Employer Consolidation and Wages: Evidence from Hospitals*

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Abstract

We test whether wage growth slows following employer consolidation by examining hospital mergers. We find evidence of reduced wage growth in cases where both (i) the increase in concentration induced by the merger is large and (ii) workers' skills are industry-specific. In all other cases, we fail to reject zero wage effects. We argue that the observed patterns are unlikely to be explained by merger-related changes aside from labor market power. Wage growth slowdowns are attenuated in markets with strong labor unions, and we do not observe reduced wage growth after out-of-market mergers that leave local employer concentration unchanged.

JEL codes: I11, J31, J42, L41

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1 Introduction

Labor market concentration has been advanced as a possible contributor to income inequality and wage stagnation. Recent academic work has documented a negative relationship between labor market concentration and wages (Azar et al. 2017; Benmelech et al. 2018; Qiu and Sojourner 2019; Rinz 2018), leading to pressure on antitrust authorities to consider labor monopsony effects in merger review.[1,2] Merger review provides a natural policy lever for curtailing labor market consolidation through established regulatory mechanisms (CEA 2016; Naidu et al. 2018; Hemphill and Rose 2018; Marinescu and Hovenkamp 2018; Krueger and Posner 2018). However, there is limited *direct* evidence to suggest that employer mergers meaningfully reduce wage growth. If expanded merger review is a leading proposal for dealing with labor market concentration, then it is important to examine whether actual mergers—as opposed to other sources of variation in employer concentration—have contributed to slower wage growth. Indeed, even if concentration can be shown to causally reduce wages, antitrust authorities are not currently empowered to prosecute high *levels* of concentration in the absence of a merger or other anticompetitive conduct (Rose 2018).

To provide evidence on this question, we examine the effects of hospital mergers between 2000 and 2010 on the wages of hospital workers. We begin by documenting that the negative relationship between concentration and wages found in recent papers replicates in the hospital context, and then turn to an analysis that isolates merger-induced concentration changes. In descriptive aggregate regressions of wages on hospital concentration, we estimate that wages in markets with a Herfindahl-Hirschman index (HHI) of 2,500 are 1 to 4 percent lower than in perfectly competitive markets, even after controlling for labor market (commuting zone) and time (year) fixed effects. Of course, this negative association between concentration and wages is consistent with explanations besides labor market power. This creates a challenge in translating these results, and the results of other recent work in this literature, into prescriptions for antitrust policy. For instance, if an employer exits a market due to weakening product demand, such a change will both raise measured employer concentration and sap local labor demand. Either of these forces may reduce wages, and thus

[1]In September 2017, Senator Amy Klobuchar (D-MN) introduced a Senate bill that would have required antitrust authorities to include labor market considerations in merger review (Hipple 2017). In October 2018, the Federal Trade Commission held hearings to debate the consideration of labor market power in merger review (Arlington and States 2018).

[2]Hereafter, we refer to monopsony and related notions of employer market power using the more general terminology "labor market power."

separating the effects of employer consolidation from other coincident changes in labor demand is a significant empirical challenge.

Our main analyses therefore directly examine the impact of concentration-increasing mergers on subsequent wage growth. By focusing on merger-induced changes in employer concentration, we isolate the portion of the aggregate relationship between concentration and wages that is most directly related to employer consolidation. In difference-in-differences regressions, we compare wage growth in labor markets that experience a concentration-increasing merger to wage growth in labor markets without any merger activity. We examine wage trajectories separately as a function of workers' skill level and the industry specificity of their human capital. We group workers into three categories: unskilled workers whose skills are not specific to the hospital industry, such as cafeteria workers; skilled workers in non-medical occupations, such as the employee benefits department; and skilled health care professionals, specifically nursing and pharmacy workers. We find varied results depending on (i) the worker category and (ii) the magnitude of the change in concentration induced by the merger, with patterns consistent with theory.

For unskilled workers, we find no evidence of differences in wage growth post-merger, irrespective of the change in employer concentration induced by the merger. For the two categories of skilled workers, we find evidence of reduced wage growth, but only in cases where the concentration increase induced by the merger is large. For the top quartile of concentration-increasing mergers, we estimate that wages are 4.1 percent lower for skilled non-health professionals and 6.3 percent lower for nursing and pharmacy workers than they would have been absent the merger, measured over the four years following the merger. These estimates imply 1.1 percentage point slower annual wage growth for skilled non-health professionals and 1.7 percentage point slower growth for nursing and pharmacy workers, representing wage growth reductions of more than 25 percent of baseline wage growth rates. Moreover, the estimated effects do not appear to be generated by pre-merger differences in wage trends nor post-merger changes in local economic conditions. While the magnitudes we find are specific to our empirical context, the findings are consistent with the broader economic mechanism of an increase in labor market power dampening wage growth. Wages of unskilled workers, whose effective set of potential employers is much broader than hospitals, see no discernible change. Wages of workers whose skills are less transferable to employers outside the hospital industry are adversely affected by mergers, but only when the mergers are large enough to meaningfully affect labor market concentration.

The observed post-merger reductions in wage growth are consistent with at least two broad classes of mechanisms: classical monopsony and labor market search frictions (Robinson 1969; Mortensen and Pissarides 1999). Under classical monopsony, an employer's power to pay wages below the marginal product of labor arises from the ability to restrict employment below its competitive equilibrium level. In models with labor market search frictions, on the other hand, employers need not necessarily restrict employment in order to pay wages below the marginal product of labor. To shed light on which class of models might be operative in our context, we also estimate difference-in-differences regressions with the level of employment as the outcome variable. We do not find evidence of employment suppression, which is most consistent with models of wage setting involving search frictions rather than classical monopsony.[3]

To further explore the consistency of the wage results with a labor market power mechanism, we estimate several additional specifications. First, we examine whether the presence of countervailing worker power attenuates post-merger reductions in wage growth. Estimates using two measures of worker power suggest that they do: both high levels of unionization and a pro-union environment (as measured by the absence of right-to-work laws) appear to mitigate the estimated negative wage effects. This finding is consistent with recent work showing a weaker relationship between concentration and wages in markets with high unionization rates (Benmelech et al. 2018; Qiu and Sojourner 2019). Second, we examine whether mergers in and of themselves are likely responsible for the observed wage patterns, outside of their potential wage effects through labor market concentration. To do so, we examine the wage effects of "out-of-market" mergers in which the merging hospitals are located in non-overlapping labor markets.[4] These mergers therefore do not affect local labor market concentration. Since we can plausibly rule out labor market power in these cases a priori, any observed wage impacts following such mergers can instead be attributed to alternative non-labor market power mechanisms. We find no changes in wage trajectories following out-of-market mergers, irrespective of (i) the worker category and (ii) the pre-existing level of concentration in the market.

The proper antitrust treatment of post-merger wage or other cost reductions that are obtained

[3]One reason to suspect that classical monopsony might be less relevant in the hospital context is that hospitals face relatively inelastic demand (e.g., Newhouse et al. (1993); Finkelstein (2014)).

[4]Over our sample period of 2000 to 2010, nearly half of all hospital mergers in our data did not involve any commuting zone overlap between the merging parties.

via increased market power is an unsettled question (Carlton and Israel 2011; Berman Jackson 2017; Hemphill and Rose 2018). In our context, post-merger wage reductions may on one hand be viewed as anticompetitive harm due to the merger. On the other hand, they may instead be viewed as input cost efficiencies, a completely opposite interpretation of the same phenomenon. As we illustrate in a simple conceptual framework, what arguably distinguishes wage reductions from other input cost reductions is that wage reductions have a direct effect on consumer welfare. Because the workers affected by a merger are also consumers, a tightening of their budget constraint due to reduced wages decreases their welfare. Accounting for this consumer subgroup may therefore lead to the merger decreasing consumer welfare, even if the merger leads to lower prices. This exercise shows that antitrust authorities may not need to depart from a consumer welfare standard in order to integrate labor market effects into merger review.

On balance, our results suggest that increased employer labor market power via mergers may indeed contribute to wage stagnation, but that such effects may apply in relatively narrow circumstances. Wage growth slows only following mergers that lead to substantial increases in employer concentration, and only for workers whose skills are less transferable outside of the industry. The remainder of the paper proceeds as follows. Section 2 connects the paper to the related literature. Section 3 describes our wage and merger data. Section 4 presents the empirical analysis and discusses the implications for merger review. Section 5 concludes.

2 Related Literature

Although there is little direct evidence on the wage effects of mergers, there is evidence to suggest a link between employer concentration and wages. Recent work examining the relationship between employer concentration and wages finds a robust negative association: in aggregate, higher employer concentration is associated with lower wages (Azar et al. 2017; Benmelech et al. 2018; Rinz 2018; Hershbein et al. 2019; Qiu and Sojourner 2019). This association holds across a range of data sources and specifications. Benmelech et al. (2018) focus on manufacturing, and define the geographic component of the labor market at the narrow level of a county. Azar et al. (2017) examine a variety of occupations (defined by detailed six-digit SOC occupation codes) and define the geographic component of the labor market at the broader level of a commuting zone. Rinz (2018) also utilizes the commuting zone, defining industries using four-digit NAICS codes. Qiu and Sojourner (2019) include controls for product-market concentration in their wage regressions. The data used to construct measures of employer concentration also vary across studies. Azar et al. (2017) compute vacancy concentration using recent job postings data from an online job board, whereas Benmelech et al. (2018) and Rinz (2018) utilize long panels of actual employment from the Census Bureau's Longitudinal Business Database. Qiu and Sojourner (2019) use proprietary data from a commercial data and analytics company. Despite the differences in data sources and modeling choices, each of these studies reports a negative relationship between employer concentration and wages.[5]

As discussed in the introduction, we contribute to this literature by directly examining the wage effects of mergers. Mergers induce well-measured, discontinuous increases in concentration whose cause we can pin down cleanly. Moreover, mergers are amenable to regulatory intervention under existing legislation. Our findings suggest that the range of settings in which employer mergers meaningfully impact the labor market may be narrower than indicated by the aggregate analyses in recent papers. Nevertheless, in those settings where mergers have meaningful wage impacts, we find that they are consistent with a labor market power mechanism.

More broadly, the paper connects to the literature documenting the presence and uses of labor market power. Dube et al. (2018) find a surprising degree of employer power in seemingly competitive online markets for short-term labor. Jeffers (2017) shows diminished labor mobility due to employer non-compete clauses. Krueger and Ashenfelter (2018) document wage suppression through non-poaching agreements within franchise firms.

The paper is also closely related to the literature on hospital market power and consolidation, which is a subject of active academic and policy debate (Gaynor and Town 2012; Gaynor 2018). The market for hospital employment of nurses served as an early empirical setting for studying employer labor market power. Sullivan (1989) uses estimates of the firm-level elasticity of nursing labor supply to show that hospitals possessed labor market power in the 1980s. Staiger et al. (2010) leverage quasi-exogenous wage hikes in some hospitals to estimate the residual elasticity of nursing labor supply for competing hospitals, and conclude that hospitals have some labor market power.[6]

[5]Direct comparisons of magnitudes across studies must account for the differences in market definition, wage measures, and other empirical choices. Azar et al. (2017) estimate that increasing concentration from the 25th to 75th percentile—roughly 6,000 HHI points in their data—is associated with a wage reduction of 17 percent. Benmelech et al. (2018) estimate that a one standard deviation increase in concentration—roughly 3,500 HHI points in their data—is associated with a wage reduction of 1 to 2 percent. Rinz (2018) estimates that increasing concentration from the median to the 75th percentile is associated with a wage reduction of about 10 percent.

[6]The detailed nurse wage survey used in these papers was discontinued after 1992, prior to the start of our sample period.

Currie et al. (2005) use hospital mergers to examine the effects of system ownership on nursing employment, finding no wage effects but an increase in nurse effort. More recently, DePasquale (2018) examines nearly three decades of hospital mergers and finds no impact on average hospital salaries. Notably, these papers do not distinguish between merger events based on the degree of consolidation induced by the merger. Our paper adds to this literature by examining the magnitude of merger-induced increases in local employer concentration and by distinguishing between workers with varying levels of skill and skill specificity.

