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Patient volumes and pre and post discharge postpartum infection: a retrospective cohort study

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Abstract

Objective—To examine the association between hospital and clinician obstetric volume and postpartum infection risk in the pre and post discharge periods.

Study Design—We used data from the 2011 New York State Inpatient and Emergency Department Databases to fit GEE models to examine the effect of hospital and clinician obstetric volume on infection before discharge, and in the 30 days following discharge after delivery.

Results—Higher clinician volume was associated with lower pre-discharge infection risk (OR for 1st versus 3rd quartile was 0.84, 95% CI 0.77 to 0.98). There was an uncertain trend towards higher pre-discharge infection risk in higher volume hospitals (OR for 1st versus 3rd quartile was 1.36, 95% CI 0.79 to 2.34). We found no associations between patient volumes and post-discharge infections, although power was insufficient to rule out small associations. The joint association of hospital and clinician volumes with post-discharge infection appeared sub-multiplicative (product term OR=0.95, 95% CI 0.92–0.98).

Conclusion—This study adds to the evidence that hospital obstetric volume is positively associated with pre-discharge postpartum infections while clinician volume may be negatively associated with those pre-discharge infections. The associations between hospital obstetric volume and post-discharge infection appear to differ. These results underscore the importance of including post-discharge follow-up in hospital-based studies of postpartum infection.

Keywords

Puerperal infection; postpartum infection; patient volume; patient safety

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Introduction

A large body of literature has found relationships between patient volumes (at both facility and clinician levels) and patient outcomes. A number of studies have examined the relationship between patient volumes and perinatal outcomes. Several studies have found better neonatal outcomes with higher volume providers.(1–3) However, the relationship between volume and maternal outcomes is not as well documented. Quality research on the effect of patient volumes on maternal outcomes is crucial to determining the ideal patient volumes for both hospitals and clinicians.

Postpartum infection is a common complication of childbirth.(4) There is evidence to suggest that higher volume facilities and departments may predispose patients to certain types of infections.(5, 6) There are also reasons to hypothesize that clinician volume may affect infection risk. A handful of studies have found lower risk of infection when surgical procedures are performed by surgeons with higher patient volumes.(7–9)

We found three previous US studies that sought to examine the association obstetric volume and a broad set of postpartum infectious outcomes. Janakiraman found an increased risk of infection in higher volume facilities (although this association was not adjusted for patient mix).(10) Goff et al. also found an increasing risk of infection with increasing hospital obstetric volume, while noting that it explained a relatively small proportion of hospital differences in infection rates, and Kyser et al. found a non-monotonic relationship, where patients in hospitals with very low and very high volumes were at greater risk than patients at mid-volume hospitals.(10–12) Janakiraman et al. also found that patients attended by low volume clinicians had higher risks of infection. All three studies considered only infections occurring during the index hospitalization (that is, the hospitalization for delivery).

We are unaware of any published studies of the relationship between hospitals' or clinicians' obstetric volume and postpartum infections in the period after discharge from the hospital. This is a substantial gap in the literature, as the majority of postpartum infections are diagnosed post-discharge.(4, 13) We sought to examine the associations of obstetric volumes at the hospital and clinician levels with a variety of postpartum infections, including infections diagnosed both during the index hospitalization and in readmissions and emergency room visits following discharge.

Materials and Methods

Data Source

The study data came from the 2011 New York State Inpatient Database (SID) and the New York State Emergency Department Database (SEDD), products of the healthcare cost and utilization project (HCUP), Agency for Healthcare Research and Quality and the New York State Department of Health. These databases are derived from administrative data, and contain the universe of non-Federal hospital and emergency department discharges for New York State in 2011. Each individual patient in the SID and SEDD is given a unique identification number (based on first and last name and date of birth) which allows tracking across admissions, facilities, and settings (inpatient versus emergency department) without

compromising the privacy of the patient. Each record is also assigned a masked date variable, which allows calculation of the number of days between admissions.

Because the data source was publicly available and de-identified, this study was exempt from review by the UCLA Institutional Review Board.

