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The July effect on maternal peripartum complications before and after resident duty hour reform: a population-based retrospective cohort study

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Abstract

OBJECTIVE—To compare maternal birth complications early versus late in the academic year and evaluate resident work hour limitation's impact on the “July effect.”

STUDY DESIGN—Retrospective, population-based cohort study of 628,414 singleton births in Washington State 1987 to 2012 measuring the adjusted risk of maternal peripartum complications early (July/August) versus late (April/May) in the academic year. To control for seasonal outcome variation unrelated to trainees' involvement in care as well as long-term trends in maternal complications unrelated to variation in trainees' effect on outcomes across the academic year, we employed difference-in-differences methods contrasting outcomes at teaching to non-teaching hospitals for deliveries before and after restriction of resident work hours in July 2003.

RESULTS—Prior to resident work hour limitation in July 2003, women delivering early in the academic year at teaching hospitals suffered more complications (relative risk [RR] 1.05, 95% CI 1.00–1.09, $p=0.03$). After July 2003, complication risk did not vary significantly across the academic year except at teaching-intensive hospitals, where July/August deliveries experienced fewer complications (RR 0.95, 95% CI 0.92–0.98, $p=0.001$).

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Conflicts of interest: None

CONCLUSION—Women delivering at teaching hospitals early in the academic year suffered a modest but significant increase in complications before but not after resident work hour reform.

Keywords

July effect; Peripartum complications; Maternal morbidity; Resident duty hours; Postgraduate medical education; Difference-in-differences analysis

INTRODUCTION

Each July in the United States, approximately 25,000 new doctors begin training in their chosen specialty. Others assume increased responsibility as upper-level residents, sub-specialty fellows, or new attending physicians. Both physicians and the public worry that this transition results in worse outcomes for U.S. patients hospitalized in July or, in the United Kingdom, after the parallel transition in early August.^{1–3} While some studies have suggested the so-called “July effect” is a medical myth, larger studies tend to find a small but significantly increased risk of adverse outcomes for patients hospitalized early in the academic year.^{1,4–6}

Even a small July effect on maternal peripartum complications could result in significant morbidity burden at the population level. Neither of the two groups that examined this issue to date found a July effect during the peripartum period.^{7,8} Small patient samples, however, limited both studies’ power to detect effect sizes of the order identified by larger, better-controlled studies in other settings.¹ Additionally, data for both studies predated restrictions imposed on U.S. resident work hours in July 2003, which aimed to reduce errors related to trainee fatigue but also triggered closer supervision of trainees by attending physicians and other changes in postgraduate medical education.⁹ Such changes might be expected to mitigate the July effect.

The objective of this study was therefore to investigate the July effect on maternal birth complications before and after work hour reform in a large, population-based cohort while controlling for both seasonal outcome trends unrelated to trainee involvement and long-term trends in obstetrics and obstetrical medical education not subject to variation across the academic year.

METHODS

Study cohort

We conducted a population-based retrospective cohort study of births occurring at non-federal hospitals in Washington State in the United States from 1987 through 2012 using a database of birth certificates linked to anonymized data from the Washington State Comprehensive Hospital Abstract Reporting System. The creation and validation of this database — which successfully matches 95% of statewide births to hospital discharge data — has been previously reported.^{10,11} All successfully-linked live births occurring early (July–August) or late (April–May) in the academic year were eligible for cohort inclusion. We excluded deliveries that occurred outside the hospital. Multiple births were excluded to avoid repeated counting of maternal outcomes linked to >1 birth. To allow accurate

determination of outcomes, we also excluded deliveries missing the mode of delivery. We assumed a hospital with <10 births in a calendar year was not providing dedicated obstetrical services and excluded births at the facility for that calendar year. The Washington State Department of Health Institutional Review Board determined this analysis of anonymized data was not human subjects research and thus did not require formal review.

Outcome

The primary outcome was the occurrence of one or more maternal birth complications, identified using *International Classification of Disease, Ninth Revision, Clinical Modification* (ICD-9-CM) discharge diagnosis and procedure codes (Supplementary Table S1).¹² This outcome — a composite of peripartum bleeding, obstetric trauma (for vaginal deliveries), operative complications (for Cesarean deliveries), infection, and other major organ system complications — was adapted from previously published definitions of significant peripartum complications offered by the World Health Organization and others.^{13–16} Prespecified secondary outcomes included two specific complications drawn from the composite outcome — surgical complications of Cesarean delivery and 3rd or 4th degree perineal laceration after vaginal birth — hypothesized a priori to be particularly vulnerable to the effects of resident inexperience and supervision. Secondary outcomes also included the major categories of complication types nested within the overall composite outcome.