Finally, our paper adds to a recent wave of papers that use retrospective merger analyses to shed light on frontier issues in antitrust economics. New insights from this growing literature advance the understanding of cross-market mergers (Dafny et al.; Lewis and Pflum 2017), merger-facilitated collusion (Miller and Weinberg 2017), and the price effects of vertical mergers (Luco and Marshall 2018). Like other papers in this literature, our analysis must confront the challenge of attributing the effects of mergers to a mechanism—in this case, labor market power. Mergers may affect wages through other channels besides labor market power, such as changing the production technology of the merged entity. Such issues of attribution are not unusual in retrospective analyses of mergers, and our empirical strategy attempts to resolve them to the extent possible. Even with these caveats in place, the benefits of examining actual mergers are substantial, generating both economic insights and guidance for antitrust regulators.

3 Data

This section briefly describes the key sources of data used in the empirical analysis. Appendix A provides additional details and summary statistics. Our empirical context is the hospital industry, which is a fitting context for studying the labor market effects of mergers. The industry is large, employing 5 million workers in 2018 (BLS 2018a), and has a high rate of merger and acquisition activity. In addition, data on hospital wages are unusually comprehensive. We observe wages for essentially the universe of hospitals, measured separately for several worker categories with varying degrees of skill and skill specificity. These wage measures come from the Healthcare Cost Report Information System (HCRIS). We construct a panel of hospital ownership and mergers from multiple sources of industry data. Finally, to measure employer concentration, we utilize data on hospital employment from HCRIS and data on broader health care industry employment from the Bureau of Labor Statistics.

3.1 Wages

Our primary data source for hospital wages is the Center for Medicare & Medicaid Services' Healthcare Cost Report Information System (HCRIS) from 1996 to 2014. All Medicare-certified institutional providers, including effectively every hospital in the US, are required to submit data to HCRIS. We use these data to construct wage measures for all general acute care hospitals that are never designated as critical access hospitals, as wage data are not available for critical access hospitals.[7] HCRIS reports wage data for several dozen distinct line items corresponding to different types of workers. We aggregate workers into categories based on the occupation description and observed wage levels, grouping together worker line items on the basis of education levels, specificity of skills to the hospital industry, and similarity of hourly wages. This aggregation results in three categories of workers: unskilled workers; skilled non-medical workers; and nursing and pharmacy workers. The full list of occupations included in each category is presented in Appendix A, along with further quantitative support from the Current Population Survey (CPS) that the categories meaningfully differ by education levels, specificity of skills to the hospital industry, and worker mobility.[8]

The unskilled worker category consists primarily of blue-collar workers such as cafeteria and laundry workers. Based on CPS data, we estimate that less than ten percent of such workers have a four-year college degree. This category of workers likely has the least industry-specific skillset of the three categories, and consequently the broadest set of potential employers. The skilled worker category consists of administrative employees, social services workers, and other primarily white-collar workers. We estimate that about a third of these workers have a four-year college degree. The nursing and pharmacy worker category consists of nursing administration employees and pharmacy employees.[9] We estimate that more than forty percent of these workers have a four-year college degree. The nursing and pharmacy category of workers likely has the most industry-specific skillset of the three categories, and hospitals therefore constitute the greatest share of their set of potential employers.

[7] HCRIS does not report the value of fringe benefits, so our wage measures include only wages and salaries.

[8] Appendix A also presents the results of our main difference-in-differences regressions using an alternative definition of worker categories. The results are extremely similar.

[9] Unfortunately, our data do not contain comprehensive wage data for certain workers directly involved in health care delivery, such as physicians, many of whom are not directly employed by the hospital(s) at which they have admitting privileges.

Over the period of our data, the skilled worker category and the nursing and pharmacy category exhibit somewhat faster wage growth than the unskilled category. The median nominal wage for unskilled workers grew from \$10.18 per hour in 1996 to \$17.24 in 2014 (3.0 percent annual growth). The median wage for skilled workers grew from \$15.29 to \$31.61 (4.1 percent annual growth). The median wage for nursing and pharmacy workers grew from \$20.39 to \$39.01 (3.7 percent annual growth). Our goal in the empirical analysis is to estimate the extent to which mergers slowed this wage growth, if at all.

3.2 Employer Concentration

Ownership Data. The first step in measuring employer concentration is compiling a historical record of hospital ownership. The starting point is the American Hospital Association's (AHA) Annual Survey of Hospitals, which reports the identity of the system to which a hospital belongs, if any. We supplement the AHA data, whose updates to the ownership variables are sometimes delayed or miscoded, with M&A transaction-level data from Irving Levin's Hospital Acquisition Report. Finally, we use internet searches of archived news stories and hospital websites to verify the accuracy of the constructed ownership panel. The ownership panel covers the years 1998 to 2012.

Employment Data. Measuring employer concentration requires a measure of employer size. Our primary measure of hospital size is the hospital's number of full-time equivalent employees (FTEs). HCRIS reports employment as total employee-hours worked, which we convert into FTEs by assuming a 40-hour work week. Since the cost reports occasionally vary in the length of time covered by the report, we also adjust the calculation to ensure that differences in reporting periods are not implicitly interpreted as differences in employment.[10] Just as we define wages by worker category, an alternative approach is to define FTEs by worker category. We have also examined wage category-specific FTE measures, but the measures are highly correlated and do not yield meaningfully different insights.

[10] Specifically, hospital i's FTEs in year t are given by:

$$\text{FTEs}_{it} = \frac{365}{\text{CostReportDays}_{it}} \times \frac{\text{TotalHours}_{it}}{52 \times 40}$$

where $\text{CostReportDays}_{it}$ is the number of days covered by the cost report and TotalHours_{it} is the total number of hours worked, aggregating over all workers.

Labor Market Definition. We define the geographic component of the labor market at the level of a commuting zone. Commuting zones are geographically contiguous groups of counties between which residents commute to work, constructed based on Census commuting flow data. In the case of urban areas, the commuting zone typically encompasses the county containing the large metropolitan area as well as surrounding counties that share the same labor pool. There are 709 commuting zones in the latest definition based on the 2000 Census. Of these, 571 commuting zones have a general acute care hospital and are therefore in our sample. Azar et al. (2018) and Rinz (2018) also define labor markets using commuting zones, whereas Benmelech et al. (2018) use individual counties and Qiu and Sojourner (2019) use core-based statistical areas. We use commuting zones to avoid overstating a local employer's labor market power in counties that have few employers but neighbor other counties with additional employers competing for labor. If the commuting zone understates the true breadth of the labor market, we will be less likely to detect an effect of mergers on wages. For antitrust authorities, determining the appropriate market definition in a merger case is an extremely fact-intensive process, often involving subpoenaed information (Gaynor and Pflum 2017). In the absence of another widely accepted definition of local labor markets, executing that process for the mergers in our data is not feasible, and hence we rely on the transparent but coarse definition of the commuting zone.

Measuring Concentration. We measure employer concentration using the Herfindahl-Hirschman index (HHI) of FTEs within a commuting zone-year pair. Worker category-specific HHIs are highly correlated with this measure, with correlation coefficients exceeding 0.97. All of our empirical results are similar when using worker-category specific HHIs, so we focus on the aggregate measure for parsimony. We use HHI because of its predominance in antitrust policy: for example, thresholds for merger scrutiny outlined in the DOJ/FTC *Horizontal Merger Guidelines* are based on HHI.[11] In 1998, the median hospital is located in a commuting zone with an HHI of 2,134, growing to 2,665 by 2012. Additional summary statistics are provided in Appendix A.

Importantly, this HHI measure captures concentration only among hospital employers. We use this as our primary measure of concentration because of the richness of our data for this set of employers, but note that this measure almost assuredly overstates the degree of effective employer concentration in the relevant labor market. Unskilled workers may be able to substitute to non-

[11] Naidu et al. (2018) argue that the same thresholds should be applied to labor markets.

hospital employment in health care or to employment in other industries. The same may apply to skilled workers, albeit to a lesser extent. Nursing and pharmacy workers, who may be more constrained to health care jobs, may still be able to substitute to employment in non-hospital settings within the health care industry.

To better understand how hospital employment compares to overall health care employment, we compile data for a broader set of employers from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW).[12] The QCEW reports establishment and employment counts at the county-industry level, which we then aggregate to the commuting zone. To calculate health care industry-wide employment, we subset to employment with NAICS codes beginning with 621 (ambulatory health care services, including but not limited to physician offices), 622 (hospitals), and 623 (nursing and residential care facilities). Because employment counts in the QCEW data are not broken out by employer, we cannot calculate an alternative HHI measure for the health care industry as a whole. However, since computing the change in HHI induced by a merger only requires the shares of the merging hospitals,[13] we can use the QCEW to measure how the mergers in our sample affect overall health care employer concentration.

4 Empirical Analysis

This section presents the main results of the paper. First, we document the association between hospital concentration and wages in the raw data. These aggregate analyses mirror recent papers that find a negative relationship between employer concentration and wages (Azar et al. 2017; Benmelech et al. 2018; Rinz 2018; Qiu and Sojourner 2019). Second, we estimate difference-in-differences models that examine the labor market effects of consolidation using only variation in concentration that is generated by merger activity. This approach has the dual benefit of (i) relying on clear, well-defined shocks to concentration, and (ii) examining directly the policy lever that has been advanced as a leading potential labor market remedy. Third, we assess the extent to which strong labor unions attenuate any downward pressure on wages arising from mergers. Fourth, as a placebo test, we examine whether out-of-market mergers that do not affect local labor market

[12] A key advantage of the QCEW over the Census Bureau's County Business Patterns data, which has been used in the literature to measure labor market concentration, is that the QCEW includes government employers. Approximately 20 percent of US hospitals are government-owned (KFF 2018), so accurate measurement of health care labor market concentration requires the inclusion of government employers.

[13] The change in HHI resulting from the merger of firms A and B is twice the product of A's share and B's share.

concentration impact wage trajectories. Discussion of several additional results is interspersed throughout the text.

4.1 Aggregate Analysis

In this section, we confirm that the hospital industry exhibits the same negative association between employer concentration and wages that has been documented in the recent literature. We regress wages on employer HHI for each of the three categories of workers defined in Section 3.1:

$$\ln\left(wage_{imt}\right) = \delta_m + \tau_t + \alpha HHI_{m,t-1} + X_{imt}\beta + \epsilon_{imt} \tag{1}$$

where $wage_{imt}$ is the log of wages for a given worker category in hospital i in year t and $HHI_{m,t-1}$ is our measure of hospital employer concentration, lagged by one year.[14] The regressions include several important controls. Year fixed effects (τ_t) are included to flexibly control for national time trends in wages. We also include commuting zone fixed effects (δ_m). Labor markets with one or a few dominant employers, such as factory towns, are disproportionately rural and therefore have low costs of living. The commuting zone fixed effects condition out the negative correlation between concentration and wages that is explained by urban-rural differences, as well as other persistent unobservable characteristics of commuting zones. The estimated relationship between employer HHI and wages is therefore measured from within-commuting zone variation in employer concentration. X_{imt} contains a variety of additional market-level and hospital-level variables. To control for within-commuting zone changes in the cost of living, we include the log market rent for a one-bedroom apartment, measured from the Department of Housing and Urban Development's Fair Market Rent data (HUD 2018). To roughly control for within-commuting zone changes in health care demand, we include the log of the commuting zone's population. To control for individual hospital characteristics that may affect wages, we include hospital size (measured by log bed count), the fractions of the hospital's inpatient discharges that come from Medicare and Medicaid, the complexity of the hospital's patient population (measured by log case mix index), and the hospital's inpatient vs. outpatient mix (measured by the fraction of hospital charges owing to outpatients).

Columns 1, 3, and 5 of Table 1 report the results of these regressions, which cover the period of our ownership data (1998 to 2012). The point estimates are negative for all three worker categories,

[14] The results are similar using either further lags or contemporaneous HHI.

