Anesthesia Care Team Composition and Surgical Outcomes

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CME This article has been selected for the ANESTHESIOLOGY CME Program. Learning objectives and disclosure and ordering information can be found in the CME section at the front of this issue.

ABSTRACT

Background: In the United States, anesthesia care can be provided by an anesthesia care team consisting of nonphysician providers (nurse anesthetists and anesthesiologist assistants) working under the supervision of a physician anesthesiologist. Nurse anesthetists may practice nationwide, whereas anesthesiologist assistants are restricted to 16 states. To inform policies concerning the expanded use of anesthesiologist assistants, the authors examined whether the specific anesthesia care team composition (physician anesthesiologist plus nurse anesthetist or anesthesiologist assistant) was associated with differences in perioperative outcomes.

Methods: A retrospective analysis was performed of national claims data for 443,098 publicly insured elderly (ages 65 to 89 yr) patients who underwent inpatient surgery between January 1, 2004, and December 31, 2011. The differences in inpatient mortality, spending, and length of stay between cases where an anesthesiologist supervised an anesthesiologist assistant compared to cases where an anesthesiologist supervised a nurse anesthetist were estimated. The approach used a quasirandomization technique known as instrumental variables to reduce confounding.

Results: The adjusted mortality for care teams with anesthesiologist assistants was 1.6% (95% CI, 1.4 to 1.8) *versus* 1.7% for care teams with nurse anesthetists (95% CI, 1.7 to 1.7; difference −0.08; 95% CI, −0.3 to 0.1; $P = 0.47$). Compared to care teams with nurse anesthetists, care teams with anesthesiologist assistants were associated with non–statistically significant decreases in length of stay (−0.009 days; 95% CI, −0.1 to 0.1; $P = 0.89$) and medical spending (−$56; 95% CI, −334 to 223; $P = 0.70$).

Conclusions: The specific composition of the anesthesia care team was not associated with any significant differences in mortality, length of stay, or inpatient spending. **(ANESTHESIOLOGY 2018; 129:700-9)**

IN an effort to increase access and reduce healthcare spending, policymakers and researchers are considering alternative care models, such as the expanded use of nonphysician providers (*e.g.*, nurse practitioners and nurse anesthetists).[1–4] Increasing the use of nonphysician providers could reduce costs because, in many settings, they are paid less than physicians for similar services.[5] In addition, in the face of predicted physician shortages[6,7] the expanded use of nonphysician providers could increase access, particularly in underserved areas where physician recruitment is challenging,[8] although whether and to what extent there are shortages in the anesthesia workforce are unclear.[9] However, these potential benefits could be mitigated or even reversed if nonphysician providers are associated with lower quality care and/or more expensive practice patterns.[10] Legislation governing whether and how nonphysician providers are allowed to provide patient care (*e.g.*, scope of practice laws) is typically determined at the state level in the United States, and alteration of existing legislation is the subject of intense legislative debate in many states.

Editor's Perspective

What We Already Know about This Topic

- Both nurse anesthetists and anesthesiologist assistants work together with physician anesthesiologists as part of care teams
- It is unknown whether the specific anesthesia care team composition (physician anesthesiologist plus nurse anesthetist or anesthesiologist assistant) is associated with differences in perioperative outcomes

What This Article Tells Us That Is New

- Using national claims data for 443,000 Medicare beneficiaries, the influence of care team composition on inpatient mortality, inpatient length of stay, and inpatient spending was evaluated
- There were no significant differences in mortality, length of stay, or inpatient spending between the care team models

There are three types of anesthesia providers in the United States: anesthesiologists, who are physicians trained in the specialty of anesthesiology; nurse anesthetists; and

This article is featured in "This Month in Anesthesiology," page 1A. Corresponding article on page 627. Supplemental Digital Content is available for this article. Direct URL citations appear in the printed text and are available in both the HTML and PDF versions of this article. Links to the digital files are provided in the HTML text of this article on the Journal's Web site (www.anesthesiology.org). This work was presented on May 5, 2017, in Washington, D.C., at the 63rd annual meeting of the Association of University Anesthesiologists.

Submitted for publication September 18, 2017. Accepted for publication May 1, 2018. From the Department of Anesthesiology, Pain, and Perioperative Medicine (E.C.S.), the Center for Health Policy and the Center for Primary Care and Outcomes Research (J.M.), and the Department of Health Research and Policy (E.C.S., L.C.B.), Stanford University School of Medicine, Stanford University, Stanford, California; the Department of Analytics and Research Services, American Society of Anesthesiologists, Schaumburg, Illinois (T.R.M.); and the National Bureau of Economic Research, Cambridge, Massachusetts (L.C.B.).

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<zdoi;10.1097/ALN.0000000000002275>

anesthesiologist assistants. Nurse anesthetists are nurses who receive additional training in anesthesiology through a nurse anesthetist school. Like physician assistants more generally, anesthesiologist assistants receive training in anesthesiology through an anesthesiologist assistant program at one of 11 universities. The main differences between the two groups fall into three areas: training, licensure, and scope of practice. Both nurse anesthetists and anesthesiologist assistants obtain advanced training in anesthesiology, but nurse anesthetist schools typically require candidates to have a Bachelor of Science in nursing, professional nursing experience, and a valid nursing license. By contrast, anesthesiologist assistant programs allow for any bachelor's degree, as long as certain course requirements are met. Licensing and certification requirements for nurse anesthetists are established by the state nursing board, whereas the state medical board is responsible for licensing and certifying anesthesiologist assistants.

