"Opt Out" and Access to Anesthesia Care for Elective and Urgent Surgeries among U.S. Medicare Beneficiaries

Eric C. Sun, M.D., Ph.D., Franklin Dexter, M.D., Ph.D., Thomas R. Miller, Ph.D., M.B.A., Laurence C. Baker, Ph.D.

ABSTRACT

Background: In 2001, the Centers for Medicare and Medicaid Services issued a rule allowing U.S. states to "opt out" of the regulations requiring physician supervision of nurse anesthetists in an effort to increase access to anesthesia care. Whether "opt out" has successfully achieved this goal remains unknown.

Methods: Using Medicare administrative claims data, we examined whether "opt out" reduced the distance traveled by patients, a common measure of access, for patients undergoing total knee arthroplasty, total hip arthroplasty, cataract surgery, colonoscopy/sigmoidoscopy, esophagogastroduodenoscopy, appendectomy, or hip fracture repair. In addition, we examined whether "opt out" was associated with an increase in the use of anesthesia care for cataract surgery, colonoscopy/sigmoidoscopy, or esophagogastroduodenoscopy. Our analysis used a difference-in-differences approach with a robust set of controls to minimize confounding.

Results: "Opt out" did not reduce the percentage of patients who traveled outside of their home zip code except in the case of total hip arthroplasty (2.2% point reduction; $P = 0.007$). For patients travelling outside of their zip code, "opt out" had no significant effect on the distance traveled among any of the procedures we examined, with point estimates ranging from a 7.9-km decrease for appendectomy (95% CI, −19 to 3.4; $P = 0.173$) to a 1.6-km increase (95% CI, −5.1 to 8.2; $P = 0.641$) for total hip arthroplasty. There was also no significant effect on the use of anesthesia for esophagogastroduodenoscopy, appendectomy, or cataract surgery.

Conclusions: "Opt out" was associated with little or no increased access to anesthesia care for several common procedures.
(Anesthesiology 2017; 126:461-71)

IN the United States, anesthesia is most commonly provided by anesthesiologists (*i.e.*, physicians with specialty training in anesthesiology), nurse anesthetists, and anesthesiologist assistants. Similar to physician assistants more generally, anesthesiologist assistants only provide care under the supervision of an anesthesiologist. Generally, nurse anesthetists are supervised by an anesthesiologist, with or without the anesthesiologist formally billing. Occasionally, they are supervised by a proceduralist (*e.g.*, the surgeon performing the case). Rarely, anesthesia is provided by the proceduralist alone. Concerns over access to anesthesia care—as well as predicted future shortages of anesthesiologists[1]—have led policymakers to consider loosening the degree to which nurse anesthetists must be supervised, up to and including independent practice.

In 2001, the U.S. Centers for Medicare and Medicaid Services (CMS) issued a rule allowing states to "opt out" of the requirement that nurse anesthetists be supervised by a physician (either an anesthesiologist or a proceduralist) in order for providers to receive payment from Medicare for the anesthetic. By 2013, 17 states had decided to "opt out" of the Medicare regulations requiring supervision of nurse anesthetists; in some states, this allows for independent nurse anesthetist practice.[2]

What We Already Know about This Topic

- To conceptually increase access to care, the United States in 2001 began allowing states to "opt out" of requirements that nurse anesthetists be supervised by a physician (anesthesiologist or proceduralist) in order for providers to receive Medicare payment
- Much attention has focused on whether "opt out" affects the quality of anesthesia care, but less attention has addressed whether it increases access to care, the normative intent of the Medicare rule
- As a measure of access to care, previous studies examined the association between "opt out" and the number of surgical procedures performed

What This Article Tells Us That Is New

- This investigation examined a different dimension of access to care and the influence of "opt out": the distance patients travel to obtain surgical procedures
- For five common elective procedures and two common urgent procedures, "opt out" largely did not reduce the percentage of patients who traveled outside of their home zip code, and for patients who did travel outside of their zip code, "opt out" had no significant effect on the distance traveled
- Results demonstrate that "opt out" was associated with little or no increased access to anesthesia care for several common procedures

This article is featured in "This Month in Anesthesiology," page 1A. 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).

 Anesthesiology 2017; 126:461-71

<zdoi;10.1097/ALN.0000000000001504>

Debate over the merits of "opt out" has largely focused on whether it has affected the quality of anesthesia care.[3–12] Less work has addressed whether it has increased access to care, the normative intent of the administrative rule. Indeed, the rule states that governors must consider the availability of anesthesia care before deciding whether to "opt out." Moreover, in choosing to "opt out," governors have typically cited the necessity of improving access to anesthesia care.[13,14] Whether "opt out" has increased access remains an open question with important policy implications not just for surgical care but also for health care more broadly. In an effort to control health care spending, policymakers are considering laws and regulations that would expand the autonomy of other midlevel providers, such as nurse practitioners, in the belief that doing so will reduce costs and increase access to care.[15] Understanding whether "opt out" has increased access to care could provide a useful test case for gauging these laws' likely effectiveness. If "opt out" has not improved access to care, understanding why may help policymakers improve these laws in order to improve access.

Two previous studies examined the association between "opt out" provisions and the number of surgical procedures performed, particularly urgent procedures; no association was detected.[16,17] While useful, these studies shed light on only one measure of access to care and focused on the use of urgent surgical procedures.

For this article, we used administrative claims data from Medicare to examine whether "opt out" was associated with a different dimension of increased access to care: the distance patients travel to obtain surgical procedures. We examine this for five elective procedures common among the Medicare population: total knee arthroplasty (also known as total knee replacement), total hip arthroplasty (THA; also known as total hip replacement), colonoscopy/sigmoidoscopy, esophagogastroduodenoscopy, and cataract surgery, as well as two common urgent procedures where timely access to surgical care is important: appendectomy and hip fracture repair.

By facilitating independent nurse anesthetist practice, "opt out" could reduce the distances traveled by patients in two ways. First, the increased supply of anesthesia providers might prompt the opening of new facilities (*e.g.*, hospitals or ambulatory surgery centers). Second, the increased supply of anesthesia providers might also allow new services (*e.g.*, an orthopedic service) to be established in existing hospitals. Distance traveled has been used in many other nonsurgical studies to measure a dimension of access to care.[18–20] Increased travel distance has been shown, in some studies, to be associated with poorer surgical outcomes.[21,22] Finally, regardless of its effects on outcomes, increased travel distances represent an inconvenience for patients, particularly since absolute distances between patients and hospitals influence the choice of hospital for surgical care.[23–25]

Submitted for publication March 14, 2016. Accepted for publication December 6, 2016. From the Department of Anesthesiology, Pain, and Perioperative Medicine, Stanford University, Stanford, California (E.C.S.); Division of Management Consulting, Department of Anesthesia, University of Iowa, Iowa City, Iowa (F.D.); Department of Analytics and Research Services, American Society of Anesthesiologists, Schaumburg, Illinois (T.R.M.); Department of Health Research and Policy, Stanford University, Stanford, California (L.C.B.); and National Bureau of Economic Research, Cambridge, Massachusetts (L.C.B.).

