

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

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We analyze the labor market impact of CVS Health's acquisition of Target's pharmacy business in December 2015 using Lightcast job postings data. Using difference-in-differences and triple-difference designs, we find meaningful negative effects of the merger on posted pay. In our preferred specification, we estimate that the acquisition reduced posted pay in affected labor markets by 2.9%. We test for heterogeneous merger effects by occupational characteristics, finding that the merger caused pay to fall by more in lower-paid occupations than in higher-paid occupations.

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I Introduction

For a long time, perfect competition was the standard assumption in models of labor market 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 like [Manning \(2003\)](#)’s dynamic monopsony model with a job ladder. The model implies that all workers experience a wage markdown unless they receive infinitely frequent outside job offers, in which case equilibrium wages equal the full marginal product of labor. Manning’s 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 ([Sokolova and 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 and Marinescu, 2024](#)). Yearly, about 2% of workers work in establishments that engage in M&A activity ([Arnold, 2021](#)). In contrast to the large literature on the effects of M&A activity on product markets, there are few studies estimating the labor market effects of mergers.

In this paper, we study the effect on posted pay of a single merger of two large retail pharmacy chains in the U.S. On December 16, 2015, CVS Health acquired 1,672 Target in-store pharmacies in 47 states across the U.S. ([Target, 2015](#)).¹ To do this, we use Lightcast online job postings data, which covers the near-universe of online job postings in the U.S. from 2010–2022.² To the best of our knowledge, ours is the first formal study that estimates the effect of a single large merger in the U.S. on labor market outcomes.³

¹We sometimes refer to this acquisition as a “merger” throughout the paper despite the distinction that can be made between a merger and an acquisition. In the Industrial Organization literature, the word “merger” is used in a broader sense to refer to merger and acquisition activity.

²Using online vacancies data has become more prevalent recently in studying monopsony in labor markets, thanks to its employer identification (e.g., [Hershbein and Kahn, 2018](#); [Macaluso, Hershbein and Yeh, 2019](#); [Forsythe et al., 2020](#); [Clemens, Kahn and Meer, 2021](#); [Acemoglu et al., 2022](#); [Azar, Berry and Marinescu, 2022](#); [Callaci et al., 2024](#)).

³[Arnold \(2025\)](#) follows closely in that it uses the Lightcast job postings data to study a single merger’s

We use variations on a difference-in-differences (DiD) research design to estimate the causal effects of the CVS-Target pharmacy merger on posted pay. It is important to emphasize that since we use job postings data, our findings about merger effects on pay relate to new hires and not necessarily to incumbent workers. The core variation we exploit is geographic: 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. We consider treated commuting zones to be 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.⁴ This leaves us with 238 treated commuting zones and 469 control commuting zones.

Our first empirical strategy is therefore a simple difference-in-differences (DiD) across treated and control commuting zones. In addition, since we are studying a mega-merger between two large retail pharmacy chains, we expect the merger to have greater impact on retail industries compared to other industries. Hence, we next estimate a DiD limiting our sample only to a subsample of relevant retail industries.⁵ We also use the retail vs. non-retail dimension as another source of variation to construct a triple-difference empirical strategy, allowing us to control flexibly for differing commuting-zone-level pay trends. Across all three of these empirical strategies, we find negative effects of the merger on posted pay; in the triple-difference, which is our preferred specification, we find that the average annual salary for new hires in the retail sector dropped by 2.9% as a result of the merger. Our event study analyses show no evidence of differential pre-merger pay trends in any of the empirical strategies.

We next investigate whether the merger’s effect on posted pay differed by treatment

effect on labor markets.

⁴By pharmacy-related vacancies, we mean vacancies posted in one of the following occupations: pharmacists, pharmacy technicians, and pharmacy aides. We use these vacancies to identify which Target locations had in-store pharmacies. This is explained in detail in Section II.

⁵We explain how we construct the retail subsample in Section II.

intensity using two different measures. The first considers intensity in terms of geographical exposure, estimating the predicted change in employer concentration (ΔHHI) induced by the merger. The second considers both geographical and occupational exposure to the merger, estimating a weighted average of the merger-induced change in employer concentration in each occupation that forms part of a worker's proximate labor market. In both cases, we find a more pronounced merger effect (i.e., larger reductions in posted pay) for labor markets where the merger induced a greater increase in concentration, as compared to local labor markets where the merging firms still faced considerable competition from other employers.

Finally, 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) and outward mobility (defined as the share of workers who leave the occupation, using [Schubert, Stansbury and Taska \(2024\)](#)'s data built from resumes/job histories). We find larger effects of the merger for lower-paid workers: on average, posted pay for low-wage occupations decreased by 3.1% and, for high-wage occupations by 1%, but the latter effect is not statistically significant. In contrast, we find no differential effects by occupational outward mobility.

This paper makes three main contributions to the empirical literature. First, this paper relates to the rich literature that studies the effect of local labor market concentration on pay (e.g. [Macaluso, Hershbein and Yeh, 2019](#); [Azar et al., 2020](#); [Azar, Marinescu and Steinbaum, 2022](#); [Azar, Berry and Marinescu, 2022](#); [Benmelech, Bergman and Kim, 2022](#); [Rinz, 2022](#); [Schubert, Stansbury and Taska, 2024](#)).

Second, to address that endogeneity problem, a smaller but still significant body of literature uses mergers as a source of plausibly-exogenous variation in labor market concentration. [Arnold \(2021\)](#) uses a matched DiD design to study the wage and employment effects of all US mergers from 1999 to 2009. [Prager and Schmitt \(2021\)](#) apply a DiD ap-

proach to study the effect of 84 hospital mergers in the U.S. between 2000 and 2010 on wages, using a similar geographic-variation-based empirical strategy that we do. [Kim, Ge and Kim \(2021\)](#) analyze 13 US airline mergers from 1993 to 2018 to estimate impacts on wages and fringe benefits. Outside the U.S., [Guanziroli \(2022\)](#) studies a merger of retail pharmacy chains in Brazil, finding that wages of pharmacists dropped by 2.6% and that of salespeople decreased by 3.5%. [Thoresson \(2024\)](#) considers the opposite source of variation in concentration: the privatization and consequent de-concentration of the retail pharmacy industry in Sweden. She finds that wages for dispensing chemists increased by 2.5% to 6% for a local labor market that moved from the 75th to the 25th percentile of the labor market concentration distribution. All of these aforementioned well-identified papers confirm the negative relationship between employer concentration and pay.

In part thanks to this accumulating literature ([Harris, 2022](#)), the federal antitrust enforcers incorporated labor market competition concerns into the revised 2023 Merger Guidelines ([U.S. Department of Justice and Federal Trade Commission, 2023](#); [Posner, 2024](#)) and brought several enforcement actions to block mergers on the grounds they would result in less competition for labor. In one such action, *FTC v. Kroger*, the judge found the FTC’s theory “certainly more plausible” than the defendant’s (that greater labor market concentration due to the merger would benefit workers), but declined to rule on the FTC’s claim because

There is no economic modeling of how wages, benefits, and other compensation might change as a result of changes in bargaining power, either in absolute terms or relative to non-union grocery wages. Even if those calculations were available, plaintiffs have not explained what they believe would be a “substantial” reduction in competition. The Court simply lacks sufficient guidance on how the parties should measure changes and what changes would be meaningful. ([Nelson, 2024](#))

We offer this study in partial redress of the evidentiary absence noted by the judge. In particular, we study one specific merger, akin to what enforcers and judges do in merger

control, using a variety of empirical strategies that make use of alternative assumptions about the counterfactual evolution of pay. Hence, a key contribution of our paper is not only in estimating the effects of this specific merger, but also in introducing several estimation strategies that guide future empirical work on how to estimate merger effects on labor market outcomes.

Third, we contribute to the literature examining which occupations are most adversely affected by employer monopsony power, a question on which the existing evidence remains limited and conflicted. [Macaluso, Hershbein and Yeh \(2019\)](#) and [Azar et al. \(2020\)](#) found little cross-sectional relationship between local labor market concentration and average occupational earnings or skill level. [Prager and Schmitt \(2021\)](#) found that hospital mergers led to earnings losses among the higher-skilled workers with few options outside of hospital employers, but no effect on low-wage service workers, while [Guanziroli \(2022\)](#) found that the wages of salespeople declined more than those of pharmacists after a large pharmacy merger. [Schubert, Stansbury and Taska \(2024\)](#) showed that workers who had lower outward mobility saw greater wage reductions from employer monopsony power, regardless of the occupation's skill level or average wage rank. In our heterogeneity analysis, we find that low-wage workers are more adversely affected compared to high-wage workers.

The rest of the paper is organized as follows. Section [II](#) describes the data we use in this paper and provides important contextual information related to the merger. Section [III](#) explains the different research designs we employed to estimate the effect of the merger and presents the results. Section [IV](#) discusses the implications of our findings for the economic analysis of merger effects in labor markets. Section [V](#) concludes.

II Data

II.A Job Postings Data

Our main data source is online job posting data from Lightcast,⁶ 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 a listing date, employer name, job title, occupational classification (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.⁷ This posted salary variable is our main outcome variable.

Lightcast job posting data comes with two advantages and two 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 (Cammerraat and 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, employer, and pay. The latter’s granular geographical level is essential for our identification strategy since our treatment is assigned at the commuting zone level.

The second advantage of using LC data is that posted pay is often a more elastic in-

⁶Previously known as Burning Glass Technologies (BGT).

⁷LC 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 1st and the 99th percentile by year and six-digit SOC code to remove outliers.

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 the 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.⁸ Thus, Lightcast posted salary data aligns well with the realized pay of new hires, capturing exactly our outcome of interest. Figure [A1](#) reports the ratio of the count of job ads to total employment over time, which captures the flow component of the labor market on which our merger analysis is based.

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 19% of the vacancies between 2010 and 2022 have posted pay information. The sparsity of salary information is particularly concerning because the proportion of vacancies with salary information significantly increases after 2018, when LC added job boards to its source base with a greater prevalence of posted pay.⁹ 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](#)).

⁸The coefficient from regressing the log of salaries estimated using CPS data and earnings from Quarterly Workforce Indicators on the log of salaries reported by Lightcast at the state-quarter level over the period 2010–2016 is nearly one ([Hazell and Taska, 2020](#)).

⁹Using Lightcast data, the share of job ads with posted salary information during 2019–2022 is approximately 26%.

The wide variation between employers in the frequency of pay-posting could bias estimates that depend on firm-level posted pay to assign treatment ([Batra, Michaud and Mongey, 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. 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.

II.B The CVS and Target Pharmacy Merger

CVS Pharmacy is a leading American retail pharmacy chain owned by CVS Health. In 2015, CVS Health had more than 9,500 retail pharmacies across the U.S. ([Target, 2015](#)). CVS Health's annual revenues in 2015 amounted to \$153.29 billion ([Bullfincher.io, 2015](#)). Target is one of the largest general merchandise retailers in the U.S., and, prior to December 2015, it also operated a large retail pharmacy chain. According to Target's 2015 annual financial report, Target's 1,792 stores across the U.S. generated sales of nearly \$73.8 billion ([Target Corporation, 2015](#)). On December 16, 2015, Target announced the consummation of CVS Health's acquisition of the 1,672 Target in-store pharmacies in 47 states. As a result, CVS would operate them through a store-within-a-store format, branded as CVS/pharmacy. In addition, a CVS/pharmacy was to be included in all new Target stores that offer pharmacy services ([Target, 2015](#)).

The CVS–Target merger only directly affected labor market competition where both CVS and Target had a retail pharmacy presence ex ante. Prior to the CVS–Target merger, both parties existed in some commuting zones but not in others.¹⁰ To identify which commuting zones were affected by the merger, it is important to identify which Target lo-

¹⁰We use commuting zones as the geographic analog of local labor markets, following [Azar et al. \(2020\)](#); [Azar, Marinescu and Steinbaum \(2022\)](#).

cations had an in-store pharmacy at the time of the merger. Since we do not have access to Target’s store directories in 2015, we use LC job postings data during the period January 1, 2015–December 15, 2015 to infer Target’s pharmacy locations. We use the employer name and geographic identifiers available in the LC dataset to tag commuting zones affected by the merger.¹¹ We define treated commuting zones as commuting zones where both CVS and Target posted at least one pharmacy-related vacancy during the period January 1, 2015–December 15, 2015, indicating that both chains had at least one establishment with a retail pharmacy in that geographic labor market in the year preceding the acquisition. The pharmacy-related occupations we consider are pharmacists (SOC: 29-1051), pharmacy technicians (SOC: 29-2052), and pharmacy aides (SOC: 31-9095). By this definition, 238 commuting zones were treated by the CVS–Target merger and 469 commuting zones were not.

II.C Analysis Sample

The primary goal of this paper is to evaluate the effect of the CVS–Target merger on posted pay. Our research design exploits geographical variation in exposure to this merger based on ex ante existence of both CVS-Health and 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 352.95 million vacancies from 2010 to 2022, we first restrict our sample to postings with populated FIPS code and pay information since we assign treatment at the commuting zone level and posted annual salary is our main outcome variable. This leaves us with nearly 66.6 million vacancies. Second, we limit our sample to job ads with occupation (six-digit SOC) information, so we can control for any occupational pay trends using occupation-by-time fixed effects.¹² Thus, our analysis sample comprises 65,220,897

¹¹For this task, our goal is to identify Target stores with an in-store pharmacy. Hence, our variables of interest are location and employer name. We use the full universe of 2015 postings with location information used all LC online job postings between January 1, 2015 and December 15, 2015 including the postings with missing salary information as long as they have known location and employer.

