Cohort Turnover and Operational Performance: The July Phenomenon in Teaching Hospitals

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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 operational performance in the context of teaching hospitals. Specifically, we examine the impact of the annual July turnover of residents in U.S. teaching hospitals on the average length of hospital stay and mortality rate in teaching hospitals relative to a control group of non-teaching hospitals. Despite the anticipated nature of the cohort turnover and the supervisory structures that exist in teaching hospitals, the annual July turnover of residents results in increased resource utilization (i.e., longer average length of stay) for both minor and major teaching hospitals, relative to a control group of non-teaching hospitals. We find limited evidence of negative effects on quality as measured by mortality rates. In major teaching hospitals, we find evidence of a substantial anticipation effect that presents as a gradual decrease in operational performance beginning several months before the actual cohort turnover. We identify higher overall quality of nursing and increased intensity of potential quality assurance as managerial levers for mitigating the decrease in hospital operational performance both at the time of and in the months leading up to the cohort turnover.

Key words: turnover; productivity; labor; health care; empirical operations

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1 Introduction

Nearly all managers must deal with the consequences of employee turnover within their organizations. Despite the importance of this issue, academic attention has been disproportionately focused on the *causes* rather than *consequences* of turnover (Glebbeek and Bax, 2004; Mobley, 1982). One possible explanation for the limited number of studies on the effects of turnover is the difficulty in answering this question empirically. Turnover is typically an endogenous phenomenon that may occur for various reasons not observed by the researcher. For example, more productive workers may be more likely to remain with a company longer than less productive ones (Jovanovic, 1979). Furthermore, a worker's decision to remain with a company may be influenced not only by his or her level of performance but also the company's compensation dispersion relative to its competitors (Carnahan et al., 2012) and the congruence between a worker's ability and the job's complexity (Wilk and Sackett, 1996). Under such circumstances, it is difficult to make causal inferences concerning turnover's effect on performance using organization-level data. Those studies that do examine the consequences deliver mixed findings. Though some find that employee turnover exhibits a negative effect on performance through lower levels of productivity (Batt, 2002), higher costs (Drexler and Schoar, 2014), smaller profit margins (Ton and Huckman, 2008), worse customer service (Kacmar et al., 2006; Ton and Huckman, 2008), and lower job satisfaction (Whitt, 2006), others find that the effect of turnover on performance depends on contextual factors such as interpersonal knowledge diversity among team members (Narayanan et al., 2014), psychological safety (Chandrasekaran and Mishra, 2012), 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 performance is that turnover itself appears in multiple forms. Many organizations 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 organizations 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 was 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, and residents and fellows in teaching hospitals. Given the number of individuals transitioning into or out of employment at a specific point in time, cohort turnover raises concerns about adverse effects on operational performance due to factors such as lower levels of team familiarity (Huckman et al., 2009; Huckman and Staats, 2011), 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 supervisors typically remain in place, one might expect this particular type of turnover not to have a negative impact on operational performance. In short, the impact of cohort turnover on operational performance is not obvious on an *a priori* basis.

In this paper, we consider cohort turnover among resident physicians 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. At (or slightly before) the beginning of every July, the most senior residents move on to permanent medical positions or fellowships (for further training in a sub-specialty) at other hospitals, and recent medical school graduates arrive as first-year residents (i.e., interns). Every summer, this cohort turnover leads to a discrete reduction in the average experience of the labor force at teaching hospitals. In addition, it may disrupt established teams of physicians and other caregivers within hospitals. Although most attending physicians and nurses who supervise and serve as an operational safeguard for the work of residents typically do not change roles at this time of year, either of the above effects may have negative consequences for the two major determinants of a hospital's operational performance: 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. To date, the clinical literature presents a mixed and inconclusive picture of whether the July phenomenon exists, and if so, for which outcomes.

We examine the impact of the July turnover on hospital operational performance using data on all patient admissions from a large, multi-state sample of U.S. hospitals over a 12-year period. When comparing trends in teaching hospitals to those in non-teaching (i.e., control) hospitals over the course of the year, we find a significant increase in resource utilization (as measured by risk-adjusted average length of stay (LOS)) associated with the cohort turnover. These effects are increasing in a hospital's level of teaching intensity (as measured by the number of residents per hospital bed), which reflects the degree to which a hospital relies on residents. In terms of clinical quality, we find limited evidence of a decrease in performance (as measured by risk-adjusted mortality rates) in hospitals with a higher level of teaching intensity (i.e., major teaching hospitals) but not in those with a lower level of teaching intensity (i.e., minor teaching hospitals). In major teaching hospitals, we also find evidence of a substantial anticipation effect that manifests as a gradual decrease in operational performance beginning as early as the March prior to a given cohort turnover in July. This anticipation effect may result from a transition of responsibilities in the last several months of the academic year that is not coupled with the same degree of precaution that accompanies the cohort turnover in July.

Using additional data on hospital nursing quality and staffing levels, we also identify managerial levers that hospitals may use to mitigate their decrease in operational performance both at the time of and in the months leading up to the July turnover. Specifically, we find that hospitals may be able to dampen these negative effects on operational performance by improving their overall quality of nursing and increasing their intensity of quality assurance related to the work of the residents. Though neither of these levers is costless to implement, each has significant and lasting implications for improved hospital productivity and quality beyond just the month of July.

This paper contributes to the management literature on turnover and performance by empirically examining the effects of cohort turnover. We define cohort turnover as a distinct phenomenon and find that it leads to greater resource utilization but not necessarily a lower level of clinical quality in teaching hospitals relative to non-teaching hospitals. The limited effect on clinical quality may be attributable to the fact that a cohort turnover event is anticipated, thereby allowing hospitals to prepare for it. Consistent with this possibility, we identify the presence of a substantial anticipation effect, which is important for an organization to acknowledge in assessing the impact of cohort turnover on operational performance. We identify structures and processes that can facilitate knowledge transfer from departing to entering workers and, ultimately, mitigate the negative effects of cohort turnover. An improved understanding of the implications of cohort turnover may help inform analytical models of staffing and scheduling, which tend not to address the operational costs of turnover and tend to model turnover as a stochastic event (Boudreau et al., 2003; Gans and Zhou, 2002).

2 Cohort Turnover and Performance

Cohort turnover is distinct from individual and collective turnover in that it is planned in advance, happens on a large scale, and involves the simultaneous exit of a large number of experienced employees and a similarly sized entry of new workers. These changeovers occur as a matter of policy, regardless of the underlying productivity of the workers involved. Given the large-scale nature of cohort turnover, it has the potential to affect an organization's operational performance significantly. Yet there is little known about the effects of cohort turnover on operational performance, as much of the prior work on turnover in the management literature considers only individual turnover (Campbell et al., 2012).

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 beyond annual resident turnover in teaching hospitals, including military deployments, changes of political administrations, and labor strikes. A second reason may be the assumption that the answers to questions concerning its effects are obvious; given the sheer magnitude of the change brought by cohort turnover, one might assume that it *must* have a detrimental impact on operational performance. Despite its magnitude, however, cohort turnover often occurs in a predictable fashion and the affected organizations should, theoretically, have time to anticipate and prepare for it. For example, attending physicians and nurses in teaching hospitals—being aware of the turnover that occurs each July—may focus more intently on supervising and checking the quality of the work of new residents at that time of the year. As a result, the impact of cohort turnover in settings where the formal and informal supervisory staff does not change is not obvious on an *a priori* basis.

In the management literature, there are arguably two analogs to cohort turnover that have been previously studied. One is the turnover of CEOs and top management teams (Cao et al., 2006; Messersmith et al., 2014; Tushman and Rosenkopf, 1996). Though this involves the turnover of a limited number of individuals, the significance of their roles within the organization suggests that such turnover may have a significant impact on an organization's operational performance. Nevertheless, perhaps due to the endogenous nature of this type of turnover event, this relationship has been difficult to identify and empirical findings to date deliver mixed results. Another is collective turnover, which refers to the aggregate departure of employees 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 potential moderators of the relationship between collective turnover and performance: leaver proficiencies, time dispersion, positional distribution, remaining member proficiencies, and newcomer proficiencies (Hausknecht and Holwerda, 2013).

Looking beyond the management literature, there is a body of research in the medical literature that examines the effects of the annual resident turnover in teaching hospitals on various clinical outcomes. To date, this literature presents mixed and inconclusive findings (Young et al., 2011). While several studies suggest that patients admitted in July have similar mortality outcomes (van Walraven et al., 2011) and morbidity outcomes (Ford et al., 2007) as patients presenting in other months, other studies show the opposite, suggesting that patients exhibit worse outcomes in July in terms of mortality rates (Englesbe et al., 2007), morbidity (Haller et al., 2009), medication error rates (Phillips and Barker, 2010), and hospital-acquired complications (Wen et al., 2015). In addition to these mixed findings, several of the individual papers in this literature face either methodological or contextual limitations, which we address below.

