MONOPSONY IN THE LOW-WAGE LABOR MARKET? EVIDENCE FROM MINIMUM NURSE STAFFING REGULATIONS

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Abstract—This paper provides direct evidence on the extent of monopsony power in the low-wage labor market by estimating the firm-level elasticity of labor supply for nurse aides in the long-term care (nursing home) industry. Using exogenous variation in hiring induced by the passage of a state minimum nurse staffing law, I find that facilities initially out of compliance with the new law did not have to raise their wage offers relative to their competitors in order to hire more nurses. While this is consistent with perfect competition in simple monopsony models of the labor market, I discuss how the results may be more ambiguous in more complicated models.

I. Introduction

LABOR market models that assume employers have market power in wage setting have received renewed interest in economics (Ashenfelter, Farber, & Ransom, 2010; Manning, 2003). The conceptual underpinnings of such models stem from Joan Robinson (1933), who illustrated that under monopsony (or oligopsony)—situations where few firms control most of the employment opportunities in a market—workers may be paid wages below their marginal revenue products. Perhaps because Bunting (1962) showed that big firms rarely employ a large fraction of the workers in a local market, models of imperfectly competitive labor markets have been relegated to studies of rarified occupations such as coal miners, school teachers, and registered nurses, where employment concentrations are high.[1] But more recent theoretical work has shown that even in markets with an arbitrarily large number of employers, commonplace phenomena such as firm differentiation in the eyes of workers and imperfect information can generate monopsony-like dynamics (Burdett & Mortensen, 1998; Bhaskar & To, 1999; Manning, 2006).[2]

The empirical relevance of these new monopsony models can have important implications if monopsony leads to large deviations between wages and the value of marginal products for a large set of workers. Monopsony may shed light, for example, on the causes of the nursing shortage, the sources of race and gender pay gaps, the reasons for employer provision of general training, and other areas of similar public policy interest.[3] These models, should they apply even to low-wage occupations such as fast food workers, may also explain why several studies of the impact of minimum wage increases find no evidence of adverse employment effects (Card & Krueger, 1995).

Despite these stakes, economists have not reached consensus on the relevance of monopsony in modern labor markets.[4] This lack of consensus stems in no small part from the difficulty of generating direct empirical evidence on the extent of firms' market power in hiring labor. In a simple model, a monopsonistic firm hires workers to maximize profits $\pi(L) = R(L) - w(L)L$, where L is the amount of labor hired, $R(L)$ is a revenue function, and $w(L)$ is the labor supply function to the firm. The first-order conditions imply that an employer's market power, as measured by the relative gap between the wage and the worker's marginal revenue product (MRP), is proportional to the (inverse) elasticity of the labor supply to his or her firm, $\frac{MRP-w}{w} = \frac{\partial w}{w}\frac{L}{\partial L} \equiv \epsilon$. In this model and others, then, ϵ is the key parameter dictating the extent of monopsony power for firms. But since wages and employment levels are simultaneously determined, the elasticity of labor supply cannot be consistently estimated without a valid instrument for either firm-specific wages or employment. Such instruments rarely present themselves, however, and only a handful of studies attempt to estimate the elasticity of labor supply at the firm level using plausibly exogenous variation in prices or quantities.

In this paper, I exploit quasi-experimental changes in the quantity of nurses hired at different organizations to generate credible estimates of the degree of monopsony power in this labor market. The variation in nurse employment is generated by a 1999 policy change in California requiring that all long-term care (nursing home) facilities in the state employ a minimum number of nursing hours for each resident, each day. As a result of the law, all long-term care facilities have to maintain at least 3.2 hours of nursing staff per resident per day (hereafter, HPRD for hours per resident-day) and are subject to a range of penalties for noncompliance. Firms that were below the 3.2 HPRD standard before the law passed faced pressure to increase their staff in proportion to their distance from the newly established staffing requirements. The crux of the research design is to compare firms that had initial staffing levels below 3.2 HPRD to those that were already in compliance with the staffing law when it passed. I show that facilities that were below the staffing threshold hired

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[1] For example, see Boal (1995), Luizer and Thornton (1986), and Sullivan (1989).

[2] These insights were present, if not formalized, in Robinson (1933).

[3] See Manning (2003) for a comprehensive review of how a monopsony perspective leads to new interpretations of many traditional topics in labor economics.

[4] For example, see Kuhn (2004) for a thoughtful critique of the relevance of monopsony models.

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significantly more workers to comply with the law, whereas those that were already in compliance did relatively little additional hiring.

Having established that the law generated plausibly exogenous changes in employment, I test for monopsony by assessing whether changes in wages across facilities mirror those in employment. In a perfectly competitive market with an infinitely elastic firm-level labor supply curve, facilities hiring more nurses should not need to raise their wages relative to their competitors in order to increase their staffing level. If the firm-level (inverse) labor supply curve is upward sloping, however, then the facilities trying to comply with the minimum staffing mandate will need to raise their relative wage offers in order to increase their workforce. To the extent that initial staffing levels are uncorrelated with changes in other factors affecting labor supply, a condition that can be partially verified in the data, this approach will yield consistent estimates of the inverse elasticity of labor supply and offer a direct test of monopsony. The research design here is highly transparent, lending itself to credible identification of ϵ through both graphical analysis and simple instrumental variables regression.

I find that the minimum staffing legislation was quite effective in increasing staffing levels in long-term care facilities that had been below the 3.2 HPRD threshold prior to passage of the law. For nursing homes that had been in the lowest quintile of HPRD in 1999, overall hours increased by about 30% between 1999 and 2003. The increase, however, was not uniform across nursing occupations. The law had its greatest impact on the number of nurse aides—the nurse occupation requiring the least training and accordingly the lowest paid—hired in facilities with low initial staffing levels. If the labor market for nurses is monopsonistic, these large increases in staffing should have been accompanied by increased wages. However, I find no evidence of a causal impact of the legislation on wage levels for any category of nursing labor. The inverse elasticity estimates are close to 0 for nurse aides, suggesting that perfect competition may be the best model for the labor market for nurse aides. These results add interesting texture to a growing body of research on the empirical relevance of monopsony, particularly in low-wage markets.

The remainder of the paper is organized as follows. The next section reviews some recent work on monopsony. Section III describes my research design and methods, section IV describes the data, and section V presents the main empirical results. Section VI discusses the results and concludes.

II. Background and Previous Research

While the early literature on monopsony viewed employment concentration as its main source, more recent work has focused on a variety of phenomena that might give rise to an upward-sloping labor supply curve to individual firms.[5] While this shift in attention is new, the ideas are not. In *The Economics of Imperfect Competition* (1933), Joan Robinson noted that monopsony-like conditions could arise in a labor market without large firms if the labor supply curve was upward sloping. This could happen because “there may be a certain number of workers in the immediate neighbourhood and to attract those from further afield it may be necessary to pay a wage equal to what they can earn near home *plus* their fares to and fro; or there may be workers attached to the firm by preference or custom and to attract others it may be necessary to pay a higher wage. Or ignorance may prevent workers from moving from one firm to another in response to differences in the wages offered by the different firms” (Robinson, 1933, p. 296). More recent work has formalized these insights (Bhaskar & To, 1999; Staiger et al., 2010; Card & Krueger, 1995; Manning, 2003, 2006).