¹²We drop vacancies whose 2-digit SOC code is 55 which indicates that they are military specific occupations. There was 45,942 of them.

vacancies. We refer to this analysis sample as the *full* sample.

Table A1 reports average posted pay pre- and post-merger for the treatment and control group. Figure A2 depicts the trends of the posted annual salary for both the treated and control commuting zones over the period 2010–2022. Before the merger, average posted annual salary in the treated commuting zones is higher but followed a similar trend as that of the control commuting zones. We see substantial convergence in posted salaries between treatment and control following the merger.

Since we are studying the effect of a merger between two large retail pharmacy chains on posted pay, our second data-building step is to use the 4-digit NAICS code to define a retail indicator (and sample). Our retail indicator is restricted to retail industries that encompass retail pharmacies, namely, food and beverage retailers, health and personal care retailers, and other general merchandise stores including department stores and warehouse clubs. We have 854,612 vacancies posted collectively in those industries.¹³ We refer to that sample of vacancies as the *retail* subsample which we use in subsections III.B and III.C. We emphasize that our retail subsample therefore does not comprise the entirety of the 2-digit NAICS retail industry.

II.D Treatment Intensity Measures

Using our binary treatment definition, we have 238 treated commuting zones. However, the size of CVS and Target as employers in these treated commuting zones varies. Hence, we are interested in studying whether the effect of the merger on posted pay differs across these treated commuting zones depending on the extent to which they are treated. To that end, we create two measures of treatment intensity to capture the degree to which CVS gained labor market power following the merger.

¹³These are the specific 2022 4-digit NAICS codes we use to define the retail indicator: Food and Beverage Retailers (4451 and 4452), Health and Personal Care Retailers (4561), and other general merchandise stores including department stores and warehouse clubs (4551 and 4552).

II.D.1. Predicted Δ HHI

A customary measure of market concentration often used in the Industrial Organization literature is the change in the Herfindahl Hirschmann Index (“ Δ HHI”).¹⁴ Therefore, we calculate the same measure using the share of vacancies posted by CVS and Target before the merger. Specifically, we calculate the share of vacancies posted by each of CVS and Target out of all the retail vacancies posted in a given treated commuting zone in 2015.¹⁵ We then calculate the predicted Δ HHI as twice the product of the ex ante vacancy shares of each firm as such that:

$$(II.1) \quad \Delta HHI_c = 2 \times \text{CVS Vacancy Share}_{c,2015} \times \text{Target Vacancy Share}_{c,2015}$$

This measure is useful because it 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.¹⁶ We use the median value of the predicted Δ HHI to divide the treated commuting zones into two groups reflecting varying degrees of treatment intensity.¹⁷ Commuting zones with above-median Δ HHI 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. In Figure A3, we look at the trend in the average annual salary for the two groups of treated commuting zones based on predicted Δ HHI. More intensely-treated commuting zones (commuting zones with above-median predicted Δ HHI) had higher posted pay before the merger, as compared to less-treated commuting zones. Visual inspection of raw data suggests parallel

¹⁴For example, it is the main benchmark used in the Merger Guidelines when evaluating whether a merger is likely to reduce competition. (U.S. Department of Justice and Federal Trade Commission, 2023).

¹⁵For this exercise, we use all the retail job vacancies (see footnote 13) we have for the year 2015 including the ones with missing salary information since we are only interested in calculating the vacancy shares of each of the merging parties. Vacancy shares are calculated as the share of vacancies posted by each merging party separately out of the total retail job vacancies posted in a given commuting zone for each quarter of 2015, where the last quarter is only considered until December 15, 2015. Then, each firm’s vacancy share is averaged over the four quarters of 2015.

¹⁶This 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.

¹⁷Note that the predicted Δ HHI for control commuting zones is zero since either CVS’s or Target’s vacancy share (or both) is zero.

trends prior to the merger. We formally test this assumption using an event-study empirical strategy in subsection III.D.

II.D.2. *Outside Job Option Index (OJOI)*

The above treatment intensity measure, predicted Δ HHI, defines treatment at the commuting zone level, assuming that all occupations within a commuting zone are equally treated by the merger. However, it is reasonable to expect that the effects of a merger between two large national retail pharmacy chains will vary across the different occupations within each treated commuting zone. Hence, we create another treatment intensity measure which varies by occupation *within* a treated commuting zone. The idea behind this measure is to capture the fact that the outside options for some workers, whether within the same occupation or if they switch to other occupations, could be differentially affected if CVS and Target become relatively large employers in those “outside option” occupations following the merger. We create an index reflecting the degree to which the merger affected the outside job options for workers. We call this index the Outside Job Option Index (OJOI). We calculate this index for each occupation within each treated commuting zone.¹⁸

We construct the OJOI in two steps. First, we calculate Δ HHI (equation II.1) separately for each occupation-by-commuting zone cell (unlike in subsection II.D.1., where we construct the Δ HHI for the entire commuting zone). This gives us the predicted Δ HHI for each destination occupation p in commuting zone c during 2015.¹⁹ Next, we construct the OJOI for each focal occupation o and commuting zone c as a weighted average of the $\Delta HHI_{p,c,2015}$ estimates, where the weights are workers’ mobility flows from focal occupa-

¹⁸Similar to the Δ HHI measure, the OJOI for control commuting zones is mechanically zero because these are commuting zones where the employer structure did not change as a result of the merger.

¹⁹For the purpose of calculating the OJOI, we start with computing Δ HHI for each possible occupation within each of the treated commuting zones to get a measure of the intensity of merger exposure for each possible occupation. We use all the online job vacancies for the year 2015 including the ones with missing salary information with no restriction to a particular industry. Vacancy shares are calculated quarterly using 2015 data, where the last quarter is only considered until December 15, 2015. Then, we take the average of the vacancy shares for each firm across the 4 quarters.

tion o to each possible destination occupation p , denoted $\theta_{o \rightarrow p}$ (where destinations also include the original focal occupation o) as depicted in equation II.2. The weights we use are estimated from data on 6-digit SOC level occupation-to-occupation flows, constructed from US workers' resume data by [Schubert, Stansbury and Taska \(2024\)](#).²⁰

$$(II.2) \quad OJOI_{o,c,2015} = \sum_p^{N_{occupations}} \theta_{o \rightarrow p} \times \Delta HHI_{p,c,2015}$$

Intuitively, we are trying to test for differential merger effects depending on the merger-induced increase in monopsony power for the merging parties, in the possible outside job options for workers affected by the merger (whether these outside options are within the same occupation or not). For instance, we know from the occupational mobility data constructed by [Schubert, Stansbury and Taska \(2024\)](#) that when pharmacists change jobs, the probability that they take another job in the Pharmacists occupation is 82%, followed by the Medical and Health Services Managers occupation with a corresponding probability of 1%, followed by every other occupation at 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 occupation 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 large employers.

Higher values of OJOI means that the merging parties are relatively large employers in a group of workers' relevant labor market, including both workers' current focal occupation and other destination occupations that they tend to transition to. We use the me-

²⁰[Schubert, Stansbury and Taska \(2024\)](#) 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. Mobility flow $\theta_{o \rightarrow p}$ 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, $\theta_{o \rightarrow p}$ 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., $o = p$). Note that this mobility dataset used the 2010 Standard Occupational Classification (SOC) system while our dataset used the 2018 SOC codes system. In [Appendix C](#), we explain how we matched 2018 SOC codes to 2010 SOC codes.

dian value of the OJOI to divide the combinations of occupations by treated commuting zones into two groups reflecting varying degrees of treatment intensity across occupations within treated commuting zones. Figure A4 depicts the pay trends over time for the combination of occupations by treated commuting zones with OJOI below and above median. The average posted annual salary is lower for vacancies posted in occupations where both CVS and Target had significant vacancy shares before the merger.

III Empirical Strategy and Results

We are interested in estimating the effect of CVS's acquisition of Target's pharmacy business on posted pay for affected workers. To do so, we employ several different research designs. Each of these research designs takes a different stance on exactly who the "treated" individuals are by the CVS-Target merger, and what the appropriate counterfactual is for inference. The research designs are:

1. **Simple difference-in-differences:** defining treated commuting zones as those where CVS and Target pharmacies were both present before the merger, we estimate differences in posted pay for all job postings in treated and control commuting zones, before and after the merger.
2. **Retail difference-in-differences:** we run the same simple DiD model as above, but limiting our analysis only to the retail subsample: the job postings in the retail industries that encompass retail pharmacies, which are most likely to be affected by the CVS-Target merger.
3. **Retail triple-difference:** we implement a triple-difference (DDD) empirical strategy, which effectively combines our first and second research designs, estimating the difference also between retail and non-retail job postings in treated commuting zones after the merger, compared to control commuting zones.

4. **Treatment intensity by predicted Δ HHI:** we estimate treatment effects separately for commuting zones with above-median and below-median treatment intensity, defined as the merger-induced change in HHI among retail postings. The control group is the same as in the first design.
5. **Treatment intensity by outside job option index (OJOI):** we estimate treatment effects separately for occupation-commuting zone labor markets which were more or less affected by the merger, defined using the OJOI, which captures the average merger-induced Δ HHI in workers' own occupation and likely outside option occupations. The control group is also the same as in the first design.

We see these five approaches as alternative and complementary ways to estimate the labor market effects of a national mega-merger. In this section, we discuss each of these five approaches in sequence in subsections [III.A](#) through [III.E](#), presenting their results and comparing their relative strengths and weaknesses for causal identification of the merger effect. Thus, we see our contribution not only as the estimates of the labor market effects of this specific mega-merger, but also methodologically in proposing various ways to estimate the labor market effects of individual mergers more broadly. In subsection [III.F](#) we examine whether there are heterogeneous effects of the merger by occupational average salary or outward occupational mobility.

III.A Simple Difference-in-Differences

Our first approach to estimate the effect of CVS's acquisition of Target's pharmacy business is a simple DiD 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, with treated commuting

zones defined as in subsection II.B. The estimation strategy is shown in equation III.1.

$$(III.1) \quad \ln(\text{Salary}_{ieojct}) = \beta \text{Treat}_c \times \text{Post}_t + X_{ieojct} + \epsilon_{ieojct}$$

The dependent variable is the log of posted pay for vacancy i posted by employer e that belongs to occupation o in industry j 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. X_{ieojct} is a vector of fixed effects. We present our regression with progressively more stringent combinations of fixed effects, including commuting zone fixed effects, which control for static wage differentials across commuting zones due to factors unrelated to their exposure to the merger; year-quarter or occupation-by-year-quarter fixed effects, which control for national wage trends or differential occupational wage trends over time; industry fixed effects, which control for static wage differentials across industries; and employer fixed effects, which control for employer-specific wage policies.²¹ Standard errors are clustered at the commuting zone level.

Table 1 presents estimation results of equation III.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. Our preferred specification is reported in the fifth column, and includes commuting zone, occupation-by-year-quarter, industry, and employer fixed effects.²² The estimated coefficient on the post-treatment indicator for this specification suggests that posted pay decreased as a result of the merger by approximately 1.3%, on average, over the seven years following the

²¹Note that in Lightcast data, there is industry variation within employer.

²²The estimated coefficient declines meaningfully on the addition of employer fixed effects (column 4 to column 5), suggesting that shifts in employer composition, combined with different national employer pay policies, are a possible confounder when estimating the treatment effect. It's also possible that the change in employer composition or in employer-level national pay policies are an effect of the merger, and therefore that our preferred specification under-estimates the true treatment effect. Nonetheless, we focus on specification 5 with employer fixed effects as our preferred specification in all analyses. Note that the sample size drop from column 4 to column 5 is due to vacancies with missing employer information. The coefficient in column 4 remains essentially the same when we limit the sample to vacancies with *non*-missing employer information as reported in Table A2.

merger.²³

The core identifying assumption here is that posted pay in treated commuting zones would not have evolved differently than the posted pay in control commuting zones in the absence of the merger, conditional on our fixed effects. While our commuting zone fixed effects hold constant any time invariant differences across treated and control commuting zones, it is possible that 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 pay in control commuting zones was on a faster growth path than pay in treated commuting zones.

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

$$(III.2) \quad \ln(\text{Salary}_{ieojct}) = \sum_{\substack{t=-23 \\ t \neq -1}}^{28} \beta_t \mathbb{1}[t = \text{quarter}] \times \text{Treat}_c + X_{ieojct} + \epsilon_{ieojct}$$

where $\mathbb{1}[t = \text{quarter}]$ 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 (i.e., from 2010Q1 to 2022Q4). In Figure 1, we show the event study results for specifications 4 and 5 (corresponding to columns 4 and 5 of Table 1). As we mentioned earlier, specification 5, the one with commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry fixed effects, and employer fixed effects, is our preferred specification. This is the most conservative specification since it also controls for employer-specific wage policies, which we believe is essential to precisely identify the merger effect. Figure 1b shows the event study results with employer fixed effects, compared to the results without employer fixed effects in Figure 1a.