As a whole, the existing management and medical literature thus suggests that cohort turnover could have either a positive or negative overall impact on operational performance. On one hand, cohort turnover may have a negative impact on operational performance, given that several prior studies have found a negative relationship between individual turnover and productivity (Huselid, 1995; Staw, 1980). When individuals leave an organization and others must be recruited in their place, the organization may experience a decline in operational performance due to the resulting operational disruption and lower level of team familiarity (Huckman and Staats, 2011; Huckman et al., 2009; Krueger and Mas, 2004; Staw, 1980). This may manifest itself in the loss of critical information, not only as a result of employment turnover but also due to the routine changeover of workers between shifts (Hutter et al., 2006). These effects may, in turn, interfere with organizational learning to the extent that locally acquired knowledge is difficult to disseminate and lower levels of experience lead to lower levels of productivity (Adler and Clark, 1991; Argote et al., 2003; Lapré et al., 2000). In addition, organizations may experience further declines in operational performance due to the demoralization of remaining workers and the costs incurred in selecting, recruiting, and training new workers (Staw, 1980). If individual turnover results in these negative effects on operational performance, the turnover of a large cohort of individuals may amplify the magnitude of this negative effect.

On the other hand, because cohort turnover is a predictable phenomenon that is planned far in advance, organizations may be able to preempt and mitigate these negative effects on operational performance. For example, organizations may systematically transfer information from departing workers to new workers in an attempt to smooth the transition (Drexler and Schoar, 2014). Organizations may also temporarily increase their staffing or heighten the intensity of quality assurance related to the work of new employees around the time of the cohort turnover to buffer against the anticipated loss of productivity (Reagans et al., 2005). Furthermore, cohort turnover may lead to an overall *increase* in performance to the extent that outgoing workers may be experiencing burnout and new workers may be motivated to exert high levels of effort (Staw, 1980). Given that operational performance is a function of not only skill but also effort, turnover may improve average operational performance (Dalton and Todor, 1979; Staw, 1980). In fact, some prior work suggests that operational performance can degrade when turnover is *too low* due to employee stagnation (Abelson and Baysinger, 1984; Glebbeek and Bax, 2004; Whitt, 2006).

We expect that both the negative and positive effects of cohort turnover may exist concurrently. Nevertheless, on average we expect the negative effects to dominate because several of the potential positive effects are conditional on an organization taking proactive measures to mitigate the anticipated negative effects. Therefore, assuming that organizations are likely to take the path of least resistance and behave passively, we hypothesize the following:

Hypothesis 1: Cohort turnover has a negative impact on operational performance.

Next, we consider the magnitude of this effect as a function of the size of the cohort turnover. By definition, cohort turnover occurs on a large scale with a group of workers turning over at a pre-specified time. If there is, indeed, an overall negative effect of cohort turnover on operational performance, we would expect the magnitude of this effect to be increasing in the size of the cohort relative to the size of the organization. This is because a larger relative cohort would indicate that a greater proportion of the organization's workforce is turning over. In fact, it may be the case that any negative effect of cohort turnover on operational performance only appears when the relative size of the cohort exceeds a certain threshold. We hypothesize:

Hypothesis 2: Cohort turnover has a greater negative impact on operational performance in organizations that experience a greater relative magnitude of cohort turnover.

If cohort turnover has an overall negative effect on an organization's operational performance, how might organizations be able to mitigate these effects? One way to buffer against this negative impact may be to adopt organizational structures and processes that facilitate the transfer of tacit knowledge from departing to entering workers (Argote and Ingram, 2000; Argote et al., 2003). These structures and processes include, but are not limited to, the greater use of standard operating procedures (Ton and Huckman, 2008) and an increase in the intensity of quality assurance by colocated workers who are not turning over and can serve as an operational safeguard for the work of new workers (Hausknecht et al., 2009; Reagans et al., 2005; Gray et al., 2015). We thus hypothesize the following mitigation effect:

Hypothesis 3: The negative impact of cohort turnover on operational performance is less pronounced in organizations with structures and processes that facilitate knowledge transfer from departing to entering workers.

3 Setting and Data

3.1 Research Setting

We study cohort turnover in the context of teaching hospitals, which have two primary objectives: the provision of high quality medical care and the training of new physicians. These related but distinct objectives overlap within residency programs. During their three to five years of residency at roughly 1,000 teaching hospitals in the country, residents represent an important piece of a hospital's system for delivering care (Association of American Medical Colleges, 2009).

Patient care in teaching hospitals is provided by teams of medical professionals that include attending physicians, nurses, fellows, residents, and medical students. Much of the care is delivered by a resident, who supervises medical students and is supervised by an attending physician with or without additional supervision from a more senior resident or a fellow. Nurses also play a key role in delivering patient care and they help new residents learn how to deliver care effectively, efficiently, and safely. The daily activities of residents include admitting, diagnosing, treating, and discharging patients.

Residency programs in the United States are structured like schools. Each class of residents enters together at the beginning of the academic year, and senior members of each program graduate together at the end of the academic year. For most residency programs, the year officially begins on July 1 and ends the following June 30, though the annual transition 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 turnover creates potential transitional challenges in teaching hospitals—even for residents in the middle years of their programs—as each cohort of physicians becomes comfortable with new roles and responsibilities.

3.2 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 1997 to 2008. NIS contains discharge-level data for all inpatient cases at a sample of roughly 20 percent of the community hospitals[1] in the United States. Depending on the year, NIS includes information for hospitals from between 22 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), LOS, in-hospital mortality, diagnosis-related group (DRG), and comorbidities.

We link the NIS data with information from each corresponding year's AHA Annual Survey of Hospitals, which includes data on the operating and financial characteristics for nearly all of the more than 5,000 acute care hospitals in the United States. In addition to several other items, the AHA data provide information on the number of hospital beds and full-time equivalents (FTEs) for residents and nurses at each facility in a given year. Using this information, we are able to construct a measure of teaching intensity, which captures the relative magnitude of the cohort turnover at a given hospital. We calculate teaching intensity as the number of FTE residents per hospital bed. We note that because the AHA data on the number of FTE residents are only available at the hospital level and not at the level of the specialty within a hospital, we construct our measure of teaching intensity at the hospital level so that there is a "match" in the level at

[1] Community hospitals are defined by the NIS and the American Hospital Association (AHA) as "...'all nonfederal, short-term, general, and other specialty hospitals, excluding hospital units of institutions.' Included... 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).

which the key independent and dependent variables are observed.

Though we do not have readily available measures of the use of standard operating procedures, we do have access to two proxies for assessing the structures and processes that facilitate knowledge transfer from departing to entering workers. First, we use the quality of nursing care at a hospital as a proxy for the quality of workers who are not systematically turning over as a cohort and can transfer knowledge to new workers. The quality of nursing care at a hospital is an important and relevant measure because it reflects the average capability of the nurses who can help new residents learn how to deliver care effectively, efficiently, and safely while the new residents are still building familiarity with their new roles (Vogus and Sutcliffe, 2007). Furthermore, in hospitals, physicians (including the new residents) must continuously collaborate with nurses to deliver care (Senot et al., 2016). For this measure, we use data from the American Nurses Credentialing Center's (ANCC) magnet recognition program. According to the ANCC, magnet recognition is a nationally recognized credential for quality in patient care and excellence in nursing that arises from "transformational leadership, structural empowerment, [and] exemplary professional practice" (American Nurses Credentialing Center, 2015). As of July 2015, approximately 400 of the 5,000 hospitals in the United States had received magnet recognition. Once recognized, the credential lasts for four years, and most hospitals are successful in getting the credential renewed from that point forward.

Our second proxy is the relative staffing level of nurses to residents at a given hospital. This measure captures the potential intensity of quality assurance by workers who are not systematically turning over as a cohort and can serve as an operational safeguard for the work of new residents. We use a nursing-related measure because nurses play an important role in assuring the quality of resident work. This is due to the fact that nurses have both clinical and hospital-specific tacit process knowledge that is likely helpful to new residents. For example, nurses in teaching hospitals work closely with residents and have many opportunities to guide new residents in preparing patients for procedures, double-checking the dosages of medications ordered, engaging in challenging conversations with patients and families, and other duties. To operationalize this measure, we use the AHA data to construct a measure of the intensity of quality assurance, which we calculate as the ratio of FTE nurses to FTE residents. The use of a measure that captures a ratio, as opposed to a binary indicator variable, to highlight the variance across organizations builds on prior work (e.g., see Mishra and Sinha, 2016). Although this measure reflects only the *potential* (not actual) level of quality assurance by nurses, it represents a reasonable approximation of quality assurance, as the majority of nurses in teaching hospitals work closely with residents (Vallis et al., 2004).

4 Empirical Methodology

To examine the impact of the July turnover on hospital performance, as measured by resource utilization and clinical quality, we employ a difference-in-differences framework that compares changes in our operational performance measures over the course of the year in teaching hospitals relative to those in the baseline of non-teaching hospitals. Unlike individual and collective turnover, the exogenous nature of cohort turnover allows for the use of this approach to identify relative changes in LOS (proxy for resource utilization) and mortality rate (proxy for clinical quality) over the course of the year. This method of using non-teaching hospitals as a control group enables us to separate changes that are driven by the cohort turnover in July from those that are driven by unobserved factors—such as illness severity and seasonal variation—that are also present in non-teaching hospitals.

4.1 Hospital Categories

The source of identification in our empirical analysis is the varying degree to which certain types of hospitals rely on residents. To test Hypothesis 1, we divide hospitals into two categories: non-teaching hospitals and teaching hospitals. To test Hypotheses 2 and 3, we further divide teaching hospitals into two sub-categories: minor teaching hospitals and major teaching hospitals.