Finally, there are differences in state-level legislation (*i.e.*, scope of practice laws) controlling whether and how anesthesiologist assistants and nurse anesthetists may provide patient care. Current laws allow for nurse anesthetist practice in all 50 states, whereas anesthesiologist assistants may practice in only 16 states and the District of Columbia. In addition, anesthesiologist assistants always provide care under the supervision of an anesthesiologist. For nurse anesthetists, the situation is more complex. In states that have not chosen to "opt out" of federal regulations requiring physician supervision of nurse anesthetists, nurse anesthetists must practice under the supervision of a physician, although not necessarily an anesthesiologist. In states that have opted out of federal regulations requiring physician supervision of nurse anesthetists (see appendix table A.1 in the Supplemental Digital Content, http://links.lww.com/ALN/B729), nurse anesthetists have the *de jure* ability to practice without any physician supervision, although even in these states, *de facto* supervision is common,[11] particularly because hospitals, insurers, patients, and surgeons may impose additional restrictions limiting the ability of nurse anesthetists to practice independently. The scope of practice laws that define the legal ability of nurse anesthetists and anesthesiologist assistants to provide care remain contested at the state level. For example, as of 2013, 17 states had chosen to opt out of federal regulations requiring physician supervision of nurse anesthetists, and the decision to opt out remains contentious in the remaining states.

Although opt out concerns the regulation of nurse anesthetists, in this article, we focus on scope of practice laws governing the use of anesthesiologist assistants. As previously noted, current legislation permits anesthesiologist assistant practice in 16 of 50 states and the District of Columbia, and there are ongoing efforts to increase the number of states where anesthesiologist assistants may practice. Arguments against expanding the number of states where anesthesiologist assistants may practice generally focus on the possibility that health outcomes may be worse when anesthesiologist assistants provide anesthesia care. Although the differences in training and background between nurse anesthetists and anesthesiologist assistants may make this a theoretical possibility, it should be noted that generally, nurse anesthetists and anesthesiologist assistants practice in the setting of an anesthesia care team consisting of a physician anesthesiologist who supervises an nurse anesthetist or anesthesiologist assistant. Thus, the presence of the supervising physician could mitigate any systemic differences in background and training between anesthesiologist assistants and nurse anesthetists.

Ultimately, whether anesthesia care teams with anesthesiologist assistants have poorer outcomes than care teams with nurse anesthetists is an empirical question, and to date, there have been no large-scale studies examining differences in outcomes between anesthesiologist assistants and nurse anesthetists.[12] Understanding whether the specific composition of the anesthesia care team (physician anesthesiologist plus nurse anesthetist or physician anesthesiologist plus anesthesiologist assistant) is associated with differences in outcomes could inform efforts to expand the number of states where anesthesiologist assistants can practice. Moreover, it could also help inform the broader debate over the proper regulation of nonphysician providers. In this study, we used a large data set of administrative health claims to evaluate the hypothesis that there would be differences in outcomes (mortality, length of stay, and costs) between care teams consisting of physician anesthesiologists and anesthesiologist assistants compared to care teams consisting of physician anesthesiologists and nurse anesthetists.

Materials and Methods

Data

The data used for this study consisted of health insurance claims for a random 20% sample of U.S. Medicare beneficiaries enrolled in the traditional fee-for-service Medicare plan. In the United States, Medicare is a public insurance program that primarily provides health insurance for the elderly (persons 65 yr or older), although the program also covers some younger persons with significant disabilities and those with end-stage renal disease. In 2010, more than 80% of Medicare beneficiaries consisted of persons ages 65 yr and older.[13] Generally speaking, Medicare beneficiaries can choose from either a traditional fee-for-service plan, for which the federal agency administering Medicare—the Centers for Medicare and Medicaid Services—is the primary payer, or they can choose to be enrolled in a managed healthcare plan. With the latter, Medicare essentially subcontracts out the provision of health care to private health insurers, who bear all the costs for an individual's care. Roughly two thirds of Medicare beneficiaries are enrolled in the traditional fee-for-service plan.[14] Health insurance claims data for beneficiaries enrolled in the traditional fee-for-service Medicare plan are available for researchers upon approval of a data use agreement with the Centers for Medicare and Medicaid Services and payment of required fees. The Medicare data are highly detailed and include information such as admission and

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discharge dates, discharge diagnosis codes that can be used to identify patient comorbidities, codes for any surgical procedures that were performed, and the total amounts spent during a given admission.

Sample

To construct our sample, we began by using the inpatient file to identify all inpatient admissions with a surgical diagnosis-related group that occurred: (1) between January 1, 2004, and December 31, 2011, and (2) in a state that allowed for anesthesiologist assistant practice during this study period (n = 2,602,686; see Supplemental Digital Content, http://links.lww.com/ALN/B729, appendix table A.1, for a list of these states). We then attempted to match the inpatient claim to a claim submitted by an anesthesia provider by identifying claims submitted by an anesthesiologist, nurse anesthetist, or anesthesiologist assistant that (1) had an appropriate procedure code (Current Procedural Terminology codes 00100 to 01999), (2) had a date of service corresponding to the date of the primary surgical procedure reported on the inpatient claim, and (3) were submitted for the same patient as the patient listed on the inpatient claim. Details on how we performed this match can be found in the data appendix (Supplemental Digital Content, http://links.lww.com/ALN/B729). Ultimately, we were able to find a match for 1,064,591 admissions. Our inability to find a match for nearly half of the surgical admissions is because many diagnoses classified as "surgical" do not always require surgery. For example, one common surgical diagnosis, small bowel obstruction, is often managed without surgery.[15] Moreover, not all surgeries receive care from an anesthesia provider.

From this set of admissions, we then applied several exclusion criteria. First, we excluded patients under 65 yr, to focus on the elderly Medicare population, and patients more than 89 yr, as many established quality measures impose this restriction (n = 223,884).[16] Second, we excluded cases in which the surgical procedure code was missing (n = 25,863). Third, we excluded cases where patient race or sex was unknown (n = 2,382), as well as cases with missing costs (n = 3). Fourth, because the goal of our study was to compare outcomes when anesthesiologist assistants and nurse anesthetists are supervised by physician anesthesiologists, we excluded cases where neither a nurse anesthetist nor anesthesiologist assistant was involved in the patient's care (*i.e.*, provision of care by a physician only; n = 296,511), as well as a small number of cases where both were involved in the patient's care (n = 84). In addition, because nurse anesthetists in opt-out states may potentially practice independent of supervision by an anesthesiologist, we excluded any cases that occurred in an opt-out state in the years after the enactment of opt out (n = 19,567; see appendix table A.1 for a list of the opt-out states and the year of enactment, http://links.lww.com/ALN/B729). Finally, we excluded any surgery types for which we had fewer than 100 observations (n = 31,204), as well as cases from any hospital with fewer than 100 observations (n = 21,995), resulting in a final sample of 443,098 cases representing 353 surgery types from 845 hospitals (see Supplemental Digital Content, http://links.lww.com/ALN/B729, appendix fig. A.1 for a flow chart providing further details on sample construction).

