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Monopsony power and relative wages in the labor market for nurses

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Abstract

This paper examines the thesis that monopsony power is an important determinant of wages in nursing labor markets. Using data from the 1985–93 Current Population Surveys, measures of relative nurse/non-nurse wage rates for 252 labor markets are constructed. Contrary to predictions from the monopsony model, no positive relationship exists between relative nursing wages and hospital density or market size. Nor is support found for the presence of monopsony power based on evidence on union wage premiums, slopes of experience profiles, or the mix of RN to total hospital employment.

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1. Introduction

The labor market for registered nurses has received considerable attention from researchers owing, at least in part, to the reported shortages that have appeared

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periodically in nursing markets. [1] One explanation for nursing shortages, popular in the nursing literature and economics textbooks, is that hospitals face an upward sloping labor supply curve and thus possess monopsony (or oligopsony) power (one of the earliest statements is Yett, 1970). The upward sloping supply curve results in a lower wage and employment level for nurses than would occur if the market were competitive. Monopsony would help explain reported shortages, since hospitals will list vacancies and desire to hire additional workers at the monopsonistic wage, but would decrease their profitability were they to raise wages to attract more applications.

Although the monopsony model has considerable theoretical appeal, empirical evidence for monopsony power in nursing labor markets is mixed. Previous studies have focused either on the potential for monopsony power, based on estimates of labor supply curve elasticities facing hospitals (e.g., Sloan and Richupan, 1975; Link and Settle, 1979; Link and Settle, 1981; Sullivan, 1989; and Hansen, 1991), or examined the relationship between hospital wages, employment, and market structure (e.g., Hurd, 1973; Link and Landon, 1975; Sloan and Elnicki, 1978; Feldman and Scheffler, 1982; and Bruggink et al., 1985).

In perhaps the most careful and detailed study of monopsony to date, Sullivan (1989) utilizes data for 1979–85 from the American Hospital Association's Annual Surveys of Hospitals in order to estimate the inverse elasticities of labor supply facing hospitals. Following research by Bresnahan (1981) and Baker and Bresnahan (1988), he takes the equilibrium market structure as given and then estimates the supply elasticities under three alternative assumptions about the nature of the market equilibrium (employment setting, wage setting, and consistent conjectural variations). These estimates translate into a one year labor supply elasticity of about 1.25, and a three year elasticity of about 4.0. Contrary to expectations from the monopsony model, Sullivan's results do not differ substantially between metropolitan and non-metropolitan hospitals. Nor does he compare these results to those performed on any alternative ''non-monopsonistic'' occupation. [2] We argue below that an important limitation of this approach is that the presence of an upward sloping labor supply curve is necessary but not sufficient evidence of a monopsonistic outcome. Rather, wage and employment outcomes predicted by the monopsony model must be directly tested.

[1] See, for example, Aiken (1987), Buerhaus (1987), McKibbin (1990), and Hassanein (1991). More recently, reports of nursing shortages have declined (Brider, 1993).

[2] Hansen (1991) provides an extension of the Sullivan study. She sets up a general nonlinear supply–demand model that includes both competition and monopsony as subcases. In her model, an exogenous shock in an input market that pivots rather than shifts the supply curve allows the competitive case to distinguished from the monopsonistic case. Empirically, Hansen can not reject the null hypothesis of competition in the market for nurses. When she runs the test separately for rural and urban nurses, she again finds no evidence for monopsony.

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Previous studies examining the relationship between market structure and nursing wages generally find the positive relationship between wages and the degree of competition predicted by monopsony theory. Because larger (more competitive) markets tend to have both higher skill workers and higher wages in nursing and alternative non-nursing occupations (due in part to cost-of-living differences), these studies do not provide a convincing test of monopsony power. An exception is a careful study by Adamache and Sloan (1982), who examine the real wages of RNs, LPNs, kitchen workers and secretaries employed in hospitals. In contrast to the above studies, they find no effect of concentration on entry level compensation for union or nonunion workers, after controlling for cost-of-living and population density.

Adamache and Sloan (1982) also analyze the relationship between unions, tenure, and monopsony. They argue that a negative correlation between tenure and turnover suggests that monopsony power should depress wages relatively most for workers at the top of the wage scale; that is, flatten the wage–experience profile. In regressions with the variation in bottom-to-top pay for kitchen workers, RNs, and LPNs as dependent variables, they find no significant effect of concentration on wage dispersion among RNs and LPNs, contrary to the prediction of the monopsony model of less dispersion in more concentrated markets. They also argue that unions should have countervailing power that offsets the effects of monopsony. They again find no evidence for this proposition. [3]

Our study extends the work of Adamache and Sloan (1982) and others by analyzing how wage and employment outcomes differ across markets more and less likely to be monopsonistic. We test the prediction that relative nursing wage rates for registered nurses (and perhaps other nursing personnel) will be lowest in relatively small labor markets with a limited number of employers. An important contribution of the study is the use of a large dataset on individual workers constructed from the monthly Current Population Survey Outgoing Rotation Group (CPS ORG) files for the period October 1985 through December 1993. These data allow us to examine the relative wage rates of hospital and non-hospital registered nurses (RNs), licensed practical nurses (LPNs), and nursing aides, as compared to alternative control groups of non-health related workers, across 202

[3] Robinson (1988) addresses the monopsony question by looking at differences across markets in employment and occupational mix. Under the assumption that non-nurse labor markets are competitive, the marginal factor cost of nurses is more expensive relative to non-nurses in monopsonistic than in competitive labor markets. Both employment and the proportion of all hospital jobs filled by nurses, therefore, should be higher in competitive than in highly concentrated nursing labor markets. Consistent with the monopsony model, he finds that in a cross section of hospitals, total employment (controlling for measures of output) and the ratio of RNs to LPNs initially increases as market concentration decreases. The relationship is nonlinear, however, with a reversal among markets with extremely low concentration.

metropolitan areas and 50 non-urban state groups. The 1985–93 period is particularly appropriate for study, since most of these years have been described as characterized by widespread and sustained nursing shortages (McKibbin, 1990). Contrary to the predictions of the monopsony model, we find no evidence that the relative wages of nursing personnel are positively related to either labor market size or hospital density.

The scope of the paper is as follows. In Section 2, we examine the theory and testable implications of monopsony models of nursing labor markets. The data set is described in Section 3, followed in Section 4 by a presentation of descriptive evidence on the relative wage rates for nursing personnel during the 1985–93 period. Our estimation approach is outlined and empirical results are presented in Sections 5 and 6. A brief conclusion follows.

2. Monopsony in nursing labor markets: Theory and implications

Monopsony here refers to a labor market where there is a limited number of employers (e.g., hospitals). Each firm faces an upward sloping labor supply curve of nursing personnel when making its hiring and salary decisions, with the marginal labor cost (MLC) exceeding labor's opportunity cost at each level of employment. Fig. 1 illustrates the standard single buyer monopsony model. Profit maximization by the hospital would lead to employment at E_m, where the hospital's marginal revenue product (MRP) equals MLC, and a wage W_m that will just attract employment E_m. Both employment and wages would be lower than would exist in a competitive labor market, where employment and wages would tend toward E_c and W_c. At the profit maximizing wage for the monopsonist, there

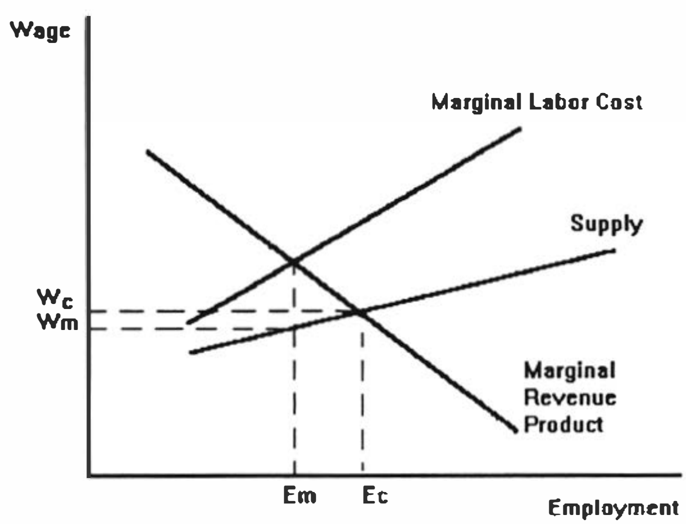

Fig. 1. Monopsony in the labor market.

exists a nursing "shortage" in the sense that the hospital would prefer to hire more nursing personnel at wage W_m, but is unable to do so. An increase in employment beyond E_m would require the hospital to raise wages, but this action would in turn lower its profits. [4]

In the more likely case where there exist several employers, wage–employment outcomes are theoretically indeterminate. If hospitals and other large employers act in collusion (e.g., implicitly or explicitly through information sharing arrangements) to "cooperate" and limit wages and employment to the level that maximizes *joint* profits, then something similar to the monopsonistic outcome can be achieved. It is widely recognized that this is not a stable equilibrium. Wage–employment outcomes similar to competitive outcomes can obtain, even when the number of employers is small and labor mobility is weak, if employers behave in a noncooperative and rivalrous fashion. That is, it will be in the interest of individual employers to raise wages above the prevailing monopsonistic level in order to attract large supplies of nurses away from their competitors. If enough employers behave in this fashion, maximizing individual rather than joint profits leads to the competitive outcome.

An alternative to the collusion model are noncooperative leader–follower models with a dominant employer and a competitive fringe. For example, a large hospital may set its wage, based in part on the expected reaction (i.e., employment) by smaller wage-taking employers. The precise wage–employment outcome in such models varies with the specific assumptions. But these models typically lead to a prediction of lower wages and employment than with a competitive outcome (see Sullivan, 1989, for a presentation of alternative models).

An additional possibility is that of a monopsonist that can wage (i.e., price) discriminate. Perfect wage discrimination would imply that a monopsonist pays individual nurses exactly their reservation wages; that is, bids up the labor supply curve. In this (admittedly unlikely) case, monopsony would imply, as before, lower wages (except for workers at the margin), but would not imply a decrease in employment or a "shortage" of nurses. Wage discrimination by employers with monopsonistic power might be evinced by a larger residual variance of wages in monopsonistic than in competitive markets. That is, there would be greater wage dispersion among nurses with given characteristics (i.e., productivity), corresponding to the dispersion in reservation wages, whereas employers in competitive markets must pay the same market wage to all workers of equal productivity.

