COHORT TURNOVER AND PRODUCTIVITY: THE JULY PHENOMENON IN TEACHING HOSPITALS

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March 2014

ABSTRACT

We consider the impact of *cohort turnover*—the planned simultaneous exit of a large number of experienced employees and a similarly sized entry of new workers—on productivity in the context of teaching hospitals. Specifically, we examine the impact of the annual July turnover of residents in American teaching hospitals on levels of resource utilization and quality in teaching hospitals relative to a control group of non-teaching hospitals. We find that, despite the anticipated nature of the cohort turnover and the supervisory structures that exist in teaching hospitals, this annual cohort turnover results in increased resource utilization (i.e., longer length of hospital stay) for both minor and major teaching hospitals, and decreased quality (i.e., higher mortality rates) for major teaching hospitals. Particularly in major teaching hospitals, we find evidence of a gradual trend of decreasing performance that begins several months before the actual cohort turnover and may result from a transition of responsibilities at major teaching hospitals in anticipation of the cohort turnover.

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We thank Jay Bhattacharya, David Cutler, Arnold Epstein, William Evans, LeRoi Hicks, Barbara McNeil, Paul Oyer, Douglas Staiger, and seminar participants at the University of Pennsylvania, Harvard Business School, Stanford Graduate School of Business, the 14th Annual Health Economics Conference, the National Bureau of Economic Research's Summer Institute, the 2013 INFORMS Healthcare Conference, and the 2013 INFORMS Annual Meeting for helpful comments on an earlier version of this manuscript. We also thank Juliana Pakes for information regarding the Charlson Index and Sarah Woolverton for expert coding assistance. We acknowledge financial support from the Division of Research and Faculty Development at the Harvard Business School.

1. INTRODUCTION

Nearly all managers must deal with the consequences of employee turnover within their organizations. Despite the importance of this issue, several authors have observed that academic attention has been disproportionately focused on the *causes* rather than *consequences* of turnover (Glebbeek and Bax 2004, Mobley 1982, Staw 1980). One possible explanation for the limited number of studies on the effects of turnover is the difficulty in answering this question empirically. Turnover is an endogenous phenomenon that may occur for a wide variety of reasons that are not observed by the researcher. For example, more productive workers may be more likely to remain with a given company longer than less productive ones (Jovanovic 1979). Under such circumstances, it is difficult to make causal inferences concerning turnover's effect on productivity and performance using firm-level data. Those studies that do examine the consequences deliver mixed findings. While some find that employee turnover exhibits a negative effect on performance through lower levels of productivity (Batt 2002), smaller profit margins (Ton and Huckman 2008), and worse customer service (Kacmar et al. 2006, Ton and Huckman 2008), others find that the effect of turnover on performance depends on contextual factors such as how knowledge is embedded in an organization's structure (Hausknecht and Holwerda 2013, Rao and Argote 2006) or the degree of process conformance (Ton and Huckman 2008).

A second issue concerning the effect of turnover on firm performance is that turnover itself appears in multiple forms. Many firms face a continuous stream of *individual turnover* in which employees leave and are replaced by new workers at various points throughout the year. In such settings, there is no one particular time during the year when managers are required to train and orient a large portion of their workforce. In contrast, other firms bring on new employees in large numbers at discrete points in the year. For example, law and consulting firms tend to start most of their new employees in late summer or early fall. These new employees must all be trained and integrated into the firm at one time. In the law and consulting examples, the potential negative effects of the large inflow of new workers may be buffered by the fact that firms do not face the simultaneous exit of large portions of their experienced workers. Rather, these departures occur in a roughly continuous manner throughout the year.

An extreme, though not uncommon, form of this discrete scenario is what we term *cohort turnover*—the planned simultaneous exit of a large number of experienced employees and a similarly sized entry of new workers—and serves as the focus of this study. Cohort turnover is related to, but distinct from, *collective turnover*, which refers to the aggregate departure of employees within an entity regardless of whether the departure was planned in advance or accompanied by a similarly sized entry of newcomers (Hausknecht and Trevor 2011). Examples of cohort turnover can be found in changeovers that occur between military units in combat, political administrations,[1] and residents and fellows in teaching hospitals. Given the number of individuals transitioning either into or out of employment at a specific point in time, cohort turnover raises concerns about adverse effects on productivity due to factors such as operational disruption (Krueger and Mas 2004) or the loss of the tacit knowledge (Polanyi 1966) held by departing workers. However, because cohort turnover is, by definition, anticipated and those in supervisory roles typically remain in place, one might expect cohort turnover to not have a negative impact on productivity. In short, the impact of cohort turnover on productivity is not obvious on an *a priori* basis.

In this paper, we consider cohort turnover among house staff (i.e., residents) in teaching hospitals. Residency represents a new physician's first assignment following medical school and typically lasts from three to five years depending on the physician's area of specialization.[2] In certain specialties, residency is followed by a one- or two-year fellowship, during which the doctor receives further training in a sub-specialty. At (or slightly before) the beginning of every July, the most senior residents move on to permanent medical positions or fellowships at other hospitals, and recent medical school graduates arrive as first-year residents, also known as interns. Every summer, this turnover leads to a discrete reduction in the average experience of the labor force at teaching hospitals. In addition, this changeover may disrupt established teams of doctors and other caregivers within hospitals. Although most attending physicians who supervise residents typically remain constant at this time of year, either of these effects may have potentially troubling consequences for the two major determinants of hospital productivity—resource utilization (a proxy for cost) and clinical outcomes (a measure of quality).

This cohort turnover, colloquially referred to as the "July phenomenon," is often mentioned in the lore of medical professionals. Many physicians have, perhaps jokingly, counseled patients not to get sick in July. As of yet, the clinical literature presents an inconclusive picture of whether the July phenomenon exists, and if so, for which outcomes. As we discuss below, several of these studies are limited either by relatively small sample sizes or issues concerning their empirical strategy for identifying effects.

We examine the impact of the July turnover on hospital productivity using data on all patient admissions from a large, multi-state sample of American hospitals over a 16-year period. By comparing trends in teaching hospitals to those for non-teaching (i.e., control) hospitals over the course of the year, we find significant negative effects of the residency turnover on hospital efficiency (as measured by risk-adjusted, average length of stay (LOS)). For the most part, these effects are increasing in the degree to which a hospital relies on residents (as measured by the number of residents per hospital bed). We find a

[1] For example, Boylan (2004) examines turnover in the United States Attorney's Office and finds that 40% of turnover between 1969 and 1999 occurred during a President's first year in office.
[2] Residency for some surgical specialties lasts up to seven years.

similar negative effect on clinical quality (as measured by risk-adjusted mortality rates) in major teaching hospitals but not in minor teaching hospitals. Interestingly, we also find evidence of an anticipation effect in major teaching hospitals, which begins as early as the March prior to a given cohort turnover in July and is suggestive of a transition of responsibilities in the last several months of the academic year at hospitals with the highest levels of teaching intensity.

2. EMPLOYEE TURNOVER AND PRODUCTIVITY

As noted by several authors (Glebbeek and Bax 2004, Mobley 1982, Staw 1980), there is significantly more literature examining the causes, rather than consequences, of turnover. Further, the literature that *does* address the consequences of turnover is split over the direction of these effects.

In a balanced review of the potential consequences of turnover, Staw (1980) notes several theoretical, negative effects of turnover on organizations. These include selection, recruitment, and training costs for replacement workers, operational disruption, and demoralization of remaining workers. In a sample of nearly 1,000 firms, Huselid (1995) found a negative relationship between turnover and productivity (measured by sales per employee) and corporate financial performance (as measured by Tobin's *q*).

One explanation for the hypothesized negative effect of turnover on productivity and performance is that job exits interfere with learning by individuals or teams. Several studies find that worker productivity improves with experience (Hellerstein and Neumark 1995, Levhari and Sheshinski 1973, Maranto and Rodgers 1984, Newell and Rosenbloom 1981). While experience with a given firm likely leads to higher productivity, the converse may also be true—more productive workers may be less likely to leave a firm (Jovanovic 1979). Further, Price (1977) notes that younger workers exhibit higher rates of turnover than older workers. To the extent that instances of turnover are not randomly distributed across workers with different levels of underlying productivity, the turnover of individual employees may be an endogenous event.

To address this issue, several studies have used levels of union presence as a proxy for the workforce stability that firms experience in the absence of turnover. Brown and Medoff (1978) suggest that this stability accounts for some portion of the positive relationship they observe between unionization and productivity. Similarly, Clark (1980) finds a positive relationship between unionization and productivity, though he suggests that additional evidence is required to establish the degree to which this relationship is explained by lower turnover. Freeman and Medoff (1984) provide a summary of the factors—including, but not limited to, lower turnover—that may explain this relationship.

While these negative effects have received significant attention, several studies discuss turnover as a contingent, or even positive, phenomenon.[3] For example, Jovanovic (1979) presents turnover as a key element in the process of improving matches between employers and employees over time. Staw (1980) describes performance as a function of both skill and effort. As a result, in many settings—particularly high-stress professions that may lead to employee burnout—the relationship between tenure and performance may assume an inverted-U shape. Employee skill will initially increase faster than effort decreases; as burnout begins, however, effort will decline faster than skill improves. Assuming that performance follows an inverted-U shape over time, turnover may thus improve average performance. Beyond the replacement of less productive with more productive workers, turnover of poor performing employees may also serve to improve the morale and motivation of workers who remain with the firm (Staw 1980).

The replacement of low performing workers and the increased motivation of remaining employees both suggest that turnover could have particularly strong positive effects in settings requiring substantial levels of innovation and adaptation (Dalton and Todor 1979, Mobley 1982, Staw 1980). In support of this claim, experimental work by Argote et al. (1995) finds that while turnover has a negative effect, on average, on group performance, this effect is more pronounced for simple tasks than for complex activities requiring innovation. Argote and Epple (1990), however, also find that turnover does not appear to have a negative effect on firm productivity in settings where work is relatively standardized, as the knowledge required to perform a task is codified and can be easily transferred to new workers. Similarly, Ton and Huckman (2008) find that increasing turnover does not negatively impact store performance high process conformance settings where employees follow standardized policies and procedures, whereas there exists a negative effect of turnover in low process conformance settings. Together, these studies suggest that tasks requiring intermediate levels of innovation may be the most susceptible to performance declines following turnover.

