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ABSTRACT

Classic Monopsony or New Monopsony? Searching for Evidence in Nursing Labor Markets*

The market for hospital registered nurses (RNs) is often offered as an example of "classic" monopsony, while a "new" monopsony literature emphasizes firm labor supply being upward-sloping for reasons other than market structure. Using data from several sources, we explore the relationship between wages and measures of classic and new monopsony. Micro wage data for 1993-2002 provide little evidence of classic monopsonistic outcomes in the long run, the relative wages of RNs in 240 U.S. labor markets being largely uncorrelated with market size or employer concentration. A short-run relationship is found, with RN wages declining in markets with increased hospital system concentration. Measures of new monopsony use data on mobility to proxy inverse supply elasticities. No relationship is found between these measure and nursing wages, but evidence supporting new monopsony is found for women elsewhere in the labor market. RNs display greater inter-employer mobility than do women (or men) in general. Two conclusions follow. First, evidence of upward sloping labor supply need not imply monopsonistic outcomes. Second, nursing should not be held up as a prototypical example of monopsony.

JEL Classification: J42, I10, J30

Keywords: monopsony, nurses, wages, hospitals

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I. Introduction

A common textbook example of monopsony is the market for registered nurses (RNs) employed by hospitals.[1] A recent paper on nursing monopsony states: "Thus, if one found no evidence of monopsony in this market, it would be difficult to argue that monopsonistic competition was a pervasive feature of the labor market"(Staiger, Spetz, and Phibbs 1999, p. 2). The empirical literature, however, provides mixed conclusions. One strand focuses on empirical estimates of RN labor supply elasticities facing hospitals. This research (Sullivan 1989; Staiger, Spetz, and Phibbs 1999) generally finds evidence of upward sloping labor supply curves and concludes that this result supports the existence of monopsony. Another strand (e.g., Adamache and Sloan 1982; Hirsch and Schumacher 1995) investigates whether relative wage and/or employment outcomes vary with respect to hospital concentration, labor market size, and the like. Such studies provide little support for the classic monopsony model.[2]

These disparate results might be reconciled in several ways. One argument is that oligopsony need not produce stable labor market outcomes; these may vary across time and with respect to market conditions. By this argument, one must search across different time periods and labor markets to determine the prevalence of oligopsonistic outcomes. A second argument is that oligopsony is widespread, with employers in both concentrated and non-concentrated labor markets facing upward sloping firm-level supply curves owing to imperfect worker mobility (Manning 2003). Following the argument of the "new monopsony" literature, market structure measures have limited relevance.[3] Failure to find a relationship between, say, nursing wages and the number of hospitals or city size is not evidence against oligopsony. Rather, employers in large and small markets alike face upward sloping supply curves and behave as oligopsonistic competitors. A third argument is that upward sloping labor supply is a necessary but not sufficient condition for monopsonistic outcomes (Hirsch and Schumacher 1995). Although evidence may support existence of

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[1] Five leading undergraduate labor economics texts were examined. Four of five identify hospital RNs as a frequent example of monopsony. Two of the four note that evidence for nursing monopsony is mixed, whereas the other two provide no such qualification. Boal and Ransom (1997) provide a comprehensive survey of the monopsony literature.

[2] Sullivan (1989), who fails to find interarea wage differences consistent with monopsony, can be included in the latter as well as the former group. His paper is generally cited as providing evidence in support of monopsony.

[3] Discussing the "market structure" approach, Bhaskar, Manning, and To (2002, p. 170) state: "The classic study of Bunting (1962), for example, examined whether wages are lower in labor markets with fewer employers. Little evidence for this is found, but this is actually a test of classic monopsony; oligopsonistic competition does not necessarily rely on "large" employers. For example, models that emphasize the costs of job search typically assume each employer is infinitesimally small in relation to the market, so employer concentration is irrelevant."

upward sloping supply facing individual employers, it need not follow that monopsonistic outcomes result.[4]

This paper addresses these contrasting views in an examination of wage determination in nursing labor markets. We provide two-step cross-section tests of "classic" monopsony, relating the relative wages of hospital RNs in urban markets to the concentration of hospital employers and market size, the latter approximating the number of non-nursing as well as non-hospital nursing employers. We provide evidence for alternative time periods in which market conditions and hospital market structure varied. This exercise allows us to assess whether monopsonistic power waxes and wanes and whether such tests are sensitive to the time period studied. We also examine whether changes in relative RN wages across time periods are correlated with changes in hospital concentration. Although open to interpretation, we argue that wage level results test for existence of long-run monopsonistic outcomes, whereas wage change estimates provide a measure of short-run outcomes.

We also search for oligopsonistic power as described in the new monopsony literature. Manning (2003) proposes a simple measure of monopsony – the proportion of new hires or recruits from outside employment. If the proportion of new hires from employment (i.e., from other jobs) is high, the suggestion is that workers are mobile (elastic firm-level labor supply) and monopsonistic power is weak. If new hires come primarily from outside employment (unemployment or out of the labor force), there exists little mobility across employers and monopsonistic power is strong. We calculate job transition rates for hospital RNs and a control group of female workers across urban labor markets and examine whether measures of "new" monopsony are correlated with measures of classic monopsony. If highly correlated, cross-sectional "outcome tests" relating wages to market structure and size provide evidence on the new as well as old monopsony approaches. If weakly correlated (as is the case), the alternative measures permit one to distinguish between the effects of classic and new monopsonistic power.

In what follows, we first discuss the market structure approach of classic monopsony. Changes in hospital market structure, combined with the inherent instability of oligopsonistic outcomes, provide a strong rationale for examining evidence in multiple time periods. We next examine data on transitions among new hires in nursing (and non-nursing) in order to construct measures of the new monopsony. We

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[4] An additional argument is that labor supply studies finding a positive inverse elasticity fail to measure *long-run* supply. Despite the difficulty in estimating firm-level labor supply curves, we do not challenge the conclusion that hospitals (among other firms) face upward-sloping labor supply curves over a lengthy planning horizon.

then turn to the evidence on outcomes, relating wages for nurses (relative to a control group) to differences across markets in measures of classic and new monopsony.

II. Structural Changes in Health Care Labor Markets

Most RNs are employed in hospitals. It is the dominant role of hospitals in the RN labor market that has made nursing one of the most common examples of monopsony. The effect of hospitals on nursing wages and employment, however, need not be invariant over time. Two changes in recent years may have affected hospital market power. First, the share of total RN employment within hospitals has declined. Second, merger activity has led to consolidation within the hospital industry. The former trend should weaken and the latter strengthen hospitals' oligopsonistic power.

Information on occupation and industry in the Current Population Survey (CPS) permits us to identify RNs and their sector of employment. Based on compilations from the CPS, 73% of the 1.42 million RNs employed in the U.S. in 1985 were employed in hospitals. RN employment had risen to 2.24 million by 2002, but the share employed in hospitals had fallen to 62%. Much of the movement of RN employment out of hospitals occurred during the mid-1990s following structural changes in the industry. RNs increasingly can seek employment in physician-owned specialty facilities, where outpatient procedures once conducted in hospitals are now performed. Jobs in these facilities typically have Monday-Friday daytime work schedules without overtime or on-call duty. The movement of health personnel out of hospitals is not unique to RNs. During the same 1985-2002 period, the percentage of licensed practical nurses (LPNs) employed in hospitals fell from 57% to 28%; the percentage of nursing aides fell from 31% to 23%.[5]

The financial environment and market structure of the health care industry has been in continuous change since the mid-1980s. Important have been the rise of managed care and hospital consolidation (Gaynor and Haas-Wilson, 1999). The early 1990s was a period in which managed care and the use of cost containment strategies expanded. These changes placed strong downward pressure on growth rates in health care expenditures. Such cost consciousness affected labor utilization and wages. Hospital mergers began increasing during the early 1990s. The peak year was 1996, with 235 hospital mergers involving 768 facilities, with 15% of all hospitals being involved in a merger that year. Following this consolidation,

[5] These figures are calculated from the CPS monthly earnings files using employment weights. Schumacher (1997, Table 1) provides annual hospital and nonhospital employment for RNs and LPNs beginning in 1977. In 1977, 72% of RNs and 63% of LPNs were employed in hospitals.

merger activity slowed during the late 1990s and early 2000s. In 2002, for example, there were 60 mergers involving 163 facilities (*Modern Healthcare* Staff Reports, January 11, 1999 and January 28, 2003). Consolidation has generated attention from industry analysts regarding its effect on health care costs (Fong 2003) and its implications for antitrust policy (Taylor 2003). Managed care and cost containment lowered medical costs relative to what they would otherwise have been, but did not permanently lower growth rates in health expenditures. Since the late-1990s, health care costs have risen sharply. Per-capita health expenditures increased by 9.3% in 2002 ($5,440 per capita), accounting for 14.9% of GDP, an increase from 13.3% in 2000 following stability through much of the 1990s (Levit et al. 2004).

There has been considerable research on the effects of hospital market structure on the efficiency of *output* markets.[6] There is relatively little research on the effects of hospital market structure on labor markets. Our focus is on market power among hospitals in nursing labor markets, the exercise of which may well vary over time owing to demand or supply shocks. We search for classic monopsony during two rather different periods – the early and mid-1990s at which time the industry was restructuring as a result of strong cost pressures and the late 1990s and early 2000s during which health care expenditures resumed rapid growth. Relative nursing salaries fell or held steady during the former period (after years of exceptional growth) and rose during the latter period.[7]

III. New Views on Monopsony: Examining Mobility in Nursing Labor Markets

The new monopsony/oligopsony literature (Baskgar, Manning, and To 2002; Manning 2003) emphasizes that monopsonistic power is widespread. Because of imperfect information, firm-specific training, worker-specific attachment to firms, and immobility arising from various sources, employers face upward sloping labor supply curves. Profit maximizing wages and employment, it is argued, should vary with respect to the labor supply elasticity. A familiar characterization is that the proportional gap between the marginal revenue product and wage is equal to the inverse of the labor supply elasticity.

$$(Y\text{-}w)/w = \varepsilon \qquad (1)$$

Here Y is the marginal revenue product, w the wage, and ε the *inverse* of the labor supply elasticity. The competitive case of an infinitely elastic labor supply curve facing firms ($\varepsilon = 0$) implies zero exploitation;

[6] See Lynk (1995), Melnick, Keeler and Zwanziger (1999), Dranove and Ludwick (1999), and Gaynor and Vogt (2003).
[7] Hirsch and Schumacher (1995) conduct an analysis of classic monopsony for the years 1986-93, a period of rising relative wages and reported nursing shortages. Combined with the current study, evidence on classic monopsony is available for three periods.

that is, no mark-up of labor's marginal cost relative to the wage. Oligopsony or monopsonistic competition requires only that firms' labor supply curves be upward sloping. The slope need not be highly correlated with measures of employer concentration and labor market size.