Study Group

The study group consists of women who delivered an infant in a New York State hospital in 2011 and were assigned a unique identification number. Deliveries are identified using the method developed by Kuklina et al.(14) We exluded women if they were transferred from another hospital prior to delivery, or had unknown transfer status because they were exposed to multiple hospitals and clinicians, and we were unclear if it would be more appropriate to use the volumes of the pre or post transfer hospital and clinician. Because including multiple deliveries per patient would dictate that we treated individual deliveries as repeated measures within patients, we excluded a small number (<300) of second deliveries to women who had multiple deliveries in 2011. For pre-discharge infections, we considered only those conditions which were recorded as not present on admission. For post-discharge infections, we excluded women who developed an infection during the index hospitalization.

We sought to limit our study to hospitals and clinicians that were intended to be providers of labor and delivery services, and excluded those providers that occasionally performed deliveries as emergency providers. To this end, we excluded deliveries in hospitals with fewer than 25 deliveries and clinicians with fewer than ten deliveries in 2011.

Outcomes

The outcomes of interest were postpartum infections presenting during hospitalization for delivery of an infant or in a readmission or emergency department visit with 30 days after discharge following delivery. We coded this as a dichotomous variable equal to 1 for one or more infections, and 0 for no infection. Because all data were from 2011, women who delivered after December 1st had less than 30 days of follow-up. Table 1 lists types of infections and associated ICD-9 CM codes. We chose to include ICD-9 CM codes 674.12 and 674.14 (disruption of cesarean wound), and 674.32 and 674.34 (other complications of obstetrical surgical wounds) because cesarean wound infections comprise a large proportion of postpartum infections, there are no specific ICD-9 CM codes for cesarean wound infections, and we expect a large proportion of surgical wound complications to be infections based on prior research.(4, 15, 16)

Obstetric Volume Measures

The predictor variables of interest were obstetric volumes at the hospital and clinician (physician or midwife) levels. These were measured as the numbers of deliveries at each hospital and by each clinician in 2011.

Covariates

Patient level covariates were selected to provide as complete of a case mix adjustment as possible, and included age, race, expected payer (private, Medicaid, or other) and a set of

obstetric and non-obstetric comorbidities. Obstetric comorbidities are listed in Table 2. As we did not wish to adjust for potential intermediates between obstetric volume and infections, we focused on conditions that typically present prior to labor, and thus prior to hospital admission.

To adjust for non-obstetric comorbidities we used a modified version of the method developed by Elixhauser et al.(17) The Elixhauser index is a widely used method to adjust for hospital case mix, and in particular, to account for the fact that certain hospitals have a disproportionate number of patients at high risk for adverse events. While it was not developed specifically to adjust for risk of infection, it is frequently used to risk adjust patient populations for studies of infectious complications, has been used previously to risk adjust in studies of postpartum infection, and has been shown to predict certain infectious outcomes.(11, 18) As some of the conditions in the Elixhauser index are very uncommon among childbearing women, we combined conditions affecting less than 500 deliveries (approximately 1 of every 400 deliveries) into composite variables. Non-obstetric comorbidities are listed in Table 3. Given that cesarean delivery and length of hospital stay were possible intermediates in the volume-infection pathway, and length of stay could be affected by infection, we did not include cesearean section or length of hospital stay as adjustment covariates.

Hospital teaching status is also used as a covariate. Hospitals with Obstetrics and Gynecology residency programs during the 2011 calendar year were identified using the American Medical Association's *Graduate Medical Education Directory* for 2010–2011 and 2011–2012.

Data Analysis

We fit a series of multivariable adjusted logistic regression models based on generalized estimating equations (GEE) to quantify the associations of obstetric volumes at the hospital and clinician levels with postpartum infections while accounting for the clustering of outcomes within clinicians and within hospitals. Because some clinicians attended deliveries in multiple hospitals, we used Miglioretti and Heagerty's method for marginal modeling of non-nested multilevel data.(19) Finally, we used the non-nested modeling approach to fit models adjusted for the complete set of covariates. We used complete record analysis for multivariable models, as race or clinician volume were missing for only approximately 1% of the sample, and all other variables were never missing. As we sought to examine whether the effect of obstetric volume on postpartum infection risk varied between pre-discharge and post-discharge infections, we ran separate models for the prediction of pre-discharge and post-discharge infections. To ensure that our odds ratio compared relatively low volume and relatively high volume hospitals and clinicians, hospital and clinician volume were recentered to the first quartile, and rescaled to the interquartile range.