In addition to task characteristics, several other factors may affect the degree and direction of turnover's impact on performance. These include the degree of hierarchy within an organization (Carley 1992), whether turnover itself is voluntary or involuntary (Price 1977), whether turnover occurs in a predictable manner (Staw 1980), and the absolute level of turnover in the organization. With respect to this final factor, some have suggested that the relationship between turnover and performance exhibits an inverted-U shape with a medium level of turnover being preferred to both low and high levels (Abelson and Baysinger 1984).

[3] Dalton and Todor (1979) and Staw (1980) provide reviews of both the positive and negative effects of turnover on performance.

Beyond its mixed findings, much of the prior work on turnover either explicitly or implicitly considers only individual turnover; significantly less attention has been devoted to cohort turnover, the phenomenon we consider in this paper. There are arguably two analogs to cohort turnover that have been considered in the literature. One is the turnover of management teams (e.g., Tushman and Rosenkopf 1996). Much of the research literature on the turnover of management teams focuses on the *determinants* of such activity (e.g., Fee and Hadlock 2004, Hayes et al. 2005), thereby treating turnover as the dependent, rather than independent, variable. It is not clear, however, how one should expect findings on management turnover to generalize to the performance of line workers. While not a direct study of cohort turnover among line workers, Krueger and Mas (2004) find evidence that a period of significant labor unrest—including a strike and the large-scale use of replacement workers—in a Firestone tire plant was associated with reduced product quality. The effects identified in their work speak to the impact of labor unrest on performance, but do not separate the effects of worker discontent from the impact of the cohort turnover that occurs when striking employees are replaced.

Another analog to cohort turnover is the recent conceptualization of *collective turnover*, which refers to the aggregate departure of employees within an entity regardless of whether the departure was planned or accompanied by a similarly sized entry of newcomers (Hausknecht and Trevor 2011). This construct has yet to be studied empirically, though one conceptual paper has suggested five characteristics that may moderate the relationship between collective turnover and performance: leaver proficiencies, time dispersion, positional distribution, remaining member proficiencies, and newcomer proficiencies (Hausknecht and Holwerda 2013).

One reason for the lack of attention to cohort turnover may be the fact that it occurs less frequently than individual turnover. Nevertheless, it takes place in several important settings including military deployments, changes of political administrations, labor strikes, and, of course, annual house staff turnover in teaching hospitals. A second reason for the lack of attention placed on cohort turnover may be the assumption that the answers to questions concerning its effects are obvious—given the sheer magnitude of the change it brings, cohort turnover *must* have a detrimental impact on performance. Despite the magnitude of cohort turnover, however, it often occurs in a predictable fashion and the affected organizations should, theoretically, have time to anticipate and prepare for its occurrence. For example, attending physicians in teaching hospitals—being aware of the turnover that occurs each July—may focus intently on supervising new residents at that time of the year. As a result, the impact of worker turnover in settings where the formal and informal supervisory staff (in this case, attending physicians and non-physician clinical staff, such as nurses and technicians) does not change is not obvious on an *a priori* basis.

Perhaps the most attractive empirical characteristic of cohort turnover is that it typically occurs for exogenous reasons that are independent of the performance of individual workers. Such changeovers occur as a matter of policy, as is the case with teaching hospitals where the annual turnover takes place regardless of the underlying productivity of the physicians and hospitals involved. In contrast, instances of individual turnover may occur due to particular characteristics of the departing or entering worker—such as motivation or ability—that remain unobserved by the researcher and may bias statistical estimates of turnover's impact on performance.

3. COHORT TURNOVER IN TEACHING HOSPITALS

It is widely agreed that teaching hospitals have at least two primary objectives—the provision of high quality medical care and the training of new doctors. These related but distinct objectives overlap within medical residency programs. Medical school graduates in the United States apply for residencies at any of the roughly 1,000 teaching hospitals in the country (Association of American Medical Colleges 2009). Depending on a physician's specialty, residencies typically last for three to five years, during which time residents represent an important piece of a hospital's system for delivering care.

Patient care in teaching hospitals is provided by teams of medical professionals that include attending physicians, fellows, residents, and medical students. Much of the care for patients is provided by a resident, who supervises medical students and is supervised by an attending physician with or without additional supervision from a more senior resident. The daily activities of residents include admitting, treating, and discharging patients. In some departments, fellows (i.e., physicians who have completed their residencies and are training in a sub-specialty) may provide an intermediate level of supervision between residents and attending physicians.

Residency programs in the United States are structured like schools. Each class of residents enters together at the beginning of the academic year, and the senior members of the program all graduate together at the end of the academic year. For the vast majority of residency programs, the year officially begins on July 1st and ends the following June 31st. The annual transition, however, does not occur all on one day. Typically, hospitals complete the transition over a two-to-three week period, lasting from the middle of June through the first week of July.

This changeover creates the potential for turmoil in teaching hospitals as each cohort of doctors becomes comfortable with new roles and responsibilities. With respect to the changeover, Claridge et al. (2001) note, "During this time of year, there is clearly a feeling of apprehension among providers of health care, as well as among many patients." Gawande (2002) echoes these concerns:

> In medicine we have long faced a conflict between the imperative to give patients the best possible care and the need to provide novices with experience. Residencies attempt to mitigate potential harm through supervision and graduated responsibility…But there is

> still no getting around those first few unsteady times a young physician tries to put in a central line, remove a breast cancer, or sew together two segments of colon. No matter how many protections we put in place, on average these cases go less well with the novice than with someone experienced.

These anecdotal observations suggest the need for systematic analysis of the implications of this annual turnover for medical productivity.

Until recently, most of the medical literature on staffing and performance in teaching hospitals dealt with issues concerning limitations on resident work hours (Gaba and Howard 2002, Laine et al. 1993, Leach 2000, Steinbrook 2002, Thorpe 1990, Weinstein 2002) or differences in outcomes on weekends and weekdays (Bell and Redelmeier 2001, Dobkin 2003, Hendry 1981)—two periods when the average level of on-duty-physician experience is expected to differ substantially. In the past five years, the medical literature has started to pay more attention to the July phenomenon.

The medical literature to date on the July phenomenon presents mixed findings. Several studies show that patients who are admitted in July have similar mortality outcomes (Alshekhlee et al. 2009, Bruckner et al. 2008, Ehlert et al. 2011, Englesbe et al. 2009, Inaba et al. 2010, McDonald et al. 2013, Schroeppel et al. 2009, Smith et al. 2006, van Walraven et al. 2011) and morbidity outcomes (Ehlert et al. 2011, Ford et al. 2007, McDonald et al. 2013, Smith et al. 2006) when compared to patients presenting in other months. Yet, other studies show the opposite, suggesting that patients exhibit worse outcomes in July when measured in terms of mortality rates (Anderson 1990, Dasenbrock et al. 2012, Englesbe et al. 2007, Jen et al. 2009, Shuhaiber et al. 2008), morbidity (Anderson 1990, Dasenbrock et al. 2012, Englesbe et al. 2007, Haller et al. 2009), or medication error rates (Phillips and Barker 2010). Young et al. (2011) note that much of this literature exhibits methodological limitations that render the findings inconclusive.

Collectively, this literature faces two categories of limitations, one methodological and the other contextual. In terms of methods, many studies do not adjust for risk, adjust for variation by season of the year, or use suitable concurrent controls. All of these are necessary adjustments given the observational design of the studies that test for the July phenomenon. For example, one study claims to reject the existence of a July phenomenon in obstetrics at teaching hospitals serving Medicaid patients across the country (Ford et al. 2007). That paper compares patient outcomes at teaching hospitals in July with those in the rest of the year and does not identify any significant differences between the two periods. Given their study design, however, the authors are unable to control for seasonal variations in patient outcomes that could affect outcomes at all hospitals regardless of teaching status. For example, as we will illustrate later, patients admitted to hospitals in the winter have higher mortality rates than those admitted in the summer. Without some baseline, such as a control group of non-teaching hospitals, to adjust for exogenous changes in patient outcomes, a comparison of outcomes at two different times of the year may be confounded by these omitted variables.

With respect to context, most of these prior studies examine the presence of the July phenomenon either within a single hospital site or region or for a specific category of high-risk patients (e.g., very low weight infants, ICU patients, spinal surgery patients, trauma patients) (Barry and Rosenthal 2003, Jena et al. 2013). While this focus on a narrow study population helps reduce patient heterogeneity, it may limit the generalizability of the research findings to the extent that residents' level of responsibility for treatment may vary across clinical areas, particularly early in their post-graduate training. For example, Barry and Rosenthal (2003) test for the July phenomenon in a sample of 28 hospitals in northeast Ohio. Similar to our approach, they compare teaching and non-teaching hospitals in terms of LOS and risk-adjusted mortality, though they focus solely on patients in intensive care units (ICUs). They do not find evidence of the July phenomenon in this population, but do note that their findings may not generalize to non-ICU patients. Specifically, they suggest that, given the severity of patients in this setting, ICU residents may receive higher levels of supervision than their non-ICU counterparts.

Further, these studies aim to explain outcomes in specific clinical areas, which are typically measured at the department level, as a function of teaching intensities that are based on hospital-level figures for the number of residents per bed. Given that a hospital's teaching intensity might vary across specific clinical areas (e.g., a hospital may have a more substantial surgical residency than radiology residency), it is not clear that there is always a "match" in the level at which the dependent and key independent variables are observed.

In this paper, we address these methodological and contextual limitations by using a large national sample of discharge-level data from both teaching and non-teaching hospitals. As we discuss in the following section, we employ a multivariate analysis with a difference-in-differences framework to identify relative changes in risk-adjusted length of stay and risk-adjusted mortality over the course of the year. This approach allows us to separate changes that are driven by the cohort turnover in July from those that are driven by other unobserved factors that are also present in non-teaching hospitals.