Table 1: HHI and Wages, 1998-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Unskilled		Skilled		Nursing & Pharmacy	
	OLS	IV	OLS	IV	OLS	IV
HHI_{t-1}	-0.049	-0.032	-0.168***	-0.198***	-0.058	-0.128**
	(0.035)	(0.049)	(0.046)	(0.071)	(0.039)	(0.058)
Observations	41,893	41,893	42,555	42,555	42,502	42,502
R-squared	0.783	0.654	0.699	0.611	0.745	0.681
Estimated wage difference between HHI = x and HHI = 0:						
HHI = 1,500	-0.7%	-0.5%	-2.5%	-2.9%	-0.9%	-1.9%
HHI = 2,500	-1.2%	-0.8%	-4.1%	-4.8%	-1.4%	-3.1%
HHI = 5,000	-2.4%	-1.6%	-8.1%	-9.4%	-2.8%	-6.2%
HHI = 10,000	-4.7%	-3.1%	-15.5%	-17.9%	-5.6%	-12.0%

Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include commuting zone and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges. For readability, the coefficient estimates are scaled so that they reflect the effect of HHI when HHI is measured on a scale between zero and one. The instrument is merger-induced concentration changes.

although the negative relationship is statistically significant only for the skilled worker category. Taking the point estimates at face value, wages in a market with an HHI of 5,000 are 2.4 percent lower for unskilled workers than in an otherwise observably similar perfectly competitive market, 8.1 percent lower for skilled workers, and 2.8 percent lower for nursing and pharmacy workers. Omitting the additional controls can meaningfully affect the estimates. For instance, if we omit all of the additional controls—retaining only the commuting zone and year fixed effects—the point estimates for unskilled, skilled, and nursing and pharmacy wages are -0.059, -0.180, and -0.079, respectively, with all of the estimates statistically significant at the 10 percent level or better. All of these estimates are qualitatively similar to the negative employer concentration-wage relationship documented in the literature.

However, even given a rich set of controls, some of the measured relationship between concentration and wages in these aggregate regressions may be attributable to omitted variables affecting wages that are also correlated with concentration. For example, a negative economic shock may raise the probability of employer exit, thereby increasing HHI among the remaining employers, while simultaneously driving down wages. For antitrust policy, the most directly relevant metric is instead the relationship between wages and the portion of concentration that is attributable to mergers. We therefore isolate the portion of concentration that is attributable directly to employer consolidation through mergers by instrumenting for HHI using merger-induced changes in HHI. The instrument varies by commuting zone and year and measures the cumulative merger-induced change in HHI in the commuting zone from the start of the sample period.[15] Merger-induced changes in HHI are highly predictive of total changes in HHI: the first-stage coefficient on merger-induced HHI is 1.01, with a first-stage F-statistic of 854.

Columns 2, 4, and 6 of Table 1 report the results of the IV regressions. The estimates foreshadow our findings from the difference-in-differences analysis in Section 4. For the unskilled worker category, the relationship between instrumented employer concentration and wages remains negative and insignificant, and the magnitude of the point estimate is smaller than the OLS estimate. For the skilled worker category, the estimate remains negative and highly statistically significant. For the nursing and pharmacy category, the magnitude of the point estimate more than doubles and becomes statistically significant at the 5 percent level. These differences between the OLS and IV estimates highlight the fact that aggregate regressions of wages on concentration are identified from a variety of factors besides employer consolidation. The raw variation in concentration may be a result of organic employer growth, local economic shocks, firm exit, or other factors that may also affect wages. Since antitrust authorities seeking to address the purported link between concentration and wages can act primarily on mergers, such regressions cannot serve as a complete basis for antitrust policy. We therefore turn next to a retrospective evaluation of mergers' effects on wages.

4.2 Difference-in-Differences Analysis

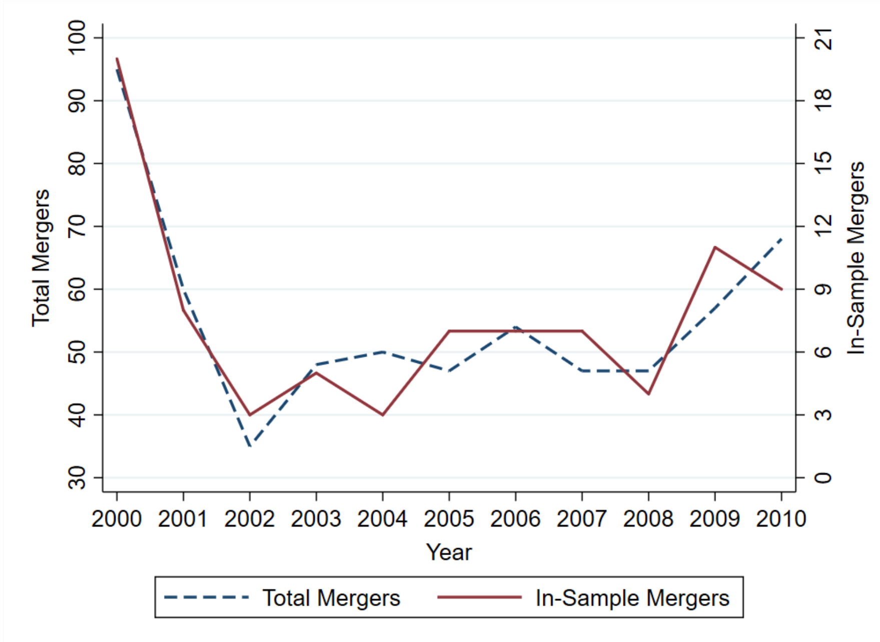
In this section, we examine the labor market effects of consolidation using only variation in concentration that is induced by merger activity. We use difference-in-differences models to estimate wage trajectories following well-defined merger events. This event study approach allows us to examine the relationship between employer consolidation and wages found in Table 1 in greater detail, including checking for differential pre-trends in wages prior to observed merger events.

Merger Sample and Control Group

We focus on commuting zones that experienced a single instance of a merger-induced increase in concentration between 2000 and 2010. We restrict the sample to the years 2000 and 2010 so that we

[15]The commuting zone fixed effects absorb the differences between commuting zones in initial HHI at the start of the sample period.

Figure 1: Merger Counts, 2000–2010



Number of hospital mergers per year. Mergers may be excluded from the sample if they involve employers in non-overlapping labor markets, or if they occur in commuting zones that experience more than one concentration-increasing merger over the course of the sample period.

have at least four years of pre- and post-merger wage data for all mergers in the sample. There are 84 such cases. Because of the prevalence of consolidation in the industry, many commuting zones experience concentration-increasing mergers in multiple years. In these cases, it is less clear how to define the pre- and post-periods for the difference-in-differences analysis, and there are greater concerns about unobservables driving widespread merger activity also affecting wage trends.[16] Figure 1 plots the count of mergers between 2000 and 2010, both overall and for the 84 mergers we examine in the difference-in-differences analysis. The distribution of in-sample mergers over years is very similar to the overall distribution. The modal year is 2000. Merger activity then slumps in the mid-2000s before accelerating again at the end of the decade.

Table 2 provides summary statistics for the 84 commuting zones experiencing a single concentration-increasing merger. The median treated commuting zone has eight hospitals and total hospital employment of 7,000 full-time equivalents. The median merger in this group induces a hospital employer concentration increase of 401 HHI points. In addition to examining these mergers as a group, we also estimate specifications that test for heterogeneity by the magnitude of the concen-

[16]For estimates using all merger-induced concentration increases in the data, see the IV results in Table 1.

Table 2: Characteristics of Merger-Treated Commuting Zones

	Average	25th percentile	50th percentile	75th percentile
Hospitals	8.2	4.0	8.0	10.5
Hospital beds	1,656	542	1,354	2,231
Inpatient discharges	79,151	22,953	56,386	105,996
Hospital FTEs	9,637	2,983	7,043	13,272
Pre-merger HHI (hospitals)	3,135	2,038	2,750	4,051
ΔHHI (hospitals)	920	114	401	1,115
ΔHHI (health care)	246	14	59	199

	Hospital FTEs	ΔHHI (hospitals)	Health care employment	ΔHHI (health care)
1st quartile ΔHHI	18,489	63	59,876	11
2nd quartile ΔHHI	10,940	235	29,636	56
3rd quartile ΔHHI	6,919	618	21,482	125
4th quartile ΔHHI	2,200	2,764	5,502	790

Notes: Values are in the year of the merger. "Hospitals" refers to non-specialty, non-critical access general acute care hospitals. ΔHHI (hospitals) is defined using hospital FTEs to calculate shares, whereas ΔHHI (health care) is defined using total health care employment to calculate shares.

tration increase induced by the merger. The bottom panel of Table 2 provides further detail on the available variation in the change in concentration. For the bottom quartile of mergers, the change in concentration is small: on average, a 63 point increase in HHI for hospital employment and a mere 11 point increase for overall health care employment. While the second and third quartiles of mergers involve more meaningful changes in concentration when considering only hospital employment (average increases of 200 points or more), the effect of these mergers on concentration remains modest when considering overall health care employment (average increases around 100 points or less). Only in the top quartile do the mergers involve substantial increases in concentration both for a hospital-only labor market definition and an overall health care labor market definition.

Since an increase in labor market power may affect wage-setting at all firms within the market, the treatment group includes both those hospitals that are directly involved in a merger event and also the other hospitals in that commuting zone. There are 569 hospitals in the 84 treated commuting zones. Of these, 30 percent of treated hospitals are directly involved in the merger events under examination, while the other 70 percent are bystanders to those mergers (i.e., they compete in the same market). In the results below, we also discuss the estimates from specifications that separately estimate wage effects by the hospital's involvement in the merger (footnote 22).

We define the control group as hospitals in commuting zones without any merger activity between 2000 and 2010. There are 293 such commuting zones, containing a total of 819 hospitals. While baseline wages for the three worker categories are fairly well balanced across treatment and control hospitals, there are other observable differences (see Table B.1). The most immediate difference is that control commuting zones tend to be smaller than treated commuting zones, although this difference is much smaller for mergers in the top quartile of ΔHHI. Similarly, control hospitals tend to be smaller than treated hospitals, in both bed count and patient volume. While these differences are not particularly surprising—mergers are less prevalent in smaller areas—they do potentially raise concerns that wage trends at hospitals in control commuting zones do not represent reasonable counterfactual wage trends for hospitals in treated commuting zones, especially in the bottom three quartiles. We address these concerns in two main ways. First, we estimate specifications with leads and lags that examine whether there are any departures in wage trends between treatment and control hospitals prior to the mergers under examination. Second, we estimate specifications that expand the control group by imposing less stringent requirements on merger activity in control commuting zones.[17] Further discussion of the alternative control groups and the corresponding regression results, which do not meaningfully impact the interpretation of the estimates presented here, is contained in Appendix B.

Regression Specification and Results

To measure the effect of mergers on wages, we estimate:

$$\ln\left(wage_{imt}\right) = \delta_i + \tau_t + \alpha post_{mt} + X_{imt}\beta + \varepsilon_{imt} \tag{2}$$

We estimate the model separately for each of the three worker categories. The variable of interest is $post_{mt}$, which is an indicator for whether and when commuting zone m is treated: i.e., experienced a within-market hospital merger in year $t' \leq t$. As is standard, the model includes hospital fixed effects (δ_i) and year fixed effects (τ_t), and thus the effect of mergers is identified by within-hospital changes in wages following a merger event, flexibly controlling for nationwide wage trends. X_{imt}

[17]Specifically, we include hospitals in commuting zones with merger activity that did not affect labor market concentration: that is, out-of-market mergers. We do not include these markets in our main control group because out-of-market mergers may affect wages through mechanisms other th a labor market power (we examine these effects directly in Section 4.4). Expanding the control group to include commuting zones with out-of-market mergers nearly doubles the size of the control group. We also use the expanded control group to construct a matched control group based on hospital and market characteristics.

contains the same market-level and hospital-level variables as the aggregate regressions in Section 4.1. For hospitals in the treatment group, we subset the data to the four years preceding and the four years following the merger event in order to focus on wage trends immediately surrounding the merger. The year of the merger is excluded from the regressions, since mergers generally happen during a calendar year and the year of acquisition belongs partially in the pre-period and partially in the post-period. We cluster the standard errors at the hospital level and weight observations by the hospital's inpatient discharge volume.[18]

The top panel of Table 3 (columns 1 to 3) presents the estimates of equation (2). Each column reports the difference-in-differences estimate for the corresponding worker category. When pooling all mergers together, we fail to reject the null hypothesis that consolidation has zero effect on wages. The estimates are statistically insignificant and the magnitudes of the point estimates are small, indicating merger effects of less than one percent. Despite the overall negative relationship between concentration and wages seen in Section 4.1, the null results in the top panel of Table 3 are arguably unsurprising. The median labor market experiencing a hospital merger sees its health care employer concentration rise by only 59 HHI points. This HHI increase is analogous to a merger of two employers in a market that initially has eighteen employers with equal labor market shares.