Only a handful of studies have attempted to estimate the elasticity of labor supply to the firm using a research design that addresses the simultaneous determination of wages and employment. Sullivan (1989) used length of stay and caseload variables to instrument for the number of licensed vocational and registered nurses hired by a hospital and estimated ϵ at about about .79 (with standard error of .13) over a one-year period and .26 (.07) over a three-year period.[6] Staiger et al. (2010) use legislated wage changes in Veterans Affairs (VA) hospitals to identify the firm-level labor supply elasticity of RNs.[7] Using gaps between the newly legislated RN wage and wages at the time of the legislation as instruments for wage changes, the estimated labor supply elasticity over a two-year period ranges from near 0 to 0.2 (0.13), implying an inverse elasticity far from the 0 assumed by a model of perfectly competitive labor markets.

Several other studies have found indirect evidence for monopsony, and this evidence suggests that the phenomenon may not be limited to a subset of skilled workers in industries where employment tends to be concentrated in large employers. The highest profile among these studies is surely Card and Krueger’s (1995) survey of research showing negligible or even positive employment effects of the minimum

[5] Studies such as Hurd (1973), Link and Landon (1975), and Feldman and Scheffler (1982) claimed to find evidence of monopsony by documenting a negative correlation between employment concentration and the wages of registered nurses (RNs) in cross-sections of metropolitan areas or cities in the United States. Subsequent studies by Adamache and Sloan (1982) and Hirsch and Schumacher (1995) found that such correlations were not robust, however, to controls for cross-market differences in population density or wages in other occupations.

[6] In a static model, these elasticities can be used to compute the “markup” of marginal product over wages using equation (1). In this case, the estimates would imply wages are between 43% (for one-year changes) and 21% (for three-year changes) below marginal product. In a dynamic setting, however, this “rate of exploitation” is a weighted average of short- and long-run elasticities where the weights are a function of a firms’ discount rate. Assuming a long-run elasticity of 0, Boal and Ransom (1997) suggest Sullivan’s estimates imply that wages might be set between 87% and 96% of marginal product. Using this logic, they characterize Sullivan’s results as being suggestive of only slight market power for hospitals.

[7] Falch (2010) uses a similar research design applied to school teachers in Norway.

wage. Card and Krueger suggest that monopsony models are better able to reconcile the results of minimum wage studies than perfect competition models and suggest as well that monopsony models based on search costs might best describe the labor market dynamics of low-skilled workers in the fast food industry.[8] This claim remains controversial, as many economists dismiss out of hand the notion that important frictions exist for low-skilled workers.

This paper makes several contributions to the literature. First, the study sheds light on the degree of monopsony power in the low-wage segment of the labor market, a subject that may hold the key to understanding the employment effects of raising the minimum wage and cannot be addressed by studying teachers or registered nurses, relatively high-wage occupations. Second, this study estimates labor supply elasticities using exogenous variation in nurse employment generated by a minimum quantity constraint. Although research designs based on legislated wage changes can convincingly solve the endogeneity problem, they may suffer from a distinct drawback. Under monopsony, the employment response to an exogenous wage increase is potentially nonlinear: for small wage increases, employment expands along the (upward-sloping) labor supply curve, but for large enough increases, employment will eventually begin to contract along the labor demand curve (Brown, Gilroy, & Kohen, 1982). Thus, unless firms remain supply constrained over the full increase in wages, the change in observed employment will not identify the slope of the labor supply curve. Since this condition is hard to verify, studies that show wage increases resulting in little employment change may be consistent with either an inelastic labor supply response or with offsetting adjustments along relatively elastic labor supply and demand curves.[9] By relying on a minimum quantity constraint for identification, I am able to avoid speculation about whether individual firms are supply constrained over the relevant range of changes in prices and quantities.

III. Minimum Staffing Legislation and Research Design

On July 22, 1999, California Governor Gray Davis signed Assembly Bill (AB) 1107, a law that responded to concerns over the quality of care in nursing homes by establishing minimum nurse staffing levels for all California nursing homes effective on January 1, 2000.[10] AB 1107 required that all skilled nursing and intermediate-care facilities provide a minimum of 3.2 nursing hours per resident-day and established a range of penalties for noncompliance. The law was silent on the skill mix of this care, so firms could comply by increasing their total hours worked by the combination of three types of nurses: registered nurses (RNs—the most skilled), licensed vocational nurses (LVNs), or nurse aides (NAs—the least skilled).

Theoretical models of monopsony suggest a general model for labor supply to the individual firm can be written as $w_i = f(n_i, X_i, X_i^*)$, where w_i is a firm's wage, n_i is its employment of nurses, X_i is a vector of firm and local market characteristics, and X_i^* represents the actions (for example, wages or employment levels) of competitor firms. In what follows, I remain agnostic about which source of monopsony is most important in the labor market for nursing home, and focus on estimating the firm-level labor supply elasticity that is central to wage dynamics in all models.[11] I adopt a simple reduced-form model of labor supply to the firm for a particular type of labor:

$$w_{irt} = \beta_0 + \beta_1 n_{irt} + \alpha_i + \theta_{rt} + \epsilon_{irt}, \tag{1}$$

where w_{irt} represents real hourly wages for workers in a particular occupation at facility i in region (regions are usually counties, see note 17 for a detailed description) r and year t, n_{irt} represents total hours worked, and ϵ_{irt} represents other supply and demand shocks affecting wages and employment. The equation will be estimated separately for RNs, LVNs, nurses' aides, and other nonnursing labor groups. α_i represents fixed firm and labor market characteristics affecting the desirability of employment such as like proximity to population centers. These effects are controlled for by estimating equation (1) in differences of different lengths (one to four years), which also allows me to estimate the elasticity of supply in both the short and slightly more medium run. It would seem natural, as Sullivan (1989) finds, that the labor supply curve becomes more elastic over longer windows as more adjustment can occur. Finally, θ_{rt} represents a region- (county-) specific time effect that is meant to absorb changes in local labor market conditions such as wages at competitor firms. This will approximately control for changes in X^* to the extent that firms are small relative to the market

[8] Card and Krueger note, however, that monopsony models are not consistent with all the evidence on the impacts of minimum wages, noting that output prices do not respond as predicted by monopsony theory. Aaronson and French (2007) examine product market responses to minimum wage increases separately and find responses consistent with a competitive model of the labor market. In the market for nurses, Hirsch and Schumacher (2005) find no correlation between wages and the fraction of new recruits from nonemployment—a proxy for monopsony power in some models.

[9] Staiger et al. (2010) cite reports of nationwide nursing shortages to argue that hospitals are likely to be supply constrained, and Falch (2010) observes a measure of excess supply at the firm level, which he uses to limit his analysis to firms that appear to be supply constrained.

[10] Facilities were notified, however, that enforcement of the new standard would begin in April 2000. A separate law, AB 394, addressed hospitals but was not implemented until 2004.