Non-teaching hospitals are those 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 and have teaching intensities (i.e., FTE residents per 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 teaching intensity is used by the Medicare Payment Advisory Commission (MedPAC) to distinguish minor and major teaching facilities (MedPAC, 2002). We repeat our analyses 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 (2.3%) of hospitals that are considered major teaching hospitals under this latter definition, we maintain MedPAC's 0.25 threshold. We note, however, the lack of substantive difference in our main findings when using either of these threshold values.

We present descriptive statistics for each of the three hospital categories as well as for the entire sample in Table 1. The first row illustrates the differences in average teaching intensity across the three groups. The average teaching intensities of non-teaching and minor teaching hospitals are similar (0.02 and 0.07, respectively) while that of major teaching hospitals (0.55) 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 teaching intensity increases.

Table 1: Descriptive Statistics by Hospital Type

	Non-Teaching		**Minor Teaching**		**Major Teaching**		**Full Sample**	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Residents Per Inpatient Bed	0.02	0.04	0.07	0.09	0.55	0.33	0.13	0.25
Inpatient Hospital Beds	224	142	402	236	580	244	348	239
Inpatient Admissions/Year	10,824	7,423	19,719	11,001	28,718	12,281	17,057	11,824
Patient Age	49.3	8.4	46.5	9.2	43.6	8.5	47.3	9.0
Medicaid Admissions/Total Admissions	17%	12%	16%	13%	24%	15%	18%	13%
Medicare Admissions/Total Admissions	40%	13%	34%	12%	29%	11%	36%	13%
Risk-adjusted Average LOS (days)	4.4	0.9	4.7	0.7	5.2	0.8	4.7	0.9
Risk-adjusted Mortality Rate	2.1%	0.6%	2.1%	0.5%	2.2%	0.5%	2.1%	0.6%
Observations (hospital-years)	5,537		1,369		431		7,337	
Percentage of Total Sample	75.5%		18.7%		5.9%		100.0%	

Notes. Observations are at the hospital-year level and cover the 12-year period from 1997 to 2008. *Source*: NIS, 1997-2008.

4.2 Risk Adjustment of Dependent Variables

To account for systematic differences in the level of patient severity at non-teaching, minor teaching, and major teaching hospitals, we risk adjust the dependent variables: LOS and mortality rate. Table 1 illustrates that such differences in patient characteristics exist across hospital types in our sample. Rows 4 through 6 of Table 1 illustrate that teaching intensity is correlated with the demographics of a hospital's patient base. In particular, non-teaching hospitals attract older patients (49.3 years) than either type of teaching hospital (46.5 and 43.6 years for minor and major teaching hospitals, respectively), possibly due to the differences in the geographic regions in which different types of hospitals tend to be concentrated. In addition to having younger patients, major teaching hospitals also have a higher percentage of Medicaid patients than the other groups. These relationships are consistent with the fact that many teaching hospitals are located in densely populated cities.

We note that, to the extent that these differences in risk across types of hospitals remain constant over the course of the calendar year, the risk adjustment we perform using the clinical and demographic characteristics of individual patients would not be necessary to identify the effect of cohort turnover. It is possible, however, that risk differences across types of hospitals are *not* constant over the calendar year. For example, to the degree that, within older populations, relatively healthy individuals tend to 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. Our approach to risk adjustment using individual clinical and demographic characteristics addresses this concern.

To risk adjust our dependent variables, we adopt an approach that has been widely used in the operations management literature on productivity and quality of care (Huckman and Pisano, 2006; Huckman, 2003; KC and Staats, 2012). As covariates in our equations to calculate expected LOS and mortality, respectively, we include patient age, age squared, gender, and an indicator for Medicaid as the primary payment source. The Medicaid variable is included as a proxy for the patient's socioeconomic status. We also include each patient's primary diagnosis or procedure as captured by one of more than 400 diagnosis-related groups (DRGs) assigned by the Centers for Medicare & Medicaid Services; our regressions include fixed effects for each of these DRGs. Finally, we include the Charlson index, a measure of comorbidities that increase a patient's risk of mortality (Charlson et al., 1987). Given our DRG fixed effects, the Charlson index captures the within-diagnosis severity of a patient's illness.

For LOS, we use a simple linear regression to calculate expected values. Given that the in-hospital mortality variable is binary, we use logistic regression to obtain the estimated probability of death for each patient discharge. These equations are run separately for each calendar year for computational ease, as each year has approximately 6 million observations. The observed and expected values for LOS and mortality 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 LOS ($RALOS_{h,m,t}$) is:

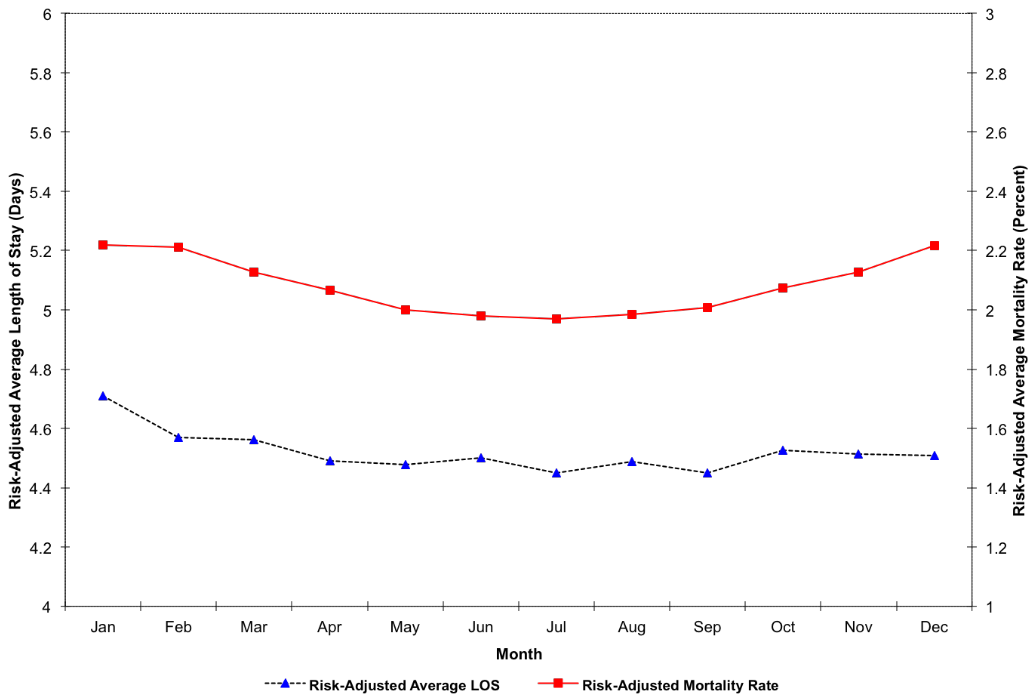
$$RALOS_{h,m,t} = \frac{OLOS_{h,m,t}}{ELOS_{h,m,t}} \times \overline{OLOS_t} \tag{1}$$

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. $\overline{OLOS_t}$ is the average observed LOS for the entire sample in year t and is used to normalize the value of $RALOS_{h,m,t}$.

In rows 7 and 8 of Table 1, we present the risk-adjusted average LOS and mortality rate, respectively, for each type of hospital. We find that risk-adjusted average LOS 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. In addition, we find that the risk-adjusted mortality rate is higher in major (2.2%) teaching facilities compared to non-teaching and minor teaching facilities (2.1%).

In Figure 1, we see that risk-adjusted LOS and risk-adjusted mortality vary over the course of the calendar year. Risk-adjusted mortality is relatively high in the winter months, declines until the summer months, and then begins increasing during the fall. This pattern has been noted by epidemiologists (e.g., Gemmell et al. (2000)) and attributed to a range of factors including the impact of seasonal disease (e.g., influenza and respiratory illness). 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. With this approach, we can calculate “de-seasoned” trends in risk-adjusted LOS and risk-adjusted mortality for teaching hospitals to determine the potential effect of the July turnover.

Figure 1: Risk-adjusted Average Length of Stay and Mortality Rate by Month



Source: NIS, 1993-2008.

4.3 Empirical Specification

Our analyses rely on a difference-in-differences framework that follows the relative changes in risk-adjusted average LOS and risk-adjusted mortality for the different groups of hospitals over the course of the year. To test Hypotheses 1 and 2, we estimate the following model:

$$Y_{h,m,t} = \alpha_h + \delta_t + \mu_m + \sum_{m=1}^{12} \gamma_{1,m} \cdot (\mu_m \times MIN\_TCH_{h,m,t}) + \sum_{m=1}^{12} \gamma_{2,m} \cdot (\mu_m \times MAJ\_TCH_{h,m,t}) + \epsilon_{h,m,t} \quad (2)$$

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. The two summation terms are vectors of interactions between the indicators for minor and major teaching hospitals, respectively, and the month effects. The main effects on minor and major teaching hospitals (MIN_TCH and MAJ_TCH, respectively) are absorbed by the hospital fixed effects because specific teaching hospitals are classified as either minor or major teaching hospitals for the entirety of our sample period.[2] The coefficients $\gamma_{1,m}$ ($\gamma_{2,m}$) on the $\mu_m \times MIN\_TCH$ ($\mu_m \times MAJ\_TCH$) interactions capture the extent to which any seasonal pattern that is found for minor (major) teaching hospitals differs from that for non-teaching controls. Given that the residency changeover begins in late June for many hospitals, to measure the impact of the July turnover we compare the change in the dependent variable from May (reference month) to July for minor and major teaching hospitals, respectively, to the analogous May-to-July change for non-teaching hospitals. This difference is captured by the coefficients $\gamma_{1,7}$ and $\gamma_{2,7}$ on $\mu_7 \times MIN\_TCH$ and $\mu_7 \times MAJ\_TCH$, respectively. Each of the hospital-month observations is weighted by the total number of cases for the hospital-month to account for the fact that all values of the dependent variable are averages. We analyze the data at the hospital-month level for computational tractability, as we would otherwise have approximately 77 million observations. Finally, the standard errors are clustered by hospital to address potential lack of independence in the error term, $\epsilon_{h,m,t}$.

