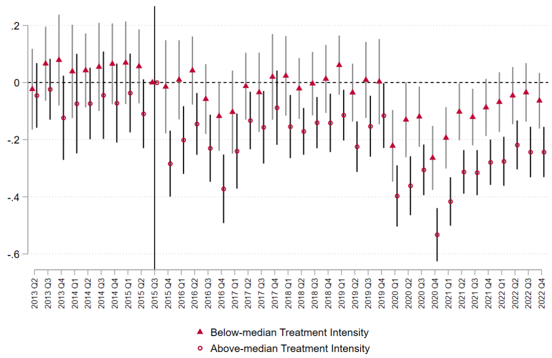
(c) Specification (3)



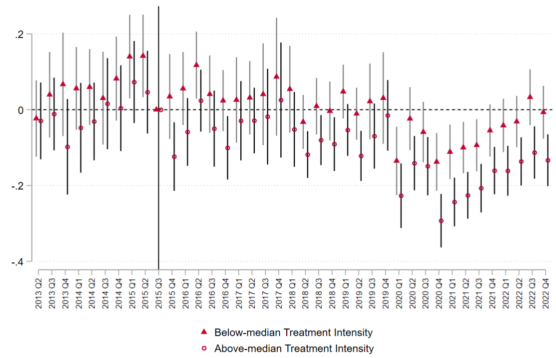
(d) Specification (4)

Notes: These figures plot $\hat{\beta}_t$ from equation 2 for the same four sets of fixed effect as each column of table 4. We compute a point estimate for each quarter starting from 2010 Q1 until the end of the sample period (i.e., 23 quarters pre-merger). However, to enhance readability and maintain a clean presentation, we present only the most recent 10 quarters pre-merger.

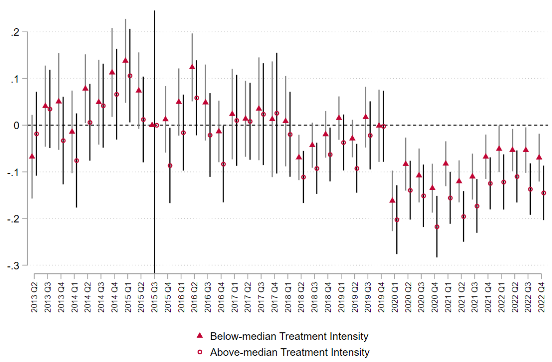
Figure 6: Event study estimates for the treatment intensity specification.



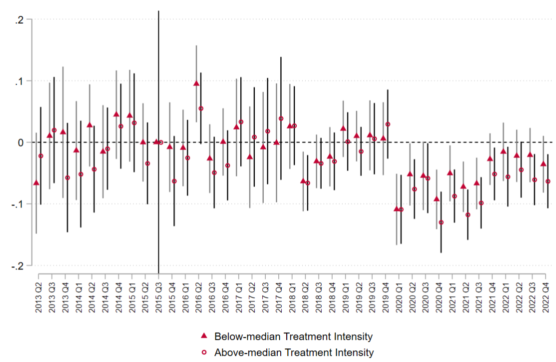
(a) Specification (1)



(b) Specification (2)



(c) Specification (3)



(d) Specification (4)

Notes: These figures plot estimates of $\hat{\beta}_{1t}$ and $\hat{\beta}_{2t}$ from equation 3 for each quarter relative to the third quarter of 2015 (i.e., one quarter before the merger) for the same four sets of fixed effects as each column of table 5. Treatment intensity for a given commuting zone is computed as twice the product of the pre-merger shares of job vacancies posted by CVS and Target out of all the vacancies posted in a given commuting zone. We compute a point estimate for each quarter starting from 2010 Q1 until the end of the sample period (i.e., 23 quarters pre-merger). However, to enhance readability and maintain a clean presentation, we present only the most recent 10 quarters pre-merger.

Figure 7: Average posted annual salary by outward occupational mobility, 2010–2022.

