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ABSTRACT

Concerns over the impacts of hospital strikes on patient welfare led to substantial delay in the ability of hospitals to unionize. Once allowed, hospitals unionized rapidly and now represent one of the largest union sectors of the U.S. economy. Were the original fears of harmful hospital strikes realized as a result? In this paper we analyze the effects of nurses' strikes in hospitals on patient outcomes. We utilize a unique dataset collected on nurses' strikes over the 1984 to 2004 period in New York State, and match these strikes to a restricted use hospital discharge database which provides information on treatment intensity, patient mortality and hospital readmission. Controlling for hospital specific heterogeneity, patient demographics and disease severity, the results show that nurses' strikes increase in-hospital mortality by 19.4% and 30-day readmission by 6.5% for patients admitted during a strike, with little change in patient demographics, disease severity or treatment intensity. This study provides some of the first analytical evidence on the effects of health care strikes on patients, and suggests that hospitals functioning during nurses' strikes are doing so at a lower quality of patient care.

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Hospitals are one of the most important employers in the United States. Thirty-five percent of U.S. health care workers, and 3.25% of all U.S. workers, work in hospitals.[1] Due to the importance of hospitals in providing health care to our nation, and fears that work stoppages could place patient health in jeopardy, hospitals were excluded from collective bargaining laws for almost three decades after other sectors were allowed to unionize. Once allowed to do so in 1974, however, hospitals quickly became one of the most important sources of union jobs in the U.S. Over fifteen percent of hospital employees are members of a union[2], representing six percent of all union employees in the U.S. While unionization has been declining in its traditional industrial home, it is growing rapidly in the hospital sector, with the number of unionized hospital workers rising from 679,000 in 1990 to nearly 1 million in 2008.[3] Despite the rapid unionization of the hospital sector, we know little about the original government concern that led to the long delay in permitting unionization: do strikes jeopardize patient health?

In this paper, we carefully examine the effects of nursing strikes at hospitals on patient care and health outcomes. Nurses are a crucial part of the hospital production function and are, as one hospital CEO said, "the heart and soul of the hospital."[4] They serve as the surveillance system of hospitals for detection and intervention when patients deteriorate, and are viewed by many patients as more important to their total recuperation process than their own attending physicians (Kruger and Metzger, 2002). Thus, one might presume that strikes by nurses would be harmful to patients' health. Yet, at the same time, a large literature in health economics documents substantial overtreatment in hospitals in the U.S.; for example Fisher et al. (2004) find no association between increased treatment intensity across medical centers and improved

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[1] http://www.bls.gov/oco/cg/cgs035.htm, http://www.bls.gov/news.release/ecopro.t04.htm
[2] This figure represents the number of hospital employees that are union members. The percentage of hospital employees covered by a collective bargaining agreement is 17% (Source: Unionstats.com).
[3] Source: Unionstats.com
[4] Draper (2008)

long-term survival. From this, one might infer that reduced treatment intensity due to nursing strikes might be innocuous. Thus, ex-ante, the impact of nursing strikes on outcomes is ambiguous.

To address this question, we turn to one of the U.S. states with the most hospital strikes in recent decades, New York State. A key advantage of this state for our analysis is that information on strikes can be matched to hospital discharge records which provide information on both treatment intensity and two key measures of outcomes, patient mortality and hospital readmission. We have gathered data on every hospital strike over the 1984 to 2004 period in New York State. We carefully match each striking hospital over this period with a set of control hospitals in their area, and examine the evolution of outcomes before, during, and after the strike in the striking versus control hospitals.

Our results are striking: there is a meaningful increase in both hospital mortality and hospital readmission among patients admitted during a hospital strike. Our central estimates suggest that the rate of hospital mortality is 19.4% higher, and rates of hospital readmission are 6.5% higher, among those admitted during a strike than among patients in nearby hospitals at the same time. We show that this deterioration in outcomes occurs only for those patients admitted during the strike, and not for those admitted before or after to the same hospitals. And we find that these changes are not associated with any meaningful change in the composition of patients admitted during the strike or the treatment intensity for patients admitted during these strikes.

We also find evidence of a more severe impact of these strikes on patients whose conditions require more intensive nursing inputs, and that outcomes are no better for patients admitted to striking hospitals who employ replacement workers. Overall, our findings suggest that strikes lead to lower quality of medical care in hospitals.

Our paper proceeds as follows. Part I provides background on hospital unionization and on the literature on strikes and firm outcomes. Part II discusses our data on both strikes and patient outcomes. Part III discusses our empirical strategy and issues. Part IV presents the results on mortality and readmission, while Part V presents results on utilization measures. Part VI examines the heterogeneity in these strike effects. Part VII concludes.

Part I: Background

Hospital Unionization

Organized labor in the hospital industry is a relatively recent phenomenon when compared with the industrial sector. While initially covered under the pro-union Wagner Act of 1935, collective bargaining in hospitals was limited due to the passage of the National Labor Relations Act (NLRA) of 1947. This act, which outlined unfair labor practices on the part of unions, also excluded both government and nonprofit hospitals from the right to unionize.

This restriction was based on the Congress's belief that unionization could interfere with the delivery of essential health and charitable services.[5] One of the main arguments justifying the exclusion of nonprofit hospitals was the contention that allowing nonprofit hospital coverage would "open the way for strikes, picketing, and violence which could impede the delivery of health care." (Zacur 1983, p.10) Hospital administrators argued for the importance of maintaining this exclusion, emphasizing that hospitals "absolutely cannot afford any interruptions in service caused by work stoppages. Healthcare facilities are not like assembly lines." (Fink 1989, p.167)

After lobbying efforts by hospital-employee organizations, in 1974, President Nixon

[5] While this restricted the rights of most employees in the sector from unionizing, eight states passed legislation during this period that granted collective bargaining rights to not-for-profit hospitals. The eight states were Connecticut, Massachusetts, Michigan, Minnesota, Montana, New York, Oregon and Pennsylvania.

signed Public Law 93-360, reversing the 27 year exclusion. This law subjected all nongovernmental health care facilities to federal labor law, as governed by the NLRA. While this law allowed for union organization of health care facilities, the perceived vulnerability of health care institutions to strikes prompted Congress to add amendments to this legislation applying exclusively to nongovernmental health care institutions. Twomey (1977) notes that these amendments included longer government notification periods than would be required of a non-health care facility to the Federal Mediation and Conciliation Service (FMCS) in the event of a contract renewal (90 days versus the usual 60 days), or strike (10-day notice period versus no notice).

Huszczo and Fried (1988) show that the percent of hospitals with collective bargaining agreements increased from 3% in 1961 to 23% in 1976, and conjecture that PL 93-360 played a significant role in this increase. Furthermore, in recent years, the health care sector has been the most active sector of the economy for new organizing.[6] Table 1 shows strike activity by industry for the years 1984-2004 as reported by the FMCS. The health care industry has experienced significant strike activity since 1984 with a greater number of strikes than all industries aside from manufacturing, construction and retail.[7]

Strikes and Firm Performance

A substantial economics and industrial relations literature exists analyzing the occurrence, timing, size, duration, and economic impact of strikes. Kaufman (1992) provides an excellent survey of this literature and categorizes these studies into 3 main areas: theoretical

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[6] See NLRB, Sixty-Eighth Annual Report Of The National Labor Relations Board For The Fiscal Year Ended September 30, 2003, At Table 16 (2004).
[7] The FMCS data do not differentiate between types of health care facilities, such as hospitals and nursing homes.

studies identifying the root causes of strikes, empirical studies analyzing variation in strike activity, and empirical studies measuring the impact of strikes on firms and industry.

Our study is most closely related to the literature on the effects of strikes on firm and industry performance. This is a growing literature which focuses mostly on the effects of strikes in manufacturing industries. The outcomes of interest include measures such as firm output, profitability, and capital market reaction to strikes. Multi-industry studies such as Neumann (1980), Neumann and Reder (1984), Becker and Olson (1986), and Kramer and Vasconcellos (1996) find that strikes lead to a 2-4% decline in firm market value. McHugh (1991) examines the productivity of struck firms in nine manufacturing industries and finds a negative direct impact of strikes on average labor productivity. Similar findings are echoed in studies of specific industries such as the airline industry, where DeFusco and Fuess (1991) find negative stock market returns of 2.6-5.3% during strikes, and Kleiner, Leonard & Pilarski (2002) find that productivity fell greatly at commercial aircraft manufacturing plants during strikes; these effects did not persist in the long-run, however, with their plant returning to pre-strike levels of productivity within one to four months. Schmidt and Berri's (2004) study of professional sports strikes indicates that strike costs are significant during the strike period, but are limited to the strike period, with almost immediate return to pre-strike levels of consumer demand for sporting events.