Both specifications of the event study results in Figure 1 show that treated and control commuting zones had parallel pre-trends in posted pay for the five years prior to the

²³The dependent variable is in log form, so we exponentiate coefficients here and elsewhere for interpretation. Precisely, posted pay declined by $[e^{-0.0128} - 1] * 100 \approx -1.3\%$.

merger. 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. Thus, to invalidate a finding of a negative wage effect of the merger, it would need to be the case that wages in untreated commuting zones were on a faster growth trend than wages in treated commuting zones in 2016-2022 after the merger took place – but *not* in 2010-2015 before the merger. The best way to rule out this claim would be to include commuting-zone-by-time fixed effects which control for differential pay trends across commuting zones. However, this is impossible to implement with a DiD framework where treatment is assigned at the commuting zone level.²⁴

III.B Retail Difference-in-Differences

Our analysis above estimates the effect of the merger on posted pay across *all* vacancies in a commuting zone. An alternative approach to estimate the effect of the merger on wages is to focus only on the subsample of job postings that we expect to be most affected by the merger. Since the merger is between two large retail pharmacies, in this section, we restrict our sample only to job postings in the retail industries (as defined in subsection II.C) which we would expect to have been most affected by the merger, and re-estimate our commuting-zone level DiD model from subsection III.A.

We present our results in Table 2. In our preferred specification, column 5, we estimate that posted pay in the retail subsample was 3.8% lower in treated commuting zones after the merger. Event study plots are shown in Figure 2. As with our simple DiD regression, the core identifying assumption is that the trend in posted pay for retail vacancies in treated and control commuting zones would have been the same absent the merger, conditional on our fixed effects. Given the parallel pre-trends shown in the event study, the core threat to identification is therefore that other economic factors changed in treated commuting zones around the time of the merger (in 2015), such that retail pay in treated

²⁴This is because commuting-zone-by-time fixed effects are perfectly collinear with the post-treatment indicator in the DiD setup.

commuting zones grew more slowly than retail pay in control commuting zones for reasons unrelated to the merger.

III.C Retail Triple-Difference

Our third research design is a triple-difference (DDD) model, where the three differences are (i) pre- and post-merger, (ii) treated and control commuting zones, and (iii) retail and non-retail vacancies. Thus, this design effectively combines our simple commuting-zone level DiD from subsection III.A with our retail subsample analysis in subsection III.B. Conceptually, it is similar to estimating one DiD for retail vacancies and another for non-retail vacancies, and therefore the coefficient of interest is the difference between the coefficients from these two separate DiD regressions.

In employing this design, we assuage the core identification concern with both of the previous strategies: namely, that commuting zones where CVS and Target were both present before the merger may have been hit with a correlated economic shock—unrelated to the merger—around 2015 in a way that caused posted pay to grow more slowly than in other commuting zones. Our DDD strategy effectively uses non-retail postings to identify commuting-zone specific differences in posted pay in treated as compared to control commuting zones, before vs. after the merger, and then detects where there is a differential relative evolution of posted pay in the retail subsample in treated vs. control commuting zones after the merger. Hence, we estimate the merger effect on the retail subsample after controlling for differential average pay changes in treated vs. control commuting zones.

The core identifying assumption is that the trend in posted pay in the retail subsample in treated commuting zones, relative to the commuting zone average, would have been similar to the trend in posted pay in the retail subsample in non-treated commuting zones relative to their commuting zone averages. In other words, unlike the DiD, where the posted pay in control commuting zones is the counterfactual, using the DDD, the relative difference in posted pay trends between retail and non-retail industries in control

commuting zones is the counterfactual.

The DDD model is shown in equation III.3.

$$(III.3) \quad \ln(\text{Salary}_{ieojct}) = \beta_1 \text{Treat}_c \times \text{Post}_t \times \text{Retail}_j + \beta_2 \text{Treat}_c \times \text{Post}_t + X_{ieojct} + \epsilon_{ieojct}$$

The coefficient of interest is β_1 , the coefficient on the triple interaction term. Treat_c and Post_t are the same indicator variables we defined and used in equation III.1. Retail_j is an indicator for the retail industries that comprise retail pharmacies, as defined in subsection II.C. The vector of fixed effects X_{ieojct} remains the same as in prior research designs, except that we now estimate these fixed effects separately for retail and non-retail postings (to allow commuting-zone and employer level pay to vary for retail and non-retail postings, and to allow occupation-specific time trends to vary for retail and non-retail postings).²⁵ Standard errors are clustered at the commuting-zone-by-retail level.

We show the results for this DDD model in Table 3. In our preferred specification (5), we find that the merger lowered posted pay by 2.9% in the retail subsample in treated commuting zones, with the estimate statistically significant at the 5% level. Note that this coefficient estimate is smaller than the coefficient estimate on our retail-only difference-in-differences in Table 2, suggesting that some portion of the slower growth of retail pay in treated commuting zones after 2015 may have been due to exogenous commuting-zone-level economic shocks (now captured by β_2 in equation III.3)– but that even controlling for these commuting-zone level pay trends, there was meaningfully slower growth in retail postings’s pay in treated commuting zones after the merger.

We present the event study results in Figure 3. Both panels show that, for our preferred specification, there was no difference in posted pay in retail postings in treated vs. control commuting zones, as compared to non-retail postings in treated vs. control commuting zones, in the year prior to the merger. (There is, perhaps, a slight upward pretrend, with posted pay in retail in treated commuting zones slightly lower than in

²⁵Note that while we can estimate $\text{Treat}_c \times \text{Post}_t$, we do not separately estimate the other triple-difference coefficients ($\text{Post}_t \times \text{Retail}_j$, $\text{Treat}_c \times \text{Retail}_j$, Treat_c , Retail_j , and Post_t) since they are absorbed by the occupation-by-quarter-by-retail, commuting-zone-by-retail, and industry fixed effects.

non-treated commuting zones two years before the merger). After the merger, we see a strong and statistically significant downward shift in pay in retail in treated commuting zones, which persists over time. Thus, since we do not find any evidence of a negative pretrend in treated cells before the merger, for the negative estimate in this specification to not be a causal effect of the merger, we would have to believe that there was a *retail-only* economic shock that differentially affected treated and control commuting zones around the time of the merger, but did not affect the rest of the local economy in the same way. ²⁶

III.D Treatment Intensity: Δ HHI

In subsections III.B and III.C, we estimated the effect of the merger by focusing our attention on postings in our retail industry subsample, which we expect to be most affected by the merger between these large retail employers, and comparing outcomes across treated and control commuting zones. An alternate way to estimate the differential effect of the merger is to note that even among treated commuting zones, some were more affected by the merger than others. In this section, we estimate whether there were differential effects of the merger on posted pay for more- and less-affected commuting zones, using the merger-induced change in the Herfindahl-Hirschman Index Δ HHI (defined in subsection II.D.1.) as one possible treatment intensity measure. Specifically, we augment our simple DiD regression in equation III.1 with interactions between the post-treatment indicator and, respectively, a below-median and an above-median indicator of

²⁶One possibility that could prompt treated and control commuting zones to trend differently around the time of the merger is that commuting zones with both Target and CVS stores are more urban. Target stores are usually located in densely populated areas that tend to have strong economic activity (and hence more job postings (Jordan Bean, 2021)), and CVS lacks presence in rural areas as opposed to its significant presence in high-population areas (Marissa Evans, 2014). In Appendix B, we re-estimate equation III.3 with urban-by-time fixed effects, effectively allowing for pay trends to differ flexibly between urban and non-urban areas to account for the possibility of divergent pay trends in treated and control commuting zones. Table B1 show that our results are unchanged, suggesting that differential pay growth in urban vs. non-urban areas around and after 2015 is not confounding our core finding.

our predicted Δ HHI treatment intensity measure.

$$(III.4) \quad \ln(\text{Salary}_{ieojct}) = \beta_1 \text{Below-median } \Delta \text{ HHI}_c \times \text{Post}_t \\ + \beta_2 \text{Above-median } \Delta \text{ HHI}_c \times \text{Post}_t + X_{ieojct} + \epsilon_{ieojct}$$

Below-median Δ HHI_c is an indicator for commuting zone c whose Δ HHI is below sample median and *Above-median* Δ HHI_c is an indicator for commuting zone c whose Δ HHI is above the sample median. Therefore, β_1 estimates the effect of the merger on commuting zones that did not experience a major concentration change (relative to control commuting zones), and β_2 estimates the effect of the merger on commuting zones where there was significant consolidation. We further run this empirical strategy in event study format. For this strategy, the core identifying assumption is that posted pay in commuting zones with above median Δ HHI would not have evolved differently than posted pay in commuting zones with below median Δ HHI, or than posted pay in control commuting zones, conditional on our fixed effects.

We report estimates of equation III.4 in Table 4 for the same five fixed effects specifications, and event study plots in Figure 4. Across all five specifications, the merger effect is larger (more negative) for more-treated commuting zones. Specifically, in our preferred specification (column 5), the merger effect is larger (-1.6%) in the commuting zones in the top half of the treatment-intensity distribution than in the bottom half (-0.9%, and not statistically significant) – indicating that where the merger had a bigger effect on employer concentration, pay was more affected (although the difference between the two coefficients is not statistically significant at conventional levels, with a p-value of 0.28). 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. Our finding of a larger wage effect in markets with a larger merger-induced change in concentration echoes those of [Arnold \(2021\)](#) and [Prager and Schmitt \(2021\)](#).

III.E Treatment Intensity: Outside Job Option Index

The analysis above defines treatment at the commuting zone level, meaning that all occupations within a commuting zone are considered equally treated by the merger. But a merger between two large retail pharmacy employers will affect the labor market for some occupations far more than for others. In this section we use the OJOI – a weighted average of the change in HHI induced by the merger across an occupation and its outside option occupations, as defined in subsection II.D.2. – to proxy for occupation-specific treatment intensity within each treated commuting zone. We use the median value of our OJOI treatment intensity measure to divide the combination of occupations-by-treated-commuting-zones in our full sample into two groups: below-median and above-median. Then, we augment the simple DiD regression in equation III.1 with an interaction term for each group as shown in equation III.5.

$$(III.5) \quad \ln(\text{Salary}_{ieojct}) = \beta_1 \text{Treat}_c \times \text{Post}_t \times \text{Below-median OJOI}_{oc} \\ + \beta_2 \text{Treat}_c \times \text{Post}_t \times \text{Above-median OJOI}_{oc} + X_{ieojct} + \epsilon_{ieojct}$$

*Below-median OJOI*_{oc} is an indicator for vacancies in occupation *o* and commuting zone *c* whose OJOI is below the sample median and *Above-median OJOI*_{oc} is an indicator for vacancies in occupation *o* and commuting zone *c* whose OJOI is above the sample median. Therefore, β_1 and β_2 capture the effect of the merger on occupation-by-commuting-zone cells in which the merger did cause significant consolidation in their relevant labor market relative to occupation-by-commuting-zone cells that did not experience significant concentration change in their relevant labor market. Again, we further run this empirical strategy in event study format. We expect the merger effect on pay to be larger (more negative) for the occupation-by-commuting-zone cells with 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.

We present results for our analysis in Table 5. Among commuting zones where both

Target and CVS had a pre-merger presence, and in our preferred specification (5) with employer fixed effects, occupation-CZ cells with above-median OJOI saw a decline in pay of 1.7%, compared to a decline in pay of 0.96% for occupation-CZ cells with below-median OJOI. The difference between the two coefficients is statistically significant at the 5% level. Event studies in Figure 5 show no significantly different pre-trends for occupation-CZ cells with above- vs. below-median OJOI, and show that the differential wage effect starts to emerge relatively soon after the merger and widens over time.

All of the estimates (point estimates and 95% confidence intervals) for the preferred specification for each of the research designs discussed in subsections III.A-III.E are summarized in Figure 6a.

III.F Heterogeneous Effects of the Merger by Occupation

As discussed in the Introduction, 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 two dimensions: occupational salary and outward occupational mobility.

First, we examine heterogeneity by average occupational salary. To identify whether low-pay or high-pay occupations are more adversely affected by the merger, we augment our retail DDD model in equation III.3 (our preferred research design) with an interaction term signifying whether the average annual salary of the occupation in question is above or below the median, using the Bureau of Labor Statistics Occupational Employment and Wages Survey (OEWS) annual salary estimates from 2015.²⁷ Table 6 reports the estimated coefficients. According to our preferred specification reported in column 5, workers in lower-wage occupations are the most adversely affected by the merger: pay falls by 3.1% for workers in below-median-salary occupations and by 1% for workers in above-median-salary occupations, though the latter effect is not statistically significant. The difference

²⁷The median in this case is \$48,150.

between the two coefficients is statistically significant at the 5% level.

Second, we examine heterogeneity of the merger’s effects by occupations’ degree of outward mobility, using outward occupational mobility data from [Schubert, Stansbury and Taska \(2024\)](#). Specifically, we compare occupations which are above or below median outward mobility 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 $t + 1$.²⁸ To the extent that the merger increases CVS and Target’s labor market power, we expect that the effects of the merger on posted pay will be greater for workers who had limited ability to leave their occupation (to avoid any potential wage-suppressive effects of the merger). Table [A3](#) lists the top ten occupations (six-digit SOC) in terms of observation count for below-median and above-median leave share categories. Table [7](#) reports the estimated coefficients. In our preferred specification 5, we find no difference in the effect of the merger by occupational outward mobility. We visualize both dimensions of heterogeneity in Figure [6b](#), which plots the estimated coefficients and the 95% confidence intervals from specification (5) of Tables [6](#) and [7](#).