Manning proposes a simple measure to approximate monopsonistic power, measured by the share of new employees ("recruits") that move from other employers rather than from unemployment or out of the labor force.[8] The intuition is that it is the ability and willingness of workers to move between employers that most constrains monopsonistic power. The lower the proportion of new hires coming from employment with other firms, the lower are expected wages and employment, all else the same. Letting R be the number of newly hired workers (recruits) in a given market, each of whom arrives from one of three states – e being employment elsewhere, u being unemployment, or n being outside the labor force. The measure of monopsony power, M, the fraction of new recruits from non-employment, is:

$$M = (u+n)/R, \qquad (2)$$

or, equivalently,

$$M = 1 - (e/R). \qquad (3)$$

Manning shows that this "back of the envelope" measure is a good proxy for monopsony power (an inverse measure of competition) in many labor market models, and is necessarily a monotonic function of a measure derived in the Burdett-Mortenson (1998) model of equilibrium wage differentials.

Manning calculates estimates of M for the U.S. and U.K. labor forces. The U.S. measures are compiled from the 1994-2000 monthly Current Population Surveys, utilizing the rolling panel nature of the CPS. Manning obtains an estimate of M = .55, with values of .59 for women and .50 for men (Manning, 2003, Table 2.2). He states that differences in M across types of workers mirror wage differences, basing this conclusion on regression correlates of transition probabilities – female, black, and poorly-educated new hires having higher probabilities of a transition from non-employment than from employment.[9]

The "new monopsony" literature focuses on the labor supply elasticity, assuming that a finding of upward sloping supply is sufficient evidence for monopsonistic power. Manning (2003), for example, points to two unpublished studies that provide what appear to be good instruments to identify firm-level labor

[8] Ideally, one would estimate the elasticity of firm-level labor supply curves, but this is difficult given a paucity of firm data and difficulty identifying employment effects associated with exogenous wage change.
[9] An exception is Hispanic new hires, who are less likely to have come from non-employment.

supply. He cites each of these as providing strong evidence for the existence of monopsonistic power (in the markets on which these studies are based) and as examples of the type of work of which more is needed. In short, the literature on monopsony appears to assume that evidence of upward sloping labor supply curves necessarily implies lower wage rates.

We accept the premise that there exists substantial heterogeneity and wage variability across firms, and that many if not most employers face upward sloping labor supply curves in the short and medium runs, and perhaps over a long-run horizon. We do not accept the premise that upward sloping labor supply curves *necessarily* imply wages that vary with the labor supply elasticity. The wage paid to workers affects such things as retention, applicant queues, search costs and strategies, unmeasured worker quality, and voluntary and required worker effort. "Imperfect" worker mobility may reflect factors such as implicit incentive contracts, specific training, and worker rents, most often associated with higher rather than lower wages. Moreover, because there exist multiple employers of RNs even in small (classical) oligopsonistic markets, wage outcomes are indeterminate. A priori, we do not know how many employers are required to produce relatively competitive wage outcomes. Nonzero inverse labor supply elasticities should not be regarded as sufficient evidence that employers possess and are exercising employer power with respect to wages.

If wages need not vary with measured labor supply elasticities, it becomes essential to look at evidence. Here we examine whether or not wages vary systematically with measures such as *M* that, according to the new monopsony literature, reflect cross-market differences in labor supply elasticities and monopsonistic power in labor markets.

IV. Data and Method of Analysis

A. Data

We use four data sources. The primary data for this study are drawn from the monthly Current Population Survey (CPS) Outgoing Rotation Group (ORG) earnings files for January 1993 through December 2002. The analysis is divided into two periods, 1993-1997 and 1998-2002. The CPS provides a reasonably large sample of RNs and large representative samples of health care and non-health workers from which appropriate control groups can be constructed. We characterize the earlier period as one when the market for RNs was weakening, due primarily to the growth of managed care and other cost containment measures. We characterize the latter period, 1998-2002, as one of strengthening labor markets, with rapid growth in health expenditures accompanied by rising real wages.

For most years in our sample, workers in the CPS are assigned to one of 240 "markets" comprising 191 Metropolitan Statistical Areas or Consolidated Metropolitan Statistical Areas (MSA/CMSA) and 49 state groups that include all workers within a state not living in an MSA/CMSA.[10] Thus we have representative national samples with all workers assigned to a market. Unlike the metropolitan areas, the non-urban state groups may not form a unified labor market. They are retained since classic monopsony power is more likely where there are few employers.

Our principal nursing sample includes all hospital registered nurses (RNs). Our principal control group for RNs is area-specific and includes women (94% and 92% of RNs are female in our 1993-97 and 1998-2002 CPS samples, respectively) with either an associate or baccalaureate degree, employed in non-health related occupations within the following broad occupational categories: executive, administrative and managerial; professional specialty; technicians and related support; sales; administrative support and clerical; and service (except protective and household services). Excluded are all health-related workers since hospital behavior and wage policies can affect their earnings. Preliminary analysis indicated that our principal results are not sensitive to the choice of control group workers.[11]

The RN and control group samples include employed wage and salary workers ages 18 and over, with positive weekly earnings and hours. The wage on the primary job is measured directly for workers reporting hourly earnings and who do not receive tips, commissions, or overtime. For others the wage is calculated by dividing usual weekly earnings on the primary job (which includes usual tips, commissions, and overtime) by usual hours worked per week.[12] Usual weekly earnings are top-coded at $1,923 through

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[10] There are 173 MSAs, 18 CMSAs, and 49 non-urban state groups (all of New Jersey and D.C. are in MSA/CMSAs). CMSAs contain two or more primary metropolitan statistical areas (PMSAs). Metropolitan areas are based on 1993 Census definitions, used in the CPS beginning in September 1995. For the January 1993 through May 1995 CPS files, which use 1983 Census definitions, we reassign workers wherever possible into areas using the more recent designations. For these early years, workers are divided into 231 markets (182 MSA/CMSAs and 49 state groups). In the analysis using the 1993-97 period there are 255 unique market areas, with 15 areas included only for 1993 through early 1995 and 24 included only during late 1995 through 1997. We do not include workers for June-August 1995, where no metropolitan identifiers are available.

[11] Excluded from the control group are males, females with less than an associates degree or more than a B.A. degree, workers employed in health-related occupations within the broad occupational categories in our sample, and workers in the following non-selected occupational categories: private household services; protective services; farming, forestry, and fishing; precision production, craft, and repair; machine operators, assemblers, and inspectors; transportation and material moving; and handlers, equipment cleaners, helpers, and laborers.

[12] Those who report "variable" hours worked per week have their implicit wage calculated, if necessary, using hours worked last week.

1997 and at $2,885 beginning in 1998. Those at the cap are assigned mean earnings above the cap based on year and gender-specific estimates that assume a Pareto distribution for earnings beyond the median.[13] We omit the few workers with measured hourly earnings less than $3 or greater than $150 (in 2002 dollars).

Workers who have had their earnings, occupation, or industry allocated by the Census are also excluded. Very few workers have occupation or industry allocated. A large number (20-30%) have earnings imputed by the Census based on a cell hot-deck procedure in which nonrespondents are allocated the earnings of a "donor" with an identical set of match characteristics. CPS imputation rates have been particularly large in recent years. It is important that imputed earners be excluded owing to "match bias" (Hirsch and Schumacher, 2004). First, since non-responding hospital RNs are not in general matched to the earnings of other hospital RNs (industry is not a match criterion and occupation is defined at a broader level than RN), wage differences between RNs and the control group are biased toward zero. Second, location is not an explicit donor match criterion in the monthly ORGs, causing wage differences across areas to be artificially compressed.[14] Following the above sample restrictions, the CPS sample sizes of hospital RNs and the college-educated female control group are 7,982 (or 11,893 including all RNs) and 63,464 for the 1993-97 period and 6,732 (10,754 all RNs) and 61,391 during 1998-2002.

A second data source is the Annual Survey of Hospitals conducted by the American Hospital Association. Data for individual hospitals are available on such things as location, employment, average daily census, number of beds, and hospital system name. We make use of the AHA survey to calculate Herfindahl-Hirschman indices of hospital concentration by market area, corresponding to the metropolitan and non-metropolitan areas identified in both the CPS and AHA survey.[15] The Herfindahl index, *HI*, is defined as $HI_k = \Sigma_k s_i^2$, where k indexes the market area and s_i is the proportional market share of each hospital (or hospital system) in an area. *HI* is bounded [0,1] with low values representing a high degree of product and labor market competition and 1 representing a single hospital (or system) in the market. Four *HI* indices are compiled, alternatively using the hospital "average daily census" or "number of beds" as the

[13] Estimates of gender-specific means above the cap for 1973-2003 are posted at http://www.unionstats.com. Values are about 1.5 times the cap, with somewhat smaller female than male means and modest growth over time.

[14] Allocated earners cannot be reliably identified (and excluded) between January 1994 and August 1995. All persons surveyed in June-August 1995 were excluded due to absence of metropolitan identifiers (footnote 10).

[15] Summary statistics by metropolitan area from the AHA survey are published in *Hospital Statistics*. These include, among other things, number of hospitals, employment by type of personnel (RNs, LPNs, etc.), compensation costs, total expenditures, patient days, and costs per patient. These data were used in Hirsch and Schumacher (1995) to construct a hospital density measure (number of hospitals per square mile).

output measure and individual hospitals or hospital systems as the observation unit (in the latter, all area hospitals part of the same system are counted as a single hospital).[16] We show results for *HI* based on the average daily census since there is no meaningful difference between results using the Census and number of beds. Although typically similar, results are shown using both the system and individual hospital measures. Because relative differences across markets in number of hospitals change slowly, we match hospital information for 1993 to our 1993-97 CPS sample and hospital information for 2000 to the 1998-2002 CPS sample (we were unable to calculate system-based concentration using the 1995 AHA survey).