Outcomes

We evaluated three primary outcomes: inpatient mortality, inpatient length of stay, and inpatient spending. Death and length of stay were directly obtained from the claims data, with length of stay being defined as the number of days between the admission and discharge dates plus one (so that a patient admitted and discharged on the same day had a length of stay of one day). For inpatient spending, we summed the total amounts paid to the hospital for the given stay, as well as *all* spending on individual healthcare providers (*e.g.*, spending for the surgeon, the anesthesiologist, and any additional consultants) between the admission and discharge dates. Dollar amounts were adjusted to year 2016 dollars using the consumer price index.[17]

Exposure

Our main independent variable of interest was whether an anesthesiologist assistant or nurse anesthetist was part of the anesthesia care team. We identified this based on the anesthesia claim for the given procedure, which reports the specialty of the anesthesia provider (anesthesiologist, anesthesiologist assistant, or nurse anesthetist).

Additional Variables

We obtained a robust set of additional variables to adjust for potential confounding. First, race, age, and sex were directly obtained from the claims data. Second, using the diagnosis codes reported on the inpatient claim, we used previously described methods[18] to measure the presence of the medical comorbidities (*e.g.*, diabetes, hypertension) that are used to determine the Elixhauser index, an index that is frequently used for risk adjustment.[18,19] A list of the comorbidities we measured is provided in table 1. Finally, we used the primary International Classification of Diseases, Ninth Revision (ICD-9) procedure code reported on the inpatient claim to adjust for the primary surgery that was performed.

Statistical Analyses

To assess differences in characteristics between surgical cases with anesthesiologist assistant and nurse anesthetist involvement, we used a *t* test for continuous variables (*e.g.*, age) and a chi-square test for discrete variables (*e.g.*, comorbidities). However, because of our large sample, even trivially small differences may be statistically significant. Therefore, we used Hedges's g to estimate the magnitude of the standardized difference between the two groups of cases. Specifically, Hedges's g is the actual difference between the means of two groups divided by the population SD,[20] with values of less than 0.2 typically representing small differences between

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Table 1. Sample Summary Statistics

	NA (n = 421,230)	AA (n = 21,868)	*P* Value	Hedges's g
Age, yr	75 (75–75)	75 (75–75)	< 0.001	0.05
Male, %	44.0 (43.8–44.1)	44.0 (43.3–44.6)	0.97	< 0.01
White, %	89.3 (89.2–89.4)	86.1 (85.6–86.5)	< 0.001	0.10
Congestive heart failure, %	10.6 (10.5–10.7)	11.0 (10.5–11.4)	0.10	0.01
Arrhythmia, %	16.8 (16.7–16.9)	16.4 (15.9–16.9)	0.14	0.01
Valvular disease, %	5.4 (5.4–5.5)	6.5 (6.2–6.8)	< 0.001	0.05
Pulmonary circulation disorders, %	1.5 (1.4–1.5)	1.6 (1.5–1.8)	0.06	0.01
Peripheral vascular disease, %	10.2 (10.1–10.3)	11.7 (11.3–12.2)	< 0.001	0.05
Hypertension, uncomplicated, %	55.4 (55.2–55.6)	55.6 (54.9–56.2)	0.70	< 0.01
Hypertension, complicated, %	8.3 (8.3–8.4)	9.8 (9.4–10.2)	< 0.001	0.05
Paralysis, %	0.6 (0.6–0.6)	0.7 (0.6–0.7)	0.17	0.01
Other neurologic disorders, %	4.0 (4.0–4.1)	3.8 (3.5–4.0)	0.07	0.01
Chronic pulmonary disease, %	17.8 (17.7–18.0)	17.6 (17.1–18.1)	0.39	0.01
Diabetes, uncomplicated, %	20.0 (19.8–20.1)	20.6 (20.0–21.1)	0.023	0.02
Diabetes, complicated, %	3.7 (3.6–3.7)	3.7 (3.3–3.9)	0.88	<0.01
Hypothyroidism, %	11.9 (11.9–12.0)	11.6 (11.1–12.0)	0.12	0.01
Renal failure, %	7.8 (7.7–7.9)	9.5 (9.1–9.9)	< 0.001	0.06
Liver disease, %	1.3 (1.3–1.3)	1.3 (1.1–1.5)	0.76	< 0.01
Peptic ulcer disease, without bleeding, %	0.6 (0.5–0.6)	0.5 (0.4–0.6)	0.06	0.01
AIDS/HIV, %	0.0	0.0	0.18	< 0.01
Lymphoma, %	0.8 (0.8–0.8)	0.9 (0.7–1.0)	0.23	< 0.01
Metastatic cancer, %	3.7 (3.6–3.8)	3.7 (3.4–3.9)	0.76	< 0.01
Solid tumor, without metastasis, %	11.9 (11.8–12.0)	11.5 (11.1–11.9)	0.07	0.01
Rheumatoid arthritis/collagen vascular disease, %	3.1 (3.0–3.1)	3.2 (2.9–3.4)	0.48	< 0.01
Coagulopathy, %	2.8 (2.7–2.8)	3.3 (3.0–3.5)	< 0.001	0.03
Obesity, %	6.0 (5.9–6.1)	6.4 (6.1–6.7)	0.014	0.02
Weight loss, %	3.4 (3.3–3.4)	4.3 (4.1–4.6)	< 0.001	0.05
Fluid and electrolyte disorders, %	14.0 (13.9–14.1)	15.3 (14.8–15.8)	< 0.001	0.04
Blood loss anemia, %	1.5 (1.4–1.5)	1.6 (1.4–1.7)	0.39	0.01
Deficiency anemia, %	1.2 (1.1–1.2)	1.4 (1.2–1.5)	0.011	0.02
Alcohol abuse, %	0.8 (0.8–0.9)	0.9 (0.7–1.0)	0.71	< 0.01
Drug abuse, %	0.8 (0.7–0.8)	0.6 (0.5–0.7)	0.008	0.02
Psychoses, %	0.6 (0.6–0.6)	0.5 (0.4–0.6)	0.29	0.01
Depression, %	6.3 (6.2–6.4)	5.8 (5.5–6.1)	0.005	0.02

