An Original Study

Hip Fracture Outcome: Is There a "July Effect"?

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Abstract

We assessed the differential complications and mortality rates of teaching versus nonteaching hospitals in July against other month-to-month differences in a cohort of 324,988 elderly patients hospitalized for a femoral neck or intertrochanteric fracture (data taken from the 1998–2003 National Inpatient Sample). Demographics were similar between teaching and nonteaching hospitals and across admission months. The overall mortality rate was 3.64% and was slightly higher in teaching hospitals compared with nonteaching hospitals (3.69% vs. 3.61%, relative risk [RR] = 1.0062, 95% CI 0.99-1.02). The adjusted relative risk (RR) for mortality in July/August was significantly higher than the overall adjusted RR and compared with all other month pairs, indicating higher in-hospital mortality rates in teaching hospitals compared with nonteaching hospitals. Intraoperative complications and length of stay were statistically significantly greater in teaching hospitals but did not demonstrate a "July effect." Teaching hospitals had lower perioperative complication rates.

Elderly hip fracture patients treated at teaching hospitals had 12% greater relative risk of mortality in July/August (ie, experience a "July effect") compared with nonteaching hospitals during that time period (1998-2003). Although various methods exist for exploring the "July effect," it is critical to take into account inherent month-to-month variation in outcomes and to use nonteaching hospitals as a control group.

The "July effect" is the hypothesized negative effect that new and inexperienced staff members have on patient outcomes. Explicitly, as July brings the transition of medical students to interns, the transition of interns to residents, and the arrival of new staff, the additional responsibilities of new personnel in July may be accompanied by decision-making errors secondary to inexperience and inefficiency. The literature on this topic is scant and controversial. Although several authors have reported no difference in outcomes, including mortality,[1-8] others have reported differences supporting the July effect relative to increased costs, longer hospital stays, and higher complication rates.[1,3,9-11] However, no study has demonstrated differential complications and mortality rates of teaching versus nonteaching hospitals in July that stand out in comparison with other month-to-month differences.

We conducted a study to determine whether the July effect occurs when junior residents and new orthopedic staff participate in surgical and perioperative care for a common orthopedic condition. Specifically, we examined whether complication rates for hip fracture patients, including mortality rates, were higher in July and August than in other months in teaching hospitals versus nonteaching hospitals. Within an orthopedic context, hip fracture patients were considered an ideal population for studying this effect because the diagnosis confers considerable morbidity and mortality, and often the patients themselves are fragile. Therefore, if inexperience has an effect, then these patients are most likely susceptible.

Patients and Methods

Data Source

For this study, we used the 1998–2003 National Inpatient Sample (NIS), databases designed to approximate a stratified sample of US hospitals and weighted to be nationally representative. A complete description of NIS is available at the Healthcare Cost and Utilization Project Web site.[12]

Target Population

Patient inclusion criteria were age 65 or older and hospitalization for a femoral neck or intertrochanteric fracture. Exclusion criteria were lack of a valid admission month and failure to code teaching hospital status, in-hospital mortality, length of stay (LOS), or sex.

Outcomes and Prognostic Factors

Hip fracture outcomes were studied at teaching and nonteaching hospitals during July and August by examining in-hospital mortality, intraoperative and perioperative complication rates, LOS, and hospital charges.

As the primary research question concerned increased risk during the transition period for new medical personnel, and as this period usually occurs in July and August, admission month was the primary prognostic factor. It was grouped into six 2-month categories *(January/February,*

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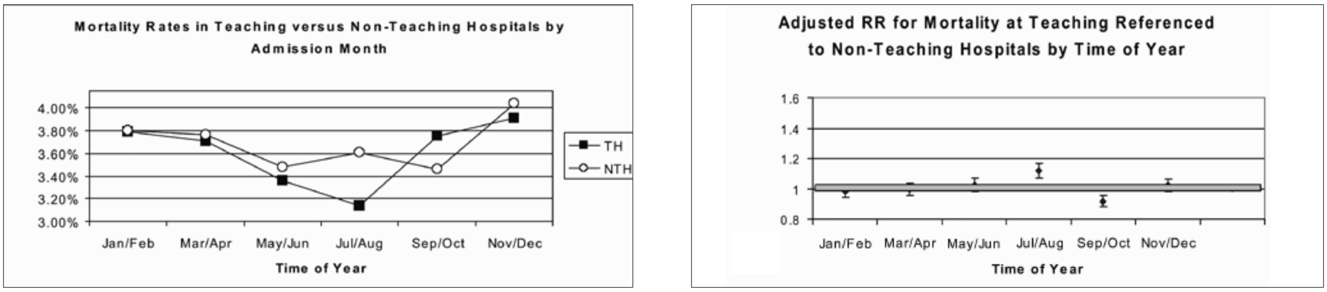