Materials and Methods

Data

Our data 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 aged 65 yr and older.[26] As a general rule, Medicare beneficiaries can choose either to be enrolled in the traditional fee-for-service plan, for which the federal agency administering Medicare—the CMS—is the primary payer, or they can choose to be enrolled in a managed healthcare plan. Under 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.[27] Health insurance claims data for beneficiaries enrolled in the traditional fee-for-service plans are available for researchers upon approval of a Data Use Agreement with CMS and payment of required fees.

The Medicare data are detailed. They include demographic information such as age, race, sex, county of residence, and zip code (also known as postal code) of residence. The data also provide diagnosis codes (International Classification of Disease, Ninth Edition), which can be used to draw inferences about a patient's comorbidities, as well as procedure codes (*e.g.*, Current Procedural Terminology® codes used to classify specific procedures) that can be used to identify whether they received a given procedure.

Sample

We constructed a sample consisting of all persons who received a total knee arthroplasty, THA, hip fracture repair, appendectomy, cataract surgery, colonoscopy/sigmoidectomy, or esophagogastroduodenoscopy between 1999 and 2011 and who were continuously enrolled in Medicare fee-for-service for the entirety of the calendar year they received their procedure. We identified patients who received these procedures by isolating claims submitted by a healthcare provider (located in the carrier claims file) with a procedure code (*i.e.*, Current Procedural Terminology® code) associated with the given procedure. A list of the relevant procedure codes and the references used to identify these codes is provided in the supplemental content (appendix table A, http://links.lww.com/ALN/B360). These claims were then linked to a corresponding claim submitted by a healthcare facility (*e.g.*, hospital or ambulatory surgery center). Medicare does not provide any explicit way of linking provider claims to facility claims. Each provider claim reports the dates for which the patient received care at the facility; we assumed that a facility claim was linked to the provider claim if the date on the provider claim fell within the range of dates listed on the facility claim.

After excluding persons who received more than one of the procedures we studied in a given calendar year, we arrived at an initial sample of 3,818,148 cases. We then applied several exclusion criteria. First, we excluded cases in which we identified more than one facility claim associated with the given carrier claim (n = 135,436), which occurred when there were two or more facility claims whose dates overlapped the carrier claim. Second, total knee arthroplasty, THA, cataract surgery, hip fracture repair, and appendectomy are nearly always performed at a surgical facility (*i.e.*, hospital). By contrast, colonoscopy and esophagogastroduodenoscopy can be performed in a physician's office. Therefore, we excluded claims for total knee arthroplasty, THA, cataract surgery, hip fracture repair, and appendectomy for which we could not identify an associated facility claim (n = 43,395). Third, colonoscopy, esophagogastroduodenoscopy, and cataract surgery do not always require anesthesia care. Since our interest was in cases that require anesthesia care, we excluded colonoscopy, esophagogastroduodenoscopy, and cataract surgery cases without a corresponding anesthesia claim (n = 2,186,866) from our main analysis although we retained these cases for some secondary analyses in the Secondary Analyses subsection. To identify a corresponding anesthesia claim, we isolated carrier claims with an appropriate anesthesia procedure code (supplemental content, appendix table A, http://links.lww.com/ALN/B360) on a date corresponding to the same date as the procedure itself. Fourth, we excluded cases where we could not identify the patient or facility zip code (n = 134,256) or the case was missing other patient demographic information (n = 9,993). Fifth, we excluded cases where the calculated distance was more than 500 km (310 miles; n = 25,536). The rationale for this exclusion was that patients who traveled this far for their procedure were likely doing so for reasons unrelated to the availability of care in their local area (*e.g.*, presence of family or an urgent procedure occurring while on vacation). Finally, we excluded patients under the age of 65 (n = 139,851). Our final sample consisted of 1,142,815 cases. A flow diagram describing how the sample was created is provided in the supplemental content (appendix fig. 1, http://links.lww.com/ALN/B360).

Outcomes

Our outcome of interest was the distance traveled by the patient to receive their procedure. Our basic approach was to identify the zip code of the patient, as well as the zip code where the procedure took place using the methods described in the subsequent paragraph. We then used data from the U.S. Census to identify the latitude and longitude for the central point within each zip code.[28] Using this latitude and longitude, the Haversine formula was then used to calculate the distance between two zip codes. This approach has been used in many previous studies to identify the distances between patients and hospitals/nursing facilities.[18–20,25]

Identifying the zip code of each patient's residence is straightforward, as it is directly provided in the annual demographic files provided by Medicare. For our study, there are two potential zip codes where the procedure may have taken place: the zip code of the facility and that of the physician. The latter, provided by the physician, may represent a mailing address or the location of the physician's office. For the procedures of total knee arthroplasty, THA, appendectomy, cataract surgery, and hip fracture repair, we used the zip code of the facility, as these procedures almost certainly take place in a surgical facility. Since colonoscopy and esophagoduodenoscopy can take place in a surgical facility or a physician's office, we used the facility zip code if present but, otherwise, the zip code of the physician.

Independent Variables

Our primary independent variable of interest was whether "opt out" was in effect in the given state during the year the procedure was performed. Specifically, we created a dummy variable equaling 1 in the "opt out" states starting the year after the "opt out" year (supplemental content, appendix table B, http://links.lww.com/ALN/B360). For non-"opt out" states and for "opt out" states in the years before and including the "opt out" year, the variable equaled zero. It is important to note that 2011 was the last year for which we could obtain data. Since Kentucky "opted out" in 2012, our analysis, therefore, implicitly treated Kentucky as a non-"opt out" state (*i.e.*, did not account for the effect of "opt out" in Kentucky).