We begin our examination of the impact of the July turnover on hospital productivity by comparing trends in non-teaching hospitals to those in teaching hospitals. On one hand, we expect that the turnover of resident staffs will have a negative impact on hospital productivity, given the preponderance of evidence suggesting a negative relationship between individual turnover and productivity (Huselid 1995, Staw 1980); if individual turnover has a negative effect on productivity, we would expect that the turnover of a large cohort of individuals would amplify the magnitude of this negative effect. On the other hand, because the turnover of resident staffs is a predictable phenomenon that is planned far in advance, we also expect that teaching hospitals may be working to minimize these negative effects on productivity. For example, teaching hospitals may increase the level of supervision of resident staffs at the time of the cohort turnover. Taken together, we think both of these effects may exist concurrently but expect that the negative effect dominates on balance. Thus, we hypothesize the following:

Hypothesis 1: Cohort turnover will negatively impact the productivity of teaching hospitals compared to non-teaching hospitals.

Next, assuming an effect of cohort turnover exists, we use a measure of teaching intensity that varies across teaching hospitals to examine whether the effects of cohort turnover are greater at hospitals with a higher level of teaching "intensity" as measured by the number of residents per hospital bed. While these hospitals may be more attune to the potential negative consequences of turnover, they would also be the ones to feel the effects of turnover more prominently given the greater magnitude of the turnover itself. Therefore, we hypothesize:

Hypothesis 2: Cohort turnover will more negatively impact the productivity of teaching hospitals with a higher level of teaching intensity than those with lower teaching intensity.

4. DATA

The primary source of data for this analysis is the Healthcare Cost and Utilization Project (HCUP) Nationwide Inpatient Sample (NIS) for each year from 1993 to 2008.[4] NIS contains discharge-level data for all inpatient cases at a sample of roughly 20 percent of the community hospitals[5] in the United States. Depending on the year, NIS includes information for hospitals from between 17 and 42 states (Agency for Healthcare Research and Quality 2013).

For each patient, NIS provides information on patient age and gender, admission source, expected primary payer (i.e., Medicare, Medicaid, private including HMO, self pay, no charge, and other), length of stay (LOS), total charges, and in-hospital mortality. In addition, NIS includes detailed data on a patient's principal and secondary diagnoses and principal and secondary procedures.

We link the NIS data with information from the AHA Annual Survey of Hospitals, which includes data on the operating and financial characteristics for nearly all of the more than 5,000 hospitals

[4] The NIS database is administered by the Agency for Healthcare Research and Quality (AHRQ), previously known as the Agency for Health Care Policy and Research (AHCPR).

[5] The NIS definition of "community hospital" is the same as that used by the American Hospital Association (AHA): "…'all nonfederal, short-term, general, and other specialty hospitals, excluding hospital units of institutions.' Included among community hospitals are specialty hospitals such as obstetrics-gynecology, ear-nose-throat, short-term rehabilitation, orthopedic, and pediatric. Excluded are long-term hospitals, psychiatric hospitals, and alcoholism/chemical dependency treatment facilities" (Healthcare Cost and Utilization Project 1999).

in the United States. In addition to several other items, the AHA data provides information on the number of hospital beds and full-time residents (including interns) at each facility in a given year. Using this information, we are able to construct our measure of teaching intensity—full-time residents per hospital bed.

Our final sample of facilities is limited to those that appear in both the NIS and AHA databases. The appendix presents the number of hospitals that appear in our sample and in the NIS by year. For each year and state, the table provides the number of hospitals appearing in the NIS and in our matched NIS-AHA sample. Most of the discrepancies between the matched sample and the NIS are due to the fact that certain states opted not to provide the hospital identifiers required to match NIS and AHA data in certain years. In other rare cases, a hospital may appear in the NIS but not the matched sample because that facility did not appear in the AHA data for a given year.

5. EMPIRICAL METHODOLOGY

5.1. Hospital Categories

The source of identification in our empirical analysis is the varying degree to which certain types of hospitals rely on residents. Initially, we divide hospitals into two categories—non-teaching hospitals and teaching hospitals. We then subdivide teaching hospitals into two categories: minor teaching hospitals and major teaching hospitals. Non-teaching hospitals are those that are not listed as teaching hospitals in the NIS. These facilities have very few, if any, residents. As such, we would not expect them to be affected by the cohort turnover in July. Minor teaching hospitals are those that are listed as teaching hospitals in the NIS data and have resident intensities (i.e., full-time residents per inpatient hospital bed) that are less than 0.25. Major teaching hospitals are those facilities listed in the NIS as teaching hospitals and with teaching intensities equal to or greater than 0.25. This threshold for resident intensity is used by the Medicare Payment Advisory Commission (MedPAC) to distinguish minor and major teaching facilities (Medicare Payment Advisory Commission 2002). We ran a version of our analysis using Jena et al.'s (2013) higher threshold of 0.60 residents per bed as the boundary between minor and major teaching hospitals. Due to the small percentage of hospitals (1.77% of total hospitals) that are considered major teaching hospitals under this definition, we maintained MedPAC's 0.25 threshold for major teaching hospitals. We note, however, the lack of substantive difference in our main findings when using either of these threshold values.

Figure 1 illustrates that, in the aggregate, risk-adjusted LOS and risk-adjusted mortality vary quite substantially throughout the calendar year. For example, risk-adjusted mortality is relatively high in the winter months of December, January, and February. Between February and December, mortality declines until the summer months before increasing during the fall. This pattern has been noted by epidemiologists (e.g., Gemmell et al. 2000) and has been attributed to a range of factors including the impact of seasonal disease (e.g., influenza and respiratory illness) and weather. Similarly, risk-adjusted LOS also shows seasonal patterns. Key to the empirical strategy in our paper is the use of non-teaching hospitals as a control for these seasonal changes in outcomes, which should affect all hospitals regardless of teaching status. We can thus calculate "de-seasoned" trends in LOS and mortality for teaching hospitals to determine the extent of potential effects around the July turnover.

Table 1 presents descriptive statistics for each of the three hospital categories as well as for the entire sample. The first row illustrates the differences in average teaching intensity across the three groups. This average measure increases from 0.01 for non-teaching facilities to 0.07 and 0.56 for minor and major teaching hospitals, respectively. We note that the average teaching intensities of non-teaching and minor teaching hospitals are quite similar while that of major teaching hospitals is substantially larger than that for either of the other two categories. In terms of both measures of facility size—hospital beds and admissions per year—hospitals get progressively larger as the level of teaching intensity increases. Teaching intensity is also correlated with the demographics of a hospital's patient base. In particular, non-teaching hospitals attract older patients than either type of teaching hospital, possibly due to the differences in location of the different types of hospitals, on average. The average age for patients at non-teaching facilities is 49.1 versus 45.6 and 42.7 for minor and major teaching hospitals, respectively. In addition to having younger patients, the major teaching hospitals in our sample also have a higher percentage of Medicaid patients than the other groups. Moving from non-teaching to minor teaching to major teaching, this percentage increases from 16% to 17% to 25%. These relationships are consistent with the fact that many teaching hospitals are located in densely populated cities.

Table 1 also presents information on the risk-adjusted average length of stay (LOS) and mortality rate for each type of hospital. In terms of risk-adjusted average LOS, we find that resource utilization increases with teaching intensity. This trend is consistent with the claim that major teaching hospitals tend to attract the most complex cases among the three groups. For risk-adjusted mortality, we find that the mortality rate is higher in minor and major teaching facilities (2.3%) compared to non-teaching facilities (2.1%), which is consistent with the earlier claim that teaching facilities attract the more complex cases.

5.2. Empirical Specification

Our multivariate analyses rely on a difference-in-differences framework that follows the relative changes in risk-adjusted LOS and risk-adjusted mortality for the different groups of hospitals over the course of the year. To test our hypotheses, we estimate each of the following models:

$$Y_{h,m,t} = \alpha_h + \delta_t + \mu_m + \beta_1 \cdot TCH_{h,m,t} + \sum_{m=1}^{12} \beta_{2m} \cdot \left(\mu_m \times TCH_{h,m,t}\right) + \varepsilon_{h,m,t} \qquad (1)$$

$$\begin{aligned} Y_{h,m,t} = {} & \alpha_h + \delta_t + \mu_m + \beta_1 \cdot MIN\_TCH_{h,m,t} + \beta_2 \cdot MAJ\_TCH_{h,m,t} + \\ & \sum_{m=1}^{12} \beta_{3m} \cdot \left(\mu_m \times MIN\_TCH_{h,m,t}\right) + \sum_{m=1}^{12} \beta_{4m} \cdot \left(\mu_m \times MAJ\_TCH_{h,m,t}\right) + \varepsilon_{h,m,t} \end{aligned} \qquad (2)$$

In both models, Y represents the dependent variable of interest (i.e., risk-adjusted average LOS or risk-adjusted mortality), α_h is a vector of hospital fixed effects, δ_t is a vector of year fixed effects, and μ_m is a vector of fixed effects for each month of the year. Given that the residency changeover begins in late June for many hospitals, we compare the change in the dependent variable from May to July for teaching hospitals to the similar change for non-teaching hospitals to measure the impact of the July turnover.[6]

In model (1), *TCH* is an indicator for teaching hospitals. The next term on the right-hand side of (1) is a vector of interactions between *TCH* and the month effects. Thus, the coefficients on the month-*TCH* interactions capture the extent to which any seasonal pattern that is found for teaching hospitals differs from that for the non-teaching controls. In model (2), *MIN_TCH* and *MAJ_TCH* are indicators for minor and major teaching hospitals, respectively.[7] The next two terms on the right-hand side of (2) are vectors of interactions between the two teaching hospital categories and the month effects. Here, the coefficients on the *MIN_TCH* (*MAJ_TCH*) interactions capture the extent to which any seasonal pattern that is found for minor (major) teaching hospitals differs from that for non-teaching controls. Each of the observations in (1) and (2), respectively, is weighted by the total number of cases for the hospital-month pair to account for the fact that all of the dependent variables are averages. Finally, the standard errors are clustered by hospital to address potential lack of independence in the error term, $\varepsilon_{h,m,t}$.