The table presents summary statistics for our sample, separately for patients receiving care from a nurse anesthetist (NA) or an anesthesiologist assistant (AA). *P* refers to the statistical significance of differences between the two groups, assessed by *t* test for age and by chi-square test for the remaining variables. Hedges's g refers to significance in terms of magnitude between the two groups, with values less than 0.2 representing small differences, values from 0.2 to 0.5 representing moderate differences, and values more than 0.5 representing large differences. 95% CIs are shown in parentheses.

two groups, values of 0.2 to 0.5 representing moderate differences, and values larger than 0.5 representing large differences.[21]

A simple comparison of outcomes between care teams with nurse anesthetists *versus* anesthesiologist assistants is likely to be confounded. To address this issue, our analysis adjusted for a robust set of potential confounders, such as patient characteristics (age, race, and sex), year of surgery, patient medical history (the set of comorbidities comprising the Elixhauser index; shown in table 1), and the ICD-9 procedure code for the given admission.

However, confounding from unobserved differences between the cases assigned to care teams with anesthesiologist assistants and nurse anesthetists could persist despite adjusting for the observable factors described above. As a first step toward minimizing confounding, our analysis also included fixed effects for each hospital to control for time invariant observable and unobservable characteristics (*e.g.*, academic status, general case mix) specific to the hospital. In essence, by adding hospital fixed effects, our approach compares outcomes between care teams with anesthesiologist assistants and nurse anesthetists *within a given hospital* who are involved in similar types of surgeries for similar types of patients.

While comparing outcomes *within* a given hospital avoids confounding that could occur because of differences between hospitals that use care teams with anesthesiologist assistants and those that use nurse anesthetists, it does not address the issue of confounding between anesthesiologist assistant and nurse anesthetist cases *within* a given hospital (*e.g.*, the possibility of schedulers preferentially assigning lower-risk cases to care team with anesthesiologist assistants). Therefore, we employed an instrumental variable approach to further minimize confounding. The instrumental variable approach identifies the causal effect of a policy or treatment using an instrument, which is

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any variable that (1) influences the independent variable of interest (in this case, whether the patient received care from an anesthesiologist assistant) but (2) is otherwise independent of the outcomes of interest (after controlling for the remaining independent variables). In effect, the instrument is used to quasirandomize patients to anesthesiologist assistants. For this analysis, we used variation in the daily number of anesthesiologist assistants available to do cases on the given day of surgery. There is likely to be day-to-day fluctuation in the number of anesthesiologist assistants available to do cases for several reasons. First, anesthesiologist assistants take vacation or call in sick. Second, "full-time" work for many anesthesiologist assistants involves less than 5 days per week. Finally, laws permitting anesthesiologist assistant practice changed during the study period (Supplemental Digital Content, http://links.lww.com/ALN/B729, appendix table A.1). For example, North Carolina passed legislation enabling anesthesiologist assistant practice in 2007, and Oklahoma followed in 2008. All these factors drive day-to-day fluctuations in the number of anesthesiologist assistants available to do work, which directly impacts the probability that an anesthesiologist assistant will be part of the care team for a given case. For example, if a patient arrives for surgery on a day when an anesthesiologist assistant has called in sick, an anesthesiologist assistant is less likely to be assigned to their care team. Moreover, none of these factors driving anesthesiologist assistant availability is likely to be associated with unobservable surgical and patient characteristics that might impact outcomes, particularly because decisions about anesthesiologist assistant scheduling (*e.g.*, the setting of vacation schedules) and laws permitting anesthesiologist assistant practice are typically made well in advance of the date of surgery. Although we do not directly observe the number of anesthesiologist assistants available to do cases on the given day, we do observe a closely related proxy: the daily percentage of a given hospital's cases that involved anesthesiologist assistants. The daily percentage of cases involving anesthesiologist assistants should reflect the number of anesthesiologist assistants available to do cases, because if there are fewer anesthesiologist assistants available to do cases, the hospital must find other providers (*e.g.*, anesthesiologists or nurse anesthetists) to do the cases. A conceptually similar approach has been used to identify the effect of teacher quality on long-term outcomes.[22]

We implemented our instrumental variable approach using a multivariable two-stage least-squares regression. The regression model included the adjustments for potential confounders (*e.g.*, patient sex, medical history, hospital fixed effects) previously described and used the daily percentage of a given hospital's surgeries that were performed by care teams with anesthesiologist assistants as an instrument for whether the patient actually received care from a care team with an anesthesiologist assistant. Further details of our instrumental variable approach are provided in the technical appendix found in the Supplemental Digital Content (http://links.lww.com/ALN/B729). All statistical analyses were performed using STATA 14.0 (STATA Corporation, USA). Because our study reports a negative finding, we did not adjust our significance thresholds for multiple comparisons, because in the light of a negative finding, *not* adjusting for multiple comparisons is conservative.

Our study design and this manuscript were prepared in accordance with the Strengthening the Reporting of Observational Studies in Epidemiology guidelines. The study protocol was approved by the Stanford Institutional Review Board (Stanford, California), who also issued a waiver of consent.