Furthermore, because some clinicians had implausibly high numbers of deliveries, we conducted a number of sensitivity analyses to account for possible assignment of deliveries to clinicians that did not actually perform the delivery. Given that the clinician identifier was based on the license number, deliveries done by non-licensed resident physicians and other trainees may be attributed to other clinicians. Thus, we performed analyses limited to non-

teaching hospitals. In addition, we ran models where we imputed a clinician level volume of 365 for the reported volume for very high volume clinicians (defined here as clinicians with more than 365 reported deliveries) and models where very high volume clinicians and the deliveries assigned to them were excluded.

We conducted all analyses in SAS version 9.3 (SAS Inc., Cary, NC).

Results

There were 233,865 deliveries to women in New York State non-Federal hospitals in 2011. We excluded 9,925 women who were missing the unique identifier that enabled tracking across visits, 237 hospitalizations for second deliveries from women who had more than one delivery in a calendar year, 4,089 women who transferred from other facilities prior to delivery or had missing transfer status, one woman with a missing clinician identifier, and 1,659 women who delivered with very low volume providers or hospitals, leaving a final group of 217,954 women followed up for postpartum infections. The first quartile, median, and third quartiles of hospital delivery volumes, were 503, 1,274, and 2,313, with a maximum volume of 7,519. The quartiles of clinician volume were 43, 79 and 126, with a maximum of 806.

Two thousand three hundred twenty seven women developed a pre-discharge infection. One thousand, seven hundred and sixty seven infections were diagnosed in emergency rooms, and 870 infections were diagnosed in readmissions. In total, 2,549 women had at least one infection in the post-discharge period; a small number of women had infections presenting in both emergency department visits and readmissions. In the pre-discharge period, the most common types of infection were endometritis (47% of infections), surgical wound complications (33%), and urinary tract infections (a distant third at 16%). In the post-discharge period, urinary tract infections (47%) and surgical would complications (39%) predominated, and endometritis were less common (12%). Ten percent of pre-discharge infections and seven percent of post-discharge infections involved sepsis or bloodstream infections, and ten percent of cases in each period involved multiple documented infections.

2,481 women had missing values for race, leaving 215,473 women (92% of all deliveries) for inclusion the multivariable models. Results of the regression models are shown in Table 4. Hospital and clinician deliveries are rescaled to the interquartile range (1,810 deliveries per hospital and 93 deliveries per clinician) and recentered to the first quartile. The unadjusted odds ratios for the effect of a volume increase equal to the interquartile range were 2.22 (95% confidence interval 1.71 to 2.90) and 0.87 (95% confidence interval 0.77–0.98), respectively. For post discharge infections, the unadjusted odds ratios were 0.94 (95% CI 0.74 to 1.21) and 1.09 (95% CI 0.99–1.12). The multivariable adjusted odds ratios were 1.36 (95% CI 0.79 to 2.34) for hospital volume and 0.84 (95% CI 0.73 to 0.98) for clinician volume. For post-discharge infections, these odds ratios were 0.85 (95% CI 0.69 to 1.06) and 1.04 (95% CI 0.96 to 1.13). Exclusion of women who had less than 30 days of post discharge due to year end deliveries did not alter the results and conclusions substantially (results are available from the authors upon request).

Restriction to cesarean deliveries did not alter the effect measures appreciably in either pre-discharge or post-discharge infections. In the model restricted to vaginal deliveries, the odds ratio for the effect of hospital volume on pre-discharge infection was 1.81 (p=0.06), while the effect of clinician volume on pre-discharge infection did not change substantially, and the effect of both hospital and clinician volume on post-discharge infection was very close to null.

Odds ratios for all quadratic terms for hospital effects were below one, but not statistically significant while odds ratios for quadratic terms for clinician volume were equal to 1.02 and marginally significant for all pre-discharge models. Odds ratios for quadratic terms for both hospital and clinician volumes were very close to null in all post-discharge models. Product terms were close to one and non-significant for pre-discharge models, but slightly below one for all post-discharge models.

In the sensitivity analysis, restriction to non-teaching hospitals had little effect on the point estimate for the effect of clinician volume on pre-discharge infections, but the confidence intervals were substantially wider due to inclusion of fewer hospitals (OR=0.85, 95% CI 0.67 to 1.09). For the model where an annual delivery volume of 365 was imputed for clinicians with very high reported volumes, the odds ratio moved toward, but not to, the null, and the standard error increased (OR=0.93, 95% CI 0.72 to 1.21). The odds ratio for the model where patients of clinicians with very high reported volume were excluded was very close to the all patient multivariable adjusted odds ratio, but was no longer significant at α=0.05 (OR=0.88, 95% CI 0.67 to 1.16. For post-discharge models, there was no clear trend in the association with either hospital or clinician volume in any of the sensitivity analyses.

