Industrial & Labor Relations Review

Volume 57, *Issue* 3 2004 *Article* 6

The Effect of Registered Nurses' Unions on Heart-Attack Mortality

Michael Ash* Jean Ann Seago†

*University of Massachusetts, Amherst,
†University of California, San Francisco,

The Effect of Registered Nurses' Unions on Heart-Attack Mortality*

Michael Ash and Jean Ann Seago

Abstract

Although hospital work organization affects patient outcomes and in some states registered nurses (R.N.'s) are increasingly forming unions, the relationship between R.N. unions and patient outcomes has received little attention. This study examines the relationship between R.N. unionization and the mortality rate for acute myocardial infarction (AMI), or heart attack, in acute-care hospitals in California. After controlling for patient and hospital characteristics, the authors find that hospitals with unionized R.N.'s have 5.5% lower heartattack mortality than do non-union hospitals. This result remains substantively unchanged when the analysis accounts for possible selection bias—specifically, the possibility that unionized hospitals have certain important but unobservable characteristics, independent of unionization, that affect patient care.

*For financial support, the authors are grateful for the 1998 American Nurses Foundation grant, UCSF, the Center for the Health Professions (Edward O'Neil and Jonathan Showstack, Directors), and the UCSF Center for California Health Workforce Studies (Kevin Grumbach, Director). For data, they thank Harold S. Luft and Patrick Romano from the California Hospital Outcomes Project (CHOP), sponsored by the California Office of Statewide Health Planning and Development (OSHPD). For editorial comments, they thank George Akerlof, University of California, Berkeley; Lee Badgett, University of Massachusetts, Amherst; and Joanne Spetz, Research Fellow, Public Policy Institute of California and Assistant Adjunct Professor, University of California, San Francisco. For research assistance, they thank Radoslaw Macik and the Department of Economics at the University of Massachusetts, Amherst, and David Biondi-Sexton and the Department of Community Health Systems at UCSF.

THE EFFECT OF REGISTERED NURSES' UNIONS ON HEART-ATTACK MORTALITY

MICHAEL ASH and JEAN ANN SEAGO*

Although hospital work organization affects patient outcomes and in some states registered nurses (R.N.'s) are increasingly forming unions, the relationship between R.N. unions and patient outcomes has received little attention. This study examines the relationship between R.N. unionization and the mortality rate for acute myocardial infarction (AMI), or heart attack, in acute-care hospitals in California. After controlling for patient and hospital characteristics, the authors find that hospitals with unionized R.N.'s have 5.5% lower heart-attack mortality than do non-union hospitals. This result remains substantively unchanged when the analysis accounts for possible selection bias—specifically, the possibility that unionized hospitals have certain important but unobservable characteristics, independent of unionization, that affect patient care.

During a lengthy and acrimonious dispute with the Kaiser Permanente healthcare corporation, the California Nurses Association (CNA), a labor union for registered nurses (R.N.'s), mounted a public relations campaign with the message that changes in the work of R.N.'s and other healthcare workers jeopardize patient safety and negatively influence patient outcomes (Sherer 1994). Kaiser Permanente, of course, denied the charges.

The role and image of unions for R.N.'s in California have changed in the past de-

*Michael Ash is Assistant Professor, Department of Economics and Center for Public Policy and Administration, University of Massachusetts, Amherst. Jean Ann Seago is Associate Professor, Department of Community Health Systems, and Research Fellow, Center for the Health Professions, University of California, San Francisco (UCSF), and she was 1998 American Organization of Nurse Executives/American Nurses Foundation Scholar.

For financial support, the authors are grateful for the 1998 American Nurses Foundation grant, UCSF, the Center for the Health Professions (Edward O'Neil and Jonathan Showstack, Directors), and the UCSF Center for California Health Workforce Studies (Kevin Grumbach, Director). For data, they thank Harold S. Luft and Patrick Romano from the California Hospital Outcomes Project (CHOP), sponsored by the California Office of Statewide Health Planning and Development (OSHPD). For editorial comments, they thank George Akerlof, University of California, Berkeley; Lee Badgett, University of Massachusetts, Amherst; and Joanne Spetz, Research Fellow, Public Policy Institute of California and Assistant Adjunct Professor, University of California, San Francisco. For research assistance, they thank Radoslaw Macik and the Department of Economics at the University of Massachusetts, Amherst, and David Biondi-Sexton and the Department of Community Health Systems at UCSF.

A data appendix with additional results and copies of the computer programs used to generate the results presented in the paper are available from Michael Ash, Department of Economics, Thompson Hall, University of Massachusetts, Amherst, MA 01003; mash@econs.umass.edu.

Industrial and Labor Relations Review, Vol. 57, No. 3 (April 2004). © by Cornell University.
0019-7939/00/5703 $01.00

cade as nurses have increasingly joined unions.[1] R.N. unions have characterized changes in the healthcare sector as a threat both to their members and to the well-being of patients (American Organization of Nurse Executives 1994; Harris 1996; Sherer 1994). On October 1, 1995, the CNA severed its association with the American Nurses Association (ANA), the leading professional association for R.N.'s, which has had an ambivalent orientation regarding the unionization of nurses (California Nurse 1994; Moore 1995). The build-up to the disassociation produced tension in the California nursing community and among nurse managers, nurse educators, and staff nurses. Although changes in the organization of healthcare are associated with patient outcomes, little evidence yet supports the spirited rhetoric of either healthcare corporations or care providers' unions about the organization of work and patient outcomes.

Other than Seago and Ash's (2002) analysis of mortality from acute myocardial infarction (AMI), or heart attack, there has been little systematic investigation of how R.N. unionization affects patient outcomes. Our earlier work on a sample of California hospitals showed that, controlling for a host of observable hospital and patient characteristics, hospitals with unionized R.N.'s have lower heart-attack mortality rates than do non-union hospitals.

Unobservable features of unionized hospitals may confound an assessment of causality in the relationship between unionization and patient health. In this study, we examine the relationship between the heart-attack mortality rate and the presence of an R.N. union in the acute-care hospitals of California, and we devote special attention to the potential role of unobserved confounding factors. The data collected by Seago and Ash (2002) include both the union status of non-healthcare hospital workers and the date of any subsequent R.N. unionization for hospitals that were not unionized at the time of the study period. We use the additional data to apply specification and selection tests to the basic association. The unionization of non-healthcare workers demonstrates the broad unionizability of the hospital but should not directly affect patient outcomes. Hospitals that are yet-to-unionize may share characteristics of currently unionized hospitals, but if the union effect is causal, they should not manifest the lower mortality of the actually unionized hospitals.

Work Organization and Productivity

Many studies show that organizational characteristics of hospitals influence patient outcomes. Showstack and others (Flood, Scott, and Ewy 1984b, 1984a; Mitchell and Shortell 1997; Showstack, Kenneth, and Garnick 1987) have found that higher patient volumes in hospitals are associated with reduced patient mortality in both medical and surgical patients. These studies conclude that patient volume is a proxy for experience or expertise of the hospital staff and care providers. In a quasi-experimental setting that controlled for hospital-patient-procedure selection, McClellan, McNeil, and Newhouse (1994) and McClellan, Henson, and Schmele (1994) found that cardiac technology was of limited value in reducing heart-attack mortality, but that the quality of care in the first 24 hours following the attack had important effects on the likelihood of death. Aiken, Smith, and Lake (1994), Aiken and Fagin (1997), and Aiken, Slaone, Lake, Sochalski, and Weber (1999) found that the organizational characteristics of "magnet hospitals," distinguished by good human-resource practice for R.N.'s, are related to decreased 30-day mortality rates and improved patient satisfaction. The

[1]While the national R.N. union coverage rate slipped slightly over the 1990s, from about 20% to about 18% (Hirsch and Macpherson 2003), California has had a resurgence of organizing activity; in our sample of California hospitals, unions organized at 12 hospitals and were decertified at none between 1993 and 1998. Between 1997 and 2002 alone, the CNA won elections to represent R.N.'s in over 20 hospitals (Joanne Spetz, personal correspondence, September 26, 2002).

first of those studies showed that good outcomes are not simply a matter of skill mix, measured by the ratio of R.N. hours to total nursing staff hours, but are also related to nurses' autonomy and control of practice (Aiken et al. 1994). Aiken et al. (1999) found that a higher ratio of R.N.'s to patients is associated with reduced 30-day mortality and increased patient satisfaction. Kovner and Gergen (1998) found that, controlling for a wide variety of hospital characteristics, the number of care hours provided by R.N.'s is inversely related to adverse events following surgery, including urinary tract infection and pneumonia. Employee satisfaction is also related to positive patient outcomes (Aiken et al. 1994).

Unions can have complex effects on health organizations and thereby affect the quality of care. We explore three avenues through which unions may affect the quality of care. Under labor law, the unionization decision of R.N.'s at a hospital designates the union as the exclusive bargaining agent for all R.N.'s at that hospital and mandates collective bargaining between the nurses' union and hospital management. The institutional arrangement of collective bargaining mandates that management and the union negotiate in good faith on a defined set of conditions and terms of employment, including wages and benefits, overtime, the work burden, and staffing levels.