A third data source is the full CPS (i.e., all rotation groups) for 1994-2002. These data are used to measure the proportion of new recruits hired from outside the labor force, from unemployment, and from employment elsewhere, allowing us to estimate the new monopsony measures M^{RN} and M^{C} in a manner similar to Manning (2003). The CPS design is that each household is in the survey for four consecutive months in one year (rotation groups 1-4), out for eight months, and then in the same four months the following year (rotation groups 5-8).[17] Beginning in 1994, individuals employed in the previous month are asked if in their primary job they have the same employer as in the previous month (variable PUIODP1). This question is asked of rotation groups 2-4 and 6-8, but not 1 and 5. To calculate *M*, we begin with currently employed wage and salary workers, ages 20 to 64 (we exclude full-time students less than age 25; older students are not identified). We then reach back to the previous month and see (among other things) if they were employed, unemployed, or out of the labor force. This large sample is then restricted to the small subset who are new hires or recruits – those currently employed who state they have a different employer than in the prior month or during the prior month were either unemployed or out of the labor force. *M* equals one minus the ratio of those who have changed employers to total recruits in a given market, calculated using CPS employment weights. By restricting the samples to those who are currently wage and salary women within certain occupational groups and education, those employed as RNs in hospitals, or the

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[16] We chose an output measure (census or beds) rather than employment to calculate hospital concentration, since RN employment is a monopsonistic outcome we examine subsequently. The AHA survey designates an MSA/PMSA location and the state. In order to get an exact match for the CMSAs designated in the CPS, the hospital data were aggregated from the component PMSAs. Information on small MSAs that are not separately designated in the CPS was combined with that on the non-metropolitan portion of the state to calculate values for our 49 non-urban CPS state areas. We thank Joanne Spetz and Wendy Dyer for their substantial assistance in accessing the AHA data and calculating the Herfindahl indices.

[17] The "outgoing rotation groups" (rotations 4 and 8) are asked questions on current earnings, hours, and union status. These monthly quarter samples make up the ORG earnings files used in our wage analysis.

like, one can construct alternative measures of M nationally and for each labor market area (households that move between months fall out of the CPS, as do individuals who change households).[18]

Ideally, our CPS measure of monopsony, M^{RN}, could be measured reliably in each market. Unfortunately there are only 645 hospital RN *recruits* in our 1994-2002 CPS sample. This sample provides useful information on national mobility patterns for hospital RNs as compared to other groups of workers, but does not permit us to construct a reliable measure of M^{RN} across our 240 labor markets.[19]

Instead of relying on a CPS measure of M^{RN}, we use a fourth data set, the National Sample Survey of Registered Nurses (SRN). The SRN is a voluntary survey of roughly 30,000 licensed RNs. The 1984, 1988, 1996, and 2000 (but not 1992) surveys ask RNs whether they were employed *as an RN* on the same date one year ago and, for those who answer yes, whether their current employer is the same as one year ago. Using this information, we can construct measures of M^{RN} by metropolitan area based on a considerably larger sample size of RNs in the SRN than in the CPS. New recruits are defined as all current RNs, minus those who were employed as an RN one year ago and have the same employer. The measure M^{RN} is equal to $1-e/R$, where R represents recruits (defined above) and e is the number of RNs employed one year ago as an RN but with a different employer.

There are differences in the SRN and CPS measures. Unlike the CPS, the SRN retains individuals who have changed households or whose household has moved. Also, the SRN uses a one year rather than one month time frame. Finally, the "inside" group e moving from employment in the SRN includes only those from another RN job, with the "outside" group u+n consisting not only of those unemployed and not in the labor force, but also those moving from non-RN occupations. These differences are advantageous. First, the SRN retains RN recruits who have moved in conjunction with their new job. Second, the labor supply elasticity facing hospitals might better be proxied by transitions between RN jobs absent transitions from non-RN to RN jobs. Third, transitions between RN jobs sometimes include brief voluntary breaks in employment. Using the CPS monthly time frame, these latter job transitions will be measured as movements

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[18] Measures of M are calculated from September 1995 forward for all 240 markets. Data for January 1994 through May 1995 are used for those areas that map cleanly into one of these 240 markets. Area identifiers are not available during June-August 1995.

[19] In an initial draft of the paper, prior to use of the SRN, we constructed a measure of predicted M^{RN} from the CPS, the predicted values based on values of M constructed for all female health industry workers and the relationship (using weighted least squares) between this measure (in quartic form) and that of M^{RN} across the largest markets. Predicted M^{RN} was uncorrelated with nursing wage rates across labor markets. Absence of a relationship may reflect the unimportance of new monopsony in nursing labor markets, noise in this measure of M^{RN}, or both.

from unemployment or out of the labor force. Using the SRN one-year retrospective question, brief employment breaks are bypassed, being counted instead as cross-employer transitions and not movements from unemployment or outside the labor force. The SRN does have limitations. Sample sizes in many markets are not large, so measurement error remains a concern (such markets receive lower weight in the analysis). Moreover, response to the SRN is voluntary, and the population of respondents need not be representative of all RNs. SRN sample weights, designed to account for the probability of survey non-response, are used to calculate M^{RN}. Remaining biases not accounted for by the weights could affect values of M^{RN}, although it need not follow that *relative* values of M^{RN} across labor markets are affected.

B. Method of Analysis

The comparison of nursing and non-nursing wages is an essential component of this study. By measuring nursing wages relative to a comparison group within the same labor markets, we control not only for differences in measurable worker characteristics, but also for cost-of-living differences, area-specific amenities and disamenities, unmeasured labor quality specific to an area, and other market-specific wage determinants (e.g., demand shocks) that otherwise are not readily measured.

We provide analysis using both a single-step and two-step estimation process. We first describe a two-step process that has intuitive appeal and provides information necessary for visual representation of the relationship of relative nursing wages and labor market measures of size, hospital concentration, and M. We then turn to a single-step specification.

In the first-step of the two-step process, we estimate micro wage equations from which we obtain area-specific nursing wage differentials for each of the MSA/CMSAs and non-urban state groups. These differentials represent the difference between nursing and non-nursing wages in each labor market, conditional on measurable characteristics that vary across individuals within markets.

A second-step equation relates RN wage differentials to measures of classic and new monopsony. The nursing/non-nursing area differentials (n=240) estimated in the first-step are regressed on market characteristics that vary *across* but not within areas, including market size, hospital concentration, and M. The classic monopsony model predicts that relative nursing wages should be lower in less competitive markets, therefore increasing with respect to hospital (employer) concentration and market size (proxying non-hospital and non-nursing employment opportunities). The new monopsony model downplays the importance of market structure, but argues that the relative nursing wage should decrease with respect to *M*, higher values of *M* reflecting less elastic firm-level labor supply curves. In addition, the second-step equation includes the means of first-step variables, which reduces the likelihood of spurious correlation (Baker and Fortin 2001). Weighted least squares (WLS) estimation is appropriate in the second-step equation. As shown by Saxonhouse (1976), when using regression parameters as a dependent variable, each observation should be weighted by the inverse of that parameter's standard error, thus giving higher weight to markets in which the wage gap estimates are precise.[20]

More formally, first-step wage equations, including RN and control group workers, take the form:

$$ln\mathrm{W}_{\mathrm{itk}} = \alpha + \Sigma\beta_j X_{jitk} + \Sigma\gamma_k AREA_{kit} + \Sigma\varphi_k RN\cdot AREA_{kit} + e_{itk}, \qquad (4)$$

where lnW_{itk} is the natural log of hourly earnings of worker *i* in time period *t* in labor market *k* (where *k*=1, ..., 240), α is the control group intercept for area k=1, *RN* is a dummy variable equal to 1 for RNs, *X* includes individual-level variables (indexed by *j*) affecting nursing and control group wages with β_j the attaching coefficients, and e_{itk} is the error term. *AREA* is a set of 239 dummy variables corresponding to the 191 CMSA/MSAs and 49 non-urban state areas, with γ_k (*k*=2, ..., 240) representing the area wage differentials for the control group relative to the omitted reference area, and φ_k (*k*=1, ... , 240) measuring the 240 area-specific wage differentials for nurses relative to the control group in each area.

Variables included in *X* are years of potential experience and its square, the state unemployment rate in year *t*, and dummies for schooling degree (3), marital status (2), race/ethnicity (3), gender, part-time, union membership, public-sector status, region (8), and year (4). Equation (4) is estimated separately for the 1993-97 "weakening" and 1998-2002 "tightening" periods.

A second-step weighted least squares (WLS) regression can then be estimated with the area-specific nursing wage differentials φ_k as the dependent variable (n=240). Specifically, we estimate:

$$\varphi_k = \Phi + \theta HI_k + \Sigma\psi_s SIZE_{sk} + \zeta M_{\mathrm{k}} + \Sigma\beta' X'_k + \nu_k. \qquad (5)$$

Here, φ_k is the nursing differential for area k estimated in the first-step regression, Φ is the intercept; *HI* the Herfindahl index and θ its coefficient, $SIZE_{sk}$ are dummy variables representing seven metropolitan area size groups (indexed by *s*; non-urban state areas are the reference group) and ψ_s are the corresponding coefficients, *M* the proportion of new recruits from outside employment with ζ its coefficient, and ν_{k} is a

[20] Qualitative results are not sensitive to the choice of weights, being the same using sample size *n* or $\sqrt{n}$ by market.

random error term. X' represents the city-specific means of the individual X's from the first-step equation and β' is the set of coefficients (Baker and Fortin, 2001).

As stated previously, the expectation from classical monopsony is that θ, the coefficient on *HI*, will be negative and that ψ_s, the coefficients on metropolitan area size, will be positive and increasing with respect to size. The new monopsony literature argues that M^{RN} provides a reasonable proxy for the inverse elasticity, implying a negative coefficient ζ.

Wage level equations such as (5) rely on cross-section differences to identify the *long-run* impact of market structure on relative wages. The two-step approach and presence of data for two time periods permit us to estimate the relative wage equation in difference form, with changes in relative RN/control group wages a function of changes in market structure. This approach is likely to identify *short-run* effects; that is, we observe short to medium-run responses of nursing wages to changes in hospital market structure. Under the plausible assumption that wages adjust more quickly than does long-run labor supply (the latter requiring time to acquire RN training and to move across cities), short-run wage change should be large compared to long-run change (e.g., wage effects from merger demand shocks will be moderated as supply becomes more elastic over time).[21] Specifically, we estimate

$$\Delta\varphi_k = \Phi' + \Sigma\beta'\Delta X'_k + \theta'\Delta HI_k + \Sigma\psi_s' SIZE_{sk} + \nu'_k., \qquad (6)$$

where Δ is the difference operator between our two time periods and θ' provides an estimate of the short-run response of nursing wages to changes in hospital concentration. Because our unit of observation is the labor market, changes in relative market size (population) are minimal so $\Delta SIZE$ is not included. Nor do we control for ΔM, small samples causing any measure of ΔM to contain more noise than signal and driving its coefficient toward zero. We include $SIZE_s$, allowing wage changes to vary with market size.