Exposures

The primary exposure of interest was delivery early in the academic year, defined as births in July or August. Deliveries in April or May formed the late academic year comparison group. We excluded June deliveries since graduation of responsibility in residency programs can occur in the latter half of this month. Teaching hospitals were defined as those sponsoring or affiliated with a residency training program in family medicine or obstetrics and gynecology. Each hospital's teaching status was determined from birth-year specific residency affiliation data obtained from the Graduate Medical Education Directory, published each year by the American Medical Association, and review of publicly available data.¹⁷ Using this data, hospitals' teaching status was reassigned annually, with the result that hospitals that gained or lost residency programs contributed data during residency-affiliated years as teaching hospitals and non-affiliated years as non-teaching hospitals. As in previous reports, we classified teaching intensity at these hospitals as low, moderate or high using the resident-to-bed ratio reported to Medicare (see Supplemental Appendix S1).¹⁸ Births were categorized as before or after implementation of the 80-hour resident workweek in July 2003. Birth weight was divided into low (<2500g), normal (2500–3999g), or high/macrosomia (≥4000g) categories.¹⁹ Gestational age — determined from the provider's estimated age or, when this value was implausible (<17 weeks, N=9) or missing (N=42,247), from the mother's last menstrual period — was categorized as preterm (<37 weeks), term (37–41 weeks), or post-term (≥42 weeks).²⁰ Maternal age at birth was categorized as 19, 20–34, or ≥35 years.²¹ Parity was categorized as 0, 1–4 or ≥5 previous births.²² Maternal race and ethnicity were recorded from birth certificate data given reported associations with birth outcomes and seasonal birth patterns.²³ To allow risk adjustment for maternal comorbidities, we identified women with diabetes, gestational hypertension, and chronic hypertension based on (1) a

diagnosis on the birth certificate and/or (2) a compatible ICD-9-CM discharge diagnosis (Supplementary Table S2).²⁴ Since birth certificate data on chronic maternal heart or lung disease were not available throughout the study period, these conditions were identified from discharge diagnoses alone.

Statistical analysis

The primary statistical analysis employed multivariable Poisson regression with clustered robust variance estimators²⁵ to implement difference-in-differences models comparing the adjusted risk of ≥ 1 peripartum complication between women delivering early versus late in the academic year at teaching and non-teaching hospitals before and after implementation of the July 2003 resident work hour restrictions (see online supplement for additional detail). Difference-in-differences models, used in a number of studies of the July effect, employ concurrent controls (women delivering at non-teaching hospitals) to account for seasonal outcome variation not related to trainee involvement in patient care.^{1,4,18,26,27} Critically, this analysis also controls for secular trends across the study period in peripartum complication rates, obstetrical management practices, and obstetrical training practices not subject to variation across the academic year. Covariates associated with birth month and peripartum complications — including maternal age, parity, race, the presence of ≥ 1 maternal comorbidity, and infant birth weight — were included in the multivariable model to adjust for differences in baseline risk. Subjects with missing data for any covariate were excluded from the multivariable analysis.

To evaluate whether more resident involvement influenced the strength of the July effect, we conducted a subgroup analysis focusing on deliveries at moderate and high-intensity teaching hospitals (Medicare-reported resident-to-bed ratio >0.10 during birth year). We also performed a sensitivity analysis to investigate potential bias due to complete case analysis by repeating the primary Poisson regression using multiply imputed data (Supplementary Appendix S1). Finally, since family medicine residents might be involved in fewer Cesarean deliveries, we performed a sensitivity analysis excluding hospitals with obstetric residents to determine if our findings for surgical complications of Cesarean delivery changed when restricted to hospitals with only family medicine residents.

Bivariable comparisons employed chi-square tests. Relative risks (RR) are reported with 95% confidence intervals. Confidence intervals for the latter are derived from robust standard errors clustered on hospital to account for non-independence of outcomes within centers. A two-sided p value <0.05 was considered significant. Assuming a baseline 10% risk of ≥ 1 peripartum complication and based on the effect sizes reported in a recent systematic review,¹ we calculated that 115,000 eligible teaching hospital births in each birth era evenly divided between the studied months would yield 80% power to detect a relative risk increase of 5% for July/August births for each studied era. All analyses were performed in Stata version 14.0 (StataCorp LP, College Station, TX).