The most convincing analyses of union productivity effects have examined physical output rather than value-added or other output measures, because unions may affect pricing strategies as well as physical product (Booth 1995). In an early survey of the union effect on productivity, Freeman and Medoff (1984) found a positive association in many studies, although the case for causality was limited and the analysis was limited to goods production (construction, manufacturing, and mining). More recent studies, surveyed in Booth (1995) and Addison and Hirsch (1989), have found differing effects across industries and depending on research design. A firm-level analysis of the cement industry found a positive union effect on productivity (Clark, cited in Booth 1995).

Register (1988) examined productivity and cost differences between union and non-union hospitals. He used data on two sets of hospitals that varied in union status: hospitals in metropolitan areas that are either overwhelmingly union or non-union; and a sample of union and non-union hospitals in Ohio. The study found both higher wages and higher productivity in union hospitals. The productivity advantage more than offset the higher wage; thus the unit labor cost was, on average, lower in union hospitals. The measure of output in the study—days of patient care stratified by intensity—was consistent with the application of the goods-production model to the provision of services.

In the context of the union productivity literature that addresses the quality of output in the public and quasi-public service sector (health, child and elder care, and education, for example), our study follows a new line of inquiry. The quality dimension of productivity is particularly important in this sector because consumers have difficulty monitoring quality and contracting on the basis of quality, and neither market nor public mechanisms may discipline bad performers. The results in these studies have varied across sectors and research designs. For example, Hoxby (1996) found that student performance, measured by high-school completion, declines in school districts undergoing unionization drives. Howes (2001) found increased patient satisfaction in home care after unionization and a large wage increase.

Wage Effects and Seniority

Research indicates that R.N. unions are associated with slightly higher R.N. wages (Hirsch and Schumacher 1995; Wilson, Hamilton, and Murphy 1990), although Hirsch and Schumacher (1998) found smaller union wage premiums among healthcare workers than among similar workers in other industries and smaller union premiums among R.N.'s than among less-skilled health workers.

A higher wage may shock management

into improving H.R. practice and other aspects of work organization (Booth 1995). For example, a higher wage might improve the quality of the nursing staff by changing hospital hiring practices. Facing the higher wage, employers may become more selective in hiring and limit hires of nurses with the associate's degree in nursing in favor of nurses with bachelor's degrees or graduate education.

Unions typically institute seniority systems, which raise the wage and improve the working conditions of nurses with greater tenure. The literature shows some evidence that higher wages reduce nurse turnover (Aiken 1989; Spetz 1996). Seago and Ash (2002) demonstrated that nurse turnover is lower in hospitals with R.N. unions. The higher wage for unionized nurses and the seniority system may discourage quits, leading to a nursing work force with longer tenure at the particular hospital, greater experience with the procedures of the hospital, and better capacity to meet patient needs. On the other hand, unions may offer job protection for nurses with poor work records, and this aspect of unionization would, presumably, be bad for patient outcomes.

Addison and Hirsch (1989) reported that the union productivity effect is largest in industries where the union-non-union wage differential is highest. Given the historically low union premium in nursing, which our study corroborates, we might expect only a limited productivity effect. However, in recent work, Currie, Farsi, and MacLeod (2002) noted that in hospital-nurse bargaining, working conditions other than wages may be more contested. If the union effect on working conditions is large but manifests itself in ways other than an increased wage, then a substantial productivity effect would be consistent with Addison and Hirsch (1989).

Formal and Informal Communication

The increased communication mandated by the collective bargaining process may directly facilitate the transfer of information that can reduce patient mortality. The particular contents of formal communication under collective bargaining, that is, wages and working conditions, would not seem likely to contribute directly to reduced mortality, although we do not dismiss the possibility out of hand.

The collective bargaining relationship may facilitate other formal modes of communication. Labor law forbids negotiation between management and non-union employee organizations; the reason for the provision is to prevent management from manipulating such organizations to turn them into "company unions." Employers may hesitate to form collaborative committees out of concern that establishing employee organizations without the collective-bargaining relationship would constitute a violation of labor law. Also, employees may have reservations about participating in non-union employee organizations. Thus, the collective bargaining relationship may facilitate other formal modes of communication between nurses, hospital management, and the medical staff, such as quality circles, which Baskin and Shortell (1995), Shortell and Hull (1996), Shortell, Jones, Rademaker, et al. (2000), Shortell, Zazzali, Burns, et al. (2001), and Ferlie and Shortell (2001) have identified as improving patient outcomes.

The effect of unionization on informal communication may be a more important channel for affecting quality of care. By offering protection from arbitrary dismissal or punishment and by implementing a grievance procedure, R.N. unions may limit the intensely hierarchical character of the relationship between nurses and the medical staff. The protections offered by unionization may encourage nurses to speak up in ways that improve patient outcomes but might be considered insubordinate and, hence, career-jeopardizing without union protections (Gordon, Benner, and Noddings 1996; Gordon 1998).

The increased tenure resulting from union negotiation of higher wages and a seniority system may also improve informal communication between R.N.'s and the medical staff of the hospital. Unions may affect the organization of nursing staff or the way nursing care is delivered in a fashion that facilitates R.N.-M.D. communication. This communication exemplifies the "voice" function of unions described by Freeman and Medoff (1984). Communication between R.N.'s and M.D.'s is under constant stress from the hierarchical character of hospital workplace relations, but the physicians and surgeons of a hospital may come to trust the opinion of a particular nurse as the nurse's judgment is borne out over years of actual experience. By increasing the tenure of nurses, nurses' unions may increase the trust between nurses and M.D.'s in a particular hospital setting.

On the other hand, unions may create an adversarial or rule-bound work environment that interferes with communication and care. There are indications in the literature that some R.N.'s view union activity as unprofessional (Breda 1997) and believe that managers and unions are normally adversarial (Breda 1997; Flarey, Yoder, and Barabas 1992). Unionization is associated with declining employee morale and job satisfaction (American Organization of Nurse Executives 1994; Sherer 1994; Harper, Motwani, and Subramanian 1994). Managers and administrators typically view union activity negatively and resist R.N. unionization. Most administrators assume that managing in a union environment makes their jobs more difficult (Harper et al. 1994; Flarey et al. 1992).

Staffing Levels

The work burden is one of the mandated topics of collective bargaining, and staffing levels have been a particular focus of activism by R.N. unions in California. Unions may improve the quality of care by negotiating increased staffing levels, which improve patient outcomes (Kovner and Gergen 1998; Needleman, Buerhaus, Mattke, Stewart, and Zelevinsky 2002). On the other hand, unions may raise wages to such an extent that the employer slows hiring or stops hiring nurses, adversely affecting staffing. There is some indication that unionization of hospitals is related to increasing costs (Wilson et al. 1990).

In summary, there is a substantial literature on patient outcomes related to hospital organizational variables, but there are unanswered questions. In our analysis, we explore the association between the presence of R.N. unions and patient outcomes. It is possible that the wage is the important factor in attracting and retaining high-quality nurses and that unions' only function is to win higher wages. In this case, the wage, not the union, would be the causal factor, and the union would be only an instrumental factor (Hirsch and Schumacher 1995, 1998; Spetz 1996). The situation is analogous for increased staffing as a result of collective bargaining. By controlling directly for wages and staffing, we can examine to what extent unions have an effect on care over and above their direct effect on wages and staffing. The current data will permit only limited resolution of the mechanisms by which unions affect care. Furthermore, we overlook union-effected organizational changes in hospitals that do not affect patient care.

Confounding Factors

Mortality is predicted by patient characteristics, as well as by hospital characteristics and environmental factors. We seek to determine for acute-care hospitals in California if, controlling for these factors, there is a relationship between the heart-attack mortality rate and the presence of a bargaining unit for R.N.'s.

Many complex factors determine the survival of patients suffering from heart attacks. To make our analysis useful and our estimates of the R.N.-union effect unbiased, we must consider to what extent these various factors are correlated with R.N. unionization. Because geographic regions in California are very different from each other and the presence or absence of unions within geographic areas may correlate with other regional characteristics associated with health, we control for regional characteristics as well as hospital and patient characteristics.

Patient, Hospital, and Union Selection

Patient characteristics such as sex, age, race, insurance category, risk characteristics, and illness severity are associated with adverse patient outcomes, including mortality (Aiken et al. 1999). Mitchell and Shortell (1997) argued that mortality rates seem to be more closely related to patient variables, while other adverse events may be a more sensitive marker of organizational variables.

Because a greater proportion of all unionized hospitals than of all non-union hospitals are in urban areas, the heart-attack patients treated in unionized hospitals are likely to differ systematically from those treated in non-union hospitals. We do not directly examine the selection of patients to union or non-union hospitals, but we surmise that patients treated in unionized hospitals have average characteristics that in some dimensions increase and in other dimensions decrease their probability of mortality. For example, patients treated in unionized hospitals are more likely to be African American (an increased risk factor for mortality) but are less likely to have had the substantial delay in treatment associated with transit time to rural hospitals (a decreased risk factor for mortality).

Our strategy for addressing the nonrandom matching of patients and hospitals is to use mortality data that have been risk-adjusted for patient age, gender, type of heart attack, and chronic illnesses. By using both adjusted and raw data, we can directly test whether unionized hospitals indeed receive a nonrandom patient load and whether the patients are, on average, positively or negatively selected. Furthermore, because urban location appears to be such an obvious basis of patient selectivity, we directly include controls for the location of the hospital in a rural or urban area.