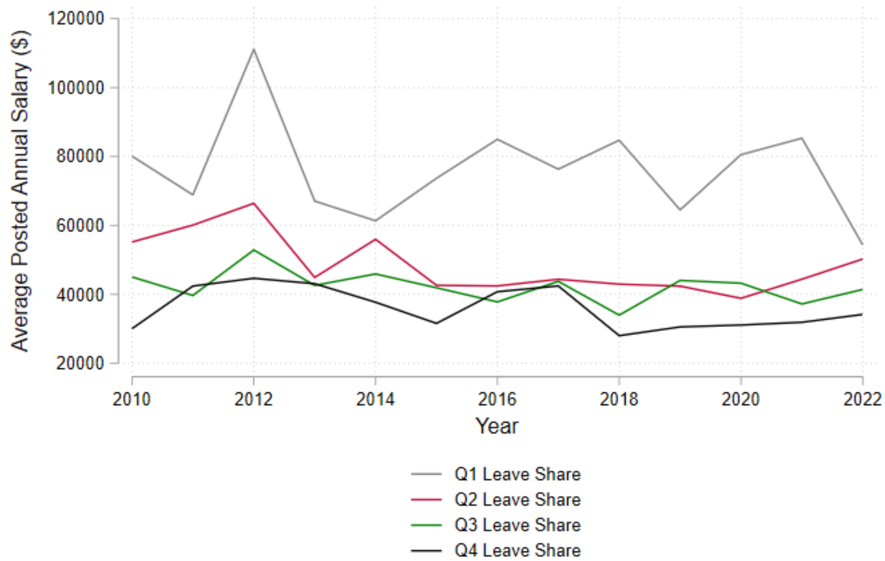


Figure 8: Correlation between posted annual pay and outward occupational mobility.

Figure 9: Average posted annual salary by outside job option index (OJOI) rank, 2010–2022.