Discussion

Given that our study adds to the literature on the relationship between hospital and clinician patient volumes and risk of maternal infection, it is important to consider our results within the context of previous findings. While we failed to find a statistically significant association between hospital volume and postpartum infection risk in all multivariable adjusted models, we would not conclude that our results are in conflict with those of Janakiramen et al. and Goff et al. that higher hospital volumes are associated with higher infection risks in the pre-discharge period simply because their results are significant at α=0.05 and ours are not. We chose to restrict our analysis to deliveries in New York State because New York both provides the ability to track patients between admissions and ER visits and provides unique clinician identifiers to both physicians and midwives. However, this restriction meant that our study sample included both fewer hospitals and fewer patients than those of previous studies, leading to a lower probability of statistical significance given a true association. Nonetheless, the point estimate for the association of pre-discharge hospital volume and infection was above one for all models, which lends support to the hypothesis that the true odds ratio for the association could be above one, even if the association is not significant at α=0.05, and the confidence interval for this estimate covers values that correspond to a substantially elevated risk of infection in larger hospitals..

Higher clinician volumes were consistently associated with decreased risk of pre-discharge infections in all models. In the three models included to provide a sensitivity analysis of the possible effect of attributing deliveries to clinicians that did not attend them, the association was not significant at the α=0.05 level. In the models where high volume clinicians were either excluded, or assigned a delivery volume of 365, the effect measure was attenuated, whereas in the teaching only sub-analysis, the point estimate actually moved away from the null (but the variance increased dramatically due to inclusion of fewer hospitals). This suggests that misclassification of clinician delivery volume may play some role in its observed association with pre-discharge infection risk, but it is difficult to know how much. We have no way of validating the accuracy of the attending clinician, and are unaware of any studies that have attempted to do so with hospital discharge data sets.

As the odds ratios for all quadratic terms for hospital volume were statistically non-significant, we were unable to find evidence of non-monotonicity in the effect of hospital volume on infection risk, but could not conclusively rule out departures from monotonicity. We did find marginally significant effects of the quadratic term for clinician volume, suggesting a possible non-monotonic effect.

We did not find similar associations for post-discharge infections. In our main multivariable adjusted analysis, the direction of the association was reversed for both volume measurements, with hospital volume associated with a slight decrease in risk, and provider volume associated with a slight increase in risk, however, neither was significant. The direction of the association between hospital volume and infection was not consistently above or below null in all models. Odds ratios for the association of clinician volume with infection were at or slightly above null for all models. The product term in post discharge models was slightly below one in all cases except for the non-teaching only sub-analysis, suggesting possible departures from multiplicativity of the effects of hospital and clinician volumes on post-discharge infections.

Before discussing the implications of these findings, we would like to discuss the strengths and weaknesses of this study. The principle strength of this analysis is the inclusivity of the study group, which contains the vast majority of births in New York State, and 92% of deliveries in Non-Federal New York hospitals. This aids internal validity as the study population is almost identical to the source population, limiting the potential for selection bias. In addition, the hospital volume measure has very little potential for misclassification.

A limitation of this study is the potential for incomplete reporting of outcome variables. Validation studies of administrative data for obstetric conditions and complications, including postpartum infections, have generally found high specificity, but sometimes low specificity.(20–22) It should be noted that we would only expect low sensitivity with high specificity to bias if misclassification is differential with respect to exposure status.(23)

Another potential limitation of this study is the possible misclassification of clinician delivery volume due to incorrect assignment of deliveries to clinicians. The clinician identifier was based on license number, so deliveries performed by unlicensed trainee clinicians, could not be properly attributed.(24) In addition, we were unable to account for

the participation of multiple clinicians in one delivery. Because the clinician identifier allowed tracking of individual clinicians across multiple hospitals, we were able to construct a clinician volume measure that accounted for deliveries in New York State, but not in other states. This might have led to underestimates of volume for clinicians who regularly practiced in multiple states, or who moved in or out of New York State partway through the year. We were also unable to identify clinicians who had low volume because they retired or stopped performing deliveries partway through the year. We expect that clinicians who performed deliveries in multiple states or stopped doing deliveries partway through the year are a small proportion of total clinicians, and do not expect this to affect our estimates substantially. We also noted that some clinicians had implausibly high numbers of deliveries. We have no method to validate the clinician volume values or determine how often misattribution of deliveries occurs, but we have conducted sub-analyses to try and account for the possible effect of misclassification.