Two recent studies have examined the effect of strikes and labor relations on the quality of production. Krueger and Mas (2004) examined a long strike which involved the hiring of replacement workers at a tire plant between 1994 and 1996. They found that tires produced during these years were ten-times more likely to be defective, with particularly pronounced increases in defective units coinciding with periods when replacement workers worked together with returning strikers. Mas (2008) found that workmanship for construction equipment produced at factories that experienced contract disputes was significantly worse relative to equipment produced at factories without labor unrest, as measured by the resale value of the equipment. His estimates indicate that equipment produced in facilities undergoing labor disputes were discounted in the resale market by approximately 5%.

Strikes and Outcomes in the Health Care Sector

The effects of labor unrest in the health care industry may be particularly pronounced, given its labor-intensive production process, and the potentially serious consequences of substandard health care production. Health care production is particularly labor intensive, with labor's share of production accounting for nearly 60% of hospital costs. [8] Nurses in particular, constitute the largest group of workers in a hospital and have the biggest impact on a patient's experience in the hospital. Hospital administrators acknowledge that "nurses are the safety net. They are the folks that are right there, real time, catching medication errors, catching patient falls, recognizing when a patient needs something [and] avoiding failure to rescue." [9] Consequently, work stoppages involving nursing personnel have the potential to significantly disrupt hospital operations, with potentially serious consequences for patients. Furthermore, the complex nature of health care delivery necessitates the close coordination of workers who exhibit a great degree of interdependence (Cebul et al., 2008). Healthcare institutions may thus be particularly susceptible to labor unrest which disrupt these complex processes

In addition, a change in the intensity and quality of nursing inputs brought about due to

[8] American Hospital Association Trendwatch Report, 2009 [Online]. Available at: http://www.aha.org/aha/research-and-trends/chartbook/ch6.html [Accessed 9 March 9, 2010].
[9] (Draper 2008). Failure to rescue is a situation where caregivers fail to notice or respond when a patient is dying of preventable complications in a hospital.

strikes has the potential to adversely affect the patient outcomes. A number of studies have suggested that a decrease in the nurse-to-patient ratio is associated with increases in mortality and other adverse inpatient events (e.g. Aiken et al., 2002; Needleman et al., 2002). Moreover, even if staffing ratios are maintained during a strike through the use of replacement workers, the quality and familiarity of these replacement workers with hospital processes may affect the care delivered to patients during strikes. For example, the results in Aiken et al. (2003) suggest that higher quality workers (as measured by education level) are associated with lower mortality rates, while Phibbs et al. (2009) document increases in length of stay for hospitals employing temporary contract workers.

At the same time, a large body of research suggests that patients may be over-treated in the hospital. As a result, the reductions in care that result from strikes may not be particularly harmful on the margin. Fisher et al. (2003) show that in regions with high rates of inpatient care utilization, quality of care, functional status and patient satisfaction are no better than in low utilization regions. Baicker and Chandra (2004) control for within-state variation and find that states with higher Medicare spending per beneficiary have lower-quality care. Fisher et al. (2004) extend this analysis to academic hospitals and find no association between increased treatment intensity across medical centers and improved long-term survival for three of their measured outcomes, while finding a small increase in the risk of death as intensity increased for two other conditions analyzed.

Despite the increased role of organized labor in the health care industry, few studies have examined the role of labor unrest on health care production, and the results of these studies offer no clear conclusions as to the effect of these strikes on patients. Early work on health care strikes by James (1976) and Pantell and Irwin (1979) examine the effects of physician strikes on patient care. James (1976) investigates the impact of a physician work slowdown tied to increased malpractice rates in Los Angeles. He finds that causes of death shifted over the course of the slowdown, with decreases in deaths from elective surgery and increases in deaths associated with emergency room transfers. On the other hand, Pantell and Irwin (1979) find no significant effects on appendectomy outcomes during a one-month anesthesiologist strike in San Francisco.

In the only study of the impact of a nurses strike on patient care, Mustard et al. (1995) report a 15% decrease in the caesarian birth rate, as well as an increase in the rate of adverse newborn outcomes during a month-long Ontario nurses strike. They conjecture that the result "is most plausibly attributed to disruption in the normal standards of care rather than to the change in the rate of operative management." Finally, Salazar et al. (2001) examine the effect of an emergency room residents strike at a Spanish hospital during which staff physicians filled in for the striking residents. They find decreases in the number of tests ordered, as well as a decrease in patient length of stay compared with the same hospital during a non-striking period, with no significant changes in mortality or readmission rates.

Part II: Data

Strike Data

As a condition of the passage of PL 93-360, health care unions are required to submit written notice specifying the exact date and time of striking or picketing activity to both the potentially struck health care institution and the Federal Mediation and Conciliation Service (FMCS), 10-days prior to any work stoppage. The FMCS issues a monthly report showing work stoppages for all industries, and maintains an electronic database of these work stoppages for all industries dating back to 1984. This database contains information on the employer struck, employer location and industry, the union involved, the beginning and end dates of strikes, as well as the size of the bargaining unit struck. In some cases, the names of the types of workers that struck (e.g. clerical workers, technicians etc.) are also included. Our strike data were obtained from the FMCS via a Freedom of Information Request in January 2008. It contains all work stoppages in the health care industry from 1984-2004.[10]

The FMCS data show strike activity in the health care industry is concentrated in relatively few states, with 4 states accounting for nearly 60% of health care strikes. Because our strike data cover a period during which health care workers were allowed to organize (and thus the observed strikes are likely not due to union recognition), variation in state union concentration can likely explain a large portion of this variation. For analysis and discussion of the reasons for state variation in health care unionization rates see Freeman (1998) and Holmes (2006). Our analysis focuses on hospitals in New York State which accounted for one in every six health care facility strikes in the United States during our sample period.

The focus of our study is hospitals providing inpatient care. The FMCS data does not distinguish hospitals from other health care facilities, nor does it report the names of the facilities struck in a uniform manner (i.e. a struck facility may be referred to as "Catholic Health Care" rather than St. John's Hospital). Hospitals were thus identified manually in the data using both hospital name and facility address, and were checked using the New York State Hospital Profile website.

Hospitals employ a diverse group of workers, ranging from those who provide little or no

[10] Our 1983 strikes were found using a Lexis-Nexis search for hospital strikes in New York State for the year 1983. This search revealed five additional strikes that we incorporate into our analysis. We note that although our empirical specification contains outcome data for 6-months prior to the striking period, because 4 of the 1983 strikes begin in either April or May of 1983, our current results contain only 4 or 5 pre-strike months for these strikes.

patient care (e.g. laundry workers and parking attendants) to those with whom the primary responsibility for the patient rests (e.g. physicians and nurses). Because we wish to focus on nurses strikes, we are particularly interested in identifying the group(s) of workers that struck at each hospital. Using only the data provided by the FMCS, we were able to identify the struck bargaining unit in 38% of the strikes using either the union name (e.g. New York State Nurses Association) or the name of the title of the union representative (e.g. Nursing Representative, RN Representative). For cases in which the bargaining unit was not clearly specified in the data (such as strikes with missing bargaining unit data or involving unions with diverse groups of workers), the construction of our dataset required searching news archives for articles detailing the bargaining unit involved in each strike. In the cases where we could not obtain this information from news archives, hospital administrators, as well as the listed union, were contacted and followed up. If bargaining unit information could not be obtained, these hospitals were dropped from our sample.[11]

Our final sample covers 50 strikes at 43 hospital facilities during the years 1983-2004. Using this sample, the strike data were manually matched by hospital name and address to physical facility identifiers in the New York State hospital discharge data (see below), as were data on the exact dates of the hospital work stoppages. For strikes which name a hospital with multiple campuses, all campuses under common ownership are classified as struck.[12]

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[11] There were only 3 strikes at two facilities that were dropped.

