Do Strikes Kill?

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Hospitals are one of the most important employers in the United States. 35% of U.S. health care workers, and 3.25% of all U.S. workers, work in hospitals.[1] Hospitals are also one of the most important source of union jobs in the U.S. Over 15% of hospital employees are members of a union[2], representing 6% 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 today.[3]

An important bargaining tool for unions is the strike. A large literature over the past several decades has explored the causes and consequences of strikes. In particular, recent studies have documented a substantial reduction in the quality of production due to strikes (Kruger and Mas 2004; Mas 2008). Yet this literature has not been extended to the health care sector, where the consequences of lower quality due to strikes may be particularly dire. The small literature on medical care strikes and outcomes has focused on particular case studies and has delivered no consistent conclusion as to the impact on health care productivity.

In this paper, we carefully examine the effects of nursing strikes at hospitals on patient care and 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 (Krueger and Metzger 2002). Yet, at the same time,

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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)

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 long-term survival. 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 17% higher, and rates of hospital readmission are 10% 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.

We also find some evidence that strikes are associated with more intensive treatment of patients, with longer lengths of stay and a longer delay between admission

and primary procedure. Overall, our findings suggest that strikes lead to higher costs and 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, while Part V presents results on utilization measures. Part VI 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

[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.

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) However, after lobbying efforts by hospital-employee organizations, in 1974, President Nixon signed Public Law 93-360 which reversed the 27 year exclusion and 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]

[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.

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 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

[8] American Hospital Association Trendwatch Report, 2009

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, Rebitzer, Taylor and Votruba (2008)]. Healthcare institutions are thus particularly susceptible to labor unrest which disrupt these complex processes.

At the same time, a large body of research suggests that patients may be overtreated in the hospital, and that 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

[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.

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

[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 outome 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.

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 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. The data appendix lists each strike that we analyze in our data, the dates that each occurred, as well as the source from which the bargaining unit information was obtained.

[11] There were only 3 strikes at two facilities that were dropped. One of the facilities, Brookdale Hospital, struck twice during our period and was much larger than the average in our data (greater than 600 discharges per week versus around 200 in our sample). However, it is unlikely that this strike involved healthcare workers, given the union name and number of workers that struck. The other hospital (Columbia Memorial Hospital) averaged about 150 discharges per week.

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]

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.

[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 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. We analyzed an alternate set of striking hospitals, including League hospitals if there is *ever* mention in any League document from 1976-1996 of the profession of either registered nurse or licensed practical nurse being covered at this hospital. Thus undoubtedly produces a noisier measure of our strike variable. The magnitude of the in-hospital mortality effects are directionally similar using this criteria, albeit smaller and significant at the 5% level.

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 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

[13] The News Review Online, July 8, 1999, "No cure in sight for CSH strike", Newsday, July 17, 1999, page A21 "OK'd Pact Ends Hospital Strike", Newsday, July 15, 1999, page A31 "Striking Nurses Approve Contract", Newsday, July 2, 1999, page A29 "Central Suffolk Hospital Workers Go Out on Strike", Newsday, June 30, 1999 page A48 "Central Suffolk Hospital Nurses Approaching Strike Deadline",

[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.

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 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 a 3-digit 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-time period. Case mix is commonly used in administrative data to measure overall illness severity and case complexity.

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,

[15] http://www.health.state.ny.us/statistics/sparcs/sysdoc/operguid.htm

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.[16] Approval for these restricted data 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.[17] 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.

[16] 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.

[17] 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%.

We consider two measures of patient outcomes that may be impacted by strikes. Our primary outcome of interest is in-hospital mortality, defined as death occurring between admission and before discharge, irrespective of the cause of death. This is a clear measure of hospital performance along a dimension with unambiguous welfare implications. Of course, a limitation of our analysis is that we only know within-hospital mortality, and not 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.