Hospital ownership and control. Arrow (1963) suggested that the not-for-profit hospital may have advantages over the for-profit hospital in meeting patient need under conditions of uncertainty in diagnosis and treatment. It is also possible that the not-for-profit organization is an easier environment for unionization, partly because management lacks strong incentives to resist it. If not-for-profit organization is associated both with improved patient outcomes and with unionization, then an analysis that fails to incorporate hospital ownership and control will spuriously associate these characteristics. We control directly for the ownership and control of hospitals by including indicator variables for public and for-profit hospitals, with not-for-profit hospitals as the excluded category.

Expensive technologies. High-value capital assets of a hospital, including sophisticated cardiac technology, may be attractive to unions as a basis for extracting union rents (payments to union labor above the competitive wage). Unions may, therefore, tend to form at these hospitals. Mitchell and Shortell (1997) and others (Hartz, Krakauer, and Kuhn 1989; Manheim, Feinglass, Shortell, and Hughes 1992) have consistently found high technology in hospitals to be related to lower mortality. In contrast, McClellan, NcNeil, and Newhouse (1994) and McClellan, Henson, and Schmele (1994), using a quasi-experimental design, found cardiac technology to be of limited value in reducing heart-attack mortality.

If expensive technologies are both attractive to unions and useful in reducing mortality, a naïve analysis would spuriously find unions associated with reduced mortality. We have a three-fold strategy for addressing this problem. First, we directly control for the presence of expensive cardiac technologies using several measures of these technologies described below. Second, we test how the presence of non-healthcare bargaining units at the hospital, such as unionized engineers and food service workers, affects the mortality outcome. Unions of non-healthcare workers would have the capacity to reap the same rents created by the presence of these expensive technologies, but we expect them to have no effect on mortality. If our analysis finds an effect of non-healthcare unions on mortality, then the union effect is likely spurious.

Last, in addition to union status in the outcome-measurement period (1991–93), we also observe whether hospitals not unionized as of 1991–93 subsequently unionized over the period 1993 to 1998. Hospitals that subsequently unionized are comparable in their characteristics to hospitals that had already unionized. If our analysis shows an effect of subsequent unionization on the earlier mortality outcome, then the association of the effect with unionization is spurious.

Other sources of rent. When certain diseases and medical and surgical procedures are involved, higher volumes of patients treated at the hospital have been shown to improve patient outcomes (Flood et al. 1984b; Luft and Romano 1993; Showstack et al. 1987). Hospitals with more than 100 beds have better patient outcomes than hospitals with 100 or fewer beds (Luft and Romano 1993; Grumbach et al. 1995; Phillips, Luft, and Ritchie 1995). Again, these larger, high-volume hospitals may be a source of rents for organized labor at the hospital, and the same spurious correlation may appear. Our strategy in this case is similar to our strategy for addressing the spurious correlation with high-tech apparatus: we directly control for hospital size and volume of activity, and we use non-healthcare unions and future unionizers as specification tests.

Just as unions may extract rents for their members from the capital stock of a hospital, so too they can extract rents from the human capital stock of other persons working at the hospital. R.N. unions may therefore be attracted to hospitals that have strong prospects of yielding these human-capital rents, such as teaching hospitals or hospitals with many or highly skilled M.D.'s. There are studies indicating that increased R.N. hours, increased M.D.'s, and increased total staff hours have a positive impact on patient outcomes (Shortell et al. 1994; Zimmerman et al. 1991; Zimmerman et al. 1994). Again we control directly for human capital characteristics of the medical and care staff of the hospital, and we use non-healthcare unions and future unionizers to test for spurious association.

Adversarial industrial relations. Adversarial industrial relations may be a cause of unionization rather than an effect. As noted above, employee satisfaction is related to positive patient outcomes (Aiken et al. 1994). If adversarial industrial relations and diminished employee satisfaction cause both unionization and worsened patient outcomes, then our analysis might spuriously associate the union with worsened patient outcomes. Because we have no direct measure of already existing adversarial industrial relations, the measured effect of R.N. unions on patient outcomes will be inclusive of already existing adversarial industrial relations and, hence, biased toward finding that unions are bad for patient outcomes.[2] To the extent that the presence of non-healthcare unions or of future unionizers is a marker for adversarial industrial relations at the hospital, we will address the issue through the identification strategy we have described.

Labor relations are frequently most adversarial in the period immediately after unionization (and they often calm down with the settlement of the first contract). In addition to union status in the outcome-measurement period (1991–93), we observe the date of unionization, enabling us to distinguish between hospitals with recently formed unions (after 1987) and long-standing unions (unionized before or by 1987). We run additional specifications of the model with indicator variables entered separately for hospitals in these two categories. The test of the effect of adversarial relations in a unionized setting is carried out by examining the coefficients on both indicator variables.[3]

[2]A referee observes that in the face of adversarial industrial relations, R.N.'s may actually give better care in order to arouse patient or physician support for the unionization effort, a possibility we acknowledge.

[3]The difference suggests that first-differenced approaches to the effect of unionization, such as that employed by Hoxby (1996) in her study of teacher unionization, may mistakenly capture adversarial relations in the unionization process rather than the effect of the union itself.

Methods

The outcome measure is the risk-adjusted heart-attack mortality rate for the hospital, a measure described in more detail below. Because hospital mortality can be a function of many factors other than whether hospital nurses are unionized, we estimate multivariate regression models to control for these other characteristics. After we explored descriptive statistics stratified by union status, we used a multivariate regression model to test the statistical significance of the variable of interest in the presence of covariates.

Sample

Our analysis includes all acute-care hospitals in California.[4] California was selected because (1) it has risk-adjustment mortality data for AMI, a common Diagnostic-Related Group; (2) Californian hospitals vary in union status; and (3) the investigators are familiar with the healthcare system of the state and associated databases. We made a determined effort to contact every acute care hospital in California, although non-response is potentially nonrandom. Inclusion criteria were all (non-Federal) acute care hospitals in the state of California that reported discharge data in the 1991–93 California Office of Statewide Health Planning and Development (OSHPD) Hospital Disclosure database. We excluded children's hospitals, psychiatric hospitals, rehabilitation hospitals, and long-term care hospitals, because there are too few specialty hospitals in the state to make an adequate comparison. All the Kaiser Foundation hospitals (24 hospitals, with all R.N.'s represented by unions) were excluded because state law exempts them from reporting Hospital Disclosure (financial and accounting) data to OSHPD.

Mortality, Union Status, and Confounding Variables

The dependent variable is the risk-adjusted 30-day AMI mortality rate for the hospital. The risk-adjusted mortality rate at the hospital level was developed in the California Hospital Outcomes Project (CHOP) for the years 1991–93 (Zach, Romano, and Luft 1997; Romano, Luft, Rainwater, and Zach 1997; Luft and Romano 1997; OSHPD 1997).[5] The risk-adjustment model used in the CHOP study accounts for the following patient characteristics: age; sex; type of heart attack; and chronic diseases (specifically, congestive heart failure, central nervous system disease, renal failure, diabetes, malignant neoplasm, hypertension, previous coronary artery bypass graft surgery, thyroid disease) that were present on admission to the hospital (Luft and Romano 1997; Romano, Luft, Rainwater, and Zach 1997). The CHOP study implements a model of patient mortality at the patient level controlling for patient characteristics and including hospital fixed effects. CHOP (cautiously) reports the hospital fixed-effect as a measure of hospital performance. In most specifications, our analysis uses the hospital-level hospital fixed effects as the dependent variable. In one specification, we use the raw AMI mortality rate for each hospital as an alternative dependent variable in order to explore the effect of the risk-adjustment. All variation is at the hospital level.

We pool outcome data from the three study years to reduce measurement error in

[4] A power analysis indicated that a multiple linear regression model including 16 covariates and one predictor (union versus non-union) with a sample size of 236 would have 80% power to detect a squared multiple correlation (R^2) of 0.08 at $\alpha < 0.05$. Our sample of approximately 350 hospitals should therefore be adequate for inference.

[5] Because the outcome variable is a rate between zero and 100%, a logistic regression may be more appropriate. On the other hand, the simple linear regression offers the benefit of easy interpretation of the coefficients. We re-estimated all models with the logistic specification, that is, $\ln(MR/(1-MR))$ as the dependent variable, where *MR* is the mortality rate. The signs and statistical significance of the results were identical.

the outcome variable, although we examined unpooled variants. An important concern is that mortality rates may not reliably reflect quality in hospitals with very small numbers of cases. Table 1, which shows correlation matrices for risk-adjusted mortality rates across years stratified by the number of cases, demonstrates the problem. The median hospital had a three-year average of 51 or more heart-attack cases per year. The cross-year correlation of risk-adjusted mortality for hospitals that had 51 or more cases per year was statistically significant (Table 1, Panel A). The cross-year correlation of risk-adjusted mortality of hospitals that had fewer than 51 cases per year is small and not statistically significant (Panel B). Hospitals with lower numbers of cases produce less reliable measures of risk-adjusted mortality; the risk-adjusted mortality in one year is a poor estimate of the risk-adjusted mortality in another year.