The bottom panel of Table 3 (columns 4 to 6) reports the results from specifications that estimate separate merger effects by the increase in concentration induced by the merger. As described earlier (Table 2), only mergers in the top quartile represent substantial changes in overall health care employer concentration according to standard benchmarks. For the bottom three quartiles of mergers, the difference-in-differences estimate is statistically insignificant and generally small in magnitude for all three worker categories. That is, we cannot reject that wage growth rates remain the same following mergers in the bottom three quartiles. We find statistically significant wage effects only for mergers in the top quartile of ΔHHI.[19] For the skilled worker category, we estimate that nominal wages are 4.1 percent lower over the four years following the merger than they would have been absent the merger. For the nursing and pharmacy worker category, we estimate that

[18]Weighting by the hospital's total employment or worker category-specific employment yields largely similar estimates, with slightly larger magnitudes for the skilled worker category and slightly smaller magnitudes (of similar significance) for the nursing and pharmacy worker category.

[19]This pattern does not appear to be explained purely by differences in *levels* of pre-merger or post-merger concentration. In regressions separating the difference-in-differences estimates by HHI levels rather than changes, we continue to estimate a negative effect on skilled and nursing and pharmacy wages for the highest quartiles, but the point estimates are smaller and not statistically significant. These results underscore the importance of the change in concentration induced by a merger, as opposed to the level of concentration alone.

Table 3: Mergers and Wages: Difference-in-Differences Estimates

	(1)	(2)	(3)
	Unskilled	Skilled	Nursing & Pharmacy
Post	0.004	-0.007	-0.007
	(0.004)	(0.008)	(0.006)
Observations	17,458	17,453	17,328
R-squared	0.912	0.852	0.874
	(4)	(5)	(6)
	Unskilled	Skilled	Nursing & Pharmacy
Post × 1st quartile ΔHHI	0.004	0.004	0.002
	(0.006)	(0.010)	(0.009)
Post × 2nd quartile ΔHHI	0.003	-0.024	-0.005
	(0.009)	(0.016)	(0.010)
Post × 3rd quartile ΔHHI	0.006	0.002	-0.019
	(0.008)	(0.021)	(0.014)
Post × 4th quartile ΔHHI	0.007	-0.042**	-0.065***
	(0.014)	(0.019)	(0.023)
Observations	17,458	17,453	17,328
R-squared	0.912	0.853	0.875
H_0: no heterogeneity	0.991	0.104	0.035**

Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges. The bottom row reports the p-value of a test of the null hypothesis that the post×ΔHHI quartile effects are equal to one another.

nominal wages are 6.3 percent lower.[20] In terms of wage growth, these estimates imply that post-merger annual wage growth (measured over the four years following the merger) is 1.1 percentage points slower for skilled workers and 1.7 percentage points slower for nursing and pharmacy workers than would be expected absent the merger. Average annual nominal wage growth, as indicated by the year fixed effects estimates and the summary statistics in Section 3.1, ranges from 3 to 4 percent. The estimates for mergers in the top quartile of ΔHHI therefore represent substantial slowdowns in wage growth.[21] We find similar results using an alternative definition of worker categories (Table

[20]Exponentiating the coefficients for interpretation, $\exp(-0.042) - 1 = -0.041$ and $\exp(-0.065) - 1 = -0.063$.

[21]To the extent there is any misreporting of wages in HCRIS, it is unclear why hospitals in markets affected by mergers would have a differential incentive to under-report wage costs to CMS.

A.1).[22]

On balance, these results suggest that for employer consolidation to put downward pressure on wages, a substantial increase in concentration is required. The results also highlight the importance of an appropriate labor market definition. We find significant effects only for the skilled worker category and the nursing and pharmacy worker category, both of which require relatively industry-specific human capital. The unskilled category consists of workers with less industry-specific human capital, such as cafeteria workers. It is therefore likely that the relevant employer concentration for this category does not rise by nearly as much as our hospital and health care HHI measures would suggest. Of course, we cannot rule out that meaningful employer consolidation on a broader scale would have negative wage effects for these workers.

Wage Trends Prior to Mergers

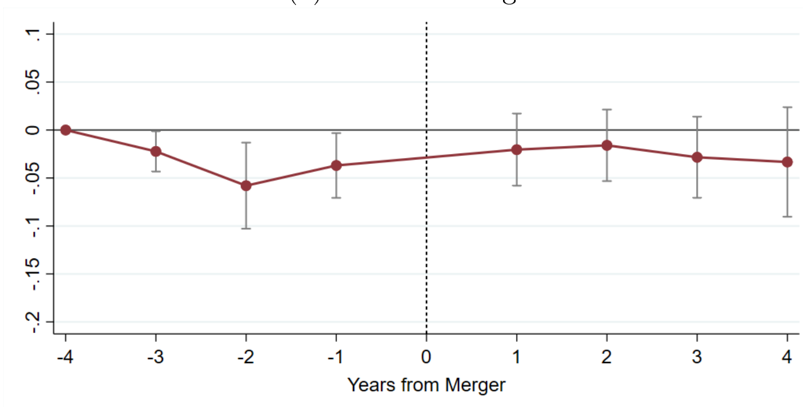
The above difference-in-differences estimates will yield a biased estimate of the causal effect of mergers if the error term in equation (2) is correlated with mergers. This would be the case if, for example, acquirers strategically seek out target hospitals that are projected to have lower labor cost growth in the future. While we cannot rule out such anticipatory acquisitions, we can check for differential wage trends between treatment and control hospitals in the years leading up to a merger. For mergers in the top quartile of ΔHHI, Figure 2 plots the coefficients from regressions that replace the single $post_{mt}$ indicator in equation (2) with lead indicators for the four years leading up to a merger and lag indicators for the four years following a merger.[23] We do not detect differential pre-trends for the skilled category or the nursing and pharmacy category, though the leads and lags are less precisely estimated for the skilled category. For the unskilled category, wages grow slower among treatment hospitals than control hospitals leading up to the merger, but there is no evidence of a difference post-merger. The slowdown in nursing and pharmacy wages following a

[22]We have also explored variation in the estimated wage effects following mergers in the top quartile of ΔHHI as a function of the hospital's involvement in the merger. To do so, we estimate separate merger effects for (i) the hospital(s) belonging to the acquiring system, (ii) the hospital(s) acquired in the transaction, and (iii) the non-merging hospitals in the same market. For the nursing and pharmacy worker category, wage slowdowns are very similar for both merging and non-merging hospitals in the same market: the estimates (estimate (standard error)) are all negative and similar in magnitude for acquirers (-0.068 (0.039)), targets (-0.072 (0.049)), and their non-merging rivals (-0.054 (0.018)). For the skilled worker category, on the other hand, the estimated effect appears to be driven by the merging hospitals: the estimates are negative both for acquirers (-0.066 (0.026)) and targets (-0.052 (0.030)), but statistically indistinguishable from zero for their non-merging rivals (0.020 (0.027)).

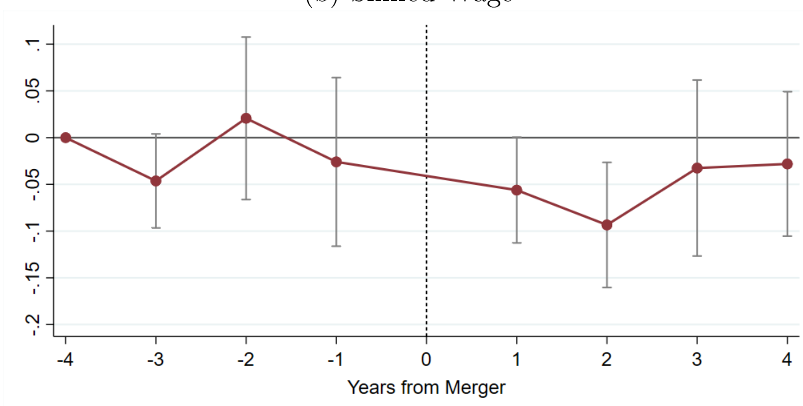
[23]In analogous regressions using all four quartiles of ΔHHI, we cannot reject the null hypothesis of common wage trends pre-merger, and all of the lag indicators' 95 percent confidence intervals include zero.

Figure 2: Leads & Lags Estimates: Top Quartile of ΔHHI Mergers

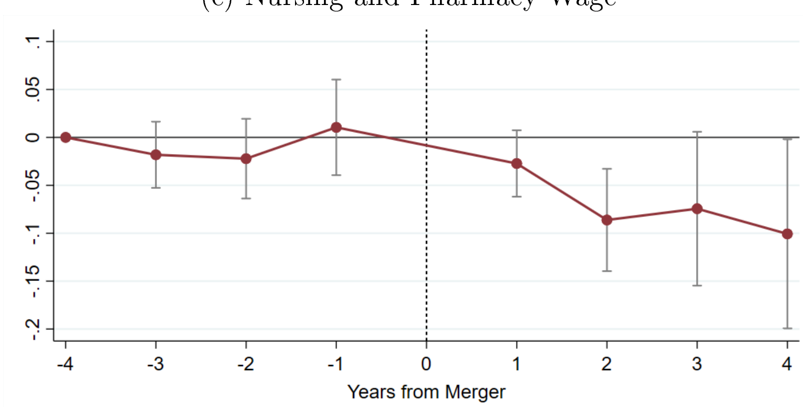
(a) Unskilled Wage



(b) Skilled Wage



(c) Nursing and Pharmacy Wage



The figure plots coefficients for lead and lag indicators up to four years prior to or following a merger, from a regression where these indicators replace the single Post variable in the bottom panel Table 3. Four years before the merger is the omitted category. Vertical bars represent 95 percent confidence intervals. We cannot reject the null hypothesis of no difference in pre-trends in wages between the treatment and control groups for the skilled category and the nursing and pharmacy category. For the unskilled category, wages grow slower among treatment hospitals than among control hospitals leading up to the mergers, but there is no evidence of a difference following the mergers.

merger is persistent, continuing at least four years after the merger event. Skilled wages, on the other hand, grow slower than in control markets in the two years immediately following the merger, but subsequently appear to recover.

Local Economic Conditions

Even absent differential pre-trends in wages, the wage effects documented in Table 3 could in principle be explained by overall local economic conditions rather than by mergers. This would be the case if mergers occur differentially in markets that are about to experience an economic slowdown. To check for this possibility, Panel A of Table 4 reports difference-in-differences estimates for three coarse measures of the strength of the local economy: the unemployment rate, total population, and the market rent for a one-bedroom apartment.[24] We estimate these regressions at the level of the commuting zone and focus on mergers in the top quartile of ΔHHI. We do not find evidence of differentially worsening economic conditions in the treatment markets (nor do we detect differential pre-trends in models with leads and lags). The difference-in-differences estimates for unemployment and population are small and not statistically significant. Only one-bedroom apartment rent appears to evolve differentially in treatment markets, but the positive estimate suggests that the treatment markets' rental housing markets are differentially heating up rather than slowing down. In sum, we do not find evidence that the wage effects can be explained by overall local economic conditions.

Labor Quantity

A pattern of slowing wage growth following employer consolidation is consistent with at least two broad classes of explanations for employer market power: classical monopsony and labor market search frictions. A monopsonist employer can drive wages below the marginal product of labor by restricting employment below its competitive equilibrium level (Robinson 1969). Under classical monopsony, reductions in wages are therefore associated with corresponding reductions in labor quantity. By contrast, in a labor market with search frictions, lower wages need not arise via

[24]Unemployment and population data are drawn from the Bureau of Labor Statistics' Local Area Unemployment Statistics (BLS 2018b). Rent data are drawn from the Department of Housing and Urban Development's Fair Market Rent data (HUD 2018).