IV Discussion

As discussed in the Introduction, a large body of literature relating employer concentration and broader monopsony power to wage suppression motivated a policy shift in antitrust, toward scrutinizing labor market effects of challenged conduct. That shift is embodied in the 2023 Merger Guidelines and in enforcement actions taken by federal, state, and private plaintiffs. In one such action, *FTC v. Kroger*, the judge ultimately ruled there was insufficient economic analysis of merger effects in labor markets, both in the

²⁸[Schubert, Stansbury and Taska \(2024\)](#) 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 see [Schubert, Stansbury and Taska \(2024\)](#). Note that this mobility dataset used the 2010 Standard Occupational Classification (SOC) system while our dataset used the 2018 SOC codes system. In [Appendix C](#), we explain how we matched 2018 SOC codes to 2010 SOC codes.

evidentiary record for that case and in the form of usable benchmarks from other mergers, for her to have sufficient grounds to rule that the merger was illegal due to its anti-competitive effect on labor markets. The contribution of this paper is to take the literature from where it was when the guidelines were adopted (that labor market monopsony is pervasive and detrimental to workers, and that it can be exacerbated by mergers in general) to practitioners having a sufficient toolbox to put to use in specific cases as a matter of course.

Our findings 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 national mega-merger between CVS and Target’s retail pharmacy business. We have presented five different approaches to estimate this effect, and across all five strategies, we have found consistent evidence pointing to a negative effect of the CVS-Target merger on posted pay for affected workers.

Across all five strategies, we find consistent evidence of a negative merger effect on posted pay. Our simple DiD and retail-only DiD strategies show that posted pay in treated commuting zones grew more slowly than in control commuting zones after the merger, conditional on our fixed effects. Our triple-difference strategy addresses the concern that this may reflect differential commuting-zone-level economic trends unrelated to the merger, using non-retail pay trends as a control. Our Δ HHI treatment intensity analysis shows that more-treated commuting zones saw larger pay declines than less-treated ones, which in turn saw slower pay growth than control commuting zones. Similarly, our OJOI analysis shows that within treated commuting zones, more-exposed occupations experienced larger pay declines than less-exposed ones.²⁹ All estimates control for time-invariant commuting-zone pay differentials, occupation-specific pay trends, and employer-specific pay policies through our fixed effects specifications.

²⁹While both treatment intensity measures are derived from the merger-induced change in employer concentration, they are only modestly correlated, as shown in [Figure A5](#). That two analyses leveraging different sources of variation in treatment intensity yield consistent results strengthens our conclusion that we are identifying a causal effect of the merger.

The CVS-Target merger appears to have robustly and statistically significantly depressed wages for affected workers. But the magnitude of the effect also matters: Which of these estimates should be our preferred estimate of the merger’s effects? The answer to this depends on which of the identification assumptions appear most plausible, and on where the delineation is drawn around the “affected” group of workers. Note that the coefficient magnitudes are not directly comparable across strategies. Our simple DiD strategy estimates an average effect across all job postings in treated commuting zones; our retail DiD and DDD strategies estimate an effect for retail job postings only; and our treatment intensity strategies estimate the difference in effect between more- and less-treated cells (considering all job postings, not just retail). We think all of these have a role to play in quantifying and delineating the merger’s effects. Note also that regardless of strategy, the merger effects we estimate pertain only to the pay in posted job ads. Figure [A1](#) shows that job ads account for a small share of total employment, so our estimates should not be interpreted as reflecting the pay effect of the merger for all workers.

To the extent that we need to highlight a preferred strategy, we focus on the triple-difference estimate that the CVS-Target merger reduced posted annual salary for retail workers by 2.9% (and present this estimate in the abstract and introduction).³⁰ This triple-difference estimate is the most robust to concerns about differential commuting-zone level economic trends emerging around the time of the merger. Note, however, that it is almost certainly a lower bound of the true effect of the merger, since it uses non-retail pay as a control group, when in reality the effects of the merger may not have been concentrated only on retail but may have spilled over to non-retail industries.

Moreover, we showed that the effect of the merger on posted pay was larger for lower-wage occupations. This finding provides a clear answer to the question raised by [Azar](#)

³⁰This finding compares to a 3.9%–8.9% wage effect of completely de-concentrating the Swedish retail pharmacy labor market in [Thoresson \(2024\)](#), and a -2.8% effect of the Brazilian retail pharmacy merger studied by [Guanzioli \(2022\)](#) (although his preferred specification is more similar to our Table 1, specification 5, which we estimate as -1.28%. Even then, there are differences between his setting and data structure and ours.)

[et al. \(2020\)](#) regarding which class of workers (low-paid versus high-paid) suffer the most when employer concentration increases in a labor market, at least with regard to one particular merger and its associated treatment group. 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.

Our OJOI treatment intensity analysis shows that 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, Stansbury and Taska \(2024\)](#) 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.

Furthermore, our estimate of the ΔHHI treatment-intensity strategy (equation [III.4](#)) 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 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.

As [Schubert, Stansbury and Taska \(2024\)](#) emphasize, workers most harmed by labor market concentration are likely those who **i.** work in occupations with a high degree of employer concentration and **ii.** would move primarily to occupations that also feature

high employer concentration when they change their jobs. This paper can be interpreted as lending support to these two 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.

V 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 five methodological approaches to estimate the average treatment effect of the merger using online vacancies data covering the period 2010–2022: a simple commuting-zone-level DiD, a retail-only DiD, a retail triple-difference, and two DiD strategies which leverage variation in treatment intensity by commuting zone and occupation. In addition, we test for heterogeneous effects based on occupational characteristics: occupational pay and outward occupational mobility.

We find evidence of reduction in posted pay in the retail sector by 2.9%, 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. This paper contributes to the scarce yet evolving literature that focuses on the labor market repercussions of mergers. Further, it presents different analytical tools to analyze the labor market outcomes following employer consolidation using one national mega-merger. Finally, it provides clarity as to whether employer monopsony power differs depending on occupational characteristics, including the accessibility of jobs outside a worker's current job.

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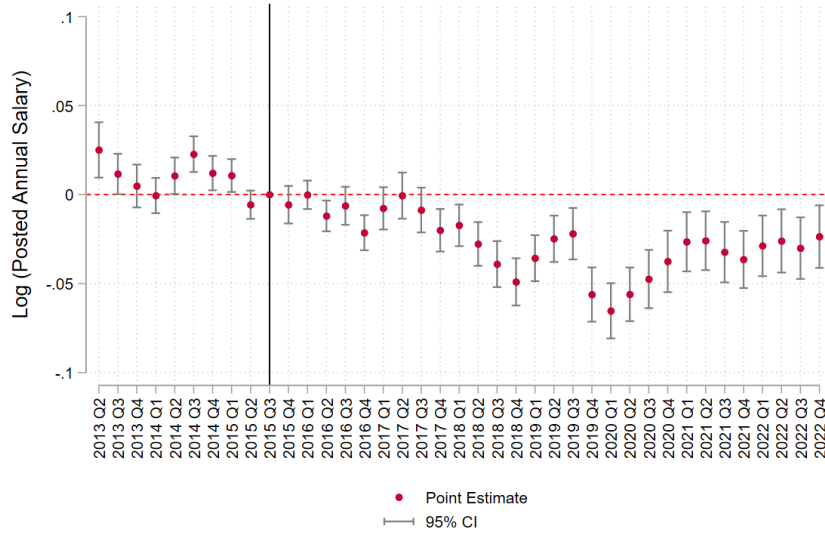
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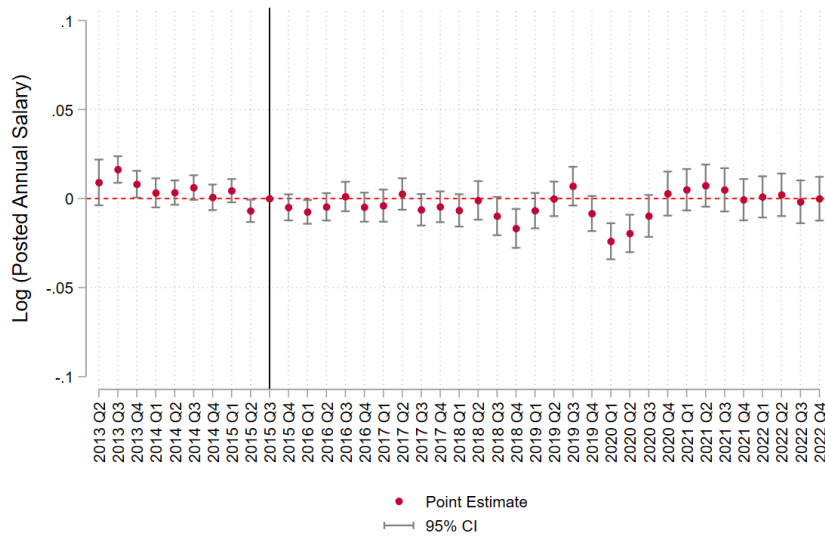
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Figures

Figure 1. Event study estimates for the simple difference-in-differences (DiD).



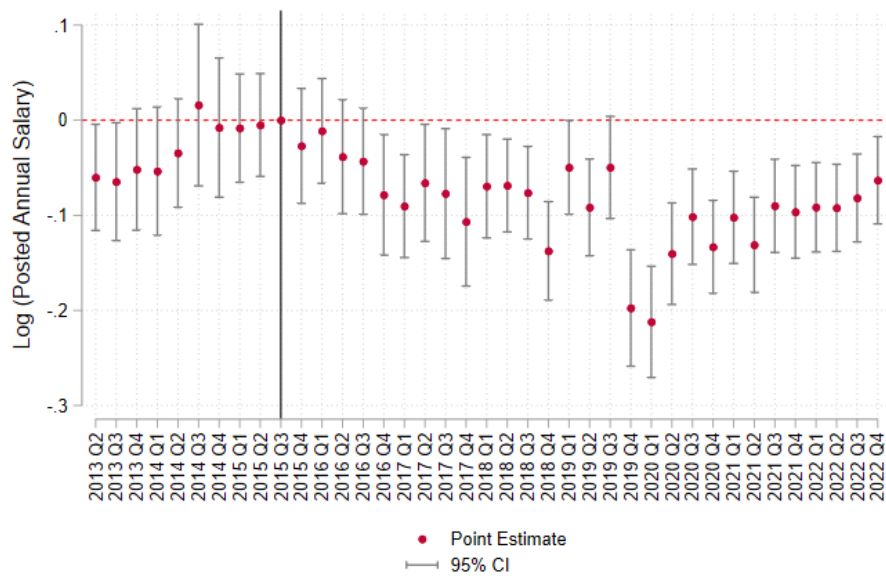
(a) Specification (4)



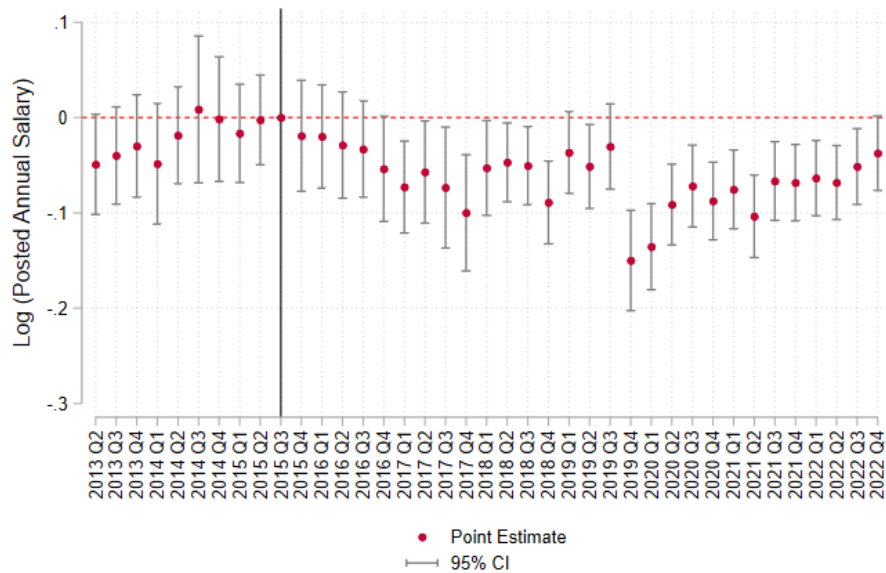
(b) Specification (5)

Notes: These figures show $\hat{\beta}_t$ from estimates of equation III.2 for specifications (4) and (5) reported in Table 1. 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 2. Event study estimates for the simple difference-in-differences (DiD) restricted to the retail subsample.