[12] A unique feature of many metro-New York City hospitals is their participation in industry-wide contracts covering dozens of facilities through the League of Voluntary Hospitals and Homes (League), an association of non-profit medical centers, hospitals, nursing homes and their affiliated facilities. The League acts as the bargaining agent for its members in labor contracts and represents them primarily in labor negotiations with 1199 Service Employees International Union (1199). Three of the strikes that occur during our sample period involve the League. Because League strikes sometimes involved dozens of facilities striking simultaneously, no publicly available sources explicitly documented the struck bargaining units at each individual hospital during League strikes. Therefore, we assumed knowledge of the correct group of striking workers at a League hospital only if we could find specific information on the bargaining unit struck at a particular hospital during a specified strike. For example, evidence of nurse representation at a League hospital in 1973 is not taken as evidence of representation in 1989

The genesis of these strikes is varied; based on our newspaper research, most were over wages, while some were over nurse staffing ratios. For example, on July 1, 1999 Central Suffolk Hospital, a 153-bed facility in Riverhead Long Island, was struck by 253 registered nurses, technicians and other staff who were members of the New York State Nurses Association. The striking employees had been working without a contract for 6 months and were demanding a contract providing 3-percent raises for each year of the contract, retroactivity to the end of their previous contract, better staffing, and job security guarantees. Hospital management, claiming large losses from cuts in Medicare reimbursement, countered with 2-percent raises per year and refused to grant the union retroactive pay raises for the 6-month period without a contract.

The strike lasted 17 days, during which the hospital hired replacement workers to fill in for the striking nurses. Hospital administrators claimed that all services functioned normally, with no disruption in care. Union members, on the other hand, claimed to have heard from Health Department inspectors that six medication errors were made, four of the replacement workers were sent home for incompetence, and that narcotics were missing in one department. The strike was ultimately settled with an agreement that granted union members a 2.5-percent raise, retroactive to April 1 and an acknowledgement from hospital spokeswoman Nancy Uzo that to work with the replacements is "not the same as working with people who have worked here for five or ten years."[13]

Table 2 and table 3 show the characteristics of the sample of strikes we use over the 1984-2004 period. Our sample contains 43 different facilities, 5 of which were struck twice and

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unless a specific document makes reference to nurses striking in 1989. Using these criteria, we include 6 struck League hospitals in our sample, dropping all hospitals without specific bargaining unit knowledge.

[13] Bleyer, B., 1999. Central Suffolk Hospital Nurses Approaching Strike Deadline. Newsday, 30 June p. A48. Anonymous, 1999. Central Suffolk Hospital Workers Go Out On Strike. Newsday, 2 July p. A29. Gannon, T., 1999. No Cure in Sight for CSH Strike. The News Review Online, [internet] 8 July. Available at: http://www.timesreview.com/_nr_html/nr07-08-99/stories/news1.htm [Accessed 9 March 2010]. Freedman, M., 1999. Striking Nurses Approve Contract. Newsday, 15 July p.A31. Freedman, M., 1999. OK'd Pact Ends Hospital Strike. Newsday, 17 July p.A21.

one of which was struck three times, for a total of 50 strike-facility combinations.[14] Strike duration is right-skewed, with the median strike lasting 19 days, and a mean strike length of 32 days. Twenty-one of our 50 striking hospitals admitted fewer than 30 patients per day. Three-fourths of our strikes are concentrated in the downstate area (regions 5-11), though our sample is distributed across all regions, with at least one strike from each of the 11 New York State regions. Table 3 reveals that 26 of our 50 strikes occurred in 1990 or earlier. For the pre-1991 strikes, 46% of these lasted 4 weeks or longer, and 19% a week or less. For the post-1990 strikes, fewer strikes last for an extended period of time, with only 29% lasting 4 weeks or longer and 42% for 7 or fewer days, though this period saw a number of especially long strikes, such as those at Nyack Hospital in 1999 (180 days struck) and St. Catherine of Siena Hospital in 2002 (105 days struck).

Hospital Discharge Data

Each short-term non-federal hospital in New York State is required to submit discharge data to the New York State Department of Health through the Statewide Planning and Research Cooperative System (SPARCS). SPARCS has collected, at the patient level, detailed data on patient characteristics (e.g. age, sex, race), diagnoses (several DRG and ICD-9 codes), treatments (several ICD9 codes), services (accommodation), and total charges for every hospital discharge in New York State since in 1982. These data are reviewed for quality and completeness by the New York State Department of Health. Failure to submit these data can carry consequences for

[14] Though there were a total of 51 strikes in our initial sample, because one hospital closed completely during its strike and therefore admitted no patients while struck, it is excluded from the sample.

the hospitals, including the withholding of reimbursement.[15] Our data include the universe of discharges from New York State from 1983-2005.

We include for each discharge abstract record a 3-digit Diagnosis Related Group (DRG) weight as reported for the years 1983-2005 by the Center for Medicare and Medicaid Services (CMS), matching each year of discharge data with the corresponding year provided by CMS. This enables the creation of a case mix index for each hospital-day. Case mix is commonly used in administrative data to measure overall illness severity and case complexity. As an additional illness severity control, we include for each administrative record the unweighted comorbidity illness components of the Charlson Index, an index shown to be strongly associated with mortality **(**Quan et al., 2005**)**.[16]

As noted earlier, the strikes in our data typically last for a matter of days or weeks. Unless strike effects persist for a period long before and after a strike, identification of strike effects requires data collected at sufficiently precise time intervals so as to allow for outcome measurement at the weekly or even daily level. The standard issue, non-identifiable SPARCS discharge files, however, allow only for the identification of the month and year of any given admission, discharge or procedure. Our analysis makes use of restricted data elements not available in the public use data files, including the year, month and day of each admission, discharge, and procedure, as well as well as identifiers which enable the longitudinal tracking of patients within and across New York State facilities.[17] Approval for these restricted data

[15] http://www.health.state.ny.us/statistics/sparcs/sysdoc/operguid.htm
[16] Our identification of these conditions utilizes code made available through the University of Manitoba Centre for Health Policy at:
http://mchp-appserv.cpe.umanitoba.ca/viewConcept.php?conceptID=1098#a_references
[17] Prior to 1995, patients in the New York State data could not be tracked longitudinally across facilities, due to the lack of a unique personal identification number which is consistent across hospitals (same-hospital readmission is identifiable prior to 1995). Beginning in 1995, New York hospitals began collecting an element consisting of a combination of a patient's last name, first name, and social security number which enabled the calculation of patient readmission. Accordingly, all strikes in our data occurring before 1995 contain no patient readmission measures.

elements required authorization from a Data Protection Review Board (DPRB) overseen by the state.

For our analysis, we will use all data from each SPARCS region in which there is a strike during the 1-year period surrounding the strike. The SPARCS region is a geographical subdivision of the New York State, as defined by the New York Department of Health. These regions correspond closely to the Health Service Areas (HSA), commonly used measures used to define hospital inpatient activity by New York State, though there are fewer HSAs, due mostly to the consolidation of the 5 boroughs as an HSA. For each region in the year surrounding the strike, we will use all discharge records from hospitals providing short-term inpatient care.[18] Our sample therefore consists of all hospitals in any SPARCS region in the one year time period surrounding the date of a strike in that region.

We consider two measures of patient outcomes that may be affected by strikes. Our primary outcome of interest is in-hospital mortality. This is a clear measure of hospital performance along a dimension with unambiguous welfare implications. Following Gowrisankaran and Town (1999) and Geweke, Gowrisankaran and Town (2003), we consider an in-hospital mortality measure which records as mortality a death occurring within the first ten days of a patient's date of admission. This short follow-up period is chosen because beyond this point, hospitals sometimes transfer terminally ill patients to other facilities, and thus deaths occurring after the first ten days may not reflect initial management and care.[19] Of course, a limitation of our analysis is that we only know within-hospital mortality, and not mortality

[18] While this allows for the possibility of using some discharges from hospitals providing care that might be different than the striking hospitals (all of which are general hospitals), using American Hospital Association survey information from 1984 and 1999, the authors calculate within an HSA, the share of discharges from non-general hospitals in New York State is less than 5%.