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 and Hamilton (2000), Kessler and Geppert (2005)]. This measure has also recently been proposed by policymakers as a quality metric by which Medicare reimbursement could be tied.[18]

We also consider a number of measures of hospital inputs as dependent variables: the length of stay for the patient; the number of procedures performed while in the hospital; and the number of days between admission and principal procedure. We also explored using total charges incurred to the patient as a measure of total resource

[18] Bernadine Healy, "Health Reform, Too Tough on Hospital Readmission" U.S. News and World Report, May 1, 2009

utilization, but 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 a measure of patient illness severity. Whether such a measure should be included is unclear since severity codes may themselves by impacted by the 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 393,483 hospital/days of data from 289 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 of patients admitted that day that was during the strike. 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 27.5, or approximately 10,038 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 1.035 reflects that hospitals in our sample treat slightly more resource intensive patients than does the average U.S. hospital. The average in-hospital mortality rate is 3.3%, while the average readmission rate (available only post-1995) is 14.2%. Fifty-eight percent of the patients in our sample are female, two-thirds are white, and the average age and number of conditions with which a patient is diagnosed are 44.2 and 3.3 respectively. Four tenths of one percent of patients are admitted during a strike, and roughly the same share of patient stays are exposed to a strike.

Part III: Empirical Strategy

Our basic empirical strategy is to examine the utilization and outcomes in striking hospitals before and after the strike, 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:

$$OUTCOME_{hrd} = \alpha + \beta STRIKE_{hrd} + \gamma PDEM_{hrd} + \delta_h + \eta_d + \mu_y * \sigma_r + \varepsilon$$

In this equation, OUTCOME is one of our measures of outcomes that might be impacted by the strike (average daily mortality or average daily rates of readmission), STRIKE is one of our three measures of strike impact (1/0 measure of whether that day was during a strike, percent of patients admitted that day who are exposed to a strike at some point in their stay, and the percent of the stay of patients admitted that day that occurs during a strike), and PDEM is the mean characteristics of patients admitted that day (case mix index, number of diagnoses, age, share white and share female). We also include a full set of fixed effects for each hospital 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. Finally, we include a full interaction of year dummies with SPARCS region dummies to account for any differential time trends by areas.

With this specification, our identifying assumption is that the only reason for differing 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 and less treatment either in nearby hospitals, or in striking hospitals after the strike has ended. We find evidence for neither.

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 effects 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 0.54% higher than comparable patients admitted before or after the strike. This represents an increase of 16% relative to the baseline mortality rate of 3.4%, a sizeable increase.

The next column adds demographic characteristics, and the results are very similar, with the mortality coefficient rising to 0.55%. The third column in this first panel adds indicators for patient severity, and the result is once again very similar, with a coefficient of 0.59%. This represents an increase of 17% relative to the baseline mortality rate of 3.4%. The coefficients on the severity 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.

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 measure 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. The first three columns of Table 6 therefore replicated our mortality results, including all controls, for that subsample. We once again find sizeable and significant mortality effects. The estimates are somewhat smaller than over the full period, ranging from 0.37% to 0.47%. The mean is somewhat smaller as well, at 2.96%, so this represents a range of 13% to 16% mortality effects, which are comparable to the full sample findings.

The remainder of the table replicates the results from Table 5 for the readmission dependent variable. 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 1.2 to 1.5%, off a base of 14%, so this represents a roughly 10% 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; none of the 20 dummies are significant for mortality, and four are significant for re-admission, two negative and two positive.

The lagged effects of the strike, showing the impact after the strike had concluded, are more mixed. For mortality, there is little lagged effect of strikes; that is, there is no significant effect on those admitted even right after the strike. This suggests that there are no long lasting effects on treatment quality of the strike. But for readmissions the positive impact persists at 11-15 days after the strike, suggesting some longer lasting effects.

Selection Bias Concerns

As noted earlier, a final 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 falls 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 (using our first strike exposure measure, admitted during a strike) where the dependent variable is the mean characteristics of patients admitted that day: average age, gender, race, casemix index, and number of procedures. 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 example, we find that strikes are associated with a -0.018 change in casemix index, off a mean of 1.035. This is a reduction of 1.5%. Given the standard error, this implies that the most case mix could have fallen would be 4.5%, which is very modest given our 10% to 17% outcome effects.

Another way to test for selection is to consider impacts on nearby hospitals. If striking hospitals are admitting only the sickest patients, then nearby hospitals should be admitting somewhat healthier patients. To address this, we next examine 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 choose 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 two key outcome variables, are shown in Table 9. We find that there are actually *positive* effects on nearby hospitals, and in two of the four cases these effects are significant – although considerably smaller than the effect on the striking hospital itself. Thus, if anything, the results suggest that patients are getting *sicker* patients, so that selection is not driving our findings.