Monopsonistic power may also take the form of a flatter wage–experience profile in smaller, less competitive markets. If experienced (often married) nurses are less mobile than younger entry level (and less often married) nurses, wages

[4] If monopsony is accompanied by monopoly in the product market, then output, employment, and wages will be even lower than with monopsony alone.

should not rise as quickly with respect to experience in less competitive markets. [5] Reputation and implicit contracts play an important role here. On the one hand, if wages are determined competitively at entry and contracts are short run, entry-level nurses will demand even higher initial wages since they do not expect employers to increase wages fully with respect to future productivity. This reinforces our prediction about relatively flatter slopes of earnings profiles in smaller markets. [6]

The monopsony outcome requires that there be limited long-run mobility of nursing personnel across and within labor markets. Existence of an upward sloping *market* supply curve (i.e., a positive employment–wage relationship) does not imply non-competitive outcomes. A necessary but *not* sufficient condition for the monopsonistic wage–employment outcome is that there be an upward sloping labor supply curve facing *individual* hospitals (employers). One may find empirical evidence of upward sloping short-run labor supply curves facing hospitals (Sullivan, 1989) because of long-run implicit contracts between nurses and hospitals. Such contracts require that hospitals are able to structure compensation in ways that increase the attachment of nurses to their current employer, and that hospitals develop a reputation for non-opportunistic behavior. Firm-specific training, pay sequencing that back-end loads compensation (wages or fringes), and various forms of incentive contracts act to lower worker mobility and break down the equality between wages and workers' spot marginal products (see Lazear, 1991; and Hutchens, 1989). A decrease in wages owing to a hospital-specific demand shock, for example, would not result in the loss of all nurses to other employers. What appears to be evidence for monopsony (i.e., a less than perfectly elastic labor supply schedule) may in fact reflect implicit contracts that maximize the joint surplus of hospitals and nurses. [7]

For these reasons, testing whether hospitals face upward sloping labor supply curves is not a sufficient test of the monopsony model. Rather, one must examine

[5] This is essentially the argument made by Adamache and Sloan (1982).

[6] On the other hand, hospitals may develop a reputation as non-opportunistic employers, in which case long-run implicit contracts should lead to a compensation profile that maximizes the joint surplus of hospitals and nurses. In this latter case, the present value of lifetime earnings is at least as high as in markets where wages equal workers' spot marginal products. Absent further assumptions about differences in implicit contracts, incentive effects, and pay sequencing in competitive and monopsonistic labor markets, few testable implications arise.

[7] The arguments in this paragraph apply not only to nursing labor markets. In fact, the prevalence in nursing of transferable general training, rather than nontransferable firm specific training, implies that implicit long-term contracts are even more important in many non-nursing labor markets. If we are correct, then studies such as the one by Sullivan (1989) should find evidence of upward sloping firm-level labor supply curves in a wide variety of occupations. The presence of implicit contracts may help account for the "surprising" finding by Sullivan that the inverse elasticities of nursing labor supply curves did not differ between hospitals in large metropolitan markets and those in smaller markets.

directly whether the wage–employment outcomes predicted by the monopsony model, as compared to outcomes from the competitive model, in fact occur. The approach taken in this paper, therefore, is to examine directly testable implications that follow under most variants of the monopsony model. Most important is the prediction that wages will be lower in monopsonistic labor markets than in otherwise similar competitive markets. And, except in the unlikely event that an employer can perfectly price discriminate, employment and the ratio of employment to other factors will be lower.

Because monopsony requires limited mobility of labor and a relatively small number of employers, we should find that the monopsony outcome is more prevalent the smaller the labor market and the fewer the number of employers. Rural and small town markets are likely to have relatively few hospitals, nursing homes, and doctors' offices over large geographic areas, while nurses in such markets (particularly those who are married) may have limited mobility. If monopsonistic power is a major factor in the nursing labor market, it is these markets where its effects should be observed. By contrast, highly populated urban areas, with numerous hospitals and many alternative nurse and non-nurse employment opportunities, are least likely to show the effects of buyer power in the labor market.

Our empirical test of monopsony therefore requires that we determine whether wages in a labor market are low relative to a counterfactual competitive outcome, and to measure market characteristics such as size and/or number of employers. Such information allows us to examine whether wage rates diverge from those observed in competitive markets, and whether the pattern of divergence is consistent with that predicted by the monopsony model. Because wages vary across areas, in part because of unmeasured site-specific amenities and locational attributes (Roback, 1982), our wage measure will rely on a comparison of nursing wages to those of a control group of non-nursing workers with similar characteristics in the same market. We will also examine alternative implications of the monopsony model, including its potential effects on wage–experience profiles, distortions of the employment mix, and the role of labor unions as a countervailing force.

3. Data

The primary data for this study are drawn from the monthly Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for the period October 1985 through December 1993 (99 surveys). The CPS ORG files have not been used previously in the nursing literature. The CPS is conducted by the Bureau of the Census and includes large representative samples of U.S. households. In each month's survey a quarter of the sample (the outgoing rotation groups) are asked not only the standard demographic and employment questions, but also questions from an earnings supplement that includes responses on weekly earnings, hours worked per week, and union status. [8] The annual CPS earnings files, containing data for all 12 monthly surveys in each year, are not public use tapes, but are made available through the research and data services staff at the Bureau of Labor Statistics. Beginning in October 1985, the CPS identified each individual's location as either in or out of one of 202 Metropolitan Statistical Areas or Consolidated Metropolitan Statistical Areas (MSA/CMSA) with populations of 100,000 (in July 1983). [9] Those not in a designated MSA/CMSA either reside in a small MSA or a non-metropolitan area. Thus we have representative national samples with all workers assigned to one of 252 market areas (202 MSA/CMSAs and 50 non-urban state groups).

Our nursing sample includes all hospital and non-hospital registered nurses (RNs), licensed practical nurses (LPNs), and nursing aides who are employed wage and salary workers ages 18 and over with positive weekly earnings and hours. Workers whose primary activity is school, whose implied real wage rate is less than a dollar, or who have had their occupation allocated by the Census are excluded from the sample. Sample sizes of these groups for the October 1985 through December 1993 period are as follows: RNs – 24,345, RNs employed in hospitals – 17,296, LPNs – 6,119, and aides – 20,166.

In addition, we utilize three control groups to construct measures of relative wages by labor market. We construct a large initial control sample that includes all female workers (meeting the same criteria as above, except the occupation non-allocation requirement) in non-health related occupations within the following broad occupational groups: executive, administrative and managerial; professional specialty; technicians and related support; sales; administrative support and clerical; and service (except protective and household services). This large control sample is then divided into three distinct control groups along educational lines. Those with at least a college degree comprise the control group with whom the wages of RNs are compared ($n = 127{,}831$); those with a high school degree or some college make up the control group for LPNs ($n = 341{,}365$); and those with less than a high school degree are used as the control group for aides ($n = 38{,}689$).

[8] The sample design of the CPS is that a household is included for 4 months, followed by 8 months out, followed by 4 months in. Only the outgoing rotation groups (rotations 4 and 8) are asked the earnings questions. Hence, the ORG or earnings files include all individuals surveyed in the CPS, but only once in a year.

[9] There are 181 MSAs and 21 CMSAs. The latter (with the exception of St. Louis) contain two or more primary metropolitan statistical areas (PMSAs). Prior to October 1985, there were identifiers in the CPS for only 44 large Standard Metropolitan Statistical Areas (SMSAs).

We should emphasize at the outset that our basic results and conclusions are not sensitive to the choice of control groups. [10]

The CPS ORG files have several advantages relative to other data sources. In contrast to data from the AHA surveys, the CPS provides information on individual worker (but not hospital) characteristics. [11] CPS data also are available in a timely fashion on an annual basis and information is available on current earnings, hours, union status, and occupation, as opposed to the previous year's earnings and occupation on the longest job held last year as in the Census of Population (union status is not reported in the Census). The CPS also includes information on nursing personnel employed not only in hospitals, but also outside of hospitals within the same labor market. Previous literature has focused primarily on hospital employees. Most important for our study, the CPS provides large representative samples of potential groups with whom nurses can be compared, with information taken from the same surveys and with detailed area (i.e., labor market) identifiers. The primary disadvantage of the CPS is that it lacks information on occupational grade and responsibilities (apart from the designation of RN, LPN, or aide) or on employer characteristics, apart from industry (hospital, nursing home, etc.) and sector (private, federal, state, or local).

The comparison of nursing and non-nursing wages is a critical component of this study. By measuring nursing wages relative to control groups within the same labor markets, we are able to control not only for differences in measurable worker characteristics, but also for cost of living differences, area amenities or disamenities, area-specific unmeasured labor quality, differences in working conditions, and other market-specific wage determinants that otherwise are not easily measured. The measurement of relative wages for nurses within areas is made possible by comparing nursing wages (RNS, LPNs, and aides) within each labor market to the wages of their respective control group in that market, after controlling for measurable worker and market characteristics. The implication of the monopsony model is that nursing wages, relative to their control groups, will be lower in labor markets that are small and with a limited number of employers. Our method should be reliable unless *unmeasured* area-specific determinants of *relative* nursing to control group wages are systematically related to market size and number of employers.

[10] Because monopsony power might in principle affect the wages of health professionals outside nursing, we chose to exclude all workers in health-related occupations from the control groups. By way of summary, excluded from the control group samples are males; female workers in health-related occupations within the selected broad occupational categories; and workers in the following non-selected occupational groups: 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.

[11] Sullivan (1989) provides a good discussion of the AHA data.