[11] The source of monopsony power may have implications for specifying the labor supply equation. For example, if the dominant forces in wage determination arise from oligopsonistic competition for workers among differentiated firms, then understanding which firms compete with each other and the nature of this competition is important. On the other hand, if search frictions are the prime cause of monopsony power, then worrying about the actions of other firms may be less important. There is, however, scant evidence on this. Sullivan (1989) finds little impact of competitors' actions on wages in the short run, but some impact over a three-year period. Staiger et al.'s (2010) estimates of elasticities do not seem heavily influenced by the inclusion of competitors' wages.

(so X^* is nearly the same for all firms within a market).[12] With data differenced over d years and region-specific time trends, equation (1) becomes

$$\Delta^d w_{ir} = \beta_1^d \Delta^d n_{ir} + \theta_r^d + \Delta^d \epsilon_{irt}. \quad (2)$$

In equation (2), β_1^d represents the inverse elasticity of supply (over d years), and a test of $\beta_1^d = 0$ amounts to a test of the null hypothesis of a perfectly competitive labor market. The primary empirical challenge involved in consistently estimating β_1^d is that $\Delta^d \epsilon_{ir}$ may be correlated with $\Delta^d n_{ir}$. In particular, if the firm-specific demand-side shocks leading to employment changes are correlated with supply-side shocks, then estimates of the inverse elasticity will be inconsistent.[13]

To address the potential simultaneity of employment changes, I exploit changes in the employment of nurses induced by the introduction of the minimum staffing standard. I define a variable GAP_i equal to the absolute value of the difference between facility i's staffing level in HPRD before the legislation went into effect and the 3.2 threshold if the firm is below the threshold, and equal to 0 if the firm is above the threshold. GAP_i thus represents the quantity of nurse labor a firm would need to hire in order to be in compliance with the law. To mitigate bias caused by mean reversion, I use an average of 1997 and 1998 staffing levels as an estimate of each facility's preperiod staffing level. I then estimate equation (2) with two-stage least squares, using GAP_i as an instrument for $\Delta^d n_{ir}$. So long as the (within-county) unobserved determinants of wages are not changing in a way that is correlated with a firm's average 1997–1998 staffing level, then this approach should allow identification of β_1^d. This identification condition is partially testable, and I present several pieces of evidence that it appears satisfied in this setting. In most of the analyses that follow, I present estimates from a more flexible model that allows changes in the outcomes to depend on a linear function of the preperiod staffing level, as well as GAP. In this case, the coefficient on GAP is identified by the difference in this relationship (between staffing changes and initial levels) for facilities below the threshold (affected by the law) and those above the threshold.

IV. Data and Summary Statistics

All data used in the paper come from the Long-Term Care Facilities Annual Financial Data collected by California's Office of Statewide Health Planning and Development (OSHPD). These data derive from disclosure reports submitted by long-term care facilities and audited by staff at OSHPD as part of a certification process for nursing homes to be eligible to receive payments from Medicaid and Medicare. I start with the 1,223 nursing homes that submitted data at some point between 1995 and 2004 that were licensed as skilled nursing facilities and were privately run—by investors (87%), nonprofits (8%), or religious organizations (5%). When multiple reports exist for the same facility in a given year, I use the single report accounting for the most days in that year. I extract the 1,090 firms that report staffing data in each year between 1995 and 1999 and drop 4 firms that have extraordinarily high staffing levels (average HPRD over 7) for the full period. Finally, I use the subset of 1,031 of these facilities that report staffing data in every year from 1999 through 2003 to balance the panel. This eliminates about 5% of facilities relative to those present before passage of the law, but is unlikely to induce survivorship biases in what follows as this attrition is unrelated to prereform staffing levels.[14] Table 1 presents basic descriptive statistics (as of 1999) for the 1,031 facilities used in the analyses, broken down by quartiles of the average 1997–1998 staffing distribution.

The OSHPD data report total hours worked and total wages and salary paid (excluding benefits) for four types of nurses—managers, registered nurses, licensed vocational nurses, and nurse aides—and many other nonnursing occupations, such as housekeepers and dietitians. I compute average hourly wages by facility and occupation by dividing total wages and salary by total hours worked. For nurses, there is a clear hierarchy across occupations, with supervisors earning the most, at an average of \$31.78 per hour (excluding benefits) in 1999, RNs next at \$23.84, followed by LVNs at \$18.29, and nurse aides at \$9.50. These wage differentials reflect differences in preparation and training requirements for each occupation. RNs are typically required to complete between two and six years of postsecondary education, whereas LVNs typically require only one year. Nurse aides require no formal training to be hired, but must complete 100 hours on the job and 50 hours of classroom training to be certified and must pass a state medical exam within four months of being hired. Unfortunately, the OSHPD data do not contain much useful information about the composition of workers within-occupations, so changes in average wages at a firm may represent a combination of within-occupation changes, for example, in average facility tenure, and wages paid for a given quality of worker. That said, the high turnover rates for nurse aides shown in table 1 suggest that most may be making close to starting wages, and so compositional changes may not be influencing the results presented below.

V. Results

In Manning's text on monopsony Manning (2003), he suggests several empirical regularities found in labor markets that may be more easily reconciled with monopsony models than with perfect competition. Before turning to the main analyses, it is provocative to note that at least two of these

[12] For nurse aides, the set of likely alternative employers probably includes many firms employing low-skilled labor outside the health care industry, and viewing a particular nursing home as atomistic within its county may be quite reasonable. Including explicit controls for geographically proximate competitors, wages might be more important for labor groups with high occupation-specific human capital such as RNs or LVNs, but previous work has provided mixed evidence on this point.

[13] See Manning (2003) for an illustration in a simple model.

[14] A chi square test of the null that the attrition rate from 1999 to 2003 is equal across ten deciles of the preform staffing (HPRD) distribution (described below) fails to reject (p-value = 0.602).

Table 1.—1999 Descriptive Statistics for Long-Term Care Facilities in Analysis Sample by Staffing Level

		Quartiles of 1997–1998 HPRD Distribution			
	All	First	Second	Third	Fourth
Number of beds	101.3	93.3	103.1	106.6	102.3
	[48.9]	[40.7]	[43.4]	[52.2]	[56.7]
Average employees	100.4	81.5	96.5	104.5	119.4
	[47.3]	[33.2]	[38.0]	[46.2]	[59.4]
Total care health revenues ($1,000s)	3,812.7	3,137.8	4,003.0	3,999.0	4,112.5
	[2,092.2]	[1,656.0]	[1,877.5]	[2,067.4]	[2,529.7]
Average occupancy	87.8	89.3	87.7	86.9	87.2
% patient-days paid by Medical	67.9	77.8	72.8	65.9	51.8
% patient-days self-paid	24.9	13.3	18.3	26.6	41.6
Average direct care nurses	60.8	49.9	58.6	64.3	70.4
Average number of nurse aides	40.5	33.8	39.8	43.6	44.8
Total number of nurse aides (NAs)	63.8	50.9	60.0	66.3	78.2
NAs employed continuously 1 year	24.0	19.7	22.8	25.8	27.9

There are 1,031 firms overall, with 258 or 257 in each of the quartiles based on the average 1997–1998 staffing (HPRD) level. All firms in the fourth quartile are in compliance with the 3.2 HPRD threshold taking effect in 2000. Sixteen percent of facilities in the third quartile and none in the lower two quartiles are in compliance. Standard deviations of selected variables are in brackets.

