

Employer Consolidation and Wages: Evidence from Hospitals[†]

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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 consider alternative explanations and find that the observed patterns are unlikely to be explained by merger-related changes besides labor market power. Wage growth slowdowns are attenuated in markets with strong labor unions, and wage growth does not decline after out-of-market mergers that leave local employer concentration unchanged. (JEL G34, I11, J22, J24, J31, J42, R32)

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, Marinescu, and Steinbaum 2017; Benmelech, Bergman, and Kim 2019; Qiu and Sojourner 2019; Rinz 2018), leading to pressure on antitrust authorities to consider labor monopsony effects in merger review.¹ Merger review provides a natural policy lever for curtailing labor market consolidation through established regulatory mechanisms (CEA 2016; Naidu, Posner, and Weyl 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

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¹ In September 2017, Senator Amy Klobuchar (D-MN) introduced a 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 (FTC 2018).

wages, antitrust authorities in the United States 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. 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 job tasks are generally not specific to the hospital industry, such as cafeteria workers; skilled workers in nonmedical occupations, such as workers in the employee benefits department; and skilled health care professionals, specifically nursing and pharmacy workers. We find a pattern of results consistent with theory, with estimates varying depending on (i) the worker category and (ii) the magnitude of the change in concentration induced by the merger.

For unskilled workers, we do not find 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. Measured over the four years following the merger, for the top quartile of concentration-increasing mergers, we estimate that wages are 4.0 percent lower for skilled non-health professionals and 6.8 percent lower for nursing and pharmacy workers than they would have been absent the merger. These estimates imply 1.0 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. We fail to find evidence that the estimated effects are driven by market changes other than the mergers themselves. The estimated effects do not appear to be generated by pre-merger differences in wage trends, post-merger changes in local economic conditions, or the overlap of our sample with the Great Recession. The results are also robust to alternative constructions of the control group.

Next, we explore potential economic mechanisms through which mergers may dampen wage growth. The results described thus far are consistent with merger-induced increases in labor market power reducing wage growth. We find wage effects only for workers with the most industry-specific skills, and only then when mergers constitute a substantial increase in employer concentration. Nevertheless, mergers may affect wages through other mechanisms, such as managerial changes designed to reduce labor costs. We test for this possibility by examining the wage effects of "out-of-market" mergers in which the merging hospitals are located in non-overlapping labor markets, and thus the merger does not affect local labor market concentration. Because we can plausibly rule out labor market power effects 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 preexisting level of concentration in the market. We also further test for the labor market power mechanism by examining whether

the presence of countervailing worker power attenuates post-merger reductions in wage growth. Estimates using two measures of worker power suggest that it does: 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. In short, the totality of our results suggests that increased labor market power is involved in the post-merger wage growth slowdowns we document.

The existence of labor market power is itself consistent with several models of the labor market that generate upward-sloping labor supply curves for individual firms. 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 (Robinson 1969). Other models of the labor market, however, do not require employment suppression to generate wages below the marginal product of labor. Models with labor market search frictions or bargaining over wages can generate post-merger wage reductions without any corresponding reduction in employment (Mortensen and Pissarides 1994). For instance, if a merger weakens the value of workers' outside option in wage negotiations, employers will in general be able to command a greater proportion of the available rents, even without any changes in the size or composition of the employer's workforce (Hemphill and Rose 2018). To shed light on whether classical monopsony alone is sufficient to explain our results, we also estimate difference-in-differences regressions with the level of employment as the outcome variable. We do not detect evidence of employment suppression, a result that is most consistent with models of wage setting involving bargaining or search frictions playing some role. One reason why classical monopsony may be less relevant in our context than in other industries is that hospitals face relatively inelastic demand in the output market, in large part because the end consumer does not pay the marginal price for hospital services due to health insurance (Newhouse and the Insurance Experiment Group 1993; Finkelstein 2014). Thus, unless hospitals can maintain their output levels while decreasing their labor inputs, classical monopsony effects are arguably unlikely.

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 I connects the paper to the related literature. Section II describes our wage and merger data. Section III presents the main difference-in-differences analysis, followed by a deeper examination of mechanisms in Section IV. Section V concludes.

I. Related Literature

Predictions of employer mergers causing wage reductions generally rely on the assumption that the affected labor market is not perfectly competitive. In a perfectly competitive labor market, employers are wage takers and thus mergers are unlikely to affect wages, other than through a merger-induced change in the marginal product of labor. There is a rich empirical literature documenting the existence of imperfectly competitive labor markets. Earlier papers in this literature often

estimated the marginal product of labor and inferred employer market power if the estimated marginal product exceeded wages (Scully 1974). More recent papers often proceed by estimating or calibrating the inverse elasticity of labor supply, inferring employer market power from an upward-sloping labor supply curve facing an individual employer. This approach has been taken in a variety of industries and markets, including nurses (Sullivan 1989; Staiger, Spetz, and Phibbs 2010), grocery store workers (Ransom and Oaxaca 2010), educators (Ransom and Sims 2010, Goolsbee and Syverson 2019), migrant workers (Naidu, Nyarko, and Wang 2016), and occupations represented in job vacancy data (Azar, Berry, and Marinescu 2019).

The labor supply elasticity test can be used to detect employer market power arising from a variety of sources. A pure monopsonist employer arises when it is the only firm in the labor market, and can therefore drive down wages by restricting employment below the competitive level and walking back along the upward-sloping labor supply curve (Robinson 1969). The same mechanism can arise from oligopsonist employers jointly restricting employment below the competitive level (Boal and Ransom 1997). We refer to this type of employer market power as “classical monopsony.” In practice, economists generally view classical monopsony as rare, instead invoking more recent advances in understanding the sources of upward-sloping labor supply curves to explain employer market power (Boal and Ransom 1997). These sources include employer differentiation, search frictions, switching costs, and other models where wages are negotiated (see, for example, Boal and Ransom 1997, Mortensen and Pissarides 1999, Eckstein and van den Berg 2007).

The empirical literature documenting upward-sloping labor supply curves establishes that many labor markets are characterized by employer market power. However, as is the case for product markets in which producers have market power, the mere existence of market power does not trigger antitrust enforcement action (Rose 2018). In the United States, antitrust authorities can take action only if there is anticompetitive conduct, such as wage-fixing, or in the event of a merger that is projected to increase market power. There has been a recent wave of interest in potentially anticompetitive conduct in labor markets, such as the non-poaching agreements studied by Krueger and Ashenfelter (2018).