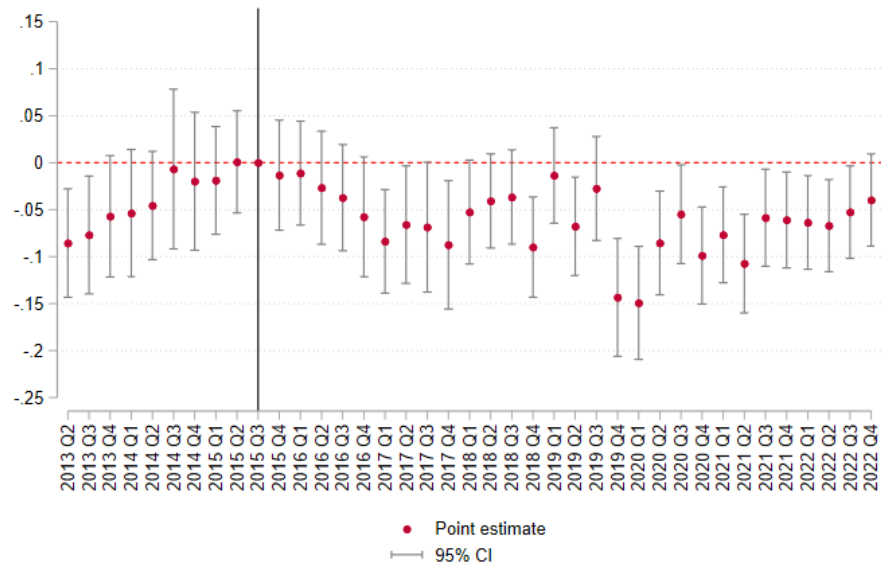
(a) Specification (4)



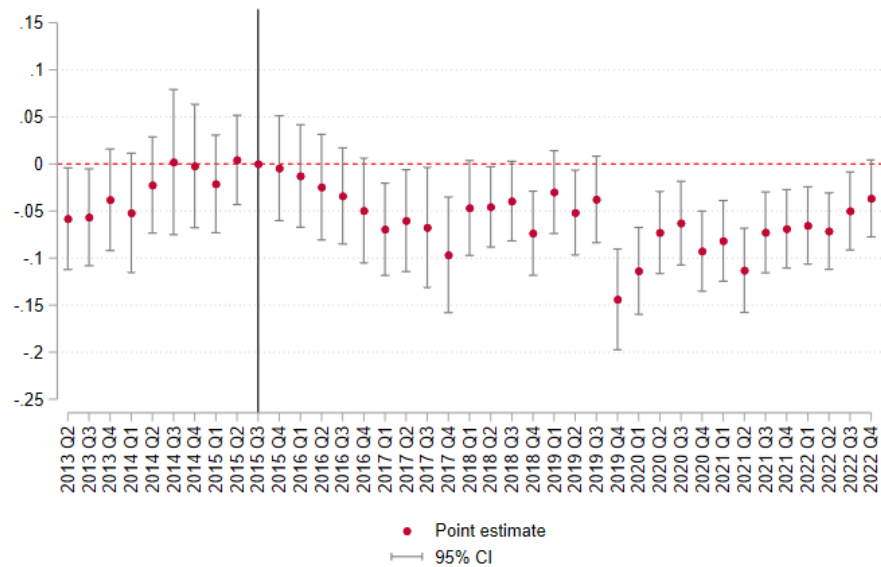
(b) Specification (5)

Notes: These figures show $\hat{\beta}_t$ from estimates of equation III.2 while *restricting* the sample to job vacancies posted in the retail industry which we define in subsection II.C. We present here the results for specifications (4) and (5) reported in Table 2. 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 3. Event study estimates for the retail triple-difference (DDD) model.



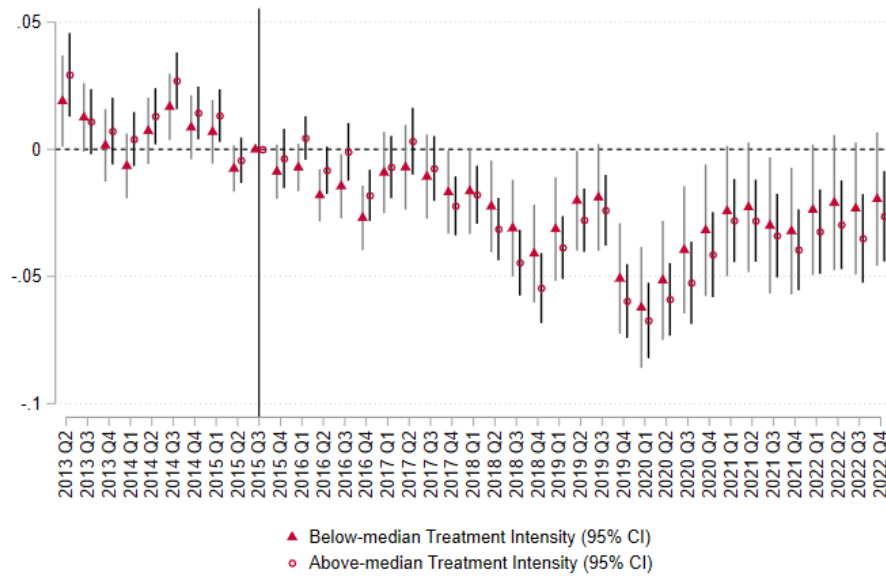
(a) Specification (4)



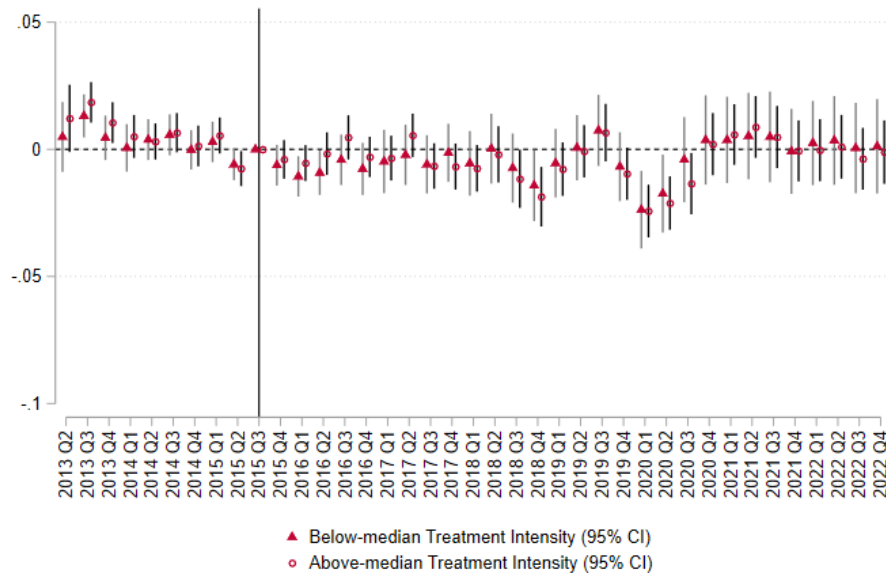
(b) Specification (5)

Notes: These figures show $\widehat{\beta}_{1t}$ estimated using a modified equation III.3 to get an estimate for each quarter relative to the third quarter of 2015 (i.e., one quarter before the merger) for specifications (4) and (5) reported in Table 3. 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. Note that for this research design, we drop observations between Dec. 16th, 2015 and Dec. 31st, 2015 from the analysis altogether, because otherwise 2015Q4 includes observations from both the pre- and post-period and the quarter-by-retail FEs do not fully absorb the 'post' indicator.

Figure 4. Event study estimates for the treatment intensity empirical strategy using predicted Δ HHI.



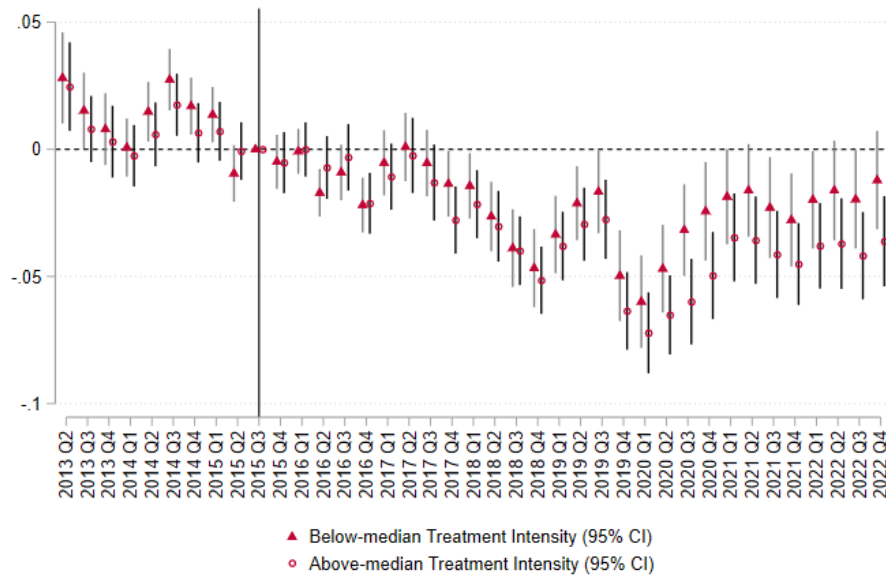
(a) Specification (4)



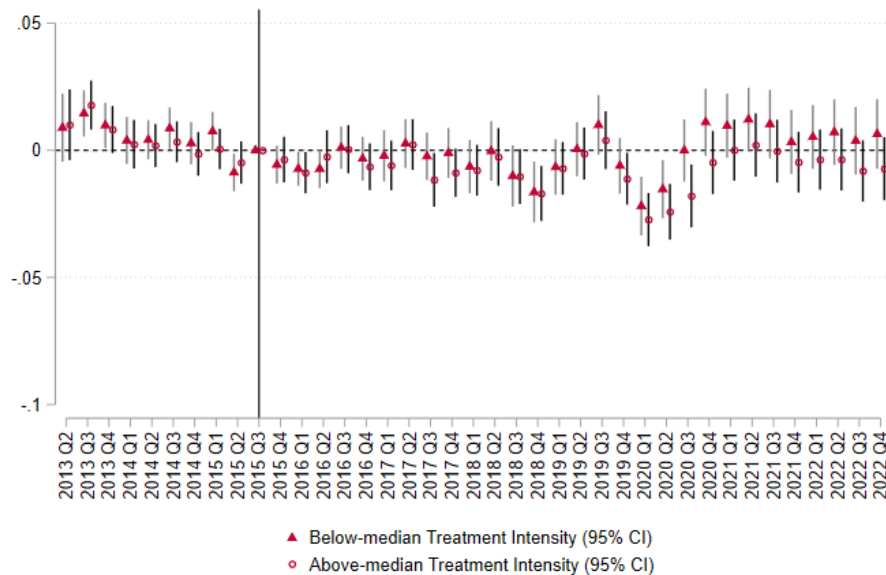
(b) Specification (5)

Notes: These figures show $\hat{\beta}_{1t}$ and $\hat{\beta}_{2t}$ from estimates of equation III.4 for each quarter relative to the third quarter of 2015 (i.e., one quarter before the merger) for specifications (4) and (5) reported in Table 4. We calculate predicted Δ HHI using equation II.1 and use its median value to divide treated commuting zones into below- and above-median. We compute estimates 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 5. Event study estimates for the treatment intensity empirical strategy using OJOI.



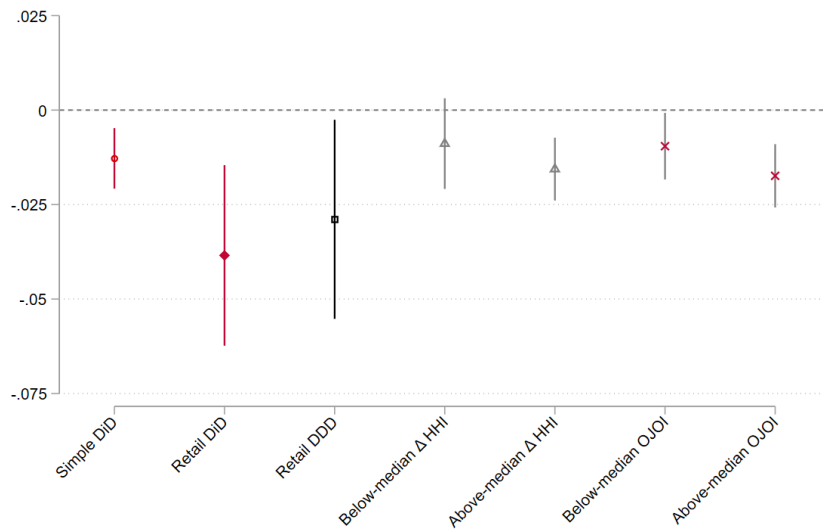
(a) Specification (4)



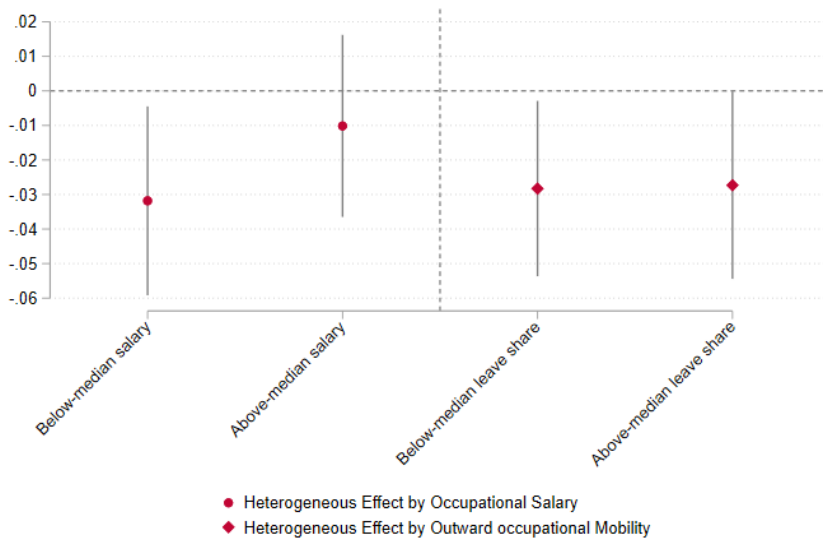
(b) Specification (5)

Notes: These figures show $\widehat{\beta}_{1t}$ and $\widehat{\beta}_{2t}$ from estimates of equation III.5 for each quarter relative to the third quarter of 2015 (i.e., one quarter before the merger) for specifications (4) and (5) reported in Table 5. We calculate OJOI using equation II.2 and use its median value to divide the combinations of occupation-by-treated-commuting-zone into below- and above-median. We compute estimates 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. Visual representation of the estimated merger effects across different research designs and by occupational heterogeneity.