In addition to our "opt out" variable, we also obtained several covariates at the patient and county levels. At the patient level, we incorporated age, sex, and race as controls, as well as the comorbidities listed in table 1. We chose these comorbidities as they are constituents of the Elixhauser set of comorbidities, which is frequently used for risk adjustment.[29] To determine the presence of a comorbidity, we extracted all of an individual's inpatient, outpatient, and carrier claims that were filed during the year of surgery. A patient was then deemed to have the given comorbidity if they had at least one claim with a relevant International Classification of Disease, Ninth Edition, diagnosis code.[30]

At the county level, we incorporated several controls based on the patient's county of residence: total population, share of the population that was white, share of the population that was male, share of the population aged more than 65 yr, and median income for the county. These variables were obtained from the U.S. Census Bureau. They were chosen as they could potentially affect the market for hospital and healthcare services.[31]

Table 1. Characteristics of Study Patients in "Opt Out" and Non-"Opt Out" States

	Non-"Opt Out" States (n = 954,789)	"Opt Out" States (n = 188,026)	*P* Value
Type of procedure			
Total knee arthroplasty	115,560	26,839	N/A
Total hip arthroplasty	51,185	12,730	N/A
Cataract surgery	319,882	91,375	N/A
Colonoscopy/sigmoidoscopy	181,217	16,588	N/A
Esophagogastroduodenoscopy	180,950	16,400	N/A
Appendectomy	14,264	4,304	N/A
Hip fracture repair	91,731	19,790	N/A
Patient demographics			
Male, %	37.5 (0.0495)	38.0 (0.112)	< 0.001
White, %	88.5 (0.0326)	90.1 (0.0686)	< 0.001
Age, yr	74.7 (0.579)	75.2 (1.30)	< 0.001
Rural county of residence, %	23.2 (0.0432)	33.0 (0.108)	< 0.001
Median income of county of residence, $	47,627 (14.9)	48,362 (26.5)	< 0.001
Patient comorbidities, %			
Congestive heart failure	16.7 (0.0382)	14.7 (0.0817)	< 0.001
Cardiac arrhythmia	27.7 (0.0458)	26.0 (0.101)	< 0.001
Valvular disease	16.3 (0.0378)	12.7 (0.0767)	< 0.001
Pulmonary circulation disorders	3.51 (0.0189)	3.25 (0.0409)	< 0.001
Peripheral vascular disease	17.5 (0.0389)	13.3 (0.0784)	< 0.001
Hypertension, uncomplicated	76.1 (0.0437)	71.3 (0.104)	< 0.001
Hypertension, complicated	12.5 (0.0339)	8.71 (0.0651)	< 0.001
Paralysis	1.19 (0.0111)	1.06 (0.0236)	< 0.001
Neurologic disorders	7.68 (0.0273)	6.69 (0.0576)	< 0.001
Chronic pulmonary disease	27.0 (0.0454)	24.5 (0.0992)	< 0.001
Diabetes, uncomplicated	18.1 (0.0394)	14.5 (0.0811)	< 0.001
Diabetes, complicated	5.17 (0.0227)	3.25 (0.0409)	< 0.001
Hypothyroidism	21.5 (0.0420)	20.6 (0.0932)	< 0.001
Renal failure	7.56 (0.0271)	6.50 (0.0568)	0.215
Liver disease	5.14 (0.0226)	3.57 (0.0428)	< 0.001
Peptic ulcer disease	3.68 (0.0193)	2.71 (0.0374)	< 0.001
HIV/AIDS	0.0757 (0.00282)	0.0457 (0.00493)	< 0.001
Lymphoma	1.41 (0.0121)	1.14 (0.0245)	< 0.001
Metastatic cancer	2.36 (0.0156)	1.71 (0.0299)	< 0.001
Cancer	15.7 (0.0372)	12.8 (0.0772)	< 0.001
Rheumatoid arthritis	7.82 (0.0275)	7.13 (0.0594)	< 0.001
Coagulopathy	6.68 (0.0256)	4.40 (0.0473)	< 0.001
Obesity	5.61 (0.0236)	5.79 (0.0539)	< 0.001
Weight loss	6.26 (0.0245)	4.73 (0.0490)	< 0.001
Fluid/electrolyte disturbances	17.9 (0.0392)	15.9 (0.0843)	< 0.001
Blood loss anemia	4.16 (0.0204)	2.84 (0.0383)	< 0.001
Deficiency anemia	12.0 (0.0332)	8.44 (0.0641)	< 0.001
Alcohol abuse	1.31 (0.0116)	1.43 (0.0274)	< 0.001
Drug abuse	0.883 (0.00957)	0.914 (0.0220)	0.215
Psychosis	3.14 (0.0178)	2.97 (0.0391)	< 0.001
Depression	12.5 (0.0339)	11.6 (0.0739)	< 0.001

This table presents the summary statistics for our sample, stratified by "opt out" and non-"opt out" states. SEs are shown in parentheses. The column "*P* value" represents the results of a Student's *t* test (for age and median income) or chi-square test (for the remaining variables) used to assess the statistical significance of differences between the two groups.

AIDS = acquired immunodeficiency syndrome; HIV = human immunodeficiency virus; N/A = not applicable.

Statistical Analyses

A simple cross-sectional comparison of travel distances across zip codes could be subject to confounding since states that choose to "opt out" differ in many ways from states that did not. For example, Iowa was the first state to "opt out," and it is geographically large compared to Delaware, a state that has not chosen to "opt out."

While our analysis did include several controls for observable differences between "opt out" and non-"opt out" areas—such as differences in income and patient comorbidities—a straightforward cross-sectional comparison could still be vulnerable to confounding from unobservable differences between states. Therefore, we utilized a difference-in-differences analysis to reduce confounding further. The difference-in-differences approach is frequently used in policy analysis[32–34] and has been used in the previous literature examining the effects of "opt out."[16,17] Under this approach, zip code–specific controls were used to adjust for unobservable zip code level factors. Therefore, rather than comparing travel distances *across* areas, the difference-in-differences approach identifies the effect of "opt out" by estimating *within zip code* changes in travel distance over time among the "opt out" states.

Simple "before-after" comparison can still be confounded by secular time trends, such as general changes in surgical practice over time. Therefore, the second step of a standard difference-in-differences approach involves the use of year effects in order to control for secular time trends at the national level. In addition to these controls, we incorporated additional linear and quadratic state trends to control for unobserved, secular trends occurring at the state level.

We implemented our difference-in-differences analysis by using regression analyses in which the dependent variables were measures of distance traveled, and the key independent variable was the measure of whether opt out is in effect in a given state. The models controlled for the patient and county factors (table 1), as well as zip code controls, year controls, and controls for linear and quadratic state trends.