A second source of data is *Hospital Statistics*, a summary of information collected and published by the American Hospital Association (1989–90) based on their Annual Survey of Hospitals. Data by metropolitan area and non-metropolitan area by state are provided on such things as number of hospitals, employment by type of personnel (RNs, LPNs, etc.), compensation costs, total expenditures and per patient costs, patient days, and number of trainees. Because relative differences across areas in number of hospitals changes slowly, we matched the 1989 figures to our 1985–93 sample. [12]

4. Nursing wages and employment, 1985–93: A descriptive overview

Prior to our formal analysis, we provide a descriptive overview of relative wages for nursing personnel. Table 1 presents mean real wages for RNs, LPNs, and nursing aides by year, and the ratio of these wages to the wages of their respective control groups. Wage rates are measured by usual weekly earnings divided by usual hours worked per week, in constant December 1993 dollars (based on the monthly CPI-U). [13] Real wages for RNs increased substantially during the 1985–93 period, both absolutely and relative to the control group of female college graduates in alternative occupations. For example, the average real wage for hospital RNs increased from $15.46 in 1985:4 to $18.70 in 1993, while the wage relative to the control group rose from a ratio of 1.12 to 1.27 during this brief period. Particularly rapid wage increases occurred during 1987–90, a period often characterized as one of severe nursing shortages. Note that the increase in relative wage rates for RNs is particularly noteworthy because RNs are being compared to college educated women in white collar occupations. This is the very group of workers whose wages rose most rapidly during the period; that is, there were widening skill and narrowing gender wage gaps during this period (Levy and

[12] *Hospital Statistics* provides data at the MSA/PMSA level and for the non-metropolitan portion of each state. In order to get an exact match for the 21 CMSAs designated in the CPS, the hospital data were aggregated from the component PMSAs. Information on small MSAs (below 100,000 population) that are not separately designated in the CPS was added to that on the non-metropolitan portion of the state to calculate values for our 50 non-urban state areas.

[13] Weekly earnings are top-coded at $999 per week in surveys through 1988, and at $1,923 beginning in January 1989. A maximum of 1.2 percent of RNs are at the earnings cap in any year (1988); 0.3 percent of the RN sample is at the cap in 1993. The highly educated RN control group includes 3.9 percent of the sample at the earnings cap in 1988, and 0.7 percent in 1993. Workers at the lower cap during 1985–88 are assigned earnings based on mean real earnings above that same real dollar amount in 1989, calculated separately for the three groups of nursing personnel combined with their control groups. The assigned mean earnings typically ranged between $1,300 and $1,400 in nominal weekly earnings (and $1,700–$1,800 in December 1993 earnings). No adjustment is made for workers at the higher cap beginning in 1989.

Table 1
Mean wage rates and relative wage ratios for nursing personnel and control groups by year, 1985–93

	1985:4	1986	1987	1988	1989	1990	1991	1992	1993	*n*
Hospital RNs	15.46	15.67	16.06	16.97	17.69	18.01	18.48	18.42	18.70	17,296
Hosp. RNs/ Control_{RN}	1.12	1.10	1.11	1.18	1.23	1.22	1.26	1.25	1.27	127,831
All RNs	14.98	15.35	15.62	16.44	16.99	17.38	17.69	17.73	18.07	24,345
RNs/Control_{RN}	1.09	1.08	1.08	1.14	1.18	1.18	1.21	1.20	1.23	127,831
LPNs	10.12	10.28	10.54	10.79	10.86	11.02	11.20	11.57	11.47	6,119
LPNs/Control_{LPN}	1.09	1.08	1.10	1.12	1.15	1.16	1.18	1.22	1.21	341,365
Nursing Aides	7.37	7.57	7.56	7.45	7.69	7.81	7.80	7.70	7.86	20,166
Aides/Control_{A}	1.04	1.03	1.05	1.01	1.11	1.10	1.11	1.08	1.11	38,689

Shown are mean real wage rates and wage ratios by year. Data are from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for October 1985 through December 1993. Sample sizes (n) listed on the rows showing wage ratios are for the designated control groups. Wage rates are measured by usual weekly earnings divided by usual hours worked per week, in December 1993 dollars (indexed by monthly CPI-U). The sample includes all RNs, LPNs, and Aides, ages 18 and over, with positive earnings and hours, excluding workers whose primary activity is schooling and with a real wage rate less than one dollar. The three control groups include all women, ages 18 and over and with the same restrictions as above, in non-health related occupations within the following broad occupational groups:executive, administrative and managerial; professional specialty; technicians and related support; sales; administrative support and clerical; and service (except protective and household services). The RN control group includes those with at least a college degree; the LPN control group those with a high school degree or some college; and the Aides control group those with less than a high school degree.

Murnane, 1992). LPNs also realized substantial absolute and relative wage gains over the period, whereas nursing aides received relatively small gains.

Table 2 provides mean wage rates and relative wage rates by nursing group for the pooled 1985–93 period, disaggregated by labor market size. As noted previously, the CPS designates individuals as residing either in one of 202 MSA/CMSAs with a population that exceeded 100 thousand (in 1983), or elsewhere in one of the 50 states. These 252 areas are grouped into eight size categories based on 1990 Census of Population counts: the 50 non-urban state areas; and groups of MSA/CMSAs with populations below 200 thousand, between 200–300 thousand, 300–500 thousand, 500 thousand to 1 million, 1–2 million, 2–5 million, and 5 million and over (the latter category includes five CMSAs). The 50 non-urban state areas include 32.4 percent of the nursing personnel and 26.6 percent of the control group sample.

It is evident from Table 2 that wage rates for RNs, LPNs, and aides (as well as the control group occupations) rise substantially with respect to labor market size. Hospital RNs, for example, earned $19.25 in the largest metropolitan areas, as compared to $15.78 in non-urban areas. Although the finding of lower wages in small markets provides evidence superficially supportive of the monopsony model, it is not compelling evidence since differences in other wage determinants also

Table 2
Mean wage rates and relative wage ratios for nursing personnel and control groups by area size category

	All	Size 1	Size 2	Size 3	Size 4	Size 5	Size 6	Size 7	Size 8
Hospital RNs	17.47	15.78	16.64	17.17	16.72	17.34	17.45	18.40	19.25
Hosp. RNs/ Control_{RN}	1.20	1.27	1.35	1.20	1.24	1.25	1.26	1.18	1.15
All RNs	16.89	15.18	16.06	16.70	16.18	16.73	16.85	17.86	18.85
RNs/Control_{RN}	1.16	1.22	1.30	1.17	1.20	1.21	1.22	1.14	1.12
LPNs	10.94	9.95	10.24	11.14	10.96	11.33	11.18	11.79	13.02
LPNs/Control_{LPN}	1.15	1.22	1.20	1.15	1.25	1.21	1.18	1.14	1.16
Nursing Aides	7.68	6.86	7.14	8.03	7.40	7.74	7.57	8.41	9.06
Aides/Control_{A}	1.07	1.05	1.09	1.14	1.13	1.09	1.11	1.10	1.09
Sample sizes: all nursing groups	50,630	16,421	2,289	2,233	3,160	5,776	3,746	8,108	8,897
Sample sizes: all control groups	507,885	134,858	19,492	24,270	29,978	62,015	43,414	95,052	98,806
Mean number of hospitals	22.0	55.2	3.3	4.6	7.0	12.1	19.4	44.2	138.0
Mean hospitals per 100 square miles	0.49	0.18	0.39	0.44	0.46	0.60	0.73	1.07	1.63
Mean hospitals per 100 thousand pop.	1.87	2.02	2.27	1.87	1.81	1.62	1.43	1.49	1.36
Number of MSA/ CMSAs or [state groups]	252	[50]	49	38	42	36	16	16	5

See the note to Table 1. Data are pooled over 1985:4–1993:4. Size categories 2–7 are MSA/CMSAs; Size category 1 includes non-urban state groups. Populations among the size categories are as follows: Size 1 = 50 non-urban state areas (includes non-metropolitan area workers, and those residing in metropolitan areas with populations less than 100,000); Size 2 = 100–200 thousand; Size 3 = 200–300 thousand; Size 4 = 300–500 thousand; Size 5 = 500 thousand to 1 million; Size 6 = 1–2 million; Size 7 = 2–5 million; Size 8 = 5 million and over.

lead to systematic wage differentials by market size. As seen in Table 2, when the wage rates of nursing personnel are measured relative to their respective control groups, relative wages for RNs and LPNs are lowest in the largest labor markets. For example the relative wages of hospital RNs to their control group is 1.27 in the 50 non-urban markets, 1.35 in metropolitan areas with populations 100,000 to 200,000, and only 1.15 in the largest CMSAs. The wage ratios for nursing aides indicate low relative wages in nonurban labor markets, but no size pattern across urban markets. Subsequent analysis will examine relative wage differences across labor markets following control for other measurable wage determinants.

Although not the main focus of the paper, we present in Table 3 regression estimates from standard log wage equations estimated for hospital RNs, all RNs, LPNs, Aides, and their respective female control groups. Variables included are years of schooling completed; years of potential experience (measured by age minus schooling minus six) and its square; and dummy variables for union

Table 3
Standard log wage equation estimates for nursing personnel and control groups