In all models and in the summary statistics, the data are weighted by the number of heart-attack cases, because hospitals with higher numbers of cases have lower variance estimates of risk-adjusted mortality. Although the risk-adjusted mortality numbers were unbiased for all hospitals, the lower variance estimates are closer to a true measure of hospital quality. Specifically, the mortality rate is a more accurate measure of quality for hospitals with more cases. The approach is analogous to treating the data as if they were patient-based rather than hospital-based with hospital-wide values assigned to all patients at the hospital. Because we use the risk-adjusted outcome data and weight the analysis by caseload, our analysis is similar in interpretation to a patient-level analysis clustered by hospital.

The key independent variable is R.N. union status. We ascertained union status first by exploring the web pages of hospitals and government agencies (for example, National Labor Relations Board). We then called R.N. union offices, hospital personnel offices, and hospital nursing offices between December 1998 and April 1999. The telephone survey questions asked about the presence or absence of an R.N. union, the name of the union, the presence or absence of unions for other employees in the hospital, the names of all the other unions, and when all unions were formed.

Table 1. Inter-Year Correlation of Risk-Adjusted AMI Mortality, by Caseload.

	1991	*1992*
A: Hospitals with 51 or More Cases per Year		
1992	0.354 [0.000]	
1993	0.373 [0.000]	0.327 [0.000]
B: Hospitals with 50 or Fewer Cases per Year		
1992	0.016 [0.8351	
1993	0.109 [0.149]	0.171 [0.343]

Notes: Top value is Pearson's r. Bottom value in brackets is p-value.

Most of the additional variables on hospitals come from OSHPD Hospital Disclosure Reports. These data are collected annually from all hospitals in the state and include information about service provision, finances, and resource use. The database also includes information about capital acquisition, labor staffing, and the provision of medical care in each revenue unit of the hospital. The reported and derived variables include the number of hospital beds and discharges from nursing units that care for cardiac-related patients, specifically medical-and-surgical intensive care units, coronary care units, other intensive care units, and acute medical-and-surgical nursing units; a technology index derived from a principal components factor analysis of all the cardiac specialty services; availability of providers, specifically, R.N. hours, total staff hours, and the number of active medical staff with clinical specialties in any cardiovascular specialty, internal medicine, or therapeutic radiology; and the average wage of R.N.'s adjusted for the local area cost of living (Table 2).

The last of those variables was computed as follows. The average hourly wage was computed for R.N.'s in hospital units associated with heart attacks: medical-and-sur-

Table 2. Variable Definitions and Data Sources.

Source and Variable	*Definition*
Dependent Variables	
California Health Outcomes Project (OSHPD)	
Risk-Adjusted AMI Mortality Rate	30-day death rate adjusted for severity of illness, age, and gender of the AMI patient population.
Raw AMI Mortality Rate	30-day raw death rate of the AMI patient population.
Independent Variables	
Primary Data Collection	
R.N. Union, 1993	Union for R.N.'s present as of 1993.
R.N. Union Long	Union for R.N.'s present as of 1987.
R.N. Union New	Union for R.N.'s formed 1988–1993.
R.N. Union Coming	Union for R.N.'s formed 1994–1998.
Non-Healthcare Union	Union for non-healthcare hospital employees, including clerical, engineering, and food services.
Annual Hospital Disclosure Report (OSHPD)	
Size, by Staffed Beds	Number of staffed beds. Three categories: 100 or fewer; 101–300 (reference); 301 or more.
Control or Ownership	Not-for-profit (reference); for-profit; public.
Annual Discharges	Number of hospital discharges from AMI-related cost centers.
Cardiac Technology Index	Constructed by principal components analysis of all the cardiac services; see text.
R.N. Hours per Discharge	Number of R.N. hours per AMI-related discharge.
Non-R.N. Hours per Discharge	Number of total staff hours per AMI-related discharge, excluding R.N.'s.
M.D.'s per Discharge	Number of M.D.'s per AMI-related discharge.
Within-HSA R.N. Wage Premium	R.N. wage relative to R.N. wages in other hospitals in the same Health Services Area.
Rural Hospital	One of 74 hospitals designated as rural acute-care hospitals in California (OSHP D, special correspondence).
Area Resource File (U.S. Bureau of the Health Professions)	
Large Metropolitan	Central counties of metropolitan areas with 1 million or more population.
Medium Metropolitan	Other metropolitan counties.
Non-Metropolitan	County not in a metropolitan area.

gical intensive care; coronary care; other intensive care; and medical-and-surgical acute units. The hospital average wage was regressed on a set of dummy variables for each Health Service Area (HSA),[6] and then residuals from the regression were used as the wage index variable in the final models. The measure indicates how much the hospital's wage for R.N.'s in heart care exceeds that of other hospitals in the HSA. The within-HSA wage residual was used as the wage index to control for price and amenity differences across HSA's, and is, hence, analogous to an opportunity-wage index.[7] Alternative specifications using the

[6]The federal government designates HSA's for planning and resource analysis of hospitals and other health services. There are 205 HSA's in the United States, each consisting of either an entire state or an aggregate of counties (Grumbach et al. 2001). California has 58 counties and 14 federally designated HSA's.

[7]We thank an anonymous referee for this helpful observation.

nominal hospital wage or the hospital wage relative to per capita county income yielded similar results.

The cardiac-technology index is derived from a principal components analysis of all the cardiac specialty services (coronary intensive care unit, cardiac catheterization, echocardiology lab, heart surgery services, open heart surgery services, mobile cardiac care, and cardiac clinic). It replaces all the cardiac service variables. The advantage of the cardiac technology index over individual dummy variables is a reduction in dimensionality that increases the adjusted R-squared. The key results with the cardiac specialty services included separately are very similar to those with the index and are available from the authors. See Table 2 for variable definitions and data sources.

Based on the conceptual grouping of variables, several regression specifications were estimated. All the models include the R.N. union variable as a predictor, because it is the variable of interest. The first specification examines the bivariate relationship between mortality and union status. The second specification includes all of the confounding factors discussed above, including volume (hospital size classes by number of beds and cardiac-related hospital discharges), staff hours per cardiac discharge, the number of M.D.'s per cardiac-related discharge, an indicator for teaching hospital, the cardiac-technology index, and hospital-location variables. Specifications 3, 4, and 5 add the local area wage premium and R.N. hours per cardiac-related discharge, variables that are themselves likely affected by unionization. Hence, specification 5 examines the effect of unionization over and above its effect on both of these specific outcomes of collective bargaining.

Results

Of the 385 eligible acute care hospitals during the sample period, we obtained complete data on 344. Summary statistics for R.N.-union and R.N.-non-union hospitals are reported in Table 3. As of 1993, 27% of the California hospitals in our sample had R.N. unions.[8] Smaller and non-metropolitan hospitals are less likely to be unionized. Union hospitals have more cardiac-related discharges, more M.D.'s per cardiac-related discharge, and more complex technology, specifically heart surgery services and open heart surgery. Union hospitals also pay somewhat higher R.N. hourly wages, both in absolute terms and relative to non-union hospitals in the same market. Consistent with the findings in Hirsch and Schumacher (1998), we find a fairly small pay premium, well under 10% within local markets. Unionized hospitals apply slightly fewer R.N. hours and slightly more non-R.N. hours to the average case. We report five specifications for the regression analysis in Table 4.

Union results. The most interesting finding is a statistically significant association between the presence of an R.N. union and lower risk-adjusted heart-attack mortality in all specifications. In Specification 1 of Table 4, the bivariate regression with no controls for confounding factors, the presence of a nurse's union is associated with a 1.0 percentage point reduction in AMI mortality. The average heart-attack mortality in our sample is 14.6%; so the presence of a nurse's union is associated a 6.8% reduction in mortality. The adjusted R-squared for the regression is 0.02.[9]

Column (2), the estimation with full controls, is our preferred specification for estimating the effect of R.N. unions on heart-

[8] A referee notes that the union coverage rate for R.N.'s in California in Current Population Survey data is approximately 40%; including the Kaiser hospitals and weighting by R.N. hours, we find 42% coverage.

[9] Although state law exempts the Kaiser hospitals from the Annual Hospital Disclosure Report, they do report mortality data, and we collected the unionization data as well. The risk-adjusted mortality at Kaiser hospitals is 13.7%, with a standard deviation of 2.8 percentage points, compared to a state mean of 14.6% with a standard deviation of 3.1 percentage points. We estimated a variant of the bivariate regression specified in column (1) of Table 4 including the Kaiser hospitals in the sample, and the coefficient on R.N. unionization was slightly higher (–1.2) with a similar standard error (–0.31).

Table 3. Summary Statistics by 1993 Union Status.