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 10 shows our basic results for three measures of treatment intensity: length of stay, number of procedures performed during the stay, and days between admission and principal procedure. Since the results in this section are sensitive to sample period, we show them both for the full sample, and for the post-1994 sample for which re-admission information is available. We just show results for our first strike exposure measure (admitted during a strike); the results for other measures are similar.[19]

For length of stay, we find a positive but insignificant impact over the full period, and an effect which is actually somewhat smaller but is highly significant over the post-1995 sub-period. Clearly there is much more noise in the measure of length of stay in the earlier period which interferes with identifying the strike impact. The later period estimate implies a rise in length of stay of 3.7%.

The results once again differ across time periods for number of procedures. Over the entire period, the estimate is negative but insignificant. But over the post-1995 sample, the estimate is positive and significant. Once again, the estimate in the latter period is much more precise, and we cannot rule out that the coefficients in both periods are the same. The latter period effect is about 3.6% of the baseline, very comparable to the length of stay effect.

The results are more consistent for our third measure of treatment intensity, the number of days between admission and principal procedure. Over both the full period and the sub-period there is a positive and significant coefficient on the strike indicator,

[19] One exception is that the length of stay coefficient is much larger when the exposure measure is share of the stay exposed to a strike, but that could very well be due to endogeneity in that exposure measure in this particular regression.

suggesting that strikes lengthen the span between admission and procedure by 0.1 to 0.17 days. Relative to the means of these variables, that is a rise of roughly 6-9% in this duration.

Thus, while the evidence over the full sample is somewhat weak, the findings suggest that strikes are associated with *more*, and not less, intensive treatment of patients. Thus, the poor outcomes associated with strikes are not due to a lack of treatment intensity. Strikes clearly lower production efficiency in hospitals.

One reason for differing impacts in the post-1995 period may be the use of temporary replacement workers. Many hospitals in New York hire temporary replacement workers to fill in for striking nurses 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. Most strikes that occur during this time period include the use of temporary replacement workers. Of course, it is unclear whether replacement workers can substitute for striking workers. For example, Cramton and Olson (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.

Unfortunately, however, once temporary workers were allowed, the vast majority of hospitals used them. As a result, it is hard to separate temporary worker effects from other changes over time in the impact of strikes. We have attempted to run regressions in the post-1995 period that differentiate strikes using temporary workers from those that do not, and our power was not sufficient to rule out or in a differential impact of temporary workers. Since our mortality results are similar across sample periods, however, the results certainly suggest that at least for this outcome measure, the use of temporary workers did not matter.

Part VI: Conclusions

A long standing concern with strikes as a means of resolving labor disputes is tht 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.

We find that in fact strikes were very costly to hospital patients in New York. We estimate that a patient admitted during a strike is about 0.54 percentage points (17%) more likely to die in the hospital. In our sample, there were 37,534 patients admitted during strikes. Our findings therefore imply that 200 more individuals died because of strikes than would have died had there been no strike. By a similar calculation 560 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; if anything, intensity of care goes *up*

during strikes. So this is very clear evidence of a reduction in productivity; hospitals functioning without their nurses are doing so at a much lower quality of patient care.

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
Distribution of Struck Hospital Size	
(Avg. # of daily admissions 6-months prior to strike)	
5-14 admissions	11
15-29 admissions	10
30-45 admissions	17
45+ admissions	12
Distribution of Facilities Struck Across Regions	
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	27.6	24.0
Daily Case-Mix Index	1.035	0.279
Outcome Variables		
In-hospital Mortality	0.033	0.044
Length of Stay	7.45	4.58
Total Procedures Performed	1.61	0.70
Days Between Admission and Principal Procedure	2.03	2.22
30-day Readmission Rate	0.142	0.079
Patient Characteristics		
Average Age	44.22	11.02
Proportion Female	0.58	0.11
Proportion White	0.67	0.29
Number of Diagnoses	3.29	1.20
Strike Exposure		
Admitted During Strike	0.00398	0.06298
Percent of Patients Admitted Exposed to Strike	0.00484	0.06298
Percent of Patient Stay Exposed to Strike	0.00396	0.05692
Distribution of Admission Type		
Emergency	0.54	0.20
Non-emergency	0.46	0.21

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.