	Hosp. RNs	All RNs	$Control_{RN}$	LPNs	$Control_{LPN}$	Aides	$Control_A$
Hospital	–	0.1237	–	0.0562	–	0.2041	–
		(21.27)		(5.09)		(29.02)	
Nursing home	–	−0.0592	–	−0.0102	–	0.0061	–
		(−6.55)		(−0.86)		(0.94)	
Physician office	–	−0.0972	–	−0.0652	–	0.2805	–
		(−9.37)		(−3.95)		(16.38)	
School	0.0278	0.0303	0.0761	0.0184	0.0712	0.0349	0.0258
	(19.11)	(23.08)	(69.27)	(6.41)	(92.94)	(25.12)	(23.91)
Experience	0.0123	0.0119	0.0243	0.0109	0.0240	0.0111	0.0115
	(15.71)	(17.54)	(60.72)	(8.85)	(120.16)	(16.97)	(21.96)
$Exp^2/100$	−0.0243	−0.0244	−0.0543	−0.0200	−0.0422	−0.0176	−0.0150
	(−12.94)	(−15.85)	(−56.24)	(−7.81)	(−102.62)	(−14.06)	(−17.93)
Union coverage	0.0198	0.0290	0.0757	0.0453	0.1361	0.1169	0.2076
	(3.43)	(5.20)	(20.76)	(4.13)	(59.28)	(15.85)	(30.09)
Male	−0.0060	0.0156	–	0.0019	–	0.0215	–
	(−0.60)	(1.62)		(0.10)		(2.38)	
Married with spouse present	0.0187	0.0276	0.0623	0.0190	0.0608	0.0506	0.0617
	(2.70)	(4.11)	(17.74)	(1.48)	(28.27)	(6.88)	(9.04)
Married previously	0.0083	0.0240	0.0457	0.0131	0.0344	0.0088	0.0208
	(0.98)	(3.03)	(10.05)	(0.95)	(13.95)	(1.09)	(2.91)
Black	−0.0994	−0.1089	−0.0548	−0.0418	−0.0878	−0.0608	−0.0442
	(−10.46)	(−12.65)	(−11.28)	(−3.77)	(−36.20)	(−9.69)	(−7.26)
Other race	−0.0244	−0.0310	−0.0794	−0.0411	−0.0659	−0.0289	−0.0097
	(−2.48)	(−3.19)	(−12.70)	(−1.88)	(−16.37)	(−2.10)	(−0.98)
Part-time	0.0326	0.0226	−0.2503	0.0077	−0.2495	−0.0240	−0.1491
	(6.15)	(4.70)	(−70.69)	(0.91)	(−155.33)	(−4.16)	(−35.08)
Federal	0.0265	0.0382	0.1651	−0.0526	0.2056	0.1295	0.2763
	(2.25)	(3.21)	(23.46)	(−2.26)	(58.19)	(6.34)	(18.92)
State	0.0020	0.0240	0.0900	0.0540	0.0694	0.1129	0.0688
	(0.19)	(2.74)	(18.89)	(3.32)	(20.73)	(12.45)	(5.94)
Local	−0.0157	−0.0221	0.0429	−0.0332	−0.0210	0.0308	0.0514
	(−1.78)	(−2.95)	(11.44)	(−2.38)	(−8.47)	(3.26)	(7.20)
Kids 1	0.0126	0.0022	−0.0131	0.0084	−0.0183	0.0050	−0.0053
	(1.93)	(0.37)	(−3.58)	(0.82)	(−9.88)	(0.71)	(−0.93)
Kids 2	0.0205	0.0105	−0.0343	0.0081	−0.0243	0.0099	−0.0124
	(3.03)	(1.68)	(−8.56)	(0.76)	(−11.58)	(1.27)	(−1.88)
Kids 3	0.0010	−0.0081	−0.0816	0.0256	−0.0632	0.0078	−0.0280
	(0.10)	(−0.93)	(−12.04)	(1.74)	(−19.24)	(0.75)	(−3.12)
Kids 4+	0.0161	−0.0024	−0.1130	0.0253	−0.0890	0.0033	−0.0723
	(1.08)	(−0.17)	(−8.60)	(1.05)	(−15.65)	(0.22)	(−5.60)

Table 3 (continued)

	Hosp. RNs	All RNs	Control_{RN}	LPNs	Control_{LPN}	Aides	Control_{A}
metrop. size (7) region (8) quarter (32)	yes	yes	yes	yes	yes	yes	yes
R^2	0.162	0.185	0.208	0.180	0.259	0.278	0.186
n	17,296	24,345	127,831	6,119	341,365	20,166	38,689

Data are from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for October 1985 through December 1993. Dependent variable is the log of the real wage (weekly earnings divided by hours, in December 1993 dollars). Separate regressions are estimated for each nursing group and the respective control groups. In addition to coefficients shown, all regressions include dummies for metropolitan area size (7), region (8), and quarter (32). Variables are defined in the text. T-ratios are shown in parentheses.

coverage, male (for the nursing regressions only), race (black and other nonwhite; white is the reference group), part-time status (less than 35 hours per week), sector (federal, state, or local, with private the reference group), marital status (married with spouse present and ever married without spouse present, with never married the reference group), number of own children ages 17 or below in household (1, 2, 3, and 4 or more, with no children the reference group; male RNs are included in the reference group among women with no children), Census region (8 dummies for 9 regions), labor market size (7 dummies, with non-urban state areas the reference group), and 32 quarter dummies (with 1985:4 the reference group). The nursing but not the control group regressions include dummy variables for employment in a hospital, nursing home, or selected health practitioner's office (most of the nurses in this group are employed in physician offices), with other location of employment the omitted reference group.

As expected, RNs in hospitals are awarded considerably higher wages than RNs with similar characteristic outside of hospitals, reflecting both unmeasured skill differences and wage premiums for job disamenities (e.g., night shift and weekend work). RNs employed in hospitals realize a 20.1 percent wage premium relative to RNs employed in nursing homes, and a 24.7 percent premium relative to those in the offices of physicians and other health practitioners. Consistent with much of the literature, union–nonunion wage differentials among RNs are relatively small, with only a 2.9 percent wage differential between RNs covered and not covered by collective bargaining agreements. [14]

[14] All percentage wage differentials are approximated by $[\exp(\beta)-1]100$, where β is the logarithmic differential between the two groups under comparison [Giles (1982) compares alternative approximation methods]. Estimated union wage premiums are highly similar when membership rather than coverage is included as the unionization variable.

Black RNs have wage rates 10.3 percent lower than among white nurses with similar measured characteristics, a racial differential somewhat larger than that among female workers economy-wide. Prior studies (e.g., Mennemeyer and Gaumer, 1983; Link, 1988; and Lehrer et al., 1991) have found small racial gaps or even a black wage advantage. These studies have utilized older data or narrower samples, and have not included as detailed region and city size controls. Note that within-nursing returns to schooling are small, as opposed to the total rate of return seen for the broad control group. [15] RNs employed by the federal government (3.0 percent of all RNs), state government (5.7 percent), and local government (8.2 percent) realize wage differentials of 3.9, 2.4, and −2.2 percent, respectively, relative to the 83.1 percent of RNs employed by private-sector employers.

There are important differences between RN and non-RN wage determination. In contrast to the large gender wage differentials observed economy-wide, male RNs realize wage rates not significantly different from female RNs (the point estimate implies a 1.6 percent difference). Also in contrast to evidence for the labor market as a whole, there is no wage penalty associated with part-time employment, with part-time RNs displaying a small (2.3 percent) wage advantage. [16] And unlike the control group of women, for whom the presence of children is likely to be a proxy for occupational skill level and past investments in human capital, female RNs with children suffer no wage disadvantage relative to male RNs and female RNs without children.

Not shown in Table 3 are the coefficients on the quarterly dummies. Fig. 2 provides a diagram that charts the quarterly dummies or logarithmic wage changes from 1985:4 through 1993:4 (with 1985:4 = 0), for hospital RNs ($n = 17{,}296$), the control group of college educated women ($n = 127{,}831$), and *all* employed non-student U.S. wage and salary females ($n = 674{,}281$) and males ($n = 724{,}970$) ages 18 and over (the specification for the economy-wide groups are identical to that for the RN control group, except for the addition of broad occupation and industry dummies). The real wages of hospital RNs, following control for measurable characteristics, rose 0.174 log points (19.0 percent) between 1985:4 and 1993:4. By contrast, the control group of college educated women realized real

[15] As is typical of occupation-specific studies, one obtains a small coefficient on schooling that measures only within-occupation and not total returns to schooling. For years through 1991, the CPS provides information on years of schooling completed, but not degree. Beginning in 1992, the CPS provides information on type of degree completed. Data for 1992–93 indicates that RNs with bachelor's degrees earn significantly more than RNs with associate and diploma degrees, although the differential (assuming a two year difference in schooling) implies a rate of return to schooling for RNs with bachelor's degrees about half as large as that realized by the general labor force (i.e., similar to the differential implied by the years of schooling coefficients shown in Table 3). Studies examining returns to nursing education include Mennemeyer and Gaumer (1983), Link (1988), and Lehrer et al. (1991).

[16] Part-time nurses are less likely to receive health insurance, pension, and other non-wage benefits than are full-time nurses. The same is also true, however, among the control group samples where a large part-time wage penalty is observed.

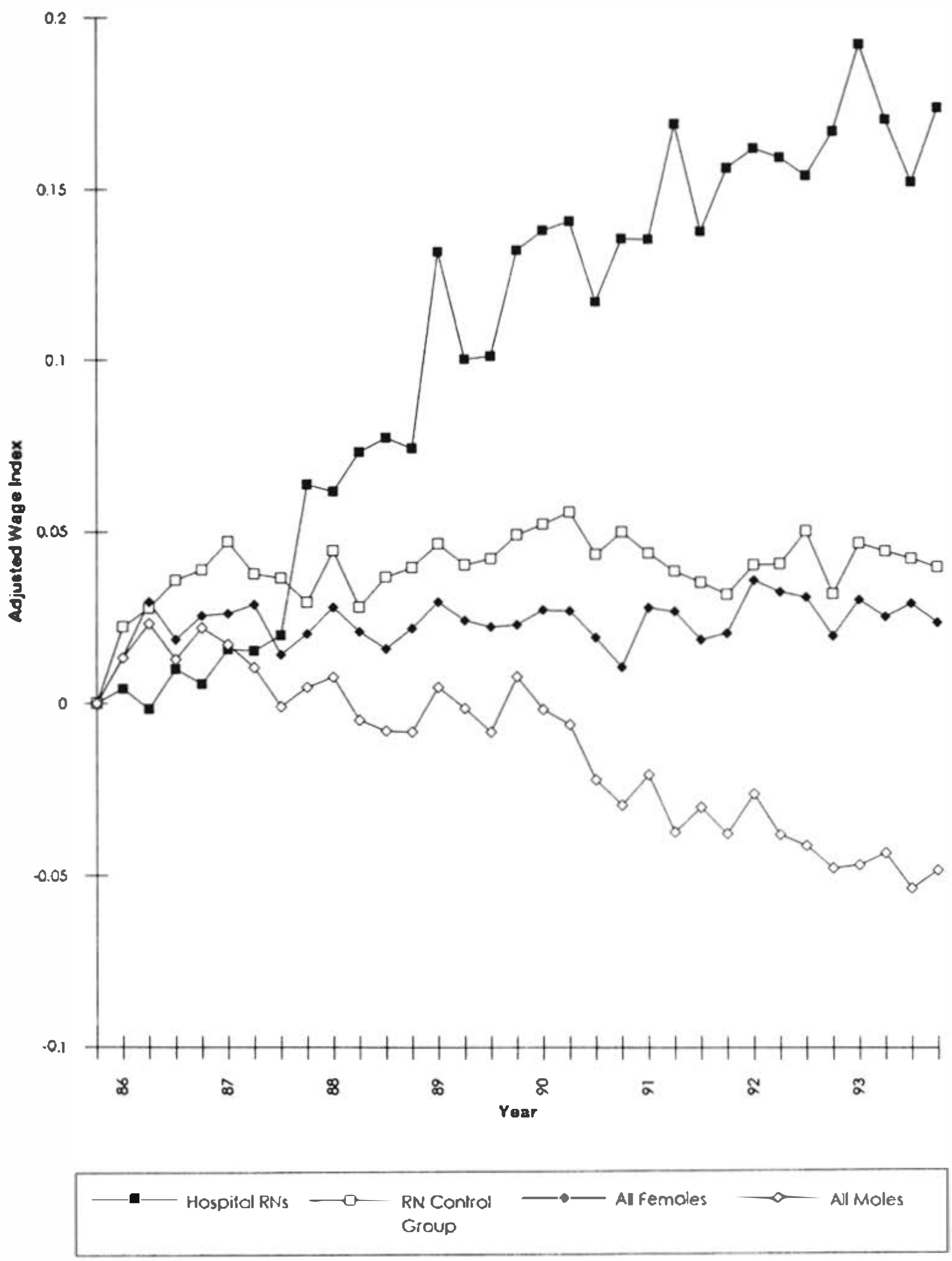

Fig. 2. RN and control group log wage changes, 1985:4–1993:4. Data are from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for October 1985 through December 1993. Sample sizes are 17,296, 127,831, 674,281, and 724,970 for hospital RNs, the RN control group, all females, and all males, respectively. The diagram presents logarithmic wage changes from 1985:4 through 1993:4, following control for measurable wage determinants. Shown are the coefficients on quarterly dummy variables (1985:4 = 0) from separate log wage equations for the four worker groups. Full results for hospital RNs and the RN control group are reported in Table 3. Variables included, in addition to quarter, are years of schooling, potential experience and its square, union coverage, marital status (2), race (2), part-time, public sector (3), children (4), region (8), and metropolitan area size (7).