Meanwhile, recent work connecting employer concentration to wages has focused primarily on describing the relationship between the two in cross-sectional and longitudinal data. Across a range of data sources, industries, and modeling choices, these papers typically find a negative association: higher employer concentration is associated with lower wages (Azar, Marinescu, and Steinbaum 2017; Rinz 2018; Benmelech, Bergman, and Kim 2019; Hershbein, Macaluso, and Yeh 2019; Qiu and Sojourner 2019).² The difficulty in translating these results into prescriptions

² Azar, Marinescu, and Steinbaum (2017) examine a variety of occupations (defined by detailed six-digit SOC occupation codes) and define the geographic component of the labor market as the commuting zone. Benmelech, Bergman, and Kim (2019) study manufacturing and define the geographic component of the labor market as the county. Rinz (2018) examines four-digit NAICS code industries, using commuting zones to define geographic labor markets. Qiu and Sojourner (2019) examine three-digit Census industry code industries (defining occupations using Census occupational codes), using core-based statistical areas to define geographic labor markets. The data used to construct measures of employer concentration also vary across studies. Azar, Marinescu, and Steinbaum (2017) compute vacancy concentration using recent job postings data from an online job board, whereas Benmelech, Bergman, and Kim (2019) and Rinz (2018) utilize long panels of business employment from the Census Bureau’s Longitudinal Business Database. Qiu and Sojourner (2019) use proprietary data from a commercial data and analytics company. Although broadly similar, there are also meaningful differences in these studies’ empirical specifications. For

for antitrust policy is that they do not clearly isolate an economic mechanism driving the negative relationship between employer concentration and wages. Consider, for example, an employer exiting a market due to weakening product demand. Even if individual employers lack market power, the corresponding decline in the demand for labor may decrease equilibrium wages, and this decrease coincides with an increase in measured employer concentration. That is, a negative relationship between employer concentration and wages can obtain even if labor markets are perfectly competitive.

Careful studies of employer mergers can therefore complement the findings from this existing literature by more clearly differentiating between employer market power and other potential mechanisms. As noted by Berry, Gaynor, and Scott Morton (2019), the ideal research design accounts for the particular nature of competition in the given empirical context. Because the nature of competition varies substantially across industries, a research design tailored to industry-specific idiosyncrasies is difficult to achieve with multi-industry data. Industry studies of employer mergers, such as this paper, complement existing findings by providing more comprehensive evidence on economic mechanisms.

The existing literature on how employer mergers and acquisitions affect wages is small, and its empirical findings are mixed. Some papers find negative effects of mergers on wages (Lichtenberg and Siegel 1990, Li 2012), others find no effect (Brown and Medoff 1987; Currie, Farsi, and MacLeod 2005; DePasquale 2018), and some even find positive effects (McGuckin and Nguyen 2001, Dranove and Lindrooth 2003).³ The existing literature has largely focused on the effect of mergers on the merging employers themselves, through mechanisms such as changes in management technology, elimination of duplicated jobs, and renegotiation of employment contracts between the merged employer and the (often unionized) workforce. Given this focus, existing studies typically do not distinguish between merger events based on the degree of employer consolidation induced by the merger. This feature of the literature makes it difficult to infer whether the estimated effects are attributable to merger-induced changes in labor market power or to within-firm explanations, especially in light of the mixed directions of the estimated effects. We add to this literature by examining the magnitude of merger-induced increases in local employer concentration. To our knowledge, the only papers that exploit mergers as a source of variation in employer concentration to measure the effect of employer concentration

instance, Qiu and Sojourner (2019) include controls for product-market concentration. Comparing the estimated magnitude of the employer concentration-wage relationship across these studies is complicated by the differences in market definition, wage measures, and other empirical choices. Azar, Marinescu, and Steinbaum (2017) estimate that increasing concentration from the twenty-fifth to seventy-fifth percentile, roughly 6,000 HHI points in their data, is associated with a wage reduction of 17 percent. Benmelech, Bergman, and Kim (2019) estimate that a two standard deviation increase in concentration, roughly 6,000 to 7,000 HHI points in their data, is associated with a wage reduction of 2.9 percent. Rinz (2018) estimates that increasing concentration from the median to the seventy-fifth percentile, roughly 1,200 HHI points in his data, is associated with a wage reduction of about 10 percent. Qiu and Sojourner (2019) estimate that a one standard deviation increase in concentration, roughly 880 HHI points in their data, is associated with a wage reduction of 14 to 26 percent.

³Dranove and Lindrooth (2003); Currie, Farsi, and MacLeod (2005); and DePasquale (2018) all examine hospital mergers. Dranove and Lindrooth (2003) find mixed effects on wages depending on the degree of operational integration. Although neither Currie, Farsi, and MacLeod (2005) nor DePasquale (2018) find an effect on wages, both find other effects on workers. Currie, Farsi, and MacLeod (2005) find an increase in nurse effort, which is consistent with a negative effect on quantity-adjusted wages. DePasquale (2018) finds employment decreases following some types of mergers.

on wages are contemporaneous studies by Arnold (2019) and Benmelech, Bergman, and Kim (2019). Like these papers, we add to the literature on mergers and wages by highlighting the economic mechanism of labor market power. We further elucidate this mechanism by distinguishing between workers with varying levels of skill and skill specificity within the same employer.

Finally, our paper is related 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 (Lewis and Pflum 2017; Dafny, Ho, and Lee 2019), merger-facilitated collusion (Miller and Weinberg 2017), the effects of mergers on acquired firms' behavior (Eliason et al. 2020), 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.

II. Data

This section briefly describes the key sources of data used in the empirical analysis. Online Appendix Section A provides additional details. Our empirical context is the hospital industry, which is a fitting context for studying the labor market effects of mergers. The industry is large, with its 5 million workers accounting for approximately 3.5 percent of total US employment in 2019 (BLS 2019a, b), 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. Besides wage data, we also require information on mergers and employer concentration, which we obtain from multiple sources of industry and government data.

A. Wages

Our primary data source for hospital wages is the Centers for Medicare and Medicaid Services' (CMS) Healthcare Cost Report Information System (HCRIS) from 1996 to 2014 (CMS 2015b). All Medicare-certified institutional providers, including effectively every hospital in the United States, are required to submit data to HCRIS on an annual basis. We use these data to construct wage measures for all general acute care hospitals that are never designated as critical access hospitals (critical access hospitals are not required to report wage data). For each hospital-year, HCRIS reports wage information for several dozen distinct line items corresponding to different types of workers. For each line item, HCRIS reports the total amount paid by the hospital to employees falling under that line item, the total hours that

those employees worked, and the corresponding hourly wage.⁴ 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 nonmedical workers, and nursing and pharmacy workers. The full list of occupations included in each category is presented in online Appendix Section A, along with further quantitative support from the Bureau of Labor Statistics' Current Population Survey (CPS) that the categories meaningfully differ by education levels, specificity of skills to the hospital industry, and worker mobility. Our wage measure for each category is the average hourly wage paid by the hospital to workers in the category, calculated as the total amount paid to workers divided by the total hours worked.

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 10 percent of such workers have a four-year college degree. This category of workers likely has the least industry-specific skill-set 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 one-third of these workers have a four-year college degree. The nursing and pharmacy worker category consists of nursing administration employees and pharmacy employees. We estimate that more than 40 percent of these workers have a four-year college degree. The nursing and pharmacy category of workers likely has the most industry-specific skill-set of the three categories, and hospitals therefore constitute the greatest share of their set of potential employers. 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 nominal annual growth; 0.6 percent in real terms). The median wage for skilled workers grew from \$15.29 to \$31.61 (4.1 percent nominal annual growth; 1.8 percent in real terms). The median wage for nursing and pharmacy workers grew from \$20.39 to \$39.01 (3.7 percent nominal annual growth; 1.3 percent in real terms). Our goal in the empirical analysis is to estimate the extent to which mergers slowed this wage growth, if at all.