5.3. Risk-Adjustment of Dependent Variables

As suggested in Table 1, the average severity of patients likely differs across the different types of hospitals. To the extent that the differences in patient severity for major teaching, minor teaching, and non-teaching hospitals vary systematically over the course of the calendar year, risk adjustment is required to ensure proper identification of any July phenomenon. For example, to the degree that, within

[6] Due to the fact that the residency changeover begins in third and fourth weeks of June at several hospitals, LOS and mortality results for that month represent a mixture of outcomes from both before and after the transition. We thus use the comparison of May to July to measure the July phenomenon. This difference captures the change in the dependent variables from the month that precedes the beginning of the changeover for *any* hospital to the month of the cohort turnover for the vast majority of teaching hospitals.

[7] Despite the inclusion of hospital fixed effects, the non-interacted coefficients on minor (β_1) and major (β_2) teaching status are identified by those facilities that change between minor and major teaching status across years. For example, a hospital may have a teaching intensity of 0.25 (making it a minor teaching hospital) in one year and 0.30 (making it a major teaching hospital) in the next.

older populations, relatively healthy individuals may move from cold climates in northeastern states—which tend to have a high concentration of teaching hospitals—to warmer southern and western states during the winter months, the age-adjusted mortality risk for the hospitalized population in the northeast will increase *ceteris paribus* during this period of the year.

The covariates in our risk-adjustment equation are patient age; age squared; gender; diagnosis-related group (DRG); an indicator for Medicaid as the primary payment source; and the Charlson index. The DRG captures the patient's main diagnoses and procedures as assigned by the Centers for Medicare & Medicaid Services. The Medicaid variable is included as a proxy for the patient's socioeconomic status.[8] The Charlson index is a measure of comorbidities that increase a patient's risk of mortality (Charlson et al. 1987).

Given that the in-hospital mortality variable is binary, we use logistic regression to obtain estimated probability of death for each patient discharge. For LOS, we use a simple linear regression to calculate predicted values. The risk-adjustment equations are run separately for each calendar year. The observed and expected values for mortality and LOS are then averaged by hospital and month. The risk-adjusted value of each dependent variable is calculated as the ratio of the observed-to-expected rate for a given hospital-year. For example, the risk-adjusted length of stay ($RALOS_{h,m,t}$) is:

$$RALOS_{h,m,t} = \frac{OLOS_{h,m,t}}{ELOS_{h,m,t}} * \overline{OLOS_t} \qquad (3)$$

where $OLOS_{h,m,t}$ and $ELOS_{h,m,t}$ are the observed and expected LOS, respectively, for hospital h in month m of year t. $OLOS_t$ is the average mortality rate for the entire sample in year t and is used simply to normalize the value of $RALOS_{h,m,t}$.

6. RESULTS AND DISCUSSION

6.1. Base Results

Table 2 presents results from our estimation of (1), the basic regression comparing trends in non-teaching hospitals to those in teaching hospitals. The coefficients in this table represent the change in the dependent variable for all teaching hospitals relative to the change for non-teaching hospitals over the same period. As noted earlier, we use the month just prior to the beginning of the resident turnover (May) as the baseline. A positive coefficient indicates that, on average, teaching hospitals experience a larger

[8] With linear and quadratic terms for patient age included in the regression, we do not include a separate term for Medicare status. While it would be useful to include an indicator for HMO patients—who may be healthier, on average, than patients in other payer categories—the HCUP data does not distinguish HMO patients from those with other forms of private insurance (e.g., indemnity).

increase in the outcome measure than do non-teaching hospitals over the same period of time. For example, in Column 1, the value of 0.041 for the September coefficient for the teaching hospitals suggests that the *change* in LOS from May to September is 0.041 days greater for teaching hospitals than for non-teaching hospitals. Similarly, the December coefficient for the same group suggests that the change in LOS from May to December is 0.028 days greater for teaching hospitals than for non-teaching hospitals.

For our purposes, the coefficients of greatest interest are those for July, when the vast majority of teaching hospitals experience turnover of their residents. In terms of LOS (Column 1), the July coefficient for teaching hospitals is 0.042 and is significant at the 1% level. To provide a perspective on the magnitude of this effect, we note that the average risk-adjusted LOS for teaching hospitals is 5.18 days. If we assume that LOS is proportional to hospital costs, these results suggest that costs increase by roughly 0.8% in teaching hospitals following the July turnover.

The estimated coefficients on LOS for teaching hospitals exhibit an overall, yet non-continuous, decline in magnitude during the months from July to December and remain significantly different from the May baseline. By January, LOS falls back to its value in the baseline May period. Nevertheless, the coefficients for August, September, October, November, and December are not each significantly different from that for July, so we are not able to reject the hypothesis that LOS for teaching hospitals increases in July and remains at that higher level for the final five months of the calendar year. Though none of the coefficients for January, February, March, and April are significantly different from that for May, each is significantly lower than each of the July, August, September, October, November, and December coefficients at the 2% level. This general reduction in the estimated coefficient over the course of the academic year suggests that house staffs at teaching hospitals may benefit from experience-based improvement in performance in terms of resource utilization, as measured by LOS.

The fact that LOS, and, in turn, resource utilization, increases following the July turnover does not provide conclusive evidence concerning its effects on medical productivity. To address this issue, one also needs to consider the impact of turnover on medical quality. Column 2 of Table 2 presents results from our estimation of (1) using risk-adjusted mortality—a proxy for quality—as the dependent variable. Here, we find that teaching hospitals do not experience a significant change in their risk-adjusted mortality rates in July. However, we do find evidence of learning over the course of the academic year. Although the levels for July through December are not significantly different from the May baseline, each of the coefficients for January through April is significantly lower than each of those for July through October at the 5% level. Thus, we find partial support for Hypothesis 1, as the cohort turnover of resident staffs in July is associated with negative effects on productivity driven by increased resource utilization (as measured by LOS) with no significant change in quality (as measured by mortality).

In addition, we find suggestive evidence of an anticipation effect that begins earlier in the calendar year than the actual cohort turnover in July. Specifically, after a period of general decline in LOS from July though March, LOS begins to increase gradually in April before reaching its peak in July. During this period, there is a temporary flattening of the trend from May to June that is statistically distinguishable (at the 5% level) from the change between June and July. Similarly, after a period of continuous decline in relative mortality at teaching hospitals from August through February, we find that mortality begins to increase gradually starting in March and peaking in August. The magnitude of each month-to-month change from February to March, March to April, April to May, and May to June is not statistically distinguishable from that between June and July, suggesting that the increase in mortality may be beginning before the actual cohort turnover in July. Given the relatively smooth and continuous nature of this pattern over the course of first half the year—where an initial increase in LOS or mortality is not followed by a subsequent decrease until after the July turnover—we believe that the anticipation effect could be related to the July turnover rather than resulting from a random event.

Though our data do not allow for conclusive explanations, anecdotal evidence suggests that the negative effects on LOS and mortality at teaching hospitals during this anticipatory period may be explained by either or both of the following: (1) a gradual transition to greater responsibility in the last several months of the academic year for those residents who will remain at a given hospital during the next year to prepare for the upcoming cohort turnover in July or (2) a decline in performance at teaching hospitals as senior residents or fellows "wind down" their appointments and become involved with the process of finding new positions at other hospitals. With respect to the first explanation, some residency programs start giving first-year residents (i.e., interns) greater responsibility beginning in March or April, such as by making them responsible for holding the admission pager. This may involve the intern being the first to triage a new patient, assuming the care of patients coming from the emergency department, and initiating the diagnostic workups on patients. Unlike earlier in the academic year, these tasks are now completed with less oversight from second-year residents. Whether due to a transition of responsibility or a decline in performance, it seems as if the changes that are commonly attributed to the period immediately following the July turnover may actually impact performance during several months on either side of the actual transition date. That is, any observed July phenomenon may begin before July due to anticipation of the annual turnover.

In Table 3, we present results from our estimation of (2), the basic regression using three discrete categories of teaching status**.** The coefficients in this table represent the change in the dependent variable for minor and major teaching hospitals relative to the change for non-teaching hospitals over the same period, using May as the baseline month. These results are presented graphically in Figures 2a and 3a for the change in the risk-adjusted length of stay and risk-adjusted mortality, respectively, for minor and major teaching hospitals relative to the change for non-teaching hospitals.[9]

In terms of LOS (Column 1), the July coefficient for minor teaching hospitals is 0.030 and is significant at the 1% level. Given that the average risk-adjusted LOS for minor teaching hospitals is 5.11 days, these results suggest that costs increase by roughly 0.6% in minor teaching hospitals following the July turnover. The estimated coefficients on LOS for minor teaching hospitals exhibit an overall, yet non-continuous, decline in magnitude during the months from July to December and remain significantly different from the May baseline, although the coefficients from August through December are each not significantly different from that for July. By January, LOS falls back to its value in the baseline May period, and the coefficients from January through April are each significantly lower than each of the July, August, September, November, and December coefficients at the 2% level. This general reduction in the estimated coefficient over the course of the academic year is suggestive of experience-based improvement in performance in terms of resource utilization, as measured by LOS.

Consistent with the view that the July phenomenon should increase with the intensity of a hospital's teaching program, we find that, relative to minor teaching hospitals, major teaching facilities show even stronger evidence of such a trend in LOS. Specifically, these hospitals experience a positive and significant (at the 1% level) increase in LOS relative to non-teaching hospitals following the July turnover, and the effect remains for approximately six months. The magnitude of this effect (0.067) is more than twice that for minor teaching hospitals (0.030) and is significantly different from the minor teaching coefficient at the 3% level. This effect for major teaching hospitals represents a 1.2% increase relative to the average LOS for such facilities (5.43 days). As with minor teaching hospitals, the effects for August through December are significantly different from the May baseline and decline in estimated magnitude over time. Again, however, these coefficients are not statistically distinguishable from the July estimate.[10] By January, LOS falls to the point where it is insignificantly different from May, but significantly lower than each of the July through December coefficients at the 1% level. These results provide additional support for the contention that house staffs learn over the course of the academic year.