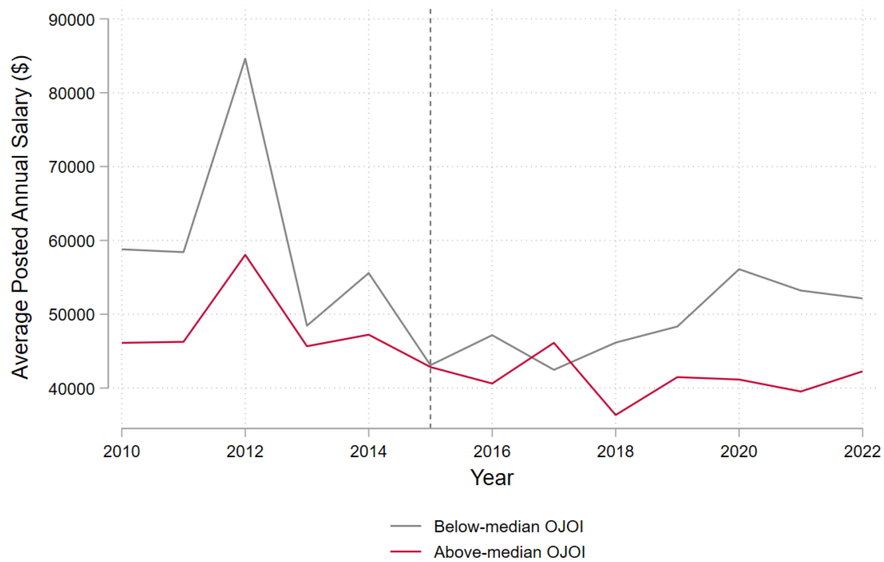
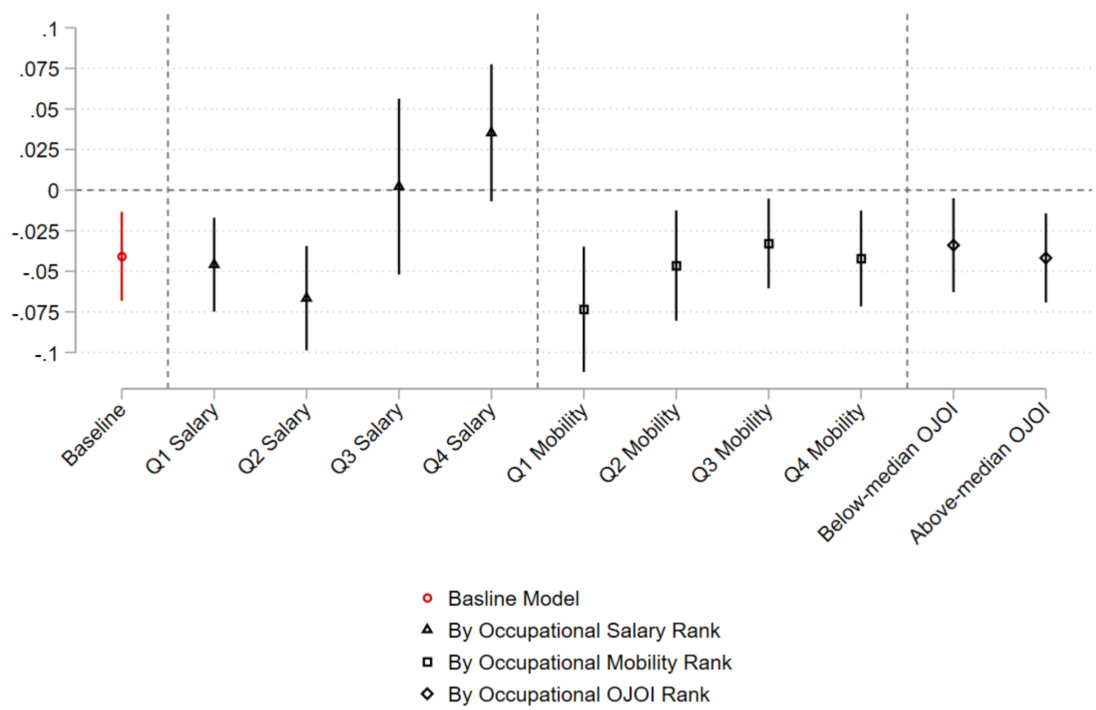


Figure 10: DiD coefficient estimates for the baseline model and heterogeneous effects by occupational characteristics: salary, mobility, and OJOI ranks.



Tables

Table 1: Number of vacancies with with posted salary information sourced from either LinkedIn or Indeed, by retail pharmacy chain, 2010-2022

Pharmacy Chains	Count
Kroger	26,608
Walmart	26,055
Walgreens	15,933
CVS	15,924
Albertsons	15,147
Target	12,256
Publix	4,453
Rite Aid	1,940
Costco	1,411
Hannaford	340
Health Mart	79
Safeway	12
Harris Teeter	3
Total	120,161

Note: These vacancies were dropped from our sample to eliminate the probability of including vacancies with imputed salary information.

Table 2: Average annual salary by treatment & time of treatment, 2010-2022.

	Pre-treatment		Post-treatment	
	Untreated CZs	Treated CZs	Untreated CZs	Treated CZs
Salary	41,925	48,608	43,256	41,906
	(39,454)	(41,270)	(31,985)	(27,463)
Obs.	9,352	56,051	56,084	622,404

Notes: Treated commuting zones are those in which both CVS and Target posted pharmacy-related vacancies during January 1 - December 15, 2015. Observation counts are of job vacancies within treat-by-post cells. Standard errors in parenthesis.

Table 3: Treatment intensity summary statistics.

	Posted Annual Salary (USD)		Vacancy Share (%)		Δ HHI
	Pre-merger	Post-merger	CVS	Target	
Below-median TI	45,165	41,068	4.26	6.24	50
Above-median TI	51,323	42,407	9.42	8.94	154

Notes: Commuting-zone-level treatment intensity is computed as twice the product of the pre-merger vacancy shares of the CVS and Target. Vacancy shares are calculated as the share of vacancies posted by each merging party separately out of all the total retail-industry job vacancies (the retail industries mentioned in Section 3.2) posted in a given commuting zone for each quarter of 2015, where the last quarter is only considered until December 15, 2015. Then, the each party's merging share is averaged over the 4 quarters of 2015.