B. 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 (AHA 2016). We supplement

⁴HCRIS does not report the value of fringe benefits, so our wage measures include only wages and salaries. Notably, Qiu and Sojourner (2019) directly study health insurance benefits in addition to wages. See Section IVB for further discussion of non-wage compensation in our setting.

the AHA data, whose updates to the ownership variables are sometimes delayed or miscoded, with merger and acquisition transaction-level data from Irving Levin's Hospital Acquisition Report (Irving Levin Associates Health 2017). Finally, we use internet searches of archived news stories and hospital web sites 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. Because 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.⁵ Just as we define wages by worker category, an alternative approach is to define FTEs by worker category. We have examined wage category-specific FTE measures, but the various FTE measures are highly correlated and do not yield meaningfully different insights.

Labor Market Definition.—We define the geographic component of the labor market at the level of a commuting zone. A priori, the relevant geographic delineation of a labor market need not coincide with the geographic delineations of the employers' product markets, so we do not rely on definitions developed for hospital output markets. 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. 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 in our primary specifications.⁶

Measuring Concentration.—We use the HCRIS data to measure employer concentration using the Herfindahl-Hirschman index (HHI) of hospital FTEs

⁵ Specifically, hospital i 's FTEs in year t are given by

$$FTEs_{it} = \frac{365}{CostReportDays_{it}} \times \frac{TotalHours_{it}}{52 \times 40},$$

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

⁶ In robustness checks, we use two alternative geographic market definitions, one broader (hospital referral regions) and the other narrower (core-based statistical areas) than commuting zones. Throughout the analysis, we rely on several data sources for the relevant geographic crosswalks: Economic Research Service (2012); Fowler, Jensen, and Rhubart (n.d.); Dartmouth Atlas (2019); Department of Commerce (1995); Historical Delineation Files: CBSA to CSA (2003); Autor and Dorn (n.d.).

within a commuting zone-year pair. HHI is defined as the sum of squared total FTE employment shares among hospitals in the market, combining the shares of hospitals under the same ownership. Worker category-specific HHIs are highly correlated with this measure, with correlation coefficients exceeding 0.97, 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. In addition, predicted wages can be expressed as a function of HHI in several models of labor markets (Jarosch, Nimczik, and Sorkin 2019; Arnold 2019). 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 online Appendix Section A.

Mergers increase HHI, with the magnitude increasing in the employment shares of the merging hospitals. Specifically, 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. As explained further in Section IIIA, we use the change in hospital employer HHI to bin merger events according to the magnitude of the HHI increase induced by the merger.⁷ Importantly, this HHI measure captures concentration only among hospital employers. We use this as our primary measure because of the richness of our data for this set of employers, but note that it almost assuredly overstates both the degree of effective employer concentration in the relevant labor market and the increase in concentration induced by mergers. Unskilled workers may be able to substitute to non-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 the hospital mergers we examine affect overall health care employer concentration, we also compile data for a broader set of health care employers from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW).⁸ The QCEW reports employment counts at the county-industry level, which we then aggregate to the commuting zone (BLS 2019b). 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 HHI *levels* for the health care industry as a whole. However, because computing the *change* in HHI induced by a merger only requires the shares of the merging hospitals, we can use the QCEW to measure how the mergers in our sample affect overall health care employer concentration.

⁷ For the purpose of screening horizontal mergers, Nocke and Whinston (2020) argue that there are both theoretical and empirical reasons to focus on the change in HHI rather than also considering the level.

⁸ 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.

III. Difference-in-Differences Analysis

This section presents our main difference-in-differences results estimating the effects of mergers on wages. We find that mergers reduce wages, but only when (i) the increase in concentration induced by the merger is large and (ii) workers' skills are industry specific. As with any difference-in-differences design, further work is required to demonstrate that the observed effects are attributable to the treatment (in our case, mergers), and that the treatment operates through the mechanism of interest (in our case, labor market power). To facilitate the exposition, we defer the detailed examination of mechanisms to Section IV. In this section, we first build the regression specification and then present the main difference-in-differences results. We close the section by examining pretrends in wages and assessing whether mergers coincide with other changes in local economic conditions that could plausibly depress wage growth.

A. 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 through 2010 so that we 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.⁹ 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 1 provides summary statistics about the merger-induced increases in concentration in the affected commuting zones. The median merger induces a hospital employer concentration increase of 401 HHI points, but there is substantial variation across commuting zones. The bottom panel of the table bins commuting zones into quartiles based on the corresponding increase in hospital employer concentration.¹⁰ Online Appendix Figure A.3 visually displays the concentration increases in each bin. 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. Although 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

⁹For estimates using commuting zones with multiple mergers during the sample period, see online Appendix Section B7.

¹⁰The binning of mergers is nearly identical when based on the change in overall health care employer concentration instead of hospital employer concentration.

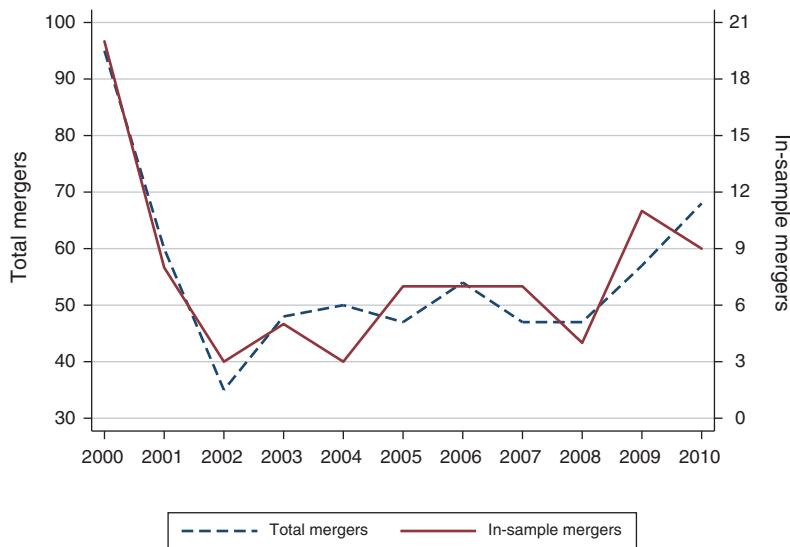


FIGURE 1. MERGER COUNTS, 2000–2010

Notes: 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.