[19] Additionally, in two separate studies of heart disease patients, McClellan, McNeil, and Newhouse (1994) and McClellan and Staiger (1999), find that there is a very strong correlation between short period measures of mortality (7-day mortality) and longer period mortality measures (30-day mortality).

following hospital stays. Thus, it is possible that any mortality increases that we find may reflect shifts in the timing of deaths; for example, Cutler (1995) finds that prospective reimbursement under Medicare led to a short run rise in mortality but no long run effect.[20]

Our second major outcome measure is hospital readmission, which is defined in our data as an inpatient re-hospitalization, for any reason, which occurs within 30 days of the discharge. Hospital readmission is often an indicator of poor care or missed opportunities to improve quality of care during a hospital admission (MEDPAC 2007), and has been widely used by health economists as a proxy for the quality of hospital care (Cutler 1995; Ho & Hamilton, 2000; Kessler & Geppert, 2005). This measure has also recently been proposed by policymakers as a quality metric to which Medicare reimbursement could be tied (Bhalla & Kalkut, 2010).[21]

We also consider as dependent variables two utilization measures of hospital inputs: the length of stay for the patient and the number of procedures performed while in the hospital. In addition, we explored using total charges incurred to the patient as a measure of total resource utilization, though the results were sufficiently imprecise that we could not rule out either very large or small effects.

We also control for a variety of patient characteristics. All models control for available patient demographics, including age, gender, race (white vs. non-white), and the number of conditions with which each patient is diagnosed upon their hospital admission. In addition, we can use data on diagnosis codes to form measures of patient illness severity. Whether such a measure should be included is unclear since severity codes may themselves by impacted by the

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[20] We also ran all of our results using in-hospital mortality defined as a death between admission and discharge date. Our results are similar using this as an outcome measure.

[21] We considered both any-hospital readmission (as reported in the paper) as well as readmission excepting transfer to an acute care hospital. The results are similar using both measures of readmission.

strike. We find no such effect on severity, however, and our results are not affected by the inclusion of this control, as we discuss below.

Since the relevant variation is at the hospital/day level, we aggregate our data to that level; our sample consists of 392,679 hospital/days of data from 288 hospitals for our 50 hospital-strike combinations. We use three measures of "exposure" of patients to a strike. The first is a dummy variable for whether the patient's day of admission was during the strike. This is the most straightforward measure but suffers from the problem that patients may be impacted by strikes that occur after their admission to the hospital. We therefore consider two alternatives: the share of patients admitted in that day who are exposed at some point during their stay to a strike; and the share of the stay that was during a strike, among patients admitted that day. These are more complete "exposure" measures but may suffer from the fact that length of stay may be impacted by the strike. In fact, as we show, our results are very robust to the exposure measure used.

The means for our sample are presented in table 4. The mean number of daily admissions for hospitals in our sample is 28, or approximately 10,220 yearly admissions. Using the AHA average number of discharges per bed for the U.S. for 1994 (the mid-point of our sample), this translates to approximately 271 beds. The average daily case mix index of 1.01 reflects that hospitals in our sample treat patients with a resource need comparable to the average U.S. hospital. The average 10-day in-hospital mortality rate is 1.9%. The average readmission rate (available only post-1995) is 13.8%. Fifty-eight percent of the patients in our sample are female, two-thirds are white, and the average age is 44.5. The number of conditions and number of Charlson comorbidities with which a patient is diagnosed are 3.4 and 0.56 respectively. Four-tenths of one percent of patients in our data are admitted during a strike.

Part III: Empirical Strategy

Our basic empirical strategy is to examine the utilization and outcomes in striking hospitals during the strike, relative to outcomes the rest of the year in that hospital, and relative to the other hospitals in their region during this same period. The unit of observation is the hospital (h), within region (r), by date of admission (d). To do so, we will run regressions of the form:

$$OUTCOME_{hrd} = \alpha + \beta STRIKE_{hrd} + \gamma PDEM_{hrd} + \delta_h + \eta_d + \mu_y * \sigma_r + \varepsilon_{hrd} \qquad (1)$$

In this equation, OUTCOME is one of our measures of outcomes that might be affected by the strike (average daily mortality or average daily rates of readmission), STRIKE is one of our three measures of strike impact/exposure, and PDEM is the mean characteristics of patients admitted that day (case mix index, number of diagnoses, Charlson comorbidities, age, share white and share female). We also include a full set of fixed effects for each hospital (δ_h) and a set of fixed effects for date of admission, which includes year effects, fixed effects for each of the 52 weeks, and fixed effects for each of the 7 days of the week (η_d). Finally, we include a full interaction of year dummies (μ_y) with SPARCS region dummies (σ_r) to account for any differential time trends by area.

With this specification, our identifying assumption is that the only reason for changing outcomes in striking hospitals, relative to others in their region, is the strike itself. We are able to rule out concerns about permanent differences between striking and non-striking hospitals through the use of hospital fixed effects; we are only looking at differences that emerge during the strike, relative to the remaining period of the year when there is no strike.

There are two potential concerns with such an approach. The first is that there are underlying trends in hospital outcomes that are concurrent (or even causing) the strike. For example, deteriorating conditions in a hospital may cause both worsening outcomes over time and the desire to strike. As discussed above, we have found no evidence of this as a cause of strikes. Nevertheless, we will carefully investigate the dynamics in outcomes around strike periods to see if there is any evidence of deteriorating outcomes preceding strikes.

The second concern is that the strike itself may change the composition of patients in the hospital, leading to changes in outcomes through composition bias and not real changes in treatment. For example, if strikes lead to admissions of only sicker patients, then this would be associated with both worse outcomes and more intensive treatment. Indeed, strikes are associated with reductions in hospital admissions. But we find no evidence that they are associated in any way with changes in patient demographics or case mix. Moreover, such a hypothesis would suggest that strikes would be associated with improved outcomes in nearby hospitals, or in striking hospitals after the strike has ended. We find evidence for neither. Finally, we show in section VI that for strikes where replacement workers are used, there is no decline in admissions, yet we continue to see adverse effects on outcomes.

Part IV: Patient Outcome Results

In this section, we examine the impact of strikes on in-hospital mortality and hospital readmission. Table 5 presents our basic results for inpatient mortality. The first panel uses an indicator for the day of admission being during the strike as our measure of strike exposure. The first column shows a regression of average daily mortality for patients admitted that day on an indicator for whether that day was during a strike. This regression includes only the fixed effect for hospital, time, and region×time interactions, as well as the strike indicator. We find a highly significant increase in patient mortality associated with being admitted during a strike: among patients admitted during the strike, inpatient mortality is 0.36% higher than comparable patients admitted before or after the strike. This represents an increase of 19.4% relative to the baseline mortality rate of 1.86%, a sizeable increase.

The next column adds demographic characteristics, and the results are very similar, with the mortality coefficient rising to 0.37%. The third column in this first panel adds indicators for patient severity, and the result is once again very similar, with a coefficient of 0.35%. The coefficients on the case mix and Charlson comorbidity measures are positive and highly significant, as would be expected: mortality rates are higher for admission days with a sicker case mix. There is also a positive association with average age, and a negative association with percent female. Interestingly, controlling for these other characteristics, there is no association with the share of patients who are white, and a negative association with the total number of conditions with which a patient is diagnosed.

The next two columns extend the results to consider our two alternative measures of strike exposure. When strike exposure is measured as the percentage of patients admitted that day who are exposed to the strike, the coefficient is slightly smaller; when it is measured as the percentage of the stay that occurs during the strike, the impact is slightly larger. Overall, our findings are not sensitive to either controls or the measure of strike exposure.

Table 6 repeats this exercise for our other measure of patient outcomes, hospital readmissions. As noted earlier, readmissions information is only available after 1995, so our sample is restricted to the 14 strikes that took place during that period. As with mortality, there is a highly significant and robust increase in readmissions associated with strikes. For our strike admission indicator, we find that strikes are associated with a rise in readmission rates of 0.9% in the richest specification, off a base of 13.8%, so this represents a roughly 6.5% increase. The results are once again very robust with respect to the inclusion of demographic and severity controls, and with respect to the measure of strike exposure used.