In Column 2 of Table 3, we consider the impact of the July turnover on risk-adjusted mortality. Though minor teaching hospitals do not experience significant changes in mortality during the course of the academic year, major teaching facilities do show evidence of a July phenomenon with respect to this

[9] In Figures 2b and 3b, we plot the risk-adjusted length of stay and risk-adjusted mortality, respectively, by hospital type (non-teaching, minor teaching, and major teaching). Note that these two figures represent the change in the dependent variable for all hospital types relative to the May baseline only, and *not* relative to the change for non-teaching hospitals.

[10] We note, however, that the coefficient for July is significantly higher than that for December at the 13% level. This provides additional suggestive evidence of learning over the course of the academic year.

outcome measure. As with LOS, major teaching hospitals experience an increase in their risk-adjusted mortality rate in July. This increase of 0.048 percentage points is significant at the 5% level. The magnitude of this July phenomenon represents a 2.1% increase relative to the average mortality rate of 2.30% for major teaching hospitals. Evidence of learning is again present in the coefficients for the remainder of the academic year. Although the levels for August through December are not significantly different from the May baseline, the level for November and December are each significantly lower than that for July at the 3% level. In addition, each of the coefficients for January through April is significantly lower than each of those for August through October at the 1% level, which is suggestive of learning over the course of the academic year. Thus, we find support for Hypothesis 2, in which the cohort turnover of resident staffs in July most negatively impacts the productivity of teaching hospitals with the highest levels of teaching intensity.

For major teaching hospitals, we again find suggestive evidence of a turnover anticipation effect. After a period of continuous decline in mortality from August through February, we find that mortality begins to increase gradually starting in March and peaks in July. The change from February to March is not statistically distinguishable from the change between June and July, suggesting that the increase in mortality may be beginning before the actual cohort turnover in July. Similarly, after a period of continuous decline in LOS from August through March, LOS begins to increase gradually in April before peaking in July, though the pre-July trend is not as pronounced for LOS as it is for mortality.

6.2. Extensions and Robustness

Though suggestive of declines in medical productivity following cohort turnover, our results are potentially consistent with alternate explanations. Therefore, we examine the robustness and extension of our results through several additional analyses.[11]

6.2.1. Testing for Patient Self-Selection and Increased Transfers

One alternate hypothesis is that elective patients may recognize July to be a time of turmoil for teaching hospitals and may decide to avoid those facilities at that time of the year. Under the reasonable assumption that these elective patients tend to be healthier than those who lack choice regarding their admission to the hospital, this self-selection by patients (on dimensions that are potentially unobservable

[11] In addition to the analyses presented in this section, we also considered evaluating the robustness of our findings by assessing the relative changes in risk-adjusted LOS and mortality in various subsets of the data (e.g., medical versus surgical patients, patients with high-mortality diagnoses such as acute myocardial infarction or stroke). However, because the AHA data only provides residency data at the hospital level and not at the level of specific diagnoses or clinical specialties (e.g., cardiology residents or orthopedic surgery residents), we are not able to accurately "match" a hospital's teaching intensity in a specific area with its risk-adjusted performance in that same area.

to researchers) could thus leave teaching hospitals with relatively sicker patient populations at precisely the time we estimate their resource use to be increasing and their outcomes to be declining. If such selection were occurring, we would be mistaken to assume that the effects we observe were simply due to a decline in productivity.

We offer two tests of the selection hypothesis in Columns 1 and 2 of Table 4. First, if elective patients are, in fact, selecting away from teaching hospitals in July, teaching hospitals should experience a decline in their number of admissions relative to non-teaching facilities in July. Second, if the selection away from teaching hospitals leaves them with relatively sicker patient populations in July, teaching hospitals should experience an increase in the expected mortality relative to non-teaching facilities in July. We estimate two separate regressions of the same form as (2) but with the number of hospital admissions and expected mortality, respectively, as the dependent variable.

For the first regression with the number of hospital admissions as the dependent variable (Column 1), the results are mixed. In particular, the July coefficient for minor teaching hospitals is negative, though its magnitude is quite small (11 fewer admissions in July) and represents a decrease of only 0.85% relative to the average of 1,291 admissions per month for minor teaching hospitals. Meanwhile, the July coefficient for major teaching hospitals is positive (4 more admissions in July), though it is not statistically significant at conventional levels. For the second regression with the expected mortality as the dependent variable (Column 2), we do not find significant evidence of patient selection in July as measured by a change in expected mortality, though the data suggests a slight relative increase in expected patient mortality in August and September. Together, these results suggest no clear connection between changes in expected LOS or expected mortality around the July turnover and admission patterns at the hospitals experiencing those changes. In short, there is no clear evidence of patient selection away from the teaching hospitals that appear most affected by the July phenomenon.

Our analysis of admissions is related to a second potential explanation for our base results: that major teaching hospitals are receiving a higher percentage of patients transferred from minor teaching or non-teaching hospitals during the summer months due to potential excess hospital capacity during warmer months. To the extent these transfer patients are relatively sick compared to those typically seen at teaching hospitals in warmer months, one might expect both LOS and mortality to increase at the teaching hospitals receiving them. In Column 3 of Table 4, we thus repeat our analysis using the percentage of cases transferred from another hospital as the dependent variable. We do not find a systematic change in transfer rates for either minor or major teaching hospitals around the July turnover.

6.2.2. Results for Patients Admitted from the Emergency Department

Although we do not find strong support for the selection hypothesis, we conduct an additional analysis to further examine the robustness of our base results. We do this by limiting our sample to only the inpatient cases that arrive through the emergency department (ED) and are less likely to involve a patient choosing among hospitals. These cases are thus arguably less susceptible to endogeneity concerns than elective cases scheduled in advance.[12] Further, ED cases are more serious on average than those that do not enter through the ED, resulting in higher average values for LOS and mortality (bottom two rows of Table 5) than for the overall population that includes elective patients (bottom two rows of Table 3). As a result, we would expect the magnitude of the coefficients for July to be even greater than those from our base results.

We estimate a regression of the same form as (2) on this subset of cases. The results are similar to our base results and are presented in Table 5. In terms of risk-adjusted LOS, the July coefficients for minor and major teaching hospitals are 0.040 and 0.126, respectively, both of which are significant at the 1% level and are statistically significantly different from each other. These findings are consistent with our hypothesis that there exists a negative impact of July turnover on productivity that is increasing in teaching intensity. In addition, in line with our expectations, each of these coefficients is greater in magnitude than those from our base results. The estimated coefficients on LOS exhibit a general decline in magnitude during the months from July to December, which is suggestive of learning over the course of the academic year. In the months preceding the July turnover, we find a slight anticipation effect in minor teaching hospitals that begins in April (significant at the 10% level), though we do not find statistically significant evidence of a turnover anticipation effect in major teaching hospitals. We speculate that major teaching hospitals may not experience as much of a turnover anticipation effect when restricted to this subset of cases because the higher underlying mortality of this subpopulation dampen the increase in LOS typically found during this period leading up to the July turnover. This reasoning would also suggest that the July coefficient for this subpopulation may be an underestimate of the true effect of the cohort turnover.

The results for risk-adjusted mortality are also consistent with our base results. We find no strong evidence of a July effect or an anticipation effect in minor teaching hospitals. However, in major teaching hospitals, we find strong evidence for both. The July coefficient for risk-adjusted mortality in major teaching hospitals is 0.108, which represents a 3.2% increase relative to the average mortality rate of 3.42 for major teaching hospitals. The estimated coefficients on mortality exhibit a general decline in magnitude during the months from July to February, after which there is a gradual increase until peaking in July. This is suggestive of both learning over the course of the academic year, and a turnover anticipation effect that begins in the months preceding the actual cohort turnover in July.

[12] Each year, 36% to 48% of inpatient cases arrive through the ED.

6.2.3. Results Using Alternate Model Specifications

One potential concern with our base specification is that is relies on observations at the hospital-month level. Given that mortality is a relatively rare event in most hospitals, the use of monthly mortality rates at the hospital level may result in a "noisy" measure of performance. To address this possibility, we modify our basic specification to include a vector of fixed effects for seven multi-month periods during the year as opposed to each month of the year. We specify these multi-month periods as January through February, March through April, May, June, July through August, September through October, and November through December. As in our original specification, we isolate June because the residency changeover begins in late June for many hospitals. We present these findings in Table 6.

Our findings highlight the robustness of our base results. Specifically, we find evidence of a substantial increase in LOS at teaching hospitals in the period just following the resident turnover. The July-August coefficients for minor and major teaching hospitals are 0.029 and 0.065, respectively, and both are significant at the 1% level. The magnitude of this effect increases with the hospital's teaching intensity, with the July-August coefficient for major teaching hospitals being significantly different from the minor teaching coefficient at the 2% level. This increase in LOS is sustained through the rest of the calendar year before it returns to April-May levels in the period of January through February. We again observe a slight anticipation effect in the major teaching hospitals, where there is an increase in LOS between the March-April period and the May baseline. With respect to mortality, we find no evidence of a significant change in the July-August period at minor teaching hospitals. We do, however, observe an increase in mortality in the July-August period in major teaching hospitals (a magnitude of 0.041 percentage points that is significant at the 6% level) and find evidence of an anticipation effect that begins in the March-April period.

Because LOS and mortality could also be affected by the extent to which a hospital is crowded (i.e., operating at a higher capacity), we also estimate a model that accounts for hospital crowding. Because our empirical model accounts for hospital fixed effects, we assess whether a hospital is more or less crowded compared to its average level by using the number of total admissions in a month. In addition, we include the squared term of the total number of admissions in a month as an additional covariate to account for potential non-linearity in the effect of hospital crowding on LOS and mortality. We find our base results to be robust to this alternate specification as well.