TABLE 1—OBSERVED MERGERS AND COMMUTING ZONE EMPLOYER CONCENTRATION

	Number of in-market hospitals	Hospitals only			All health care Δ HHI
		Pre-merger HHI	Post-merger HHI	Δ HHI	
All (average)	8.2	3,135	4,055	920	246
All (median)	8.0	2,750	3,377	401	59
Δ HHI quartile averages:					
First quartile Δ HHI	12.9	1,961	2,024	63	11
Second quartile Δ HHI	9.6	2,793	3,028	235	56
Third quartile Δ HHI	7.1	3,205	3,823	618	125
Fourth quartile Δ HHI	3.3	4,580	7,344	2,764	790

Note: The *Hospitals only* columns use only hospital employment to compute shares, whereas the *All health care* column also includes non-hospital health care employment.

health care employment (average increases around 100 points or less). Only in the top quartile do the mergers involve substantial increases in concentration both in terms of hospital employment and overall health care employment.¹¹ Given these stark differences across merger events, in addition to examining all 84 commuting zones as a group, we also estimate specifications that test for heterogeneity by the magnitude of the concentration increase induced by the merger. Because our main

¹¹ All the top-quartile mergers imply sufficiently large likely increases in product market concentration that the merger would be presumed to be likely to enhance market power under the *Horizontal Merger Guidelines* (see online Appendix Figure A.4). However, many of these transactions may have been unchallenged because the dollar values of the transaction were below the \$50 million reporting threshold under the Hart-Scott-Rodino Act (Wollmann 2019). Of the transaction amounts reported in the Irving Levin data, about two-thirds of top-quartile mergers were below the reporting threshold.

TABLE 2—TREATMENT AND CONTROL HOSPITAL OBSERVABLE CHARACTERISTICS

	Treated hospitals	Control hospitals	Std. diff.	Treated hospitals by quartile of Δ HHI			
				First quartile	Second quartile	Third quartile	Fourth quartile
Unskilled wage	\$10.94	\$10.56	0.175	\$11.45	\$10.72	\$10.55	\$10.25
Skilled wage	\$16.60	\$15.95	0.151	\$17.44	\$16.39	\$15.67	\$15.60
Nursing and pharmacy wage	\$21.72	\$21.74	0.004	\$22.13	\$21.35	\$21.68	\$21.03
Total FTEs	1,129	749	0.400	1,310	1,153	945	622
Inpatient discharges	9,452	5,701	0.519	10,815	9,745	7,981	5,461
Beds	219	141	0.528	245	225	191	137
Case mix index	1.383	1.293	0.371	1.396	1.399	1.367	1.299
Percent Medicare	0.400	0.454	0.357	0.359	0.417	0.429	0.474
Percent Medicaid	0.124	0.148	0.250	0.113	0.116	0.135	0.170
Percent outpatient charges	0.400	0.454	0.397	0.379	0.419	0.409	0.426
One-bedroom rent	\$444	\$384	0.588	\$491	\$422	\$415	\$355
CZ population (millions)	1.068	0.343	1.082	1.614	0.857	0.619	0.193
CZ per capita income	\$25,859	\$22,830	0.602	\$27,629	\$25,828	\$23,720	\$22,635
CZ percent unemployment	0.044	0.053	0.342	0.041	0.042	0.048	0.060
CZ percent age 65 or older	0.134	0.136	0.080	0.123	0.140	0.136	0.161
Nurse unionization rate	0.159	0.121	0.292	0.223	0.123	0.087	0.143

Notes: Values are for 1998 if available and the first year that a hospital appears in the data otherwise. *Std. diff.* reports the standardized difference between the treated and control hospitals.

treatment sample consists of 84 merger events, each quartile in these specifications consists of 21 merger events.¹²

Because 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 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 remaining 70 percent are bystanders to those mergers (i.e., they compete in the same market).

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. Table 2 reports average observable characteristics for hospitals in the treatment and control groups. Although baseline wages for the three worker categories are fairly well balanced across treatment and control hospitals, there are other observable differences. 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 Δ HHI. Similarly, control hospitals tend to be smaller than treated hospitals, in both bed count and patient volume. Although 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 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 (see Section IIID). Second, we estimate specifications that expand

¹²To further expand the sample, we estimate specifications including commuting zones with multiple merger events in online Appendix Section B7.

the control group by imposing less stringent requirements on merger activity in control commuting zones, as well as specifications using a matched control group (see Section IIIF).

B. Regression Specification

To measure the effect of mergers on wages, we estimate a regression of the following form for each of the three worker categories:

$$(1) \quad \ln(\text{wage}_{imt}) = \delta_i + \tau_t + \alpha \cdot \text{post}_{mt} + \beta \cdot \mathbf{X}_{imt} + \varepsilon_{imt},$$

where wage_{imt} is the wage for a given worker category in hospital i in year t . The variable of interest is post_{mt} , which is an indicator for whether and when commuting zone m is treated, that is, whether it 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.¹³

The vector \mathbf{X}_{imt} contains a variety of additional market-level and hospital-level variables. To control for within-commuting zone changes in economic conditions (e.g., differential impacts of the Great Recession), we include the logarithm of per capita income and the unemployment rate. To control for within-commuting zone changes in the cost of living, we include the logarithm of the market rent for a one-bedroom apartment. To roughly control for within-commuting zone changes in health care demand, we include the logarithm of the commuting zone's population and the fraction of the population who are Medicare-aged (65 or older). To control for individual hospital characteristics that may affect wages, we include hospital size (measured by the logarithm of 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 the logarithm of case mix index), and the hospital's inpatient versus outpatient mix (measured by the fraction of hospital charges accounted for by outpatients).¹⁴

¹³ A recent literature in econometrics has shown that difference-in-differences models of this form yield a weighted average of all possible permutations of pairwise difference-in-differences estimators, where a pair is either the never-treated control group paired with a cohort of observations treated at time t , or a cohort of observations treated at time t paired with a cohort of observations treated at time $t' > t$ (Goodman-Bacon 2018; see also Callaway and Sant'Anna 2019 and de Chaisemartin and D'Haultfoeuille 2019). We address this point in two ways. First, we estimate event study-style specifications with saturated leads and lags (see Section IIID), which do not use comparisons between treated commuting zones for identification. Second, we estimate separate regressions for each treated cohort (following Goodman-Bacon 2018) and then take a weighted average of the cohort-specific point estimates (in the spirit of Callaway and Sant'Anna 2019), with weights equal to each cohort's share of all treated observations. These decomposed-and-reweighted estimates, which also do not use comparisons between treated commuting zones for identification, are essentially indistinguishable from our baseline results (see online Appendix Section B2).

¹⁴ Income measures are drawn from the Bureau of Economic Analysis' Local Area Personal Income tables (BEA 2019). Unemployment and total population measures are drawn from the Bureau of Labor Statistics' Local Area Unemployment Statistics (BLS 2019a). Residential apartment rents are measured from the Department of Housing and Urban Development's Fair Market Rent data (HUD 1996). The Medicare-aged fraction of the population is measured from the National Bureau of Economic Research's population estimates files, which are in turn compiled by the Census Bureau's Population Estimates Program (USCB 1996, 2016). Hospital characteristics are measured from HCRIS (CMS 2015a, b).

TABLE 3—DIFFERENCE-IN-DIFFERENCES ESTIMATES

	Unskilled (1)	Skilled (2)	Nursing and pharmacy (3)
Post	0.005 (0.005)	−0.006 (0.008)	−0.007 (0.006)
Observations	17,458	17,453	17,328
R^2	0.913	0.852	0.875
	Unskilled (4)	Skilled (5)	Nursing and pharmacy (6)
Post \times first quartile Δ HHI	0.004 (0.006)	0.005 (0.010)	0.002 (0.009)
Post \times second quartile Δ HHI	0.007 (0.009)	−0.022 (0.016)	−0.001 (0.010)
Post \times third quartile Δ HHI	0.007 (0.008)	0.002 (0.021)	−0.019 (0.014)
Post \times fourth quartile Δ HHI	0.002 (0.014)	−0.041 (0.019)	−0.070 (0.022)
Observations	17,458	17,453	17,328
R^2	0.913	0.853	0.875
H_0 : no heterogeneity	0.978	0.105	0.016

Notes: All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, percent Medicare, percent Medicaid, percent outpatient charges, (log) per capita income, percent unemployment, and percent of the population age 65 or older. 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 Δ HHI quartile effects are equal to one another.