Table 4: DiD estimates – Baseline specification.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.172***	-0.105***	-0.102***	-0.0408***
	(0.0295)	(0.0218)	(0.0178)	(0.0140)
Constant	10.66***	10.60***	10.60***	10.55***
	(0.0247)	(0.0183)	(0.0150)	(0.0117)
Observations	743,882	743,835	739,756	722,072
R-squared	0.073	0.367	0.521	0.625
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: This table reports $\hat{\beta}$ estimated using equation 1 with four sets of fixed effects included in the regression equation. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 5: DiD estimates – Heterogeneous effects by treatment intensity.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Post $\times$ Below-median treatment intensity	-0.107***	-0.0685***	-0.0835***	-0.0376**
	(0.0350)	(0.0248)	(0.0198)	(0.0166)
Post $\times$ Above-median treatment intensity	-0.223***	-0.135***	-0.118***	-0.0437***
	(0.0300)	(0.0238)	(0.0204)	(0.0153)
Constant	10.66***	10.61***	10.60***	10.55***
	(0.0238)	(0.0180)	(0.0150)	(0.0117)
Observations	743,882	743,835	739,756	722,072
R-squared	0.074	0.367	0.521	0.625
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: This table reports estimates of $\hat{\beta}_1$ and $\hat{\beta}_2$ from equation 3, with four different sets of fixed effects. The treatment intensity measure is computed as twice the product of the ex-ante vacancy shares of each merging party. Vacancy shares are calculated as the share of vacancies posted by each merging party separately out of all the total retail-industry job vacancies (the retail industries mentioned in Section 3.2) posted in a given commuting zone for each quarter of 2015, where the last quarter is only considered until December 15, 2015. Then, the each party's merging share is averaged over the 4 quarters of 2015. Treatment intensity is calculated at the commuting-zone level to divide treated commuting zones into two groups based on the value of the median of the treatment intensity measure. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 6: Top five occupations in terms of observation count for each outward occupational mobility (leave share) quartile.

Outward Mobility Rank	Occupation Title (6-digit SOC)	Count
	Pharmacists	22,430
	Heavy and Tractor-Trailer Truck Drivers	13,029
Q1	Registered Nurses	2,245
	Hairdressers, Hairstylists, and Cosmetologists	1,971
	Demonstrators and Product Promoters	1,460
	Pharmacy Technicians	46,191
	Merchandise Displayers and Window Trimmers	10,717
Q2	Janitors and Cleaners, Except Maids and Housekeeping Cleaners	8,378
	General and Operations Managers	7,975
	Bakers	7,609
	Retail Salespersons	125,713
	First-Line Supervisors of Retail Sales Workers	116,711
Q3	Customer Service Representatives	39,138
	Sales Representatives, Wholesale and Manufacturing, Except Technical and Scientific Products	22,502
	Laborers and Freight, Stock, and Material Movers, Hand	21,990
	Stock Clerks and Order Fillers	46,609
	Cashiers	30,899
Q4	Combined Food Preparation and Serving Workers, Including Fast Food	16,306
	Shipping, Receiving, and Traffic Clerks	6,823
	Protective Service Workers, All Other	6,442

Notes: Outward occupational mobility is computed from 16 million unique US workers' resumes collected by Lightcast. Leave share is a proxy for outward occupational mobility which is the share of observations in year t that are observed in a different occupation (six-digit SOC) in year $t+1$. For more details on the Lightcast resume data and the construction of these occupational mobility measures, see Schubert et al. (2024).