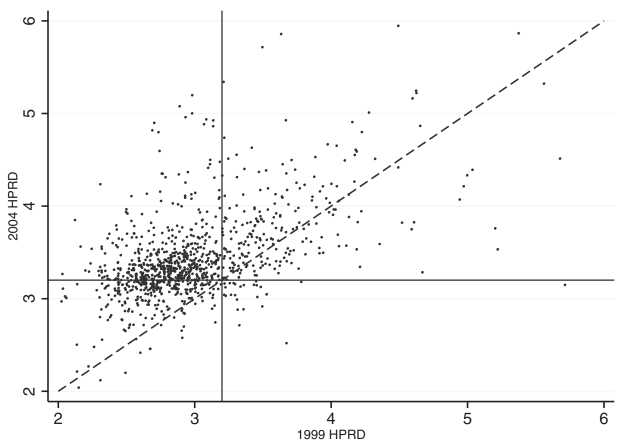
phenomena—an employer-size wage effect and wage dispersion for similar workers—are present in the nursing home labor market. First, there is a positive correlation between facility size and average nurse wages, even when estimated with firm fixed effects. In a regression of the log of average wages on the log of the number of total employees and a full set of facility fixed effects using a panel of facilities covering 1995 to 2004, the coefficient on the log of total employment is .127 (.0084) for all nurses' wages and .176 (.0099) for nurse aides' wages. Moreover, there is substantial heterogeneity in the average wages of nurses across different facilities, even within the same geographic county. For example, the R^2 on a regression of firm-level average wages on a set of 31 county dummies (see note 17) is only 0.16 for registered nurses and 0.56 for nurse aides, suggesting that the law of one price may not hold in these markets. Though neither of these facts is inconsistent with a perfectly competitive model, Manning (2003) suggests that each might be taken as prima facie evidence of an upward-sloping labor supply curve and imperfectly competitive labor market.

I document the effects of the 2000 minimum staffing legislation for nursing homes in California. I first explore the effects on nurse staffing levels and composition at nursing homes and then investigate whether these effects were accompanied by wage effects that may be consistent with monopsony. In the final subsection, I present estimates of the elasticity of labor supply for each type of nursing labor. In each section, I present nonparametric analyses of the raw data that illustrate the basic findings, and then parametric estimates.

A. *Effects of Legislation on Staffing Levels in Nursing Homes*

In each year before passage of the staffing law from 1995 to 1999, about 25% of nursing homes employed more nurses than the 3.2 HPRD threshold. This fraction began to rise immediately following passage of the law, to about 50% in 2001 and about 70% by 2004. While this break in the overall time series is suggestive of a causal effect of the law's passage

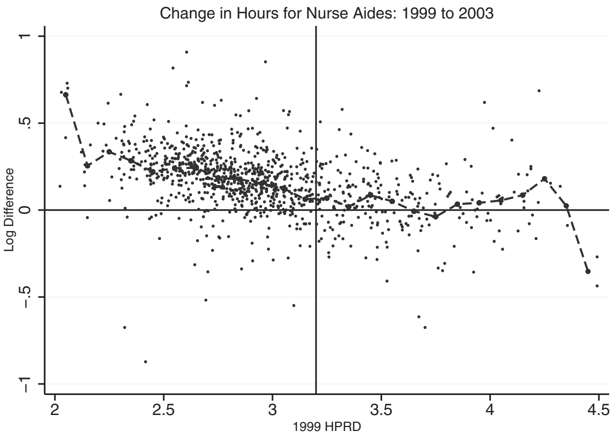
Figure 1.—Changes in Total Nursing Hours per Patient-Day by Initial Staffing Levels, 1999–2004



Each dot represents one organization and indicates its 1999 total employment of nurses in hours per resident-day (HPRD) on the *x*-axis, and 2004 value of HPRD on the *y*-axis. The dashed 45-degree line represents no change in total nurse employment between 1999 and 2004.

on staffing levels, we can rule out the possibility that other forces occurring contemporaneously drove up staffing levels by relating the magnitude of staffing increases to firms' initial staffing levels. In figure 1, I plot the firms' HPRD staffing levels in 1999 on the *x*-axis against their staffing level in 2004 on the *y*-axis, with each dot showing changes in staffing levels for one firm in the data. With the introduction of the minimum staffing legislation, organizations that were below the standard in the preperiod (to the left of the vertical line in the figure) needed to increase their staffing level in order to comply. Those above the standard faced no such pressure. The figure shows that organizations respond accordingly: about 95% below the standard raised their staffing levels, shown by the fact that nearly all dots to the left of the vertical line are above the 45 degree line, with average deviations increasing for organizations with lower initial staffing levels. In contrast, only about 58% that were above the 3.2 threshold in the preperiod raised their staffing levels. A similar analysis plotting changes from 1995 to 1999 shows most organizations

FIGURE 2.—FOUR-YEAR CHANGES IN LOG ANNUAL HOURS OF NURSE AIDES AFTER MINIMUM STAFFING LEGISLATION ENACTED IN 1999



Each dot represents data for one facility. The dashed line connects local averages of the employment changes for organizations in .10-wide HPRD bins. Facilities with extreme values of HPRD in 1999 have been trimmed for presentation purposes.

TABLE 2.—EFFECT OF MINIMUM STAFFING LEGISLATION ON EMPLOYMENT OF NURSE AIDES (LOG OF TOTAL HOURS), THREE YEARS POSTPOLICY

	1	2	3	4
$GAP_{\overline{9798}}$	0.2314	0.2245	0.2176	0.1501
	(0.0252)	(0.0242)	(0.0249)	(0.0429)
$HPRD_{\overline{9798}}-3.2$				−0.0421
				(0.0232)
Constant	0.0820	0.0862	NA	NA
	(0.0110)	(0.0102)	(—)	(—)
County fixed effects	No	No	Yes	Yes
Outliers removed	No	Yes	Yes	Yes
R^2	0.0767	0.0804	0.1118	0.1169
Facilities (N)	1,031	1,030	1,030	1,030
Partial F	84.60	86.11	78.61	12.65

The dependent variable across all columns is the difference in the log of average total annual hours worked by nurse aides, 2003–1999. Columns 2–4 omit one nursing home that experiences more than a 1.5 log point change in nurse aide staffing. Robust standard errors are in parentheses.

clustered around the 45 degree line, supporting a key identification condition of the research design: in a period with no policy change, the trends (changes) in staffing are not correlated with initial staffing levels (or more specifically, with *GAP*).

The new staffing regulations required that the sum of hours worked by all nurse occupations per resident exceeded 3.2 HPRD. With no further requirements placed on the skill mix of their staffing, we might expect that facilities would comply with the law primarily by hiring the least costly type of nurse.[15] Figure 2 confirms this prediction, showing the employment effects of the minimum staffing law for nurse aides over a four-year period (1999–2003). The dots in the figure represent each organization in the sample, with the change in log hours plotted on the y-axis and the facility's initial 1999 HPRD staffing level on the x-axis. The dashed line connects local averages of the employment changes for facilities grouped into bins .1 HPRD wide (for example, the value immediately to the right of the vertical line at 3.2 is the average change in employment for all firms with 1999 HPRD between 3.2 and 3.3). The figure shows that facilities above the threshold saw little change in NA hours worked on average over this four-year time span. The most striking aspect of the figure, however, is a large increase in hours worked by NAs that is strongly related to how far below the staffing threshold a firm was before the law passed. For facilities in the lowest quintile of 1999 HPRD staffing levels, nurse aide hours increased by more than 20% on average. In light of the fact that nurse aides already comprise two-thirds or more of all hours of care at these facilities, this represents a significant increase in the overall number of hours of care.