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, because 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.

C. Main Results

The top panel of Table 3 (columns 1 to 3) presents the estimates of equation (1). 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 1 percent. Despite the recent literature documenting a negative association between employer concentration and wages (see Section I), the null results in the top panel of Table 3 are arguably unsurprising. The median labor market experiencing a hospital merger sees overall health care employer concentration rise by only 59 HHI points. This HHI increase is roughly analogous to a merger of two employers in a market that initially has eighteen equally sized employers.

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 1), 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 are unchanged following mergers in the bottom three quartiles. We find statistically significant wage effects only for mergers in the top quartile of ΔHHI .¹⁵ For the skilled worker category, we estimate that nominal wages are 4.0 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 nominal wages are 6.8 percent lower.^{16,17} In terms of wage growth, these estimates imply that post-merger annual wage growth (measured over the four years following the merger) is 1.0 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 IIA, ranges from 3 to 4 percent. The estimates for mergers in the top quartile of ΔHHI therefore represent substantial slowdowns in wage growth.

We also explore 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 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 (-0.073 (0.036)), targets (-0.072 (0.048)), and their non-merging rivals (-0.055 (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 (-0.067 (0.026)) and targets (-0.052 (0.030)), but statistically indistinguishable from zero for their non-merging rivals (0.022 (0.027)).

Overall, these results suggest that for employer consolidation to put downward pressure on wages, a substantial increase in concentration is required. Moreover, the results highlight the importance of appropriately defining the relevant labor market. We find significant effects only for the skilled worker category and the nursing and

¹⁵This 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 (see Nocke and Whinston 2020).

¹⁶Exponentiating the coefficients for interpretation, $\exp(-0.041) - 1 = -0.040$ and $\exp(-0.070) - 1 = -0.068$.

¹⁷The results are robust to estimating the regressions only with hospital and year fixed effects, excluding other controls. For mergers in the top quartile of ΔHHI , the estimate for the skilled worker category is -0.036 (statistically significant at the 10 percent level) and the estimate for the nursing and pharmacy worker category is -0.061 (statistically significant at the 1 percent level). All other estimates are smaller in magnitude and statistically indistinguishable from zero.

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 employer consolidation on a broader scale would have negative wage effects for these workers.

D. Wage Trends Prior to Mergers

The difference-in-differences estimates above will yield a biased estimate of the causal effect of mergers if mergers, and especially large mergers, disproportionately occur in markets that would have experienced a slowdown in wage growth even absent the merger. 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. Although 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.

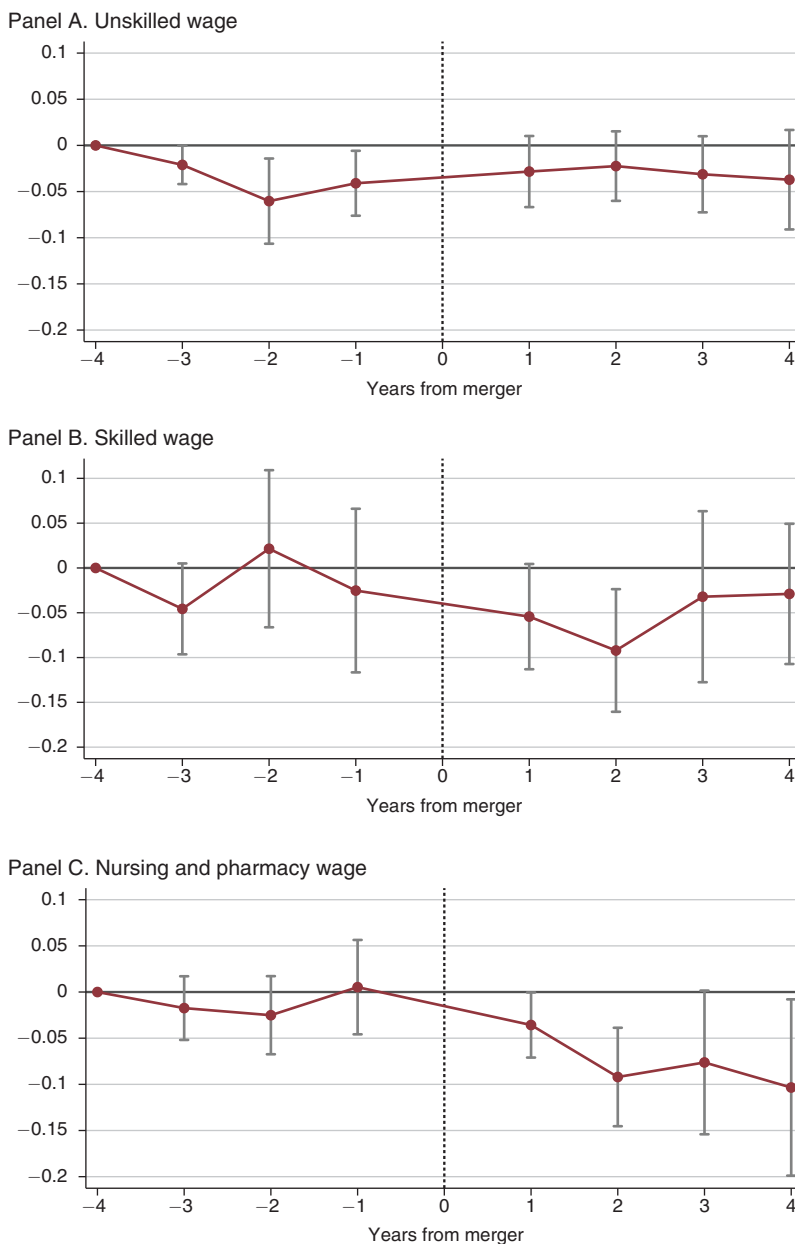
For mergers in the top quartile of ΔHHI , Figure 2 plots the coefficients from regressions that replace the single post_{mt} indicator in equation (1) with lead indicators for the four years leading up to a merger and lag indicators for the four years following a merger. We do not detect differential pretrends 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 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.¹⁸

E. Local Economic Conditions

Even absent differential pretrends 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, with the slowdown not being fully captured by the unemployment and per capita income controls. 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 per capita income) for mergers in the top quartile of ΔHHI .¹⁹ Because these measures do not vary across hospitals within a commuting zone, we estimate these regressions at the commuting zone level. We do not find

¹⁸In analogous regressions using all four quartiles of ΔHHI , for every worker category we cannot reject the null hypothesis of common wage trends pre-merger, and all of the lag indicators' 95 percent confidence intervals include zero.

¹⁹See online Appendix Section B3 for the estimates for all four quartiles of ΔHHI .