Table 7: 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	-0.341***	-0.0760***	-0.111***	-0.0459***
	(0.0294)	(0.0224)	(0.0190)	(0.0147)
Treat × Post × Q2 salary	0.00227	-0.124***	-0.124***	-0.0666***
	(0.0287)	(0.0226)	(0.0199)	(0.0164)
Treat × Post × Q3 salary	0.122***	-0.0741***	-0.0473	0.00218
	(0.0306)	(0.0257)	(0.0330)	(0.0276)
Treat × Post × Q4 salary	0.214***	-0.218***	-0.0199	0.0353
	(0.0345)	(0.0262)	(0.0244)	(0.0215)
Constant	10.66***	10.60***	10.60***	10.54***
	(0.0244)	(0.0178)	(0.0149)	(0.0116)
Observations	743,592	743,545	739,485	721,801
R-squared	0.231	0.368	0.522	0.626
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 earnings distribution are $35,140, $48,150, and $69,060, respectively. The OEWS annual salary data for the year 2015 was missing the following occupations: Actors (27-2011), Entertainers and Performers, Sports and Related Workers (27-2099), Musicians and Singers (27-2042), and Dancers (27-2031). 286 observations in our sample belonged to one of those occupations and were dropped accordingly. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 8: DiD estimates – Heterogeneous effects by outward occupational mobility rank, measured based on the leave share estimates.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post × Q1 leave	0.159***	-0.286***	-0.128***	-0.0734***
	(0.0334)	(0.0286)	(0.0243)	(0.0197)
Treat × Post × Q2 leave	-0.100***	-0.114***	-0.113***	-0.0465***
	(0.0306)	(0.0252)	(0.0215)	(0.0173)
Treat × Post × Q3 leave	-0.182***	-0.0796***	-0.0888***	-0.0329**
	(0.0292)	(0.0211)	(0.0182)	(0.0141)
Treat × Post × Q4 leave	-0.354***	-0.0703***	-0.118***	-0.0421***
	(0.0295)	(0.0209)	(0.0187)	(0.0150)
Constant	10.66***	10.60***	10.60***	10.55***
	(0.0244)	(0.0175)	(0.0150)	(0.0118)
Observations	743,591	743,545	739,485	721,801
R-squared	0.126	0.368	0.521	0.625
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: Occupations are ranked into four quartiles based on the occupation's leave share: the probability that a worker who leaves a job in that occupation is observed in a different occupation in their next job, according to the resume data compiled by Schubert et al. (2024). Standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 9: DiD estimates – Heterogeneous effects by outside job option index (OJOI).

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post × Below-median OJOI	0.0818**	-0.0953***	-0.0925***	-0.0339**
	(0.0414)	(0.0232)	(0.0188)	(0.0147)
Treat × Post × Above-median OJOI	-0.211***	-0.107***	-0.103***	-0.0418***
	(0.0298)	(0.0218)	(0.0178)	(0.0140)
Constant	10.67***	10.60***	10.60***	10.55***
	(0.0245)	(0.0182)	(0.0149)	(0.0117)
Observations	743,580	743,534	739,474	721,794
R-squared	0.091	0.367	0.521	0.625
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: Outside job option index (OJOI) is the weighted average of twice the product of the share of vacancies posted by each of CVS and Target in a given occupation (6-digit SOC) out of all the vacancies posted in that occupation, weighted by the probability that an individual working in an initial occupation o in year t moves to a destination occupation p in year $t+1$. For instance, if an individual works in occupation o and is located in commuting zone c, the $OJOI_{o,c}$ is the weighted average of twice the product of the vacancy share of each of the merging parties in all the possible outside occupations p —including cases when $o = p$— in commuting zone c that the worker could move to. We divide the combination of occupations o and commuting zones c for the treated commuting zones into two groups based on the median value of the OJOI. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