	No R.N. Union N = 250		*R.N. Union N = 94*	
Variable	*Mean*	*Standard Deviation*	*Mean*	*Standard Deviation*
Dependent Variables				
Risk-Adjusted AMI Mortality Rate	14.96	3.14	13.95	2.63
Raw AMI Mortality Rate	15.13	3.45	14.53	3.04
Union Status				
R.N. Union New (1988–1993)	0%		9%	
R.N. Union Long (Before 1988)	0%		91%	
R.N. Union Coming (1994–1998)	5%		0%	
Non-Healthcare Union	3%		59%	
Other Hospital Characteristics				
Annual Discharges (100s)	58.5	39.9	72.9	52.7
R.N. Hours per Discharge	28.8	10.7	28.4	7.5
Non-R.N. Hours per Discharge	26.5	15.6	28.2	12.1
Staff M.D.'s per Discharge	0.043	0.117	0.113	0.246
Board Certified M.D.'s per Discharge	0.009	0.011	0.010	0.010
Hourly Wage of R.N.'s in AMI Units	$24.63	$3.04	$28.52	$4.28
Within-HSA R.N. Wage Premium	$0.21	$2.71	$1.59	$2.34
Hospital Size by Staffed Beds				
100 or Fewer Beds	11%		7%	
101–300 Beds	48%		38%	
More Than 300 Beds	41 %		55%	
Cardiac Treatment Services				
Coronary ICU	92%		97%	
Cardiac Catheterization	69%		76%	
Echocardiology	91%		93%	
Electrocardiology	96%		96%	
Heart Surgery Services	60%		62%	
Open Heart Surgery Services	56%		61%	
Mobile Cardiac Services	8%		8%	
Cardiology Clinic	22%		32%	
Cardiac Tech Index	1.63	0.70	1.74	0.62
Teaching Hospital	24%		39%	
Control or Ownership				
Not-for-Profit Hospital	73%		58%	
For-Profit Hospital	18%		13%	
Public Hospital	8%		28%	
Rural Hospital	9%		5%	
Location				
Non-Metropolitan	4%		3%	
Medium Metropolitan	19%		16%	
Large Metropolitan	77%		80%	

attack mortality. When we introduce the full set of controls for confounding factors in Specification 2, the adjusted R-squared increases to 0.10, and the presence of an R.N. union is associated with a 0.84 percentage point, or 5.8%, reduction in mortality. That is, the full set of independent variables reduces the estimated coefficient on R.N. union by approximately one-fifth, and the coefficient remains highly significant, with a standard error of 0.35 and a p-value of 0.02.

We introduce R.N. hours per related discharge in Specification 3, introduce the wage expressed as the within-HSA premium in Specification 4, and include both vari-

Table 4. Main Regression Results.

Description	[1] Bivariate	[2] Preferred	[3] R.N. Hours	[4] R.N. Wage	[5] R.N. Wage and Hours
R.N. Union 1993	–1.013*** (0.3492)	–0.836** (0.354)	–0.865** (0.3566)	–0.734** (0.3649)	–0.762** (0.3671)
R.N. Hours per Discharge			–0.015 (0.0223)		–0.016 (0.022)
Within-HSA R.N. Wage Premium				–0.071 (0.0622)	–0.073 (0.0622)
Annual Discharges (100s)		–0.009* (0.0049)	–0.009* (0.0049)	–0.009* (0.0049)	–0.009* (0.0049)
100 or Fewer Beds		–0.948 (0.6404)	–0.913 (0.6429)	–0.997 (0.6416)	–0.962 (0.6439)
More Than 300 Beds		–0.265 (0.4242)	–0.263 (0.4245)	–0.229 (0.4251)	–0.226 (0.4255)
Non-R.N. Hours per Discharge		0.0017 (0.011)	0.008 (0.0143)	0.0024 (0.011)	0.009 (0.0143)
Cardiac Tech Index		–0.918*** (0.3047)	–0.866*** (0.3142)	–0.903*** (0.3048)	–0.847*** (0.3144)
Teaching Hospital		0.2471 (0.4171)	0.3006 (0.4245)	0.2298 (0.4171)	0.285* (0.4245)
Staff M.D.'s per Discharge		–0.341 (1.1386)	–0.261 (1.1452)	–0.428 (1.1406)	–0.346 (1.146)
For-Profit Hospital		0.8513* (0.4526)	0.8857* (0.4557)	0.8278* (0.4529)	0.8636* (0.4558)
Public Hospital		0.2942 (0.4762)	0.2879 (0.4766)	0.2295 (0.4793)	0.2215 (0.4797)
Rural Hospital		–0.353 (0.7604)	–0.374 (0.7617)	–0.411 (0.7618)	–0.436 (0.7631)
Non-Metropolitan		0.3284 (1.0154)	0.3032 (1.0169)	0.4182 (1.018)	0.3934 (1.0192)
Medium Metropolitan		–0.37 (0.4238)	–0.42 (0.4303)	–0.334 (0.4247)	–0.386 (0.431)
Intercept	14.959*** (0.1944)	17.003*** (0.6047)	17.207*** (0.6729)	16.954*** (0.6059)	17.167*** (0.6734)
Adjusted R^2	0.0211	0.1038	0.1024	0.1047	0.1034

Notes: The dependent variable is the risk-adjusted hospital AMI mortality rate, 1991–93. Regressions are weighted by the number of AMI cases at each hospital. Standard errors in parentheses.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

ables in Specification 5; the logic of their inclusion is that they are likely to be affected by R.N. unionization. Neither variable has a statistically significant coefficient, although both terms have negative point estimates, suggesting that a higher wage and more R.N. hours are both associated with decreased AMI mortality. The adjusted R-squared does not exceed that of the preferred specification for any of these variants. At the point estimates for wage, the implication is that paying a ten-dollar-per-hour premium above the HSA average would reduce mortality by about 0.7 percentage point. At the point estimate for R.N. hours, the implication is that increasing R.N. hours by about 5 hours (16%) per AMI-related discharge would reduce mortality by about 0.1 percentage point.

Furthermore, neither the inclusion of

R.N. wage nor that of R.N. hours substantially affects the coefficient on R.N. union, which falls only slightly in magnitude, from –0.84 to –0.76, with their joint inclusion in Specification 5. The estimate of the union effect on mortality through changes in nurse quality and communication, over and above the effect of unionization through its effect on wages and hours, remains at roughly 0.8 percentage point, or 5.5%. Thus, the union has an effect beyond simple increases in wage and simple increases in R.N. hours.

Adversarial industrial relations. To test the hypothesis that adversarial industrial relations around a unionization process may cause higher mortality, even if the presence of a union is ultimately beneficial, we examine the difference in patient outcomes between hospitals that have recently undergone unionization and those with long-standing unions. Of hospitals with unionized R.N.'s in 1993, Table 3 shows that 9% had unionized since 1987.

Hospitals that organized between 1988 and 1993 actually have a positive, but small and statistically insignificant, association with mortality, which indicates that they are indistinguishable from non-unionized hospitals. Hospitals with long-standing R.N. unions, those organized by 1987, have lower mortality: the point estimate increases in magnitude from –0.84 for all R.N.-unionized hospitals to –0.96 for long-standing R.N.-unionized hospitals.[10]

These results suggest possible explanations that warrant further investigation if the basic union result proves to be causal. First, the very process of unionization may generate adversarial industrial relations that prevent the immediate realization of the care-quality improvements possibly associated with the presence of unions. Second, adversarial industrial relations may both adversely affect patient outcomes and induce unionization. Third, the benefits of unionization may only arrive after reorganization of the work process, that is, after the implementation of selection, communication, and retention policies and processes that we describe above. The current data do not permit discrimination among these theories, but the difference in care outcomes between hospitals that have recently unionized and those with long-standing unions is intriguing.

Other results. Few other variables are significant at conventional levels in the specifications reported in Table 4. Consistent with past studies, we find that hospitals with higher volume, that is, more cardiac-related discharges, have lower mortality rates. The presence of more cardiac services, as measured by the technology index, is significantly related to lower mortality rates ($p < 0.01$), although in the specification with all of the individual specialty services entered separately, no one service was significantly different from zero. A one standard-deviation increase in the index is associated with a reduction in mortality of about 0.9 percentage point. Larger hospitals (those with more than 300 beds) and smaller hospitals (those with 100 or fewer beds) had point estimates of lower mortality than did mid-range hospitals, although the result was not statistically significant. For-profit hospitals had a higher mortality rate than did public and nonprofit hospitals, and the result was significantly different from zero in some specifications. Additional M.D.'s per cardiac discharge was associated with reduced AMI mortality, although again the effect was not statistically significant.

The explanatory power of these regressions is, in general, low. In no case does the adjusted R-squared value exceed 0.11. The standard deviation of the mortality rate is approximately 3 percentage points (Table 3). Even assuming no correlation between the variables, the combined effect of R.N. unionization (accounting for –0.8 percentage point) and a one standard-deviation increase in the technology index (accounting for –0.9 percentage point) would decrease mortality by 1.7 percentage points, only slightly more than one-half of a standard deviation. Heart-attack mortality has high random variation even after we adjust

[10]Detailed results of these specifications are available from the authors.

extensively for patient risk and hospital characteristics.

Specification-Test and Selection Results

Patient characteristics and risk adjustment. We first examine the effect of risk adjustment on the results by using raw rather than adjusted mortality as the dependent variable. In column (1) of Table 5, we use the same preferred set of regressors as in Specification 2 of Table 4, but we use non-adjusted, rather than risk-adjusted, heart-attack mortality as the dependent variable. The estimated coefficient on R.N. union with the unadjusted mortality data is –0.15, and the estimate is not statistically significant. The difference means that unionized hospitals receive a caseload substantially more at risk of death than do non-unionized hospitals. Failure to risk-adjust would have meant incorrectly attributing the higher mortality at unionized hospitals to their unionized status rather than to their more difficult caseload.