Estimation can be readily reduced to a single-stage framework (albeit with a different implicit weighting structure) through use of appropriate interaction terms letting *RN* and (1-*RN*) represent RNs and the control group, respectively[22]

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[21] Wage change or longitudinal analysis is often used to account for unmeasured fixed effects that may bias coefficients of interest. In this application, we cannot identify important fixed effects; specifically, omitted determinants of RN wages that are highly correlated with hospital concentration. Thus, we interpret the wage level results as providing unbiased long-run estimates and the wage change results as providing short-run estimates.

[22] In the single-step approach, OLS standard errors on variables measured at the labor market level are biased, since errors are correlated across workers within labor markets (Moulton 1990). We present “robust” standard errors corrected for within-market correlation.

$$\ln W_{itk} = \alpha + \Sigma\beta_j X_{jitk} + \Sigma\Omega_s SIZE_{sk} + \Sigma\psi_s RN\cdot SIZE_{sk} + \Gamma HI_k + \theta RN\cdot HI_k + \zeta^{C}(1\text{-}RN)\cdot M^{C}{}_{k} + \zeta^{RN} RN\cdot M^{RN}{}_{k} + \omega_{itk}. \quad (7)$$

The wage equation, estimated by time period, provides a common set of coefficients on the same set of control variables included in (4). RN interaction terms are included on the area size variables, hospital density, and the monopsony measure M (M^{RN} for RNs and M^{C} for the control group). Based on classic monopsony, the parameter θ should be negative and ψ_s should increase with area size. The new monopsony literature predicts that ζ^{RN} should be negative. A negative ζ^{C} would signify broader new monopsony effects among the control group of educated women.[23] In subsequent work, we show results in which RN and control group wages can vary with "cross-group" as well as "own-group" *M*.

Before turning to the evidence, we note an assumption that underlies our measures of relative RN wages within markets. The analysis assumes that unmeasured differences across markets in cost of living, labor quality, working conditions, and area amenities affect nurses and the control group in similar fashion. Such an assumption appears reasonable since the control group selected provides an opportunity cost measure of the long-run alternatives available to nursing personnel, and cost of living and area amenities should be similar for nurses and the control group. Moreover, the control group wage (conditional on measured characteristics) need not provide a perfect measure of the relative wage; rather, our methodology is appropriate as long as errors in the measure of relative wages are not systematically correlated with market size, hospital concentration, or *M*.

A potential bias is possible correlation between unmeasured labor quality and market size. Older literature argued that worker quality is higher in larger cities (Fuchs 1967). More recent studies suggest that productivity and nominal wages are higher in large markets not because of unmeasured worker endowments, but owing to economies of scale, scope, complementarities, and positive externalities, with a higher cost of living necessary to produce equilibrium (e.g., Rauch 1993; Quigley 1998; Glaeser and Maré 2001). Our relative wage measure properly controls for unmeasured labor quality or productivity if these vary with respect to market size similarly for both groups of workers. If nursing quality rises faster with respect to size than does control group quality, our test would be biased toward the finding of classic monopsony (i.e.,

[23] We calculate both M^{C} for the control group of professional women and a measure of M for the larger sample of all women with some college or above. These measures had nearly identical means, but M^{C} varied considerably across markets due to small samples. We include as our measure of M^{C} its *predicted* value based on a WLS cross-market regression of M^{C} on M for all college women. The intercept was close to zero and slope close to unity.

of rising relative nursing wages with respect to size). If nursing quality rises more slowly, our test is biased against the finding of monopsony. Since M^{RN} is largely uncorrelated with market size, unmeasured productivity differences are less of a concern for this measure.

V. Evidence

A. Descriptive Results

Table 1 provides means of RN wages for the hospital RN sample nationwide and by market size, both for the "weakening" 1993-1997 and "tightening" 1998-2002 periods. In the earlier period, the relative nursing wage is 1.43, a 43% wage advantage compared to the college-educated female control group (and 1.37 when non-hospital RNs are included). Relative wages show no evidence of increasing with market size. If anything, they decrease with size, inconsistent with classic monopsony, but similar to results seen for an earlier period in Hirsch and Schumacher (1995). For the latter 1998-2002 period, the relative wage is 1.38 and, again, there appears to be a negative relationship with market size.

Table 1 also provides the mean number of hospitals, the number of hospital systems, and hospitals per 100 square miles across all markets by market size.[24] The table also shows the Herfindahl index at the hospital and system levels. Large urban areas have more hospitals and employers. However, the number of hospitals fell between 1993 and 2000, overall and in every city size category. For the entire U.S., the average number of hospitals fell by about 10 hospitals over the period. The mean number of hospital systems decreased by 14, reflecting the hospital consolidation occurring over the period. The number of hospitals per square mile, the density measure used by Hirsch and Schumacher (1995), increases with market size, but has declined between 1993 and 2000, most noticeably in larger markets.

Figures 1a and 1b provide a more complete representation of the relationship between the RN/control wage and market size (the log of population). We normalize the relative wage at zero, with the value shown on the vertical axis being each market's relative wage minus the unweighted mean across all 240 markets. For both the 1993-97 and 1998-2002 periods, observations are clustered around zero for markets of all sizes, with a hint of a weak negative relationship between the relative RN wage and size. As expected, small sample sizes of RN's in less-populated markets lead to higher dispersion across smaller than larger markets. Our initial evidence is that there is no sign of a positive relationship between relative

[24] Means of the hospital statistics in Table 1 are compiled across the CPS sample of hospital RNs. This is equivalent to compiling weighted means across labor markets, with the sample of size of RNs as weights.

nursing wages and market size in weakening or strengthening labor market periods. The relationship is examined subsequently, allowing for covariates within a regression framework.

The principal route through which monopsony is believed to impact nursing wages is through the market power of hospitals. Figures 1c and 1d show the relationship between the RN/Control group wage differentials across markets (normalized to zero) and the Herfindahl concentration index for hospitals in each market, as measured by the average daily census. There is no support for the negative relationship predicted by classic monopsony theory. It can readily be seen that the relationship is flat, indicating no substantive correlation (absent covariates) between relative wages and employer concentration.

Figures 2a and 2b show the relationship between hospital concentration (measured by *HI*) and market size, measured by population. Three patterns are evident. First, concentration decreases with respect to size. Second, concentration is greater when calculated on a system rather individual hospital basis. And third, the greater concentration using the system measure is more evident in 2000 than in 1993, as expected given merger activity during the 1990s.

Descriptive data on the new monopsony measures of labor market competition are provided in Table 2.[25] We focus first on M^{RN}, the proportion of new recruits from outside employment for hospital RNs. We then turn to the CPS measures for the college-educated female control group and several broader worker groups. The mean of $M^{RN\text{-}CPS}$ is .418, indicating that 42% of hospital RN recruits come from outside the labor market, with 58% coming from employment elsewhere.[26] Among those coming from outside employment, .137 are from *u* and .281 from *n*; among the .582 coming from employment, .349 have moved from an RN hospital job, .082 from an RN job outside a hospital, and .151 from a non-RN occupation (these figures are not shown in Table 2). $M^{RN\text{-}SRN}$, used in subsequent analysis, has a mean of .326, indicating that 67% of hospital RNs recruits in the SRN were employed elsewhere as an RN one year ago (we know nothing more about their previous job). The lower value of $M^{RN\text{-}SRN}$ than of $M^{RN\text{-}CPS}$ arises from the one-year time frame in the SRN as compared to one-month in the CPS, thus allowing nurses who take a short time off between nursing jobs to be counted as moving from employment rather than non-employment. Working in the opposite direction is the fact that the non-employment category in the SRN includes RNs previously employed as non-RNs. We use $M^{RN\text{-}SRN}$ to measure *cross-market* differences in new monopsony owing to its

[25] Unlike Table 1, measures of *M* are presented for the entire U.S. and not by market size. *M* varies little by size.
[26] The corresponding figure for non-hospital RNs is *M* = .436.

greater sample size of recruits (7,834 versus 645) and the preferable one-year time frame. $M^{RN\text{-}CPS}$ at the *national* level is useful because one can identify (as above) features of the prior as well as current job and because it can be compared to CPS values of M calculated for alternative worker groups.

The new monopsony literature emphasizes that positively sloped labor supply curves and employer power exist independent of market structure or classical monopsony measures. Neither $M^{RN\text{-}SRN}$ nor M^C (or other CPS measures of M) is found to vary with respect to market size. Nor is $M^{RN\text{-}SRN}$ highly correlated with measures of hospital concentration. These weak correlations permit a cleaner statistical delineation to be made between the influences of classic and new monopsony on wages.

An important finding is that $M^{RN\text{-}CPS} < M^C$ (.42 versus .51), implying that the hospital market for RNs is *less* monopsonistic than the market for the college-educated female control group. Table 2 also provides CPS measures of M for all women and men, and for women and men classified by low and high education (low being high school or below). Not only is M for hospital RNs lower than among the control group, it is also substantially lower than the .58 value among all women workers and lower than values of M calculated separately for high and low education women. The value of M for RNs is also lower than among all men (.42 versus .47), being substantially lower than the .52 value for less-educated men and similar to the .40 value for more highly-educated men. These comparisons are suggestive. First, new monopsony is more likely to affect low-skill than high-skill workers and women more than men, a point made by Manning (2003). Second, whatever one thinks of new monopsony, the mobility of hospital RNs across jobs makes them a questionable candidate for a prototypical worker group harmed by employer power. These issues are examined in Section C.

B. Tests for Classic Monopsony

In this section we examine the relationship of relative RN wages with measures of classic monopsony – labor market size and employer concentration. Table 3 displays regression results from the single step approach (equation 7) and from the second-step WLS approach (equation 5). The top panel of Table 3 shows results for 1998-2002, while the bottom panel does so for 1993-1997. Included in the table are coefficients on the city size dummies and the Herfindahl index, each of these interacted with RN when using the single-step approach.