Sensitivity Analyses

We examined the robustness of the baseline statistical approach described above to alternative models of risk adjustment. Relative to our baseline approach, each of these alternative models achieves greater precision and statistical power but is more vulnerable to confounding from unobservable differences between patients that are treated by care teams with nurse anesthetists compared to patients that are treated by care teams with anesthesiologist assistants. First, we considered a model in which we used hospital fixed effects to model differences in unobservable factors across hospitals but in which we did not implement our instrumental variable approach. Rather, in this model, the independent variable of interest was simply whether an anesthesiologist assistant or nurse anesthetist was part of the care team. Second, we estimated a model that used a random-effects approach instead of a fixed-effects approach to model differences across hospitals. Finally, although our baseline approach used the ICD-9 procedure code to model surgical complexity, we considered two alternative ways of modeling surgical complexity. First, we estimated models in which we adjusted for the diagnosis-related group instead of the ICD-9 procedure code. Second, we estimated models in which we modeled surgical complexity using a random effects approach based on the ICD-9 procedure code.

Study Funding

This project received funding from the American Society of Anesthesiologists (ASA; Schaumburg, Illinois). The details of the funding mechanism are described in the Research Support section, and crucially, Drs. Sun and Baker retained final control over study design, manuscript formulation, and publication decisions. As part of the peer-review process for this manuscript, the original request for proposals from the ASA, the authors' initial research proposal to the ASA, and the final submitted proposal and plan for analysis after comments from the ASA were provided to Anesthesiology and the reviewers. Of note, the initial request for proposals called for the examination of additional outcomes related to patient safety indicators developed by the Agency for Healthcare Research and Quality (Rockville, Maryland).[16] During the early stages of this study, the investigators realized that although the study was adequately powered to find differences in mortality, length of stay, and costs, many of these

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patient safety indicators imposed additional exclusion criteria that would drastically reduce the sample size. As a result, we elected not to proceed further with analyses on these outcomes. In the interest of transparency, during the peer review process we performed these additional analyses, which can be found in the Supplemental Digital Content (http://links.lww.com/ALN/B729) as appendix table A.3. These results do not differ significantly from the results reported here.

Results

Our final sample consisted of 421,230 surgical cases in which the care team consisted of a physician anesthesiologist and a nurse anesthetist, and 21,868 cases in which the care team consisted of a physician anesthesiologist and an anesthesiologist assistant (table 1). Care teams with anesthesiologist assistants had younger patients (average age 75 yr, $P < 0.001$) who were less likely to be white (86.1 *vs.* 89.3%, $P < 0.001$). There was no significant sex difference between the two groups (44% male for both groups; $P = 0.97$). For 19 of the 31 comorbidities we examined, such as congestive heart failure and liver disease, there were no statistically significant differences. Of the remaining 12 comorbidities, 10 had a higher prevalence among cases with anesthesiologist assistant care teams (*e.g.*, coagulopathy, peripheral vascular disease), whereas 2 (drug abuse and depression) were less prevalent among this group. However, while statistically significant, the magnitude of differences between the two groups was fairly small for all of the characteristics we examined (Hedges' g less than 0.15 for all characteristics).

The unadjusted mortality for cases with anesthesiologist assistant care teams and for cases with nurse anesthetist teams was 1.7% (95% CI, 1.5 to 1.9, for anesthesiologist assistant teams and 95% CI, 1.6 to 1.7, for nurse anesthetist teams; $P = 0.87$ for the difference; fig. 1). After adjusting for observable and unobservable differences in case mix, patient characteristics, and hospital characteristics using the methods previously described, we found a slightly lower mortality for cases with anesthesiologist assistant care teams (1.6%; 95% CI, 1.4 to 1.8) compared to cases with nurse anesthetist care teams (1.7%; 95% CI, 1.7 to 1.7), although this difference was not statistically significant (0.08 percentage points; 95% CI, −0.3 to 0.1; $P = 0.47$). Although the unadjusted length of stay was higher for cases with anesthesiologist assistant care teams (6.7 *vs.* 6.4 days; $P = 0.06$), the risk-adjusted length of stay was approximately 6.4 days for both groups (95% CI, 6.4 to 6.4, for nurse anesthetists *vs.* 95% CI, 6.3 to 6.5, for anesthesiologist assistants; fig. 2), and the difference was not statistically significant (−0.009 days; 95% CI, −0.1 to 0.1; $P = 0.89$). Unadjusted medical spending was higher for care teams with an anesthesiologist assistant ($23,630 *vs.* $21,803; $P < 0.001$), but adjusted medical spending was lower ($21,841 *vs.* $21,897; fig. 3), and the implied $56 reduction in spending was not statistically significant (95% CI, −334 to 223; $P = 0.70$). Our findings were robust to several alternative statistical models, such as the model where we used random effects to adjust for differences across hospitals (table 2).

Discussion

In the United States, anesthesia care is often provided in the setting of an anesthesia care team consisting of nonphysician providers (anesthesiologist assistants and nurse anesthetists) who work under the supervision of a physician anesthesiologist. Although nurse anesthetists can practice nationwide,