In the specification with raw, non-adjusted mortality as the dependent variable, the coefficient on the cardiac technology index is –1.83, nearly twice as large as in the equivalent regression with adjusted mortality. The difference is statistically significant. This result suggests that hospitals with extensive cardiac technology treat patients less at-risk of death, and failure to risk-adjust the data would lead to an overestimate of the effectiveness of the technology.

The R-squared for this regression is 0.16, by far the highest in the study. The non-random character of patient-hospital assignment accounts for the difference in explanatory power between the adjusted and non-adjusted regressions.

Non-healthcare unions. As we indicated in our discussion of methods, we are worried about the interpretation of causality in our analysis. Our proposed remedy is to examine the "effect" on mortality of non-healthcare unions at the hospital. To the extent that non-healthcare unions are associated with reduced heart-attack mortality, we may be concerned that the relationship between unionization and mortality is spurious, caused by some third factor, for example, high-tech capital, which both reduces mortality and attracts unionization. As shown in Table 3, in hospitals with R.N. unions, the non-healthcare unionization rate is 59%. Of hospitals without R.N. unions, only 3% have unionized non-healthcare workers. Identification relies heavily on the hospitals with R.N. unions but without non-healthcare unions, a substantial sample.

The specification test is to include the presence of a non-healthcare union and to exclude the presence of an R.N. union, with all the other covariates from Specification 2, that is, the preferred specification. The hypothesis that R.N. unions indeed affect mortality suggests that the equation is misspecified, but the relatively high correlation of 0.70 between R.N. and non-healthcare unions will attribute some of the R.N. union effect to the non-healthcare union. The results are reported in column (2) of Table 5. When the non-healthcare union indicator is included instead of the R.N. union indicator, the coefficient is a statistically insignificant –0.5, with a standard error of 0.4. In column (3), where we exclude from the regression hospitals that have R.N. unions and compare mortality at hospitals with and without non-healthcare unions, we find that unionized hospitals have lower mortality, with a point estimate of –0.72, but the estimate is far from statistically significant, with a standard error of 1.23. On balance, these results suggest that R.N. unions, rather than hospital unionizability, are associated with the reduction in AMI mortality.

An alternative approach is an attempt to control for factors that may both cause union selection and affect patient outcomes. For this test, we include both the R.N. union indicator and the non-healthcare union indicator. The resulting coefficient on non-healthcare union, reported in column (4) of Table 5, is a positive but statistically insignificant 0.4, and the coefficient on R.N. union is statistically significant and actually increases slightly in

Table 5. Specification and Selection Tests.

Description	*[1]* *Raw Mortality*	*[2]* *Non-Health Union, Spec. Test*	*[3]* *Non-Health Union, Spec. Test (excl. R.N. unions)*	*[4]* *Non-Health Union, Selection*	*[5]* *Heckman Selection*	*[6]* *Future R.N. Union Spec. Test*
R.N. Union 1993	–0.186			–0.94**	–0.74	
	(0.3784)			(0.4494)	(0.6072)	
Non-Healthcare Union		–0.481	–0.715	0.1988		
		(0.4193)	(1.2312)	(0.5289)		
Inverse Mills Ratio					–0.082	
					(0.4204)	
R.N. Union Coming						–1.042
						(0.86)
Annual Discharges (100s)	–0.008	–0.009*	–0.013*	–0.009*	–0.009*	–0.014**
	(0.0052)	(0.0049)	(0.0069)	(0.0049)	(0.0049)	(0.0068)
100 or Fewer Beds	–0.851	–0.899	–0.541	–0.953	–0.943	–0.499
	(0.6846)	(0.6442)	(0.7741)	(0.6414)	(0.6418)	(0.7732)
More Than 300 Beds	0.1285	–0.327	0.4989	–0.244	–0.265	0.4932
	(0.4534)	(0.4287)	(0.5435)	(0.4283)	(0.4248)	(0.5402)
Non-R.N. Hours per Discharge	0.0018	0.0012	–0.006	0.0017	0.0015	–0.005
	(0.0118)	(0.0111)	(0.0128)	(0.011)	(0.0111)	(0.0128)
Cardiac Tech Index	–1.833***	–0.92***	–0.691*	–0.923***	–0.923***	–0.652*
	(0.3257)	(0.3069)	(0.3684)	(0.3053)	(0.3061)	(0.3693)
Teaching Hospital	0.2775	0.2169	–0.332	0.2466	0.241	–0.333
	(0.4458)	(0.4195)	(0.545)	(0.4176)	(0.4188)	(0.5437)
Staff M.D.'s per Discharge	–1.749	–0.325	–0.595	–0.416	–0.376	–0.688
	(1.2171)	(1.1627)	(1.9687)	(1.1576)	(1.1543)	(1.9658)
For-Profit Hospital	0.3532	0.8055*	1.2479**	0.8616*	0.8565*	1.1915**
	(0.4839)	(0.4556)	(0.5442)	(0.4541)	(0.4541)	(0.5453)
Public Hospital	–0.43	0.1497	0.819	0.2i1	0.2658	0.7845
	(0.509)	(0.4797)	(0.714)	(0.4808)	(0.4988)	(0.7128)
Rural Hospital	–0.098	–0.313	–0.189	–0.348	–0.354	–0.216
	(0.8129)	(0.7652)	(0.8549)	(0.7615)	(0.7616)	(0.8529)
Non-Metropolitan	0.0283	0.2978	–0.192	0.3211	0.3264	–0.175
	(1.0855)	(1.0221)	(1.1816)	(1.0169)	(1.017)	(1.1777)
Medium Metropolitan	–0.867*	–0.379	–0.879*	–0.364	–0.366	–0.907*
	(0.453)	(0.4268)	(0.5169)	(0.4247)	(0.4248)	(0.5127)
Intercept	18.71***	16.923***	16.876***	16.997***	16.997***	16.873***
	(0.6464)	(0.6077)	(0.7098)	(0.6057)	(0.6065)	(0.7079)
Adjusted R^2	0.162	0.0923	0.079	0.1015	0.1012	0.0834

Notes: The dependent variable in column (1) is the raw hospital AMI mortality rate, 1991–93. The dependent variable in columns (2)–(6) is the risk-adjusted hospital AMI mortality rate, 1991–93. Regressions are weighted by the number of AMI cases at each hospital. Standard errors in parentheses.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

magnitude to –0.89. Again, the specification test provides evidence for the hypothesis that R.N. unions indeed have a causal association with reduced AMI mortality. The results of the specification tests are essentially unchanged when we repeat the exercise with the inclusion of an additional indicator for unionized licensed vocational nurses (L.V.N.'s) and nurses' aides.

Our next test is to apply the Heckman two-step selection correction to control for nonrandom selection of hospital union status. Although no exclusion restriction is computationally necessary to estimate the Heckman method, we propose the presence of a non-healthcare union as an appropriate excluded variable.[11] In the first stage, we estimate a probit for the presence of an R.N. union at the hospital, including all of the variables in our preferred specification as well as the presence of a non-healthcare union. The only statistically significant variables in the first stage are the presence of a non-healthcare union, with a large coefficient and a p-value below 0.0001, and public ownership of the hospital, with a p-value of 0.04.[12] The low levels of statistical significance of other variables in the estimation suggest that union and non-union hospitals do not differ substantially in the observables. In the second-stage OLS estimation, we include the inverse Mill's ratio computed from the probit results to capture features of the hospital that are correlated both with propensity to unionize and potentially with patient outcomes. The results are reported in column (5) of Table 5. The coefficient on the inverse Mill's ratio is small and far from statistical significance, suggesting a small role for nonrandom hospital-union selection correlated with patient outcomes. The coefficient on R.N. union remains negative and stable at –0.74, but it is no longer statistically significant, having a standard error of 0.61. Increases in standard error are endemic in two-stage methods such as the Heckman selection correction, and thus the result adds only weak evidence for the relationship between the presence of an R.N. union and reduced heart-attack mortality.

Future unionizers. An alternative approach to the specification tests and selection corrections based on non-healthcare unions is to examine hospitals in which the R.N.'s were not unionized during the study period but subsequently did unionize. As Table 3 shows, 12 hospitals, or about 5% of hospitals that did not have R.N. unions in 1993, unionized over the 1993–98 period. We use the same specification test approach, that is, including future unionizers instead of the currently unionized, and excluding the currently unionized from the analysis.

In most dimensions, the average characteristics of the future unionizers are between those of the currently unionized hospitals and the never-unionized hospitals. Future unionizers do broadly resemble the currently unionized hospitals. Among the characteristics examined in Table 3, we can reject at the 5% level the hypothesis that future unionizers and currently unionized hospitals have equal mean characteristics in 1993 only for three variables: the presence of non-healthcare unions (currently unionized hospitals have more), the absolute wage rate (currently unionized hospitals pay more), and ownership and control (future unionizers are more likely to be not-for-profit and less likely to be public).[13]

When future unionizers are included instead of the currently unionized, the estimated effect of –1.0 percentage point, reported in column (6) of Table 5, is negative and of the same magnitude as that on currently unionized, but the coefficient falls far short of statistical significance at conventional levels. The standard error is 0.86 and the p-value is 0.23. The high and negative, albeit statistically insignificant, coefficient on future unionizers leaves concern that union-hospital selection contaminates the causal interpretation of the results. Because the number of future unionizers is small, the test has low power

[11]We have a rare instance of an appropriate exclusion restriction for the first-stage probit of the Heckman selection correction, which is not an appropriate instrument for 2SLS. The presence of non-healthcare unions is inappropriate for 2SLS; we argue that the presence of a non-healthcare union is correlated with the unobservable features of a hospital that make it both high-quality with respect to patient outcomes and a likely candidate for unionization.