The size coefficients (Ω_s) shown in columns 1 and 2 measure market size wage gaps among the control group, relative to the omitted base of workers in non-metro state areas. As widely recognized, wages increase substantially with size. For example, in 1998-2002 wages are .205 log points (22.8%) higher in metropolitan areas with 1-2 million than in non-metro state areas (column 1). The wage advantage for those in the largest metropolitan areas (5 million plus) is .380 (46.2%). The nominal wage advantages seen in large cities increased in the late 1990s, the equivalent gaps being smaller in the earlier 1993-97 period (.149 and .284 log points, respectively).[27]

The interaction terms of RN with market size (ψ_s), shown in columns 1′ and 2′, measure the wage differential for RNs relative to the college-educated control group in each market size category. Classic monopsony implies increases in ψ_s with respect to size. As seen in the table, this is not the case in either time period. If anything, the RN/control group wage declines as size increases, relative nursing wages in 1998-2002 being around .12 log points lower in cities of 1-2 and 2-5 million than in non-urban state areas. The comparable figure for the largest metropolitan areas is -.18 log points. The same qualitative pattern can be seen for 1993-97. Columns 3′ and 4′ present the WLS second-step results. Apart from small MSAs of 100-200 thousand, little difference is found in relative RN to control group wages across markets of various sizes in 1993-97. In contrast, 1998-2002 results clearly indicate lower relative RN wages in large metropolitan areas, with point estimates similar to those seen in the single-step equations.

It is not clear what explains *lower* relative wages for RNs in large cities. A possible explanation is that it reflects relative homogeneity in (unmeasured) nursing quality across markets, coupled with increasing quality by city size among the control group. Based on literature cited previously, we are skeptical that such a pattern of unobserved quality is strong. The flip side of such an explanation would be relative homogeneity in the control group work force, with unmeasured nursing quality decreasing with city size. We cannot dismiss such a conjecture out of hand, but would like to see evidence. A second type of explanation focuses on the demand side. If large-city hospitals are more financially constrained than in other markets, we might expect to see lower wages, higher turnover, and more vacancies in large-city hospitals. Whatever the explanation, the pattern seen is opposite of that predicted by classic monopsony.

[27] Theory predicts that utility for equally skilled workers is equivalent (at the margin) across labor markets. Wages do not rise fully with cost of living, however, since area amenities (good climate, etc.) increase land prices and decrease wages, all else the same. DuMond, Hirsch, and Macpherson (1999) find that wages increase roughly half as fast as measured cost-of-living across U.S. urban areas. If wages are (inappropriately) adjusted fully for cost-of living, indexed wages *decrease* with respect to city size.

A more direct test of classic monopsony is to examine the relationship between relative nursing wages and the Herfindahl index, holding market size constant.[28] These results are also seen in Table 3. Estimates of θ (*RN•HI* interaction term coefficients), seen in columns 1′ and 2′, should be negative if wages increase with market competitiveness.[29] For the 1993-97 period, estimated values of θ are -.051 and -.035 using the non-system and system *HI*, respectively, values that are insignificant and tiny in magnitude. For the more recent 1998-2002 period, we obtain positive values of .011 and .065, respectively.[30] Second-step WLS results are shown in columns 3′ and 4′ – estimates of the *HI* coefficients are effectively zero. These results are consistent with Hirsch and Schumacher (1995), who for an earlier period fail to find a relationship between RN wages and hospital density, an imperfect proxy for concentration.

In short, it is difficult to find wage evidence supporting the presence of classic monopsony in nursing labor markets for the long run. Moreover, evidence is lacking for multiple periods with different product and labor market conditions. However, we find suggestive evidence that competition affects *short-run* wage adjustments. Figure 3 shows the simple correlation between changes in area wages ($\Delta\varphi_k$) and system HI (ΔS-HI$_k$). There appears to be a small inverse relationship between the change in the area wage differential and the change in system HI, suggesting that relative wages fall when hospitals become more concentrated. The top section of Table 4 presents results from estimation of equation (6), which regresses the change in relative RN/control group wages between 1993-97 and 1998-2002 on changes in the Herfindahl index, along with other covariates. Here we find evidence consistent with monopsonistic employer power. Coefficients on ΔHI are negative, significant, and robust to alternative specifications (lines 2-6), indicating that the substantial merger activity during the 1990s was associated with slower RN wage growth. Using the system measure, the coefficient on $\Delta S\text{-}HI_k$ is about -.40, implying a change in concentration of .05 (roughly the change in *HI* for RNs at the means, as seen in Table 1) is associated with a .02 or 2% relative RN wage decrease. This is not a huge change, but it is not trivial. For the quartile of labor markets with the largest increase in *HI* (compared to the lowest), the relative RN wage decreases by

[28] The city size results shown in Table 2 hold constant hospital concentration, as measured by *HI*. Because size coefficients absent inclusion of *HI* are highly similar, these results are not shown.
[29] A positive coefficient would be consistent with rent sharing if hospitals facing less product market competition were both able and willing to pay higher relative wages.
[30] Similar results are obtained when *HI* is calculated using beds instead of patient days. If metro size interactions with *RN* are excluded, θ estimates are increasingly positive (since *HI* and size are inversely correlated), inconsistent with classical monopsony.

.034 log points (not shown). Coefficient estimates are smaller using the hospital-based measure of ΔHI.

What do we make of this evidence? The results suggest that in the short run, employer concentration matters and impacts relative wages. Less certain is our ability to distinguish the effects of changes in oligopsonistic employer power from those resulting from market-wide shifts in labor demand. Might wage decreases in markets with increases in hospital concentration simply reflect a decline in patient and RN demand? We cannot know for sure. When we include a variable measuring the change in the log of patient days (line 4), the coefficient on ΔHI is affected little.[31]

Our interpretation of the evidence is that RN wage changes have occurred in response to changes in hospital concentration, consistent with classical monopsony. The absence of such evidence in wage *level* equations over different time periods (Table 3), however, indicates that wage differences associated with employer consolidation are not sustainable.[32] Over time, there is sufficient RN mobility within and across labor markets such that wage differentials associated with market structure are not sustained. In the middle panel of Table 4, we provide estimates from employment change equations. Employment growth is unaffected by changes in hospital concentration.

C. Tests for New Monopsony

Table 5 displays regression results testing for new monopsony using Manning's proxy of the inverse elasticity. The new monopsony literature predicts that wages are negatively related to M, the proportion of new recruits hired from outside employment rather than from other employers. We examine the response of wage rates to M^{RN}, the proportion of hospital RN new hires from outside RN employment, and M^{C}, the proportion of new hires from outside employment among the college-educated female control group. We show estimates using individual wage observations for RNs and the control group and estimates from a second-step RN/control group WLS wage regression across 238 U.S. labor markets (excluded are two small markets where M^{RN} could not be calculated).

The top panel provides wage equation results for hospital RNs only, first with M^{RN} included (plus

[31] An additional control variable that we included (for the subset of markets for which it was available) was the penetration of HMOs. If anything wage increases were higher in markets with large HMO shares. The coefficient on a change in HMO penetration was effectively zero. Note that most HMO growth occurred prior to 1998-2002 (our latter period), so noise-to-signal is likely to be high using an HMO change variable. We thank Laurence Baker for providing the HMO data (see Baker 1995; Baker and Phibbs 2002).

[32] As stated earlier, we know of no omitted fixed effect that is driving the wage level coefficient on *HI* to zero. Such a wage determinant would need to be correlated with relative RN wages and HI in opposite directions, but not strongly correlated with other covariates.

controls, listed in the table note) and then including M^C as well as M^{RN}. The coefficients ζ^{RN} are effectively zero, indicating no relationship between the wages of individual RNs and the market level of M^{RN}, with or without control for M^C. Thus, initial evidence rejects the thesis that new monopsony is an important determinant of nursing wages.

In the middle panel of Table 5, we estimate micro wage equations for the college-educated female control group. Here we find evidence supporting the approach taken by the new monopsony literature, control group wages varying inversely with M^C, but unaffected by values of M^{RN}. While the sign (and statistical significance) is consistent with new monopsony, the magnitude is not particularly large, a coefficient of ζ^{C} = -.10 implying that the wage difference between labor markets with *M* equal to .60 versus .40 is about .02 log points or 2% (as seen in Table 2, .60 and .40 roughly reflect the means of *M* for low-educated women and high-educated men).

The bottom panel of Table 5 presents second-step WLS results regressing the relative RN wage by market on measures of *M* (and all variables included previously in column 3′ of Table 3). No significant relationship exists. But the estimate of ζ (the coefficient on M^{RN}) is negative, consistent with the new monopsony literature. A value of ζ = -.04 implies that a .20 difference across markets in M^{RN} would be associated with an .008 (less than 1%) difference in the RN wage. More substantial (but insignificant) is the positive coefficient on M^C; that is, we observe higher relative RN wages where *control group* wages are negatively affected by a high inverse supply elasticity in the non-RN labor market.

Although our principal interest is testing for monopsony – classic and new – in the market for RNs, we also examine the effects of the new monopsony measure *M* on the wages of workers throughout the labor market. In Table 6 we present wage regression results for the period 1998-2002 among non-student women and men, ages 20 to 64, classified by low and high education (high school or less versus more than high school). Values of *M* across our 240 labor markets are calculated by gender and education using all rotation groups from the monthly CPS for the 1994-2002. The results reveal a negative effect of *M* on the wages of women, but this effect is concentrated among women with a low level of education. In the regression run separately for the low education group, the coefficient on *M* is -.180. A change in *M* from, say .5 to .6 (roughly a one standard deviation change and the difference between high and low educated women) is associated with about 2% lower wages. Thus, for less-educated women, for whom values of *M* are relatively high, monopsonistic employer power has a modest impact on wages.[33] Results for men also produce negative coefficients on *M*, but these are close to zero (and insignificant).

Our interpretation of these results is that whatever one thinks of the new monopsony literature, its effects in nursing labor markets are modest. This conclusion is based first on evidence of at most a weak relationship between wages for RNs and a measure of new monopsony. Second, new hires in nursing are far more likely to move from employment at other firms than are new hires outside of nursing, implying that the RN labor supply facing hospitals is more elastic than is labor supply facing firms in general.

That nursing may be less prone to monopsonistic power than are other occupations should not be surprising. Few of the skills acquired by RNs in school and on the job are hospital (firm) specific, making movement of nurses across hospitals or into non-hospital employment highly feasible. Firm-specific attachment that leads to upward-sloping labor supply seems no more likely for hospital RNs than for workers in other occupations. Information barriers typical in labor markets with highly heterogenous employers seem unlikely among hospital RNs. Nursing, however, continues to be held up as a prime example of a monopsonistic labor market. The "new" monopsony literature prominently cites the paper by Staiger, Spetz, and Phibbs (1999), which finds evidence of an upward sloping supply curve of RNs in California V.A. hospitals, as a prime example of the type of study required to make the case for monopsonistic power (Bhaskar, Manning, and To, 2002; Manning, 2003). We do not dispute that labor supply curves facing firms (hospitals) are upward sloping – we do not accept that this is sufficient evidence to show that there are low wage outcomes and the exercise of monopsonistic power.