wage growth of 0.040 log points. The 0.134 log point (14.3 percent) increase in hospital RN relative to control group wages, conditional on characteristics, is similar to the 13.4 percent increase (from 1.12 to 1.27) in the relative mean wage ratios, not conditional on characteristics, presented previously in Table 1, line 2. Wage growth for workers economy-wide was of course slower during this period. For all employed women, real wage growth from 1985:4 through 1993:4, conditional measured characteristics, was only 2.3 percent (0.023 log points), whereas real wages for men fell by 4.7 percent (-0.048 log points). [17]

5. Testing for monopsony effects on wages: Specification and evidence

5.1. *Specification*

A two-step estimation procedure is used to test for monopsony effects on wages. We subsequently report results from alternative specifications of the two-step model, as well as results obtained from a single-step estimation procedure.

In order to control for differences across labor markets in cost of living, opportunity costs of labor, unobserved labor quality, working conditions, and area-specific amenities and disamenities, we compare nursing wages to those of selected control groups. In the first-step, we estimate area-specific nursing wage differentials for 252 areas, where the wage differential represents the difference between the nursing and non-nursing wage in that labor market, conditional on measurable characteristics that vary at the individual level *within* labor markets. The monopsony model predicts that the nursing differential should be lowest in the least competitive markets, with the differential increasing with respect to hospital density and market size. In the second stage, therefore, we examine whether the nursing/non-nursing area differentials ($n = 252$) estimated in the first-step vary systematically with labor market characteristics that vary *across* but not within areas; specifically, labor market size, hospital density, and region.

An important assumption of this analysis is that unmeasured differences across markets in cost of living, labor quality, working conditions, and area amenities are properly controlled for to the extent that these unmeasured wage determinants affect nursing personnel and the control groups in a similar manner. Such an assumption is reasonable since the control groups selected provide an opportunity cost measure of the long-run alternatives available to nursing personnel, and cost of living and area amenities should be roughly similar for nurses and the control groups. Moreover, the control group wage (conditional on measured characteris-

[17] Standard errors attaching to the quarterly dummies are approximately 0.017 in the hospital RN, 0.010 in the RN control group, and 0.004 in both the national female and male regressions.

tics) 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 or hospital density.

We have no a priori expectation of the direction of bias associated with unmeasured labor quality. It is fair to assume that worker quality among *both* nursing and non-nursing personnel is higher in larger markets. Our measure of relative wages properly controls for unmeasured labor quality if labor quality increases with respect to market size at a similar rate for both groups. If nursing quality rises faster with respect to size than does quality of the control group, then our test would be biased toward the finding of monopsony (i.e., of rising relative nursing wages with respect to size). If nursing quality rises more slowly, our test is biased against the finding of monopsony. Random measurement error in the relative wage variable does not bias coefficients since the measurement error will be on the left-hand-side of the regression equation. It does, of course, lead to a lower goodness of fit for the second-step equation.

More formally, the first-step log wage equations are pooled cross-sections for the 1985–93 period, run separately for RNs, LPNs, and aides, each paired with their appropriate control group. They take the form:

$$\ln W_{itk} = \alpha + \Sigma \beta_j X_{jitk} + \Sigma \Omega_q \mathrm{QUARTER}_{qt} + \Sigma \Gamma_q \mathrm{NURSE} \cdot \mathrm{QUARTER}_{qt} + \Sigma \varphi_k \mathrm{AREA}_{kit} + \Sigma \phi_k \mathrm{NURSE} \cdot \mathrm{AREA}_{kit} + e_{itk}, \quad (1)$$

where $\ln W_{itk}$ is the natural logarithm of hourly earnings (usual weekly earnings divided by usual hours worked per week) of worker i in time period t in labor market k (where $k = 1,\ldots,252$); α is the control group intercept for area $k = 1$ in 1985:4; NURSE is a dummy variable equal to 1 for nursing personnel, X includes individual-specific variables (indexed by j) affecting nursing and control group wages, with β_j the attaching coefficients; QUARTER represents dummies for the quarters (q) 1986:1–1993:4 with Ω_q measuring the quarterly movement of control group wages and Γ_q the movement of nursing relative to control group wages; and e_{itk} is the error term.

AREA is a set of 252 dummy variables corresponding to the 202 CMSA/MSAs and 50 non-urban state areas, with φ_k ($k = 2,\ldots,252$) representing the area wage differentials for the control group relative to the omitted reference area, and ϕ_k ($k = 1,\ldots,252$) measuring the 252 area-specific wage differential for nurses relative to the control group in each area. The ϕ_k, measuring the relative nursing to control group wage by area, conditional on measured characteristics and assuming a common wage structure, is then utilized as the dependent variable in the second-step equation.

Variables included in X are years of schooling; years of potential experience and its square; and dummies for union coverage, marital status (2), part-time status, race (2), public-sector status (3), and children in household (4). These are variables that vary among individuals *within* labor markets. Excluded are variables that vary across but not within labor markets (e.g., region, market size, number of employers).

A second-step weighted least squares (WLS) regression is then estimated with the area-specific nursing wage differentials ϕ_k as the dependent variable ($n = 252$). Specifically, we estimate:

$$\phi_k = \Phi + \Theta \ln(\mathrm{HOSP/SQMI})_k + \Sigma \psi_s \mathrm{SIZE}_{sk} + \vartheta \mathrm{TRAINING}_k + \Sigma \tau_r \mathrm{REGION}_{rk} + \nu_k. \quad (2)$$

Here, ϕ_k is the nursing differential for area k estimated in the first-step regression, Φ is the intercept; $\ln(\mathrm{HOSP/SQMI})_k$ is the natural logarithm of the number of hospitals per square mile 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; $\mathrm{TRAINING}_k$ the ratio of trainees to total personnel with ϑ its coefficient; REGION_{rk} are eight dummies representing the nine Census regions, with τ the region coefficients; and ν_k is a random error term. Eq. (2) is estimated by WLS, using the square root of the nurse plus non-nurse sample sizes within each labor market as weights. [18]

Coefficients from the first-step regression (Eq. (1)) capture within-area effects owing to variation across individuals in measurable characteristics, with fixed area wage effects measured by coefficients on the area dummy variables. Differences in area nursing wage differentials are explained in turn (Eq. (2)) by hospital density, area size, and other variables that vary across but not within areas. The monopsony model predicts that Θ, the coefficient on the log of hospitals per square mile will be positive, at least for hospital RNs. It also implies that ψ_s, the coefficients on metropolitan area size, will be positive and increasing with respect to size. That is, relative to the smallest markets (i.e., the reference group of non-urban state groups) where relative nursing wages are most likely to be depressed by monopsony, larger areas should provide higher relative wages. The variable TRAINING is included to capture the presence of university and other training hospitals. The net effect of TRAINING cannot be predicted, since such hospitals may be associated with differences in input and output mix, nursing quality, and labor supply.

From the first-step regressions, we obtain area specific differentials measuring the log wage differential between nursing personnel and their respective control groups. These coefficients make up the dependent variable in the second-step WLS equation, seen in Table 4. The unweighted means of the nurse/non-nurse log wage differentials (the ϕ_k's) across areas (with standard deviations and interquartile ranges in brackets) are: 0.406 [0.111, 0.133] for hospital RNs, 0.361

[18] Dickens and Ross (1984) outline a similar two-step procedure intended to avoid the downward bias in standard errors that accompanies single-step estimation matching grouped to individual data.

[0.103, 0.132] for all RNs, 0.178 [0.136, 0.136] for LPNs, and 0.008 [0.104, 0.115] for aides. The coefficients are estimated with a high degree of precision, except in the smaller MSAs. In the combined hospital RN and control group wage equation, for example, all estimates of the differential are positive, while 237 of the 252 coefficients are significantly different from zero at the 0.05 level. Below, we focus primary attention on RNs within hospitals because hospital employees are most likely to be directly affected by the monopsony power of hospitals, because labor supply should be least elastic among nursing groups requiring the most extensive and specific (i.e., non-transferable) training, and because this is the group among whom shortages have been observed.