For roughly 20% of our cases (n = 219,767), the patient's zip code and the facility's zip code were the same, leading to an estimated travel distance of 0. When there are many observations for which the dependent variable is 0, a simple regression analysis will tend to be downward biased; in other words, the estimated effect will be lower in magnitude than the true effect. As a result, a simple regression analysis would be biased toward a finding that "opt out" had no effect on access.[35] To address this issue, we performed two regressions. The first analysis was a multivariable regression in which the dependent variable was an indicator variable equaling 1 if the patient and the facility shared the same zip code, and the independent variables were the variables outlined in the Independent Variables subsection. Our coefficient of interest was the coefficient associated with our "opt out" variable, which can be interpreted as the effect of "opt out" on the absolute percentage point change on the percentage of patients travelling outside of their zip code for their procedure. Our second analysis was a multivariable linear regression in which the dependent variable was the actual estimated travel distance. The independent variables were the same as those described for the previous analysis. This analysis was restricted to patients with a nonzero travel distance. For this second analysis, our coefficient of interest was the coefficient associated with our "opt out" variable, which demonstrates the change in travel distance associated with "opt out" *among patients with a nonzero travel distance* (*i.e.*, among patients who traveled outside their zip code). This two-step approach has been used in other studies to obtain nonbiased estimates when a large proportion of observations assume a value of 0.[36,37]

Zip codes do not have the same size (land area),[24] so our distance measures are sensitive to the size of a given zip code. Mechanically, patients residing in larger metropolitan zip codes (*i.e.*, larger land area) are less likely to have to travel outside of their zip code to receive care and should they do so are more likely to travel a longer distance to do so (since the land area of the zip code is larger). Our use of zip code fixed effects addresses this issue since these effects would control for unobservable zip code factors that are fixed over time, such as land area. For some descriptive analyses, we did adjust our results for land area (Technical Appendix in the online supplemental content, http://links.lww.com/ALN/B360).

We performed our analyses with STATA 14.0 (StataCorp, USA) and adjusted our SEs and CIs for clustering at the state level.[38] Further details of our approach are found in the Technical Appendix in the online supplemental content (http://links.lww.com/ALN/B360).

Secondary Analyses

We conducted several sets of secondary analyses. First, we conducted two sensitivity analyses to examine the robustness of our results to alternative measures of access. Of the seven procedures we examined, three (colonoscopy, esophagastroduodenoscopy, and cataract surgery) do not always require anesthesia, by which we mean here and elsewhere the presence of an anesthesiologist and/or nurse anesthetist. In the baseline analyses described in the Statistical Analyses subsection, we excluded patients undergoing these three procedures who did not receive anesthesia. Therefore, for these three procedures, our estimates reflect the effect of "opt out" on the travel distances for patients undergoing these procedures *who ultimately received anesthesia for their procedure*. However, "opt out" could also make it more likely that patients undergoing these procedures would receive anesthesia to begin with. To assess this possibility, we conducted a separate set of analyses for these three procedures in which we included patients undergoing these procedures who did *not* receive anesthesia (n = 1,639,944). For these analyses, we estimated a linear regression similar to the analyses described in the previous subsection, where the dependent variable was an indicator variable assuming the value of 1 if the patient received anesthesia for their procedure and 0 otherwise. Our independent variable of interest was our "opt out" variable, and we incorporated the same set of additional controls that were used for our baseline analyses.

Second, suppose that, as a result of "opt out," a new surgery center opens, thereby providing the opportunity for patients to receive a total knee arthroplasty closer to home. However, suppose that the vast majority of patients living in the zip code decline to use the surgery center. Our baseline analyses would generally identify no change in access—given the small proportion of patients using the new surgery center, the effect on overall travel distance will also be small. To address this possibility, we, therefore, conducted analyses where the dependent variable was the annual *minimum* distance traveled by any patient within the given zip code (for a given procedure). In contrast to the previous analyses, where the unit of observation was the individual patients, these analyses were performed at the zip code level using the same covariates described previously, except that we converted patient-specific variables (*e.g.*, age) to average values at the zip code level (*e.g.*, average age at the zip code level). Because using the minimum distance traveled by any patient is susceptible to outliers, we also considered an alternative specification in which we used the fifth and tenth percentiles of distance traveled. For a given zip code, there need to be at least 19 patients receiving for a given procedure in order to estimate the fifth percentile without bias and at least nine patients for the tenth percentile. Therefore, for these additional analyses, we used the minimum distance traveled for those zip codes that did not have sufficient patients to calculate the given percentile.

We also conducted two subanalyses in which we examined whether the effects of "opt out" were different between two populations most likely to see a benefit: patients residing in zip codes with long travel distances to begin with and low-risk patients. Hypothetically, low-risk patients may be more likely to benefit from "opt out" since sicker patients may be more likely to choose or require care at a tertiary referral center, regardless of travel distance. For the first subanalysis, we restricted our sample to zip codes whose initial average travel distance was greater than 50 km (roughly 30 miles). For the second subanalysis, we used a previously developed empirical weighting of the Elixhauser comorbidities.[29] As a score of 10 on this index broadly corresponds to a score of 3 on the Charlson index, we restricted our sample to patients with a score of 10 or less.

Study Design

The construction of our study sample (such as the inclusion and exclusion criteria), as well the elements of our baseline analysis (such as the choice of statistical model, choice of outcome, and choice of additional covariates), was determined before initiating our analysis. Our additional analyses were conducted *post hoc* based on the finding of our baseline analysis, as well as comments made during the revision process.

Results

Table 1 presents descriptive statistics for our sample, which included 954,789 cases taking place in non-"opt out" states (*i.e.*, states that never chose to "opt out") and 188,026 cases that occurred in "opt out" states (*i.e.*, states that ultimately chose to "opt out"). For the cases occurring in "opt out" states, 112,221 occurred before "opt out' and 75,085 occurred afterward. Patients undergoing a procedure in an "opt out" state were more likely to be males (38.0% *vs.* 37.5%; $P < 0.001$) and whites (90.1% *vs.* 88.5%) and resided in counties with higher median incomes ($48,362 *vs.* $47,627; $P < 0.001$). They were also more likely to reside in rural counties, as defined using the 2006 Office of Management and Budget delineation of rural counties[39] (32.7% *vs.* 23.1%; $P < 0.001$). The presence of every comorbidity we included in our analysis was higher among patients in non-"opt out" states ($P < 0.001$ for every comorbidity).

Table 2 presents descriptive statistics concerning travel distances in our sample. The first set of columns report the percentage of patients who traveled outside of their home zip code for each of the procedures, adjusted for zip code land area. Across all the procedures, a solid majority of patients traveled outside of their home zip code (ranging from 77 to 88% of patients). The table also reports the average distances traveled, ranging from roughly 20 km for patients undergoing colonoscopy/sigmoidoscopy to roughly 40 km for patients undergoing THA. There were no significant differences between the "opt out" and non-"opt out" states for either of these distance measures.