To test Hypothesis 3 using our first proxy, we estimate the following model:

$$\begin{aligned} Y_{h,m,t} = & \alpha_h + \delta_t + \mu_m + \sum_{m=1}^{12} \lambda_{1,m} \cdot (\mu_m \times MIN\_TCH_{h,m,t}) \\ & + \sum_{m=1}^{12} \lambda_{2,m} \cdot (\mu_m \times MAJ\_TCH\_NONMAGNET_{h,m,t}) \\ & + \sum_{m=1}^{12} \lambda_{3,m} \cdot (\mu_m \times MAJ\_TCH\_MAGNET_{h,m,t}) + \epsilon_{h,m,t} \end{aligned} \tag{3}$$

Y, α_h, δ_t, μ_m, MIN_TCH, and $\epsilon_{h,m,t}$ remain the same as in (2). We stratify major teaching hospitals into those that have never been ($MAJ\_TCH\_NONMAGNET$) and those that have been (MAJ_TCH_MAGNET) certified as "magnet" facilities since the beginning of the recognition program in 1991. ANCC magnet recognition serves as a proxy for organizational structures and processes that facilitate knowledge transfer from departing to entering workers that is independent of a hospital's staffing levels. Here, the coefficients $\lambda_{1,m}$ on the $\mu_m \times MIN\_TCH$ interactions are identical to the coefficients $\gamma_{1,m}$ on the $\mu_m \times MIN\_TCH$ interactions in (2) because the stratification of the major teaching hospitals does not affect the minor teaching hospitals. The coefficients $\lambda_{2,m}$ ($\lambda_{3,m}$) on the $\mu_m \times MAJ\_TCH\_NONMAGNET$ ($\mu_m \times MAJ\_TCH\_MAGNET$) interactions capture the extent to which any seasonal pattern that is found for major teaching hospitals without (with) magnet recognition differs from that for non-teaching controls. We examine the coefficients $\lambda_{2,7}$ and $\lambda_{3,7}$ on $\mu_7 \times MAJ\_TCH\_NONMAGNET$ and $\mu_7 \times MAJ\_TCH\_MAGNET$, respectively, to determine whether the July turnover differentially impacts the performance of major teaching hospitals

[2] Some hospitals, though very few, move across the threshold between minor and major teaching hospitals during our sample period. We assigned these hospitals to the category of teaching intensity that matched the majority of the months for which they were present in our sample.

without and with ANCC magnet recognition.

To test Hypothesis 3 using our second proxy, we estimate the following model:

$$
\begin{aligned}
Y_{h,m,t} = & \alpha_h + \delta_t + \mu_m + \rho_1 \cdot MAJ\_TCH\_LO\_QA_{h,m,t} \\
& + \rho_2 \cdot MAJ\_TCH\_HI\_QA_{h,m,t} + \sum_{m=1}^{12} \rho_{3,m} \cdot (\mu_m \times MIN\_TCH_{h,m,t}) \\
& + \sum_{m=1}^{12} \rho_{4,m} \cdot (\mu_m \times MAJ\_TCH\_LO\_QA_{h,m,t}) \\
& + \sum_{m=1}^{12} \rho_{5,m} \cdot (\mu_m \times MAJ\_TCH\_HI\_QA_{h,m,t}) + \epsilon_{h,m,t}
\end{aligned}
\tag{4}
$$

Y, α_h, δ_t, μ_m, MIN_TCH, and $\epsilon_{h,m,t}$ remain the same as in (2) and (3). We stratify major teaching hospitals into those with lower and higher intensities of potential quality assurance ($MAJ\_TCH\_LO\_QA$ and $MAJ\_TCH\_HI\_QA$, respectively). The final two summation terms are vectors of interactions between the two sub-categories of major teaching hospitals and the month effects. The coefficients $\rho_{4,m}$ ($\rho_{5,m}$) on the $\mu_m \times MAJ\_TCH\_LO\_QA$ ($\mu_m \times MAJ\_TCH\_HI\_QA$) interactions capture the extent to which any seasonal pattern that is found for major teaching hospitals with low (high) intensities of quality assurance differs from that for non-teaching controls. We examine the coefficients $\rho_{4,7}$ and$\rho_{5,7}$ on $\mu_7 \times MAJ\_TCH\_LO\_QA$ and $\mu_7 \times MAJ\_TCH\_HI\_QA$, respectively, to determine whether the July turnover differentially impacts the performance of major teaching hospitals with lower versus higher intensities of quality assurance.

We use the median of FTE nurses per FTE resident as the boundary between major teaching facilities with lower versus higher intensities of quality assurance. We also run a version of this analysis using thresholds of thirds rather than halves and note the lack of any substantive differences in our findings using either of these categorizations.

5 Results

5.1 The Impact of Cohort Turnover on the Operational Performance of Teaching Hospitals

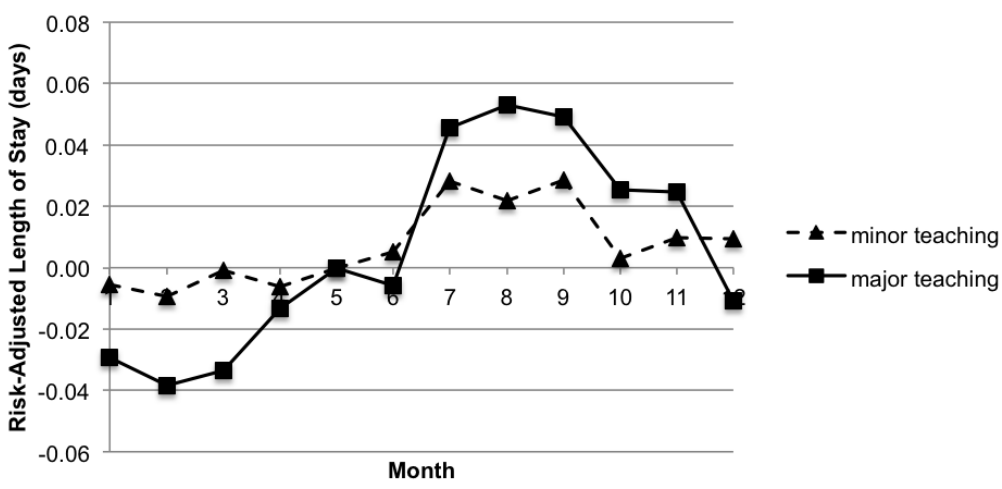
Table 2 presents the coefficients $\gamma_{1,m}$ and $\gamma_{2,m}$ on the month-MIN_TCH and month-MAJ_TCH interactions from (2), which represent the change in the dependent variable in each month relative to May for minor and major teaching hospitals, respectively, relative to the same change for non-teaching hospitals. These results are shown graphically in Figures 2 and 3 for the change in the risk-adjusted LOS and risk-adjusted mortality, respectively, for minor and major teaching hospitals relative to the change for non-teaching hospitals.

A positive coefficient $\gamma_{1,m}$ ($\gamma_{2,m}$) indicates that, on average, minor (major) teaching hospitals experience a larger increase in the outcome measure in a particular month (relative to May) than do non-teaching hospitals in the same month (also relative to May). For example, in Column 1, the value of 0.028 for the coefficient on minor teaching hospitals in September, $\gamma_{1,9}$, suggests that the change in LOS from May to September is 0.028 days greater for minor teaching hospitals than for non-teaching hospitals. To simplify the discussion, we do not always reiterate that the effects and coefficients being discussed are relative to non-teaching hospitals, but this should be assumed. We also use "LOS" to refer to risk-adjusted LOS and "mortality" to refer to risk-adjusted mortality.

5.1.1 Risk-adjusted LOS

In terms of LOS (Table 2 Column 1), the coefficient on minor teaching hospitals in July, $\gamma_{1,7}$, is 0.028 and is significant at the 5% level. The coefficient on major teaching hospitals in July, $\gamma_{2,7}$, is 0.046 and is significant at the 1% level. These results suggest an increase in costs of roughly 0.6% to 0.9% following the July turnover at minor and major teaching hospitals, respectively, if we assume that LOS is proportional to hospital costs (Fine et al., 2000). This offers support for Hypothesis 1, as the cohort turnover of residents in July is associated with a negative performance effect with respect to resource utilization as measured by LOS.

Figure 2: Change in Risk-adjusted Length of Stay Relative to Non-Teaching Baseline



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.