D. Monopsony Power, Staffing, and Effort

Currie et al. (2002) have extended the monopsony model to allow employers to set not only wages and employment, but also effort. Rather than leading to lower wages, monopsony may instead lead to higher required levels of "effort", thus lowering worker satisfaction or utility just as would lower wages. Stated differently, lower staffing decreases RN satisfaction owing to greater demands on their time and a poorer quality of patient care. The authors examine California nursing data over the 1989 to 1999 period, treating

[33] It is reasonable to ask if the relationship for less-educated women reflects monopsonistic power or if *M* is instead a proxy for some other wage determinant. One possibility is that workers with low *unmeasured* skill (i.e., holding constant measured human capital) are less attached to the labor force. Labor markets with new recruits moving disproportionately from outside employment (i.e., a high *M*) may have low levels of unmeasured skills. Observed wages may reflect these low unmeasured skills and not employer power.

takeovers by hospitals chains as increases in product and labor market concentration. They find that nurses see few declines in wages following takeovers, but realize increases in the number of patients per nurse. They find increases in employment, however, and conclude that their results are more consistent with a contracting model than with monopsony or the standard competitive model.

We examine the possibility that monoponistic power in nursing labor markets is associated not with lower wages but with higher required work effort, as measured by the staffing ratio of RNs to patients, which we designate as *S* (this is the same measure used by Currie et al., 2002). Specifically, we examine whether *S* increases with market size and decreases with respect to *HI* and M^{RN}.

Figures 4a and 4b examine the evidence on staffing. Shown in Figure 4a is the relationship between *S* across all hospitals within a metropolitan area (compiled from 1990 data published in *Hospital Statistics*) and market size, measured by the log of population. As evident from the scatter plots, there is no meaningful relationship between staffing and size. The estimated slope of a trend line is .021 with a t-statistic of 1.19. The positive sign is the opposite of that predicted by the monopsony model and the magnitude of the relationship is trivial. Figure 4b shows the relationship between staffing and hospital concentration. There is no meaningful relationship between *S* and *HI*. Nor is a significant relationship between *S* and either market size or *HI* found using regression analysis (not shown).

The new monopsony model suggests that the *S* should decrease (i.e., required effort should increase) as the nursing monopsony measure M^{RN} increases. Figure 4c indicates that there is little relationship using 2000 hospital staffing data for *S* and the 1994-2002 measure of M^{RN}. Fitting a regression line to these points indicates no meaningful relationship, a slope estimate of -.171 with a t-statistic of -1.21 (similarly, for 1993, we obtain a slope estimate of -.193 and t-statistic of -1.55). If one focuses on the data points with "reasonable" values of M^{RN}, say from .3 to .5, one sees large variability in mean staffing ratios among markets with similar levels of M^{RN}.

Although we do not find a long-run cross sectional relationship between staffing and measures of monopsony, we do find a significant negative relationship between *changes* in *S* between 1993 and 2000 and changes in the system Herfindahl index (see Table 4, bottom panel). We find that staffing falls (required effort increases) following hospital consolidation. These staffing results comport well with Currie et al. (2002), who find increased staffing ratios following California hospital mergers. Our interpretation of this relationship is similar to that given to the wage change results – changes in staffing are likely to reflect short-run response to market structure changes that are unlikely to survive in the long run.

It is plausible that adjustments to monopsonistic power might take place through staffing and work effort as well as the wage. But based on cross-sectional labor market averages, we have found no supporting evidence for a long-run effort adjustment (as measured by employment per patient) that is correlated with labor market measures of either classic or new monopsony. The staffing change equations, however, suggest that monopsonistic outcomes may be observed in the short run.[34]

VI. Conclusion

Nursing is portrayed as a prototypical example of a monopsonistic or oligopsonistic labor market. Evidence for employer power in nursing markets, however, is very much mixed. This paper attempts to reconcile this disparate literature. Using a large micro-level CPS data set on RNs, we first search for classic monopsonistic wage effects during 1993-1997 and 1998-2002, the former period a time of weakening labor markets for RNs after years of rising wages, and the latter a strengthening period. Segmenting RNs and a control group of college-educated women into 240 urban and non-urban labor markets, no evidence is found for either period showing that relative nursing wages increase with market size or hospital concentration. As in previous studies, it is difficult to find long-run nursing wage outcomes consistent with the classic monopsony model. In contrast to prior studies, we find evidence consistent with short-run monopsonistic outcomes, increases in hospital concentration being associated with slower RN wage growth and lower staffing ratios.

We also test the new monopsony model. We calculate a measure M representing the proportion of new hires within each metropolitan or non-metro state area who come from outside the labor force or from unemployment rather than moving from another job, a measure offered in the new monopsony literature as a proxy for the inverse supply elasticity. Values of M are found to be substantially *lower* for hospital RNs than for women (or men) as a whole, reflecting the fact that nursing skills are readily transferable across employers and that there exist multiple employers for most nurses. Empirical results indicate that there is

[34] State laws or collective bargaining agreements could constrain downward adjustments in RN employment (i.e., upward increases in required effort) through mandated minimum staffing ratios. A May 2003 Senate proposal from Senator Inouye would impose national minimum requirements on RN staffing, but not legislate a specific ratio. The legislation calls for ratios based on number of patients and the level and intensity of care, recommendations from registered nurses, and the preparation and experience of the caregivers (American Nurses Association 2003). For discussion of the expected effects from staffing requirements adopted in California, see Spetz (2001).

little or no relationship between RN wages and *M*, but do uncover a weak negative correlation between wages and *M* for the college-educated female control group, and a more substantial negative relationship between *M* and the wage of less-educated female workers. In short, we find little evidence that nursing wages are affected by the market power of employers, either of the classic or new monopsony type.

Two principal conclusions follow from our analysis. First, evidence of upward-sloping labor supply is not sufficient to infer monopsonistic outcomes. We do not dispute that workers are often immobile over the short and medium runs and that employers face upward-sloping supply. It does not follow that differences in labor supply elasticities necessarily generate large or systematic differences in wages. Before concluding that monopsony is important, one should measure outcomes. Second, whatever one thinks about the importance of monopsony, classic or new, hospital RNs are not a good example given their relatively high mobility across employers. Absent evidence on monopsonistic outcomes in nursing labor markets, economists should look elsewhere for a prototypical example of monopsony.

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Figure 1a: 1993-1997 RN/Control Wage Differential by City Size

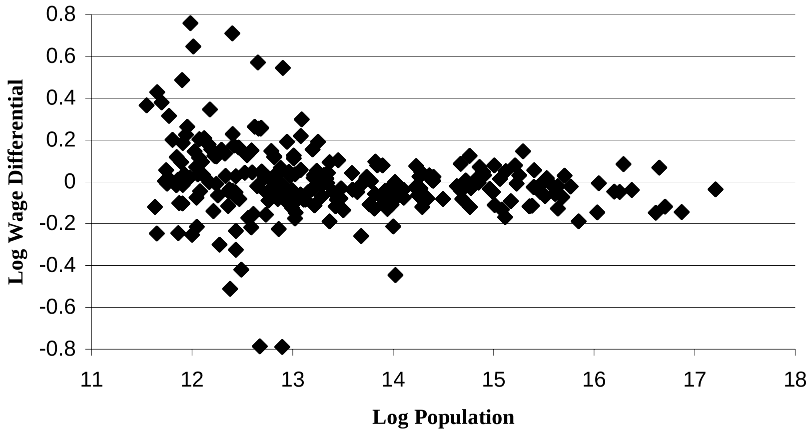


Figure 1b: 1998-2002 RN/Control Wage Differential by City Size

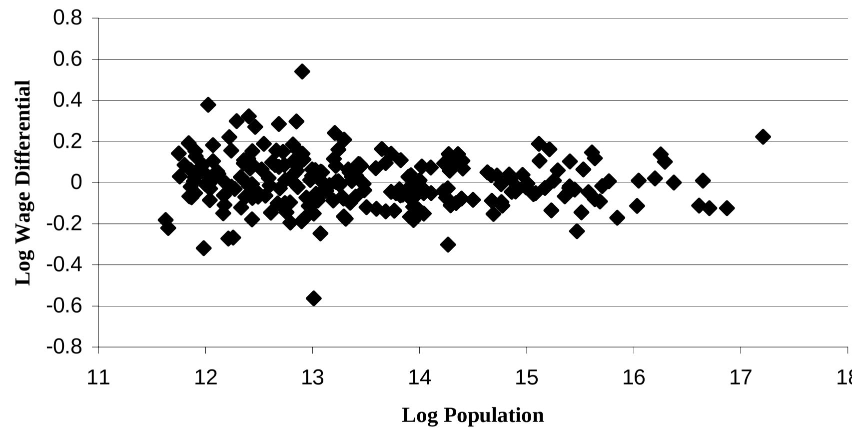


Figure 1c: 1993-1997 RN/Control Wage Differential by HI

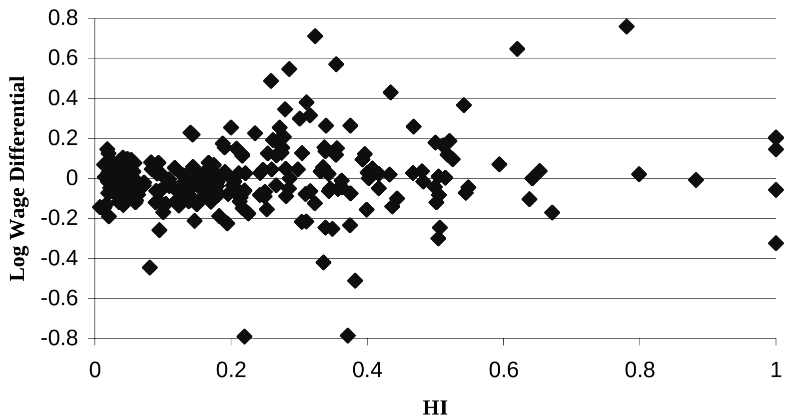
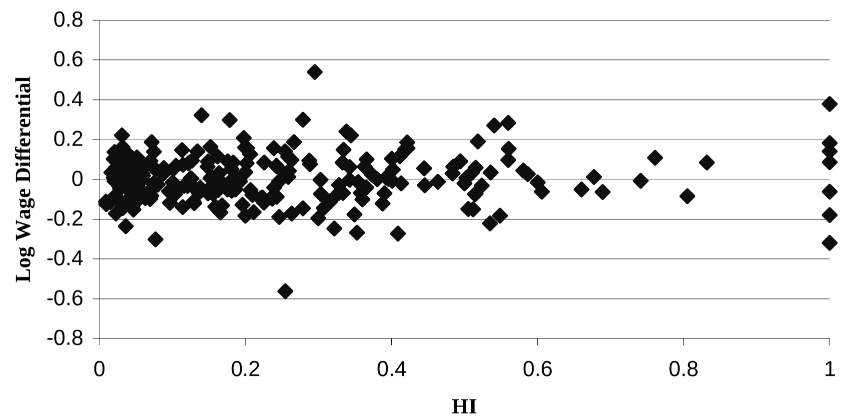


Figure 1d: 1998-2002 RN/Control Wage Differential by HI



Shown in each figure is the RN/control group log wage differential for each of 240 market areas (191 MSA/CMSAs and 49 non-urban state groups), obtained from step one of the two-step estimation procedure described in the text. Relative wage differentials are normalized at zero, with the value shown on the vertical axis being each labor market's relative wage minus the unweighted mean across all markets. HI shown in Figures 1c and 1d is the Herfindahl concentration index based on individual hospital shares of market daily census.