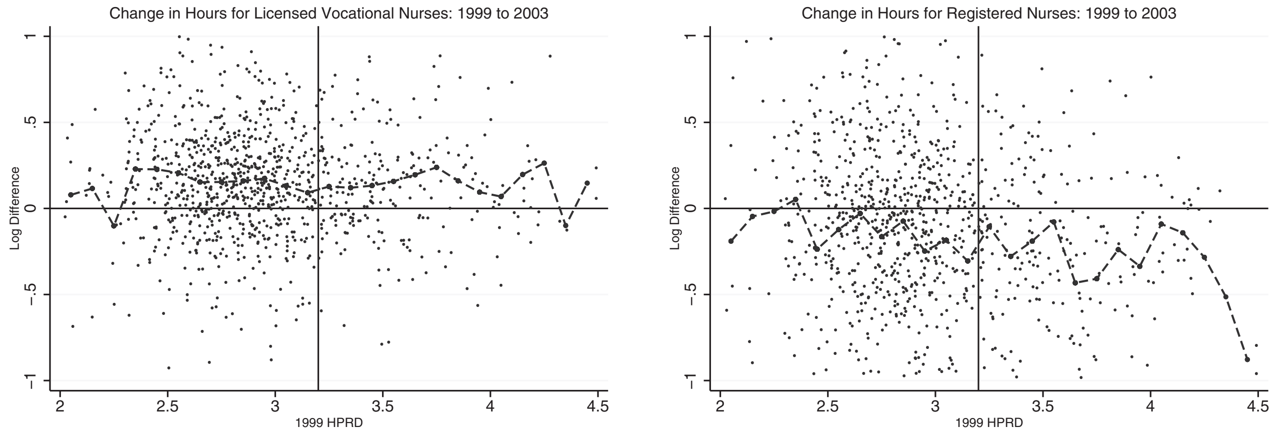
This visual evidence is borne out by regression results presented in table 2. Different columns of the table present different estimates of the effect of the minimum staffing law on changes in the log of total nurse aide hours employed from 1999 to 2003, four years after the legislation went into effect. Column 1 regresses this change in employment for nurse aides on GAP and a constant term, essentially restricting employment growth to be the same for all facilities that already had staffing levels above the 3.2 threshold. Since the average facility that was out of compliance with the law in the preperiod had a staffing level of about 2.7 HPRD ($GAP = .5$), the coefficient estimates on *GAP* in column 1 imply that the legislation caused nurse aide employment to grow by about 12% ($.5 \times .23 = .115$ log points) for the typical facility initially out of compliance. In columns 2 to 4, I omit one outlier facility with an extremely large—over 1.5 log points—change in nurse aide staffing. Column 2 shows this restriction does not affect the coefficient estimates in the baseline specification,[16], and column 3 shows that adding 31 county fixed effects to the model also results in a nearly identical estimated coefficient on *GAP*.[17] Finally, the specification in column 4 allows there to be a linear relationship between preperiod staffing levels and subsequent employment growth and identifies the causal effect of the law by the difference in this relationship for facilities that were above and below the 3.2 threshold before the law passed. This more flexible specification results in a slightly lower estimate of the causal effect—the coefficient falls to 0.150 (SE, .043)—but it remains large and strongly significant. Importantly for the instrumental variables estimates of ϵ reported below, *GAP* is a significant predictor of hours changes for nurse aides and the

[15] Federal requirements enacted in 1987 require nursing homes to have a minimum number of registered and licensed nurses on duty at all times.

[16] Eliminating outliers does not markedly affect the NA estimates in columns 3 and 4 either: estimates are within .01 if the outlier is included. Only the results for RNs (shown in table 4) in column 4 are substantially affected by the treatment of outliers, but the estimates are sufficiently imprecise that the substantive conclusions remain the same.

[17] I have one dummy for every county in California with at least one nursing home reporting data, with the exception that the following sets of contiguous counties with few observations each are grouped together into eleven county groups: Butte, Plumas, and Tehama; Mendocino, Colusa, Del Norte, Glenn, Humboldt, Lake, and Trinity; Monterey and San Benito; Placer, El Dorado, Nevada, Sierra, and Yuba; San Bernardino, Inyo, and Mono; San Diego and Imperial; San Joaquin, Alpine, Amador, and Calaveras; Shasta, Lassen, Modoc, and Siskiyou; Stanislaus and Tuolumne; Tulare and Kings; and Yolo and Sutter.

FIGURE 3.—FOUR-YEAR CHANGES IN LOG ANNUAL HOURS AFTER MINIMUM STAFFING LEGISLATION ENACTED IN 1999 FOR LICENSED VOCATIONAL AND REGISTERED NURSES



Each dot represents data for one facility. The dashed line connects local averages of the employment changes for firms in .10-wide HPRD bins. Facilities with extreme values of HPRD in 1999 have been trimmed for presentation purposes.

TABLE 3.—EFFECT OF MINIMUM STAFFING LEGISLATION ON EMPLOYMENT OF NURSE AIDES (LOG OF NUMBER OF NURSES), THREE YEARS POSTPOLICY

	1	2	3	4
$GAP_{\overline{9798}}$	0.2081	0.2077	0.1973	0.1682
	(0.0247)	(0.0247)	(0.0252)	(0.0428)
$HPRD_{\overline{9798}} - 3.2$				−0.0183
				(0.0238)
Constant	0.0642	0.0675	NA	NA
	(0.0118)	(0.0116)	(—)	(—)
County fixed effects	No	No	Yes	Yes
Outliers removed	No	Yes	Yes	Yes
R^2	0.0540	0.0558	0.0950	0.0958

The dependent variable in all columns is the difference in the log of average annual number of workers (nurse aides) per pay period, 2003–1999. Columns 2–4 omit one nursing home that experiences more than a 1.5 log point change in nurse aide staffing. Robust standard errors in parentheses.

partial F-statistics reported in table 2 suggest the instrument relevance condition is satisfied.[18]

For most occupation types, the OSHPD data record only total hours worked, making it impossible to discern whether firms are adjusting employment on the extensive or intensive margin. For nurse aides, however, the data do record the average number of nurses working in any given pay period over the year. Table 3 reports the results of analyses similar to those just reported using the log of the number of nurses, rather than hours worked, as the dependent variable. The coefficients on *GAP* across specifications are very similar to those reported in table 2. This suggests that facilities responded to the staffing mandate by hiring more nurses rather than eliciting the same number of nurses to work longer hours and perhaps paying them overtime wages.