For each month in July through September, the estimated coefficients, $\gamma_{1,m}$, on LOS for minor teaching hospitals remain significantly greater than the May baseline. By October, the change in LOS for minor

Table 2: Effects of Cohort Turnover on Hospital Performance, Using Minor and Major Teaching Categories

	Change in Dependent Variable Relative to Non-teaching Baseline (Reference=May)			
	Risk-adjusted LOS		Risk-adjusted Mortality	
Minor Teaching				
January	-0.006 (0.013)		-0.012 (0.026)	
February	-0.009 (0.011)		-0.033 (0.025)	
March	-0.001 (0.010)		-0.002 (0.025)	
April	-0.006 (0.010)		-0.012 (0.025)	
June	0.005 (0.015)		0.023 (0.021)	
July	0.028 (0.011)	**	0.030 (0.023)	
August	0.022 (0.011)	**	0.003 (0.021)	
September	0.028 (0.011)	**	0.032 (0.023)	
October	0.003 (0.012)		0.013 (0.021)	
November	0.010 (0.011)		0.001 (0.021)	
December	0.009 (0.012)		-0.001 (0.024)	
Major Teaching				
January	-0.029 (0.017)	*	-0.081 (0.031)	***
February	-0.038 (0.018)	**	-0.123 (0.027)	***
March	-0.034 (0.020)	*	-0.083 (0.026)	***
April	-0.013 (0.015)		-0.056 (0.025)	**
June	-0.006 (0.017)		0.018 (0.026)	
July	0.046 (0.016)	***	0.044 (0.026)	*
August	0.053 (0.016)	***	0.034 (0.027)	
September	0.049 (0.015)	***	0.003 (0.028)	
October	0.025 (0.018)		0.020 (0.027)	
November	0.025 (0.017)		-0.032 (0.027)	
December	-0.011 (0.019)		-0.029 (0.027)	
Mean of Dependent Variable				
Minor Teaching	4.73		2.06	
Major Teaching	5.20		2.21	
Observations	87,707		87,707	
Adjusted R-squared	0.719		0.336	

Notes. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% levels, respectively. 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.

teaching hospitals relative to non-teaching hospitals returns to being statistically indistinguishable from the May baseline. The coefficients on minor teaching hospitals in August, September, November, and December are not each significantly different from that for July, which suggests that relative LOS for minor teaching hospitals increases in July and remains at that higher level for a few months. By January, LOS in minor teaching hospitals is back to its value in the baseline May period, and the coefficients for minor teaching hospitals in January through April are each significantly smaller than each of the July, August, and September coefficients at the 5% level. This general reduction in the estimated coefficients, $\gamma_{1,m}$, over the course of the academic year suggests that residents at minor teaching hospitals may benefit from experience-based improvement in performance (i.e., learning) (Reagans et al., 2005) as measured by LOS, our measure of resource utilization and cost.

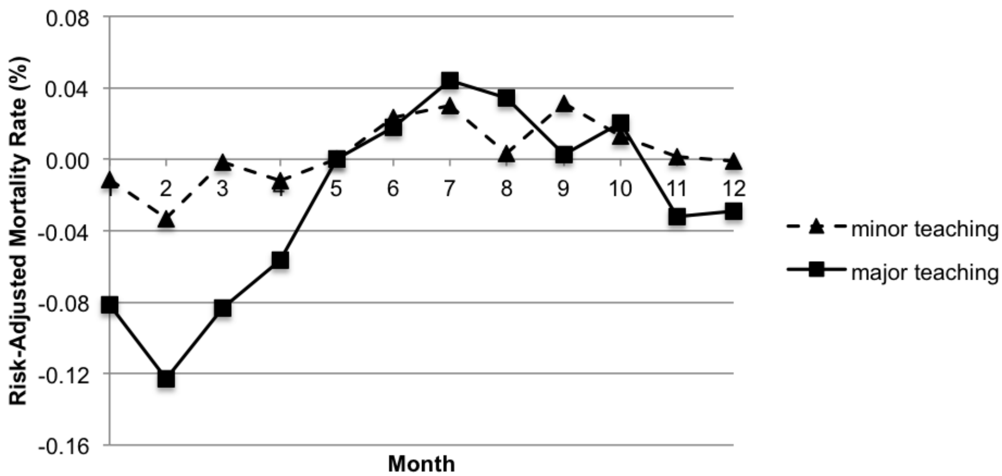
Consistent with Hypothesis 2, we find that major teaching facilities experience a greater increase in LOS relative to minor teaching hospitals. Specifically, major teaching hospitals experience a positive and significant (at the 1% level) increase in LOS relative to that for non-teaching hospitals following the July turnover, and the effect remains for approximately three months. The magnitude of this July effect ($\gamma_{2,7}$ = 0.046) is nearly twice that for minor teaching hospitals ($\gamma_{1,7}$ = 0.028), though the two coefficients are not significantly different at the 5% level. As with minor teaching hospitals, the effects for major teaching hospitals in each of the months from August through February decline in magnitude over time, though the coefficients ($\gamma_{2,m}$) in August through November are not statistically distinguishable from the July estimate for major teaching hospitals. By January, LOS falls to the point where it is significantly lower than that for May, and this coefficient is significantly lower than each of the July through November coefficients on major teaching hospitals at the 1% level. These results provide additional support for the contention that residents learn over the course of the academic year.

5.1.2 Risk-adjusted mortality

In addition to resource utilization, we also consider the impact of the July turnover on clinical quality. Column 2 of Table 2 presents the coefficients $\gamma_{1,m}$ ($\gamma_{2,m}$) on the month-MIN_TCH (month-MAJ_TCH) interactions from our estimation of (2) using mortality as the dependent variable. We find that minor teaching hospitals do not experience significant changes in mortality compared to non-teaching hospitals during the course of the academic year and that major teaching facilities experience an increase in their relative mortality in July ($\gamma_{2,7}$ = 0.044) that is significant at the 10% level. Furthermore, we do not see evidence of significant effects in the months after July at major teaching hospitals. These results offer at most only limited support for Hypothesis 1 with respect to mortality.

Evidence of learning is again present in the coefficients for the remainder of the academic year at major teaching hospitals. Although the levels in August through December are not significantly different from the May baseline, the levels for November and December are each significantly lower than that for July at the 1% level. In addition, each of the coefficients ($\gamma_{2,m}$) on major teaching hospitals in 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.

Figure 3: Change in Risk-adjusted Mortality Relative to Non-Teaching Baseline



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.

5.1.3 Anticipation effect

In major teaching hospitals, we also find evidence of a substantial anticipation effect that begins earlier in the calendar year than the actual cohort turnover in July. After a period of general decline in relative LOS from August through February, we find that relative LOS in major teaching hospitals begins to increase gradually starting in March before reaching its peak 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 LOS is beginning before the actual cohort turnover in July. Given the relatively smooth and continuous nature of this pattern over the course of first half of the year—where an initial increase in LOS is not followed by a subsequent decrease until after the July turnover—it is consistent with an anticipation effect related to the July turnover. With regard to mortality, this effect is even more pronounced. Following a period of decline in relative mortality from July through February, we find that relative mortality in major teaching hospitals begins to increase gradually starting in March before reaching its peak in July. Each change from February to March, March to April, April to May, and May to June is also not statistically distinguishable from the change between June and July.

Though our data do not allow for conclusive explanations of what drives this anticipation effect, anecdotal evidence from interviews conducted with residency program directors and chief residents suggests that the increase in LOS and mortality at teaching hospitals relative to non-teaching hospitals during this anticipatory period may be explained by either or both of the following: (a) 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 (i.e., preparing for the upcoming cohort turnover in July) or (b) a decline in performance at teaching hospitals as they become more tolerant of operating at lower levels of staffing while senior residents or fellows begin to “wind down” their appointments.

With respect to the first explanation, some residency programs begin giving first-year residents (i.e., interns) more clinical responsibilities toward the end of their first academic year, when they are perceived to be capable of assuming greater responsibility and independence. This may involve the first-year resident directly assuming the care of patients coming from the ED, being responsible for a greater number of patients per day on average (e.g., one program director estimated that interns are responsible for 7-8 patients per day in July as opposed to 10 patients per day in February), and taking a greater role in the leading the clinical decision-making process. Among surgical specialties in particular, first-year residents may be granted more time in the operating room in the latter months of the academic year, which has direct implications for the amount of time they can spend completing their non-operating room responsibilities, such as discharge planning and case management. Furthermore, to prepare them to assume their roles as second-year residents in July, some programs have each first-year resident serve as a second-year resident for a day in the spring months, typically on two or three separate occasions per first-year resident. This may involve leading medical rounds and clinical decision-making, supervising other first-year residents, and reviewing orders.

With respect to the second explanation, some residency programs report changes in the composition of attending physicians over the course of the academic year, such that more experienced attending physicians are scheduled to be more present earlier in the academic year when there is a greater need for resident supervision, leaving the less experienced attending physicians more likely to have a greater presence later in the academic year. Others describe being more tolerant of lower levels of staffing for attending physicians and residents later in the academic year. Lastly, senior residents may also experience some degree of burnout at the end of the year, making them more likely to allow a more junior resident to assume a greater amount of responsibility (e.g., more time in the operating room).