Figure. (A) Mortality rates for teaching (TH) and nonteaching hospitals (NTH) by admission month. (B) Adjusted relative risk (RR) of mortality rates for teaching and nonteaching hospitals by admission month.

March/April, May/June, July/August, September/October, November/December) to allow for better comparison of our results with those of other studies performing similar analyses[13] and to test the possible impact of new orthopedic fellows who begin in August every year.

The second primary prognostic factor was hospital teaching status, coded within NIS as *nonteaching* or *teaching*. Potential confounders considered were patient age (continuous variable), sex, and comorbidity status based on *ICD–9 (International Classification of Diseases, Ninth Revision)* codes using the Deyo-Charlson Comorbidity Index.[14] Race was not included as an adjusting variance because several states did not allow NIS coding to include race. It was unclear from the database whether the 87,993 patients (37% of the sample) missing race data were from states that do not allow race data to be captured or if the data were simply omitted.

Statistical Methods

All outcomes were analyzed using generalized linear modeling, specifying the Poisson distribution and using the log link. This modeling approach produces variable weights that reflect relative risks (RRs). RR is the ratio of the probability of an event in one population compared with that in another. Our null hypothesis was that there would be no difference in outcomes between teaching and nonteaching hospitals in July and August. Specifically, for all outcomes, the interaction of admission months and hospital teaching status was evaluated for evidence of differential outcomes in the July/August interval compared with any and all other month pairs. Furthermore, this interaction was explored with and without adjustments for the other prognostic factors, including patient age, sex, comorbidity status, admission year, and hospital region, regardless of the overall significance of the interaction. All analyses were conducted with the weighting variable provided in the NIS database to check for differences in observed profiles.

LOS and charge data were dichotomized by splitting results at the 75th percentile, and complications were dichotomized according to presence or absence. Hospital charge data for each admission year was converted to 2003 dollars based on Consumer Price Index adjustments.[15] Thus, model estimates of RRs reflect risks for in-hospital morality, higher incidence of intraoperative and medical complications, long LOS, and high hospital charges.

All statistical analyses were performed with SAS software (version 9.1.3; SAS Institute, Cary, NC) on a Windows XP Professional platform. Probability values were 2-tailed, and type I error rate was set at 0.05.

Results

Cohort Extraction

The 1998–2003 NIS database included records for 44,751,708 patients. Of these, 372,729 met the initial inclusion criteria (age 65 or older, hip fracture). Exclusion criteria resulted in a final cohort of 324,988 (87% retained). The most common reason for exclusion was lack of a valid admission month (44,656) followed by failure to code in-hospital mortality (2631), teaching hospital status (358), LOS (53), and sex (41).

Sample Characteristics

Demographics of the cohort and the hospitals are summarized in the Table. Most patients were female (76%) and white (66%). Mean patient age was 82.5 years (SD, 16.4 years). Mean Deyo-Charlson Comorbidity Index score was 0.987, with almost half (46.2%) of the patients in the sample having no comorbidities reported. About as many patients were admitted year to year with the diagnosis of hip fracture. Hospital demographics indicated that approximately 38% of hospitals were teaching hospitals.

In general, the pattern of demographics was similar between teaching and nonteaching hospitals and across admission months. The 2 exceptions were race and distribution of teaching hospitals. Whites were more likely to be treated at nonteaching hospitals, and blacks were more likely to be treated at teaching hospitals ($P<.0001$). The proportion of teaching hospitals was higher in the Northeast and Midwest than in the South and West ($P<.0001$).