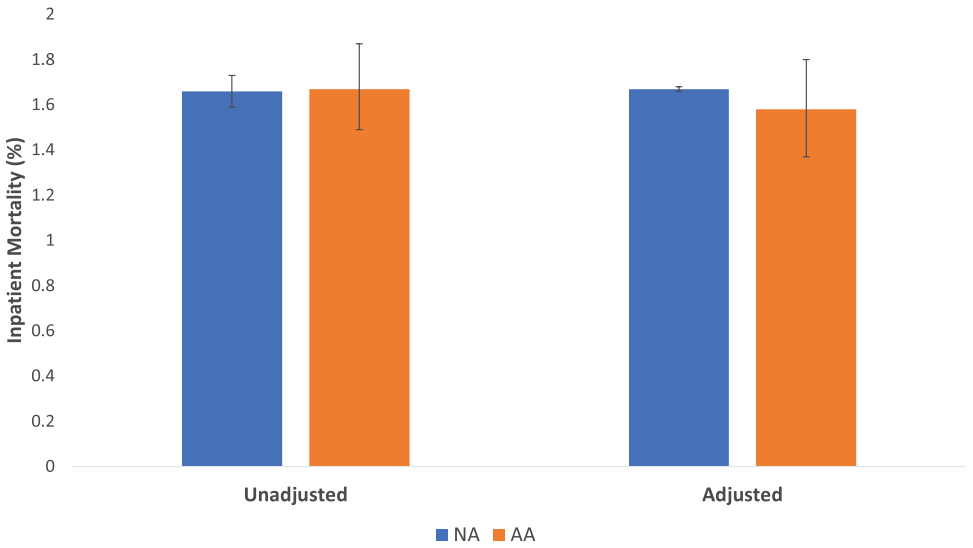

Fig. 1. Unadjusted and adjusted inpatient mortality, stratified by the composition of the anesthesia care team. The figure presents unadjusted and adjusted inpatient mortality rates separately for patients receiving care from anesthesia care team with nurse anesthetists (NA; *blue*) and care teams with anesthesiologist assistants (AA; *orange*). "Adjusted" refers to analyses that adjust for differences in surgery types, the patient characteristics listed in table 1, and hospital characteristics, using the approach described under Materials and Methods. The *error bars* represent 95% CIs and were calculated using standard errors that were clustered at the hospital level.

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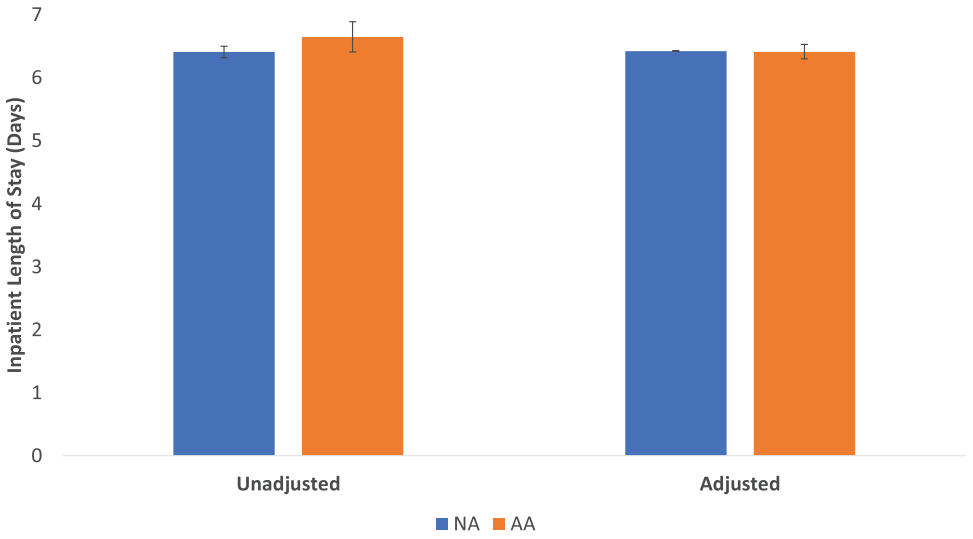

Fig. 2. Unadjusted and adjusted inpatient length of stay, stratified by composition of the anesthesia care team. The figure presents unadjusted and adjusted inpatient length of stay, separately for patients receiving care from anesthesia care team with nurse anesthetists (NA; *blue*) and care teams with anesthesiologist assistants (AA; *orange*). "Adjusted" refers to analyses that adjust for differences in surgery types, the patient characteristics listed in table 1, and hospital characteristics, using the approach described under Materials and Methods. The *error bars* represent 95% CIs and were calculated using standard errors that were clustered at the hospital level.

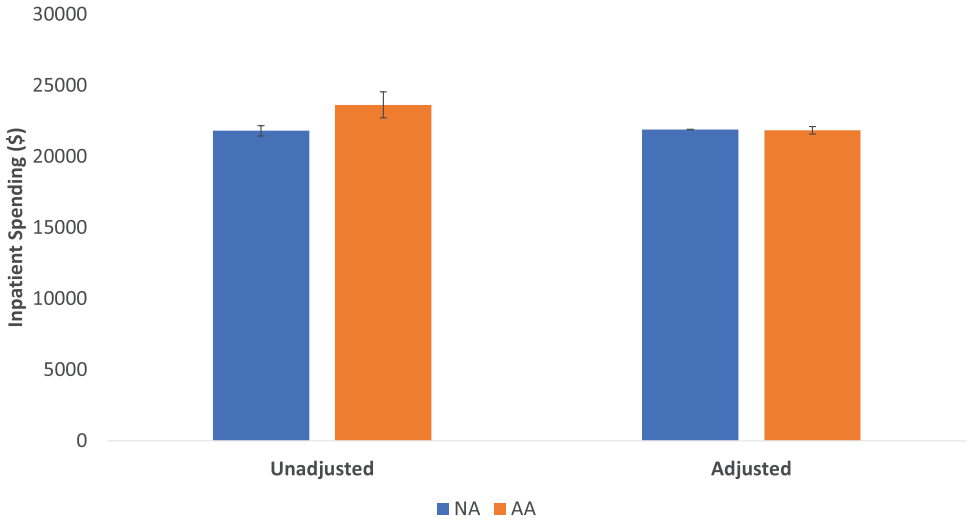

Fig. 3. Unadjusted and adjusted inpatient spending, stratified by the composition of the anesthesia care team. The figure presents unadjusted and adjusted inpatient spending, separately for patients receiving care from anesthesia care team with nurse anesthetists (NA; *blue*) and care teams with anesthesiologist assistants (AA; *orange*). "Adjusted" refers to analyses that adjust for differences in surgery types, the patient characteristics listed in table 1, and hospital characteristics, using the approach described under Materials and Methods. The *error bars* represent 95% CIs and were calculated using standard errors that were clustered at the hospital level.

anesthesiologist assistants can only practice in 16 states and the District of Columbia, and efforts to expand the areas where anesthesiologist assistants can practice have been challenged by concerns over poorer patient outcomes. However, whether these concerns have an empirical basis has not been studied. To inform policymaking regarding the scope of anesthesiologist assistant practice, we compared outcomes between care teams with nurse anesthetists to care teams with anesthesiologist assistants for elderly patients undergoing inpatient surgery. Our study found no statistically significant difference in outcomes of mortality, length of stay, and spending between these two types of care teams. In addition, the narrow CIs around our estimated results suggest that our null findings are due to a true lack of association, as opposed to imprecision in our estimates.