RESULTS

Of 648,381 singleton births between 1987 and 2012 in July/August or April/May in Washington State, 628,414 deliveries met criteria for inclusion in our study (Supplementary

Figure S1). Deliveries occurred at 79 different hospitals, including 17 affiliated with a family medicine or obstetrics residency program for all or part of the study period. These teaching hospitals accounted for 233,870 deliveries (37.3%). Maternal and infant characteristics were generally similar between births in April or May and those in July or August (Table 1). Births at teaching hospitals were more often premature and involved more women who were advanced maternal age, non-white, or nulliparous. Deliveries excluded from the multivariable analysis due to missing covariate data (N= 24,765, 3.9%) were distributed equally across the academic year but occurred more often at teaching hospitals (Supplementary Table S3). Slightly more women with missing data were non-white, older than age 35, or nulliparous.

Women were more likely to suffer at least one peripartum complication at teaching hospitals than non-teaching hospitals (19.9% vs 16.6%, $p < 0.001$). When stratified by birth before or after June 2003, we observed an increased risk of maternal complications at teaching hospitals for births early in the academic year prior to work hour alterations (RR 1.03, 95% CI 1.01–1.05) and a decreased risk (RR 0.94, 95% CI 0.92–0.97) afterward (Table 2). Compared to non-teaching hospitals, births at teaching hospitals early in the academic year were associated with an absolute 0.9% increase in maternal complications prior to work hour modifications ($p=0.002$) and a 0.8% decrease in complications thereafter ($p=0.014$). While the risk of severe perineal laceration with vaginal delivery did not vary across the academic year, surgical complications of Cesarean delivery were more common early in the academic year at teaching hospitals prior to July 2003 (Tables S4 and S5).

Figure 1 depicts the adjusted risk early in the academic year for the primary composite outcome, five major classes of peripartum complications included in the composite outcome, and the pre-specified secondary outcomes also included in the composite outcome. For the composite outcome (Table 3), adjusted risk of one or more peripartum complication was higher early in the academic year at teaching hospitals before July 2003 (RR 1.05, 95% CI 1.00–1.09, $p=0.031$). The decreased risk of complications early in the academic year at teaching hospitals post-July 2003 was not statistically significant in the primary analysis (RR 0.96, 95% CI 0.92–1.00, $p=0.079$) but was significant (RR 0.95, 95% CI 0.92–0.98, $p=0.001$) when only birth events at higher teaching intensity hospitals were considered (Table 4). In an exploratory post hoc analysis, no single diagnosis or diagnostic category included in the composite outcome seemed to drive the observed association (Supplementary Table S6).

After risk adjustment, surgical complications of Cesarean deliveries remained more common at teaching hospitals before (RR 1.41, 95% CI 1.15–1.74, $p=0.001$) but not after (RR 1.00, 95% CI 0.72–1.38, $p=0.98$) work hour revisions took effect in 2003 (Supplementary Table S7). As in the unadjusted analysis, we observed no July effect for severe perineal lacerations (Supplementary Table S8).

In the sensitivity analysis using multiply imputed data to investigate whether omitting cases with missing covariate data introduced bias into the primary analysis (Supplementary Table S9), we found that July or August teaching hospital deliveries had significantly more complications before July 2003 (RR 1.05, 95% CI 1.01–1.09, $p=0.027$) but fewer

complications after July 2003 (RR 0.95, 95% CI 0.92–0.99, $p=0.013$). Similar to the primary analysis, in the sensitivity analysis restricting teaching hospitals to those hosting family medicine but not obstetrics residents, we observed a July effect for surgical complications of Cesarean deliveries before (RR 1.40, 95% CI 1.14–2.00, $p=0.004$) but not after (RR 0.97, 95% CI 0.63–1.48, $p=0.88$) July 2003 (Supplementary Table S10).

DISCUSSION

In a large, population-based cohort of singleton births between 1987 and 2012, we found evidence of a small “July effect” on maternal peripartum outcomes before implementation of the 80-hour resident workweek in 2003. Seasonal outcome variations were not significantly different between teaching and non-teaching hospitals after July 2003. In higher intensity teaching hospitals, however, we observed a modestly decreased risk for births early in the academic year after work hour reform.