The effects of the law on licensed vocational and registered nurses were more ambiguous. In the left panel of figure 3, we see an overall increase in LVN hours worked, but little pattern related to preperiod staffing levels. The estimates in columns 1 to 3 of table 4 confirm that the law had no significant effect on their employment changes between 1999 and 2003. The coefficients on GAP are all below .057, suggesting a change for the typical facility that was out of compliance in 1999 of between 1% and 2.8%, but none of the coefficients is significantly different from 0. The right panel of figure 3 shows how facilities' staffing of RNs changed as a function of preperiod staffing levels. We see that on average, they were shedding RN staff, and higher-staffed facilities were shedding RN employment faster than those with low staffing levels. Table 4 reveals, however, that the relationship between RN staffing changes and prereform staffing levels was no different for firms initially in and out of compliance with the law: the coefficient on *GAP* is close to 0 and not statistically significant when I add a linear control for preperiod staffing level in column 6.[19] Moreover, an examination of trends in RN staffing suggests that the decline in RN employment among facilities with the highest staffing levels may have begun before implementation of the staffing law—perhaps in 1998 or 1999. One possible explanation is that pressure to reduce costs in the industry, perhaps driven by the switch to prospective payment systems in Medicare, encouraged substitution away from RNs toward less expensive LVNs and NAs (Konetzka et al., 2006). Facilities with lower staffing levels may have been more constrained in being able to replace RNs, for example, by federal regulations requiring minimum numbers of licensed and registered nurses.

A key assumption of the instrumental variables analyses that follows below is that the employment changes caused by

[18] I also explored models including the percent of total nurse hours worked by RNs and LVNs, and the interaction of that variable and *GAP* as explanatory variables. These variables are meant to capture whether facilities that had high fractions of licensed nurses initially—recall these nurses' hours counted twice under the previous staffing standard—had more leeway to substitute toward less skilled nurse aides under the new standards. While the results were consistent with the notion that firms with more skilled nurses hired nurse aides at a faster rate than those with relatively few skilled nurses, the coefficients were small in magnitude and neither economically important nor helpful in improving the partial F-statistic (treating the interaction term as an additional instrument).

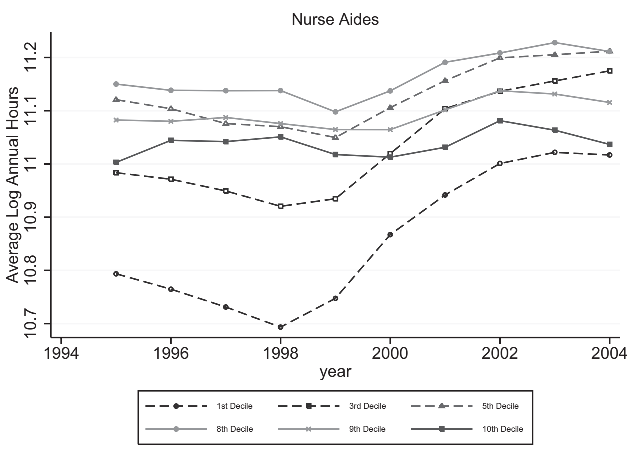
[19] Similar pictures for managers (not shown) reveal no change in their staffing levels, or any relationship with initial staffing levels.

TABLE 4.—EFFECT OF MINIMUM STAFFING LEGISLATION ON EMPLOYMENT OF LICENSED VOCATIONAL AND REGISTERED NURSES, THREE YEARS POSTPOLICY

	LVN Hours			*RN Hours*		
	1	2	3	4	5	6
$GAP_{\overline{9798}}$	0.0245	0.0201	0.0570	0.2256	0.2016	0.0182
	(0.0439)	(0.0398)	(0.0615)	(0.0776)	(0.0602)	(0.0930)
$HPRD_{\overline{9798}} - 3.2$			0.0234		−0.1153	
			(0.0301)			(0.0435)
Constant	0.1485	NA	NA	−0.2435	NA	NA
	(0.0183)	(—)	(—)	(0.0315)	(—)	(—)
County fixed effects	No	Yes	Yes	No	Yes	Yes
Outliers removed	No	Yes	Yes	No	Yes	Yes
R^2	0.0003	0.0595	0.0636	0.0072	0.0692	0.0759
Facilities *N*	1,031	1,024	1,024	1,031	975	975
Partial *F*	.31	.26	.89	8.44	11.58	.04

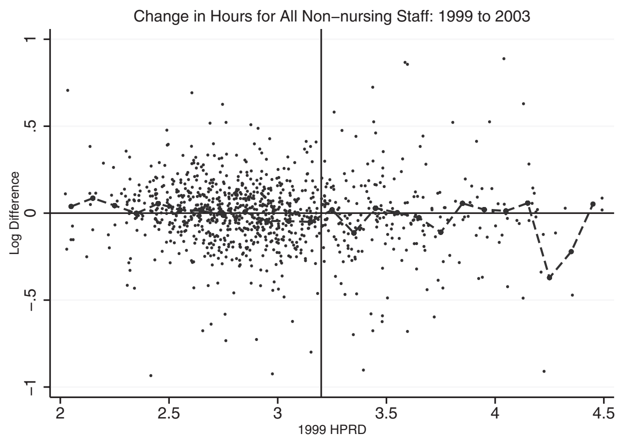
The dependent variable is the difference in the log of average total hours worked by LVNs (columns 1–3) or RNs (columns 4–6), 2003–1999. Columns 2, 3, 5, and 6 omit (typically very small) nursing homes that experience more than a 1.5 log point change LVN or RN staffing. Robust standard errors are in parentheses.

FIGURE 4.—TRENDS IN NURSE AIDE HOURS BY DECILE OF THE AVERAGE 1997–1998 STAFFING (HPRD) DISTRIBUTION, 1995–2004



The figure shows the trends in log annual hours worked for nurse aides for organizations in six groups. The six groups consist of organizations falling into the three odd-numbered lowest deciles of the 1997–1998 staffing (HPRD) distribution and the top 3 deciles. The bottom three decile groups plotted contain 110 facilites and the top three 105.

FIGURE 5.—FOUR-YEAR CHANGES IN LOG TOTAL ANNUAL HOURS AFTER MINIMUM STAFFING LEGISLATION ENACTED IN 1999 FOR ALL NONNURSING STAFF



Each dot represents data for one facility. The dashed line connects local averages of the employment changes for firms in .10-wide HPRD bins. Facilities with extreme values of HPRD in 1999 have been trimmed for presentation purposes.