The key implication of our findings is that the specific composition of the anesthesia care team—in other words, whether the physician anesthesiologist supervises a nurse

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Table 2. Association between Use of Anesthesiologist Assistants and Perioperative Outcomes, Alternative Model Specifications

Model	Outcome			Notes
	Death, %	Length of Stay, days	Inpatient Spending, $	
Baseline analysis	−0.08 (95% CI, −0.3 to 0.1) *P* = 0.47	−0.009 (95% CI, −0.1 to 0.1) *P* = 0.89	−56 (95% CI, −334 to 223) *P* = 0.70	Baseline model
Fixed effects only	−0.01 (95% CI, −0.2 to 0.2) *P* = 0.91	−0.01 (95% CI, −0.1 to 0.08) *P* = 0.78	−68 (95% CI, −292 to 156) *P* = 0.55	This model omitted the instrumental variables approach used for the baseline analyses but retained the hospital fixed effects to adjust for differences across hospitals
Random-effects model (hospital)	−0.2 (95% CI, −0.3 to 0.02) *P* = 0.09	−0.02 (95% CI, −0.1 to 0.07) *P* = 0.63	−41 (95% CI, −262 to 180) *P* = 0.72	This model adjusted for differences across hospital using a random-effects approach instead of a fixed-effects approach
Random-effects model (procedure)	0.03 (95% CI, −0.2 to 0.2) *P* = 0.76	−0.002 (95% CI, −0.1 to 0.09) *P* = 0.97	48 (95% CI, −182 to 280) *P* = 0.68	This model adjusted for surgical complexity using a random-effects approach instead of a fixed-effects approach
DRG adjustment	−0.04 (95% CI, −0.3 to 0.2) *P* = 0.74	0.05 (95% CI, −0.07 to 0.2) *P* = 0.44	237 (95% CI, −62 to 535) *P* = 0.12	This model adjusted for surgical complexity using the DRG instead of the surgical procedure code

The table presents the results of sensitivity analyses in which we considered the robustness of our results to alternative statistical model. Baseline Analysis refers to the baseline model used to produce the main results discussed in the text. The alternative statistical models were a fixed-effects only model, a model with random effects for hospitals, a model with random effects for procedure, and a model that used diagnosis-related groups (DRG) to adjust for surgical complexity. A brief description of each model is provided under Notes. The table presents the estimated association between anesthesiologist assistant care and the given outcome. For death, the table shows the estimated percentage point change in inpatient mortality. The 95% CI values shown in parentheses were calculated using robust standard errors.

anesthetist or an anesthesiologist assistant—is not likely to be associated with differences in patient outcomes. In some respects, this is not a surprising finding, because particularly at academic centers, physicians successfully supervise trainees and nonphysician providers with a wide variety of backgrounds and experience. As a particularly striking example, consider that at academic medical centers, the most experienced trainees complete training and leave on July 1, to be replaced by a new set of unexperienced medical trainees. However, a large portion of the literature suggests that this turnover (the "July effect") is not associated with any difference in patient outcomes,[23] suggesting that potentially problematic effects of rather drastic differences in experience among medical trainees can be mitigated with appropriate systems, teams, and physician supervision. In this context, we emphasize that our results do not address whether, in isolation, anesthesiologist assistants or nurse anesthetists are systemically "better" for patients. Instead, our results suggest that the team structures and physician supervision in place in this study context are able to mitigate any systemic differences between the two groups, if they exist at all.

Our study should be viewed in light of its limitations. First, as with all retrospective analyses, we cannot exclude the possibility of residual confounding. In particular, our data did not allow us to adjust for provider experience or differences in supervision ratios between anesthesiologist assistants and nurse anesthetists or differences in case assignment based on unobservable measures of patient complexity. However, we made extensive efforts to minimize the possibility of confounding. Because we compared outcomes *within* a given hospital, our results would only be confounded to the extent that *within* a given hospital, patients taken care of by anesthesiologist assistant care teams are at lower risk than those taken care of by nurse anesthetist care teams. Moreover, we found no significant differences between patients who received care from an anesthesiologist assistant care team compared to those who received care from a nurse anesthetist care team for most of the characteristics we considered (*e.g.*, patient comorbidities; see table 1), and where there were differences, they suggested that anesthesiologist assistant care teams tended to take care of *higher*-risk patients. Finally, our analysis adjusted for an extensive set of potential confounders and utilized a quasirandomization approach to further minimize confounding.

Second, our analysis was limited to elderly Medicare patients undergoing inpatient surgery, a high-risk population. Third, our study covered the time period between 2004 and 2011. Although the vast majority of the 17 jurisdictions (16 states and the District of Columbia) that enabled anesthesiologist assistant practice did so either before or during the study period, one state (Indiana) enabled anesthesiologist assistant practice in 2014, outside of our study period. Fourth, because our study could not measure supervision ratios, we cannot draw any conclusions about the optimal supervision ratio for either anesthesiologist assistants or nurse anesthetists. Fifth, our study was not designed to address the question of whether there are benefits from independent anesthesiologist assistant practice. Sixth, our study did not measure other potential quality measures (*e.g.*, mortality 30 days postdischarge, incidence of intensive care unit

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admission). Seventh, it should be noted that although all the nurse anesthetists and anesthesiologist assistants in our analysis were supervised by a physician, nurse anesthetists can (in rare cases) be supervised by the surgeon or proceduralist as opposed to a physician anesthesiologist, and we were unable to exclude cases where this could have occurred. Finally, with regards to medical spending, our study did not address whether the expanded use of anesthesiologist assistants could change the structure of the anesthesia workforce and alter the nature of competition between anesthesia providers. How these potential changes would affect the negotiated prices paid by private insurers to anesthesia providers is a subject for future study.