Table 4: Top Quartile of ΔHHI Mergers: Non-Wage Outcomes

Panel A: Commuting Zone Economic Outcomes

	(1)	(2)	(3)
	Unemployment	(log) Population	(log) One-Bedroom Rent
Post × 4th quartile ΔHHI	0.0002	-0.003	0.034**
	(0.003)	(0.013)	(0.013)
Observations	5,626	5,626	5,626
R-squared	0.838	0.998	0.962

Panel B: Labor Quantity (log FTEs)

	(4)	(5)	(6)
	Unskilled	Skilled	Nursing & Pharmacy
Post × 4th quartile ΔHHI	0.057	-0.044	0.204**
	(0.050)	(0.074)	(0.080)
Observations	14,007	13,986	13,817
R-squared	0.956	0.893	0.914

Panel C: Labor Composition (Nursing)

	(7)	(8)	(9)
	(log) RN FTEs	(log) LPN FTEs	LPN Share
Post × 4th quartile ΔHHI	0.072	0.0002	-0.006
	(0.064)	(0.127)	(0.007)
Observations	13,595	13,396	13,596
R-squared	0.972	0.795	0.843

Notes: ***p<0.01, **p<0.05, *p<0.10. The specifications in Panel A are estimated at the commuting zone-year level and include commuting zone and year fixed effects. Standard errors are clustered by commuting zone. The specifications in Panels B and C are estimated at the hospital-year level and include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges.

employment suppression.[25] If workers' expected arrival rate of new job offers falls, as it may following a decline in the number of employers, then workers facing search frictions will optimally accept lower wages (Burdett and Mortensen 1998). With this outward shift of the labor supply curve, the movement along the labor demand curve may even increase equilibrium labor quantity.

[25]Mortensen and Pissarides (1999) provide an excellent overview of the pioneering theoretical work. Eckstein and van den Berg (2007) review the corresponding empirical literature.

To check the consistency of our main results with these two classes of mechanisms, we test for changes in labor quantity following mergers in the top quartile of ΔHHI. Panel B of Table 4 reports estimates from difference-in-differences regressions with hospital-level employment of each of the three worker categories as outcome variables. For the unskilled and skilled categories, we cannot reject the null hypothesis of zero employment effects. For the nursing and pharmacy category, we estimate that employment actually increases faster in treatment markets than in control markets. This result is consistent only with the second class of explanations for employer market power: labor market search frictions. However, unlike the other outcomes we have examined, there is evidence of differential pre-trends in nursing and pharmacy worker employment. Employment grows faster preceding mergers in the top quartile of ΔHHI than in control markets. The existence of these pre-trends makes us reluctant to interpret the estimated faster employment growth too strongly. After controlling for the differential pre-trends with a linear time trend, the estimated employment effect becomes small and statistically insignificant.[26] In sum, we do not find evidence of reductions in employment growth following mergers, which would be expected to arise if classical monopsony were the dominant mechanism.[27] We therefore view these results as suggestive of a labor market search frictions mechanism.

Labor Composition

An alternative explanation for the reduced wage growth is that there is a post-merger shift in the composition of the workforce toward lower-skilled, lower-wage workers within a category. In that case, the observed wage effects may simply reflect a change in the composition of the workforce rather than any effects of labor market power per se. In the absence of worker-level data, we cannot directly test whether the observed wage slowdowns are driven by within-worker wage changes. Instead, we check for shifts in labor composition for a subset of workers where we can observe finer subcategories: nurses. Unlike the HCRIS wage data, the AHA data report separate employment figures for two subclasses of nurses: registered nurses (RNs) and licensed practical nurses (LPNs). RNs require more years of training, have more stringent licensing requirements, and earn an average

[26]The results of the main wage regressions are robust to including the same linear time trend, consistent with the fact that we do not detect differential pre-trends for wages.

[27]We also fail to detect output quantity reductions following mergers in the top quartile of ΔHHI, using either inpatient discharges or adjusted discharges (which adjusts for outpatient services) as the measure of output quantity. The difference-in-differences estimate is 0.037 (with a standard error of 0.034) for (log) inpatient discharges and 0.036 (with a standard error of 0.035) for (log) adjusted discharges.

salary of approximately 1.5 times that of LPNs (BLS 2018b). Difference-in-differences models with RN FTEs, LPN FTEs, and LPNs' share of nurse FTEs as outcome variables do not indicate a shift toward lower-compensated nurses (Panel C of Table 4). While this test does not support a shift in labor composition as the cause of the observed wage effects, our ability to firmly reject that possibility is hindered by our lack of worker-level data.

4.3 Labor Unions

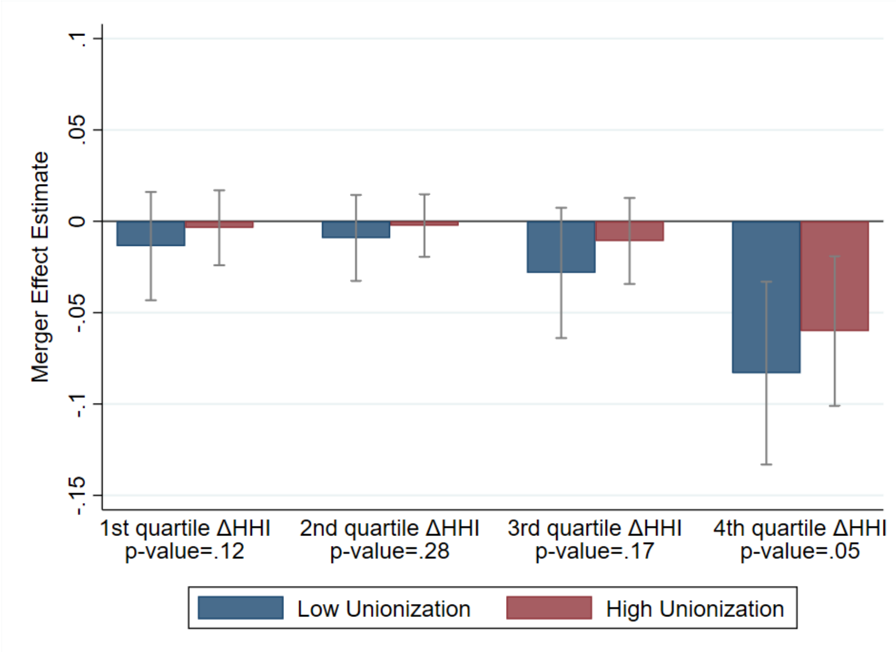
If the wage slowdowns documented in Section 4.2 can indeed be traced to post-merger increases in employer labor market power, then strong worker power may act as a countervailing force. This section therefore tests for mitigating effects of strong labor unions. We focus on the nursing and pharmacy worker category, as this is the only category for which we can construct a measure of the employee unionization rate. In addition to the unionization rate, we also examine state-level right-to-work laws. Right-to-work laws prohibit unions from collecting dues from workers whom they represent, but who are not members of the union. Unions in right-to-work states are therefore thought to have less power in wage negotiations with employers.

We calculate state-by-year nurse unionization rates from the CPS, using respondents whose primary occupation is nursing.[28] Of course, unionization rates may be endogenously determined partially as a function of labor market power. For our purposes, however, it suffices to check whether union power at the time of the merger impacts the subsequent wage trajectory. We incorporate nurse unionization rates into the regression specifications by interacting the post-by-ΔHHI quartile variables with the nurse unionization rate (and including the unionization rate as a standalone variable). The results are depicted in the top panel of Figure 3. In the figure, we evaluate the estimated effect of mergers on wages both at a low level of nurse unionization (the 25th percentile: 4.1 percent) and a high level of nurse unionization (the 75th percentile: 15.2 percent). Focusing on mergers in the top quartile of ΔHHI, where the effect of unionization on the post-merger wage effect is statistically significant at the 5 percent level, we estimate that moving from the 25th percentile of unionization to the 75th percentile eliminates about 30 percent of the post-merger wage effect. The distribution of nurse unionization rates is fairly skewed. A larger movement from the 5th percentile

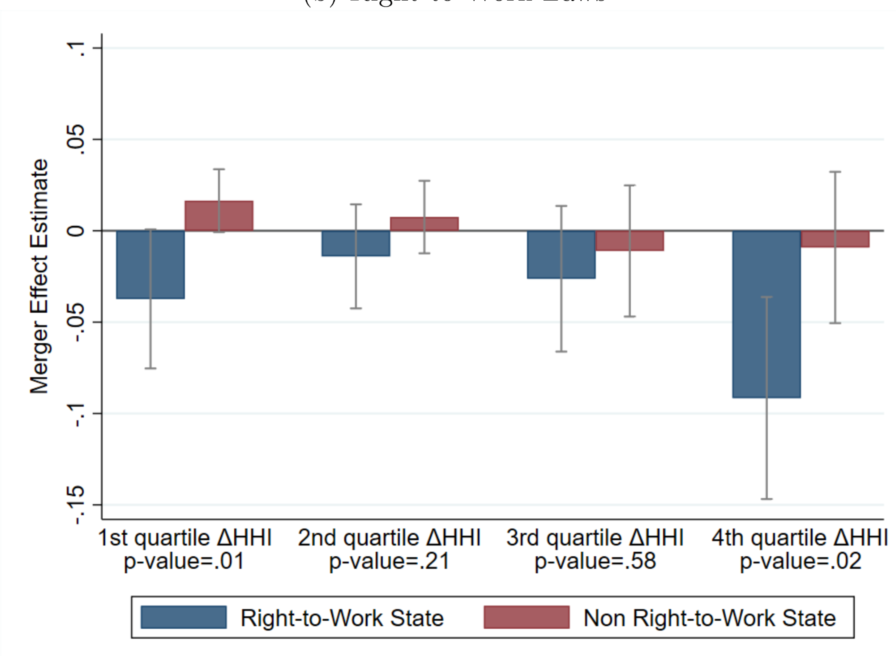
[28]While ideally we would be able to measure unionization at the hospital level, we are not aware of any comprehensive data source containing that information. Moreover, unionization rates are capable of affecting wages not only at unionized employers, but also at competing employers through the union "threat effect:" the threat that employees will unionize or quit if working conditions fall too far below those offered by the unionized employers (Rosen 1969).

Figure 3: Wage Effects and Labor Unions

(a) Nurse Unionization



(b) Right-to-Work Laws



Vertical bars represent 95 percent confidence intervals. The p-value toward the bottom of each figure is the p-value of a test of the null hypothesis that the low unionization/high unionization (right-to-work/non right-to-work) effects are equal to one another.

of unionization (1.5 percent) to the 95th percentile (40.8 percent) is estimated to eliminate about 90 percent of the post-merger wage effect. In short, high levels of unionization appear to meaningfully attenuate the estimated post-merger reductions in wage growth.

The bottom panel of Figure 3 conducts an analogous exercise, now using the presence of state right-to-work laws as the measure of union power (NRWTC 2018). If union power is an effective moderator of employer wage-setting power following a merger, then wage slowdowns will likely be larger in labor markets with right-to-work laws, which weaken unions even conditional on unionization rates. The results are similar to the specification examining nurse unionization rates. For mergers in the top quartile of ΔHHI, the estimated post-merger reductions in wage growth appear only after mergers in right-to-work states. We view these results as bolstering the interpretation of the merger effects in Section 4.2 as consequences of increased labor market power—power that can potentially be mitigated by strong labor unions.

4.4 Placebo Test: Out-of-Market Mergers

Besides labor market power, employer mergers may affect wages through alternative channels, like changes in worker productivity. Such productivity changes may arise from shifts in the managerial practices or production functions of the employers. This section provides a placebo test of the labor market power mechanism. The ideal test would isolate the effects of labor market power by examining mergers where all other merger-related mechanisms potentially affecting wages are shut down. While we do not observe mergers in which we can confidently assume negligible changes to other determinants of wages besides labor market power, we *do* observe mergers that do not appear to affect labor market concentration. In particular, many hospital mergers are "out-of-market": that is, the merging hospitals are located in different commuting zones and thus have non-overlapping labor markets.[29]

For these out-of-market mergers, any observed wage effects presumably operate through channels besides reduced competition for labor. If mechanisms besides labor market power play the dominant role in generating the post-merger wage effects documented above, then meaningful wage effects should also be observed following out-of-market mergers.[30] Examining out-of-market merg-

[29] We cannot directly rule out that the relevant geographic labor market for hospital workers is broader than the commuting zone. However, if there were no migration frictions at all, then we should not see any wage slowdowns following mergers within local labor markets, as we do in our main results.