FIGURE 2. LEADS AND LAGS ESTIMATES: TOP QUARTILE OF Δ HHI MERGERS

Notes: 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 of 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.

evidence of differentially worsening economic conditions in the treatment markets (nor do we detect differential pretrends in models with leads and lags).

TABLE 4—LOCAL ECONOMIC CONDITIONS

	Unemployment (1)	(log) population (2)	(log) per capita income (3)
<i>Panel A. Commuting zone economic outcomes</i>			
Post \times fourth quartile Δ HHI	−0.000 (0.003)	−0.004 (0.014)	0.029 (0.019)
Observations	6,130	6,130	6,130
R^2	0.841	0.998	0.975
	Drop 2008–2009 (4)	Drop CZs treated in 2008–2009 (5)	Add further recession controls (6)
<i>Panel B. Great Recession specifications</i>			
Unskilled:			
Post \times fourth quartile Δ HHI	−0.003 (0.015)	0.001 (0.015)	0.001 (0.015)
H_0 : no heterogeneity	0.891	0.706	0.893
Skilled:			
Post \times fourth quartile Δ HHI	−0.042 (0.019)	−0.036 (0.020)	−0.038 (0.020)
H_0 : no heterogeneity	0.071	0.401	0.177
Nursing and pharmacy:			
Post \times fourth quartile Δ HHI	−0.063 (0.019)	−0.077 (0.022)	−0.072 (0.023)
H_0 : no heterogeneity	0.014	0.022	0.013

Notes: The specifications in panel A are estimated at the commuting zone-year level and include commuting zone and year fixed effects. In panel B, each cell is a separate regression. The specifications are as in Table 3 but with further adjustments for the Great Recession: column 4 drops observations in 2008 and 2009, column 5 drops commuting zones treated in 2008 and 2009, and column 6 adds a control for the effect of the Great Recession on housing prices (from Mian and Sufi 2014b) interacted with the year fixed effects.

A related concern is the overlap of our sample with the Great Recession. If the commuting zones experiencing mergers were differentially affected by the Great Recession, the observed wage effects could be attributable to the Great Recession rather than mergers. Panel B of Table 4 shows that our results are robust to a variety of adjustments to examine this possibility. In column 4, we drop the recession years of 2008 and 2009. In column 5, we drop all commuting zones treated during those two years. In column 6, we add a control for changes in housing net worth using the measures from Mian and Sufi (2014a), who show that housing net worth drove employment outcomes during the Great Recession. This control is interacted with the year fixed effects to allow for differential wage trends according to the commuting zone's exposure to the recession. In all cases, we continue to estimate post-merger reductions in wage growth only for the two categories of skilled workers and only following mergers in the top quartile of Δ HHI.²⁰ In sum, we do not find

²⁰The results are similarly robust to controlling for the effects of Chinese import competition, following the basic methodology of Autor, Dorn, and Hanson (2013) using data from UN:Comtrade (2019). See online Appendix Section B3.

evidence that the observed wage effects can be explained by overall local economic conditions.

F. Robustness

Before proceeding to further analysis of mechanisms, we note that the main difference-in-differences results are largely robust to a variety of natural changes to both the data construction and the regression specification. Additional explanation of the following analyses is contained in online Appendix Section B.

First, we check the robustness of our results to two alternative definitions of the control group (see online Appendix Section B4 and Tables B.5 and B.6). The first definition expands the control group by including hospitals in commuting zones that experience only out-of-market mergers: that is, mergers that did not affect labor market concentration. We do not include these markets in our main control group because out-of-market mergers may affect wages through mechanisms other than labor market power (see Section IVA). Expanding the control group to include commuting zones with out-of-market mergers nearly doubles the size of the control group. The second definition draws on the expanded control group to construct a matched control group based on hospital and market characteristics, which permits closer matching between treated and control hospitals on observables. The results using these two alternative control groups are nearly indistinguishable from our main results.

Second, we alter the worker category definitions to examine only the largest line item in each of the unskilled and skilled worker categories (“housekeeping” and “administrative and general,” respectively), and to separate the nursing administration and pharmacy line items (see online Appendix Section B5 and Table B.7). The results are unaffected: we continue to fail to reject zero wage effects for unskilled workers, whereas we find negative wage effects for the other categories of workers when the increase in concentration induced by the merger is sufficiently large.

Third, we alter the geographic unit used for defining labor markets (see online Appendix Section B6 and Table B.8). A narrower definition of geographic markets, core-based statistical areas, yields similar results. A coarser definition of geographic markets, hospital referral regions, yields smaller (but still negative) point estimates in the top quartile of ΔHHI that are no longer statistically significant. Attenuation along these lines is to be expected when the specified geographic market is expanded beyond the true relevant labor market. Thus, these results indicate that the relevant labor market in our context may generally be narrower than the HRR.

Fourth, we expand the sample beyond the 84 commuting zones experiencing a single concentration-increasing merger by adding markets that experienced multiple such events during our sample period (see online Appendix Section B7 and Table B.9). We estimate an alternative specification in which we replace the $\text{post} \times \Delta\text{HHI}$ indicators by the *count* of mergers in each quartile of ΔHHI that occurred within the commuting zone. Doing so nearly doubles the number of in-sample treated commuting zones, at the expense of substantially complicating the model’s identifying variation. We continue to find the largest negative wage effects from mergers in the top quartile of ΔHHI and for the nursing and pharmacy

category, while the effects for the skilled category remain negative and statistically significant but shrink in magnitude.

IV. Labor Market Power versus Other Mechanisms

The analysis in Section III demonstrates that wages for skilled workers and nursing and pharmacy workers grow more slowly following mergers in the top quartile of ΔHHI than in markets without such mergers. This dampened wage growth relative to controls does not occur until after the mergers in question, and moreover, mergers do not appear to coincide with broader changes in the local economic environment that would explain depressed wage growth. Both of these results would be expected if mergers are the true underlying cause of the observed wage effects, as opposed to spuriously capturing the effect of other determinants of wages. However, even if the observed wage effects can be attributed to mergers, as we argue the analysis above shows, that does not necessarily identify *labor market power* as the operative economic mechanism. For example, mergers may bring with them changes in the management or production technology of the affected hospitals, changes which may have a standalone impact on wages irrespective of labor market power. Or, as is well known in the literature, mergers may affect price and quality outcomes in the output market (see, e.g., Focarelli and Panetta 2003, Dafny 2009, Fan 2013, Prince and Simon 2017), which may in turn affect input markets (including the market for labor). Notably, these issues would be present *even if mergers were randomly assigned*, making identification in this setting particularly challenging. Our aim in this section is to explore further the consistency of our main difference-in-differences results with a labor market power mechanism, as opposed to alternative merger-related mechanisms that may affect wages.

Before proceeding to the additional analyses, we note that the totality of results presented above is consistent with a labor market power mechanism. We find wage effects only for workers with the most industry-specific skills, and only then when mergers constitute a substantial increase in employer concentration, as would be predicted by a labor market power theory. If mergers act on wages primarily through changes in management or production technologies, such as more efficient input management or changes to labor composition, then it is not clear what generates the link between the magnitude of the concentration increase and the resulting wage effects.²¹ In this section, we explore this and other alternative explanations for our main findings. Taken as a whole, the evidence presented in this section further bolsters our interpretation of the wage effects following mergers in the top quartile of ΔHHI as being caused by a merger-induced increase in employer labor market power.