Table 2. Average Travel Distances by the Procedure, 1999 to 2012

	% Patients Traveling Outside Home Zip Code			Average Distance Traveled, km		
	Non-"Opt Out"	"Opt Out"	*P* Value	Non-"Opt Out"	"Opt Out"	*P* Value
Total knee arthroplasty	86.4 (0.906)	84.3 (0.864)	0.126	33.2 (0.881)	38.3 (2.94)	0.099
Total hip arthroplasty	87.7 (0.813)	85.6 (0.832)	0.089	34.5 (0.948)	40.6 (3.13)	0.068
Cataract surgery	81.0 (0.820)	79.4 (1.83)	0.456	23.3 (0.613)	25.0 (1.78)	0.420
Colonoscopy/sigmoidoscopy	78.8 (1.13)	72.6 (3.59)	0.117	20.3 (0.875)	22.0 (1.52)	0.338
Esophagogastroduodenoscopy	78.8 (1.04)	75.7 (3.64)	0.436	21.7 (1.10)	25.4 (1.92)	0.090
Appendectomy	79.3 (0.794)	77.5 (1.32)	0.277	24.0 (0.691)	24.5 (1.06)	0.795
Hip fracture repair	79.7 (0.941)	78.6 (1.01)	0.454	29.4 (0.928)	32.1 (1.95)	0.218

This table presents descriptive statistics for our measures of anesthesia access, stratified by patients living in "opt out" and non-"opt out" states. The first set of columns reports the percentage of patients who needed to travel outside of their own zip code for the given procedure, while the second set of columns reports the average distance traveled (in kilometers) for patients who traveled outside their zip code for the given procedure. Both of these distance measures were adjusted for zip code land area. "*P* value" refers to the statistical significance of the difference between "opt out" and non-"opt out" states. These *P* values are without correction for the multiple comparisons. SEs shown in parentheses were adjusted for clustering by state.

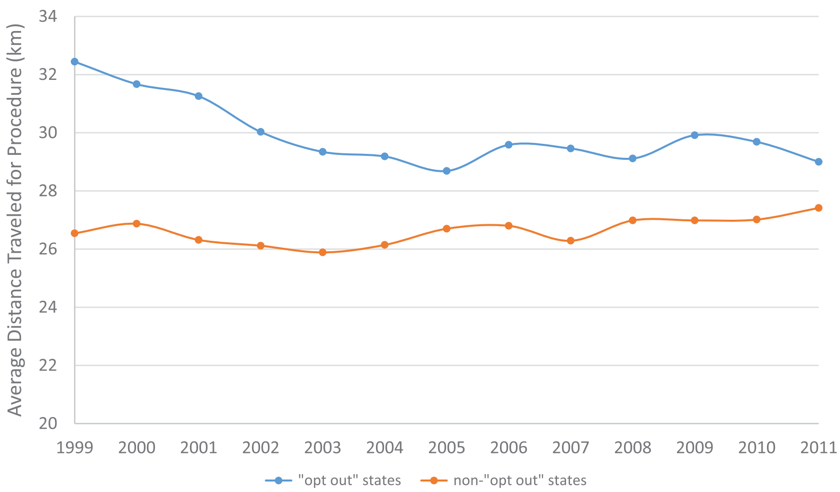

Fig. 1. Average procedure travel distance, 1999 to 2011: the average distance (adjusted for zip code land area) traveled across all seven procedures we studied (in kilometers) in "opt out" and non-"opt out" states.

Figure 1 provides descriptive trends of the average travel distances in our sample, stratified by patients located in "opt out" and non-"opt out" sates. To calculate these average travel distances, we first calculated the average travel distance for each procedure and then took the average (of these averages) across all seven procedures. Qualitatively, the figure suggests that "opt out" may not have had an effect on travel distances. Although travel distance fell in the "opt out" states fell relative to travel distances in non-"opt out" states between 1999 and 2003, this trend predated "opt out," which at the earliest took place in Iowa in December 2001. Between 2003 and 2007—when eight (of a total of 17) states implemented "opt out," travel distances in the "opt out" states remained fairly level, similar to travel distances in the non-"opt out" states.

For the most part, "opt out" was not associated with a decrease in the percentage of patients travelling outside of their home zip code (table 3), with point estimates ranging from a 0.973% point (absolute) decrease in the case of esophagogastroduodenoscopy (95% CI, −1.97 to 0.0329; $P = 0.058$) to a 1.02% point (absolute) increase in the case of appendectomy (95% CI, −6.68 to 8.73; $P = 0.791$). The sole potential exception was THA, where we observed a statistically significant decrease of 2.18% points (95% CI, −3.72 to −0.636; $P = 0.007$, without correction for multiple comparisons).

For patients travelling outside of their zip code, "opt out" had no significant effect on the distance traveled across any of the procedures we examined, with point estimates ranging from a decrease of 7.9 km for appendectomy (95% CI, −19.4 to 3.59; $P = 0.173$) to an increase of 1.55

Table 3. "Opt Out" and Procedure Travel Distance, 1999 to 2011

	Change in % Patients Traveling Outside Home Zip Code	Change in Travel Distance, km
Total knee arthroplasty	−0.0964 (−1.89 to 1.71) *P* = 0.915	1.15 (−1.10 to 3.41) *P* = 0.313
Total hip arthroplasty	−2.18 (−3.72 to −0.636) *P* = 0.007	1.55 (−5.07 to 8.17) *P* = 0.641
Cataract surgery	0.385 (−0.301 to 1.07) *P* = 0.265	0.213 (−0.780 to 1.21) *P* = 0.668
Colonoscopy/sigmoidoscopy	0.251 (−1.89 to 2.40) *P* = 0.815	0.373 (−1.44 to 2.19) *P* = 0.681
Esophagogastroduodenoscopy	−0.973 (−1.97 to 0.0329) *P* = 0.058	−0.391 (−2.59 to 1.82) *P* = 0.723
Appendectomy	1.02 (−6.68 to 8.73) *P* = 0.791	−7.90 (−19.4 to 3.59) *P* = 0.173
Hip fracture repair	−0.00734 (−2.29 to 2.28) *P* = 0.995	−1.53 (−5.31 to 2.26) *P* = 0.422

This table presents the results of two sets of analyses examining the effect of "opt out" on the distance traveled to obtain the given procedure. The first column shows the estimated effect of "opt out" on the probability that a patient had to travel outside of their zip code in absolute (percentage point) terms. The second effect shows the effect of "opt out" on the average distance traveled by patients who traveled outside of their zip code. Both sets of analyses incorporate a variety of controls including zip code effects, year effects, and controls for patient demographics, and cormorbidities. 95% CIs shown in parentheses were adjusted for clustering at the state level.

km (95% CI, −5.07 to 8.17; *P* = 0.641) for THA. Crucially, for all procedures (except appendectomy), even the lower bounds of our 95% CI suggest decreases that would be of negligible practical significance compared with the distances that influence patient decision making,[23,25] suggesting that the lack of estimated effect represents a true null effect, as opposed to imprecision in our estimates.