Finally, we estimate a model for risk-adjusted mortality that accounts for observed LOS as an additional covariate. This addresses the fact that patients who have a longer LOS are at greater risk—all else equal—of dying in the hospital. This could be an issue if unobserved heterogeneity remains despite current risk-adjustment. The regression results show a significant positive association between observed

LOS and risk-adjusted mortality, suggesting that those patients who stay longer in the hospital are also more likely to die during their stay. We note, however, that our base results of interest are largely robust to this alternate specification. Although the significance level of the July coefficient for risk-adjusted mortality in major teaching hospitals reduces from 5% to 8%, the magnitude of the coefficient remains similar (0.042 as opposed to 0.048). The magnitude and level of significance of all other coefficients of interest remain robust to this specification.

7. CONCLUSION

We examine the relationship between cohort turnover and productivity using a unique setting in which turnover occurs exogenously—the July turnover of house staffs in teaching hospitals. Overall, we find that the cohort turnover of resident physicians in teaching hospitals reduces medical productivity by increasing resource utilization and, to a lesser degree, decreasing quality.

Given that minor teaching hospitals exhibit levels of teaching intensity (0.06 residents per bed) that are much more comparable to those of non-teaching hospitals (0.01 residents per bed) than of major teaching hospitals (0.56 residents per bed), we find that it is important to distinguish major teaching hospitals from minor teaching hospitals in determining whether the July turnover affects hospital productivity. Comparing all teaching hospitals to non-teaching hospitals, we find that the cohort turnover negatively impacts the productivity of teaching hospitals by significantly increasing resource utilization—as measured by average LOS—but does not increase the rate of mortality when compared to non-teaching hospitals. We then leverage the variation in teaching intensity across teaching hospitals by comparing trends in non-teaching hospitals to those in minor versus major teaching hospitals. We find that both minor and major teaching hospitals experience a significant increase in resource utilization immediately following the July turnover, and that those effects are increasing in teaching intensity. In contrast to our findings with all teaching hospitals grouped together, we find that teaching hospitals with a higher level of teaching intensity experience a significant decrease in quality in July.

Interestingly, we find that major teaching hospitals seem to experience a gradual increase in resource utilization and a concurrent decrease in quality in the months leading up to the July turnover. This pre-July trend in major teaching hospitals suggests an anticipation effect that may be due either to the institution proactively planning for the upcoming July transition or individuals winding down their current duties and becoming involved in the process of transitioning (e.g., interviews, site visits) to new positions at other hospitals. The implication for managers and patients is that the potential decline in performance associated with the July turnover is not simply limited to the period following that turnover; it may, in fact, begin during the quarter of the year immediately preceding the turnover. That said, the performance of major teaching hospitals relative to non-teaching facilities is at its lowest level in July.

The magnitude of the estimated July effect is substantial and its effect on resource utilization appears to last for roughly six months. We find that average LOS—our proxy for resource utilization and cost—for the average major teaching hospital increases by roughly 1.2% following the July turnover and remains between 0.7% and 1.2% higher throughout the final six months of the calendar year. In addition, the average major teaching hospital experiences an increase in risk-adjusted mortality of roughly 2.1% (percent, not percentage points) in July. Determining the social cost of this increase in mortality requires assumptions about the expected longevity and quality of life of these individuals in the absence of the July turnover. Such assumptions are beyond the scope of this paper.

One question that is not resolved by our study is the degree to which managers should be concerned about turnover-related declines in productivity. On one hand, these declines likely reflect the costs associated with valuable on-the-job training. On the other, they may be larger than necessary to obtain the desired training benefit for new physicians. In the case of teaching hospitals, we thus are not arguing that an optimal residency system would result in *no* systematic change in productivity throughout the year. It is likely that no system can guarantee residents will be as productive at the beginning of their tenure as they will be at its end. Ultimately, the important question is whether declines in productivity are higher than necessary to train new workers efficiently. The question of whether there are optimal levels of pre-turnover preparation and post-turnover supervision in the face of significant on-the-job training is an interesting issue for further study in contexts both within and outside of the hospital industry.

Beyond its findings with respect to the July phenomenon, this paper has broader implications for the study of the effects of labor turnover on organizations. It provides empirical support for the contention that cohort turnover has negative implications for productivity and that these effects generally increase with the intensity of the turnover, though not always in a linear fashion. Interestingly, these results are obtained in settings (i.e., teaching hospitals) where there is assumed to be a fair degree of supervision that could arguably mitigate the negative effects of turnover. We also find initial evidence suggesting that some portion of the "negative" effects we see may occur as firms take steps to prepare in advance of a known turnover event. Some examples of potential approaches to mitigating the impact of cohort turnover include increasing levels of supervision during the periods preceding and following the turnover or implementing staggered start dates for new employees. Though we cannot assess with our data whether such efforts to smooth the negative effects of cohort turnover would ultimately lead to fewer adverse outcomes over the course of the entire year, it may be possible that, even if firms are not able to reduce the levels of turnover they face, they may be able to take steps to manage its effects.

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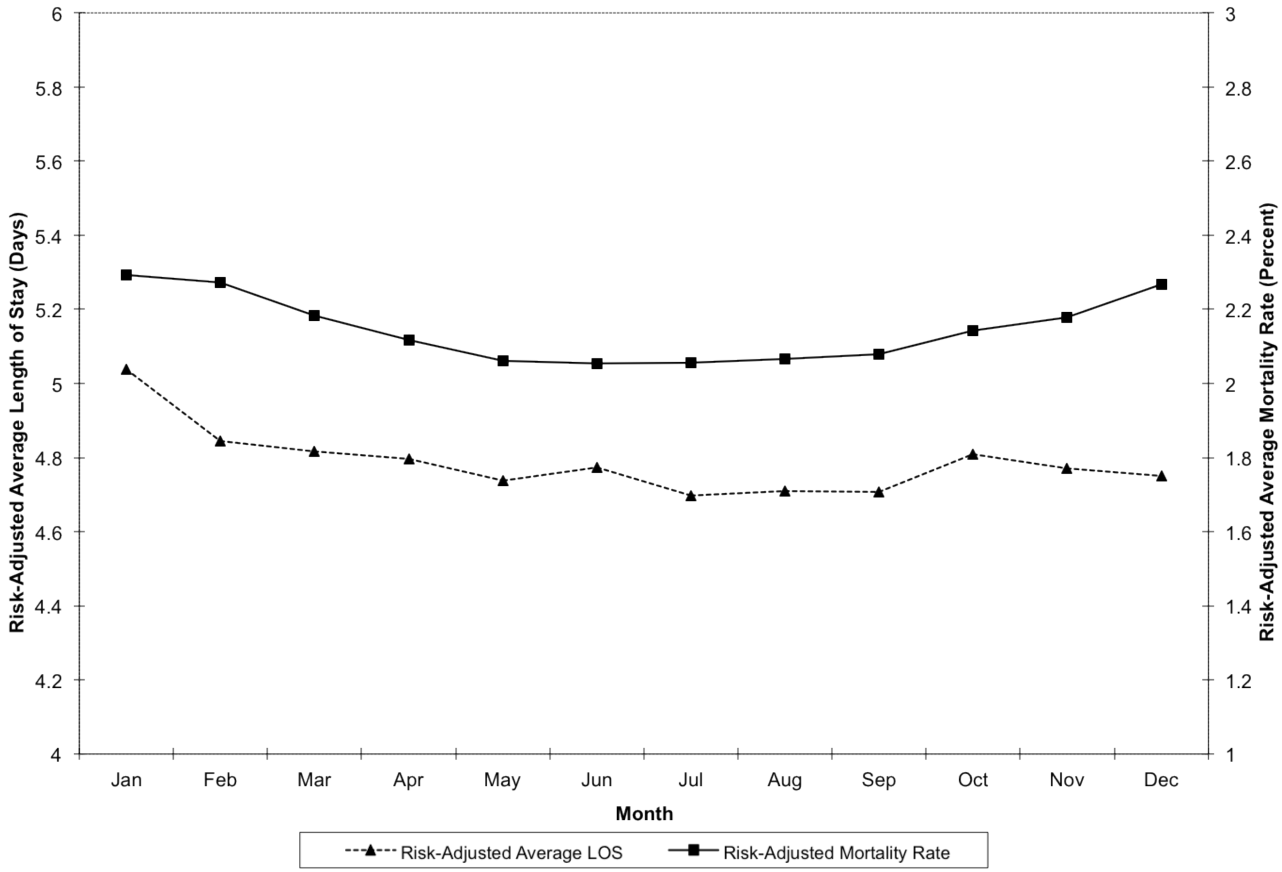
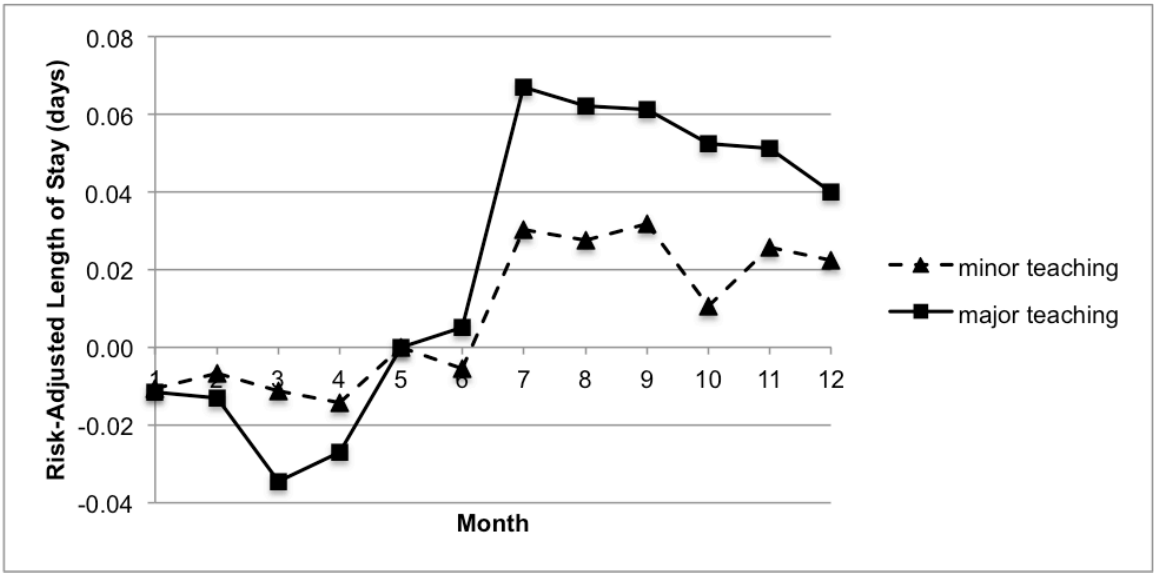
Figure 1: Risk-Adjusted Average Length of Stay and Mortality Rate by Month, 1993-2008