We are unable to distinguish physicians from midwifes. This is a limitation because midwives generally care for women with uncomplicated pregnancies and deliveries, and who are likely at lower risk for infection. We have controlled for a number of pre-labor pregnancy complications that are likely to predict pre-labor choice of midwives versus physicians as delivery attendants, but are unable to account for intrapartum transfers between clinicians, including transfers from midwives to physicians.

While we have attempted to select a fairly comprehensive set of infections, it is not possible to construct a set of all infectious conditions that might result from childbirth or medical care given during childbirth. Because women are not randomly assigned to hospitals or clinicians for delivery, there is a possibility for confounding. We have attempted to control for relevant demographic and socioeconomic factors and comorbidities, but we can’t rule out the possibility of confounding by unmeasured factors. Also, comorbidities may be underreported, leading to residual confounding. The study is limited to deliveries in New York State, which limits generalizability if the relationship between obstetric volumes and infection risks differs between geographic regions. Finally, we were only able to observe post-discharge infections that presented in emergency departments and hospital readmissions. We do not know how many infections we are missing, because it is not known what proportion of postpartum infections are treated outside of hospitals and emergency departments. We are only aware of one US study that examined postpartum infections presenting in different clinical settings, which is more than a decade old, was limited to patients in one HMO in the Northeast, and did not explicitly state how many infections were seen exclusively in outpatient settings.(4) It is possible that the predictors of post-discharge infections that present in other settings differ from those that present in hospital settings. Furthermore, due to our inability to observe diagnoses in all settings, it is important that the results of our study not be used to calculate an overall risk of post-discharge postpartum infection, or to calculate a ratio of pre-discharge to post-discharge infections.

This study builds upon previous research into the relationship between obstetric volume and postpartum infection. When combined with this previous research, our results provide strong support for the hypotheses that higher hospital volumes and lower clinician volumes are associated with a higher risk of postpartum infections in the pre-discharge period. However,

in the absence of major bias, we found that these associations do not appear to apply to infections in the post-discharge period, or at least not to those infections that present in emergency departments and readmissions. The inconsistent relationship between hospital and clinician volumes and postpartum infection makes it difficult to make conclusions about the overall risks or benefits of delivering with high or low volume providers. This difficulty is compounded by the fact that many postpartum infections are diagnosed outside of a hospital setting, and it is unknown whether the relationship between post-discharge infections and volume is the same in and outside of the hospital.

As this is, to the best of our knowledge, the first study to examine the relationship between patient volumes and pre and post-discharge postpartum infections, we consider our findings to be preliminary. Thus, it is not appropriate for us to make recommendations for clinical practice until additional research is conducted. The results and the limitations of this study have important implications for future research on the effects hospital and clinician characteristics on postpartum infection. They speak to the importance of including post-discharge infections in studies of postpartum infection, as the predictors of pre and post-discharge infections may differ. They also highlight the need for further research that includes surveillance for infections that occur in all settings, including inpatient, hospital outpatient, and office and clinic based settings, as well as the need for a validation of methods of assigning deliveries to clinicians in administrative data.

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Table 1

(Maternal) Outcomes and associated ICD-9 CM codes

Condition	ICD-9 CM Codes
Urinary Tract Infections	032.84, 5900.0, 590.01, 590.10, 590.11, 590.2, 590.3, 590.80, 590.81, 590.9, 595.0, 595.1, 595.2, 595.3, 595.4, 595.81, 595.82, 595.89, 595.9, 597.0, 597.80, 597.81, 597.89, 598.00, 598.01, 599.0
Sepsis and Bloodstream Infections	670.20, 670.22, 670.24, 670.30, 670.32, 670.34, 038.0, 038.10, 038.11, 038.12, 038.19, 038.2, 038.3, 038.4, 038.41, 038.42, 038.43, 038.44, 038.49, 038.8, 038.9, 785.52, 790.7, 995.90, 995.91, 995.92, 998.02;
Genital Tract Infections	670.10, 670.12, 670.14
Surgical Wound Complications	674.12, 674.14, 674.32, 674.34
Other Postsurgical Infections	998.5, 998.59, 998.51
Other Major Puerperal Infections	670.00, 670.02, 670.04, 670.80, 670.82, 670.84
Device Associated Infection	996.60, 996.62