(a) Average Merger Effect by Research Design



(b) Average Merger Effect by Occupational Heterogeneity

Notes: This figure depicts the point estimates and 95% confidence intervals of our preferred specification – the one that includes commuting zone fixed effects, which control for static wage differentials across commuting zones due to factors unrelated to their exposure to the merger; occupation-by-year-quarter fixed effects, which control for differential occupational wage trends over time; industry fixed effects, which control for static wage differentials across industries; and employer fixed effects, which control for employer-specific wage policies– for the different research designs we discuss in section III. These are the estimates reported in the 5th column of Tables 1 – 7.

Tables

Table 1. Simple difference-in-differences (DiD) estimates.

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Log(Salary)	Log(Salary)	Log(Salary)	Log(Salary)	Log(Salary)
Treat \times Post	-0.153*** (0.0109)	-0.0575*** (0.00675)	-0.0469*** (0.00613)	-0.0443*** (0.00625)	-0.0128*** (0.00408)
Constant	10.90*** (0.00797)	10.83*** (0.00496)	10.82*** (0.00450)	10.82*** (0.00459)	10.80*** (0.00315)
Observations	65,220,897	65,220,897	65,220,103	65,220,103	51,893,865
R-squared	0.065	0.579	0.593	0.605	0.743
CZ FE	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO	NO
Occupation FE	NO	YES	NO	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer FE	NO	NO	NO	NO	YES

Notes: This table reports $\hat{\beta}$ estimated using equation III.1 while adding a different set of fixed effects for each column. Note that we have 12,779,459 observations in our full sample that do not have employer information. We drop those observations when estimating the 5th specification which has employer fixed effects. Our preferred specification is presented in Column 5 which has commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry, and employer fixed effects. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2. Simple difference-in-differences (DiD) estimates using the retail subsample.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat × Post	-0.100*** (0.0221)	-0.0474*** (0.0168)	-0.0661*** (0.0142)	-0.0621*** (0.0140)	-0.0385*** (0.0122)
Constant	10.58*** (0.0181)	10.53*** (0.0138)	10.55*** (0.0116)	10.54*** (0.0115)	10.53*** (0.00997)
Observations	854,601	854,572	850,194	850,194	843,887
R-squared	0.086	0.521	0.589	0.594	0.679
CZ FE	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO	NO
Occupation FE	NO	YES	NO	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer FE	NO	NO	NO	NO	YES

Notes: This table reports $\hat{\beta}$ estimated using equation III.1 using the retail subsample **only**, which we define in subsection II.C, while adding a different set of fixed effects for each column. Our preferred specification is presented in Column 5 which has commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry, and employer fixed effects. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3. Retail triple differences (DDD) estimates.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat × Post × Retail	0.0582** (0.0263)	0.0112 (0.0191)	-0.0220 (0.0161)	-0.0205 (0.0160)	-0.0289** (0.0134)
Treat × Post	-0.161*** (0.0118)	-0.0605*** (0.00718)	-0.0489*** (0.00648)	-0.0465*** (0.00658)	-0.0142*** (0.00430)
Constant	10.90*** (0.00855)	10.83*** (0.00520)	10.82*** (0.00469)	10.82*** (0.00476)	10.80*** (0.00327)
Observations	65,151,348	65,151,319	65,146,149	65,146,149	51,838,728
R-squared	0.069	0.580	0.594	0.606	0.744
CZ-by-Retail FE	YES	YES	YES	YES	YES
Quarter-by-Retail FE	YES	YES	NO	NO	NO
Occupation-by-Retail FE	NO	YES	NO	NO	NO
Occupation-by-YQ-by-Retail FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer-by-Retail FE	NO	NO	NO	NO	YES

Notes: This table reports $\widehat{\beta}_1$ and $\widehat{\beta}_2$ estimated using equation III.3 while adding a different set of fixed effects for each column. Each set of fixed effects we estimate is interacted with the retail indicator (except for industry fixed effects) to allow for variations between retail and non-retail postings. Note that we have 12,760,746 observations in our full sample that do not have employer information. We drop those observations when estimating the 5th specification which has employer-by-retail fixed effects. Our preferred specification is presented in Column 5 which has commuting-zone-by-retail fixed effects, occupation-by-year-quarter-by-retail fixed effects, industry, and employer-by-retail fixed effects. Note that for this research design, we drop observations between Dec. 16th, 2015 and Dec. 31st, 2015 from the analysis altogether, because otherwise 2015Q4 includes observations from both the pre- and post-period and the quarter-by-retail FEs do not fully absorb the 'post' indicator. Robust standard errors clustered at the commuting-zone-by-retail level in parentheses.

Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 4. DiD estimates – Heterogeneous merger effects by treatment intensity using predicted Δ HHI.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Post \times Below-median predicted Δ HHI	-0.123*** (0.0160)	-0.0444*** (0.0105)	-0.0365*** (0.00967)	-0.0344*** (0.00990)	-0.00887 (0.00611)
Post \times Above-median predicted Δ HHI	-0.173*** (0.0117)	-0.0659*** (0.00714)	-0.0540*** (0.00632)	-0.0512*** (0.00643)	-0.0156*** (0.00423)
Constant	10.90*** (0.00764)	10.83*** (0.00492)	10.82*** (0.00448)	10.82*** (0.00458)	10.80*** (0.00314)
Observations	65,220,897	65,220,897	65,220,103	65,220,103	51,893,865
R-squared	0.065	0.579	0.593	0.605	0.743
CZ FE	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO	NO
Occupation FE	NO	YES	NO	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer FE	NO	NO	NO	NO	YES
P-value H_0 : Below- median=Above-median	0.006	0.055	0.082	0.102	0.277

Notes: This table reports $\hat{\beta}_1$ and $\hat{\beta}_2$ from equation III.4 while adding a different set of fixed effects for each column. Predicted Δ HHI for each treated commuting zones is computed using equation II.1. The median value of predicted Δ HHI is 80.5. We use that median value to divide treated commuting zones into two groups: below-median and above-median. Note that we have 12,779,459 observations in our full sample that do not have employer information. We drop those observations when estimating the 5th specification which has employer fixed effects. Our preferred specification is presented in Column 5 which has commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry, and employer fixed effects. 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 merger effects by treatment intensity using outside job option index (OJOI).

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat × Post × Below-median OJOI	0.102*** (0.0153)	-0.0566*** (0.00749)	-0.0409*** (0.00695)	-0.0383*** (0.00701)	-0.00969** (0.00449)
Treat × Post × Above-median OJOI	-0.408*** (0.0169)	-0.0603*** (0.00673)	-0.0544*** (0.00622)	-0.0521*** (0.00630)	-0.0170*** (0.00416)
Constant	10.90*** (0.0110)	10.83*** (0.00481)	10.82*** (0.00437)	10.82*** (0.00445)	10.80*** (0.00308)
Observations	64,506,648	64,506,648	64,505,873	64,505,873	51,286,578
R-squared	0.193	0.579	0.593	0.605	0.744
CZ FE	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO	NO
Occupation FE	NO	YES	NO	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer FE	NO	NO	NO	NO	YES
P-value H_0 : Below- median=Above-median	0.000	0.506	0.018	0.013	0.029

Notes: This table reports $\hat{\beta}_1$ and $\hat{\beta}_2$ from equation III.5 while adding a different set of fixed effects for each column. Outside job option index (OJOI) is calculated for each combination of occupation-by-treated-commuting-zone using equation II.2. We use the median value of the OJOI to divide vacancies in treated commuting zones into two groups: below-median and above-median. We were not able to match 562,754 observations (approximately 0.86% of total sample) with OJOI data because the occupations for these specific vacancies (SOC:13-1020, SOC: 51-2090, SOC: 51-3099, SOC:35-2019, SOC:47-2072, SOC:47-3014, and SOC:53-7031) were missing from Schubert, Stansbury and Taska (2024)’s data that we rely on for calculating OJOI. Hence, these observations were dropped when estimating equation III.5. We also drop 12,672,982 observations with missing employer information when estimating the 5th specification which has employer fixed effects. Our preferred specification is presented in Column 5 which has commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry, and employer fixed effects. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 6. Retail triple differences (DDD) estimates – Heterogeneous merger effects by occupational salary rank.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat × Post × Retail × Below-median salary	-0.0346 (0.0266)	0.00457 (0.0206)	-0.0270 (0.0166)	-0.0251 (0.0164)	-0.0318** (0.0139)
Treat × Post × Retail × Above-median salary	0.433*** (0.0250)	0.0329* (0.0177)	0.00993 (0.0156)	0.00931 (0.0156)	-0.0102 (0.0134)
Treat × Post	-0.163*** (0.0117)	-0.0614*** (0.00700)	-0.0497*** (0.00631)	-0.0473*** (0.00640)	-0.0147*** (0.00424)
Constant	10.90*** (0.00847)	10.83*** (0.00507)	10.82*** (0.00457)	10.82*** (0.00463)	10.80*** (0.00322)
Observations	64,952,853	64,952,824	64,947,701	64,947,701	51,678,965
R-squared	0.070	0.581	0.595	0.607	0.745
CZ-by-Retail FE	YES	YES	YES	YES	YES
Quarter-by-Retail FE	YES	YES	NO	NO	NO
Occupation-by-Retail FE	NO	YES	NO	NO	NO
Occupation-by-YQ-by- Retail FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer-by-Retail FE	NO	NO	NO	NO	YES
P-value H_0 : Below- median=Above-median	0.000	0.024	0.000	0.001	0.023

Notes: We use the BLS OEWS national wage estimates to rank occupations (six-digit SOC) based on the occupation’s average annual salary for the year 2015—the year during which the merger took place. Based on the OEWS national wage estimates, the median of the 2015 annual earnings distribution is \$48,150. We divide all occupations in our sample into two groups based on that median value: below-median and above-median occupations. 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). Our sample included 47,367 vacancies (0.07% of total sample) that belonged to one of those occupations and were dropped accordingly. Note that the OEWS wage estimates for 2015 was reported using the 2010 SOC system.

We interact the indicators for below-median-salary and above-median-salary occupations with $\hat{\beta}_1$ in equation III.3. This table reports the interacted $\hat{\beta}_1$ and $\hat{\beta}_2$ while adding a different set of fixed effects for each column. Each set of fixed effects we estimate is interacted with the retail indicator (except for industry fixed effects) to allow for variations between retail and non-retail postings. Our preferred specification is presented in Column 5. Robust standard errors clustered at the commuting-zone-by-retail level in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7. Retail triple differences (DDD) estimates – Heterogeneous merger effects by occupations’ degree of outward mobility using occupational leave share rank.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat × Post × Retail × Below-median leave share	0.218*** (0.0244)	0.0414** (0.0167)	-0.00889 (0.0150)	-0.00910 (0.0150)	-0.0278** (0.0129)
Treat × Post × Retail × Above-median leave share	0.000930 (0.0263)	0.00456 (0.0194)	-0.0235 (0.0163)	-0.0213 (0.0162)	-0.0273** (0.0138)
Treat × Post	-0.163*** (0.0117)	-0.0614*** (0.00701)	-0.0498*** (0.00634)	-0.0475*** (0.00643)	-0.0148*** (0.00424)
Constant	10.90*** (0.00848)	10.83*** (0.00508)	10.82*** (0.00459)	10.82*** (0.00465)	10.81*** (0.00322)
Observations	64,437,071	64,437,042	64,431,920	64,431,920	51,231,452
R-squared	0.069	0.580	0.594	0.606	0.745
CZ-by-Retail FE	YES	YES	YES	YES	YES
Quarter-by-Retail FE	YES	YES	NO	NO	NO
Occupation-by-Retail FE	NO	YES	NO	NO	NO
Occupation-by-YQ-by- Retail FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer-by-Retail FE	NO	NO	NO	NO	YES
P-value H_0 : Below- median=Above-median	0.000	0.000	0.042	0.084	0.943