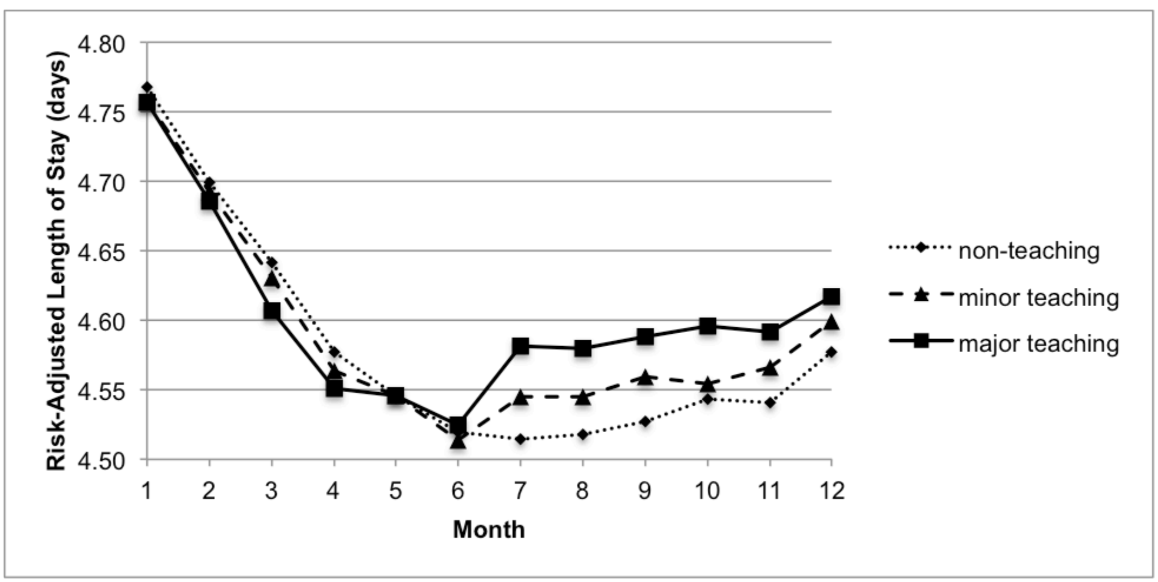
Note: The relatively sharp increase in risk-adjusted average LOS from December to January seems to be driven by scheduling preferences (i.e., where fewer elective cases are being scheduled in December and are delayed until January). We find that this increase in LOS exists only in the subgroup of patients who were *not* admitted through the emergency department, and does not exist in the subgroup of patients who *were* admitted through the emergency department. This phenomenon may be contributing to a heightened level of congestion that temporarily increases LOS in January.
Source: NIS, 1993-2008.

Figure 2a: Change in Risk-Adjusted Length of Stay Relative to Non-Teaching Baseline, 1993-2008



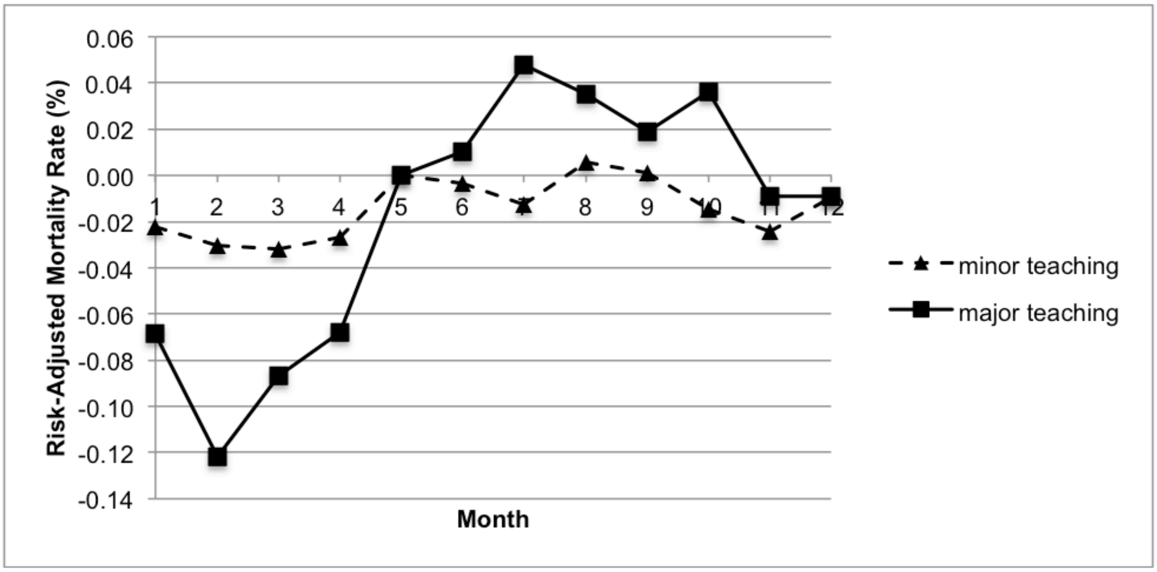
Note: Values indicate the change in the risk-adjusted length of stay for minor and major teaching hospitals relative to the baseline change for non-teaching hospitals.

Figure 2b: Risk-Adjusted Length of Stay by Hospital Type, 1993-2008



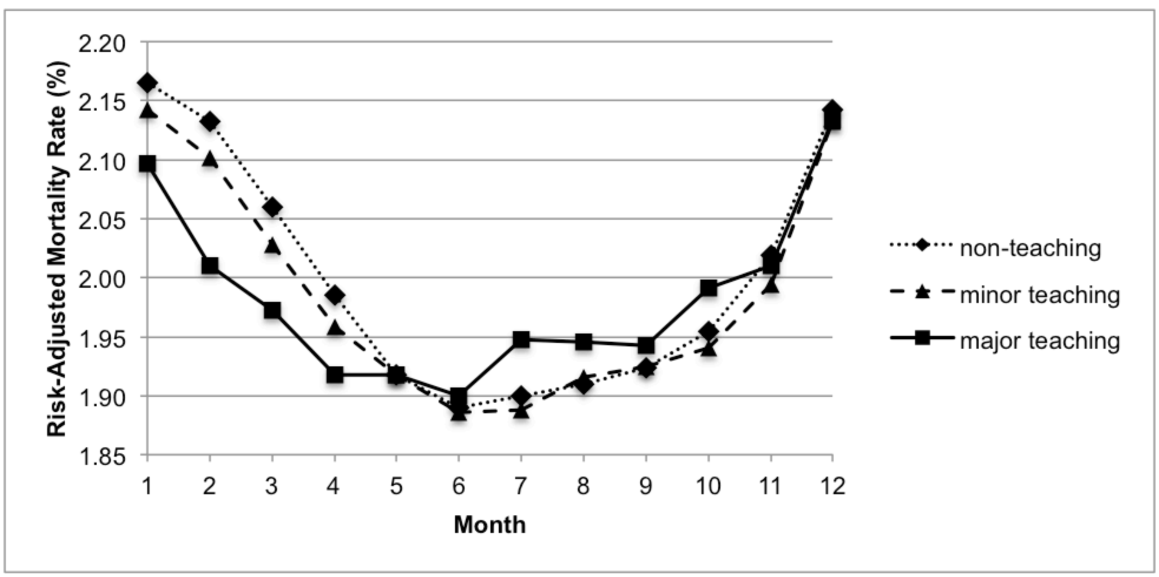
Note: Values indicate the change in the risk-adjusted length of stay by hospital type relative to the May baseline. The May baseline is set equal to the constant estimated by the basic regression (1).

Figure 3a: Change in Risk-Adjusted Mortality Relative to Non-Teaching Baseline, 1993-2008



Note: Values indicate the change in the risk-adjusted mortality for minor and major teaching hospitals relative to the baseline change for non-teaching hospitals.

Figure 3b: Risk-Adjusted Mortality by Hospital Type, 1993-2008



Note: Values indicate the change in the risk-adjusted length of stay by hospital type relative to the May baseline. The May baseline is set equal to the constant estimated by the basic regression (1).

Table 1: Descriptive Statistics by Hospital Type, 1993-2008

	Non-Teaching		**Minor Teaching**		**Major Teaching**		**Full Sample**	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Residents Per Inpatient Bed	0.01	0.04	0.07	0.08	0.56	0.30	0.12	0.23
Inpatient Hospital Beds	231	144	433	252	597	273	356	250
Inpatient Admissions/Year	10,583	7,244	19,796	10,722	28,066	12,206	16,408	11,445
Patient Age	49.1	8.9	45.6	9.2	42.7	8.5	46.9	9.2
Medicaid Admissions/Total Admissions	16%	13%	17%	13%	25%	15%	18%	14%
Medicare Admissions/Total Admissions	39%	13%	33%	12%	28%	11%	36%	13%
Risk-adjusted Average Length of Stay (Days)	4.6	1.4	5.0	0.9	5.4	1.1	4.9	1.2
Risk-adjusted Mortality	2.1%	0.6%	2.3%	0.6%	2.3%	0.5%	2.2%	0.6%
Observations (hospital-years)	8,092		1,785		552		10,429	
Percentage of Total Sample	77.6%		17.1%		5.3%		100.0%	

Note: Observations are at the hospital-year level and cover the sixteen-year period from 1993 to 2008.