Table 2

Obstetric comorbidities and associated ICD-9 CM Codes

Condition	ICD-9 CM Codes
Herpes	054–054.9
Eclampsia and severe pre-eclampsia	642.50–642.64
Hypertension and mild pre-eclampsia	642.00–642.44, 642.70–642.94
Hemorrhage, abruption placentae, and placenta previa	641.00–641.94
Fetal problems affecting the mother	656.0–656.23, 656.33, 656.50–656.93
Gestational diabetes	648.80–648.83
Grand multiparity	659.40–659.43
Inadequate prenatal care	V237
Preterm	644.2–644.21
Postdate	654.0–645.23
Prelabor rupture of the membranes	658.10–658.13
Small for dates	656.5–656.53
Large for dates/suspected macrosomia	653.5–653.53, 656.6–656.63
Previous cesarean	654.20–654.23
Uterine and cervical abnormalities	654.00–654.14, 654.30–654.64

Table 3

Non-obstetric comorbidities

Condition	Notes
Obesity	Combines Elixhauser measure with obesity affecting management of labor and delivery (649.1–649.13)
Pre-gestational diabetes	Combines Elixhauser measures for uncomplicated and complicated diabetes
Substance abuse	Combines Elixhauser measures for alcoholism and drug abuse
Iron deficiency anemia	
Rheumatoid arthritis	
Chronic blood loss anemia	
Chronic pulmonary disease	
Depression	
Hypertension with complications	
Hypothyroidism	
Fluid and electrolyte disturbances	
Neurological disorders	
Psychoses	
Valvular disease	
Other comorbidities	Combines Elixhauser measures for lymphoma, metastatic cancer, paralysis, peripheral vascular disorders, pulmonary circulation disorders, renal failure, solid tumor, liver disease, and weight loss

Table 4

Odds ratios for the association of high (3rd quartile) versus low (1st quartile) volume hospitals and clinicians * and postpartum infection

	Pre-Discharge		Post-Discharge	
	OR	**95% CI**	**OR**	**95% CI**
Unadjusted				
Hospital volume	2.22	1.71–2.90	0.94	0.74–1.21
Clinician volume	0.87	0.77–0.98	1.09	0.99–1.20
Hospital volume squared	0.83	0.77–0.90	0.97	0.90–1.05
Clinician volume squared	1.02	1.01–1.04	1.01	0.993–1.02
Product term **	1.00	0.95–1.05	0.93	0.90–0.97
Multivariate adjusted †				
Hospital volume	1.36	0.79–2.34	0.85	0.69–1.06
Clinician volume	0.84	0.73–0.98	1.04	0.96–1.13
Hospital volume squared	0.94	0.81–1.08	1.01	0.96–1.06
Clinician volume squared	1.02	0.998–1.05	1.01	0.997–1.02
Product term **	1.02	0.97–1.08	0.95	0.92–0.98
Cesarean Only (adjusted †)				
Hospital volume	1.30	0.71–2.37	0.91	0.80–1.03
Clinician volume	0.84	0.71–1.00	1.06	0.97–1.16
Hospital volume squared	0.96	0.82–1.12	1.01	0.999–1.01
Clinician volume squared	1.02	1.001–1.05	1.00	0.996–1.01
Product term *	1.02	0.96–1.08	0.99	0.98–0.99
Vaginal only (adjusted †)				
Hospital volume	1.81	0.98–3.31	1.01	0.90–1.13
Clinician volume	0.80	0.68–0.95	1.01	0.93–1.10
Hospital volume squared	0.84	0.72–0.98	1.00	0.99–1.00
Clinician volume squared	1.02	1.0001–1.05	1.01	0.999–1.01
Product term **	1.04	0.99–1.11	0.99	0.98–0.996

* Odds ratios are for the effect of an increase in annual delivery volume equal to the interquartile range of 1,810 hospital deliveries and 93 clinician deliveries. Models are recentered to the first quartile of volume (503 hospital deliveries and 43 clinician deliveries.

** Product term: hospital volume × clinician volume

† Adjusted for hospital teaching status and patient age, race, expected payer, and obstetric and non-obstetric comorbidities