Figure 2a: Hospital and System Herfindahl Indexes 1993

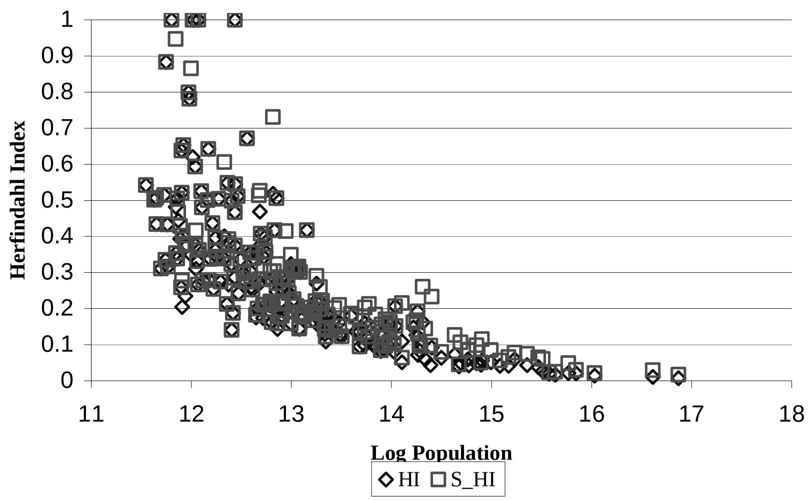
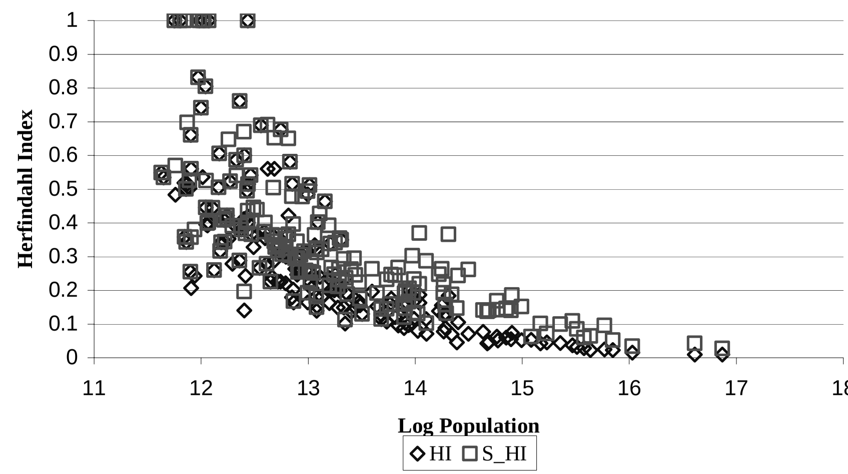
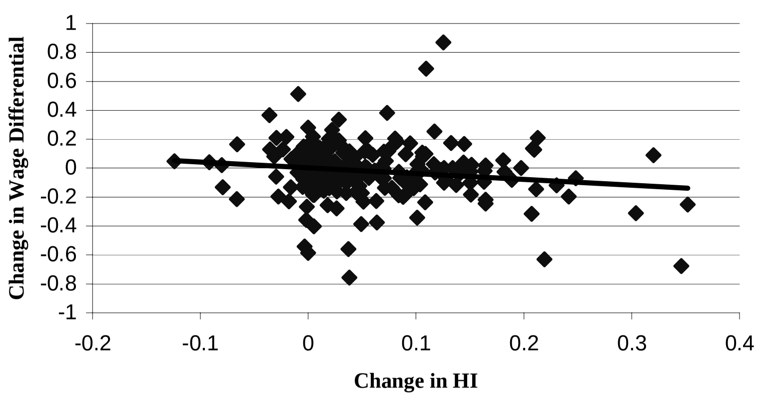


Figure 2b: Hospital and System Herfindahl Indexes 2000



The Herfindahl concentration indexes, *HI* and *S_HI*, are compiled from the 1993 and 2000 Annual Survey of Hospitals using the average daily census. *HI* is based on individual hospital shares in each market; *S_HI* is based on hospital system shares in each market.

Figure 3: Change in Area Wages and the Change in System Herfindahl



Data from the 1993 and 2000 Annual Survey of Hospitals are used to construct changes in the system Herfindahl Index. Changes in market RN/control group log wage differentials are obtained from the first step wage equations for 1993-97 and 1998-2002.

Figure 4a: RN/Patient Staffing By Market Size, 2000

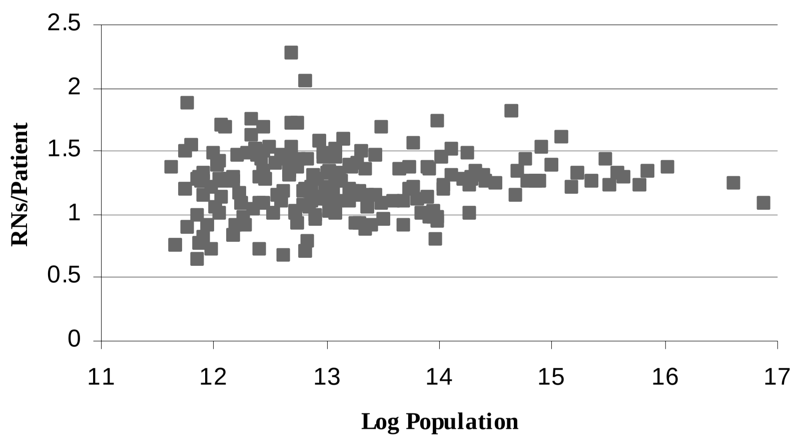


Figure 4b RN/Patient Staffing by Hospital Concentration, 2000

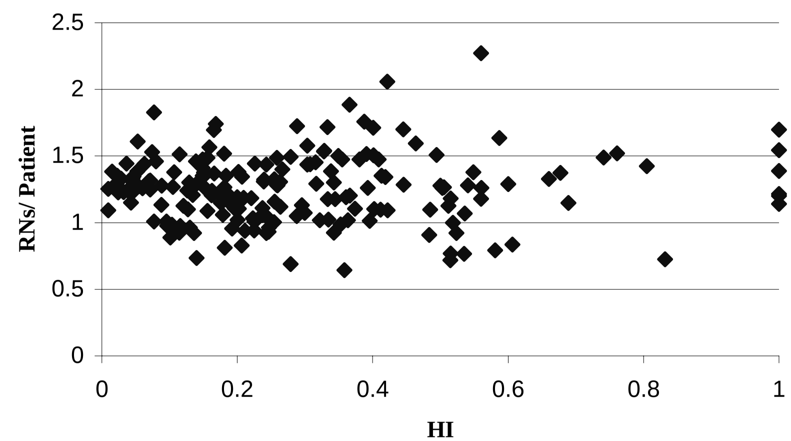
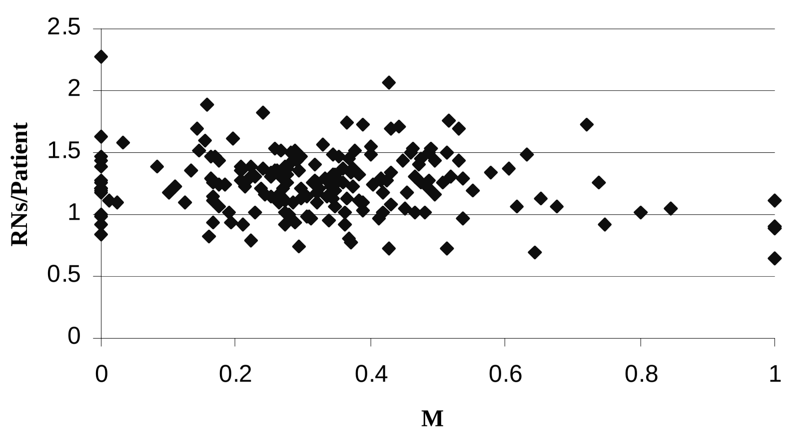


Figure 4c RN/Patient Staffing by New Monopsony, 2000



RNs/Patient is the ratio of the number of RNs in the market to the market-wide adjusted average daily census from 2000 Annual Survey of Hospitals.