For our robustness analyses, we first examined whether "opt out" was associated with an increase in the percentage of patients received anesthesia for colonoscopy/sigmoidoscopy, esophagogastroduodenoscopy, or cataract surgery (table 4). Overall, we found that "opt out" was not associated with a significant change in the percentage of patients receiving anesthesia for colonoscopy/sigmoidoscopy (0.391% point decrease; 95% CI, −2.70 to 1.91; *P* = 0.952) or esophagogastroduodenoscopy (0.903% point increase; 95% CI, −1.48 to 3.29; *P* = 0.451). We may have observed a small *decrease* for cataract surgery (1.49% point decrease; 95% CI, −2.92 to −0.0538; *P* = 0.042, without correction for multiple comparisons).

We also considered alternative specifications in which we considered whether "opt out" was associated with a decrease in the *minimum* distance traveled by patients in a given zip code (table 5). Similar to the results shown in table 3, we found that "opt out" was not associated with a significant change in travel distance using these alternative measures for any of the procedures we considered. Alternative specification examining the effect of "opt out" on the fifth percentile and tenth percentile distances traveled by patients in a given zip code also showed no significant effect (supplemental content, appendix table C, http://links.lww.com/ALN/B360).

Finally, we examined the effect of "opt out" between two subpopulations where it might be expected to have a larger effect: patients residing in zip codes with long travel distances initially and low-risk patients (table 5). For both groups of patients, our results suggest that "opt out" was not associated with reduction in travel distance.

Discussion

In this article, we examined the effect of "opt out" on access to anesthesia care using a commonly used measure of access: patient travel distances. Overall, we found that "opt out" was not associated with an increase in access as measured by the percentage of patients who avoided travel outside of their home zip code to receive their procedure or as measured by the travel distances themselves. We did find that "opt out" was associated with a 2% point reduction in the proportion of patients traveling outsider their home zip code for THA; however, given that well more than 80% of THA patients traveled outside their home zip code (table 3), this effect is of little practical significance. In addition, we found that "opt out" had no effect on the use of anesthesia for patients undergoing colonoscopy/sigmoidoscopy or esophagogastroduodenoscopy and was actually associated with a slight *decrease* in the use of anesthesia for cataract surgery. Finally, we did not observe any significant effect on travel distances between two groups of patients where "opt out" might have larger effects: low-risk patients and patients residing in zip codes with initially long travel distances. In general, even the lower bounds of our 95% CIs suggested effects that were of negligible magnitude from a practical standpoint, suggesting that our results are not due to lack of statistical power/precision, with the lone exception being our analysis of patients residing in zip codes with initially long travel distances.

There are many reasons why "opt out" could fail to increase access. First, "opt out" only means that *a federal insurer* (CMS) will pay for cases where a nurse anesthetist is unsupervised by a physician; a whole host of other parties—such as private insurers, hospitals, surgeons, and patients—may have their own preferences and impose their own requirements. To the degree that these other parties require physician supervision, "opt out" may not achieve its intended effect. Second, the implicit assumption of "opt out" is that availability of anesthesia care is the factor that limits access to surgical procedures. However, other factors, such as the availability of surgeons and hospitals, could affect access and would not be addressed

Table 4. "Opt Out" and Anesthesia for Colonoscopy, Esophagogastroduodenoscopy, and Cataract Surgery, 1999 to 2011

	Unadjusted Percentage of Patients Receiving Anesthesia, %			Effect of "Opt Out" on Change in Percentage of Patients Receiving Anesthesia
	Non-"Opt Out"	"Opt Out"	*P* Value	
Colonoscopy/sigmoidoscopy	24.8 (0.0473)	9.47 (0.0653)	< 0.0001	−0.391 (−2.70 to 1.91) *P* = 0.735
Esophagogastroduodenoscopy	23.9 (0.0452)	9.59 (0.0654)	< 0.0001	0.903 (−1.48 to 3.29) *P* = 0.451
Cataract surgery	90.1 (0.0490)	85.2 (0.106)	< 0.0001	−1.49 (−2.92 to −0.0538) *P* = 0.042

This table shows the effect of "opt out" on the proportion of patients receiving anesthesia care for patients undergoing colonoscopy/sigmoidoscopy, esophagogastroduodenoscopy, and cataract surgery. The first set of columns reports the overall proportion of patients receiving anesthesia care for the given procedure, stratified by "opt out" and non-"opt out" states. "*P* value" refers to the significance of the differences between the two groups as assessed by a chi-square test. The final column reports the estimated effect of "opt out" on the proportion of patients receiving anesthesia for their procedure, as measured in absolute (percentage point) terms. For example, "−0.0751" means that "opt out" was associated with a 0.0751% point decrease in the proportion of patients receiving anesthesia for colonoscopy/sigmoidoscopy. Not shown are controls for zip code effects, year effects, patient demographics, and comorbidities. 95% CIs are shown in parentheses and were adjusted for clustering at the state level.

Table 5. "Opt Out" and Procedure Travel Distance (Additional Analyses), 1999 to 2011