A Urban/Rural Pay Variation

One concern that may arise from our methodology of assigning treatment on the basis of geographic exposure to the CVS-Target merger is that the negative pay effects we estimate may be driven by non-merger-related pay trends in predominantly-urban versus predominantly-rural labor markets. In effect, our treatment assignment is highly correlated with urban-ness, because it interprets the presence of both CVS and Target retail pharmacy establishments in the commuting zone ex-ante as constituting exposure to the merger. That is why even though a minority of commuting zones are in our treatment group, they account for the vast majority (approximately 90%) of the overall analysis sample. Figure 3 may heighten this concern: why interpret the pay convergence between treatment and control commuting zones as due to the CVS-Target merger, and not something else that occurred prior to the observed pay convergence? The most direct way of ruling out alternative explanations for post-merger pay trends would be to include commuting-zone-by-time fixed effects in the main specification, but this is impossible to implement since the treatment assignment is at the level of the commuting zone, so our treatment indicator would be perfectly collinear with those fixed effects. In the main specifications, we estimate commuting zone fixed effects, but these do not account for trends in pay over time that may vary by commuting zone.

We address any lingering identification concerns in this appendix by re-estimating versions of equations 1 and 2 that include time-varying controls for urban-ness. We create these controls by taking advantage of the fact that the Lightcast data identifies the Metropolitan Statistical Area (MSA) where a job ad is posted, or alternatively, indicates that the geography where it was posted is not part of an MSA. MSAs are Census-designated regions around a core urban area defined by both a population threshold (50,000 in the central city) and a population density. Hence, whether a job ad is associated with an MSA is a useful binary measure of urban-ness. We interact that measure of urban-ness with calendar-quarter indicators to create Urban-by-YQ fixed effects as an additional regressor. The merger effect is then estimated using variation in exposure to the merger within cells defined by this binary measure of urban-ness.

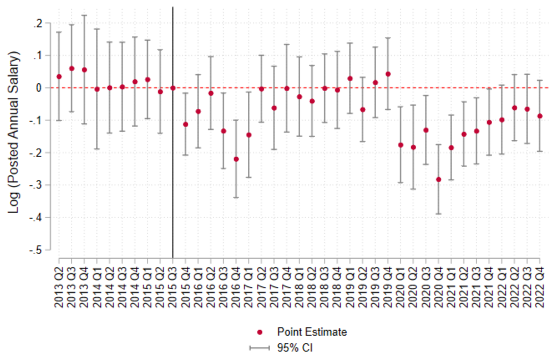
This approach radically reduces the identifying variation in the data, because the merger effect is estimated *within* urban-ness cells, and within them, there are correspondingly few units in either the treatment or the control group. Nonetheless, the results reported in table A1 and figure A1 are broadly similar to the results using the main specification, which indicates that they are not erroneously interpreting pay convergence between urban and rural areas as a merger effect.

Table A1: DiD results with urban-by-YQ FEs.

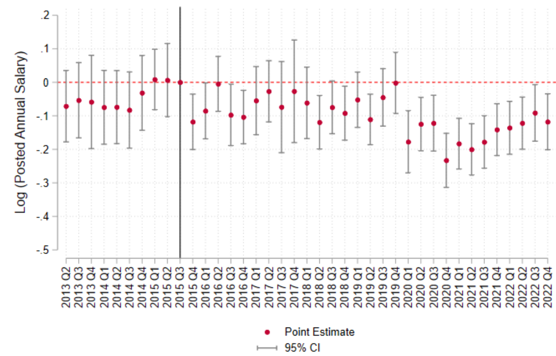
VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat $\times$ Post	-0.132***	-0.0743***	-0.0751***	-0.0277**
	(0.0300)	(0.0219)	(0.0179)	(0.0140)
Constant	10.62***	10.58***	10.58***	10.54***
	(0.0251)	(0.0183)	(0.0150)	(0.0118)
Observations	743,580	743,534	739,474	721,794
R-squared	0.077	0.368	0.522	0.626
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Urban-by-YQ	YES	YES	YES	YES
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Notes: This table reports $\hat{\beta}$ from estimating equation 1 with urban-by-calendar quarter fixed effects. Our measure of urban-ness is whether a job ad is posted in a Metropolitan Statistical Area. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