Outcomes: Mortality

In general, in-hospital mortality rates associated with hip fracture surgery were low. The cohort's overall mortality rate was 3.64%. Although month-to-month mortality rates varied, July/August rates were either the lowest or second lowest over the year. In-hospital mortality rates were slightly higher in teaching hospitals than in nonteaching hospitals, but the difference did not reach statistical significance (3.69% vs 3.61%; RR, 1.0062; 95% CI, 0.99-1.02).

Overall, the adjustment effects on the RRs were small. As expected, regarding each prognostic variable, mortality rates were significantly higher for men, patients with a higher number of comorbidities, and increasingly older patients.

The Figure (A) displays the in-hospital mortality rates in teaching and nonteaching hospitals by admission month pairs and (B) summarizes the adjusted RRs for these rates. In-hospital mortality rates for teaching hospitals compared with nonteaching hospitals for patients admitted during July/August were statistically higher than the baseline difference between the 2 types of hospitals.

In contrast, the adjusted RR for September/October was significantly lower than the overall adjusted RR. This was the only incidence of lower in-hospital mortality rates for teaching hospitals compared with nonteaching hospitals. With the exception of these 2 admission intervals, in no other admission periods were adjusted RRs outside the 95% CI for the overall adjusted RR comparing teaching and nonteaching hospitals regardless of admission month.

Complications

The overall intraoperative complication rate was 0.73%. The rate was significantly higher in teaching hospitals (0.81%) than in nonteaching hospitals (0.68%) (RR, 1.2; 95% CI, 1.16-1.25) but did not reveal a relationship between admission month and teaching status.

Perioperative complication rates, the percentages of patients having at least 1 perioperative complication, were 41.6% and 42.2% in teaching and nonteaching hospitals, respectively. Overall, teaching hospitals had a lower adjusted RR (0.98; 95% CI, 0.975-0.985) of perioperative complications, but it did not reveal a relationship between admission month and teaching status.

Length of Stay and Hospital Charges

The efficiency-of-care endpoints were LOS and hospital charges. The division for the upper quartile for LOS was 9 or more days. Mean LOS was 7.2 days (SD, 6.9 days; range, 0-365 days). Adjusted RR of long LOS in teaching versus nonteaching hospitals was 1.1 (95% CI, 1.09-1.2). There was no July effect demonstrated.

The division between the highest quartile and the three lowest quartiles for hospital charges was $27,000. Mean charges were $23,484 (SD, $27,432; range, $33.00-$1,070,000). The adjusted RR of high charges in teaching versus nonteaching hospitals was 1.29 (95% CI, 1.28-1.30). The July/August RR did not differ significantly from the overall adjusted RR between hospital types.

LOS differed by months statistically but not clinically. LOS was significantly (P<.05) shorter for July/August (8.32 days) than for January/February (8.39 days) and November/December (8.46 days) and was significantly (P<.05) longer for teaching hospitals (8.56 days) than for nonteaching hospitals (8.15 days).

Hospital charges ranged from $31,281 to $33,022 across the months. Although they were significantly different from charges for all other month pairs, July/August charges fell in the middle of this range, at $32,535. Hospital charges were significantly (P<.05) higher for teaching hospitals ($34,324) than for nonteaching hospitals ($30,112).

Table. Weighted Summary Statistics Describing Patient and Hospital Variables in Cohort

	Hospital Type		
	Teaching (n = 119,792)	Nonteaching (n = 205,196)	All (N = 324,988)
Patient Variables			
Age, mean (SD)	82.42 (16.75)	82.54 (16.24)	82.49 (16.43)
Percentage male	24.11	24.52	24.26
Comorbidites[a]	0.994 (3.36)	0.987 (3.28)	0.987 (2.28)
Race (%)			
White	62.92	67.66	65.88
Black	3.84	1.91	2.64
Hispanic	2.18	2.15	2.16
Asian	1.19	0.90	1.00
Other	1.34	1.11	1.20
Not coded[b]	28.54	26.27	27.12
Year admitted (%)			
1998	18.13	16.11	16.87
1999	17.90	16.22	16.85
2000	15.56	18.09	17.14
2001	16.17	17.21	16.82
2002	16.32	16.07	16.17
2003	15.92	16.30	16.16
Total	37.63	62.37	100.00
Hospital Variable			
Region (%)			
Northeast	27.55	18.80	22.09
Midwest	33.07	26.71	29.11
South	24.13	30.25	27.85
West	15.26	24.24	20.86

[a]Deyo-Charlson Comorbidity Index. [b]Race not coded for hospitals in several states.