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	0.00535***	0.00547***	0.00590***	0.00477***	0.00504***	0.00540***	0.00657***	0.00626***	0.00685***
	(.00164)	(.00153)	(.00169)	(0.00152)	(0.00142)	(0.00161)	(0.00203)	(0.00193)	(0.00206)
Casemix index	-	-	0.01572***	-	-	0.01569***	-	-	0.01788***
	-	-	(0.00159)	-	-	(0.00158)	-	-	(0.00184)
Avg No. of Diagnoses	-	-	0.00489***	-	-	0.00490***	-	-	0.00495***
	-	-	(0.00048)	-	-	(0.00048)	-	-	(0.00072)
Avg. Age	-	0.00126***	0.00091***	-	0.00126***	0.00091***	-	0.00137***	0.00101***
	-	(0.00003)	(0.00004)	-	(0.00003)	(0.00004)	-	(0.00006)	(0.00005)
			-		-	-		-	-
Avg. Share Female	-	-0.00338***	0.00212***	-	0.00338***	0.00213***	-	0.00545***	0.00411***
	-	(0.00080)	(0.00082)	-	(0.00080)	(0.00082)	-	(0.00128)	(0.00129)
Avg. Share White	-	0.00086	-0.00185	-	0.00085	-0.00188	-	0.00287	0.00003
	-	(0.00137)	(0.00168)	-	(0.00137)	(0.00167)	-	(0.00191)	(0.00220)
N	393483	392157	392145	393483	392157	392145	392157	392157	392145

* 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 Post-1995 Strikes on Outcomes

Dependent Variable	In-Hospital Mortality			30-Day Readmission			30-Day Readmission			30-Day Readmission		
Independent Variable	Indicator for Admitted during strike	Proportion Admitted exposed to strike	Proportion of stay that was during strike	Indicator for Admitted during strike	Indicator for Admitted during strike	Indicator for Admitted during strike	Proportion of stay that was during strike	Proportion of stay that was during strike	Proportion of stay that was during 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)	(10)	(11)	(12)
strike	0.00372*	0.00363**	0.00471**	0.01514***	0.01185***	0.01233***	0.01475***	0.01124***	0.01181***	0.01795***	0.01450***	0.01514***
	(0.00195)	(0.00175)	(0.00236)	(0.00432)	(0.00407)	(0.00395)	(0.00389)	(0.00375)	(0.00361)	(0.00451)	(0.00401)	(0.00381)
Casemix index	0.01918***	0.01918***	0.01757***	-	-	-0.00059	-	-	-0.00059	-	-	-0.00059
	(0.00146)	(0.00146)	(0.00240)	-	-	(0.00171)	-	-	(0.00171)	-	-	(0.00171)
Avg No. of Diagnoses	0.00645***	0.00645***	0.00740***	-	-	0.00893***	-	-	0.00894***	-	-	0.00894***
	(0.00037)	(0.00037)	(0.00066)	-	-	(0.00076)	-	-	(0.00076)	-	-	(0.00076)
Avg. Age	0.00048***	0.00048***	0.00053***	-	0.00220***	0.00168***	-	0.00220***	0.00168***	-	0.00220***	0.00168***
	(0.00004)	(0.00004)	(0.00006)	-	(0.00012)	(0.00013)	-	(0.00012)	(0.00013)	-	(0.00012)	(0.00013)
Avg. Share Female	-0.00013	-0.00013	-0.00019	-	-0.04393***	-0.04327***	-	-0.04393***	-0.04327***	-	-0.04393***	-0.04327***
	(0.00107)	(0.00107)	(0.00162)	-	(0.00384)	(0.00399)	-	(0.00384)	(0.00399)	-	(0.00384)	(0.00399)
Avg. Share White	0.00300**	0.00300**	0.00345	-	0.00440	0.00575	-	0.00438	0.00572	-	0.00439	0.00573
	(0.00148)	(0.00148)	(0.00224)	-	(0.00451)	(0.00475)	-	(0.00451)	(0.00475)	-	(0.00451)	(0.00475)
N	109129	109129	109129	109129	109129	109129	109129	109129	109129	109129	109129	109129