[30] A similar approach is used by Focarelli and Panetta (2003) to distinguish between efficiency and market power

Table 5: Out-of-Market Mergers and Wages: Difference-in-Differences Estimates

	(1)	(2)	(3)
	Unskilled	Skilled	Nursing & Pharmacy
Post	0.003	-0.010	0.005
	(0.008)	(0.011)	(0.008)
Observations	15,402	15,424	15,304
R-squared	0.906	0.849	0.875
	(4)	(5)	(6)
	Unskilled	Skilled	Nursing & Pharmacy
Post × 1st quartile HHI	0.010	-0.005	-0.004
	(0.011)	(0.014)	(0.010)
Post × 2nd quartile HHI	-0.006	-0.017	-0.002
	(0.010)	(0.017)	(0.012)
Post × 3rd quartile HHI	-0.007	-0.024	0.032
	(0.012)	(0.027)	(0.020)
Post × 4th quartile HHI	0.014	-0.014	0.004
	(0.016)	(0.028)	(0.024)
Observations	15,402	15,424	15,304
R-squared	0.906	0.849	0.875
H_0: no heterogeneity	0.540	0.916	0.466

Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges. The bottom row reports the p-value of a test of the null hypothesis that the post×HHI quartile effects are equal to one another.

ers therefore gives us the opportunity to rule out labor market power as the dominant mechanism, even if it does not give us the opportunity to confirm it. To examine the effect of out-of-market mergers, we estimate difference-in-differences models comparing wage trends in commuting zones with out-of-market mergers to commuting zones without any merger activity (the same control group as the analysis in Section 4.2). Analogous to the main analysis, we restrict the sample of treated commuting zones to those that experienced a single out-of-market merger during the 2000 to 2010 period, leaving 90 commuting zones. The top panel of Table 5 reports estimates from a regression mirroring equation (2), but with the treatment group now defined as hospitals in markets experiencing an out-of-market merger. We do not find evidence of post-merger wage effects: the

effects of bank mergers.

estimates are small and statistically insignificant for all three worker categories. The bottom panel breaks out the effects by the quartile of hospital employer HHI at the time of the merger.[31] While the point estimates occasionally increase in magnitude, they remain small compared to our main results and are statistically insignificant in every case. Even in extremely concentrated markets—the top quartile is almost exclusively monopoly markets—we do not find clear evidence of reduced wage growth post-merger.

If mergers affect equilibrium wages through changes in managerial know-how or other changes to firm production functions that are not directly related to labor market power, then we would expect wage trajectories following out-of-market mergers to diverge from trajectories in markets without mergers. That we find no evidence of such divergence suggests that either the results for within-market mergers are attributable to labor market power, or that the non-labor market power changes following within-market mergers differ from the changes following out-of-market mergers. This could be the case if, for example, mergers of nearby hospitals allow for a more efficient allocation of workers across locations whereas out-of-market mergers do not. While we cannot rule out that out-of-market mergers affect other determinants of wages differently than within-market mergers, these results suggest that the wage effects we observe for within-market mergers likely cannot be explained without allowing for some effect of labor market power.[32]

4.5 Implications for Merger Review

The wage slowdowns we document have normatively ambiguous welfare effects. On one hand, they can be interpreted as efficiencies arising from the merging firms' ability to source labor inputs at a lower price. On the other hand, they can be interpreted as harms to workers, although those harms may be seen as irrelevant by a regulator adhering to a consumer welfare standard focused on the output market. The proper antitrust treatment of this kind of cost reduction is an unsettled question in the courts (Carlton and Israel 2011; Berman Jackson 2017; Hemphill and Rose 2018).

In this section, we outline a simple conceptual framework demonstrating how wage effects from

[31] This is in contrast to the bottom panel of Table 3, which breaks out the effects by the quartile of the change in HHI induced by the merger. In the case of out-of-market mergers, there is no merger-induced change in HHI.

[32] Besides examining out-of-market mergers, we directly test for changes in hospital production functions by estimating difference-in-differences models with non-wage hospital operating costs as the outcome variable. We calculate non-wage hospital operating costs from the HCRIS data by taking total costs and subtracting total wages and capital related costs. We do not detect any evidence of post-merger changes in non-wage hospital operating costs: the difference-in-differences estimates are small and statistically insignificant.

mergers can be directly incorporated into merger evaluation even under a strict consumer welfare standard. The analytical details are provided in Appendix C. Consider an economy with two goods, the first produced by the industry affected by the merger (hereafter the "focal industry") and the second representing all other consumption (hereafter the "numeraire industry"). Denote the price of good $i \in \{1, 2\}$ by p_i. Denote the wage of consumers employed to produce good i by w_i. Consumers have Cobb-Douglas utility over the two goods, $U(x_1, x_2) = x_1^a x_2^b$, where x_i is the consumer's consumption of good i, and a and b are parameters greater than zero. Consumers choose consumption of the two goods to maximize their utility subject to the budget constraint that their spending does not exceed their wage: $p_1 x_1 + p_2 x_2 \leq w_i$.

Suppose that the merger changes prices in the focal industry from p_1 to p_1' and wages from w_1 to w_1'. Abstracting away from possible general equilibrium effects, suppose further that prices and wages in the numeraire industry are unaffected by the merger, and that the workers employed by each industry remain the same. What is the impact of such a merger on consumer welfare?

> Using either compensating variation or equivalent variation to measure the welfare impact of the merger:
>
> 1. For consumers employed in the numeraire industry, the welfare impact of the merger depends solely on the merger's effect on prices. If $p_1' < p_1$, then consumers employed in the numeraire industry are better off. If $p_1' > p_1$, then they are worse off.
> 2. For consumers employed in the focal industry, the welfare impact of the merger depends both on the merger's effect on prices and its effect on wages. If $\left(\frac{p_1'}{p_1}\right)^{\frac{a}{a+b}} < \frac{w_1'}{w_1}$, then consumers employed in the focal industry are better off. If $\left(\frac{p_1'}{p_1}\right)^{\frac{a}{a+b}} > \frac{w_1'}{w_1}$, then they are worse off.

In part 2 of the above results, a sufficient condition for consumers employed in the focal industry being worse off is that the percentage decrease in wages post-merger is greater than the percentage decrease in prices: i.e., $w_1' < w_1$ and $\frac{w_1'}{w_1} < \frac{p_1'}{p_1}$. Therefore, consumers employed in the focal industry can be harmed by the merger even if prices fall. It follows that recognizing not only the effect of mergers on prices but also the effect on wages may raise the fraction of mergers that regulators oppose, even while remaining firmly anchored to a consumer welfare standard.

This simple framework highlights that, after explicitly considering the welfare of consumers employed in the industry affected by a merger, the impact of that merger on consumer welfare depends on both the change in the price of the good and the change in wages. The existing literature on mergers provides ample evidence on price effects (Borenstein 1990; Kim and Singal 1993; Prager and Hannan 1998). Weinberg (2008) provides a helpful review. However, as discussed in Section 2, there is scant evidence in the literature on the wage effects of mergers. If wages are unaffected by mergers, then integrating wage effects into merger review will not affect enforcement decisions. If, on the other hand, merger-induced labor market power sometimes depresses wage growth—as our evidence suggests—then integrating wage effects may lead antitrust authorities to oppose a greater fraction of proposed mergers.

5 Conclusion

This paper provides evidence on the wage impacts of employer consolidation in the hospital industry by examining wage trajectories following hospital mergers. We find evidence of wage slowdowns, but only following mergers that induce large increases in employer concentration, and only for workers whose skills are industry-specific. Where we do find wage slowdowns, we present evidence consistent with an employer labor market power mechanism. On balance, our results suggest that increased labor market power following mergers can reduce wage growth, but that such effects may apply in narrower circumstances than suggested by aggregate estimates of the relationship between concentration and wages. Policymakers and antitrust regulators are actively debating whether labor market effects should be incorporated into merger review. We argue that doing so does not necessarily require a departure from consumer welfare as the main standard by which mergers are evaluated.

Consistent with current approaches to evaluating output market effects of mergers, our empirical results imply that the use of merger review to restrain consolidation on the basis of labor market effects should be sensitive to the specifics of the merger. Our results indicate that likely wage effects may vary substantially by worker type, in ways consistent with theory. Just as antitrust authorities consider multiple product markets affected by a single proposed merger, each merger may involve multiple relevant labor markets. In the hospital context, even very large mergers do not appear to affect wages for workers whose skills are not specific to the health care industry. Our findings thus also highlight that employer consolidation is a policy concern that extends beyond the low-skilled and low-wage workers who have been a focus of recent policy discussions (Krueger and Posner 2018; Krueger and Ashenfelter 2018). On the contrary, high-skilled workers in some industries likely face a smaller set of potential employers than lower-skilled workers whose skills are less industry-specific.

One characteristic of the hospital setting that may not generalize to other industries is that any merger that generates scrutiny due to labor market concentration is likely to get flagged on the basis of existing output market merger review guidelines. Health care workers' willingness to travel for work likely exceeds patients' willingness to travel for health care. Similarly, health care workers can likely more easily substitute to non-hospital employment than many patients can substitute to non-hospital care. Both of these features will typically make the merging hospitals a smaller part of the relevant labor market than the relevant output market. Thus, the initial scrutiny stage may generally be unaffected by adding labor market considerations to merger review.[33] In other industries, such as software development, output markets are less geographically localized, so mergers that could have large local labor market effects may fail to invite scrutiny based on output market-focused merger review practices.

[33]Note, however, that it is possible to construct examples in which considering a broader market definition would increase antitrust scrutiny. For example, consider the merger of two hospitals on opposite ends of a major city. Depending on patient and worker preferences, it is possible that patient substitution between the two hospitals is weak whereas worker substitution is strong. In such a situation, the merger may be expected to have greater labor market effects than output market effects.

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Appendices

A Data Appendix

Wages

This section provides additional detail regarding the construction of the wage measures used in the paper. Our data source for hospital wages is the Center for Medicare & Medicaid Services' (CMS) Healthcare Cost Report Information System (HCRIS). The HCRIS data include extensive information about hospital operations and finances. Wage information is contained in Worksheet S-3, Part II. We begin by restricting the data to general acute care hospitals—excluding specialty hospitals, such as dedicated pediatric hospitals and cancer centers—that are never designated as critical access hospitals. We do not have merger data for non-general acute care hospitals, and wage data are not available for critical access hospitals. HCRIS reports total wages and hours worked for several dozen separate line items, each of which is a fairly narrowly defined class of workers. We aggregate these line items into three broad categories of workers based on wage levels and the likely specificity of skills to the hospital industry.

We define the wage for *unskilled workers* as the average across the Maintenance & Repairs, Operation of Plant, Laundry & Linen Service, Housekeeping, Dietary, Cafeteria, Central Services & Supply, and Medical Records & Medical Records Library line items. The unskilled worker category consists primarily of blue-collar workers. The largest line item in the category is Housekeeping, which in 2012 accounted for 31.6 percent of hours and 25.1 percent of wages in the category. We define the wage for *skilled workers* as the average across the Employee Benefits Department, Administrative & General, Maintenance of Personnel, and Social Service line items. The skilled worker category consists primarily of white-collar workers. The largest line item in the category is Administrative & General, which in 2012 accounted for more than 85 percent of both hours and wages in the category. We define the wage for *nursing and pharmacy workers* as the average across the Nursing Administration and Pharmacy line items. In 2012, approximately half of hours and wages were accounted for by the Nursing Administration line item, with the other half accounted for by the Pharmacy line item.

In 2012, unskilled workers accounted for 13.3 percent of total hours and 7.7 percent of total wages in the HCRIS data. Skilled workers accounted for 12.6 percent of total hours and 13.8 percent of total wages. Nursing and pharmacy workers accounted for 3.8 percent of total hours and 4.7 percent of total wages. Only about half of the total reported hours and wages are broken out into distinct line items, which makes an exhaustive analysis of all hospital employees infeasible. Despite this limitation of the data, the worker categories we examine (i) account for a substantial fraction of hospital hours and wages (29.7 percent of hours and 26.3 percent of wages in 2012), (ii) span a range of skill levels, and (iii) provide variation in the ease with which workers can likely substitute to non-hospital employment.