Unlike earlier in the academic year, in the several months prior to the July turnover there may not be an expectation of an increase in LOS and mortality and, thus, teaching hospitals may not have extra precautions in place to mitigate any potential negative effects on operational performance. Whether due to a transition of responsibility or a decline in performance, our results suggest that the decline in operational performance that is commonly attributed to the period immediately following the July turnover may actually impact hospital performance beginning several months *prior* to the actual turnover event.

5.2 Mitigating the Impact of Cohort Turnover on Operational Performance

We estimate (3) and (4) to examine the extent to which hospitals may be able to mitigate the negative impact of the July turnover on operational performance. In Columns 1 and 2 of Table 3, we present coefficients $\lambda_{2,m}$ and $\lambda_{3,m}$ on the month-$MAJ\_TCH\_NONMAGNET$ and month-MAJ_TCH_MAGNET interactions from (3), which represent the change in the dependent variable in each month relative to May for major teaching hospitals without and with ANCC magnet recognition, respectively, relative to the same change for non-teaching hospitals. In Columns 3 and 4 of Table 3, we present coefficients $\rho_{4,m}$ and $\rho_{5,m}$ on the month-$MAJ\_TCH\_LO\_QA$ and month-$MAJ\_TCH\_HI\_QA$ interactions from (4), which represent the change in the dependent variable in each month relative to May for major teaching hospitals with low and high intensities of quality assurance, respectively, relative to the same change for non-teaching hospitals.

Table 3: Effects of Cohort Turnover on Hospital Performance, Stratifying Major Teaching Hospitals by ANCC Magnet Recognition and Intensity of Potential Quality Assurance

	ANCC Magnet Recognition (Yes vs. No)				**Intensity of Potential Quality Assurance (High vs. Low)**			
	Risk-adjusted LOS		Risk-adjusted Mortality		Risk-adjusted LOS		Risk-adjusted Mortality	
Major Teaching; No/Low								
January	-0.054 (0.023)	**	-0.043 (0.037)		-0.044 (0.023)	*	-0.079 (0.039)	**
February	-0.065 (0.020)	***	-0.104 (0.032)	***	-0.061 (0.021)	***	-0.141 (0.033)	***
March	-0.061 (0.023)	***	-0.083 (0.032)	**	-0.085 (0.021)	***	-0.087 (0.028)	***
April	-0.024 (0.019)		-0.031 (0.033)		-0.044 (0.020)	**	-0.086 (0.028)	***
June	-0.018 (0.020)		0.021 (0.034)		-0.025 (0.020)		0.022 (0.034)	
July	0.050 (0.020)	**	0.074 (0.033)	**	0.066 (0.019)	***	0.060 (0.033)	*
August	0.058 (0.020)	***	0.050 (0.037)		0.061 (0.021)	***	0.022 (0.034)	
September	0.050 (0.019)	***	0.028 (0.040)		0.067 (0.021)	***	0.036 (0.036)	
October	0.028 (0.023)		0.041 (0.036)		0.033 (0.026)		0.061 (0.033)	*
November	0.012 (0.022)		-0.022 (0.036)		0.029 (0.022)		0.011 (0.035)	
December	-0.007 (0.023)		0.004 (0.034)		-0.012 (0.021)		-0.010 (0.032)	
Major Teaching; Yes/High								
January	0.005 (0.022)		-0.136 (0.049)	***	-0.020 (0.024)		-0.072 (0.042)	*
February	0.000 (0.026)		-0.150 (0.040)	***	-0.018 (0.027)		-0.100 (0.039)	**
March	0.004 (0.032)		-0.084 (0.037)	**	0.019 (0.032)		-0.083 (0.038)	**
April	0.001 (0.022)		-0.092 (0.031)	***	0.021 (0.023)		-0.029 (0.035)	
June	0.011 (0.019)		0.013 (0.034)		0.013 (0.020)		0.011 (0.033)	
July	0.040 (0.021)	*	0.001 (0.035)		0.027 (0.023)		0.025 (0.036)	
August	0.046 (0.021)	**	0.012 (0.035)		0.040 (0.022)	*	0.042 (0.037)	
September	0.048 (0.021)	**	-0.033 (0.033)		0.028 (0.020)		-0.030 (0.039)	
October	0.021 (0.024)		-0.010 (0.032)		0.011 (0.022)		-0.028 (0.040)	
November	0.043 (0.025)	*	-0.046 (0.032)		0.016 (0.026)		-0.077 (0.039)	**
December	-0.016 (0.031)		-0.075 (0.036)	**	-0.011 (0.028)		-0.057 (0.042)	
Mean of Dependent Variable								
Major Teaching; No/Low	5.34		2.26		5.30		2.24	
Major Teaching; Yes/High	4.96		2.13		5.09		2.17	
Observations	87,707		87,707		87,707		87,707	
Adjusted R-squared	0.719		0.335		0.719		0.336	

Notes. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% levels, respectively. 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. All regressions also include interactions between the minor teaching hospital category and month effects, which are also not shown in the table for ease of presentation, but are identical to the coefficients for minor teaching hospitals shown in Table 2. 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.

5.2.1 ANCC magnet recognition

In Column 1 of Table 3, we consider the differential impact of the July turnover on the LOS of major teaching hospitals without and with ANCC magnet recognition. We find that the coefficient on non-magnet certified major teaching hospitals in July, $\lambda_{2,7}$, is 0.050 ($p < 0.05$) whereas the same coefficient on magnet certified major teaching hospitals in July, $\lambda_{3,7}$, is 0.040 ($p < 0.10$). We note that the estimated magnitude of this coefficient for magnet certified facilities is less than that of the coefficient for non-magnet certified facilities, though the two coefficients are not significantly different from each other at conventional levels ($p < 0.75$). In addition, while there is evidence of an anticipation effect at non-magnet certified major teaching hospitals, we find no evidence of an anticipation effect at magnet certified major teaching hospitals. This suggests that magnet certified major teaching hospitals may be able to mitigate some of the negative effects of the July turnover on LOS.

In Column 2 of Table 3, we see the differential impact of the July turnover on mortality in major teaching hospitals without and with ANCC magnet recognition. Here, the coefficient on non-magnet certified major teaching hospitals in July, $\lambda_{2,7}$, is 0.074 ($p < 0.05$) whereas the same coefficient on magnet certified major teaching hospitals in July, $\lambda_{3,7}$, is 0.001 and not statistically significant at conventional levels ($p < 0.97$). These two coefficients are statistically significantly different from each other at the 10% level. Thus, while the performance of magnet certified major teaching hospitals as measured by clinical quality does not decline at the time of the July turnover, non-magnet certified facilities experience a significant increase in their relative mortality in July. This increase of 0.074 percentage points represents a 3.3% increase relative to the average mortality rate of 2.26% for non-magnet certified major teaching hospitals. Nevertheless, for both major teaching hospitals without and with ANCC magnet recognition, we continue to find evidence of an anticipation effect that begins in March and persists until the July turnover.

5.2.2 Potential quality assurance

In Column 3 of Table 3, we consider the differential impact of the July turnover on the LOS of major teaching hospitals with low versus high intensities of potential quality assurance by nurses. We find that the July coefficient on major teaching hospitals with low intensities of quality assurance, $\rho_{4,7}$, is 0.066 ($p < 0.01$) whereas the same coefficient on facilities with high intensities of quality assurance, $\rho_{5,7}$, is 0.001 and not statistically significant at conventional levels ($p < 0.25$). We note the magnitude of this coefficient for those facilities with low intensities of quality assurance is significantly greater than that for facilities with high intensities of quality assurance, though the two coefficients are not significantly different from each other at conventional levels ($p < 0.19$). In addition, though there appears to be a strong anticipation effect at major teaching hospitals with low intensities of quality assurance, we find no evidence of an anticipation effect at those facilities with high intensities of quality assurance. This suggests that facilities with high intensities of quality assurance are able to mitigate some of the negative effects of the July turnover on LOS.

Column 4 presents the differential impact of the July turnover on mortality in major teaching hospitals with low versus high intensities of quality assurance. Here, the July coefficient on major teaching hospitals with low intensities of quality assurance, $\rho_{4,7}$, is 0.060 and statistically significant at the 10% level, whereas the same coefficient on facilities with high intensities of quality assurance, $\rho_{5,7}$, is 0.025 and not statistically significant at conventional levels ($p < 0.50$). We note the magnitude of this coefficient for those facilities with low intensities of quality assurance is more than twice that of the coefficient for facilities with high intensity of quality assurance, although these two coefficients are not significantly different from each other at conventional levels ($p < 0.44$). For both facilities with low and high intensities of quality assurance, we continue for find evidence of an anticipation effect that begins in March.

Altogether, these analyses suggest that, to some extent, major teaching hospitals that invest in higher levels of nursing quality and higher intensities of quality assurance may be able to mitigate the negative effects of the July turnover on operational performance, particularly with respect to the effects around the time of the July turnover. These facilities do not exhibit a decrease in performance with regards to LOS or mortality relative to non-teaching controls that is significant at the 5% level. In addition, we note that major teaching hospitals that are either magnet certified or have high intensities of quality assurance are also able to buffer against the anticipation effect with respect to LOS, which continues to manifest in the months preceding the July turnover at non-magnet certified facilities and those with low intensities of quality assurance. Nonetheless, even those facilities that are either magnet certified or have high intensities of quality assurance experience an anticipation effect with respect to mortality, which may indicate that even these facilities are not sufficiently buffering against the negative impact on quality that accompanies the transition of responsibilities that occurs in the months preceding the July turnover. This offers some degree of support for Hypothesis 3, which suggests that organizations with structures and processes that facilitate knowledge transfer from departing to entering workers may be able to mitigate the negative effects of cohort turnover on operational performance.