[12]Other results of the probit are available from the authors.

[13]More details of the comparison among currently unionized, never-unionized, and yet-to-unionize hospitals are available from the authors.

in distinguishing between the alternative hypotheses.

One possible interpretation is that future unionizers vary in their practices such that some of them, even before unionization, achieve the reduced mortality rates of which they are capable. The high, negative, and statistically significant coefficient on the currently unionized suggests that R.N. unions help to implement the improved practices.

Regional variation in union status and outcomes. It is possible that union status is highly correlated with features of the geographic area that influence patient outcomes, regardless of whether the hospital has an R.N. union. For example, union density—the number of union hospitals divided by total hospitals in the area—is much higher in the San Francisco Bay Area (from 65% to 95%) than in other parts of California (from 10% to 60%).

To test the robustness of the model, we excluded from our sample the 33 hospitals in the San Francisco Bay Area.[14] The coefficient on R.N. union remains stable and significant at –0.86 (0.40). We tested an additional specification with the full sample and an indicator variable for the San Francisco Bay Area; the coefficient on R.N. union again remains stable and nearly statistically significant at –0.73 (0.38). We also estimated the model with fixed effects for each of California's 14 Health Service Areas to examine the effect of within-HSA variation in union status on within-HSA differences in the outcome. This allows for comparing union and non-union hospitals within each HSA. Using the HSA indicator reduces the variation in the explanatory variables, in particular in R.N.-union status, and so the effects may become harder to observe. In the HSA fixed-effect model, the coefficient remains negative at –0.45, but it ceases to be statistically significant (p-value of 0.43) and the effect is somewhat diminished. We note that the adjusted R-squared for the HSA fixed-effect regression does not increase relative to the model without HSA fixed effects; the addition of the fixed effects fails to increase the explained variation.

[14]The San Francisco Bay Area includes Marin, San Francisco, Contra Costa, Alameda, and San Mateo Counties. We thank an anonymous referee for suggesting this approach to reconcile the strong results without area effects and the null result in the HSA fixed-effect specification.

Do R.N. Unions Cause Lower Heart-Attack Mortality?

The key issue for the study is causation. Findings from the study do not rule out the nonrandom selection hypothesis regarding the presence of unions, and this limits the causal interpretation of these results. Our analyses, which examine the association between unions and patient outcomes, may be confounded by several factors, all of which involve nonrandom selection of the union status of hospitals. The direction of bias might go in either direction. For example, unions may be easier to establish in high-quality hospitals, perhaps because these hospitals have the capacity to pay premium wages or otherwise to offer more attractive working conditions. In this case, unions do not cause good outcomes but rather select hospitals with good outcomes. On the other hand, the reverse may be true: unions may thrive in hospitals that have poor outcomes, possibly because morale is low. In the latter case, a correlative analysis would associate unions with bad outcomes, although again the relationship is not causal. In either case, the observed correlation is a biased estimate of a causal relationship, and we have no *a priori* means of determining the direction of bias.

Although the use of instrumental variables techniques would be an attractive econometric solution to this problem, we could not identify suitable instrumental variables. Therefore, we have relied on specification tests to identify spurious relationships and the Heckman method to correct for nonrandom union selection. Because of the small number of hospitals in the proposed counterfactual group, no single test is of high power. Yet the result proves robust with respect to both the non-healthcare union approach and the future-unionizers approach. Our results demonstrate that there is a positive relationship between patient outcomes and R.N. unions. The tests are independent and support the basic result: the presence of an R.N. union is associated with a reduction in mortality of about 0.8 percentage point, or 5.5%, even after we control for hospital characteristics and make our best effort to control for union-hospital selection.

We finally consider the possibility that existing employee discontent will both affect patient outcomes (American Association of Nurse Executives 1994; Sherer 1994; Aiken et al. 1994; Wilson et al. 1990) and spur unionization. Although the argument has merit, we were unable to obtain data specifically related to union activity, so we were unable to address this problem. This problem would tend to bias the results toward a finding that unionization is bad for patient outcomes, which suggests that our result in the opposite direction may be underestimated. Our results on long-standing versus recent R.N. unions suggest that the hypothesis of already existing adversarial relations may apply.

The mechanism by which unions affect patient outcomes demands further explanation, and the type of data employed in this study cannot shed light on the specifics of the work process. However, we do observe that the relationship persists even when we control for the wage and hour changes that unions may cause. We need to explore further the relationship between unions and patient outcomes, including a richer analysis of causality in the relationship and more detailed examination of the mechanisms at work. Our findings indicate that some feature of the work environment in union hospitals, beyond wages and number of hours, predicts better outcomes. We have yet to examine the type of nursing care delivery system, skill mix and tenure among R.N.'s, and specific institutions that facilitate communication. Perhaps having an R.N. union promotes stability in staff, autonomy, collaboration with M.D.'s, and control of practice decisions that have a positive influence on patient outcomes. Accounting for union status is important when examining organizational factors that influence patient outcomes.

Our findings have implications for the operation of hospitals, organizations whose governance and goals are of a mixed public-private character. The implications therefore lie somewhere between recommendations for public policy and suggestions for private, human-resource management. Hospitals with non-unionized R.N.'s may consider facilitation of, or at least employer neutrality in, R.N. efforts to unionize. Hospitals both with and without unionized R.N.'s may want to identify which of the workplace practices in unionized hospitals affect patient outcomes and encourage those practices that improve patient outcomes. Recall that the net favorable R.N.-union effect we identified is potentially an amalgam of outcome-improving practices (retention, quality-selection, and communication) and outcome-harming practices (adversarial workplace relations and personnel budget-squeeze). The net favorable R.N.-union effect that we measure may not represent the theoretical best-practice. Further research may help determine which clinical practices by nurses save lives and which mechanisms in unionized hospitals support such life-saving practices.

REFERENCES

Addison, John T., and Barry T. Hirsch. 1989. "Union Effects on Productivity, Profits, and Growth: Has the Long Run Arrived?" *Journal of Labor Economics,* Vol. 7, No. 1 (January), pp. 72–105.

Aiken, Linda. 1989. "The Hospital Nursing Shortage. A Paradox of Increasing Supply and Increasing Vacancy Rates." *Western Journal of Medicine,* Vol. 151, pp. 87–92.

Aiken, Linda, and Claire Fagin. 1997. "Evaluating the Consequences of Hospital Restructuring." *Medical Care,* Vol. 35, No 10 Suppl. (October), pp. OS1–4.

Aiken, Linda, Douglas M. Sloane, Eileen T. Lake, Julie Sochalski, and A. Weber. 1999. "Organization and Outcomes of Inpatient AIDS Care." *Medical Care,* Vol. 37, No. 8 (August), pp. 760–62.

Aiken, Linda, Herbert L. Smith, and Eileen T. Lake. 1994. "Lower Medicare Mortality among a Set of Hospitals Known for Good Nursing Care." *Medical Care,* Vol. 32, No. 8 (August), pp. 771–87.

American Organization of Nurse Executives. 1994. "When Work Redesign Prompts Unionization Activity." *Nursing Management,* Vol. 24, No. 9 (September), pp. 36, 38.

Arrow, Kenneth. 1963. "Uncertainty and the Welfare Economics of Medical Care." *American Economic Review,* Vol. 53, No. 5 (December), pp. 941–73.

Baskin, Susan T., and Stephen Shortell. 1995. "Total Quality Management: Needed Research on the Structural and Cultural Dimensions of Quality Improvement in Health Care Organizations." *Journal of Health Administration Education,* Vol. 13, No. 1 (Winter), pp. 143–54.

Booth, Allison L. 1995. *The Economics of the Trade Union.* Cambridge: Cambridge University Press. Section 7.4, p. 196.

Breda, K. L. 1997. "Professional Nurses in Unions: Working Together Pays Off." *Journal of Professional Nursing,* Vol. 13, No. 2 (March–April), pp. 99–109

California Nurse. 1994. "CNA Considers Action on State Single Payer Initiative." *California Nurse,* Vol. 90, No. 2 (February), pp. 8–9, 11.

Currie, Janet, Mehdi Farsi, and W. Bentley MacLeod. 2002. "Cut to the Bone? Hospital Takeovers and Nurse Employment Contracts." National Bureau of Economic Research Working Paper 9428.

Ferlie, Ewan, and Stephen Shortell. 2001. "Improving the Quality of Health Care in the United Kingdom and the United States: A Framework for Change." *Milbank Quarterly,* Vol. 79, No. 2, pp. 281–315.

Flarey, Dominick, S. Yoder, and Mark C. Barabas. 1992. "Collaboration in Labor Relations: A Model for Success." *Journal of Nursing Administration.*

Flood, Ann Barry, W. Richard Scott, and Wayne Ewy. 1984a. "Does Practice Make Perfect? Part I: The Relation between Hospital Volume and Outcomes for Selected Diagnostic Categories." *Medical Care,* Vol. 22, No. 2 (February), pp. 98–114.