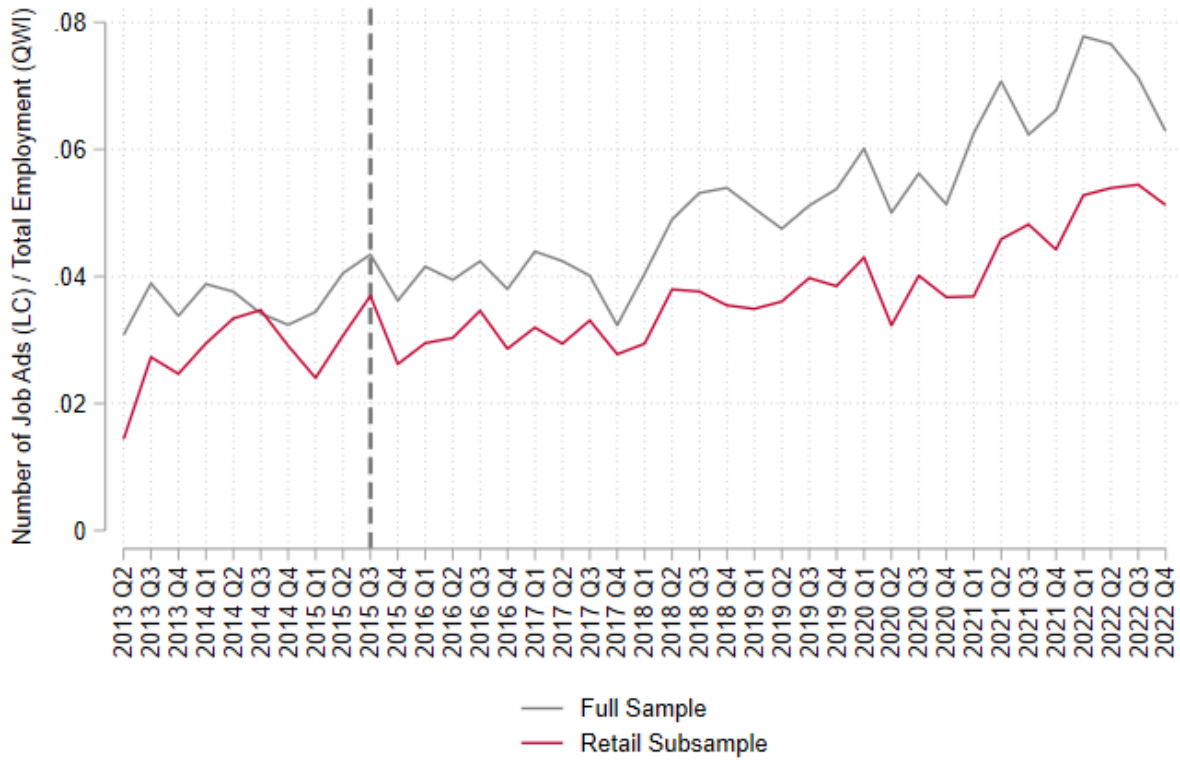
Notes: Occupations are ranked and divided into two groups based on the occupation’s leave share: the probability that a worker who leaves a job in that occupation is observed in a different occupation in their next job, according to the resume data compiled by [Schubert, Stansbury and Taska \(2024\)](#) as explained in [footnote 28](#). We interact the indicators for below-median-leave-share and above-median-leave-share occupations with $\widehat{\beta}_1$ in equation III.3. This table reports the interacted $\widehat{\beta}_1$ and $\widehat{\beta}_2$ while adding a different set of fixed effects for each column. Each set of fixed effects we estimate is interacted with the retail indicator (except for industry fixed effects) to allow for variations between retail and non-retail postings. Our preferred specification is presented in Column 5 which has commuting-zone-by-retail fixed effects, occupation-by-year-quarter-by-retail fixed effects, industry, and employer-by-retail fixed effects. Robust standard errors clustered at the commuting-zone-by-retail level in parentheses.

Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A Appendix

Appendix Figures

Figure A1. Total vacancy count as a share of total employment.



Notes: This figure plots the ratio of the count of job ads in Lightcast divided by the total employment count from Quarterly Workforce Indicators, for both the Full Sample and the Retail Sample (where the latter is defined equivalently in LC and QWI).

Figure A2. Average posted annual salary by commuting zone-based treatment, 2010–2022.

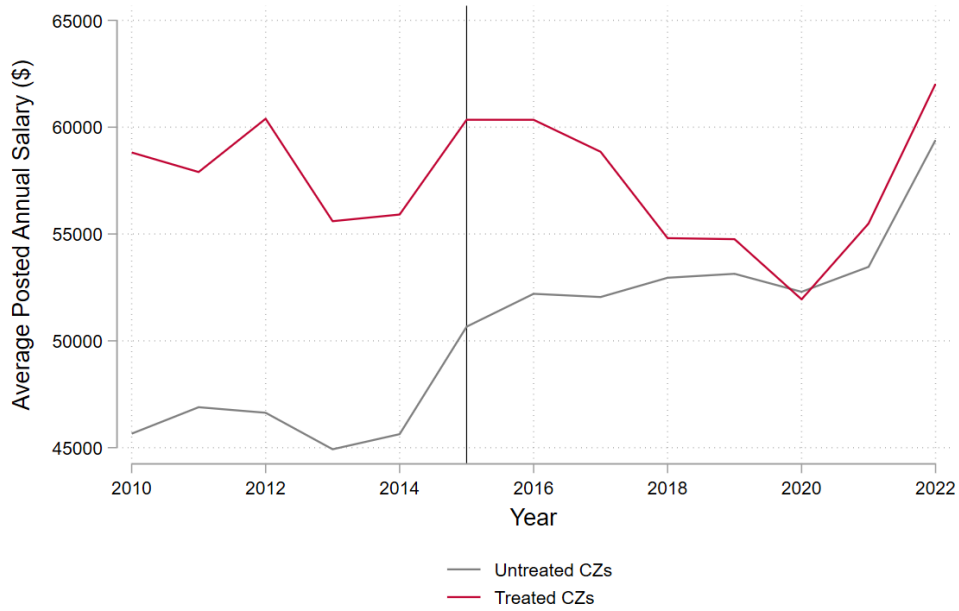


Figure A3. Average posted annual salary by commuting zone-based treatment intensity using predicted Δ HHI, 2010–2022.

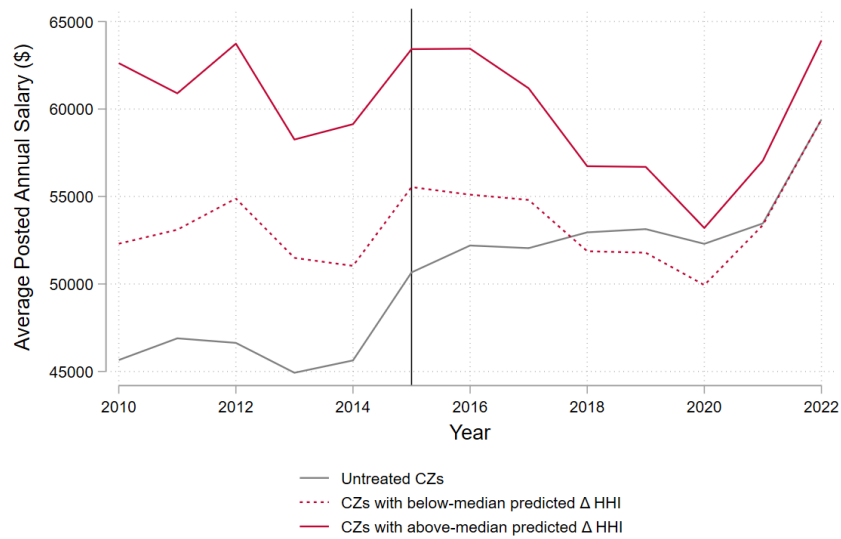


Figure A4. Average posted annual salary by outside job option index (OJOI) rank, 2010–2022.

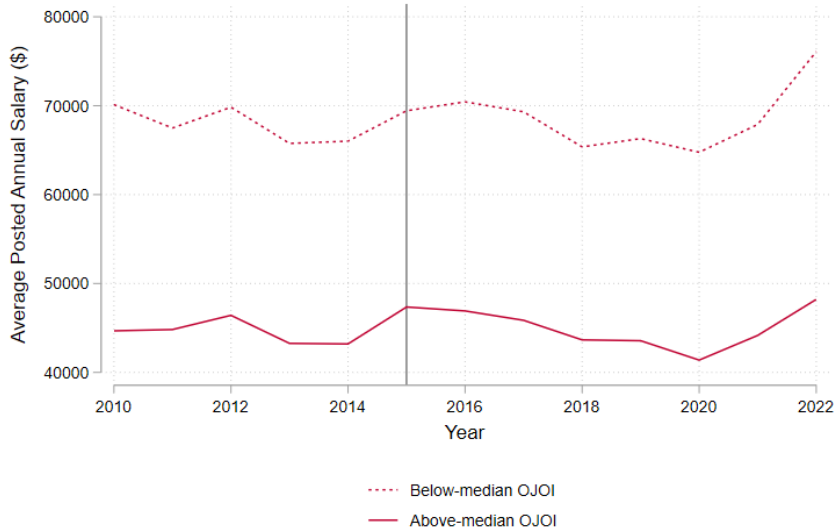
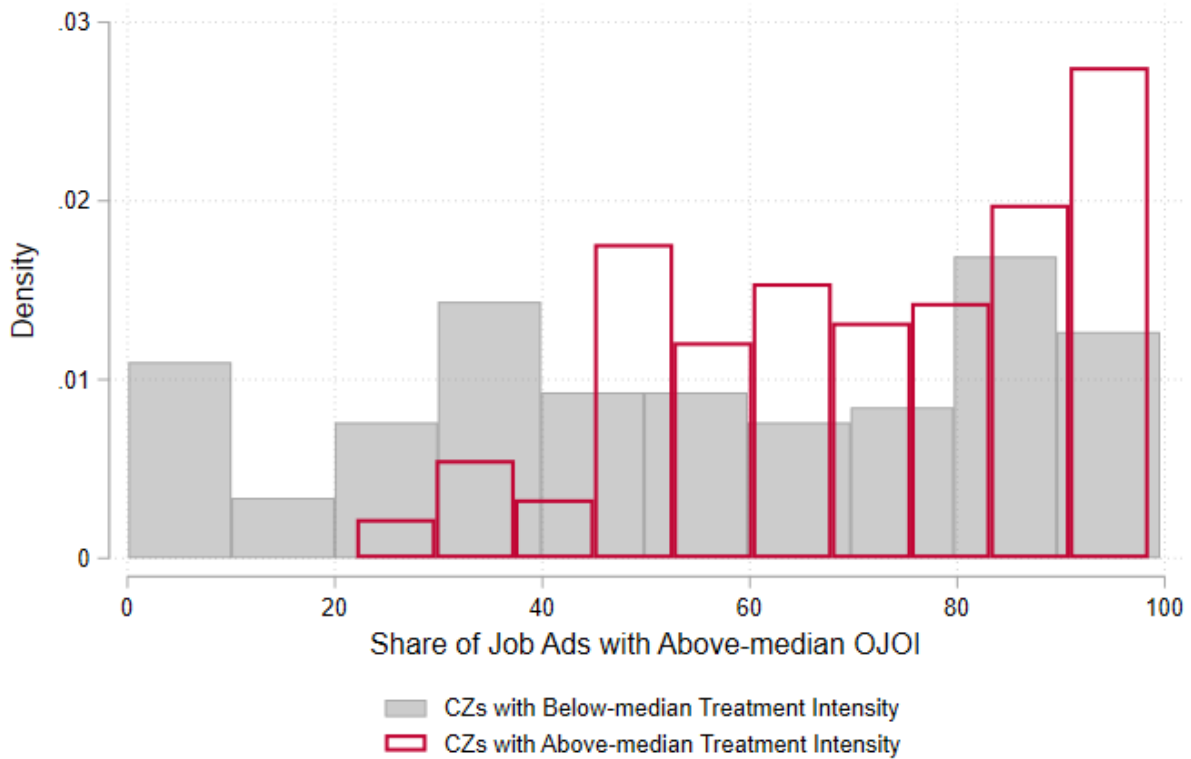


Figure A5. Comparing distribution of share of job ads with above-median OJOI across CZs with below- and above-median Δ HHI.



Notes: This figure depicts a histogram of the share of the job ads that have above-median outside job option index (OJOI) out of all total vacancies for each treated commuting zone. We create this histogram separately for commuting zones with above-median predicted Δ HHI and below-median predicted Δ HHI.

Appendix Tables

Table A1. Average annual salary by treatment & time of treatment, 2010-2022.

	Pre-treatment		Post-treatment	
	Untreated CZs	Treated CZs	Untreated CZs	Treated CZs
Salary	46,858 (33,366)	58,112 (39,287)	54,649 (36,485)	57,059 (37,975)
Obs.	1,546,076	11,975,760	3,803,448	47,895,613

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

Table A2. Simple DiD estimates using vacancies with no missing employer.

VARIABLES	(1) Log(Salary)	(2) Log(Salary)	(3) Log(Salary)	(4) Log(Salary)	(5) Log(Salary)
Treat \times Post	-0.146*** (0.0115)	-0.0533*** (0.00770)	-0.0422*** (0.00692)	-0.0390*** (0.00707)	-0.0128*** (0.00408)
Constant	10.90*** (0.00891)	10.83*** (0.00596)	10.82*** (0.00536)	10.82*** (0.00547)	10.80*** (0.00315)
Observations	52,441,438	52,441,438	52,440,475	52,440,475	51,893,865
R-squared	0.063	0.590	0.605	0.621	0.743
CZ FE	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	NO	NO	NO
Occupation FE	NO	YES	NO	NO	NO
Occupation-by-YQ FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer FE	NO	NO	NO	NO	YES

Notes: This table reports $\hat{\beta}$ estimated using equation III.1 with the restriction to vacancies with no missing employer information. We add a different set of fixed effects for each column. This table is an additional robustness check to the estimates in Table 1 to ensure that our results do not change when dropping those observation with missing employer information. Our preferred specification is presented in Column 5 which has commuting zone fixed effects, occupation-by-year-quarter fixed effects, industry, and employer fixed effects. Robust standard errors clustered at the commuting zone level in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A3. Top 10 occupations in terms of observation count for each outward occupational mobility category (below-median and above-median leave share).

