The Labor Market and Health Impacts of Reducing Cesarean Section Deliveries*

Sarah Miller† Petra Persson‡ Maya Rossin-Slater§ Laura R. Wherry¶

December 8, 2025

Abstract

One in three births in the United States is delivered by cesarean section (c-section). This paper studies the labor market and health effects of c-sections, using newly linked administrative data that combines the universe of California birth records with mothers' quarterly earnings. We analyze the impact of an intervention that reduced c-section rates among low-risk first-time births, and find that mothers exposed to the intervention appear to have a higher likelihood of employment in the quarter following birth, as well as a higher likelihood of returning to their pre-birth employer. These impacts attenuate over time—suggesting that a c-section primarily delays return to the labor market following childbirth—but attachment to the pre-birth employer remains higher five quarters post-birth. We find no evidence of significant impacts on maternal or infant health, indicating that the intervention-induced decline in c-sections did not come at the cost of worse outcomes. Further, among mothers who have another child, we find that exposure to the intervention at the first birth leads to a lower likelihood of c-section and preterm delivery at the second one, implying that both the economic and health benefits of reduced c-sections may compound with birth order.

JEL classification: I14, I15, J13

Keywords: c-section, maternal health, child penalty

*This research was conducted as a part of the U.S. Census Bureau's Evidence Building Project Series. Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau, the California Department of Public Health, or other data providers. The Census Bureau has ensured appropriate access and use of confidential data and has reviewed these results for disclosure avoidance protection (Project P-7523134: CBDRB-FY25-0281 and CBDRB-FY25-0435 and Project P-7503097: CBDRB-FY26-002). We are very grateful to Dr. Elliott Main, MD, the inaugural medical director of the California Maternal Quality Care Collaborative (CMQCC), for sharing data with us on hospitals' participation in the CMQCC cesarean section intervention. We also thank Dr. Main and Dr. Amanda Williams, MD, for helpful discussions regarding clinical practice in obstetrics and gynecology and labor and delivery. We thank Joshua Bricker and Taegan Mullane for excellent research assistance. Research reported in this article was supported by the Eunice Kennedy Shriver National Institute of Child Health and Human Development of the National Institutes of Health under award number R01HD115606. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health. We would also like to thank Ellen Badley, Sandra Bannerman, Colin Chew, Heather Fukushima, Steven Hoang, Amanda Jackson, Michelle Miles, Eric Neuhauser, Jenn Rico, and other staff at the California Department of Public Health (CDPH) for their help in accessing restricted California birth records, and Victoria McCoy-Cosentino at NYU for help with data use agreements. We would also like to thank Gloria Aldana, Ashley Austin, Casey Blalock, Scott Boggess, Clint Carter, Melissa Chiu, Diane Cronkite, Denise Flanagan-Doyle, Adam Galemore, Katie Genadek, Katlyn King, Shawn Klimek, Shirley Liu, Kathryn Mcnamara, Bonnie Moore, John Sullivan, Emily Wisniewski and other staff at Census, Robert Goerge and Leah Gjertson at Chapin Hall, and the Laura and John Arnold Foundation's support under their initiative to use linked data to advance evidence-based policymaking for help with the linkages to Census-held data. All errors are our own.

†University of Michigan Ross School of Business and NBER. Email: mille@umich.edu

‡Stanford University, Research Institute of Industrial Economics, and NBER. Email: perssonp@stanford.edu

§Stanford University School of Medicine, NBER, IZA. Email: mrossin@stanford.edu

¶New York University Robert F. Wagner Graduate School of Public Service and NBER. Email: laura.wherry@nyu.edu

1 Introduction

Nearly one in three births in the United States is delivered by cesarean section (c-section) (Hamilton et al., 2024). The U.S. c-section rate exceeds those in many other high-income countries, and is above what is recommended by international health organizations (Angolile et al., 2023). While a potentially life-saving intervention that is necessary in certain situations, a c-section is a major abdominal surgery that carries multiple risks, including infection, hemorrhage, and scar tissue complications, and these risks compound with each subsequent c-section delivery.[1] As c-section rates among first-time mothers have risen in the last few years (Stephenson, 2022) and vary substantially across U.S. counties and hospitals (Baicker et al., 2006; Clark et al., 2007; Kozhimannil et al., 2013; Rosenstein et al., 2021; Robinson et al., 2024; Kissel and Roy, 2025), there are growing concerns regarding overuse of the procedure, especially among low-risk first-time deliveries. Accordingly, Healthy People 2030, an initiative from the U.S. Office of Disease Prevention and Health Promotion, identifies the reduction of c-sections among low-risk first-time mothers as a critical objective.[2] Moreover, as the U.S. faces a maternal health crisis—with American mothers being two to three times more likely to die following childbirth than their counterparts in Canada and Europe (Kassebaum et al., 2016; Tikkanen et al., 2020; Gunja et al., 2024)—initiatives aiming to lower c-sections are described as important to achieving the broader goal of reducing maternal morbidity and mortality (Assistant Secretary for Planning and Evaluation, 2024).