In conclusion, among elderly patients undergoing inpatient surgery, our study found no significant differences in outcomes between care teams with anesthesiologist assistants compared to care teams with nurse anesthetists. Further work should examine whether these results extend to other patient populations and types of surgery, including, for instance, privately insured patients and outpatient surgeries. Moreover, because improving access to care is a frequently cited rationale for expanding the use of midlevel anesthesia providers,[24–26] further research should examine whether the introduction of anesthesiologist assistants has improved access to care. Finally, from a regulatory and antitrust standpoint, understanding the extent to which the introduction of anesthesiologist assistants has impacted competition among groups of anesthesia providers is a fruitful area for further research.

Research Support

Supported by funding from the American Society of Anesthesiologists (ASA, Schaumburg, Illinois; to Dr. Sun, Dr. Baker, and Ms. Moshfegh). The ASA issued a request for proposals to three organizations, including Stanford, for a study to compare outcomes between anesthesia care teams with anesthesiologist assistants and anesthesia care teams with nurse anesthetists. Drs. Sun and Baker elected to respond to the request and prepared a bid including an outline of the study methodology, which they developed without input from the ASA. In all, the ASA received three bids. The decision on which bid to accept was made by the Executive Committee of the ASA, with Dr. Miller's input. Dr. Miller is employed by the ASA.

Competing Interests

Dr. Miller is employed by the American Society of Anesthesiologists. Although all authors participated in the study design, data analysis, manuscript preparation, and publication decisions, the funding arrangement provided that the Stanford investigators—specifically Drs. Sun and Baker—had the final say over all elements of the study.

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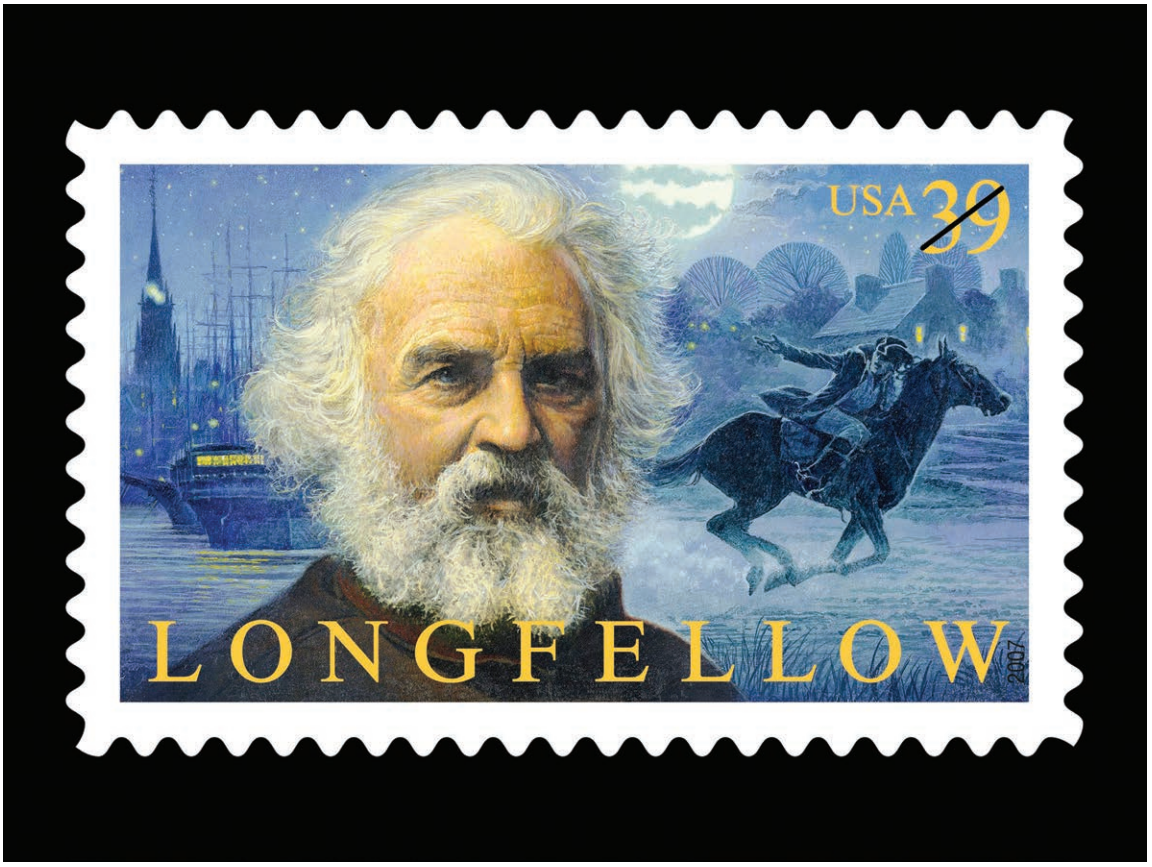
How Two Longfellows Revered Ether



Issued in 2007 with images of Boston's Old North Church and the midnight rider Paul Revere, this 39-cent U.S. postal stamp commemorated the 200th anniversary of the birth of American poet Henry Wadsworth Longfellow (1807 to 1882). Because Georgia's Crawford Long, M.D., failed to publish his use of obstetric ether more than 2 yr earlier, Longfellow's wife Fanny became the first American *recorded* to have received ether for obstetric anesthesia. (Note: Fanny's etherization by Dr. Nathan Cooley Keep occurred more than 3 months after Professor James Y. Simpson's use in Scotland of obstetric ether.) Severely burned in 1861 after her dress had caught fire, Mrs. Longfellow was given ether for analgesia before she succumbed to her injuries. While using a rug and his own body to extinguish the flames, Henry had been burned severely enough to miss Fanny's funeral and to warrant growing a beard to hide his scars. As a widower, Longfellow assuaged both his burning pain and his unrelenting grief with ether. (Copyright © the American Society of Anesthesiologists' Wood Library-Museum of Anesthesiology.)

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