A. Out-of-Market Mergers

Perhaps the most immediate threat to the interpretation of the observed wage effects as being due to labor market power is that employer mergers may affect

²¹ Similarly, if wage changes are transmitted to non-merging competitors' wages through mechanisms other than increasing labor market power, the degree of transmission would arguably be a function of employer concentration *levels* rather than concentration *changes*.

wages through alternative channels such as 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. Although 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 affect local labor market concentration. In particular, many hospital mergers are “out-of-market,” in that the merging hospitals are located in different commuting zones and thus have non-overlapping labor markets.²²

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.²³ Examining out-of-market mergers therefore provides an opportunity to rule out labor market power as the dominant mechanism, even if it does not provide an 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 IIIC). 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 (1), 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 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 level at the time of the merger.²⁴ Although 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 either that the results for within-market

²² During our 2000 to 2010 sample period, nearly one-half of all hospital mergers in our data did not involve any commuting zone overlap between the merging parties. Of course, we cannot 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.

²³ A similar approach is used by Focarelli and Panetta (2003) to distinguish between efficiency and market power effects of bank mergers.

²⁴ 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.

TABLE 5—OUT-OF-MARKET MERGERS

	Unskilled (1)	Skilled (2)	Nursing and pharmacy (3)
Post	0.002 (0.008)	−0.010 (0.011)	0.004 (0.008)
Observations	15,402	15,424	15,304
R^2	0.907	0.849	0.875
	Unskilled (4)	Skilled (5)	Nursing and pharmacy (6)
Post × first quartile HHI	0.008 (0.011)	−0.005 (0.014)	−0.005 (0.010)
Post × second quartile HHI	−0.006 (0.010)	−0.017 (0.017)	−0.002 (0.012)
Post × third quartile HHI	−0.010 (0.012)	−0.024 (0.027)	0.029 (0.021)
Post × fourth quartile HHI	0.011 (0.016)	−0.016 (0.028)	0.001 (0.024)
Observations	15,402	15,424	15,304
R^2	0.907	0.849	0.875
H_0 : no heterogeneity	0.566	0.901	0.566

Notes: All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, percent Medicare, percent Medicaid, percent outpatient charges, (log) per capita income, percent unemployment, and percent of the population age 65 or older. 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.

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. The latter could occur if, for example, mergers of nearby hospitals allow for a more efficient allocation of workers across locations whereas out-of-market mergers do not. Although 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.²⁵

B. Non-Wage Compensation

It is well known that hospital mergers can affect output market outcomes, including the prices paid to hospitals by health insurers (Gaynor and Town 2012). As hospital prices rise and drive up employer health care costs, employers may

²⁵ 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 (log) non-wage hospital operating costs: the difference-in-differences estimates are small and statistically insignificant. For example, the estimate following mergers in the top quartile of Δ HHI is 0.021, with a standard error of 0.042.

pass these increased costs through to their workers. The reduced wage growth following hospital mergers in the top quartile of ΔHHI could then, in principle, be offset by faster growth in compensation through health insurance, which we would not observe in the HCRIS data. Although this channel may still be of interest to researchers and policymakers, the driving mechanism in that case is *not* labor market power. Rather, the reduced wage growth is simply an additional consequence of increased hospital market power in the output market.²⁶ The expected direction of this effect is specific to health care employers. In other industries, increasing output market power would not be expected to directly increase health care prices. For example, Qiu and Sojourner (2019) document an overall negative relationship between employer concentration and the generosity of health insurance benefits.

To check whether output market price increases are sufficient to explain our wage results, we turn to CMS's Wage Index Files, which contain information on hospitals' health insurance spending. In 2014, the median hospital's spending on employee health insurance was \$3.43 per worker-hour. Annually, this corresponds to \$7,134 per FTE, which is broadly in line with the average of \$6,130 spent on health insurance for a single worker (without family coverage) in a contemporaneous national survey of large employers (KFF 2014). Consider the output market price increases that would be required to offset the estimated slowdown in wages for nursing and pharmacy workers. In 2014, hourly compensation including health insurance spending is roughly \$42.44 for this worker category.²⁷ Given the estimated 6.8 percent wage effect following mergers in the top quartile of ΔHHI , the increase in health insurance spending required for total compensation to be unchanged is $\$42.44 - \$3.43 - (1 - 0.068) \times \$39.01 = \$2.65$, or $\$2.65/\$3.43 = 77$ percent. Because spending on hospital services accounts for about one-third of total health care spending (CMS 2018), spending on hospital services alone would need to rise by more than 200 percent to fully explain the observed nursing and pharmacy wage effect. Performing the same calculation for skilled workers indicates that hospital prices would need to rise by more than 100 percent. Price increases of this magnitude are much larger than what has been estimated in the literature, even for hospital mergers in already concentrated markets (Dafny 2009, Gaynor and Town 2012, Lewis and Pflum 2017). Therefore, this back-of-the-envelope calculation illustrates that output market effects alone are unlikely to explain the observed post-merger reductions in wage growth for skilled and nursing and pharmacy workers.²⁸

C. Labor Unions

If the wage slowdowns documented in Section IIIC 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

²⁶ We thank an anonymous referee for raising this possibility.

²⁷ The median hourly wage for nursing and pharmacy workers in the HCRIS data in 2014 is \$39.01, so the total wage and health insurance compensation is $\$39.01 + \$3.43 = \$42.44$ (assuming hospitals' spending on employee health insurance does not vary by worker type).

²⁸ Moreover, to the extent that output market price increases generate downward pressure on wages, one might expect that such downward pressure would apply across the full spectrum of workers, not only the skilled and nursing and pharmacy workers for which we estimate suppressed wage growth.

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 (Flood et al. 2018), using respondents whose primary occupation is nursing.²⁹ Of course, unionization rates may be endogenously determined partially as a function of employer labor market power. For our purposes, however, it suffices to check whether union power at the time of the merger affects the subsequent wage trajectory. We incorporate nurse unionization rates into the regression specifications by interacting the post-by- Δ HHI quartile indicators with the nurse unionization rate (and including the unionization rate as a standalone variable). The results are depicted in panel A of Figure 3. In the figure, we evaluate the estimated effect of mergers on wages both at a low level of nurse unionization (the twenty-fifth percentile: 4.1 percent) and a high level of nurse unionization (the seventy-fifth percentile: 15.2 percent). Focusing on mergers in the top quartile of Δ HHI, 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 twenty-fifth percentile of unionization to the seventy-fifth percentile eliminates about 25 percent of the post-merger wage effect. The distribution of nurse unionization rates is fairly skewed. A larger movement from the fifth percentile of unionization (1.5 percent) to the ninety-fifth percentile (40.8 percent) is estimated to eliminate about 80 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.

Panel B 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). There is little within-state variation in right-to-work laws during our sample period, so this figure primarily exploits across-state variation interacted with the post-by- Δ HHI quartile indicators. 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 Δ HHI, the estimated post-merger reductions in wage growth appear only in right-to-work states. We view these results as bolstering the interpretation of the merger effects in Section IIIC as consequences of increased labor market power: power that can potentially be mitigated by strong labor unions.