Discussion

In July/August, elderly patients with hip fractures treated surgically at teaching hospitals had a 0.42% increased risk for in-hospital mortality compared with similar patients treated at nonteaching hospitals (3.5% vs 3.92%; adjusted RR, 1.12).

Interestingly, despite slightly higher mortality rates at teaching hospitals, July/August rates were generally the lowest rates across the months. Although the lower rate for both types of hospitals in July/August does not fit the traditional definition of the July effect, the increased RR for mortality in teaching compared with nonteaching hospitals does. In our opinion, this is the appropriate definition of the "July effect" and is no less compelling in the face of lower overall in-hospital mortality rates during July and August. The other outcomes, including intraoperative and perioperative complications, LOS, and hospital charges, did not demonstrate patterns consistent with the July effect.

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Our study found that intraoperative complication rates were higher at teaching hospitals. These rates varied, with teaching hospitals sometimes having the higher and sometimes the lower complication rates. Adjusted rates were not specifically higher in July/August. Similarly, Banco and colleagues[2] were unable to show a difference in spine surgery complication rates relative to new fellows' and residents' rotations. However, they analyzed rates only within a teaching hospital. On one hand, their findings are commendable, as they were derived from a prospective single-center collection of spine infection rates; on the other hand, they may be limited, as they included only 1330 surgeries over 4 years and may reflect the complicated nature of some spine procedures, which, as in the study by Smith and colleagues,[13] may not be affected by junior residents to the same extent as hip fracture care.

Perioperative complications were more common than intraoperative complications but did not differ between teaching and nonteaching hospitals. This finding may reflect the fact that the multidisciplinary care that most patients receive for hip fractures may be less susceptible to individual physicians' lack of experience. We could not find other research that specifically addressed perioperative complications in this setting.

Teaching hospitals demonstrated longer LOS and higher charges compared with nonteaching hospitals throughout the year. This finding agrees with findings from other studies.[3,10] The small difference has been detected in other large studies, such as the one conducted by Barry and Rosenthal.[3] Smaller studies, however, have failed to show a difference.[5-7]

In our study, mean LOS was half a day longer for teaching hospitals than for nonteaching hospitals but did not demonstrate the July effect. Longer LOS at teaching hospitals may have contributed to higher charges. We did not specifically analyze LOS for patients who died during hospitalization, so it is unclear if the slightly higher mortality rates for teaching hospitals are consistent or inconsistent with longer LOS and higher charges.

Several authors have found academic care to be associated with higher charges.[10,16] Rich and colleagues[10] further examined this phenomenon and found that charges decreased throughout the year as a function of resident experience. Other researchers have found trends toward higher charges at teaching hospitals, but these trends did not reach significance.[6] Higher charges have been predominately attributed to additional diagnostic tests and longer LOS. However, other authors have found no difference in charges.[3,5,13] It would be reasonable to assume that junior physicians, erring on the side of caution or academic protocol, may order confirmatory tests or additional imaging studies that experienced surgeons in nonteaching settings may feel are superfluous. From our data, it is unclear why these charges continue to increase for both teaching and nonteaching hospitals throughout the year. As we were not able to eliminate this trend by adjusting the charge data to 2003 dollars using the Consumer Price Index, this may represent the predominant national trend of health care spending outstripping general inflation. In addition, nonteaching hospitals may have had other explanations for lower charges, such as more effective cost-containment strategies.

Choice of Methods

Methodologically, evaluating for the July effect by looking at month-to-month variation in teaching hospitals assumes that month-to-month variability in outcomes of interest, such as in-hospital mortality, is independent of admission time. The data in this study challenge the validity of this assumption. Specifically, as illustrated in the Figure (B), month-to-month variation for in-hospital mortality was not random, and, in fact, a general trend toward lower in-hospital mortality in the May/June and July/August admission month pairs was observed. Furthermore, even if the assumption of independence of outcomes with admission time does generally hold, observation of a "spike" in mortality in July for teaching hospitals may or may not reflect the July effect because the lack of a control group, such as nonteaching hospitals, is not available to rule out the rival hypothesis that admissions in July over the course of time in which the data were gathered happened to be associated with higher mortality compared with other months. This is no different than the well-accepted understanding that case study designs provide less compelling information than case–control designs do.