5.2. Evidence for registered nurses

Table 4 provides alternative second-step WLS results for hospital RNs. The results in column (1) correspond to the model as specified in the previous section. *No* support is found for the thesis of monopsonistic or oligopsonistic power in nursing labor markets. The monopsony model predicts that relative RN wages should be positively related to the density of employers, as measured by the log of hospitals per square mile (ln(HOSP/SQMI)). The coefficient on ln(HOSP/SQMI), however, is close to zero and has a large standard error. This result is subsequently found for all variants of the basic model and for all nursing groups. [19]

The monopsony model also leads to the prediction that the coefficients on the SIZE dummies should be positive and increasing; that is, the lowest relative wage rates in the non-urban markets and increasing relative wages as market size increases. We do find that the relative wage differential is slightly higher (3.4 percent) in the smallest metropolitan areas (100–200 thousand population) than in the non-urban state groups, but this difference is not statistically significant. But contrary to the monopsony hypothesis, relative wages are higher in the non-urban state areas than in all other city size categories, and the differential *decreases* as metropolitan area size increases. For example, relative RN to control group wage premiums are estimated to be about 14.5 percent lower in the largest metropolitan

[19] In results not shown, we also include the log of the number of hospitals, without standardization for physical area (square miles). We estimate two hospital coefficients, one for the 50 non-urban state groups and one for the 202 CMSA/MSAs. Again, no support is found for monopsony. Ideally, we would like to have had a Herfindahl measure of hospital concentration by area, which would reflect both the number and size of hospitals. Given the complete absence of a statistical relationship between relative nursing wages and hospital density (or number of hospitals) and, as shown below, a negative relationship between relative wages and labor market size, it is unlikely that the Herfindahl index or other measures of hospital concentration could account for much of the variation in relative nursing wages. Dranove, Shanley, and Simon (1992), however, in their analysis of the "medical arms race" thesis, show that their results can be sensitive to alternative measures of the extent of the market.

Table 4
Determinants of area relative wage differentials for hospital RNs, alternative second-step WLS and single-step regression results

	(1)	(2)	(3)	(4)	(5)	(6)
Size 2:	0.0332	–	0.0412	0.0341	0.0347	0.0458
100K–200K	(1.40)		(1.99)	(1.20)	(1.42)	(2.40)
Size 3:	−0.0454	–	−0.0532	−0.0398	−0.0469	−0.0673
200K–300K	(−1.84)		(−2.48)	(−1.35)	(−1.88)	(−3.56)
Size4:	−0.0341	–	−0.0435	−0.0541	−0.0428	−0.0421
300K–500K	(−1.36)		(−1.99)	(−1.80)	(−1.79)	(−2.41)
Size 5:	−0.0541	–	−0.0599	−0.0583	−0.0495	−0.0517
500K–1M	(−2.14)		(−2.71)	(−1.93)	(−2.18)	(−3.33)
Size 6:	−0.0540	–	−0.0594	−0.0624	−0.0533	−0.0546
1M–2M	(−1.77)		(−2.24)	(−1.72)	(−2.01)	(−3.20)
Size 7:	−0.1037	–	−0.1064	−0.1157	−0.0981	−0.1149
2M–5M	(−3.37)		(−3.97)	(−3.15)	(−3.70)	(−6.88)
Size 8:	−0.1571	–	−0.1435	−0.1644	−0.1495	−0.1648
5M and over	(−3.91)		(−4.10)	(−3.43)	(−4.37)	(−8.97)
ln(POP)	–	−0.0412	–	–	–	–
		(−5.98)				
ln(Hosp/SqMi)	0.0035	0.0151	0.0110	0.0024	0.0018	−0.0127
	(0.34)	(1.40)	(1.22)	(0.19)	(0.19)	(−2.37)
Training	0.0934	0.1861	0.4058	0.2119	−0.0357	0.4780
	(0.21)	(0.43)	(1.04)	(0.40)	(−0.09)	(1.70)
Region (8)	yes	yes	yes	yes	yes	yes
n	252	252	252	252	252	145,127
R^2	0.268	0.269	0.310	0.232	0.349	0.218
F	4.945 *	–	6.699 *	3.827 *	5.379 *	19.942 *

* Signifies that the F statistic permits rejection at the 0.05 level of the null that the size coefficients are jointly equal to zero. T-ratios are in parentheses. In columns (1)–(5), the unit of observation is area, with 50 non-urban state groups plus 202 MSA/CMSAs. The dependent variable is ϕ_k, the first-step regression estimate of the area-specific log wage differential between the nursing group and the control group, conditional on person-specific variables included in the first-step equation. Column (1) is for the specification shown in the text (the dependent variable is obtained from Eq. (1) and Eq. (2) results are shown above), with estimation by weighted least squares (WLS) using $\sqrt{n}$ as weights, with n the joint nurse and control group sample size in each area; column (2) is identical to (1), except that the log of population rather than market size dummies are included (a non-urban state group dummy times ln(POP) is also included); results in column (3) use a different dependent variable, obtained from a first-step equation allowing separate RN and control group coefficients on all variables, with $\sqrt{n}$ as weights; (4) is identical to (1), except the first-step equation is estimated for only the approximate half-sample of workers in their second year in the CPS; (5) is identical to (1), except weighting is by the inverse of the error variances attaching to the dependent variable from the first-step estimates. Column (6) provides partial regression results from a single-step OLS log wage regression including all hospital RNs and the RN control group. In columns (1)–(5), eight region dummies are included to account for the nine Census regions (these are not included in the first-step). The single-step regression in (6) includes region and all other variables shown in Table 3, with common slopes on the control variables, plus nurse interactions with the quarterly dummies, hospital density, training, and the market size dummies.

areas than in the smallest markets. The magnitude of most market size coefficients is about −0.05. Although log wage differentials of about 5 percent are nontrivial, they are rather small as compared to the approximate 0.40 log differential found between RNs and their control group, even following control for measurable characteristics. The F statistic testing for the joint significance of the market size dummies is significant at standard levels ($F(7,234) = 4.945$). Market size does appear to matter, but in a way opposite from that predicted by monopsony.

Also included as a control variable is TRAINING, measuring the ratio of trainees to total employment. As discussed previously, this variable is likely to proxy factors both positively and negatively correlated with nursing wage rates. The net effect is close to zero. We do not know if the individual factors with which TRAINING is correlated (e.g., skill intensity, labor supply) are individually not important, or whether they are important but offset each other. Finally, wages for hospital RNs relative to the control group are similar across most Census regions (we do not present these results); the regional dummies are not jointly significant at standard significance levels ($F(8,234) = 0.962$). Coefficients on the hospital density and market size variables are highly similar when region dummies are excluded.

We next examine the robustness of our basic finding. Table 4 provides five alternative sets of estimates of the relationship between relative wages of hospital RNs, hospital density, and market size. In column (2), we substitute the log of population in an area for the CMSA/MSA market size dummies (we also include but do not show a non-urban state group dummy times the log of population). Consistent with our findings using size dummies, we obtain a negative (and statistically significant) coefficient on the log population variable.

The next set of estimates, shown in column (3), are based on an alternative specification of the first-step wage equation that allows for separate coefficients for the nursing and non-nursing groups on *all* variables, in addition to the inclusion of 251 dummies for area and 252 nursing-area interaction dummies. This specification produces coefficient estimates equivalent to that obtained by running separate wage equations for nurses and the control group, each with a set of area dummies. The coefficients on the 252 nursing-area interaction dummies are then employed as the dependent variable in the second-step regression (this is equivalent to subtracting control group area dummies from nursing area dummies using separate wage equations). This differential measure reflects the relative ''wage gradiant'' by area for nurses relative to the control group. That is, after allowing for separate national wage structures for nurses and non-nurses, the relative wage measure for each area represents the log wage differential for nurses in that labor market relative to a reference market, minus the log wage differential for non-nurses in that market relative to the same reference market. As expected, these wage differential measures are highly correlated with our previous measure, which represented area-specific relative wage differentials between nurses and the control group in each labor market (the simple correlation of the two measures for hospital

RNs is 0.893). Qualitative and quantitative results from the second-step regression using this alternative area wage differential measure (column 3) are highly similar to our preferred model presented in column 1.

An additional concern results from the sampling design of the CPS, wherein individuals are potentially included in the survey during the same month in two consecutive years. This implies that (a maximum of half) the individual observations in a given year are not independent of observations in adjacent years, leading to standard errors that are biased downward. This bias is not a serious concern, since our results clearly reject the hypothesis of a positive relationship between relative nursing wages and hospital density or market size. In order to avoid this problem, we provide an identical analysis as before, except that we include individuals only in their second year in the CPS (rotation group 8), thus retaining a representative sample but eliminating all double observations on individuals. These estimates are shown in column (4) of Table 4. As expected, point estimates are similar to those shown previously, but standard errors are moderately larger.

Column (5) presents results from a model equivalent to that shown in column (1), except that rather than weighting by $\sqrt{n}$, we follow the suggestion of Saxonhouse (1976), who shows that weighting by the inverse of the error variance attached to ϕ_k from Eq. (1) is appropriate when regression parameters are used as a dependent variable (this assumes observations are independent). That is, weights are proportional to the precision with which each area's wage differential is estimated. Results using this alternative weighting scheme are highly similar to those obtained when weighting by $\sqrt{n}$ (or, in results now shown, using OLS).

In column (6), we present the hospital density and market size coefficients based on results from a *single-step* log wage equation estimated with individual worker observations ($n = 145{,}127$), which includes the grouped area data and appropriate interaction terms with the size dummies. This provides similar information to that provided by the two-step procedure, but standard errors are biased downward rather substantially owing to matching grouped to individual data (e.g., Kloek, 1981; and Dickens and Ross, 1984). Shown in column (6) are coefficients on the log of hospital density variable and the nursing-size dummies from the single-step estimation. As expected, point estimates are similar to those shown previously, but t-ratios are substantially higher due to the downward bias in standard errors.

In results not shown, we estimated both first-and second-step models separately for the periods 1985–89 and 1990–93. Splitting the sample reduces substantially the number of observations within labor markets from which we estimate relative wage differentials, but does allow us to test for differences between periods. The earlier years correspond closely to a period of sustained reports of nursing shortages, while during more recent years there has been an easing of reported shortages, due in no small part to the significant wage increases that have occurred. Hence, results from the later period may differ significantly from the earlier period, or lead us to overlook evidence supporting monopsony for the earlier period. The pattern of size coefficients was similar during the two periods, however; no evidence for monopsony is found in either period.

The results in Table 4 clearly reject the monopsony model prediction of a positive relationship of relative RN wages with the number of employers and market size. The competitive labor market model would predict an *absence* of a relationship between nursing relative wages and market size, following control for other wage determinants. Neither the competitive model nor the monopsony model provides an obvious explanation for the negative relationship evident in Table 4. Taking the estimates at face value, the implication is that RNs fare worse in large urban areas, relative to alternative employment opportunities. Lower relative wages in large markets would be consistent with a finding that nursing shortages are most severe in large rather than small markets.