While measured effect sizes were small, they were in line with July effects identified by other large studies.¹ Based on an equal monthly distribution for 4,000,000 live births and conservatively estimating that 35% of U.S. births occur at teaching hospitals, our unadjusted findings suggest a July effect responsible for 2,001 additional complicated deliveries annually prior to July 2003. If significant, the trend toward a “reverse July effect” seen subsequent to the 2003 changes in postgraduate medical education would result in 1,911 avoided complications among teaching hospitals births in July and August.

Concern over trainees’ role in medical errors has prompted significant changes in graduate medical education in the United States in the last 15 years.^{9,30} Obstetrical residents reported decreased fatigue and improved quality of life after implementation of the 80-hour workweek in 2003, but the impact on patient-centered outcomes is less clear.^{31–33} Our results could indicate that, after duty hour reforms, the beneficial effects of reduced trainee fatigue or — perhaps more likely — increased supervision by and direct involvement of upper-level residents and staff physicians more than compensated for trainees’ inexperience early in the academic year. It is plausible that, as the academic year progresses and residents receive more autonomy and responsibility, the reduced clinical experience resulting from duty hour restrictions exerts a greater effect on patient outcomes,^{34–36} explaining the “reverse July effect” we detected among deliveries at higher intensity teaching hospitals after July 2003.

Compared to prior studies that did not detect an obstetrical July effect,^{7,8} we included many more women delivering early in the academic year and evaluated their outcomes against deliveries late in the academic year rather than to the entire remaining year. These factors increased the power of our study to detect a July effect of similar magnitude to that reported by other large, well-designed studies.¹ Additionally, neither of the previous studies adjusted for potential confounders of an obstetrical July effect such as maternal comorbidities, age, and parity. Most importantly, our difference-in-difference analyses referenced maternal outcomes at teaching hospitals to those at non-teaching hospitals, thereby controlling for seasonal outcome variations not attributable to trainees’ involvement in patient care or modifications of the graduate medical education system. Our analysis also accounts for

long-term shifts in obstetrical management and training during the study period unless, by differentially affecting maternal complications in April/May versus July/August, these evolving practices were of interest for the question investigated here.

As with any observational study, our results do not prove causality. Seasonal variations in birth rates and such outcomes as preterm birth have been hypothesized to result from differential conception patterns among women of differing age, socioeconomic status, health and fertility.^{23,29} If higher risk births occur early versus late in the academic year at teaching hospitals, this could bias our results. While we adjusted for plausible risk factors for adverse outcomes, we cannot exclude the possibility of residual confounding.

Most births at teaching hospitals in this study occurred at low intensity teaching hospitals as defined by a ratio of resident full-time equivalent positions to hospital beds <0.10 . More deliveries by non-resident providers instead of trainees in these facilities potentially diluted the July effect we measured. Indeed, compared to the primary analysis, the protective effect of delivering early in the academic year post-July 2003 was stronger and statistically significant at higher intensity teaching hospitals. On the other hand, the similar magnitude of the pre-work hour reform July effect measured by the primary and subgroup analysis is reassuring.

Our study has several additional limitations. We defined outcomes and maternal comorbidities from hospital discharge diagnoses. However, maternal comorbidities and pregnancy complications are underreported in both hospital discharge and birth certificate data when compared to medical records.^{11,24} Where able, we defined comorbidities by combining these data sources, which has been shown to improve sensitivity.²⁴ Nevertheless, we likely underestimated the prevalence of maternal comorbid conditions. Outcome definitions based on coded discharge diagnoses are more sensitive and specific but remain imperfect.^{11,14} Restriction of the analysis to live births may also have contributed to underestimation of maternal complications and comorbidities. Fortunately, we do not have reason to suspect differential misclassification between exposure categories. Aside from the prespecified analyses of severe perineal laceration and technical surgical complications, the analysis of individual complications nested within the composite outcome were conducted in an exploratory *post hoc* fashion and are subject to inadequate power and an increased risk of type I error due to multiple comparisons. These analyses should be considered hypothesis generating. Finally, given between-country differences in graduate medical education, our results may not be generalizable outside the United States.

CONCLUSIONS

A modest but significant July effect on women's peripartum complication risk resolved — and possibly reversed — after restriction of resident work hours in 2003. Future studies should investigate the mechanism of these changes and whether this phenomenon extends to other patient populations. Identifying aspects of medical education policy and practice influencing seasonal outcome variation could guide future graduate medical education reform and improve patient safety at teaching hospitals.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Figure 1.