Previous research on the July effect is varied in its methodology.[2,4,5,7,10] Barry and Rosenthal[3] compared teaching and nonteaching hospitals in July through September compared with other admission months. For patients in intensive care units (ICUs), they found a small decrease in LOS in nonteaching hospitals compared with teaching hospitals but no difference in mortality. Rich and colleagues[10] studied teaching hospitals only and concluded that, over the academic year, cost of care and LOS decreased as house staff experience increased, while mortality and other quality outcomes did not change. Finally, other investigators have assessed results with both methods but have favored the teaching-hospital-only analysis.[13] For example, Smith and colleagues[13] examined the interaction between teaching status and admission month and concluded there was no July effect. However, they admitted that the neurosurgical procedures that they examined may have not been amenable to junior resident participation, thus limiting the ability to detect the effect.

Importance of Cohort

Smith and colleagues[13] were appropriately concerned about selecting the appropriate cohort for study of the July effect. Elderly patients with hip fractures are the ideal population susceptible to the July effect because they are generally a medically and skeletally fragile population that is often exposed to junior resident evaluation and treatment. In studying an at-risk cohort, we increased our chances of detecting the July effect, if it did indeed exist. Less serious conditions are likely less at risk for adverse outcomes at the hands of inexperienced housestaff and therefore would not demonstrate the July effect. In addition, the large size of this database decreased the probability of type II error by missing small effect size.

Overall, the linear model appears consistent with previous research that has identified male patients, older patients, and patients with more comorbidities to be at higher risk for death after hip fracture.[17,18]

Limitations

This study has the limitations characteristic of retrospective data and the problems inherent in using databases developed independently of the specific questions being researched. As in most database analyses, users cannot independently verify the accuracy of the data, its standardization, or its input. Retrospective studies often do not capture some important data elements specific to the research question. For example, neither severity-of-fracture data nor mechanism-of-injury data were collected in the database. Likewise, small errors that reflect a resident's learning curve but that do not result in patient harm may not be discernible. In addition, for complicated patients with numerous medical comorbidities, only 15 *ICD–9* codes are captured in the database. This may cause primary comorbidities to be recorded instead of complications of hip fracture treatment. Moreover, timing of diagnosis is impossible to determine from the database. For example, patients who were admitted to the hospital with pneumonia and fell and sustained a hip fracture there would be counted as having a complication of hip fracture, rather than vice versa. However, in our experience, the total proportion of these patients is small. In further consideration of comorbidities, it is unclear if teaching hospitals' case mix is comparable with nonteaching hospitals', even after adjusting for multiple factors. For example, between teaching and nonteaching hospitals there may be differential emphasis and awareness of comorbidity coding for similar patients, which directly result in higher reimbursement. Reporting bias with regard to complications is ubiquitous but may differ between teaching and nonteaching hospitals, which may also affect our conclusions.

An additional concern is that, though we selected a particular subgroup of patients with a condition for which the care is fairly uniformly operative, we assumed that junior residents would participate in the care in both the operating room and over the perioperative period, which may vary from institution to institution. Finally, the database determines teaching status based on the presence of absence of any residency program but is not specific to orthopedic surgery residencies. However, this effect may be mitigated by the fact that hip fracture care involves multidisciplinary care, which reflects the quality of both medical and surgical care on the patient's outcome. However, as we included hospitals without orthopedic surgery residents among teaching hospitals, the July effect that we identified would be more pronounced if the hospitals had been accurately categorized as nonorthopedic teaching hospitals. Fortunately, the large size of this study and the national character of the database used make our results widely generalizable, which is not the case with previous studies limited to a specific ICU, hospital, or geographic region.

Given the results of this study, we believe that the July effect exists. Interestingly, this effect was characterized by the fact that nonteaching hospital mortality rates in the summer were lower than average, and not by the fact that teaching hospital mortality rates were higher. These results highlight the importance of cohort selection, large samples sizes gathered over multiple years, and appropriate methodologic design to evaluate for the July effect.

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This paper will be judged for the Resident Writer's Award.