If we assume that decreasing relative wages with respect to market size reflects an equilibrium differential, then the more likely explanation is the presence of unmeasured worker and job quality attributes correlated with market size. If unmeasured nursing labor quality rises more slowly with respect to size than does unmeasured non-nursing quality, our test is biased against the finding of monopsony. This latter pattern would help explain why relative nursing wages decline with market size. Given the magnitude of the negative relationship, however, the bias from relative differences in unmeasured ability would have to be implausibly large for us to have falsely rejected the monopsony outcome were it in fact present. That is, unmeasured nursing skills would have to rise with respect to market size far more slowly than do non-nursing labor skills in order for us not only to fail to find evidence for monopsony, but also find a pattern of results the opposite of that predicted by monopsony. While we are confident that such a large bias is not present, we explore in Section 6 alternative tests of monopsony.

5.3. Evidence for licensed practical nurses and nursing aides

In contrast to hospital RNs, sustained and systematic shortages of LPNs and nursing aides have not been evident. Moreover, the extent of occupation-specific training is substantially less for LPNs and aides than for RNs; hence occupational mobility is relatively greater than among RNs. For these reasons, monopsonistic wage–employment outcomes are less likely to be evident among LPNs and aides than among RNs. In order to provide a check and comparison on our hospital RN results, we estimate our two-step model for all RNs, LPNs, and aides. As before, this model measures the relationship between the labor-market specific wages of each nursing group relative to their selected control group and hospital density and market size. These results are presented in Table 5. To facilitate a comparison with hospital RNs, we present the second-step results for this group, as previously shown in Table 4, column (1).

As expected, the results for all RNs largely mirror those for hospital RNs. This is not surprising given that hospitals are the primary location of employment for

RNs (most young RNs are employed in hospitals; this proportion declines steadily with age). Results in Table 5 indicate that the relative wages of neither LPNs nor nursing aides are significantly related to hospital density. These results also are expected since hospital employment is relatively less important a location of employment for LPNs and aides than for RNs. The relative wages for LPNs tend to decline moderately with respect to market size, but the magnitude of the size differentials is small and not quite statistically significant at the 0.05 level ($F(7,231) = 2.004$). [20] Aides tend to have slightly higher relative wages in larger markets (except for the largest cities), but these differences are not close to being statistically significant ($F(7,234) = 1.349$). In short, we find no evidence that relative wages for LPNs or aides follow a pattern that would support the hypothesis of monopsonistic power. Of course, such a finding would be surprising, given that no such evidence could be found for RNs.

6. Additional evidence on monopsony and the market for nurses

In this section, we examine alternative implications of the monopsony model. We estimate differences across the market size categories in union wage premiums, in slopes of earnings–experience profiles, in wage dispersion, and in the employment mix (for previous efforts along these lines, see, among others, Adamache and Sloan, 1982). In order to conserve space, we present results only for hospital RNs and their control group, since monopsonistic outcomes should be most evident among hospital RNs.

Unions may be a countervailing force against monopsony power. If so, we would expect union wage premiums for hospital RNs to be relatively largest in small, less competitive markets. A problem in testing this thesis is that wage standardization by labor unions leads to larger union premiums in small markets not just for RNs, but also among the general labor force (Hirsch and Addison, 1986, Ch. 5). It is necessary, therefore, to compare the size pattern of union wage premiums for RNs with the pattern for workers in the RN control group.

We estimate separate log wage equations for hospital RNs and the control group of college-educated female workers in selected non-health occupations. Included in the regressions are our set of standard control variables, a union coverage dummy, and the interactions of union coverage with size dummies for the seven groups of metropolitan areas. The union coverage coefficient (shown in Table 6, lines 1a and 1b, column 1) provides an estimate of the union premium in

[20] There are no LPNs in our sample in three small MSAs. If the hospital RN market were monopsonistic but the LPN market competitive, one might predict a substitution away from RNs and toward LPNs, and higher wages for LPNs, in small markets. If long-run market supply curves for LPNs are highly elastic, however, demand shifts will have little effect on relative wages.

Table 5
Second-step WLS regression results, determinants of area relative wage differentials for nursing personnel

	Hosp. RNs	All RNs	LPNs	Aides
Size 2:	0.0332	0.0453	−0.0398	0.0095
100K–200K	(1.40)	(2.04)	(−1.41)	(0.45)
Size 3:	−0.0454	−0.0417	−0.0372	0.0439
200K–300K	(−1.84)	(−1.80)	(−1.24)	(2.00)
Size 4:	−0.0341	−0.0291	0.0120	0.0325
300K–500K	(−1.36)	(−1.24)	(0.40)	(1.62)
Size 5:	−0.0541	−0.0410	−0.0443	0.0411
500K–1M	(−2.14)	(−1.72)	(−1.46)	(2.11)
Size 6:	−0.0540	−0.0490	−0.0460	0.0404
1M–2M	(−1.77)	(−1.71)	(−1.24)	(1.76)
Size 7:	−0.1037	−0.0882	−0.0730	0.0461
2M–5M	(−3.37)	(−3.05)	(−1.93)	(1.97)
Size 8:	−0.1571	−0.1347	−0.1270	0.0248
5M and over	(−3.91)	(−3.57)	(−2.57)	(0.81)
ln(Hosp/SqMi)	0.0035	0.0070	0.0021	−0.0090
	(0.34)	(0.72)	(0.17)	(−1.05)
Training	0.0934	0.1908	0.1303	−1.0756
	(0.21)	(0.45)	(0.23)	(−2.71)
Region (8)	yes	yes	yes	yes
n	252	252	249	252
R^2	0.268	0.261	0.183	0.120
F	4.945 *	5.172 *	2.004	1.349

* Signifies that the F statistic permits rejection at the 0.05 level of the null that the size coefficients are jointly equal to zero. T-ratios in parentheses. See notes to Table 4. Estimation procedure is identical to that in column (1) of Table 4, as explained in the text (Eqs. (1) and (2)). The unit of observation is area, with 50 non-urban state groups and 202 MSA/CMSAs. Three MSA cells had no LPNs in the sample. The dependent variable is the first-step regression estimate of the area-specific log wage differential between the nursing group and the control group, conditional on person-specific variables included in the first-step equation. Estimation is by weighted least squares (WLS) using $\sqrt{n}$ as weights, with n the joint nurse and control group sample size in each area.

the non-urban state areas, while the coverage–size interaction terms measure the *difference* in the premium between the non-urban areas and the corresponding metropolitan size categories. Among hospital RNs, a relatively larger union premium is found for the rural reference group than most of the metropolitan area groups (the premium is 0.03 in the non-urban state areas, while interaction terms are negative), consistent with the monopsony prediction. Union wage effects overall are small (and often negative) and have large standard errors. The null that the coverage–size interaction terms are jointly zero is marginally rejected. Standard errors would of course be even larger were we to use a two-step estimation

Table 6
Area size differences in union wage premiums, slopes of experience profiles, wage dispersion, and the RN-labor mix

		Non-urban state reference group	Size 2	Size 3	Size 4	Size 5	Size 6	Size 7	Size 8	F
1a.	Union Coverage and Size * Coverage: Hospital RNs	0.0296 (2.48)	−0.0570 (−1.64)	−0.0100 (−0.30)	−0.0328 (−1.36)	−0.0200 (−0.93)	−0.0585 (−2.38)	0.0257 (1.52)	−0.0168 (−1.07)	2.602 *
1b.	Union Coverage and Size * Coverage: RN Control Group	0.1063 (17.75)	−0.0147 (−0.91)	−0.0367 (−2.53)	−0.0403 (−2.97)	−0.0390 (−3.88)	−0.0532 (−4.61)	−0.0562 (−6.38)	−0.0351 (−4.27)	7.441 *
2a.	ln(Experience) and Size * ln(Experience): Hospital RNs	0.0284 (7.84)	0.0010 (0.13)	0.0105 (1.22)	0.0118 (1.46)	0.0127 (1.90)	−0.0031 (−0.42)	0.0014 (0.28)	−0.0007 (−0.13)	1.137
2b.	ln(Experience) and Size * ln(Experience): RN Control Group	0.0414 (24.82)	−0.0023 (−0.56)	−0.0033 (−0.87)	−0.0047 (−1.30)	−0.0013 (−0.49)	0.0030 (1.03)	−0.0019 (−0.85)	−0.0016 (−0.72)	0.781
3a.	C.V.-Hospital RNs	9.95	8.92	9.70	9.57	10.59	10.00	10.05	10.20	–
3b.	C.V.-RN Controls	17.54	17.51	17.49	17.45	17.45	17.33	17.34	17.34	–
4.	RNs/All Personnel	0.225	0.243	0.251	0.244	0.249	0.250	0.252	0.248	–

Regression estimates in lines 1a–1b and 2a–2b are based on CPS micro log wage regressions for hospital RNs and for the RN control group, with a full set of control variables included, as shown in Table 3. In lines 1a–1b and 2a–2b, the coefficient shown under the column headed as non-urban reference group is either union coverage or ln(Experience); the coefficients under the columns headed Size 2 through Size 8 are marginal effects relative to the reference group, based on interactions of size dummies and either union coverage or ln(Experience). Each F statistic in the last column tests the null hypothesis that the set of 7 interaction terms shown on the row are jointly equal to zero, with * indicating that the null is rejected at the 0.05 significance level. The coefficients of variation shown in lines 3a–3b are based on the specification shown in Table 3 (absent size dummies), with micro log wage equations estimated separately by size category. Line 4 shows the ratio of RNs/All Personnel in hospitals, based on data from *Hospital Statistics*, 1989–90 edition.

similar to that used previously to examine relative nursing wage differentials. [21] For the control group of female workers, union wage premiums are much larger than among RNs, but we observe a similar area size pattern, with lower premiums in urban areas of all sizes. The evidence on union wage premiums does not allow us to reject the hypothesis that unions are a countervailing force to monopsony in small labor markets, but neither does it provide clear-cut support for this hypothesis.

Monopsony power is most likely to be evident among relatively older workers who are less mobile due to community ties, marriage, children, occupation-specific skills, and rates of return to investment in migration and training that diminish with respect to age. In a competitive market where workers are mobile across employers (but not labor markets), nursing personnel should receive a wage approximating their marginal revenue product. But in a monopsonistic labor market, immobile older workers are less likely to receive competitive wages, and thus wage–experience profiles should be flatter. Lehrer et al. (1991, Table 5) examine in some detail earnings profiles and returns to education by experience group among Illinois nurses. They find little difference in the slopes of earnings profiles between RNs in metropolitan and nonmetropolitan areas. As in the case of union wage premiums, it is important that differences in slopes of profiles across different size markets among nursing personnel be compared to similar evidence for non-health related workers.