Forrest plot depicting the adjusted risk of peripartum complications early versus late in the academic year before (black diamonds) and after (white diamonds) July 2003. Risk of the composite outcome, the five major subtypes of peripartum complications nested in the composite outcome, and the prespecified secondary outcomes severe perineal laceration (nested in obstetric trauma, vaginal births [VB] only) and technical surgical complications (nested in operative complications, Cesarean births [CS] only) are depicted.

Table 1

Characteristics of singleton births occurring early and late in the academic year in Washington State, 1987–2012

Subject characteristic	Births at teaching hospitals			Births at non-teaching hospitals		
	Overall (N=233,870)	July/August births (N=119,338)	April/May births (N=114,532)	Overall (N=394,544)	July/August births (N=199,426)	April/May births (N=195,118)
Maternal age (years) ^a						
19	19,784 (8.5)	10,175 (8.5)	9,609 (8.4)	38,785 (9.8)	19,570 (9.8)	19,215 (9.8)
20–34	177,764 (76.0)	90,638 (76.0)	87,126 (76.1)	307,100 (77.8)	154,910 (77.7)	152,910 (78.0)
35	36,319 (15.5)	18,523 (15.5)	17,796 (15.5)	48,646 (12.3)	24,940 (12.5)	23,706 (12.2)
Maternal race/ethnicity ^a						
Black	15,427 (6.8)	7,841 (6.8)	7,586 (6.8)	8,820 (2.3)	4,460 (2.3)	4,360 (2.3)
Hispanic	24,853 (10.9)	12,969 (11.2)	11,884 (10.7)	58,859 (15.3)	30,469 (15.6)	28,390 (14.9)
White	159,346 (70.1)	80,959 (69.7)	78,387 (70.4)	282,815 (73.3)	141,924 (72.8)	140,891 (73.8)
Other	27,837 (12.2)	14,407 (12.4)	13,430 (12.1)	35,564 (9.2)	18,194 (9.3)	17,370 (9.1)
Parity						
Nulliparous	98,442 (43.5)	50,454 (43.7)	43,988 (43.3)	157,844 (40.4)	80,292 (40.6)	77,552 (40.1)
1–4 prior births	123,802 (54.7)	62,902 (54.5)	60,900 (54.9)	225,790 (57.8)	113,652 (57.5)	112,138 (58.0)
5 prior births	4,129 (1.8)	2,104 (1.8)	2,025 (1.8)	7,140 (1.8)	3,604 (1.8)	3,536 (1.8)
Maternal comorbidity ^b	33,266 (14.2)	16,755 (14.0)	16,511 (14.4)	50,588 (12.9)	25,311 (12.7)	25,377 (13.0)
Diabetes	13,913 (6.0)	7,195 (6.0)	6,718 (5.9)	20,855 (5.3)	10,801 (5.4)	10,054 (5.2)
Chronic hypertension	6,003 (2.6)	3,046 (2.6)	2,957 (2.6)	7,538 (1.9)	3,762 (1.9)	3,776 (1.9)
Gestational hypertension	14,286 (6.1)	6,950 (5.8)	7,336 (6.4)	23,136 (5.9)	11,112 (5.6)	12,024 (6.2)
Chronic heart disease	1,114 (0.5)	559 (0.5)	555 (0.5)	1,414 (0.4)	721 (0.4)	693 (0.4)
Chronic lung disease	3,136 (1.3)	1,615 (1.4)	1,521 (1.3)	4,255 (1.1)	2,128 (1.1)	2,127 (1.1)
Cesarean delivery	50,857 (21.8)	25,962 (21.8)	24,895 (21.7)	89,645 (22.7)	45,486 (22.8)	44,159 (22.6)
Gestational age (weeks) ^a						
Preterm (<37)	19,811 (8.5)	9,969 (8.4)	9,842 (8.7)	20,88 (5.2)	10,013 (5.1)	10,075 (5.2)
Term (37–41)	205,914 (88.8)	105,137 (88.9)	100,777 (88.7)	354,753 (91.1)	179,511 (91.3)	175,242 (91.0)
Postterm (42)	6,165 (2.7)	3,121 (2.6)	3,044 (2.7)	14,403 (3.7)	7,191 (3.7)	7,212 (3.8)
Birth weight (grams) ^a						
<2500	14,761 (6.3)	7,475 (6.3)	7,286 (6.4)	12,906 (3.3)	6,514 (3.3)	6,392 (3.3)