To provide quantitative support that the worker categories differ in terms of education levels, specificity of skills to the hospital industry, and worker mobility, we calculated a variety of statistics using the Current Population Survey (CPS).[34] While occupation codes in the CPS do not match the HCRIS line items perfectly, a rough match is sufficient to make the main points. Using CPS data, we estimate that 66.0 percent of workers who would fall in our unskilled category have at most a high school diploma and only 9.0 percent hold at least a four-year college degree. In contrast, 35.4 percent of workers who would fall in our skilled category hold at least a four-year college degree, and 41.1 percent of workers who would fall in our nursing and pharmacy category hold at least a four-year college degree. Only 2.9 percent of unskilled workers are employed in the hospital industry, compared to 5.7 percent for skilled workers and 40.3 percent for nursing and pharmacy workers. Within the skilled category, the hospital industry accounts for a larger share of certain occupation codes. For example, 17.1 percent of billing and posting clerks are employed in the hospital industry. Finally, the nursing and pharmacy category also exhibits the greatest within-occupation persistence, with 61.3 percent of workers still employed in the same occupation code a year later, compared to 39.4 percent for the unskilled category.

The wage categories are also cleanly separated in terms of the wage levels observed in the HCRIS data. The line items included in the nursing and pharmacy category have uniformly higher median wages than the line items included in the skilled category, which have uniformly higher median wages than the line items included in the unskilled category. Figure A.1 plots the cumulative distribution function of each wage variable between 1998 and 2014 in four-year intervals. The skilled and nursing and pharmacy wage categories exhibit somewhat faster wage growth than the unskilled category

[34] We use the CPS extract processed and housed by IPUMS (Flood et al. 2018).

over this period. The distributions also exhibit increased wage variation within category over time. The interquartile range for unskilled wages increased from \$2.64 in 1996 to \$3.88 in 2014. For skilled wages, the interquartile range increased from \$4.67 to \$10.97, and for nursing and pharmacy wages it increased from \$4.45 to \$8.41. In percentage terms, however, these differences are less apparent. In 1996, an unskilled worker at the 75th percentile of the wage distribution made 30 percent more than a worker at the 25th percentile. In 2014, the equivalent wage difference was 25 percent.

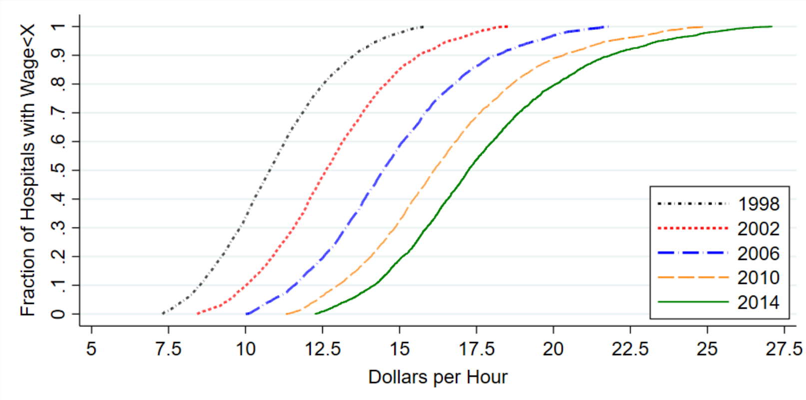
We have also estimated our main difference-in-differences regressions using an alternative definition of worker categories. With the alternative definition, we restrict the unskilled worker category to the lowest paid line items: Housekeeping, Laundry & Linen Service, Cafeteria, and Dietary. We add the Operation of Plant and Maintenance & Repairs line items to the skilled worker category, as these are the most highly compensated line items in the baseline definition of the unskilled category, and may also require technical training. Last, we split the nursing and pharmacy worker category into two, estimating separate effects for the nursing administration line item and the pharmacy line item. The estimates are reported in Table A.1. As with the results reported in the main text, we detect evidence of reduced wage growth only for the skilled, nursing, and pharmacy categories and only for large changes in HHI.

Employer Concentration

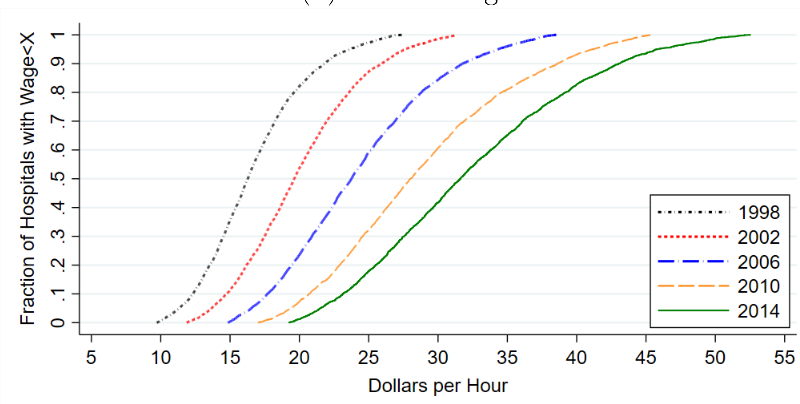
Figure A.2 provides additional summary statistics about hospital employer concentration in our data. The top panel of the figure presents the cumulative distribution function of hospital employer HHI across commuting zones in 1998 and 2012. The industry exhibits increasing concentration over time. In 1998, 17.5 percent of commuting zones had an HHI less than 2,500, compared to 13.3 percent in 2012. In 1998, 49.5 percent of commuting zones had an HHI less than 5,000, compared to 45.6 percent in 2012. The bottom panel of the figure plots the distribution of HHI across commuting zones in 2012. In general, rural areas tend to be much more concentrated than urban areas: in 2012, the correlation between commuting zone population and HHI was -0.45.

Figure A.1: Wage CDFs

(a) Unskilled Wage



(b) Skilled Wage



(c) Nursing and Pharmacy Wage

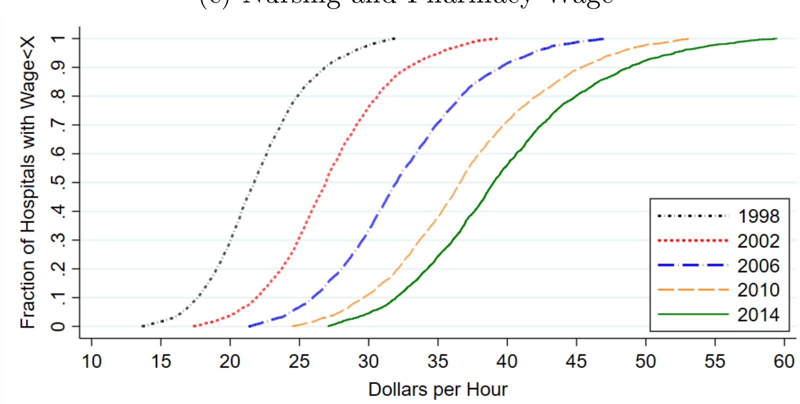


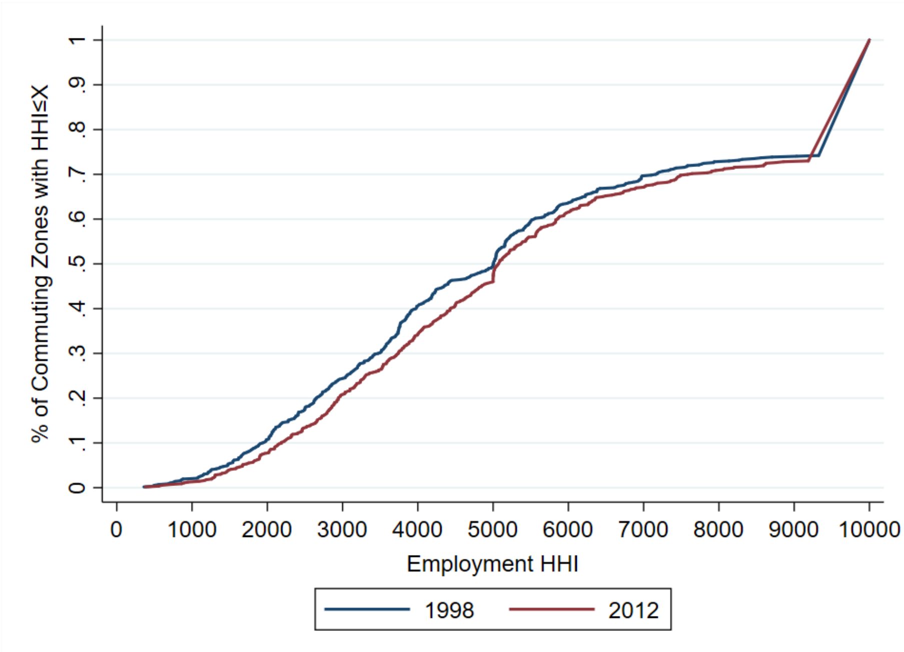
Table A.1: Alternative Wage Categories

	(1)	(2)	(3)	(4)
	Unskilled v2	Skilled v2	Nursing	Pharmacy
Post	-0.001	-0.007	-0.018**	0.003
	(0.005)	(0.007)	(0.008)	(0.006)
Observations	17,324	17,511	17,103	12,106
R-squared	0.893	0.871	0.797	0.932
	(5)	(6)	(7)	(8)
	Unskilled v2	Skilled v2	Nursing	Pharmacy
Post $\times$ 1st quartile ΔHHI	0.004	0.004	-0.008	0.013
	(0.008)	(0.009)	(0.012)	(0.008)
Post $\times$ 2nd quartile ΔHHI	-0.010	-0.027**	-0.005	-0.009
	(0.008)	(0.014)	(0.013)	(0.010)
Post $\times$ 3rd quartile ΔHHI	-0.005	0.005	-0.044***	0.006
	(0.010)	(0.016)	(0.016)	(0.014)
Post $\times$ 4th quartile ΔHHI	0.014	-0.037**	-0.075*	-0.034**
	(0.018)	(0.017)	(0.042)	(0.014)
Observations	17,324	17,511	17,103	12,106
R-squared	0.893	0.872	0.798	0.933
H_0: no heterogeneity	0.399	0.044**	0.096*	0.018**

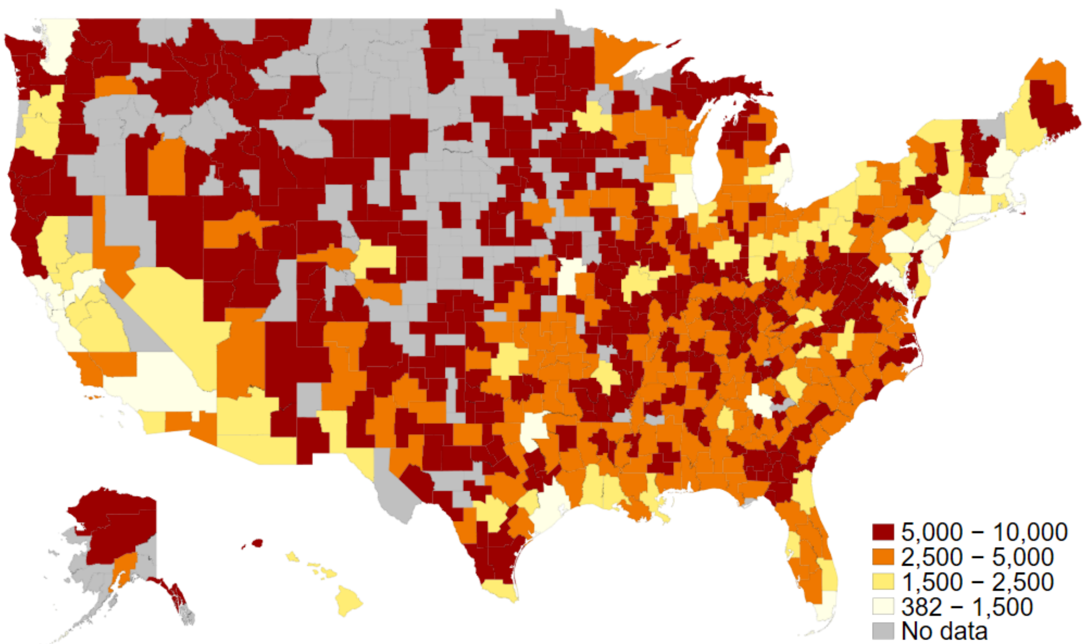
Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges. The bottom row reports the p-value of a test of the null hypothesis that the post$\times\Delta$HHI quartile effects are equal to one another.