Outward Mobility Rank	Occupation Title (6-digit SOC)	Count
Below-median leave share	Heavy and Tractor-Trailer Truck Drivers	3,009,886
	Registered Nurses	2,135,862
	Secretaries and Administrative Assistants, Except Legal, Medical, and Executive	1,649,697
	Computer Occupations, All Other	1,327,688
	Software Developers, Applications	1,313,082
	Bookkeeping, Accounting, and Auditing Clerks	953,457
	Home Health Aides	915,959
	Accountants and Auditors	868,391
	Janitors and Cleaners, Except Maids and Housekeeping Cleaners	828,324
	Security Guards	644,870
Above-median leave share	Customer Service Representatives	1,967,882
	Laborers and Freight, Stock, and Material Movers, Hand	1,880,563
	Retail Salespersons	1,363,170
	Sales Representatives, Wholesale and Manufacturing, Except Technical and Scientific Products	1,117,600
	Production Workers, All Other	943,980
	Maintenance and Repair Workers, General	798,738
	Sales Representatives, Wholesale and Manufacturing, Technical and Scientific Products	704,829
	Driver/Sales Workers	691,575
	Receptionists and Information Clerks	666,489
	First-Line Supervisors of Office and Administrative Support Workers	610,950

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 $t + 1$, as explained in [footnote 28](#). For more details on the Lightcast resume data and the construction of these occupational mobility measures, see [Schubert, Stansbury and Taska \(2024\)](#).

B 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 (238 treated commuting zones vs. 469 control commuting zones), they account for the vast majority (approximately 92%) of our full sample. Figure A2 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? Our preferred estimation approach, the retail DDD, assuages this concern by controlling for differential commuting-zone level economic trends emerging around the time of the merger. However, there could still be a concern that about differential pay trends between urban and non-urban areas.

We address any lingering identification concerns by re-estimating equation III.3 with an additional set of fixed effects 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.

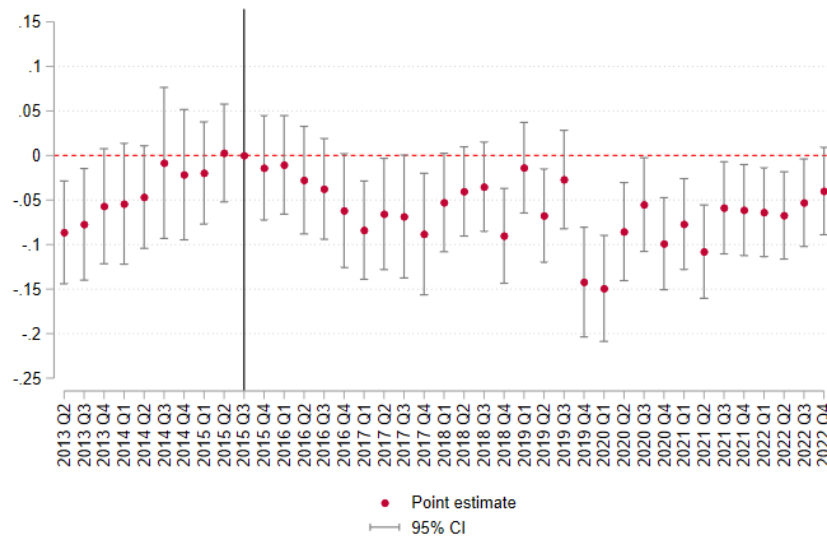
The DDD approach explained in subsection III.C assumes that posted pay in retail vacancies is more affected by the merger than that of non-retail vacancies. Hence, it used non-retail vacancies to identify commuting-zone differential pay trends. Adding urban-by-time fixed effects is instead identifying time-varying pay trends between urban and non-urban for all vacancies and not just retail postings. Table B1 presents the DDD regression results, and Figure B1 shows event study plots, after controlling for time-varying pay differentials between urban and rural areas. As you can see from both the regression table and event study plots, the results are very similar to the DDD results reported in Table 3 and Figure 3, which indicates that they are not erroneously interpreting posted pay differentials between urban and rural areas as a merger effect.

Table B1. Retail triple differences (DDD) estimates with urban-by-year-quarter fixed effects.

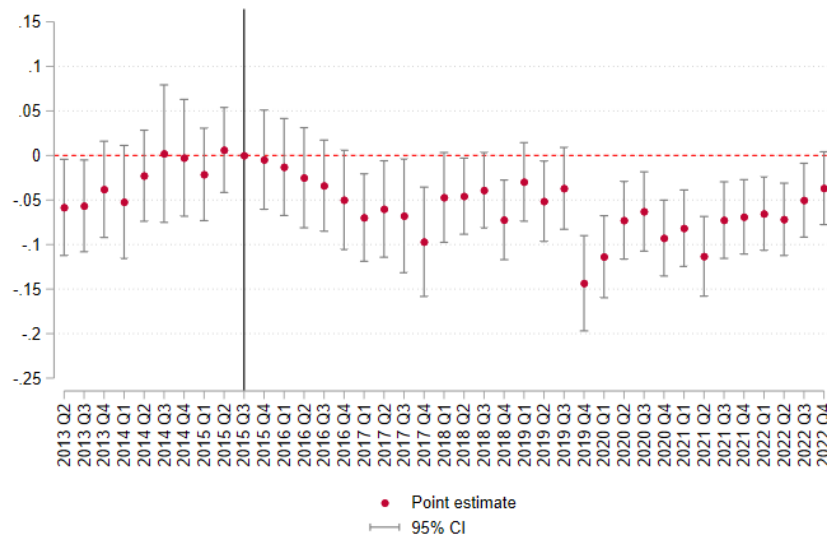
VARIABLES	(1)	(2)	(3)	(4)	(5)
	Log(Salary)	Log(Salary)	Log(Salary)	Log(Salary)	Log(Salary)
Treat × Post × Retail	0.0635** (0.0264)	0.0137 (0.0192)	-0.0199 (0.0160)	-0.0186 (0.0159)	-0.0280** (0.0134)
Treat × Post	-0.151*** (0.0122)	-0.0541*** (0.00756)	-0.0439*** (0.00688)	-0.0418*** (0.00696)	-0.0139*** (0.00453)
Constant	10.89*** (0.00880)	10.82*** (0.00548)	10.82*** (0.00498)	10.82*** (0.00504)	10.80*** (0.00345)
Observations	65,151,348	65,151,319	65,146,149	65,146,149	51,838,728
R-squared	0.069	0.580	0.594	0.606	0.744
CZ-by-Retail FE	YES	YES	YES	YES	YES
Quarter-by-Retail FE	YES	YES	NO	NO	NO
Occupation-by-Retail FE	NO	YES	NO	NO	NO
Occupation-by-YQ-by-Retail FE	NO	NO	YES	YES	YES
Industry FE	NO	NO	NO	YES	YES
Employer-by-Retail FE	NO	NO	NO	NO	YES
Urban-by-YQ FE	YES	YES	YES	YES	YES

Notes: This table reports $\widehat{\beta}_1$ and $\widehat{\beta}_2$ estimated using equation III.3 with urban-by-year-quarter fixed effects while adding a different set of fixed effects for each column. Our measure of urban-ness is whether a job ad is posted in a Metropolitan Statistical Area (MSA). Each set of fixed effects we estimate is interacted with the retail indicator (except for industry fixed effects) to allow for variations between retail and non-retail postings. Note that we have 12,760,746 observations in our full sample that do not have employer information. We drop those observations when estimating the 5th specification which has employer-by-retail fixed effects. Our preferred specification is presented in presented in Column 5. Robust standard errors clustered at the commuting-zone-by-retail level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Figure B1. Event study estimates for the triple differences (DDD) specification with urban-by-year-quarter fixed effects.



(a) Specification (4)



(b) Specification (5)

Notes: These figures show $\widehat{\beta}_{1t}$ estimated using a modified equation III.3 in which urban-by-calendar-quarter fixed effects are included. $\widehat{\beta}_{1t}$ is estimated for each quarter relative to the third quarter of 2015 (i.e., one quarter before the merger). Our measure of urbanness is whether the job ad is posted in a Metropolitan Statistical Area. We show here $\widehat{\beta}_{1t}$ for specifications (4) and (5) reported in Table 3. 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.

C Matching 2018 SOC to 2010 SOC Codes

The main dataset used in this paper is the 2024 Lightcast “US Postings” dataset. One challenge we faced with that vintage of the LC dataset was its use of the 2018 SOC system to report occupational information, whereas the occupational mobility data from [Schubert, Stansbury and Taska \(2024\)](#) use the 2010 SOC system. The 2010 classification was also used in older Lightcast vintages. To harmonize SOC codes between our 2024 LC dataset and [Schubert, Stansbury and Taska \(2024\)](#)’s mobility data, we relied on the crosswalk between 2010 and 2018 SOC codes published by the Bureau of Labor Statistics (BLS). However, that mapping was not one-to-one, meaning that some single 2018 SOC codes are mapped into multiple 2010 SOC codes and vice versa. To conduct all the occupational heterogeneity and the outside job option index (OJOI) analyses that use the mobility data, we had to construct a one-to-one matching between 2018 SOC codes and 2010 SOC codes.

Obviously, it is not a problem if a single 2010 SOC code is mapped into multiple 2018 SOC codes because we can simply assign the same 2010 SOC code to those 2018 codes. The other way around was the challenge. We had to figure out a way to map the single 2018 codes in the 2024 LC data version we have that are mapped to multiple 2010 SOC codes. For those non one-to-one mappings, we used an older vintage of the LC dataset with the 2010 SOC classification to compute the frequency of each 2010 SOC code. Then, we create a *year-specific* one-to-one crosswalk between the 2018 SOC code and the most-frequent 2010 SOC code for that year, among those 2010 codes that comprise the 2018 code.

For example, 35-2023 is the 2018 SOC code for Fast Food and Counter Workers. It is mapped to the 2010 SOC codes 35-2021 and 35-2022. In the older LC data vintage, which used the 2010 classification, we had 260,256 observations for 35-2021 and 81,074 observations for 35-2022 in 2014. Thus, we match the 2018 SOC code 35-2023 to the 2010 SOC code 35-2021 in 2014.

After we did the exercise, we were left with 13 SOC codes in our sample that we could not match to the 2010 SOC codes because they were not included in the BLS list of 2018 SOC codes, which suggests that they were misrecorded by LC. We tried to find a way to infer the corresponding 2010 SOC codes by looking at the occupation title and matching it to the closest code in 2018 taxonomy. Then, we find its match in the 2010 codes. When occupation titles were not clear, we looked at top job titles in those SOC codes to find the closest match. When we did this exercise using our full analysis sample, we were able to match all but one of those 13 SOC codes as depicted in [Table C1](#). In addition, in our replication package, we provide a clear documentation for all the steps we took to come up with this matching across the 2018 and 2010 SOC classifications.

Table C1. SOC Codes that were not matched to 2010 SOC codes using the BLS crosswalk.

Unmatched 2018 SOC	Occupation Title in LC Data	Matched 2010 SOC	2010 Occupation Title	Reason
31-1128	Home Health and Personal Care Aides	31-1011	Home Health Aides	Based on occupation's title.
25-9045	Teaching Assistants, Except Postsecondary	25-9041	Teacher Assistants (Excludes "Graduate Teaching Assistants")	Based on occupation's title.
13-1028	Buyers and Purchasing Agents	13-1020	Buyers and Purchasing Agents	Based on occupation's title.
51-2098	Miscellaneous Assemblers and Fabricators	51-2090	Miscellaneous Assemblers and Fabricators	Based on occupation's title
29-2018	Clinical Laboratory Technologists and Technicians	29-2012	Medical and Clinical Laboratory Technicians	Based on occupation's title and top job titles in terms of observation count.
25-1099	Postsecondary Teachers	NA	NA	In the 2010 codes, Postsecondary teachers was a broader SOC (25-1000) which includes different SOC codes for multiple occupations. It was hard to match it to a specific SOC code.
21-1018	Substance Abuse, Behavioral Disorder, and Mental Health Counselors	21-1011	Substance Abuse and Behavioral Disorder "Counselors"	Based on occupation's title and top job titles in terms of observation count.

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Table C1 continued from previous page

Unmatched 2018 SOC	Occupation Title in LC Data	Matched 2010 SOC	2010 Occupation Title	Reason
53-1047	First-Line Supervisors of Transportation and Material Moving Workers, Except Aircraft Cargo Handling Supervisors	53-1031	First-Line Supervisors of Transportation and Material-Moving Machine and Vehicle Operators	Based on occupation's title.
51-2028	Electrical, Electronic, and Electromechanical Assemblers, Except Coil Winders, Tapers, and Finishers	51-2022	Electrical and Electronic Equipment Assemblers	Based on occupation's title.
39-7018	Tour and Travel Guides	39-7011	Tour Guides and Escorts	Based on occupation's title and top job titles in terms of observation count.
13-2028	Property Appraisers and Assessors	13-2021	Appraisers and Assessors of Real Estate	Based on occupation's title.
47-4098	Miscellaneous Construction and Related Workers	47-4091	Segmental Pavers	Based on occupation's title and top job titles in terms of observation count.
25-2052	Special Education Teachers, Kindergarten and Elementary School	25-2052	Special Education Teachers, Kindergarten and Elementary School	Based on occupation's title.