Timing and Pre-existing Trends

One concern noted above is that our difference-in-difference identification strategy may be unable to disentangle differential trends between treatment and control hospitals. If strikes occur at hospitals where quality is exogenously deteriorating, it could give the appearance of a negative causal impact of strikes on outcomes.

Table 7 addresses this point by including in the regression, dummies that equal one for those admitted 16-20 days before the strike, 11-15 days before, 6-10 days before, and 1-5 days before, as well as 1-5 days after, 6-10 days after, 11-15 days after, and 16-20 days after the strike. As we show for both of the outcome variables in that table, there is no indication of any significant trend in outcomes before the strike; all of the dummy variables for the period beforehand are insignificant and, if positive, are small. The results are similar if we literally use 20 dummies to represent each day before the strike; three of the 20 dummies are significant for 10-day mortality, two negative and one positive, and two are significant for re-admission, one positive and one negative.

The lagged effects of the strike, showing the impact after the strike had concluded, are more mixed. For 10-day mortality, there is marginally significant lagged effect of the strike for those admitted during the 5-day period immediately following the strike. This suggests that beyond these first 5 days after the strike, there are no long lasting effects on treatment quality. For readmissions, there is little lagged effect of strikes; that is, there is no significant effect on those admitted even right after the strike.

Selection Bias Concerns

As noted earlier, another concern with our empirical strategy is that the nature of admissions may change when there is a strike. Indeed, there is a strong negative relationship between strikes and admission rates. However the fact that admissions fall does not mean that there is a change in the mix of patients admitted during a strike. In this section we explore those compositional concerns further by directly examining whether there is a change in the observable characteristics of patients admitted during a strike. Of course, this approach cannot rule out that there were unobservable differences among those admitted during a strike. But it seems unlikely, if patients admitted during a strike are very similar along all observed dimensions, that they would be very different along unobserved dimensions.

Table 8 shows the results of our basic specification where the dependent variable is the mean characteristics of patients admitted that day: average age, share female, share white, case mix index, number of Charlson comorbidities, and number of diagnoses. We also examine the change in insurance status for patients admitted during the strike using the daily share enrolled in Medicare, share enrolled in Medicaid, and uninsured individuals (those recorded as self-pay or exempt from charges). Furthermore, we analyze the change in income for patients admitted during strikes by imputing income at the zip code level.[22] In every single case there is an insignificant relationship between the average characteristics of patients and the strike indicator; that is, patients admitted during the strike are no different than those admitted in other periods. This should not be surprising given the insensitivity of the results to adding controls in our earlier tables.

These effects are not only insignificant; the confidence intervals are also very small. For

[22] Income is imputed using the median income recorded for a given zip code in the 1990 census for individuals admitted in 1983-1994, and the median income for a given zip code reported in the 2000 census for individuals admitted in 1995-2004.

example, we find that strikes are associated with a -0.013 change in case mix index, off a mean of 1.011. This is a reduction of 1.3%. Given the standard error, this implies that the most case mix could have fallen would be 2.7%, which is very modest given our 6.5% to 19.4% outcome effects.

If striking hospitals are admitting only the sickest patients, then one of two things must be happening to the healthier patients: either that they are delaying hospitalization or receiving treatment at other nearby hospitals. The former alternative is ruled out by our timing specification; delay in treatment by the healthiest patients would show up as negative lagged effects of the strike, which we do not see. The latter alternative can be tested by examining the impact of strikes on neighboring hospitals. We use two different methodologies to divide our control group into "very close" hospitals and "less close" hospitals within the region. These two methodologies follow methods used in the literature on hospital competition.

The first is to use a measure of geographical closeness: the three hospitals closest to the striking hospital as the crow flies. The second is to use a "patient flow" measure common in competition research, which finds the competitor hospitals to the striking hospital by: identifying the share of patients in the striking hospital that come from each zip code over the previous six months; ranking the zip codes from most common to least and counting down the list until we have accounted for 40% of the hospital's discharges; and then choosing any hospital that has at least 3% of the discharges in this set of zip codes.

The results from using these two different approaches, for our key outcome variables, are shown in table 9. Panel one reports the results from our specification (excluding our demographic and severity measures), using as our outcome variable the logarithm of the number of admissions at the nearby hospitals. The results indicate that nearby hospitals are admitting 2-

3% more patients during the strike, though neither of these coefficients are significant at conventional levels. Panels two and three show that there are actually *positive* mortality and readmission effects on nearby hospitals, though again in none of the cases are these effects significant. However, if anything, these results suggest that nearby hospitals are admitting *sicker* patients, so that selection is not driving our findings.

When we expand our analysis to include all hospitals in the same region as the striking hospitals, our results show a similar pattern. In order to assess the region wide effects of strikes, we run regressions of the form:

$$OUTCOME_{rd} = \alpha + \beta STRIKE_{rd} + \gamma PDEM_{rd} + \eta_d + \delta_w + \mu_y * \sigma_r + \varepsilon \quad (2)$$

where the unit of observation is the region (r) by date of admission (d). As was done in our previous specification, we include a full set of year (μ_y), week (δ_w), day (η_d) and region (σ_r) fixed effects, as well as a full interaction of year dummies with SPARCS region dummies. We measure our STRIKE variable as an indicator of whether a hospital is struck in a particular region on a specific day.

Table 10 presents our basic results for our outcome measures, using the specification in (2). Column one shows that admissions do not decline at the regional level during the strike. Both of our strike measures, however, indicate that regions with a striking hospital have worse outcomes during the strike. Our mortality regression shows considerably smaller yet significant effects at the regional level, while our readmission measure also indicates that patients in a struck region experience worse outcomes. Given that the number of regional hospital admissions does not change during a strike, this suggests that the deterioration in outcomes is not simply due to a redistribution of admission severity across the region during the strike.

Part V: Utilization Outcomes

The evidence in Part IV strongly suggests that patients admitted during strikes have significantly worse outcomes than patients admitted at other times. Is this because they receive less care, or because they receive worse care? To address this, we now turn to measures of patient treatment intensity.

Table 11 shows our basic results for two measures of treatment intensity: length of stay and number of procedures performed during the stay. For length of stay, we find a positive but insignificant impact of the strike, while for the number of procedures performed, our estimate is negative but insignificant. Thus, the findings suggest that strikes are associated with an intensity of treatment that is no more or less than the treatment received at hospitals during a non-striking period. Thus, the poor outcomes associated with strikes are not due to a lack of treatment intensity.

Part VI: Heterogeneity in Strike Effects

In the section that follows, we examine specific subsets of our data, in order to examine whether specific groups of patients are differentially impacted by the strikes. We first consider two patient subsamples from our data, grouping patients by both the treatment urgency and nursing inputs required for their specific conditions. We then divide our strike sample according to information we collected concerning the use of replacement workers for each of our strikes.

Heterogeneity by Admission Urgency

As noted above, a potential concern with our analysis is that healthier patients refrain from treatment at the striking facility. We showed previously that there is no evidence of a delay in hospital use by healthy patients or a shift to other hospitals in the area. A further means of addressing this potential concern is to split our sample into emergency patients who are indicated by the hospital as requiring immediate medical attention, and a non-emergency sample who are not indicated as such. If our results is driven by avoided care among healthy patients, then we should observe an increase in mortality and readmission for the non-emergency patients, who have the option of exercising discretion over the timing of treatment (and will thus seek treatment at a striking hospital only for more serious conditions) and no mortality effect for emergency admissions. In addition, such a distributional shift should produce a much sharper drop in the number of non-emergency patients admitted to the hospital during the strike.

To assess whether this is the case, we run our main regressions for both our outcome variables and our demographic and severity measures, splitting our sample and allowing our strike coefficient to vary for each sample. Table 12 reports the strike coefficients from our full specification for the outcome, utilization, demographic, and severity variables. Each row contains only the strike coefficient from our full regressions (which includes our full set of fixed effects, severity controls, and demographic controls), while each column indicates a specific subsample for which we estimate our model. Columns 1 and 2 report the results from our regressions for the emergency/non-emergency samples. These results indicate that the increase in mortality and readmission are likely not a result of a redistribution in admission urgency. Both the emergency and non-emergency subsamples show an increase in mortality during the strike. Mortality for patients in the emergency sample is a marginally significant 0.31 percentage points or 11.8% higher relative to the emergency baseline mortality rate of 2.6%. Mortality for patients in the elective sample is a statistically insignificant 0.18 percentage points or 19.6% higher relative to the non-emergency baseline mortality rate of 0.9%. Our readmission results are also stronger for the emergency sample, where we observe a statistically significant 1.4 percentage point increase off of our base readmission rate of 16.2%, with no significant readmission increase for the non-emergency sample.