5.3 Extensions and Robustness

Though suggestive of declines in operational performance following cohort turnover, our findings are potentially consistent with alternate explanations. Therefore, we examine the robustness of our main findings and extend our results through several additional analyses (tables available in the online supplement).[3]

5.3.1 Testing for patient self-selection and increased transfers

One alternate explanation of our findings is that elective patients may recognize July to be a time of transition 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 to researchers) could 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 cohort turnover in July.

We offer three tests of the selection hypothesis in Columns 1 through 3 of Table 4. First, if elective patients are, in fact, selecting away from teaching hospitals in July, those 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* (not risk-adjusted) mortality and LOS relative to non-teaching facilities in July. We thus estimate three separate regressions of the same form as (2) but with the number of hospital admissions, expected mortality, and expected LOS respectively, as the dependent variable.

With the number of hospital admissions as the dependent variable (Column 1), the results are mixed. In particular, the July coefficient on minor teaching hospitals is negative and significant ($\gamma_{1,7}$ = -6.9, $p < 0.05$), though its magnitude is quite small and represents a decrease of only 0.55% relative to the average of 1,244 admissions per month for minor teaching hospitals. Meanwhile, the July coefficient on major teaching hospitals—where we see the most substantial effects of cohort turnover in our base regressions—is not statistically significantly different from zero at conventional levels ($p < 0.99$). For the second regression with 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. For the third regression with expected LOS as the dependent variable (Column 3), we again do not find evidence of patient selection in July as measured by a change in expected LOS. Altogether, there is no clear evidence of patient selection away from the hospitals that are most affected by the July turnover.

[3] In addition to the analyses presented in this section, we also considered evaluating the robustness of our findings by assessing the relative changes in 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. Thus, we limit our presentation of additional analyses in this section to those for which we are able to match the level at which the key independent and dependent variables are observed.

Table 4: Effects of Cohort Turnover on Hospital Performance, Using Minor and Major Teaching Categories for Tests of Patient Self-Selection and Increased Transfers

	Total Admissions		Expected Mortality		Expected LOS		Transfer Rate	
Minor Teaching								
January	7.5 (3.4)	**	-0.008 (0.018)		-0.010 (0.013)		-0.04 (0.05)	
February	-56.1 (5.2)	***	0.024 (0.018)		-0.002 (0.014)		0.02 (0.05)	
March	14.9 (3.1)	***	0.012 (0.015)		-0.010 (0.011)		-0.02 (0.04)	
April	-21.4 (2.8)	***	-0.017 (0.017)		-0.015 (0.011)		-0.01 (0.03)	
June	-17.1 (2.7)	***	0.001 (0.016)		0.009 (0.011)		-0.04 (0.03)	
July	-6.9 (3.2)	**	0.016 (0.020)		0.013 (0.013)		-0.03 (0.04)	
August	-4.0 (3.4)		0.043 (0.017)	**	0.018 (0.018)		-0.03 (0.04)	
September	-26.2 (3.4)	***	0.023 (0.017)		0.003 (0.012)		-0.09 (0.04)	**
October	6.1 (3.4)	*	0.029 (0.017)	*	-0.006 (0.013)		-0.12 (0.06)	**
November	-40.2 (4.0)	***	-0.001 (0.017)		-0.025 (0.016)		-0.15 (0.06)	***
December	-24.0 (4.0)	***	0.002 (0.018)		-0.023 (0.013)	*	-0.09 (0.05)	*
Major Teaching								
January	11.5 (13.8)		-0.060 (0.020)	***	-0.060 (0.026)	**	-0.04 (0.10)	
February	-118.8 (16.7)	***	-0.014 (0.020)		-0.045 (0.026)	*	0.08 (0.08)	
March	22.2 (11.1)	**	-0.024 (0.018)		-0.025 (0.026)		0.14 (0.07)	**
April	-44.8 (6.8)	***	-0.037 (0.019)	**	0.021 (0.029)		0.05 (0.05)	
June	-28.5 (6.2)	***	0.000 (0.016)		0.015 (0.020)		-0.01 (0.05)	
July	-0.3 (11.9)		0.020 (0.021)		0.005 (0.025)		-0.03 (0.06)	
August	2.9 (11.8)		0.047 (0.018)	***	0.002 (0.025)		-0.01 (0.07)	
September	-47.8 (13.4)	***	0.044 (0.020)	**	-0.021 (0.026)		-0.11 (0.09)	
October	12.5 (13.6)		0.035 (0.027)		0.011 (0.054)		-0.31 (0.14)	**
November	-67.4 (13.8)	***	0.006 (0.035)		-0.018 (0.046)		-0.28 (0.12)	**
December	-53.2 (15.6)	***	-0.067 (0.021)	***	-0.073 (0.026)	***	-0.11 (0.11)	
Mean of Dependent Variable								
Minor Teaching	1,244		2.09		4.75		4.14	
Major Teaching	2,031		1.98		4.92		5.24	
Observations	87,710		87,707		87,707		87,710	
Adjusted R-squared	0.977		0.744		0.773		0.785	

Notes. Transfer rate is calculated as Transfers/Total Admissions $\times$ 100. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% levels, respectively. 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.

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

5.3.2 Results for patients admitted from the Emergency Department

Although we do not find strong support for the selection hypothesis, we conduct an additional robustness check by limiting our sample to only the inpatient cases that arrive through a hospital's ED. These cases, which constitute 36% to 48% of inpatient cases depending on the year, are arguably less susceptible to endogeneity concerns than elective cases scheduled in advance because they are less likely to involve a patient choosing among hospitals. 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 2). As a result, we would expect the magnitude of the July effect to be even greater than those from our main results.

Table 5: Effects of Cohort Turnover on Hospital Performance, Using Minor and Major Teaching Categories for Patients Admitted from the Emergency Department

	Change in Dependent Variable Relative to Non-teaching Baseline (Reference=May)			
	Risk-adjusted LOS		Risk-adjusted Mortality	
Minor Teaching				
January	0.020 (0.016)		-0.010 (0.056)	
February	-0.004 (0.015)		-0.022 (0.050)	
March	-0.002 (0.014)		0.014 (0.055)	
April	-0.008 (0.014)		0.004 (0.054)	
June	0.023 (0.013)	*	0.028 (0.044)	
July	0.052 (0.014)	***	0.064 (0.045)	
August	0.031 (0.015)	**	-0.001 (0.044)	
September	0.019 (0.017)		0.047 (0.046)	
October	0.007 (0.015)		0.037 (0.043)	
November	0.027 (0.016)	*	0.027 (0.042)	
December	0.020 (0.016)		-0.016 (0.049)	
Major Teaching				
January	-0.029 (0.030)		-0.102 (0.056)	*
February	-0.062 (0.030)	**	-0.176 (0.053)	***
March	-0.020 (0.041)		-0.070 (0.051)	
April	-0.017 (0.022)		-0.060 (0.049)	
June	0.026 (0.019)		0.073 (0.052)	
July	0.081 (0.022)	***	0.142 (0.051)	***
August	0.065 (0.026)	**	0.083 (0.060)	
September	-0.001 (0.029)		-0.029 (0.059)	
October	0.050 (0.029)	*	0.018 (0.062)	
November	0.029 (0.029)		-0.013 (0.061)	
December	0.022 (0.031)		0.008 (0.060)	
Mean of Dependent Variable				
Minor Teaching	5.13		3.22	
Major Teaching	5.78		3.33	
Observations	76,647		76,643	
Adjusted R-squared	0.811		0.235	

Notes. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% levels, respectively. 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.

We estimate a regression of the same form as (2) on this subset of cases. The results are similar to our main results (Table 5). In terms of risk-adjusted LOS, the July coefficients on minor and major teaching hospitals are 0.052 and 0.081, respectively, both of which are significant at the 1% level. In addition, in line with our expectations, each of these coefficients is greater in magnitude than those from our main results. The estimated coefficients on relative LOS in minor and major teaching hospitals 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. We also continue to find an anticipation effect at major teaching hospitals that begins in March prior to the July turnover.

The results for mortality are also similar to our main results, although here we find evidence of a significant July effect in major teaching hospitals. The July coefficient on mortality in major teaching hospitals is 0.142, which represents a 4.3% increase relative to the average mortality rate of 3.33 for major teaching hospitals. The estimated coefficients on relative mortality in major teaching hospitals 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 learning over the course of the academic year and a turnover anticipation effect that begins in the several months preceding July.

5.3.3 Results using alternate model specifications

A potential concern with our main empirical specification is that is relies on observations at the hospital-month level. Given 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 clinical quality. To address this possibility, we modify our main specification in (2) 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 main specification, we isolate June as a transitional period because the residency changeover begins in late June for many hospitals.