		Zip Codes with Initial Travel Distance > 50 km		Low-risk Patients	
	Change in Minimum Travel Distance, km	Change in % Patients Traveling Outside Home Zip Code	Change in Travel Distance, km	Change in % Patients Traveling Outside Home Zip Code	Change in Travel Distance, km
Total knee arthroplasty	0.500 (−2.15 to 3.15) *P* = 0.707 n = 87,949	1.29 (−3.89 to 6.44) *P* = 0.617 n = 24,083	0.618 (−11.2 to 12.5) *P* = 0.917 n = 21,622	0.0569 (−1.74 to 1.86) *P* = 0.950 n = 118,186	0.605 (−1.76 to 2.98) *P* = 0.611 n = 101,665
Total hip arthroplasty	1.00 (−7.28 to 9.28) *P* = 0.809 n = 49,025	−1.73 (−6.29 to 2.83) *P* = 0.449 n = 11,329	5.32 (−19.3 to 30.0) *P* = 0.666 n = 10,260	−1.51 (−3.22. 0.218) *P* = 0.086 n = 51,202	0.329 (−6.57 to 7.22) *P* = 0.924 n = 44,744
Cataract surgery	1.06 (−0.804 to 2.93) *P* = 0.259 n = 141,432	0.831 (−2.25 to 3.90) *P* = 0.590 n = 29,907	4.57 (−4.18 to 13.3) *P* = 0.487 n = 27,187	0.582 (−0.276 to 1.44) *P* = 0.179 n = 339,648	0.141 (−0.845 to 1.13) *P* = 0.776 n = 273,756
Colonoscopy/ sigmoidoscopy	1.74 (−0.643 to 4.13) *P* = 0.149 n = 72,021	−1.81 (−10.8 to 7.19) *P* = 0.688 n = 12,644	−0.953 (−21.2 to 19.2) *P* = 0.925 n = 10,596	−0.192 (−2.58 2.19) *P* = 0.872 n = 154,640	−0.418 (−2.10 to 1.27) *P* = 0.620 n = 120,592
Esophagogas troduodenoscopy	−1.50 (−5.63 to 2.63) *P* = 0.469 n = 73,041	−2.12 (−8.49 to 4.24) *P* = 0.506 n = 14,113	−0.200 (−16.6 to 16.2) *P* = 0.981 n = 12,044	−2.07 (−4.53 to 0.396) *P* = 0.098 n = 122,085	−0.311 (−3.65 to 3.02) *P* = 0.852 n = 95,126
Appendectomy	−7.06 (−16.2 to 2.10) *P* = 0.128 n = 17,304	1.96 (−38.7 to 42.7) *P* = 0.923 n = 1,213	−17.1 (−211 to 177) *P* = 0.860 n = 1,111	2.65 (−9.81 to 15.1) *P* = 0.671 n = 13,618	−3.48 (−26.1 to 19.1) *P* = 0.758 n = 10,696
Hip fracture repair	−1.73 (−6.12 to 2.65) *P* = 0.431 n = 77,089	3.54 (−6.16 to 13.2) *P* = 0.467 n = 12,306	3.03 (−20.0 to 26.1) *P* = 0.793 n = 10,651	−1.19 (−2.77 to 2.52) *P* = 0.922 n = 61,295	−2.08 (−8.12 to 3.97) *P* = 0.493 n = 48,049

This table reports the results of two additional sets of robustness/sensitivity analyses. The first column report the results of an alternative model specification in which the dependent variable was the minimum distance traveled by patients living in the given zip code. The next two columns report the results of analyses in which we restricted our sample to zip codes whose initial travel distances were > 50 km. The last two columns reports the results of analyses in which we restricted our sample to low-risk patients. All analyses incorporate a variety of controls including zip code effects, year effects, and controls for patient demographics and comorbidities. 95% CIs shown in parentheses were adjusted for clustering at the state level. "n" is the number of observations (*i.e.*, surgical cases) for the given analysis.

by "opt out." Third, "opt out" would have no effect for those instances where the nurse anesthetist is directly supervised by the proceduralist.

Our results should be viewed in light of their limitations. First, while using the distance between zip codes is an accepted method in the literature, one shortcoming is that we are unable to observe changes in distance *within* a given zip code, so that if "opt out" shortened travel distances *within a zip code* (*e.g.*, if a second, closer surgery center opened within a given zip code), we would not observe this effect. However, it does seem unlikely that the effect of "opt out" would be only to shorten distances within a zip code, without affecting the broader measures of distance that we examined. Second, since our study was conducted among the Medicare population, we were unable to ascertain the effect of "opt out" on travel distances for procedures that were rarely performed in this population but may be important from a policy standpoint. Third, while our study considered several commonly performed procedures, it is possible that "opt out" may have reduced travel distances for procedures that we did not examine. Crucially, our study does not address whether "opt out" may have improved access to care for obstetrics procedures. Fourth, we cannot exclude the possibility that the lack of effect could be explained by other confounding variables, such as unobserved factors occurring at the zip code or patient level. Finally, we note that our study did not examine whether "opt out" may have affected the level (*i.e.*, quality or effort) of supervision.

In conclusion, our article suggests that "opt out" did not significantly shorten travel distances for the procedures we examined. In combination with previous work suggesting that "opt out" did not increase the total number of surgeries performed,[40,41] our work suggests that "opt out" has not been effective in increasing two important dimensions of access to anesthesia care: number of procedures performed or decreased travel distances for surgery. In sum, policymakers should understand that "opt out" is unlikely to be a "silver bullet" when it comes to increasing access to anesthesia care. Understanding the reasons why "opt out" has not achieved its intended goal remains an area for further research.

Research Support

Support was provided solely from institutional and/or departmental sources.

Competing Interests

Dr. Sun received funding from a Mentored Research Training Grant from the Foundation for Anesthesia Education and Research, Schaumburg, Illinois. Dr. Miller is employed by the American Society of Anesthesiologists, Schaumburg, Illinois. The other authors declare no competing interests.

Correspondence

Address correspondence to Dr. Sun: Department of Anesthesiology, Perioperative and Pain Medicine, H3580, Stanford University Medical Center, Stanford, California 94305. esun1@stanford.edu. This article may be accessed for personal use at no charge through the Journal Web site, www.anesthesiology.org.