²⁹ Although ideally we would 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).

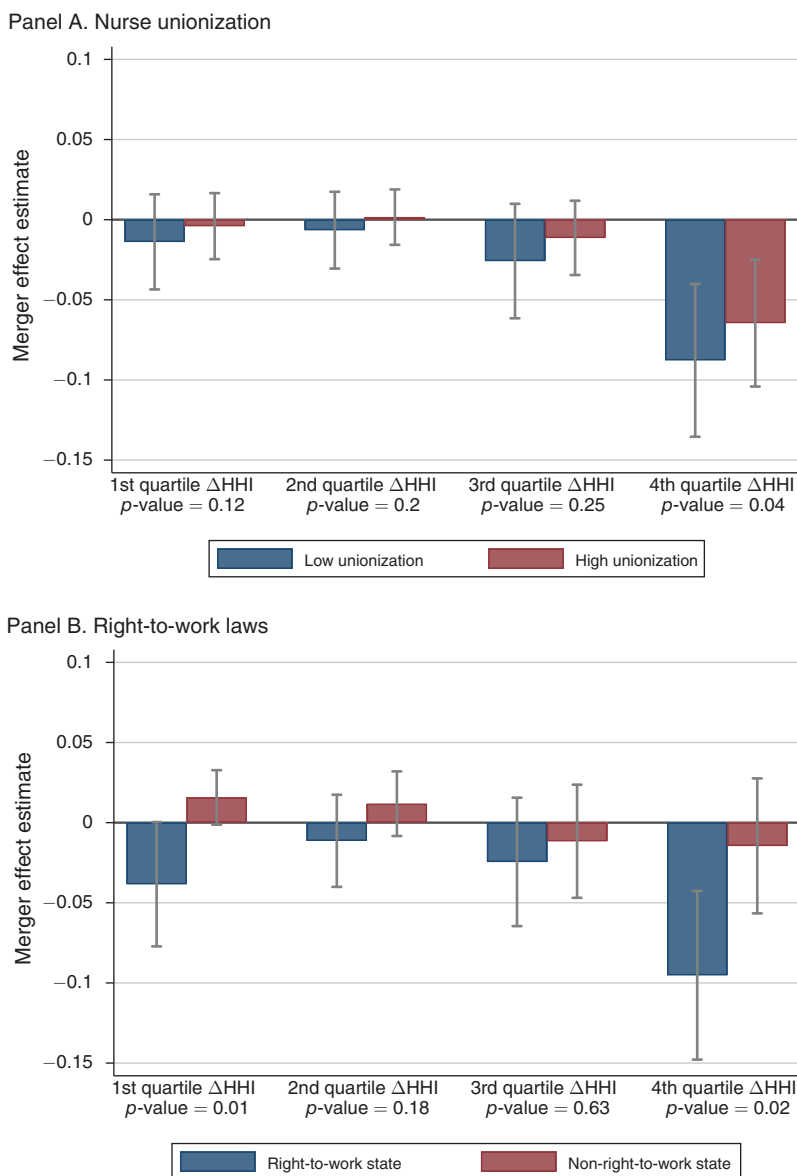


FIGURE 3. WAGE EFFECTS AND LABOR UNIONS

Notes: 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.

D. Labor Quantity and Composition

Labor Quantity.—A pattern of slowing wage growth following employer consolidation is consistent with classical monopsony as well as other explanations for employer market power. A classical monopsonist employer drives wages below the marginal product of labor by restricting employment below its competitive equilibrium level (Robinson 1969), in which case reductions in wages are associated

TABLE 6—LABOR QUANTITY AND COMPOSITION

	Unskilled (1)	Skilled (2)	Nursing and pharmacy (3)
<i>Panel A. Labor quantity (log FTEs)</i>			
Post \times first quartile Δ HHI	−0.006 (0.021)	0.024 (0.025)	−0.033 (0.030)
Post \times second quartile Δ HHI	−0.011 (0.032)	0.060 (0.036)	−0.081 (0.056)
Post \times third quartile Δ HHI	−0.002 (0.022)	−0.020 (0.055)	0.078 (0.061)
Post \times fourth quartile Δ HHI	0.045 (0.051)	−0.046 (0.075)	0.187 (0.081)
Observations	18,079	18,067	17,885
R^2	0.959	0.913	0.923
	(log) RN FTEs (4)	(log) LPN FTEs (5)	LPN Share (6)
<i>Panel B. Labor composition (nursing)</i>			
Post \times first quartile Δ HHI	0.007 (0.015)	−0.148 (0.062)	0.001 (0.003)
Post \times second quartile Δ HHI	−0.001 (0.022)	0.038 (0.052)	−0.001 (0.004)
Post \times third quartile Δ HHI	0.020 (0.041)	0.009 (0.079)	−0.005 (0.005)
Post \times fourth quartile Δ HHI	0.074 (0.065)	0.042 (0.133)	−0.005 (0.007)
Observations	17,575	17,299	17,576
R^2	0.975	0.819	0.854

Notes: All specifications include hospital and year fixed effects, plus the controls (log) one-bedroom rent, (log) population, (log) beds, (log) case mix index, percent Medicare, percent Medicaid, percent outpatient charges, (log) per capita income, percent unemployment, and percent of the population age 65 or older. Standard errors are clustered by hospital and observations are weighted by total inpatient discharges.

with corresponding reductions in labor quantity. By contrast, other sources of employer market power do not necessarily require employment suppression in order to explain wage reductions. In models with search frictions, for example, a large employer that can eliminate its own vacancies from its incumbent workers' outside option can place downward pressure on wages without reducing employment (Jarosch, Nimczik, and Sorkin 2019). Similarly, an employer that accounts for a large fraction of vacancies can reduce the expected benefits of a job search, giving employers more bargaining leverage in negotiations with their employees (by worsening the value of employees' outside option), driving down wages but not necessarily employment. Although any of these sources of employer market power can explain an upward-sloping labor supply curve facing individual employers, an observed wage reduction without a corresponding employment reduction would be inconsistent with classical monopsony operating on its own.

To check the consistency of our main results with classical monopsony as opposed to mechanisms that do not require employment reductions, we test for changes in labor quantity following mergers in the top quartile of Δ HHI. Panel A of Table 6 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 does not appear to be consistent with a classical monopsony mechanism. However, unlike the other outcomes we have examined, there is evidence of differential pretrends 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 pretrends makes us reluctant to interpret the estimated faster employment growth too strongly. After controlling for the differential pretrends with a linear time trend, the estimated employment effect becomes small and statistically insignificant, although it is not a precisely estimated zero.³⁰ Regardless, nothing in the estimates suggests *reductions* in employment growth following mergers, which would be expected to arise if classical monopsony were the sole mechanism.³¹

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 salary of approximately 1.5 times that of LPNs (BLS 2018). 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 B of Table 6). Although 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.

V. 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

³⁰The 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 pretrends for wages.

³¹We 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 0.035 (with a standard error of 0.034) for (log) inpatient discharges and 0.031 (with a standard error of 0.035) for (log) adjusted discharges.

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.

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. 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.

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