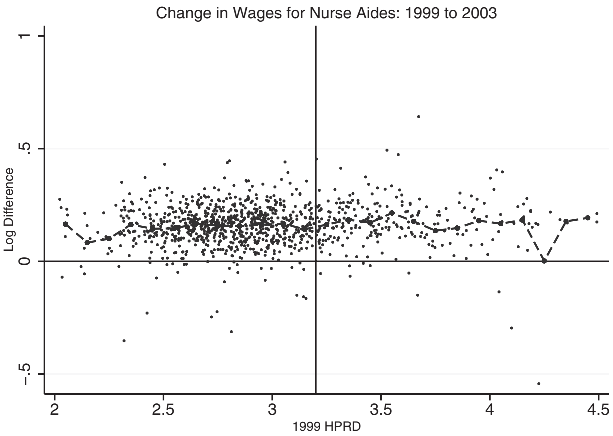
the staffing law are exogenous in the wage equation. While this cannot be fully verified, two further pieces of evidence help to build confidence that this condition is satisfied in the case of nurse aides. First, figure 4 demonstrates that trends in staffing levels in the five years leading up to the staffing bill appear to be unrelated to facilities' initial (average 1997–1998) staffing levels. In this figure, I group all facilities with average staffing ratios below the 3.2 HPRD threshold into seven groups of about 104 facilities each, ranging from those with the lowest HPRD in the preperiod to those falling just short of the threshold. Similarly, I group all facilities with staffing levels above the threshold into three groups of 100 firms each, again separated by their preperiod staffing levels. I refer to these ten groupings of facilities as deciles. Figure 4 shows trends in nurse aide staffing in each of six deciles: the first (with the lowest staffing levels), third, fifth, and eighth, nineth, and tenth.[20] The most obvious points from this figure have been discussed: after 2000, facilities that were already in compliance with the law—those in the eighth, nineth, and tenth deciles in the figure—mildly increased their employment of NAs—on the order of 5% to 8% over four years. Those below the threshold increased their NA employment significantly more, and the magnitude of the increase was largest—up to 25% or 30% over four years—for those furthest below the 3.2 HPRD threshold (first- and third-decile firms). From the standpoint of the validity of the research design employed here, however, the fact that all groups of facilities appear to follow similar trends in hours worked before the staffing legislation is noteworthy and supports the notion that those with high initial staffing levels provide a suitable counterfactual for the changes among those with low initial staffing levels after the law.[21]

The second piece of evidence lending support for the design here is presented in figure 5. The OSHPD data also contain information on total hours worked and salaries by a variety of nonnursing occupations such as housekeepers,

[20] Figures showing the omitted deciles reveal qualitatively similar patterns.

[21] Similar graphs (not shown) plotting changes in wages by decile show that preperiod trends are nearly identical across deciles for all three nurse occupations.

FIGURE 6.—FOUR-YEAR CHANGES IN LOG WAGES AFTER MINIMUM STAFFING LEGISLATION ENACTED IN 1999 FOR NURSE AIDES



Each dot represents data for one facility. The dashed line connects local averages of the wage changes for firms in .10-wide HPRD bins. Facilities with extreme values of HPRD in 1999 have been trimmed for presentation purposes.

laundry staff and dietitians. If the changes in nurse staffing described above were related to unobserved facility- level labor supply shocks, we might expect to see similar changes in the employment of these other types of workers. Figure 5 plots the changes in hours worked by all other nonnursing occupations (aggregated together) and confirms that no such patterns are evident in the data.

Taken together, the evidence suggests that the staffing law induced sizable increases in nurse aide employment proportional to *GAP*. This natural experiment will be silent on the firm-level labor supply elasticity of LVNs and RNs as the law does not induce nursing homes to hire these more skilled nurses.

B. *Effects of the Legislation on Wages, and Inverse Labor Supply Elasticity Estimates*

Figure 6 is similar to figure 2, only it depicts the reduced-form effect of the staffing law on log wages, rather than log hours for nurse aides. While overall wages grew strongly by about 16% from 1999 to 2003, the figure reveals no apparent relationship between growth in wages and initial staffing levels. In analyses similar to those in table 2, I confirm that the regression estimates of the impact of the law on wages are small (less than .03 in absolute value) and insignificant. For nurse aides, where the largest increases in staffing were observed, the point estimates suggest that an average facility with an initial staffing level of 2.7 decreased wages by about 1.7% (relative to other facilities). The results for LVNs and RNs are similar to those for nurse aides: none of the wage effect estimates are significantly different from 0 in any specification, over any time horizon.

The absence of an impact of the staffing law on wages is a key piece of evidence in assessing the importance of monopsony in the labor market. Without a significant effect on wages, I will not be able to reject the null that the inverse elasticity of labor supply is 0. In discussing the instrumental

TABLE 5.—INSTRUMENTAL VARIABLES ESTIMATES OF INVERSE LABOR SUPPLY ELASTICITY FOR NURSE AIDES

	1	2	3	4
Δ Log Hours, 2003–1999	−0.0363	−0.0368	−0.0475	−0.2281
	(0.0500)	(0.0516)	(0.0564)	(0.1549)
$HPRD_{\overline{9798}} - 3.2$				−0.0245
				(0.0203)
Constant	0.1653	0.1654	NA	NA
	(0.0085)	(0.0088)	(—)	(—)
County fixed effects	No	No	Yes	Yes
Outliers removed	No	Yes	Yes	Yes
R^2	0.023	0.023	0.090	0.015

The dependent variable in all models is the change in log total salary and wages from 1999 to 2003. Columns 2–4 omit one nursing home that experiences more than a 1.5 log point change in nurse aide staffing. Robust standard errors in parentheses.

variables results presented in tables 5 through 7, I therefore focus on the range of parameters consistent with the upper end of the 95% confidence intervals. Also, I focus on estimates of the inverse labor supply elasticity for nurse aides only. The minimum staffing policy provides too weak an instrument for changes in hours worked by higher skilled nurses, as indicated by low partial F-statistics for the first-stage regressions of hours changes on GAP reported for both LVNs and RNs in table 4.

The main result of this paper can be seen in the juxtaposition of figures 2 and 6. Facilities that were initially below the mandated staffing threshold increased their employment of nurse aides significantly relative to firms already in compliance. Despite this, the growing facilities did not have to raise their wage offers relative to their competitors in the labor market in order to attract more workers. Taken together, these facts suggest a highly elastic firm-level labor supply curve. Table 5 shows the results of instrumental variables estimation of the firm-level labor supply equation (2) for nurse aides. Across all specifications, we see that the elasticity point estimates are wrong-signed across all specifications, but the confidence intervals all overlap 0. The point estimates range from −.036 to −.22, and the upper end of the 95% confidence interval ranges from about .06 to .08. Thus, we cannot reject the null hypothesis of perfect competition (an inverse labor supply elasticity of 0), and estimates are consistent with at most a very small positive elasticity.[22]

Tables 6 and 7 examine whether the labor supply of nurse aides to nursing homes appears to be more elastic over time, and if there are differences across urban and nonurban labor markets. In both tables, the standard errors of the estimates for labor supply elasticity are too large to statistically discern

[22] Point estimates of the inverse elasticity of labor supply for RNs (not shown) based on a model with no control for initial HPRD range from −.133 to −.168, with the upper end of the confidence interval at about .034. Adding the control for HPRD leads to extremely imprecise estimates, however, due to the tenuous first-stage relationship between the policy change and the change in hours for RNs. Since nursing homes employ so few RNs—a typical home might employ four to six—adding a worker or two results in extreme percentage changes in total hours worked. Unfortunately the noise in the estimates prevents me from comparing the results for nurse aides and RNs in any reliable fashion. Moreover, the small F-statistics for the first stage, coupled with evidence that the exclusion restriction might not be satisfied, suggest that these estimates may be substantially biased.