We see no changes in utilization or severity measures for emergency patients, though the share of white patients does increase during the strike. For the non-emergency patients, we see evidence of a -0.1 point decrease in the number of procedures off of the sample mean of 1.5 procedures, and a decrease in the case mix of 0.03 points as compared to the non-emergency case mix mean of 0.87. The last row in our table reports results from a specification which uses as a dependent variable the log number of daily admission at each hospital for each type of admission. The decrease in admissions for both the emergency and non-emergency sample is quite similar, with the number of emergency admissions decreasing by 28% during a strike, and the number of non-emergency decreasing by 28.4%. Thus, these results provide further evidence that patient avoidance/selection is not driving our findings.

Differences in Nursing Intensity

Because we are specifically examining the effects of strikes involving nurses, an additional dimension along which we should observe differential strike effects is the extent to which a patient's condition depends on nursing inputs. If the effects that we observe are in fact due to the striking nurses, then we should expect particularly pronounced outcome effects of strikes on patients whose care requires a high degree of nursing attention. To account for this, we acquired a set of weights designed specifically to quantify differences in the intensity of care required for acute care patients. These nursing intensity weights (NIWs) were developed by a panel of registered nurses assembled by the New York State Nurses Association and the New York State Department of Health, and its members are representative of the state's geographic and institutional diversity. The calculation of the weights was first instituted in 1983 and has been updated for changes in DRGs as they occur. The NIWs are derived by proposing a "typical" patient scenario for each DRG and measuring the predicted nurse workload for that patient stay. Using this measure, for each year in our data, we calculate the median NIW for each diagnosis and divide our sample into diagnoses which require above and below median nursing intensity.

Our results split by nursing intensity are presented in the third and fourth columns of table 12. These results reveal that our mortality effects are more pronounced for patients whose diagnoses require more nursing resources, as evidenced by our estimate indicating a 0.35 percentage point increase in mortality during the strike relative to the 2.8% baseline for the most nursing intensive patients. For this same subsample, the readmission effect of 1.2 percentage points relative to the sample mean of 16.9% implies a 7.1% increase in readmission, though this estimate is insignificant at conventional levels. For diagnoses with below median nursing intensity, we find little evidence of a mortality or readmission effect. We find little change in utilization or demographic characteristics for both subsamples, with the exception of a small increase in the length of stay and the female share for the less nursing intensive sample.

Replacement Worker Strikes

A particularly relevant dimension over which the effects of these strikes may also differ involves the decision of the hospitals involved to hire replacement nurses. A number of New York hospitals are reported to have hired temporary replacement workers to fill in for striking nurses. This practice became particularly frequent beginning in the early 1990s, when temporary nursing agencies (e.g. U.S. Nursing Corp., Health Source) began making available to hospitals engaged in contract disputes, teams of nurses to staff hospitals in the event of a strike. Our search of news archives enabled us to distinguish 13 strikes in which it was reported that the hospital involved employed replacement workers during the strike. Using this information, we analyze separately the sets of strikes in which replacement workers were hired.

Previous literature is unclear as to whether replacement workers can substitute for striking workers. For example, Cramton and Tracy (1998) find that firms are more reluctant to use replacement workers when employees in a struck bargaining unit are more experienced. Their finding suggests that for professions which require specialized knowledge or firm specific know-how, employers do not view replacement workers as direct substitutes for striking workers. Krueger and Mas (2004), however, find that in the "highly complex, labor-intensive" tire industry, tire defects were relatively infrequent during a period in which replacement workers were employed in large numbers, with an increase in defects occurring when replacement workers and returning strikers worked together.

Our results are presented in the fifth and sixth columns of table 12. A key dimension over which these strikes clearly differ is the degree to which admissions decrease during these strikes. For our set of replacement strikes, there is no noticeable decrease in hospital admissions during the strike period, while for strikes with no indication of replacements, admissions decrease by over 55%. The mortality effects for these strike types are, however, similar in magnitude, with a 0.31 percentage point increase in mortality for the replacement-worker strikes on a base of 1.8% and a 0.35 percentage point increase for the non-replacement-worker strikes on a base of 1.9%.

We also observe similar impacts on readmission rates, but the effects are not statistically significant for the sample with no replacements used.

The results also show a difference in the utilization and severity of patients admitted to hospitals who choose to hire replacement workers. For the replacement-worker sample, there is very little change in the observable demographic, severity, and utilization patterns of patients admitted during a strike. For the remaining strikes, however, we observe a decrease in severity, as measured by the case mix index, as well as an decrease in the number of procedures performed of -0.12, a 7.4% decrease compared to the baseline of 1.62 for this sample. Overall, these results suggest that the use of replacement workers does not significantly alter our finding of worsening outcomes during the strikes. Thus, while these workers may serve as a useful bargaining tool for the hospitals, they do not noticeably improve the quality of hospital care during a strike.

Part VII: Conclusions

A long standing concern with strikes as a means of resolving labor disputes is that they may be unproductive, and recent research in some production sectors has demonstrated reduced productivity during strikes. But a sector where strikes may be particularly pernicious is hospitals, where the consequences are not just lower quality products but life and death.

To address this question, this study utilizes a unique dataset collected on every nurses' strike over the 1984 to 2004 period in New York State. Our restricted-use dataset allows us to match our strike data with exact dates of patient admission, discharge and treatment, and allows for a rich set of demographic and illness severity controls. Each striking hospital over this period is then matched with the set of hospitals in their geographic area, and the evolution of outcomes is examined before, during, and after the strike in the striking versus non-striking hospitals.

We find a substantial worsening of patient outcomes for hospitals struck by their nurses. Our mortality results show a 19.4% increase during strikes relative to their baseline values, and our estimates imply a 6.5% increase in readmission rates for patients admitted during a strike. Our results show no difference in the characteristics of patients admitted during strikes, and little difference in observable aspects of hospital utilization during these strikes. We find that patients with particularly nursing intensive conditions are more susceptible to these strike effects, and that hospitals hiring replacement workers perform no better during these strikes than those that do not hire substitute employees.

Our results imply that strikes were costly to hospital patients in New York. In our sample, there were 38,228 patients admitted during strikes, and we estimate that 138 more individuals died because of strikes than would have died had there been no strike. By a similar calculation, 344 more patients were readmitted to the hospital than if there had been no strike. Moreover, these poor outcomes do not reflect less intensity of care. So this is very clear evidence of a reduction in productivity; hospitals functioning during nurses' strikes do so at a lower quality of patient care.

The effects of these strikes must, however, be considered in the context of a total union effect on hospital output and patient outcomes. Our results reveal a short-run adverse consequence of hospital strikes. These strikes may, however, contribute to long-run improvements in hospital productivity and quality driven by union-related workplace improvement initiatives. Such improvements have been implied by both Register (1988) and Ash and Seago (2004) who respectively document both a hospital union output effect and lower heart-attack mortality rates in unionized hospitals. Future work could usefully incorporate these short term costs and longer-term benefits in a full evaluation of hospital unionization.