Table 6 (lines 2a and 2b) provides coefficient estimates on a log(experience) variable, and log(experience) interacted with the seven metropolitan size dummies, estimated separately in wage equations for hospital RNs (line 2a) and for the corresponding control group (line 2b). The log of experience is included in place of experience and experience squared in order to facilitate easy comparison across city sizes, yet still permit a concave log wage–experience profile. Looking first at line 2b, significant differences among the control group are not found across the size categories in the slopes of experience profiles (the null of equivalent slopes cannot be rejected). As predicted by the monopsony model, however, wage growth among RNs, as proxied by the slope of the experience profile, appears somewhat larger in small urban markets than in rural labor markets (line 2a). Standard errors are very large, however, and there is no difference in slopes between rural and the large metropolitan area markets. As with the prior evidence on union wage

[21] We also estimated a specification that includes the interaction of union coverage with hospital density. Counter to the monopsony model, which predicts that union–nonunion wage differentials should be larger in less competitive markets, we obtain a positive coefficient on the coverage-hospital interaction term.

premiums, evidence in support or in opposition to the hypothesis of monopsony effects on wage growth is not clear-cut. [22]

If married nurses with spouse present are relatively less mobile geographically than are never married and previously married nurses, than the wage penalty associated with low mobility should be greatest in small markets where there is the least competition. In work not shown, we estimate a wage equation in which we interact marital status with both the log of hospital density and the market size dummies. Neither the marriage–hospital interaction nor the set of marriage–size interaction terms are close to statistical significance, although signs are largely as predicted.

The presence of monopsonistic power by employers might permit the exercise of wage discrimination (i.e., bidding up a labor supply curve). This should be evinced by greater wage dispersion among nurses with similar productivities (characteristics). [23] To examine this thesis, we estimate separate log wage regressions for hospital RNs within each labor market size category. The coefficient of variation (100 times the standard deviation of the error term divided by the sample mean of the log wage) attaching to each equation provides a measure of the dispersion of wages standardized on measurable characteristics and indexed by the wage level (these are presented on line 3a of Table 6). Similar evidence is presented for the RN control group. Although monopsonistic wage discrimination might lead to greater dispersion in small than in large markets, differences by market size follow no clear pattern. The only outliers appear to be a relatively low C.V. among RNs in the smallest MSAs (Size 2) and a high C.V. in MSAs with populations 0.5–1 million (Size 5). Wage dispersion among the control group workers, standardized on characteristics, is highly similar across labor markets, although a tendency toward lower dispersion in larger markets is evident. If monopsony power among RNs exists, it does not appear to be exercised via individual-based wage discrimination.

[22] Because the proportion of RN employment in hospitals declines with age, the estimated slope of the earnings–experience profile among *all* RNs (without control for sector of employment) would be biased downward, owing to the movement of RNs from the higher wage hospital to lower wage non-hospital sector. The estimated slope of the *hospital* RN experience profile (shown in Table 6) will be biased upward (downward) if relatively more able (less able) experienced RNs remain in the hospital sector. The relatively flat cross-sectional earnings profiles observed for RNs are misleading, resulting from substantial upward profile shifts (i.e., vintage effects) for more recent cohorts. That is, cross-section profiles compare the earnings by experience across different birth cohorts, rather than following wage growth among given cohorts over time. Using a 21 year time-series cross-section of CPS files (1973–93), we estimate longitudinal profiles for various nursing cohorts. As expected, real wage growth for most cohorts of RNs has been substantially faster than that implied by the cross-section profile.

[23] A flatter experience profile lowers total wage dispersion. Here we examine whether there is greater within-cell wage dispersion, measured by dispersion following the control for experience and other wage related characteristics.

A final (albeit rather crude) test of the monopsony model utilizes information on factor mix in hospitals, or specifically, the ratio of RNs to total personnel. [24] As compared to outcomes in competitive markets, monopsony is predicted to produce both lower wages and lower relative use of RNs. As evident in line 4 of Table 6, the relative use of RNs is somewhat lower in non-urban hospitals, but varies little across hospitals in different size urban markets. [25] As was the case with differences in relative wages, union premiums, and the slopes of earnings profiles, small differences found between rural and small metropolitan markets are consistent with the monopsony model. Differences across urban areas of different sizes, however, provide no evidence in support of the monopsony model.

7. Conclusions

The hypothesis of monopsony power in nursing labor markets is frequently used to explain reported shortages of hospital RNs. Although evidence (Sullivan, 1989) has indicated that hospitals face upward sloping labor supply curves (a necessary condition for monopsony), this is not sufficient to establish monopsonistic effects on employment and wages. Previous empirical studies examining more directly the effects of market structure on wages have reached conflicting conclusions and obtained results that are often inconclusive.

This study uses individual worker data from the Current Population Surveys for the period 1985–93 in order to examine the wages of hospital and non-hospital RNs, LPNs, and nursing aides, relative to alternative control groups of female non-health related workers. Labor markets are defined by individuals' location in one of 252 U.S. geographic areas – 202 metropolitan areas and 50 non-urban state groups. Little support is found for the monopsony model. Contrary to predictions of the monopsony model, the relative wages of nursing personnel are not related to hospital density and tend to decrease rather than increase with respect to labor market size. Additional evidence on union wage effects, slopes of wage–experience profiles, wage dispersion, and the employment of RNs relative to other hospital personnel provides little support for the view that monopsony power plays an important role in nursing labor markets.

Important advantages of the study have been the ability to examine recent evidence on wage determination for large representative samples of the U.S. nursing workforce across labor markets of different size, as well as for large

[24] Robinson (1988) has examined this thesis in some detail (see footnote 3). The data on employment by area are taken from *Hospital Statistics*, 1989–90 edition. Results using an alternative measure of employment mix, the ratio of RNs to patient days, are highly similar to those shown.

[25] We do find that the use of LPNs relative to all personnel (or patient days) declines with respect to market size. This is consistent with the thesis that hospitals in larger cities utilize more sophisticated technologies and have a relatively lower demand for LPNs.

economy-wide control groups. Despite the advantages of these data, the study has inherent limitations. A crucial assumption in the paper is that the use of control groups of female workers in non-health related occupations with similar measured characteristics is an appropriate way to measure relative wages. The advantage of using area-specific control groups is that unmeasured differences in cost of living, labor quality, working conditions, and area amenities are controlled for, to the extent that area differences in unmeasured wage determinants affect nursing personnel and the control groups in a similar manner. We have argued in the paper that the control group assumption is reasonable, and that these groups provide an appropriate opportunity cost measure of the long-run alternatives available to nursing personnel. Although not a perfect control, there is no inherent bias unless relative nurse/non-nurse differences in unmeasured labor quality are correlated with hospital density and market size (the correlation with size must be strongly negative for us to have incorrectly rejected the monopsony hypothesis).

An additional limitation of the study is that our 50 non-urban state groups do not constitute unique labor markets, in part because of large distances between non-urban residents within the same state, and in part because of the close proximity of some rural residents to urban labor markets. In addition, a very small number of metropolitan areas are in close enough proximity to each other such that they might be considered a single labor market (this concern is why we chose to keep CMSAs whole, rather than break up the largest metropolitan areas into their component PMSAs). It also might be argued that there are distinct sub-markets within the largest metropolitan areas, although this argument requires the unrealistic assumption that labor mobility is highly limited within large metropolitan areas. Given the limitations of our data for non-urban areas, we cannot rule out the possibility that monopsony power is exercised among the relatively small number of nursing personnel residing in rural markets. But the absence of statistically significant differences in relative wage outcomes between non-urban state groups and metropolitan areas and between small and large metropolitan areas, as well as the absence of a relationship between relative wages and hospital density, suggest that monopsony power is rather limited in scope. Certainly, monopsony should be neither routinely nor uncritically provided as an explanation for reported shortages in nursing markets.

Although the analysis casts considerable doubt on the validity of the monopsony hypothesis, we have left unexplained why chronic shortages of registered nurses have periodically characterized nursing labor markets. The most straightforward explanation (McKibbin, 1990) is that shortages have been the result of a continuing expansion of demand, particularly for highly-skilled nurses, coupled with lagged responses in salary increases and in nursing training and labor supply. Demand increases have resulted from a growing elderly population, an increased ratio of RNs to hospital beds as a complement to more sophisticated medical technologies, and an expansion of non-hospital employment opportunities (McKibbin, 1990, pp. 15–19). By this scenario, shortages may last for relatively long periods, but should not be permanent. The substantial increase in relative nursing wages during the late 1980s and early 1990s, coupled with the easing of reported shortages in the past two years, are consistent with this explanation. We find the competitive labor market explanation persuasive, at least compared to the alternatives. It would be more compelling, however, if there were evidence available from studies that explicitly model and estimate dynamic labor demand and supply models.

The constancy of relative nursing wages with respect to hospital density and, to a lesser extent, market size (relative wages decrease rather than increase with respect to size) is largely consistent with the competitive model and inconsistent with traditional approaches to monopsony. One cannot rule out the possibility, however, that monopsony power is present throughout the nursing labor market, and is at least as strong in large cities with numerous employers as in smaller more concentrated markets. That is, one might argue that most employers behave as if they were monopsonists, even where there are large numbers of firms and worker mobility across employers. The development of models that would predict low wage, excess demand equilibria in the presence of many firms is beyond the scope of this paper. Such models, however, might help explain sustained nursing shortages and similar relative wages across small and large markets. Resort to such models has recently occurred as a means of explaining the relatively small employment effects of minimum wage laws. [26] Absent substantial theoretical progress and compelling evidence in support of a new generation of models with monopsonistic outcomes, however, we would not emphasize this explanation for nursing shortages. Our analysis casts considerable doubt on the hypothesis that the traditional monopsony model is useful in understanding either wage determination or shortages in nursing labor markets.

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[26] See, for example, Card (1992) for U.S. evidence and Machin and Manning (1994) for the U.K. Machin and Manning discuss the relevance of a dynamic monopsony model by Burdett and Mortensen (1989) that relies on the assumption of limited information by workers of alternative wage opportunities (such an assumption appears less tenable in the market for hospital RNs than for other labor markets). Rebitzer and Taylor (1993) construct an efficiency wage model in which monitoring costs increase with firm size, potentially leading to an upward sloping firm supply curve (a positive wage–employment relationship) and implications similar to monopsony.

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