TABLE 6.—INSTRUMENTAL VARIABLES ESTIMATES OF INVERSE LABOR SUPPLY ELASTICITY FOR NURSE AIDES DIFFERENCES OVER TWO- TO FIVE-YEAR TIME HORIZON

	Years since Policy Change			
	2	3	4	5
Δ Log Hours	−0.1113	−0.2843	−0.2281	−0.2677
	(0.3604)	(0.1730)	(0.1549)	(0.1577)
$HPRD_{\overline{9798}} - 3.2$	−0.0101	−0.0243	−0.0245	−0.0336
	(0.0333)	(0.0174)	(0.0203)	(0.0222)
County fixed effects	Yes	Yes	Yes	Yes
Outliers removed	Yes	Yes	Yes	Yes

The dependent variable in all models is the change in log total salary and wages from 1999 to 2003. Columns correspond to estimates of model 4 in table 5 and estimate the nurse aide labor supply elasticity over a two- to five-year time horizon (changes from 1999 to 2001 through 2004, respectively). Robust standard errors in parentheses.

TABLE 7.—INSTRUMENTAL VARIABLES ESTIMATES OF INVERSE LABOR SUPPLY ELASTICITY FOR NURSE AIDES' DIFFERENCES BY URBANICITY

	Urban Counties		Nonurban Counties	
Years since Policy	2	4	2	4
Δ Log Hours	−0.2122	−0.2246	0.8203	−0.2424
	(0.3401)	(0.1955)	(5.9186)	(0.2719)
$HPRD_{\overline{9798}} - 3.2$	−0.0258	−0.0257	0.0600	−0.0232
	(0.0379)	(0.0297)	(0.3813)	(0.0251)
County fixed effects	Yes	Yes	Yes	Yes
Outliers removed	Yes	Yes	Yes	Yes

The dependent variable in all models is the change in log total salary and wages from 1999 to 2003. Columns correspond to estimates of model 4 in table 5, and report differences in the two- and four-year estimates of ϵ between urban and nonurban counties. Robust standard errors in parentheses.

a pattern. Table 6 shows no evidence for more elastic supply over a longer time horizon: the point estimates evidence less elasticity over longer time horizons, though the differences are not statistically significant. The patterns of point estimates in table 7 are consistent with there being more monopsony power in nonurban markets for the two-year estimates but not the four-year estimates, but again the differences are dwarfed by the sampling error so an informative test is not possible.[23]

VI. Discussion

The results provide no evidence that monopsony is an important feature of the labor market, at least for less skilled nurses in the long-term care industry. For nurse aides, the dynamics of wage and employment movements in the wake of the staffing legislation are consistent with a basic model of perfect competition in the labor market: employers forced to increase their staffing levels are able to recruit as many new workers as they require at the market wage. This is the first direct evidence on the firm-level labor supply elasticity for a low-skilled occupation outside the minimum wage literature.

How should we view the empirical relevance of monopsony in the low-wage labor market in light of these results? The tests for identification support the credibility of the research design used here to estimate the impact of the staffing law on nurse aide employment and wages. Interpreting the ratio of these effects as a labor supply elasticity, however, is inherently a model-dependent exercise. If firm labor supply depends on only its (relative) wage offer, then the analyses here are sufficient to conclude that firms have little to no monopsony power in the market for nurse aides. In more complicated models, however, other considerations are relevant.

As Kuhn (2004) emphasizes, for example, in a case with heterogeneous workers, nursing homes might adjust employment on the quality margin as well as the quantity margin. If nursing homes responded to the staffing mandates by hiring lower-quality nurse aides but still paid the same wage, then we might interpret the results as consistent with an upward-sloping quality-adjusted labor supply curve. Unfortunately, as with most other studies in the literature, I do not have sufficiently detailed data on employee characteristics to allow a direct test of whether the quality of marginal recruits is changing. That said, compared to studies of registered nurses or teachers, the scope for large variation in employee quality seems limited here by the low average formal skill requirements of the job. According to the 2000 Census, about 55% of working nurse aides in California had a high school diploma or less, and 82% had no college degree of any level, including an associate degree. And though not definitive, there is no aggregate evidence using data from the American Community Survey of a decline in education levels among working nurse aides in California between 2000 and 2004.[24] A related concern is that since I use average firm-level wages for each occupation, it may be hard to detect changes in the wages offered to new recruits, the wage variable of interest. This concern is somewhat mitigated by the high turnover in nursing homes noted earlier: on average, facilities report that only about 24% of nurse aides work continuously for a full year, and so it is likely that most are receiving starting wages or an amount quite close.

It may also be that facilities can use nonwage amenities or expenditures on recruiting to attract workers, conditional on wages (Manning, 2011). To the extent that nursing homes use either strategy, the results here might understate the degree of monopsony power of nursing homes. Unfortunately, the data do not permit a test of whether facilities use these other strategies to influence their supply of labor, so this remains a topic for further research.

A final concern is that health care is one of the few sectors in the United States where unions are still prominent. The 2007 CPS Merged Outgoing Rotation Group data suggest that about 50% of registered nurses and 20% of nurse aides belong to a union in California. How might this affect the results presented here? Models of bilateral monopoly would lead to different predictions depending on the goals and relative strength of the union, but it is possible that negotiation between the union and employer could lead to results presented here even with an upward-sloping labor supply curve.

[23] In robustness tests, I also drop all chain nursing homes—defined as any home belonging to a parent company operating more than ten facilities in California—and get nearly identical results for the analyses summarized in tables 5 to 7.

[24] Analyses of IPUMS data (Ruggles et al., 2008), are available from the author by request.

It is possible, for example, that the union wanted to expand employment—unions were active in pressing for the passage of AB 1107—and promised to limit wage demands in return. Again, the scope for this type of behavior to substantially alter the analyses seems limited due to relatively low union penetration rates in the nursing home sector. For example, according to Swan and Harrington (2007), only about 14% of facilities were unionized in 1999, and it is likely that such facilities were more likely to have been the government-operated homes that were omitted from my analysis.[25]

If we accept the results as valid, the finding of a perfectly elastic labor supply curve for nurse aides adds interesting texture to the growing body of research about imperfect competition and market power in the labor market. While I suggest methodological reasons that well-done studies like Staiger et al. (2010) and Falch (2010) may have mistakenly found evidence of monopsony, the results of those papers are not necessarily at odds with the results here. Indeed, some models of monopsony predict that market power is greater in the market for registered nurses in hospitals or licensed school teachers than for the nurse aides found here. If monopsony power derives primarily from horizontal differentiation among potential employers, then there are several reasons that the RN or teacher labor market may be less competitive. For example, geographic employment concentration is significantly higher in the hospital RN market, with only about 500 hospitals compared to more than 1,200 nursing homes. Moreover, occupation-specific human capital likely accounts for a larger fraction of wages for both teachers and registered nurses given the advanced formal training and credential requirements relative to nurse aides. Both of these factors imply that switching jobs is more costly in both the hospital RN and teacher labor market, potentially giving firms more market power.

The results here are more difficult to reconcile with the notion that low-wage labor markets such as those for fast food workers are monopsonistic. While of course differences exist between the industries and occupations involved, most of these differences—for example, in skill levels or employer concentration—would seem to suggest more market power for nurse aides. In this light, it is puzzling why minimum wage studies with highly credible research designs continue to find no employment effects.[26] Future work should attempt to better understand the determinants of employer market power to reconcile the differences across industries and occupations found in the literature on minimum wages, and the growing literature on firm-level labor supply.

[25] Unfortunately, I do not have the microdata to identify which homes in my sample were unionized.

[26] See, for a recent example, Dube, Lester, and Reich (2010).

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