Our findings highlight the robustness of our main results (Table 6). We find evidence of a substantial increase in relative LOS at teaching hospitals in the period just following the cohort turnover. The July-August coefficients on minor and major teaching hospitals are 0.025 and 0.049, respectively, which are significant at the 5% and 1% levels, respectively. The magnitude of this effect increases with the hospital's teaching intensity, with the July-August coefficient on major teaching hospitals being significantly different from the minor teaching coefficient for the same period at the 10% level. This increase in LOS is sustained before it returns to the May level in the November-December period. Again, we observe a slight anticipation effect in major teaching hospitals, where there is an increase in relative LOS between the March-April period and the May baseline. For mortality, we find no evidence of a significant change in the July-August period at minor teaching hospitals. At major teaching hospitals, we observe an increase in relative mortality in the

Table 6: Effects of Cohort Turnover on Hospital Performance, Using Minor and Major Teaching Categories with Multi-Month Periods

	Change in Dependent Variable Relative to Non-teaching Baseline (Reference=May)			
	Risk-adjusted LOS		Risk-adjusted Mortality	
Minor Teaching				
Jan-Feb	-0.007 (0.011)		-0.022 (0.023)	
Mar-Apr	-0.004 (0.009)		-0.007 (0.023)	
June	0.005 (0.015)		0.023 (0.021)	
Jul-Aug	0.025 (0.010)	**	0.017 (0.020)	
Sep-Oct	0.016 (0.010)		0.022 (0.019)	
Nov-Dec	0.010 (0.010)		0.000 (0.020)	
Major Teaching				
Jan-Feb	-0.034 (0.015)	**	-0.101 (0.025)	***
Mar-Apr	-0.024 (0.015)		-0.070 (0.022)	***
June	-0.006 (0.017)		0.018 (0.026)	
Jul-Aug	0.049 (0.014)	***	0.039 (0.023)	*
Sep-Oct	0.037 (0.015)	**	0.012 (0.025)	
Nov-Dec	0.007 (0.017)		-0.031 (0.023)	
Mean of Dependent Variable				
Minor Teaching	4.73		2.06	
Major Teaching	5.20		2.21	
Observations	87,707		87,707	
Adjusted R-squared	0.719		0.334	

Notes. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% levels, respectively. 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.

July-August period ($\gamma_{2,7-8} = 0.039$) that is significant at the 10% level. In addition, we find evidence of an anticipation effect that begins in the March-April period.

Another potential concern is that the ratio of FTE nurses per FTE resident may be highly correlated with the teaching intensity of a hospital. To address this issue, we conduct additional analyses in which we match hospitals on teaching intensity (FTE residents per bed) and exploit the remaining variation in the intensity of quality assurance by separating each matched pair into a higher- and lower-quality assurance hospital based on their relative values of FTE nurses per FTE resident. With this alternate specification, the correlation between teaching intensity and quality assurance reduces from 0.47 to 0.32. This approach yields results that are similar to our main findings.

Finally, because LOS and mortality could also be affected by the extent to which a hospital is more crowded (i.e., operating at a higher level of capacity utilization), we also estimate a model that accounts for hospital crowding. 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 main results to be robust to this alternate specification.

6 Discussion and Conclusions

We utilize data on hospital inpatient discharges to examine the impact of cohort turnover on operational performance. Specifically, we examine the impact of the July turnover of resident physicians in teaching hospitals on hospital operational performance as measured by resource utilization and clinical quality. We find that that the effects of the cohort turnover appear not only in the months during and after the turnover event but also in the months *preceding* it.

First, at the time of cohort turnover in July and in the following months, we find a reduction in operational performance at teaching hospitals as evidenced by an increase in resource utilization but only limited evidence of a decrease in clinical quality. At minor teaching hospitals, we find a relative increase in risk-adjusted LOS that lasts for a few months but no accompanying increase in the risk-adjusted mortality rate. At major teaching hospitals, we find a significant increase in risk-adjusted LOS and limited evidence of an increase in risk-adjusted mortality.

Second, in the months preceding the cohort turnover, we find that teaching hospitals exhibit a gradual decrease in operational performance relative to non-teaching hospitals, which presents as an increase in the relative risk-adjusted LOS and mortality rate beginning in March. This pre-July decrease in operational performance at teaching hospitals may result from institutional efforts to preempt the turnover by increasing the responsibilities of remaining workers (e.g., making first-year residents responsible for holding the admission pager) and from departing workers winding down their current duties and becoming involved in the process of transitioning to new positions at other hospitals. We refer to this phenomenon as an anticipation effect.

Particularly at major teaching hospitals, the effect of the July turnover on resource utilization is substantial. Average risk-adjusted LOS—our proxy for resource utilization and, therefore, cost—for the average major teaching hospital increases by 0.9% following the July turnover and remains higher for several months after July. Given the slim profit margins of most teaching hospitals in the U.S. (American Hospital Association, 2015), this is an economically significant increase for multiple months each year. The increase in risk-adjusted mortality is small but also not trivial, being roughly 2.0% (percent, not percentage points) in July. Determining the social cost of this increase in mortality requires assumptions—beyond the scope of this paper—about the expected longevity and quality of life of these individuals in the absence of the July turnover.

6.1 Contributions

Our findings contribute to the literature on turnover, performance, and productivity in several ways. First, we define and examine cohort turnover as a phenomenon distinct from individual or collective turnover and empirically estimate its impact on operational performance. Most studies on turnover tend to focus on the turnover of individuals rather than large cohorts. In addition, they typically consider the causes of turnover rather than examining their consequences, perhaps because the endogenous nature of individual turnover makes the phenomenon difficult to study empirically. Nevertheless, it is important to examine the impact of the turnover of cohorts on performance because of the sizable nature of such events and their occurrence in several organizational settings including hospitals, political administrations, and military deployments.

Second, we consider the broader implications of cohort turnover by looking beyond the immediate time window of the turnover event. Because cohort turnover is, by definition, planned in advance and known to occur at a specific point in time, it may impact operational performance in advance of the actual turnover event. If there were a decline in operational performance that begins earlier than the actual turnover itself, failing to note this earlier decline in performance would result in an underestimate of the true magnitude of the effect associated with the cohort turnover. Accordingly, we identify a decline in hospital operational performance in the several months leading up to the July turnover. We define this as an anticipation effect, and are aware of no prior work that examines whether the effect of cohort turnover might precede its formal occurrence.

Third, we improve upon the methodological approaches employed by papers in the medical literature that have previously examined the effect of the July turnover on various clinical outcomes. Specifically, many prior studies on this topic do not adjust for risk, adjust for seasonal variation, or use suitable concurrent controls. All of these are necessary adjustments given the observational design of these studies. To adjust for these factors, we risk adjust our dependent variables and use a control group of non-teaching hospitals as a baseline category. In addition, prior work in the medical literature typically examines the presence of a July effect for a specific category of high-risk patients (e.g., very low weight infants, intensive care unit patients, spinal surgery patients, trauma patients). 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. Further, because the clinical outcomes that these studies aim to explain are typically measured at the department level—whereas teaching intensities are based on hospital-level figures that may vary across specific clinical areas within a hospital (e.g., a hospital may have a more substantial residency program in surgery than in radiology)—it is not clear that there is always a "match" in the level at which the dependent and key independent variables are observed. We avoid this mismatch by assessing our outcome measures at the hospital level.

6.2 Managerial Implications and Conclusions

Our results suggest that cohort turnover should be managed not only at the time of, but also during the period leading up to, the actual turnover event. We also find that operational performance declines may be mitigated if appropriately managed. These mitigation efforts may require managers to consider the size of the cohort relative to the size of the organization.

To mitigate the negative effects of cohort turnover on operational performance, managers should consider implementing structures and processes that facilitate knowledge transfer from departing to entering workers. This can be done by increasing the overall quality of the workforce that is not turning over and can thus transfer knowledge to new workers. Although this may require a significant investment in terms of financial and organizational resources, it is likely to have positive spillover effects beyond an organization's increased ability to withstand the negative effects of cohort turnover. Another approach, which may have a more immediate impact, would be to increase the intensity of quality assurance. One way to do this is by increasing the relative staffing level of those workers who are not subject to the cohort turnover who are in a position to serve as an operational safeguard for the output of new workers (Hausknecht et al., 2009; Reagans et al., 2005). In the case of teaching hospitals, these may be the nurses who work closely with residents but do not tend to turn over as a cohort in July. Given our finding that the effects of cohort turnover are increasing in its relative intensity, hospital managers might consider increasing the nurse-to-resident ratios especially in those departments that are more resident intensive within the facility.

One question not resolved by this study is the degree to which managers should be *concerned* about turnover-related declines in operational performance. 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 operational performance throughout the year. It is likely that no system can guarantee residents will be as productive and high performing at the beginning of their tenure as they will be at its end. Ultimately, a key question is whether declines in operational performance are higher than necessary to train new workers efficiently and whether those effects can be mitigated. Our findings suggest that this may be possible, assuming that the costs of improving the knowledge transfer do not outweigh the resulting benefits in terms of cost and quality performance.

The question of whether there are optimal levels of pre-turnover preparation and post-turnover employee oversight in the face of significant on-the-job training is an important issue worthy of further study in contexts both within and outside the hospital industry. In this paper, we provide some examples of potential approaches for mitigating the impact of cohort turnover. We welcome future work to extend this line of inquiry by considering other ways to mitigate these effects, such as by better utilizing information technology and increasing teamwork and coordination within work groups. Ultimately, even if organizations are not able to reduce the cohort turnover they face, they may be able to take steps to better manage its effects.

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