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Table 1

Work Stoppages by Industry, 1984-2004

Industry	Number of Strikes
Manufacturing	6,575
Retail, Wholesale & Service	1,973
Construction	928
Health Care	730
Transportation	574
Local Government	421
Food Manufacturing/Processing	362
Mining	144
Electricity & Natural Gas	120
Communications	112
Maritime	69
Petro Chemicals	60
Food Retail Sales/Distribution	46
State Government	13
Federal Government (Postal Service)	6
Other	119

Source: Federal Mediation and Conciliation Service

Table 2
The Strike Sample

	(1)
Number of Strike-Facility Combinations	50
Mean Strike Length (days)	32.1
Std. Dev. Strike Length	39.2
Median Strike Length (days)	19.0
<u>Distribution of Struck Hospital Size</u>	
(Avg. # of daily admissions 6-months prior to strike)	
5-14 admissions	11
15-29 admissions	10
30-45 admissions	17
45+ admissions	12
<u>Distribution of Facilities Struck Across Regions</u>	
Region 1	5
Region 2	1
Region 3	1
Region 4	4
Region 5	7
Region 6	5
Region 7	8
Region 8	2
Region 9	2
Region 10	8
Region 11	7

Note: Region 1 includes Allegany, Cattaraugus, Chautauqua, Erie, Genesee, Niagara, Orleans and Wyoming counties
Region 2 includes Chemung, Livingston, Monroe, Ontario, Schuyler, Seneca, Steuben, Wayne and Yates counties
Region 3 includes Broome, Cayuga, Chenango, Cortland, Herkimer, Jefferson, Lewis, Madison, Oneida, Onondaga, Oswego, St. Lawrence, Tioga and Tompkins counties
Region 4 includes Albany, Clinton, Columbia, Delaware, Essex, Franklin, Fulton, Greene, Hamilton, Montgomery, Otsego, Rensselaer, Saratoga, Schenectady, Schoharie, Warren and Washington counties
Region 5 includes Dutchess, Orange, Putnam, Rockland, Sullivan, Ulster and Westchester counties
Region 6 includes Bronx county
Region 7 includes Kings county
Regions 8 and 9 include New York county
Region 10 includes Queens and Richmond counties
Region 11 includes Nassau and Suffolk counties

Table 3
Hospital Facilities Struck in NY State

Year	1983-1986	1987-1990	1991-1994	1995-1998	1999-2004
Length less than 1 week	1	4	6	3	1
1 week≤length<2 weeks	2	2	0	1	0
2 week≤length<4 weeks	2	3	2	1	3
4 weeks ≤ length	7	5	1	2	4

Table 4
Sample Means

	Mean (1)	Std. Dev. (2)
Hospital Characteristics		
Number of Daily Admissions	28.0	24.5
Daily Case-Mix Index	1.011	0.337
Outcome Variables		
10-day In-hospital Mortality	0.0186	0.0299
Length of Stay	7.58	4.61
Total Procedures Performed	1.65	0.73
30-day Readmission Rate	0.138	0.075
30-day Readmission Rate (excluding transfers)	0.121	0.069
Patient Characteristics		
Average Age	44.46	11.14
Proportion Female	0.58	0.11
Proportion White	0.67	0.29
Number of Diagnoses	3.36	1.29
Charlson Index	0.56	0.30
Proportion Covered by Medicare	0.30	0.16
Proportion Covered by Medicaid	0.19	0.19
Proportion Uninsured	0.07	0.08
Log Income	10.57	0.39
Strike Exposure		
Admitted During Strike	0.0035	0.0588
Proportion of Patients Admitted Exposed to Strike	0.0045	0.0621
Proportion of Patient Stay Exposed to Strike	0.0036	0.0525
Distribution of Admission Type		
Emergency	0.56	0.22
Non-emergency	0.44	0.22

Notes: Case-mix index, outcome variables, patient characteristics and distribution of admission type are weighted by the total number of admissions. Readmission rates calculated for post-1995 strikes only. Individuals are recorded as uninsured if their discharge record specifies self-pay or exempt from charges. Income is imputed using the median income recorded for a given zip code in the 1990 census for individuals admitted in 1983-1994, and the median income for a given zip code reported in the 2000 census for individuals admitted in 1995-2004

Table 5

Impact of Strikes on In-Hospital Mortality

Independent Variable:	Indicator for Admitted during strike	Indicator for Admitted during strike	Indicator for Admitted during strike	Proportion Admitted exposed to strike	Proportion Admitted exposed to strike	Proportion Admitted exposed to strike	Proportion of stay that was during strike	Proportion of stay that was during strike	Proportion of stay that was during strike
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
strike	0.00363***	0.00374***	0.00355***	0.00279***	0.00299**	0.00280***	0.00435***	0.00450***	0.00429***
	(0.00118)	(0.00107)	(0.00108)	(0.00107)	(0.00096)	(0.00097)	(0.00128)	(0.00115)	(0.00115)
Avg. Age	-	0.00068***	0.00044***	-	0.00068***	0.00044***	-	0.00068***	0.00044***
	-	(0.00002)	(0.00002)	-	(0.00002)	(0.00002)	-	(0.00002)	(0.00002)
Avg. Share Female	-	-0.00285***	-0.00128**	-	-0.00284***	-0.00128**	-	-0.00285***	-0.00128**
	-	(0.00066)	(0.00064)	-	(0.00066)	(0.00064)	-	(0.00066)	(0.00064)
Avg. Share White	-	-0.00291*	-0.00376**	-	-0.00291*	-0.00376**	-	-0.00291*	-0.00377**
	-	(0.00154)	(0.00166)	-	(0.00154)	(0.00166)	-	(0.00154)	(0.00166)
Casemix index	-	-	0.00235***	-	-	0.00234***	-	-	0.00235***
	-	-	(0.00057)	-	-	(0.00057)	-	-	(0.00057)
Avg No. of Diagnoses	-	-	-0.00086***	-	-	-0.00086***	-	-	-0.00086***
	-	-	(0.00026)	-	-	(0.00026)	-	-	(0.00026)
Avg. Charlson Index	-	-	0.01473***	-	-	0.01474***	-	-	0.01473***
	-	-	(0.00060)	-	-	(0.00060)	-	-	(0.00060)
N	393960	392679	392679	393960	392679	392679	393960	392679	392679

* p<0.10, ** p<0.05, *** p<0.01

All specifications are weighted by total admissions/patient days, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals

Table 6

Impact of Strikes on 30-day Readmission

Dependent Variable	30-Day Readmission			30-Day Readmission			30-Day Readmission		
Independent Variable:	Indicator for admitted during strike	Indicator for admitted during strike	Indicator for admitted during strike	Proportion admitted exposed to strike	Proportion admitted exposed to strike	Proportion admitted exposed to strike	Proportion of stay that was during strike	Proportion of stay that was during strike	Proportion of stay that was during strike
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
strike	0.01177***	0.00903**	0.00917**	0.01120***	0.00822**	0.00841**	0.01366***	0.01073***	0.01093***
	(0.00332)	(0.00350)	(0.00361)	(0.00308)	(0.00322)	(0.00332)	(0.00363)	(0.00356)	(0.00362)
Avg. Age	-	0.00193***	0.00137***	-	0.00193***	0.00137***	-	0.00193***	0.00137***
	-	(0.00012)	(0.00013)	-	(0.00012)	(0.00013)	-	(0.00012)	(0.00013)
Avg. Share Female	-	-0.03948***	-0.03671***	-	-0.03948***	-0.03671***	-	-0.03948***	-0.03671***
	-	(0.00368)	(0.00379	-	(0.00368)	(0.00379)	-	(0.00368)	(0.00379)
Avg. Share White	-	0.00662*	0.00871**	-	0.00660*	0.00869**	-	0.00660*	0.00869**
	-	(0.00357)	(0.00350)	-	(0.00358)	(0.00351)	-	(0.00357)	(0.00351)
Casemix index	-	-	-0.00606***	-	-	-0.00607***	-	-	-0.00607***
	-	-	(0.00105)	-	-	(0.00105)	-	-	(0.00105)
Avg No. of Diagnoses	-	-	0.00142*	-	-	0.00142*	-	-	0.00142*
	-	-	(0.00076)	-	-	(0.00076)	-	-	(0.00076)
Avg. Charlson Index	-	-	0.02646***	-	-	0.02646***	-	-	0.02646***
	-	-	(0.00164)	-	-	(0.00164)	-	-	(0.00164)
N	109721	109721	109721	109721	109721	109721	109721	109721	109721

* p<0.10, ** p<0.05, *** p<0.01

All specifications are weighted by total admissions/patient days, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals

Table 7
Trends in Outcomes Before and After the Strike (in days)

Dependent Variable	10 day In-Hospital Mortality	30-day readmission
n-20 to n-16	0.00053	-0.00693
	(0.00120)	(0.00848)
n-15 to n-11	0.00251	0.00513
	(0.00160)	(0.00735)
n-10 to n-6	-0.00194	0.00241
	(0.00157)	(0.00536)
n-5 to n-1	0.00017	-0.00110
	(0.00159)	(0.00718)
strike	0.00357***	0.00944**
	(0.00108)	(0.00375)
n+1 to n+5	0.00351*	0.01135
	(0.00211)	(0.00774)
n+6 to n+10	0.00016	0.00629
	(0.00161)	(0.00843)
n+11 to n+15	0.00228	0.01165
	(0.00158)	(0.00719)
n+16 to n+20	-0.00275*	-0.00675
	(0.00143)	(0.00691)
Casemix	0.00234***	-0.00607***
	(0.00057)	(0.00105)
Avg. # of Diagnoses	-0.00086***	0.00142*
	(0.00026)	(0.00076)
Avg. Charlson Index	0.01474***	0.02647***
	(0.00060)	(0.00164)
Age	0.00044***	0.00137***
	(0.00002)	(0.00013)
Female	-0.00129**	-0.03674***
	(0.00064)	(0.00379)
White	-0.00376**	0.00872**
	(0.00166)	(0.00351)
N	392679	109721

*** p<0.01, ** p<0.05, * p<0.1

All specifications are weighted by total admissions, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals.