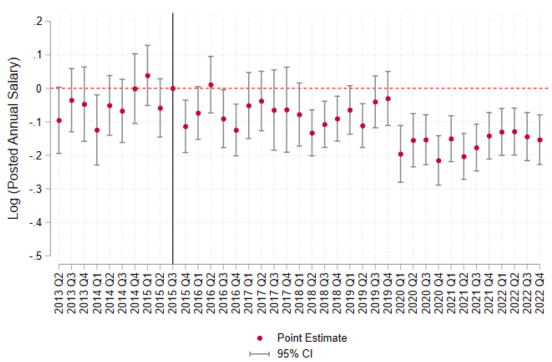
Figure A1: Event study estimates with urban-by-YQ fixed effects.



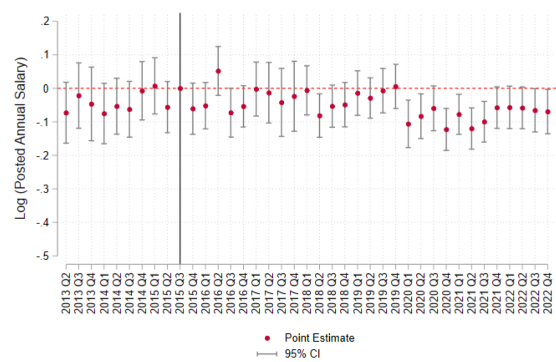
(a) Specification (1)



(b) Specification (2)



(c) Specification (3)



(d) Specification (4)

Notes: These figures report $\hat{\beta}_t$ from estimates of a modified equation 2 in which urban-by-calendar-quarter fixed effects are included. Our measure of urban-ness is whether the job ad is posted in a Metropolitan Statistical Area. We compute a point estimate for each quarter starting from 2010 Q1 until the end of the sample period (i.e., 23 quarters pre-merger). However, to enhance readability and maintain a clean presentation, we present only the most recent 10 quarters pre-merger.

B Variation in Severity of Pandemic-era Opening Restrictions

One concern that may arise from the pattern of post-treatment coefficient estimates reported in figure 5 is that the largest post-treatment coefficients we estimate occur starting in 2020Q1. The COVID-19 pandemic began in the United States in early 2020, and it had a major impact on retail labor markets (Autor et al. (2023) and Steinbaum (2023)). Hence, a reasonable concern is that the merger effects we estimate 16+ quarters following the merger are in fact due to differential exposure to pandemic-era variation in labor market restrictions that happened to be correlated with geographies treated by the CVS-Target merger four or more years earlier.

We address those concerns in this appendix by computing a commuting-zone-level index of exposure to pandemic-era economic restrictions. Specifically, the Centers for Disease Control created a county-level daily dataset on opening restrictions in place between March and May 2020. Each day is given a rating of 0-7, indicating varied levels of restrictions on mobility. We record each day coded either 6 or 7, corresponding to either near-total or total stay-at-home orders, at the county level. We then aggregate the share of days subject to such orders to the commuting zone level and compute quartiles of the resulting index, corresponding to the share of county-days subject to the most severe shutdown orders within the commuting zone. Finally, we interact those quartiles with calendar quarter indicators to create Stay-at-home-by-YQ fixed effects and include them in re-estimates of equations 1 and 2.

The results reported in table B1 and figure B1 indicate an even larger merger effect than from our baseline specification. Hence, we conclude that geographically-varied exposure to pandemic-era lockdown orders is not responsible for the large negative coefficient estimates reported for the pandemic period in our main specification. Rather, the effect of the merger seems to have been more severe where the merger reduced outside employment opportunities and hence prevented workers benefiting as much from significant pandemic-era labor market churn that on the whole raised wages for retail workers.