Table 1: Mean Nursing and Hospital Characteristics by Metropolitan Area Size

	All	Non-urban State	100-200K	200-300K	300-500K	500K-1M	1-2M	2-5M	5M +
1998-2002:									
Wage, Hosp RNs (2002$)	$23.24	$20.79	$21.85	$23.77	$22.19	$22.55	$23.01	$24.15	$26.54
Wage, All RNs (2002$)	$22.30	$19.79	$20.55	$23.05	$21.55	$21.99	$22.48	$23.16	$25.43
Hosp RNs/Control Wage	1.38	1.56	1.52	1.52	1.43	1.45	1.36	1.35	1.29
# Hospitals	49.10	57.92	3.19	3.87	6.06	10.23	16.20	32.61	108.93
# Hospital Systems	31.88	42.54	2.77	3.51	4.95	8.13	10.39	18.80	64.97
Hosp/100 Sq miles	0.54	0.14	0.31	0.29	0.34	0.55	0.58	0.67	1.08
Hosp/100K Pop	1.72	3.04	1.98	1.56	1.51	1.40	1.15	1.07	0.97
HI–Avg daily census	0.123	0.068	0.553	0.426	0.294	0.168	0.118	0.054	0.019
System HI–Avg daily census	0.169	0.092	0.605	0.454	0.340	0.219	0.208	0.132	0.052
HI–Beds	0.117	0.060	0.542	0.414	0.276	0.157	0.116	0.051	0.018
System HI - Beds	0.165	0.089	0.595	0.445	0.322	0.212	0.208	0.126	0.053
1993-1997:									
Wage, Hosp RNs (2002$)	$22.31	$20.16	$22.21	$22.31	$21.64	$21.13	$22.10	$22.90	$25.19
Wage, All RNs (2002$)	$21.48	$19.37	$21.19	$21.33	$20.79	$20.38	$21.51	$22.37	$24.15
Hosp RNs/Control Wage	1.43	1.54	1.71	1.46	1.53	1.44	1.44	1.40	1.36
# Hospitals	58.81	59.36	3.67	4.81	6.61	10.90	16.76	38.33	125.28
# Hospital Systems	46.05	46.15	3.47	4.72	5.97	9.58	13.37	27.05	98.52
Hosp/100 Sq miles	0.66	0.16	0.39	0.39	0.40	0.58	0.62	0.79	1.30
Hosp/100K Pop	1.75	2.78	2.33	1.92	1.67	1.52	1.19	1.24	1.13
HI–Avg daily census	0.098	0.062	0.457	0.352	0.263	0.150	0.111	0.049	0.016
System HI–Avg daily census	0.118	0.082	0.489	0.364	0.283	0.170	0.142	0.076	0.030
HI–Beds	0.093	0.059	0.443	0.343	0.245	0.143	0.106	0.045	0.015
System HI–Beds	0.114	0.080	0.477	0.354	0.265	0.164	0.137	0.075	0.029
#MSA/CMSAs [state groups]	240	[49]	33	31	46	32	27	13	9

RN and control group wage data are from the 1993-2002 CPS ORG earnings files. Hospital data are from the AHA Annual Survey of Hospitals for the years 1993 and 2000. The 191 metropolitan areas shown applies to September 1995 forward; the text describes metropolitan definitions for earlier periods. The 49 non-urban state areas include all workers not residing in one of the designated metropolitan areas. The Herfindahl index, *HI*, is calculated alternatively using the average daily census and number of beds, and based on individual hospital and system market shares, the latter counting as a single entity all hospitals within a system in a market area.

Table 2: New Monopsony Measure by Worker Group

Hospital Registered Nurses:	
SRN Measure $M^{RN\text{-}SRN}$	0.326
CPS Measure $M^{RN\text{-}CPS}$	0.418
Female Control Group:	
M^C	0.512
Women:	
All	0.581
Low Education	0.640
High Education	0.528
Men:	
All	0.466
Low Education	0.523
High Education	0.399

With the exception of $M^{RN\text{-}SRN}$ the new monopsony measures are compiled from the 1994-2002 CPS (all rotation groups). *M* measures the proportion of new hires from outside employment, a proxy for the inverse supply elasticity. The SRN measure of *M* is compiled from the National Sample Survey of Registered Nurses (SRN) for 1984, 1988, 1996, and 2000. Low education includes all workers with a high school degree or less; high education includes all with a degree beyond high school. See text for further discussion.

Table 3: RN and Control Group Wages, Market Size, and Hospital Concentration – Searching for Classic Monopsony

	1998-2002					
	1	1′	2	2′	3′	4′
Metro 100-200K	.017	-.032	.043	-.060	-.014	-.033
	(.033)	(.042)	(.034)	(.044)	(.041)	(.040)
Metro 200-300K	.073	-.046	.094	-.067	-.036	-.049
	(.036)	(.045)	(.035)	(.045)	(.035)	(.034)
Metro 300-500K	.109	-.068	.122	-.081	-.050	-.053
	(.025)	(.031)	(.025)	(.031)	(.029)	(.029)
Metro 500K-1M	.130	-.078	.134	-.084	-.061	-.068
	(.020)	(.022)	(.020)	(.022)	(.028)	(.028)
Metro 1-2M	.205	-.112	.202	-.114	-.093	-.094
	(.019)	(.019)	(.019)	(.019)	(.030)	(.030)
Metro 2-5M	.262	-.122	.257	-.121	-.085	-.088
	(.028)	(.019)	(.028)	(.019)	(.036)	(.035)
Metro 5M and over	.380	-.178	.375	-.175	-.151	-.158
	(.028)	(.024)	(.028)	(.024)	(.044)	(.044)
Herfindahl HI	.133	.011	--	--	-.016	--
	(.054)	(.079)			(.060)	
System Herfindahl HI	--	--	.078	.065	--	.015
			(.053)	(.081)		(.055)
	1993-1997					
	1	1′	2	2′	3′	4′
Metro 100-200K	-.046	.090	-.039	.084	.126	.129
	(.033)	(.035)	(.033)	(.035)	(.043)	(.043)
Metro 200-300K	.051	-.008	.059	-.014	.018	.021
	(.055)	(.039)	(.054)	(.038)	(.037)	(.036)
Metro 300-500K	.044	-.001	.048	-.005	.038	.039
	(.024)	(.027)	(.023)	(.027)	(.030)	(.030)
Metro 500K-1M	.096	-.072	.098	-.074	-.021	-.021
	(.019)	(.019)	(.019)	(.018)	(.029)	(.029)
Metro 1-2M	.149	-.078	.148	-.079	.017	.016
	(.018)	(.018)	(.018)	(.018)	(.031)	(.031)
Metro 2-5M	.202	-.089	.200	-.089	.037	.036
	(.026)	(.022)	(.026)	(.022)	(.037)	.036
Metro 5M and over	.284	-.137	.283	-.137	-.007	-.008
	(.026)	(.028)	(.026)	(.028)	(.040)	(.040)
Herfindahl HI	.147	-.051	--	--	.016	--
	(.054)	(.054)			(.073)	
System Herfindahl HI	--	--	.127	-.035		.008
			(.051)	(.054)		(.065)

Columns 1, 1′, 2, and 2′ show results from single-step wage equations including RNs and the control group. Robust standard errors are in parenthesis. Columns 1 and 2 present coefficients for the control group and columns 1′ and 2′for corresponding RN interaction terms. Other variables included are schooling dummies, experience and its square, dummies for part-time status, race and ethnicity (3), gender, marital status (2), union status, public employment, region (8) year dummies, RN, and RN-year interactions, and the average state-by-year unemployment rate. Columns 3′ and 4′ show second step WLS results, where the dependent variable is the first-step estimate of the RN-to-control group wage by area, with the inverse of first-step standard errors as weights. Second-step regressions include labor market means of the first-step variables. Sample sizes in the single-step procedure are 71,446 in 1993-97 and 68,123 in 1998-2002. Sample sizes (market areas) in 3′ and 4′ are 255 in 1993-97 and 240 in 1998-2002.

Table 4: The Effect of Changes in Hospital System Concentration on RN Wage, Employment, and Staffing Changes, 1993 to 2000

	All Markets	CMSA/MSAs only
ΔS-HI Coefficients from Log Wage Change Equations:		
1. Base specification	-.370 (.157)	-.418 (.174)
2. 1 minus size dummies	-.421 (.142)	-.396 (.168)
3. 1 plus change in RN employment	-.383 (.157)	-.425 (.175)
4. 1 plus change in hospital census	-.366 (.157)	-.417 (.174)
5. 1 plus %HMO	—	-.401 (.174)
6. 1 plus change in %HMO	—	-.419 (.175)
ΔS-HI Coefficients from Log Employment Change Equations:		
1. Base specification	-.133 (.204)	-.037 (.193)
2. 1 plus change in hospital census	-.093 (.175)	-.054 (.168)
3. 1 plus %HMO	—	-.039 (.193)
ΔS-HI Coefficients from Staffing Change Equations:		
1. Base specification	-.568 (.207)	-.543 (.221)
2. 1 plus change in hospital census	-.589 (.200)	-.530 (.209
3. 1 plus %HMO	—	-.546 (.221

Shown are coefficients on ΔS-HI, the change in hospital system concentration (based on average daily census) between 1993 and 2000. The base specification for each equation includes the change in market average level of the following characteristics: degree type, experience and experience squared, union status, race and ethnicity, part-time, gender, public employment, martial status, and the change in the state unemployment rate. Also included are size dummies.

Table 5: RN and Control Group Wages, Searching for New Monopsony

	(1)	(2)
RN Sample Only Effect on RN Wages from:		
M^{RN}	.004 (.044)	.004 (.044)
M^{C}	—	.060 (.083)
Sample Size	6,724	
Control Group Sample Only Effect on Control Group Wage from:		
M^{RN}	—	.031 (.044)
M^{C}	-.103 (.090)	-.097 (.091)
Sample Size	61,301	
Second-Step WLS Effect on RN/Control Group Relative Wage from:		
M^{RN}	-.037 (.050)	-.038 (.050)
M^{C}	—	.062 (.097)
Sample Size	238	

Shown in the top two panels are partial results from single-step wage equations. The bottom panel shows partial results from the second-step WLS relative wage equation. Monopsony measures M^C and M^{RN} measure the proportion of new hires from outside employment (or 1 minus the proportion from other employers), intended to proxy the inverse supply elasticity. SRN-M^{RN} is calculated from the 1984,1988, 1996, and 2000. Other measures of M are calculated by labor market using all rotation groups from the 1994-2002 CPS (see text for details). Other CPS data are from the 1998-2002 CPS ORG earnings files. Variables included in the top 2 panels are schooling dummies, experience and its square, dummies for part-time status, race and ethnicity (3), gender (for the RN equation), marital status (2), union status, public employment, year (4), region (8), area size (7) and the Herfindahl index (*HI*). The WLS second-step estimates, with the RN/Control group relative wage the dependent variable, is identical to that shown in column 3′ in the top panel of Table 3, apart from the additional variables shown here.

Table 6: New Monopsony Wage Effects by Education Level

	All	High School or Less	Some College or Beyond
Women:			
M^W	-.161	--	--
	(.101)		
M^{Low}	--	-.180	--
		(.077)	
M^{High}	--	--	-.044
			(.087)
Sample Size	266,660	106,916	159,744
Men:			
M^M	-.059	--	--
	(.096)		
M^{Low}	--	-.030	--
		(.086)	
M^{High}	--	--	-.003
			(.065)
Sample Size	267,362	115,719	151,643

Monopsony measures M^W, M^{Low}, and M^{High} measure the proportion of new hires from outside employment for all woman, women with a high school education or less, and women with some college or beyond, respectively. Measures of *M* are calculated by labor market using all rotation groups from the 1994-2002 CPS (see text for details). The wage sample is drawn from the 1998-2002 CPS ORG files. Other variables included in the wage equations are years of schooling, experience and its square, dummies for part-time status, race and ethnicity (3), marital status (2), union status, public employment, year (4), region (8), market size (7), and the state level unemployment rate by year.