Source: NIS, 1993-2008.

Table 2: Base Regressions Using Single Teaching Category, 1993-2008

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference=May)					
	Risk-Adjusted LOS			Risk-Adjusted Mortality		
Teaching						
January	-0.011	(0.012)		-0.037	(0.020)	*
February	-0.009	(0.012)		-0.059	(0.018)	***
March	-0.019	(0.011)	*	-0.049	(0.018)	***
April	-0.018	(0.009)	**	-0.040	(0.018)	**
June	-0.002	(0.012)		0.001	(0.016)	
July	0.042	(0.010)	***	0.007	(0.017)	
August	0.039	(0.010)	***	0.015	(0.017)	
September	0.041	(0.010)	***	0.007	(0.017)	
October	0.024	(0.011)	**	0.002	(0.017)	
November	0.034	(0.011)	***	-0.020	(0.017)	
December	0.028	(0.011)	**	-0.010	(0.017)	
Mean of Dependent Variable						
Teaching	5.18			2.20		
Observations	124,680			124,676		
Adjusted R^2	0.758			0.357		

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: The level of observation is the hospital-month. All regressions include fixed effects for hospital, year, and month, though these coefficients are not shown in the table for ease of presentation. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital. Observations are weighted by the total number of cases for the relevant hospital-month.

Table 3: Base Regressions Using Minor and Major Teaching Categories, 1993-2008

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference=May)					
	Risk-Adjusted LOS			Risk-Adjusted Mortality		
Minor Teaching						
January	-0.010	(0.012)		-0.022	(0.023)	
February	-0.007	(0.012)		-0.030	(0.021)	
March	-0.011	(0.011)		-0.032	(0.021)	
April	-0.014	(0.009)		-0.027	(0.020)	
June	-0.005	(0.012)		-0.004	(0.018)	
July	0.030	(0.010)	***	-0.013	(0.019)	
August	0.028	(0.011)	***	0.006	(0.018)	
September	0.032	(0.011)	***	0.001	(0.019)	
October	0.011	(0.011)		-0.014	(0.018)	
November	0.026	(0.011)	**	-0.025	(0.018)	
December	0.022	(0.011)	**	-0.010	(0.020)	
Major Teaching						
January	-0.011	(0.021)		-0.068	(0.029)	**
February	-0.013	(0.019)		-0.122	(0.025)	***
March	-0.035	(0.020)	*	-0.087	(0.025)	***
April	-0.027	(0.014)	*	-0.068	(0.023)	***
June	0.005	(0.016)		0.010	(0.023)	
July	0.067	(0.017)	***	0.048	(0.024)	**
August	0.062	(0.017)	***	0.035	(0.025)	
September	0.061	(0.017)	***	0.019	(0.027)	
October	0.052	(0.017)	***	0.036	(0.025)	
November	0.051	(0.017)	***	-0.009	(0.025)	
December	0.040	(0.019)	**	-0.009	(0.025)	
Mean of Dependent Variable						
Minor Teaching	5.11			2.17		
Major Teaching	5.43			2.30		
Observations	124,680			124,676		
Adjusted R^2	0.758			0.357		

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: The level of observation is the hospital-month. All regressions include fixed effects for hospital, year, and month, though these coefficients are not shown in the table for ease of presentation. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital. Observations are weighted by the total number of cases for the relevant hospital-month.

Table 4: Base Regressions Using Minor and Major Teaching Categories for Tests of Patient Self-Selection and Increased Transfers, 1993-2008

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference=May)		
	Total Admissions	Expected Mortality	Transfer Rate (Transfers/Total Admissions*100)
Minor Teaching			
January	7.3 (3.5) **	-0.007 (0.015)	-0.04 (0.04)
February	-56.5 (4.7) ***	0.015 (0.015)	-0.01 (0.04)
March	18.5 (3.2) ***	0.002 (0.012)	-0.03 (0.03)
April	-23.3 (2.6) ***	-0.014 (0.014)	-0.01 (0.03)
June	-19.3 (2.4) ***	0.004 (0.013)	-0.03 (0.03)
July	-11.1 (3.0) ***	0.025 (0.017)	-0.05 (0.03) *
August	-4.5 (3.0)	0.052 (0.015) ***	-0.03 (0.03)
September	-29.6 (3.1) ***	0.030 (0.014) **	-0.06 (0.04) *
October	0.3 (3.5)	0.028 (0.016) *	-0.10 (0.05) **
November	-44.3 (4.1) ***	0.011 (0.017)	-0.10 (0.05) **
December	-31.9 (4.2) ***	-0.015 (0.015)	-0.03 (0.05)
Major Teaching			
January	22.8 (11.9) *	-0.070 (0.017) ***	-0.04 (0.08)
February	-110.6 (14.1) ***	-0.019 (0.017)	0.03 (0.07)
March	27.1 (10.1) ***	-0.034 (0.016) **	0.10 (0.06) *
April	-39.0 (6.3) ***	-0.033 (0.016) **	0.01 (0.04)
June	-28.3 (6.1) ***	-0.002 (0.014)	0.01 (0.04)
July	4.4 (10.2)	0.024 (0.018)	-0.05 (0.06)
August	17.7 (10.7)	0.041 (0.016) **	-0.04 (0.06)
September	-37.7 (11.8) ***	0.041 (0.016) **	-0.10 (0.07)
October	16.8 (10.3)	0.011 (0.018)	-0.23 (0.11) **
November	-58.1 (10.9) ***	-0.033 (0.017) *	-0.19 (0.09) **
December	-45.6 (12.4) ***	-0.089 (0.020) ***	-0.01 (0.09)
Mean of Dependent Variable			
Minor Teaching	1,291	2.16	4.79
Major Teaching	1,976	2.05	5.34
Observations	124,683	124,680	124,683
Adjusted R^2	0.975	0.735	0.776

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: The level of observation is the hospital-month. All regressions include fixed effects for hospital, year, and month, though these coefficients are not shown in the table for ease of presentation. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital. Observations are weighted by the total number of cases for the relevant hospital-month.

Table 5: Base Regressions Using Minor and Major Teaching Categories for Patients Admitted from the Emergency Department, 1993-2008

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference=May)					
	Risk-Adjusted LOS			Risk-Adjusted Mortality		
Minor Teaching						
January	0.001	(0.016)		-0.022	(0.047)	
February	0.002	(0.016)		-0.013	(0.042)	
March	0.000	(0.014)		-0.038	(0.045)	
April	-0.022	(0.013)	*	-0.028	(0.044)	
June	0.012	(0.013)		-0.003	(0.038)	
July	0.040	(0.015)	***	0.000	(0.038)	
August	0.031	(0.015)	**	-0.002	(0.038)	
September	0.022	(0.016)		-0.018	(0.038)	
October	0.010	(0.016)		-0.026	(0.036)	
November	0.037	(0.016)	**	-0.007	(0.036)	
December	0.037	(0.016)	**	-0.041	(0.041)	
Major Teaching						
January	-0.012	(0.031)		-0.093	(0.053)	*
February	-0.048	(0.035)		-0.195	(0.048)	***
March	-0.019	(0.038)		-0.117	(0.050)	**
April	-0.015	(0.022)		-0.099	(0.046)	**
June	0.036	(0.021)	*	0.018	(0.047)	
July	0.126	(0.023)	***	0.108	(0.047)	**
August	0.091	(0.024)	***	0.067	(0.055)	
September	0.028	(0.028)		-0.031	(0.054)	
October	0.093	(0.027)	***	0.035	(0.054)	
November	0.060	(0.026)	**	0.006	(0.057)	
December	0.058	(0.027)	**	0.036	(0.050)	
Mean of Dependent Variable						
Minor Teaching	5.58			3.35		
Major Teaching	6.53			3.42		
Observations	109,822			109,816		
Adjusted R^2	0.750			0.250		

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: The level of observation is the hospital-month. All regressions include fixed effects for hospital, year, and month, though these coefficients are not shown in the table for ease of presentation. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital. Observations are weighted by the total number of cases for the relevant hospital-month.

Table 6: Regressions Using Minor and Major Teaching Categories with Multi-month Periods, 1993-2008

	Change in Dependent Variable Relative to Non-Teaching Baseline (Reference=May)	
	Risk-Adjusted LOS	Risk-Adjusted Mortality
Minor Teaching		
Jan-Feb	-0.009 (0.011)	-0.026 (0.019)
Mar-Apr	-0.013 (0.009)	-0.030 (0.018)
June	-0.005 (0.012)	-0.004 (0.018)
Jul-Aug	0.029 (0.009) ***	-0.003 (0.016)
Sep-Oct	0.021 (0.010) **	-0.007 (0.016)
Nov-Dec	0.024 (0.010) **	-0.018 (0.016)
Major Teaching		
Jan-Feb	-0.012 (0.018)	-0.094 (0.024) ***
Mar-Apr	-0.031 (0.015) **	-0.078 (0.021) ***
June	0.005 (0.016)	0.010 (0.023)
Jul-Aug	0.065 (0.015) ***	0.041 (0.022) *
Sep-Oct	0.057 (0.016) ***	0.028 (0.023)
Nov-Dec	0.045 (0.015) ***	-0.010 (0.022)
Mean of Dependent Variable		
Minor Teaching	5.11	2.17
Major Teaching	5.43	2.30
Observations	124,680	124,676
Adjusted R^2	0.758	0.355

,, and *** denote statistical signficance at the 10%, 5%, and 1% levels, respectively.*

Note: The level of observation is the hospital-month. All regressions include fixed effects for hospital, year, and month, though these coefficients are not shown in the table for ease of presentation. Standard errors (in parentheses) are heteroskedasticity robust and clustered by hospital. Observations are weighted by the total number of cases for the relevant hospital-month.