Table 8

Effect of Strikes on Demographic and Diagnosis Characteristics

Dependent Variable	Age	Share Female	Share White	Casemix Index	Total Diagnoses	Charlson Index
	(1)	(2)	(3)	(4)	(5)	(6)
strike	-0.075	0.007	0.014	-0.013	-0.027	0.013
	(0.582)	(0.005)	(0.012)	(0.014)	(0.067)	(0.014)
N	392679	393960	393960	392679	393960	393960

Effect of Strikes on Insurance Status and Income

Dependent Variable	Share Medicare	Share Medicaid	Share Uninsured	Log of Income
	(1)	(2)	(3)	(4)
strike	0.01360	0.01254	-0.00338	-0.00214
	(0.00858)	(0.00940)	(0.00416)	(0.00484)
N	393960	393960	393960	393518

*** p<0.01, ** p<0.05, * p<0.1

All specifications are weighted by total admissions/patient days, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals.

Table 9

Effects of Strikes on Nearby Hospitals

Outcome Measures	Log Admissions		In-hospital Mortality		30-day readmission	
Method	RADIUS 3	FLOW 3	RADIUS 3	FLOW 3	RADIUS 3	FLOW 3
	(1)	(2)	(3)	(4)	(5)	(6)
Nearby Strike	0.02116	0.02904	0.00003	0.00055	0.00154	0.00166
	(0.02337)	(0.01839)	(0.00043)	(0.00034)	(0.00214)	(0.00121)
Avg. Age	-	-	0.00045***	0.00045***	0.00133***	0.00133***
	-	-	(0.00002)	(0.00002)	(0.00013)	(0.00013)
Avg. Share Female	-	-	-0.00122*	-0.00122*	-0.03695***	-0.03694***
	-	-	(0.00067)	(0.00067)	(0.00388)	(0.00388)
Avg. Share White	-	-	-0.00372**	-0.00372**	0.00954**	0.00959**
	-	-	(0.00174)	(0.00174)	(0.00373)	(0.00373)
Casemix index	-	-	0.00240***	0.00239***	-0.00600***	-0.00601***
	-	-	(0.00058)	(0.00058)	(0.00108)	(0.00108)
Avg No. of Diagnoses	-	-	-0.00089***	-0.00089***	0.00149*	0.00149*
	-	-	(0.00027)	(0.00027)	(0.00078)	(0.00078)
Avg. Charlson Index	-	-	0.01468***	0.01468***	0.02645***	0.02645***
	-	-	(0.00062)	(0.00062)	(0.00167)	(0.00167)
N	374607	374607	373287	373287	104233	104233

* p<0.10, ** p<0.05, *** p<0.01

All specifications are weighted by total admissions, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals

Table 10
Region-level analysis

Dependent Variable	Log Admissions	In-hospital Mortality	30-day Readmission
	(1)	(2)	(3)
strike	-0.00966	0.00037***	0.00719**
	(0.01083)	(0.00010)	(0.00316)
N	92410	92410	44164

* p<0.10, ** p<0.05, *** p<0.01

All specifications are weighted by total admissions, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals

Table 11

Impact of Strikes on Utilization

Dependent Variable	Length of Stay	Number of Procedures
	(1)	(2)
strike	0.259	-0.05225
	(0.219)	(0.06466)
Casemix index	3.44809***	0.60845***
	(0.16806)	(0.02927)
Avg No. of Diagnoses	0.62696***	0.19500***
	(0.12413)	(0.02517)
Charlson Index	0.76960***	-0.09593**
	(0.28565)	(0.04200)
Avg. Age	0.03857***	-0.00296***
	(0.00554)	(0.00111)
Avg. Share Female	0.19032	-0.05683*
	(0.17610)	(0.03112)
Avg. Share White	0.52456	0.08406
	(0.36110)	(0.08177)
N	392,679	392,679

* p<0.10, ** p<0.05, *** p<0.01

All specifications are weighted by total admissions, include controls for week, year, regionXyear, day of week and hospital fixed effects. Robust standard errors are corrected for clustering within hospitals.

Table 12
Patient and Strike Heterogeneity

Subsample	Emergency (49 Strikes)	Non-Emergency (49 Strikes)	Above Median Nursing Intensity	Below Median Nursing Intensity	Replacements Used (13 Strikes)‡	No Replacements Used (36 strikes)‡
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
In-hospital Mortality	0.00308*	0.00177	0.00353**	0.00067	0.00311**	0.00346**
	(0.00169)	(0.00110)	(0.00145)	(0.00056)	(0.00122)	(0.00165)
30-day Readmission	0.01431**	-0.00001	0.01215	0.00518	0.00814**	0.00987
	(0.00637)	(0.00364)	(0.00742)	(0.00450)	(0.00373)	(0.00779)
Length of Stay	0.130	0.283	0.104	0.288**	0.202	0.267
	(0.249)	(0.367)	(0.307)	(0.126)	(0.151)	(0.172)
No. of Procedures	0.01105	-0.10274**	-0.04697	-0.01564	0.04388	-0.11950***
	(0.08081)	(0.04784)	(0.07945)	(0.05588)	(0.03756)	(0.04500)
Avg, Age	0.897	-1.668*	-0.100	-0.352	0.571	-1.222
	(0.590)	(0.905)	(0.814)	(0.566)	(0.538)	(0.842)
Share Female	0.008	0.012	0.006	0.017**	0.003	0.008
	(0.006)	(0.013)	(0.005)	(0.008)	(0.006)	(0.006)
Share White	0.023***	0.001	0.021*	0.018	0.001	0.016
	(0.008)	(0.022)	(0.011)	(0.014)	(0.011)	(0.024)
Casemix Index	-0.001	-0.034**	-0.017	-0.014	-0.005	-0.043**
	(0.015)	(0.016)	(0.023)	(0.009)	(0.011)	(0.018)
Avg Num. of Diagnoses	0.040	-0.102	-0.004	-0.065	0.016	-0.027
	(0.074)	(0.080)	(0.077)	(0.062)	(0.091)	(0.037)
Avg. Charlson Index	0.021	-0.004	0.017	-0.008	0.016	-0.005
	(0.015)	(0.018)	(0.020)	(0.010)	(0.091)	(0.015)
Log number of admissions	-0.27940***	-0.28448***	-0.25189***	-0.32319***	-0.00710	-0.55461***
	(0.08161)	(0.08122)	(0.07055)	(0.07948)	(0.04532)	(0.07837)

All regressions are weighted by the number of daily admissions at each hospital. and control for day, week, year, region, region×year and hospital fixed effects. Outcome and utilization regressions (10-day in-hospital mortality, 30-day readmission, Length of Stay, and # of procedures) include controls for demographic and severity measures.

Log(1+number of admissions) is used to enable the inclusion of observations with zero admissions in the sample.

Robust Standard Errors in parenthesis are cluster corrected at the hospital level.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

‡ For the 30-day readmission result, because 30-day readmission is available only after 1995, this result includes only 8 replacement worker strikes, and 6 strikes without replacement workers