Figure A.2: Hospital Employer Concentration

(a) CDF of HHI, 1998 and 2012



(b) HHI by commuting zone, 2012



In the DOJ/FTC *Horizontal Merger Guidelines*, markets with an HHI below 1,500 are classified as unconcentrated; between 1,500 and 2,500, moderately concentrated; and above 2,500, highly concentrated.

B Alternative Control Groups

This section provides further discussion of observable differences between hospitals in the treatment and control groups, along with regression results from specifications that modify the control group. Table B.1 reports summary statistics for the treatment and control groups prior to the mergers under examination. As explained in the main text, our preferred specification defines the control group as hospitals in commuting zones that do not experience any mergers over the course of our sample period (column "No Acq." in Table B.1). Hospitals in this control group are on average smaller than hospitals in the treatment group, and exhibit a different geographic distribution across the US.

We also repeat our main regressions with two alternate definitions of the control group. First, we expand the control group to also include commuting zones that experienced only out-of-market mergers (column "Expanded" in Table B.1). This addition nearly doubles the size of the control group. Second, we use the expanded control group to construct a set of more restrictive matched controls based on the observables in Table B.1: hospital-specific characteristics like wage levels and discharge volume, market-specific characteristics like population, and Census division. Specifically, we use 1-to-1 optimal matching using Mahalanobis distance as the distance metric. The matched controls regressions compare wage changes among hospitals affected by a concentration-increasing merger event to wage changes among observably similar hospitals that are unaffected by mergers. This approach mitigates any differences in wage trends that are attributable to selection on observables into merger events.

Regression results for the pooled difference-in-differences specification with the alternate control groups are reported in Table B.2. Columns 1 to 3 copy the results from Table 3 in the main text. We are unable to reject the null hypothesis of zero wage effects with any of the control groups, and the point estimates remain extremely similar. Specifications broken out by quartiles of ΔHHI are reported in Table B.3. Columns 1 to 3 again copy the results from Table 3 in the main text. Both the qualitative patterns and the magnitude and significance of the coefficients are very similar across the control groups. We estimate statistically significant negative wage effects only following mergers in the top quartile of ΔHHI, and only for the skilled and nursing and pharmacy worker categories.

Table B.1: Comparing Hospitals in the Treatment and Control Groups

		Control Group			Standardized Differences		
	Treated	No Acq.	Expanded	Matched	No Acq.	Expanded	Matched
Hospitals	569	819	1,576	569	–	–	–
Unskilled wage	\$10.94	\$10.56	\$10.67	\$10.49	0.175	0.127	0.209
Skilled wage	\$16.60	\$15.95	\$16.23	\$15.67	0.151	0.087	0.216
Nursing/pharmacy wage	\$21.72	\$21.74	\$22.11	\$21.29	0.004	0.084	0.093
Total FTEs	1,129	749	735	910	0.400	0.414	0.231
Inpatient discharges	9,452	5,701	5,878	7,540	0.519	0.495	0.265
Beds	219	141	146	179	0.528	0.497	0.269
Case mix index	1.383	1.293	1.301	1.348	0.371	0.338	0.141
% Medicare	0.400	0.454	0.452	0.431	0.357	0.349	0.208
% Medicaid	0.124	0.148	0.149	0.139	0.250	0.260	0.154
% Outpatient charges	0.400	0.454	0.439	0.419	0.397	0.289	0.141
One-bedroom rent	\$444	\$384	\$392	\$401	0.588	0.505	0.423
CZ population (millions)	1.068	0.343	0.486	0.540	1.082	0.870	0.788
Census division:							
East North Central	0.130	0.184	0.146	0.130	0.150	0.046	0.000
East South Central	0.100	0.087	0.129	0.100	0.046	0.090	0.000
Middle Atlantic	0.123	0.055	0.053	0.123	0.241	0.248	0.000
Mountain	0.056	0.118	0.092	0.056	0.222	0.137	0.000
New England	0.044	0.044	0.025	0.044	0.000	0.106	0.000
Pacific	0.120	0.068	0.082	0.120	0.176	0.123	0.000
South Atlantic	0.214	0.200	0.186	0.214	0.035	0.071	0.000
West North Central	0.088	0.149	0.139	0.088	0.190	0.162	0.000
West South Central	0.125	0.094	0.148	0.125	0.099	0.067	0.000

Notes: Values are for 1998 if available, and the first year that a hospital appears in the data otherwise.

Table B.2: Mergers and Wages: Difference-in-Differences Estimates

Controls		(1) Unskilled	(2) Skilled	(3) Nursing & Pharmacy
No Acquisitions	Post	0.004	-0.007	-0.007
		(0.004)	(0.008)	(0.006)
	Observations	17,458	17,453	17,328
	R-squared	0.912	0.852	0.874
Controls		**(4) Unskilled**	**(5) Skilled**	**(6) Nursing & Pharmacy**
Expanded	Post	0.001	-0.009	-0.008
		(0.004)	(0.008)	(0.006)
	Observations	28,398	28,535	28,322
	R-squared	0.910	0.849	0.876
Controls		**(7) Unskilled**	**(8) Skilled**	**(9) Nursing & Pharmacy**
Matched	Post	-0.001	-0.009	-0.009
		(0.005)	(0.009)	(0.006)
	Observations	14,322	14,332	14,240
	R-squared	0.913	0.848	0.881

Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges.

Table B.3: Mergers and Wages: Difference-in-Differences Estimates by ΔHHI

Controls		(1) Unskilled	(2) Skilled	(3) Nursing & Pharmacy
No Acquisitions	Post × 1st quartile ΔHHI	0.004	0.004	0.002
		(0.006)	(0.010)	(0.009)
	Post × 2nd quartile ΔHHI	0.003	-0.024	-0.005
		(0.009)	(0.016)	(0.010)
	Post × 3rd quartile ΔHHI	0.006	0.002	-0.019
		(0.008)	(0.021)	(0.014)
	Post × 4th quartile ΔHHI	0.007	-0.042**	-0.065***
		(0.014)	(0.019)	(0.023)
	Observations	17,458	17,453	17,328
	R-squared	0.912	0.853	0.875
Controls		(4) Unskilled	(5) Skilled	(6) Nursing & Pharmacy
Expanded	Post × 1st quartile ΔHHI	0.001	0.001	0.002
		(0.006)	(0.010)	(0.009)
	Post × 2nd quartile ΔHHI	-0.000	-0.026	-0.008
		(0.009)	(0.016)	(0.010)
	Post × 3rd quartile ΔHHI	0.005	-0.001	-0.017
		(0.008)	(0.021)	(0.014)
	Post × 4th quartile ΔHHI	0.001	-0.043**	-0.067***
		(0.014)	(0.018)	(0.024)
	Observations	28,398	28,535	28,322
	R-squared	0.910	0.849	0.876
Controls		(7) Unskilled	(8) Skilled	(9) Nursing & Pharmacy
Matched	Post × 1st quartile ΔHHI	-0.000	0.002	0.001
		(0.006)	(0.011)	(0.009)
	Post × 2nd quartile ΔHHI	-0.003	-0.026	-0.007
		(0.009)	(0.016)	(0.010)
	Post × 3rd quartile ΔHHI	0.001	-0.000	-0.019
		(0.008)	(0.021)	(0.014)
	Post × 4th quartile ΔHHI	-0.001	-0.043**	-0.064***
		(0.014)	(0.019)	(0.022)
	Observations	14,322	14,332	14,240
	R-squared	0.913	0.849	0.882

Notes: ***p<0.01, **p<0.05, *p<0.10. All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, % Medicare, % Medicaid, and % outpatient charges. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges.

C Mergers and Consumer Welfare

This section further describes the modified consumer welfare framework discussed in Section 4.5. We derive the welfare comparisons using both compensating variation and equivalent variation. The economy has two goods, the first produced by the industry affected by the merger (the "focal industry") and the second representing all other consumption (the "numeraire" industry). Denote the price of the first good by p_1 and the price of the second good by p_2. Denote the wage of consumers employed in the focal industry by w_1 and the wage of consumers employed in the numeraire industry by w_2. Consumers have Cobb-Douglas utility over the two goods, $U(x_1, x_2) = x_1^a x_2^b$, where x_1 is the consumer's consumption of the first good, x_2 is the consumer's consumption of the second good, and a and b are parameters greater than zero. Consumers choose consumption to maximize their one-period utility subject to the budget constraint that their spending does not exceed their wage. For a consumer employed to produce good i, utility-maximizing consumption is given by $x_1^* = \left(\frac{a}{a+b}\right)\left(\frac{w_i}{p_1}\right)$ and $x_2^* = \left(\frac{b}{a+b}\right)\left(\frac{w_i}{p_2}\right)$. Consumers spend the entirety of their budget, which is determined by their wage.

Now suppose that a merger occurs in the focal industry. Denote the post-merger price in the focal industry by p_1' and the post-merger wage by w_1'. Assume that prices and wages in the numeraire industry are unaffected. To compute the welfare impact of the merger, we evaluate both the compensating variation and the equivalent variation of the merger.[35] The compensating variation of the merger for consumers employed in the numeraire industry, CV_2, is given by

$$CV_2 = w_2 \left[\left(\frac{p_1'}{p_1} \right)^{\frac{a}{a+b}} - 1 \right]$$

and the equivalent variation, EV_2, is given by

$$EV_2 = w_2 \left[1 - \left(\frac{p_1}{p_1'} \right)^{\frac{a}{a+b}} \right] .$$

[35]Denote a consumer's maximum utility by $U^*(w, p)$, where w is the consumer's wage and p is the price of the first good (the only two things that may change as a result of the merger). For consumers employed in the numeraire industry, compensating variation is given by the CV_2 such that $U^*(w_2 + CV_2, p_1') = U^*(w_2, p_1)$. For consumers employed in the focal industry, compensating variation is given by the CV_1 such that $U^*(w_1' + CV_1, p_1') = U^*(w_1, p_1)$. For consumers employed in the numeraire industry, equivalent variation is given by the EV_2 such that $U^*(w_2 - EV_2, p_1) = U^*(w_2, p_1')$. For consumers employed in the focal industry, equivalent variation is given by the EV_1 such that $U^*(w_1 - EV_1, p_1) = U^*(w_1', p_1')$.

Both measures are positive—i.e., the consumer is harmed by the merger—if and only if $p_1' > p_1$. That is, the merger harms consumers employed in the numeraire industry if and only if the merger increases prices. For these consumers, changes in wages matter only to the extent that they pass through to prices.

The compensating variation of the merger for consumers employed in the focal industry, CV_1, is given by

$$CV_1 = w_1 \left(\frac{p_1'}{p_1}\right)^{\frac{a}{a+b}} - w_1'$$

and the equivalent variation, EV_1, is given by

$$EV_1 = w_1 - w_1' \left(\frac{p_1}{p_1'}\right)^{\frac{a}{a+b}} .$$

For these consumers, the impact of the merger depends both on the change in prices and the change in their wages. For both compensating variation and equivalent variation, the merger causes harm if and only if $\left(\frac{p_1'}{p_1}\right)^{\frac{a}{a+b}} > \frac{w_1'}{w_1}$. A sufficient condition for satisfying this inequality is that the merger decreases wages and that the relative decrease in wages is greater in magnitude than any decrease in prices (i.e., $w_1' < w_1$ and $\frac{w_1'}{w_1} \leq \frac{p_1'}{p_1}$).

An analysis of mergers that implicitly treats all consumers as employed in the numeraire industry may therefore miss potential welfare losses in cases where the merger depresses wages in the focal industry. A seemingly "pro-competitive" merger that decreases prices may harm welfare if the merger also results in increased labor market power. In addition, this exercise shows that the potential welfare losses from increased labor market power can in principle be integrated into merger review while remaining firmly anchored to a consumer welfare standard.