References

1. Daugherty L, Fonseca R, Kumar KB, Michaud P-C: An Analysis of the Labor Markets for Anesthesiology. Santa Monica, California, RAND Corporation, 2010
2. Miller TR, Abouleish A, Halzack NM: Anesthesiologists are affiliated with many hospitals only reporting anesthesia claims using modifier QZ for Medicare claims in 2013. A A Case Rep 2016; 6:217–9
3. Lewis SR, Nicholson A, Smith AF, Alderson P: Physician anaesthetists *versus* non-physician providers of anaesthesia for surgical patients. Cochrane Database Syst Rev 2014; 7:CD010357
4. Needleman J, Minnick AF: Anesthesia provider model, hospital resources, and maternal outcomes. Health Serv Res 2009; 44(2 Pt 1):464–82
5. Kane M, Smith AF: An American tale—Professional conflicts in anaesthesia in the United States: Implications for the United Kingdom. Anaesthesia 2004; 59:793–802
6. Fleisher LA, Anderson GF: Perioperative risk: How can we study the influence of provider characteristics? Anesthesiology 2002; 96:1039–41
7. Abenstein JP, Warner MA: Anesthesia providers, patient outcomes, and costs. Anesth Analg 1996; 82:1273–83
8. Pine M, Holt KD, Lou YB: Surgical mortality and type of anesthesia provider. AANA J 2003; 71:109–16
9. Silber JH, Kennedy SK, Even-Shoshan O, Chen W, Koziol LF, Showan AM, Longnecker DE: Anesthesiologist direction and patient outcomes. Anesthesiology 2000; 93:152–63
10. Simonson DC, Ahern MM, Hendryx MS: Anesthesia staffing and anesthetic complications during cesarean delivery: A retrospective analysis. Nurs Res 2007; 56:9–17
11. Smith AF, Kane M, Milne R: Comparative effectiveness and safety of physician and nurse anaesthetists: A narrative systematic review. Br J Anaesth 2004; 93:540–5
12. Dulisse B, Cromwell J: No harm found when nurse anesthetists work without supervision by physicians. Health Aff (Millwood) 2010; 29:1469–75
13. Gov. Beshear Takes Step to Increase Access to Safe, High-quality Anesthesia Services. Available at: http://migration.kentucky.gov/Newsroom/governor/20120427anesthesia.htm. 2012. Accessed December 30, 2016
14. Ritter B: Colo. To Opt Out of Feds' Nurse Anesthetist Rule for Rural Hospitals. Available at: http://votesmart.org/public-statement/557996/colo-to-opt-out-of-feds-nurse-anesthetist-rule-for-rural-hospitals#.WG0ioFUrLRa. 2010. Accessed December 30, 2016
15. Bodenheimer TS, Smith MD: Primary care: Proposed solutions to the physician shortage without training more physicians. Health Aff (Millwood) 2013; 32:1881–6
16. Sun E, Dexter F, Miller TR: The effect of "opt-out" regulation on access to surgical care for urgent cases in the United States: Evidence from the National Inpatient Sample. Anesth Analg 2016; 122:1983–91
17. Sun EC, Miller TR, Halzack NM: In the United States, "opt-out" states show no increase in access to anesthesia services for Medicare beneficiaries compared with non-"opt-out" states. A A Case Rep 2016; 6:283–5
18. Rahman M, Zinn JS, Mor V: The impact of hospital-based skilled nursing facility closures on rehospitalizations. Health Serv Res 2013; 48(2 Pt 1):499–518
19. Kelley-Quon LI, Tseng CH, Jen HC, Lee SL, Shew SB: Hospital type as a metric for racial disparities in pediatric appendicitis. J Am Coll Surg 2013; 216:74–82
20. Buchmueller TC, Jacobson M, Wold C: How far to the hospital? The effect of hospital closures on access to care. J Health Econ 2006; 25:740–61
21. Jackson KL, Glasgow RE, Mone MC, Sheng X, Mulvihill SJ, Scaife CL: Does travel distance influence length of stay in elective pancreatic surgery? HPB (Oxford) 2014; 16:543–9
22. Chou S, Deily ME, Li S: Travel distance and health outcomes for scheduled surgery. Med Care 2014; 52:250–7
23. Dexter F, Birchansky L, Bernstein JM, Wachtel RE: Case scheduling preferences of one Surgeon's cataract surgery patients. Anesth Analg 2009; 108:579–82
24. O'Neill L, Dexter F: Tactical increases in operating room block time based on financial data and market growth estimates from data envelopment analysis. Anesth Analg 2007; 104:355–68
25. Dexter F, Wachtel RE, Sohn MW, Ledolter J, Dexter EU, Macario A: Quantifying effect of a hospital's caseload for a surgical specialty on that of another hospital using multi-attribute market segments. Health Care Manag Sci 2005; 8:121–31
26. An Overview of Medicare. Available at: http://kff.org/medicare/issue-brief/an-overview-of-medicare/. 2016. Accessed December 30, 2016
27. Medicare Advantage Enrollees as a Percent of Total Medicare Population. Available at: http://kff.org/medicare/state-indicator/enrollees-as-a-of-total-medicare-population/?currentTimeframe=0. 2015. Accessed December 30, 2016
28. U.S. Gazetteer Files. Available at: https://www.census.gov/geo/maps-data/data/gazetteer.html. 2016. Accessed December 30, 2016
29. van Walraven C, Austin PC, Jennings A, Quan H, Forster AJ: A modification of the Elixhauser comorbidity measures into a point system for hospital death using administrative data. Med Care 2009; 47:626–33
30. Quan H, Sundararajan V, Halfon P, Fong A, Burnand B, Luthi JC, Saunders LD, Beck CA, Feasby TE, Ghali WA: Coding algorithms for defining comorbidities in ICD-9-CM and ICD-10 administrative data. Med Care 2005; 43:1130–9
31. Sun EC, Dexter F, Macario A, Miller TR, Baker LC: No significant association between anesthesia group concentration and private insurer payments in the United States. Anesthesiology 2015; 123:507–14
32. Buckley J, Shang Y: Estimating policy and program effects with observational data: The "differences-in-differences" estimator. Pract Assess Res Evaluation 2003; 8:1. Retrieved April 16, 2015 at: http://pareonline.net/getvn.asp?v=8&n=2
33. Dimick JB, Ryan AM: Methods for evaluating changes in health care policy: The difference-in-differences approach. JAMA 2014; 312:2401–2
34. Vetter TR, Mascha EJ, Kilgore ML: Physician supervision of nurse anesthetists: To opt in or to opt out? Anesth Analg 2016; 122:1766–8
35. Amemiya T: Regression analysis when the dependent variable is truncated normal. Econometrica 1973; 41:997–1016
36. Goldman DP, Jena AB, Lakdawalla DN, Malin JL, Malkin JD, Sun E: The value of specialty oncology drugs. Health Serv Res 2010; 45:115–32
37. O'Donnell O, Doorslaer Ev, Wagstaff A, Lindelow M: Nonlinear Models for Health and Expenditure Data, Analyzing Health Equity Data Using Household Survey Data: A Guide to Techniques and Their Implementation. Washington, D.C, The World Bank, 2008, pp 131–45

38. Bertrand M, Duflo E, Mullainathan S: How much should we trust differences-in-differences estimates? Q J Econ 2004; 119: 249–275
39. United States Census Bureau: Historical Delineation Files Available at: http://www.census.gov/population/metro/data/defhist.html. 2013. Accessed December 30, 2016
40. Sun EC, Dexter F, Miller TR: The effect of "opt out" regulation on access to surgical care in the United States: Evidence from the National Inpatient Sample. Anesth Analg 2016; 12:1983–91
41. Sun EC, Miller TR, Halzack NM: In the United States, "opt-out" states show no increase in access to anesthesia services for Medicare beneficiaries compared with non-"opt-out" states. A A Case Rep 2016; 6:283–5