Subject characteristic	Births at teaching hospitals			Births at non-teaching hospitals		
	Overall (N=233,870)	July/August births (N=119,338)	April/May births (N=114,532)	Overall (N=394,544)	July/August births (N=199,426)	April/May births (N=195,118)
2500–3999	190,349 (81.6)	97,588 (82.0)	97,761 (81.3)	328,884 (83.6)	166,849 (83.8)	162,035 (83.3)
4000	28,061 (12.0)	13,940 (11.7)	14,121 (12.4)	51,837 (13.2)	25,667 (12.9)	26,170 (13.5)
Birth post-July 2003	96,325 (41.2)	51,656 (43.3)	44,669 (39.0)	153,707 (39.0)	81,911 (41.1)	71,796 (36.8)

Data are presented as N (%)

^aMissing data (N, %): Maternal age (16, <0.1%); maternal race (14,893, 2.4%); parity (11,267, 1.8%); gestational age (7,278, 1.6%); birth weight (1,616, 0.3%).

^bSome women were affected by more than one comorbidity.

Table 2
Maternal peripartum complications stratified by hospital teaching status and birth era

Birth era	Hospital teaching status	July/August births	April/May births	RR (95% CI)	P value	P value for comparison by teaching status
Pre-July 2003	Teaching	14,883 (22.0)	14,963 (21.4)	1.03 (1.01–1.05) ^a	0.010	0.002
	Non-teaching	20,452 (17.4)	21,815 (17.7)	0.98 (0.97–1.00) ^b	0.065	
Post-July 2003	Teaching	8,640 (16.7)	7,936 (17.8)	0.94 (0.92–0.97) ^a	<0.001	0.014
	Non-teaching	12,442 (14.9)	10,889 (15.2)	0.99 (0.96–1.01) ^b	0.23	

Data are presented as N (%)

^a p<0.001 for comparison of teaching hospitals before and after June 2003

^b p=0.92 for comparison of non-teaching hospitals before and after June 2003

Table 3

Adjusted risk of maternal peripartum complications early in the academic year

Birth era	Hospital teaching status	RR (95% CI) ^{a,b}	P value	Difference-in-difference RR ^{a,c} (95% CI)	P value
Pre-July 2003	Teaching	1.03 (0.99–1.07) ^d	0.16	1.05 (1.00–1.09)	0.031
	Non-teaching	0.98 (0.96–1.00) ^e	0.098		
Post-July 2003	Teaching	0.94 (0.92–0.97) ^d	<0.001	0.96 (0.92–1.00)	0.079
	Non-teaching	0.98 (0.95–1.01) ^e	0.18		

^aAdjusted for maternal age, parity, and race, presence of 1 maternal comorbidity, and infant birth weight^bRelative risk (RR) for peripartum maternal complications early versus late in the academic year^cComplication risk associated with early academic year birth at a teaching hospital using women delivering at non-teaching hospital risk as the comparison group.^dp<0.001 for comparison of teaching hospitals before and after June 2003^ep=0.96 for comparison of non-teaching hospitals before and after June 2003

Table 4

Adjusted risk of maternal complications at moderate- and high-intensity teaching hospitals early in the academic year

Birth era	Hospital teaching status	RR (95% CI) ^{a,b}	P value	Difference-in-difference RR ^{a,c} (95% CI)	P value
Pre-July 2003	Teaching	1.03 (1.00–1.07) ^d	0.073	1.05 (1.01–1.09)	0.015
	Non-teaching	0.98 (0.96–1.00) ^e	0.092		
Post-July 2003	Teaching	0.93 (0.92–0.94) ^d	<0.001	0.95 (0.93–0.98)	0.001
	Non-teaching	0.98 (0.95–1.01) ^e	0.17		

^aAdjusted for maternal age, parity, and race, presence of 1 maternal comorbidity, and infant birth weight

^bRelative risk (RR) for peripartum maternal complications early versus late in the academic year

^cComplication risk associated with early academic year birth at a teaching hospital using women delivering at non-teaching hospital risk as the comparison group.

^dp<0.001 for comparison of teaching hospitals before and after June 2003

^ep=0.96 for comparison of non-teaching hospitals before and after June 2003