____. 1984b. "Does Practice Make Perfect? Part II: The Relation between Volume and Outcomes and Other Hospital Characteristics." *Medical Care,* Vol. 22, No. 2 (February), pp. 115–25.

Freeman, Richard B., and James L. Medoff. 1984. *What Do Unions Do?* New York: Basic Books.

Gordon, Suzanne. 1998. *Life Support: Three Nurses on the Front Lines.* Boston: Back Bay Books.

Gordon, Suzanne, Patricia Benner, and Nel Noddings, eds. 1996. *Caregiving: Readings in Knowledge, Practice, Ethics, and Politics.* Philadelphia: University of Pennsylvania Press.

Grumbach, Kevin, Geoffrey M. Anderson, Harold S. Luft, Leslie L. Roos, and Robert Brook. 1995. "Regionalization of Cardiac Surgery in the United States and Canada. Geographic Access, Choice, and Outcomes." *Journal of the American Medical Association,* Vol. 274, No. 16 (October), pp. 1282–88.

Grumbach, Kevin, Michael Ash, Jean Ann Seago, Janet Coffman, and Joanne Spetz. 2001. "Measuring Shortages of Hospital Nurses: How Do You Know a Hospital with a Nursing Shortage When You See One?" *Medical Care Research and Review,* Vol. 58, No. 4 (December), pp. 387–403.

Harper, Earl, Jaideep Motwani, and Ram Subramanian. 1994. "Collective Bargaining Practices in the Health Care Industry: An Empirical Analysis." *Health Care Supervisor,* Vol. 13, No. 1 (September), pp. 31–42.

Harris, Susan. 1996. "The Union Movement: Legislative and Regulatory Issues in California." *Seminars in Nurse Management,* Vol. 4, No. 1 (March), pp. 78–82.

Hartz, Arthur J., Henry Krakauer, and Evelyn M. Kuhn. 1989. "Hospital Characteristics and Mortality Rates." *New England Journal of Medicine,* Vol. 321, No. 25 (December), pp. 1720–25.

Hirsch, Barry T., and David A. Macpherson. 2003. "Union Membership and Coverage Database from the Current Population Survey: Note." *Industrial and Labor Relations Review,* Vol. 56, No. 2 (January), pp. 349–54.

Hirsch, Barry T., and Edward J. Schumacher. 1998. "Union Wages, Rents, and Skills in Health Care Labor Markets." *Journal of Labor Research,* Vol. 19, No. 1 (Winter), pp. 125–47.

____. 1995. "Monopsony Power and Relative Wages in the Labor Market for Nurses." *Journal of Health Economics,* Vol. 14, No. 4 (October), pp. 443–76.

Howes, Carollee. 2001. "The Impact of Unionization and the Living Wage Ordinance on the IHSS Homecare Workers in San Francisco County." California Homecare Research Working Group.

Hoxby, Caroline. 1996. "How Teachers' Unions Affect Education Production." *Quarterly Journal of Economics,* Vol. 111, No. 3 (August), pp. 671–718.

Kovner, Christine, and Peter J. Gergen. 1998. "Nurse Staffing Levels and Adverse Events Following Surgery in U.S. Hospitals." *Image: The Journal of Nursing Scholarship,* Vol. 30, No. 4 (Fourth Quarter), pp. 315–21.

Luft, Harold S., and Patrick S. Romano. 1993. "Chance, Continuity, and Change in Hospital Mortality Rates. Coronary Artery Bypass Graft Patients in California Hospitals, 1983 to 1989." *Journal of the American Medical Association,* Vol. 270, No. 3 (July 21), pp. 331–37.

____. 1997. *Volume 3: Detailed Statistical Results, Technical Report.* Sacramento: California Office of Statewide Health Planning and Development.

Manheim, Larry M., Joe Feinglass, Stephen Shortell, and Edward Hughes. 1992. "Regional Variation in Medicare Hospital Mortality." *Inquiry,* Vol. 29, No. 1 (Spring), pp. 55–66.

McClellan, Mary, R. Henson, and June Schmele. 1994. "Introducing New Technology: Confusion or Order?" *Nursing Management,* Vol. 25, No. 7 (July), pp. 38–41.

McClellan, Mark, Barbara J. McNeil, and Joseph P. Newhouse. 1994. "Does More Intensive Treatment of Acute Myocardial Infarction in the Elderly Reduce Mortality? Analysis Using Instrumental Variables." *Journal of the American Medical Association,* Vol. 272, No. 11 (September), pp. 859–66.

Mitchell, Pamela H., and Stephen Shortell. 1997. "Adverse Outcomes and Variations in Organization of Care Delivery." *Medical Care,* Vol. 35, No. 11 Suppl. (November), pp. NS19–32.

Moore, J. D., Jr. 1995. "Labor Looks to Lead Counter-Revolution" [published erratum]. *Modern Healthcare,* Vol. 25, No. 44 (October), pp. 46–48, 68.

Needleman, Jack, Peter Buerhaus, Soeren Mattke, Maureen Stewart, and Katya Zelevinsky. 2002. "Nurse-Staffing Levels and the Quality of Care in Hospitals." *New England Journal of Medicine,* Vol. 346, No. 22 (May 30), pp. 1715–22.

OSHPD 1997. *Report on Heart Attack, 1991–93. Volume 4: Hospital Comment Letters, Technical Report.* Sacramento: California Office of Statewide Health Planning and Development.

Phillips, Kathryn A., Harold S. Luft, and James Ritchie. 1995. "The Association of Hospital Volumes of Percutaneous Transluminal Coronary Angioplasty with Adverse Outcomes, Length of Stay, and Charges in California." *Medical Care,* Vol. 33, No. 5 (May), pp. 502–14.

Register, Charles A. 1988. "Wages, Productivity, and Costs in Union and Nonunion Hospitals." *Journal of Labor Research,* Vol. 9, No. 4 (Fall), pp. 325–45.

Romano, Patrick S, Harold S. Luft, Julie Rainwater, and Aandra Zach. 1997. *Report on Heart Attack, 1991–93. Volume 2: Technical Guide, Technical Report.* Sacramento: California Office of Statewide Health Planning and Development.

Seago, Jean A., and Michael Ash. 2002. "Registered Nurse Unions and Patient Outcomes." *Journal of Nursing Administration,* Vol. 32, No. 3 (March), pp. 143–51.

Sherer, J. L. 1994. "Union Uprising: California Nurses React Aggressively to Work Redesign." *Hospital Health Network,* Vol. 68, No. 24 (December), pp. 36, 38

Shortell, Stephen, and Kathleen E. Hull. 1996. "The New Organization of the Health Care Delivery System." *Baxter Health Policy Review,* Vol. 2, pp. 101–48.

Shortell, Stephen, Robert H. Jones, Alfred W. Rademaker, et al. 2000. "Assessing the Impact of Total Quality Management and Organizational Culture on Multiple Outcomes of Care for Coronary Artery Bypass Graft Surgery Patients." *Medical Care,* Vol. 38, No. 2 (February), pp. 207–17.

Shortell, Stephen, James Zazzali, Lawton R. Burns, et al. 2001. "Implementing Evidence-Based Medicine: The Role of Market Pressures, Compensation Incentives, and Culture in Physician Organizations." *Medical Care,* Vol. 39, No. 7 Suppl. 1 (July), pp. I62–78.

Shortell, Stephen, Jack E. Zimmerman, Denise M. Rousseau, et al. 1994. "The Performance of Intensive Care Units: Does Good Management Make a Difference?" *Medical Care,* Vol. 32, No. 5 (May), pp. 508–25.

Showstack, Jonathan, Kenneth E. Rosenfeld, Deborah W. Garnick, Harold S. Luft, Ralph Schaffarzick, and J. Fowles. 1987. "Association of Volume with Outcome of Coronary Artery Bypass Graft Surgery: Scheduled vs. Nonscheduled Operations." *Journal of the American Medical Association,* Vol. 257, No. 6 (February 13), pp. 785–89.

____. 1987. "Association of Volume with Outcome of Coronary Artery Bypass Graft Surgery." *Journal of the American Medical Association,* Vol. 257, No. 18 (May 8), pp. 24–38.

Spetz, Joanne. 1996. "Nursing Wage Premiums in Large Hospitals: What Explains the Size-Wage Effect?" *AHSR FHSR Annual Meeting Abstracts,* Vol. 13, pp. 100–101.

Wilson, C., C. Hamilton, and E. Murphy. 1990. "Union Dynamics in Nursing." *Journal of Nursing Administration,* Vol. 20, No. 2 (February), pp. 35–39.

Zach, Andra, Patrick S. Romano, and Harold S. Luft. 1997. *Report on Heart Attack, 1991–93. Volume 1: User's Guide, Technical Report.* Sacramento: California Office of Statewide Health Planning and Development.

Zimmerman, Jack E., William A. Knaus, Elizabeth A. Draper, et al. 1991. "Organizational Correlates with ICU Efficiency and Effectiveness." *Critical Care Medicine,* Vol. 19, No. 2, p. S35.

Zimmerman, Jack E., Denise M. Rousseau, Joanne Duffy, et al. 1994. "Intensive Care at Two Teaching Hospitals: An Organizational Case Study." *American Journal of Critical Care,* Vol. 3, No. 2 (March), pp. 129–38.