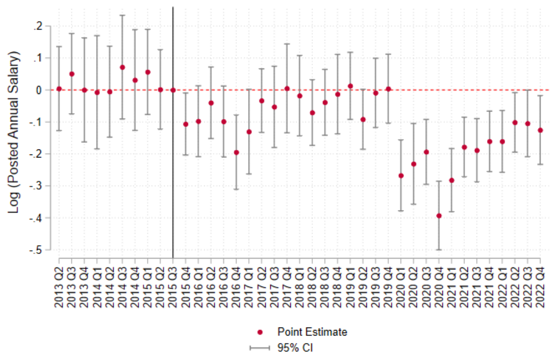
That interpretation is broadly consistent with the results for pharmacy-related occupations in BLS pay data reported by Bounthavong (2024), which indicate severe reductions in pharmacy-related occupational pay during the pandemic. The further insight we add is to tie the geographic pattern of those reductions to the CVS-Target merger.

Table B1: Estimates of equation 1 with stay-at-home-by-YQ fixed effects.

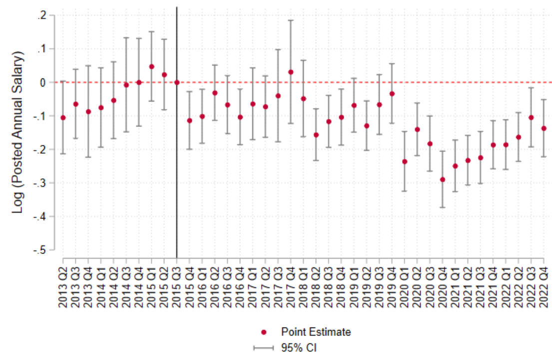
VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)
Treat × Post	-0.164***	-0.100***	-0.107***	-0.0532***
	(0.0328)	(0.0239)	(0.0186)	(0.0144)
Constant	10.65***	10.59***	10.60***	10.55***
	(0.0277)	(0.0201)	(0.0157)	(0.0122)
Observations	659,111	659,065	655,093	639,708
R-squared	0.077	0.371	0.525	0.628
CZ FE	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO
Stay-at-home-by-YQ	YES	YES	YES	YES
Occupation FE	NO	YES	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES
Employer FE	NO	NO	NO	YES

Notes: We construct the stay-at-home orders index from daily CDC data on the severity of county-level shutdown orders from March-May 2020. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

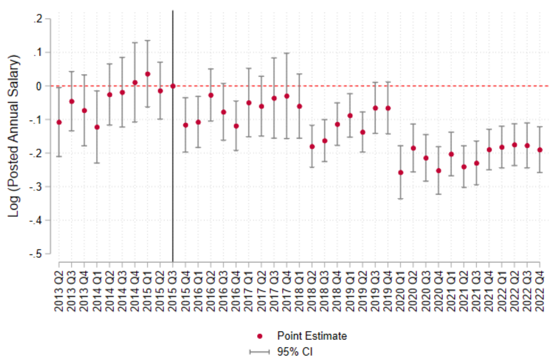
Figure B1: Event study estimates with stay-at-home-by-YQ fixed effects.



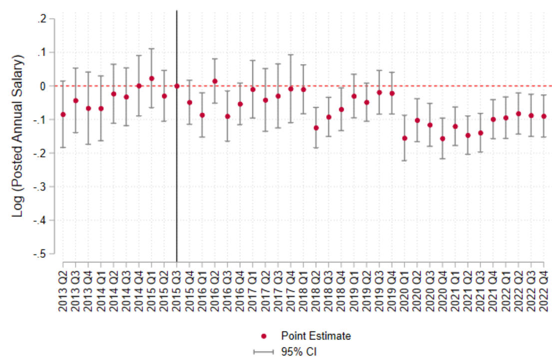
(a) Specification (1)



(b) Specification (2)



(c) Specification (3)



(d) Specification (4)

Notes: These figures report $\hat{\beta}_t$ from estimates of a modified equation 2 in which stay-at-home-by-calendar-quarter fixed effects are included. Our stay-at-home orders index is constructed from daily CDC data on the severity of county-level shutdown orders from March-May 2020. We compute a point estimate for each quarter starting from 2010 Q1 until the end of the sample period (i.e., 23 quarters pre-merger). However, to enhance readability and maintain a clean presentation, we present only the most recent 10 quarters pre-merger.