Table 7

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

Dependent Variable	In-Hospital Mortality	30-day readmission
n-20 to n-16	0.00009	-0.00553
	(.00178)	(0.00868)
n-15 to n-11	0.00182	0.00641
	(.0019)	(0.00951)
n-10 to n-6	-0.00218	0.00492
	(.00172)	(0.00739)
n-5 to n-1	0.00031	0.00139
	(.00216)	(0.00758)
strike	0.00593***	0.01268***
	(.0017)	(0.00403)
n+1 to n+5	0.00374	0.01447*
	(.00232)	(0.00754)
n+6 to n+10	-0.00111	0.00656
	(.00206)	(0.00991)
n+11 to n+15	0.00221	0.01347*
	(.00225)	(0.00740)
n+16 to n+20	0.0003	-0.00647
	(.0021)	(0.00686)
Casemix	0.01567***	-0.00066
	(.00158)	(0.00170)
# of Diagnoses	0.00490***	0.00887***
	(.00048)	(0.00076)
Age	0.00091***	0.00168***
	(.00004)	(0.00013)
Female	-0.00215***	-0.04327***
	(.00082)	(0.00400)
White	-0.00185	0.00573
	(.00168)	(0.00474)
N	392145	109129

*** 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 8

Effect of Strikes on Patient Characteristics

Dependent Variable	Age	Share Female	Share White	Casemix Index
	(1)	(2)	(3)	(4)
strike	-0.083	0.008	0.014	-0.018
	(0.592)	(0.005)	(0.013)	(0.018)
N	392,157	393,483	393,483	392,145

*** 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: Excluding Nearby Hospitals

Outcome Measure	**In-Hospital Mortality**		**30-Day Readmission**	
Method	**RADIUS 3**	**FLOW3**	**RADIUS 3**	**FLOW3**
strikeclose	0.00123*	0.00064	0.00334	0.00452***
	(0.00074)	(0.00044)	(0.00284)	(0.00151)
Casemix index	0.01601***	0.01595***	-0.00054	-0.00140
	(0.00163)	(0.00167)	(0.00175)	(0.00184)
Avg No. of Diagnoses	0.00480***	0.00480***	0.00905***	0.00932***
	(0.00050)	(0.00049)	(0.00079)	(0.00079)
Avg. Age	0.00091***	0.00091***	0.00163***	0.00160***
	(0.00004)	(0.00004)	(0.00014)	(0.00004)
Avg. % Female	-0.00213**	-0.00213***	-0.04389***	-0.04391***
	(0.00083)	(0.00083)	(0.00406)	(0.00402)
Avg. % White	-0.00200	-0.00195	0.00605	0.00497
	(0.00172)	(0.00173)	(0.00508)	(0.00516)
N	372820	373268	103772	104220

Table 10

Impact of Strikes on Treatment Intensity

Dependent Variable	Length of Stay		Number of Procedures		Days Between Admission and Principal Procedure	
Strike Sample Period	Full	Post-1995	Full	Post-1995	Full	Post-1995
	(1)	(2)	(3)	(4)	(5)	(6)
strike	0.292	0.232***	-0.054	0.062**	0.166**	0.105**
	(0.226)	(0.082)	(0.061)	(0.027)	(0.076)	(0.053)
Casemix index	2.194***	2.596***	0.4640***	0.6221***	0.7789***	0.9493***
	(0.199)	(0.122)	(0.0319)	(0.0197)	(0.0799)	(0.0604)
Avg No. of Diagnoses	0.779***	0.726***	0.1744***	0.1392***	0.1854***	0.2533***
	(0.106)	(0.038)	(0.0223)	(0.0187)	(0.0260)	(0.0156)
Avg. Age	0.0578***	0.0068	-0.0022*	-0.0069***	0.0284***	0.0079***
	(0.0057)	(0.0043)	(0.0012)	(0.0012)	(0.0021)	(0.0016)
Avg. Share Female	-0.1054	0.0520	-0.0728**	-0.0026	0.0002	0.0873*
	(0.1803)	(0.1127)	(0.0302)	(0.0204)	(0.0520)	(0.0510)
Avg. Share White	0.5499	-0.2915**	0.0963	0.0011	-0.3630***	-0.1124
	(0.3482)	(0.1415)	(0.0801)	(0.0332)	(0.0903)	(0.0757)
N	392,145	109,129	392,145	109,129	372693	103,248

* 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.