While the possible health benefits of lowering c-section rates are widely discussed, there may be implications for other aspects of women's lives as well. Recovery from a c-section delivery is longer than from an uncomplicated vaginal birth—a fact that is reflected in the differences in statutory durations of state-level paid maternity and short-term disability leave policies (usually 8 weeks for c-sections and 6 weeks for vaginal deliveries).[3] Therefore, women with a c-section delivery may face additional hurdles when returning to work, which could in turn affect their future careers and economic livelihoods. While the "child penalty"—the idea that childbirth may lead to short- and

[1] The rate of repeat c-sections is around 85 percent in the United States (Hamilton et al., 2024). That is, the vast majority of women who have a c-section delivery and go on to have more children end up with multiple c-sections over their lifetime.

[2] See: https://odphp.health.gov/healthypeople/objectives-and-data/browse-objectives/pregnancy-and-childbirth/reduce-cesarean-births-among-low-risk-women-no-prior-births-mich-06.

[3] For example, California's State Disability Insurance program states: "Without medical complications, you can receive benefits up to four weeks before your expected delivery date and up to six weeks after your delivery. For c-section, you can receive benefits up to eight weeks after delivery." Source: https://edd.ca.gov/siteassets/files/pdf_pub_ctr/de2515.pdf.

long-term earnings losses among mothers—has received a significant amount of attention in the recent literature (Lundborg et al., 2017; Kleven et al., 2021; Andresen and Nix, 2022; Gallen et al., 2023; Bögl et al., 2024; Kleven et al., 2024), less is known about the role of the delivery mode in shaping women's labor market trajectories.

This paper analyzes the causal impacts of a large state-level intervention targeting hospital c-section rates. We use a novel linkage between the universe of California birth records from the Department of Public Health for years 2013–2019 and quarterly earnings data from from the Longitudinal Employer-Household Dynamics (LEHD) database curated by the U.S. Census Bureau. We study an intervention designed and implemented by the California Maternal Quality Care Collaborative (CMQCC)—a leading statewide perinatal quality collaborative focused on maternity care[4]—that offered a broad range of tools to hospitals to encourage them to reduce c-section delivery rates among low-risk births. Specifically, the program targeted births that were nulliparous (i.e., first-time), term (i.e., gestation length of at least 37 weeks), singleton, and vertex (i.e., head-first), or "NTSV" births. All hospitals with a 2015 NTSV c-section delivery rate greater than 23.9 percent were invited to participate, and the intervention was conducted in three phases with different hospital cohorts.[5] We use variation in the timing of the intervention across hospitals in event-study and difference-in-differences (DD) models, and study its effects on c-section rates, maternal and infant health, maternal labor market outcomes, subsequent fertility, and outcomes among subsequent births. To address concerns regarding bias in DD models due to staggered treatment timing and heterogeneous effects (De Chaisemartin and d'Haultfœuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021; Sun and Abraham, 2021; Athey and Imbens, 2022; Roth et al., 2023), we use the Callaway and Sant'Anna (2021) estimator.

Our results show that the CMQCC intervention reduced c-section rates among NTSV deliveries by about 3.0 percentage points (9.7 percent compared to the pre-treatment mean). This estimate is in line with the findings of CMQCC's research team (Rosenstein et al., 2021).[6] The intervention has received widespread press coverage touting its success, and has since been considered a national model to

[4]According to the Centers for Disease Control and Prevention (CDC), there are 36 active state-based perinatal quality collaboratives (see https://www.cdc.gov/maternal-infant-health/pqc/state-pqcs.html). California was the first to establish one in 1997. CMQCC was later established in 2006 as a collaborative intended to address maternal health specifically. More details are here: https://www.cmqcc.org/about/what-we-do.

[5]This threshold aligned with a national goal set by the U.S. Department of Health and Human Services under Healthy People 2020 for the c-section rate to be less than 23.9 percent among low-risk women with no prior c-section deliveries (U.S. Department of Health and Human Services, 2017).

[6]Specifically, Rosenstein et al. (2021) used logistic regression models to compare c-section rates for NTSV births at participating and non-participating hospitals, as well as relative to rates at participating hospitals before the intervention, with a "modified stepped-wedge analysis." The study reports an adjusted odds ratio of 0.87 [95%CI, 0.85-0.89], which is a 13 percent decline.

inform other efforts across the country (Agency for Healthcare Research and Quality, 2022).[7] We do not find any significant changes in maternal morbidity, or rates of low birth weight (less than 2,500 grams) or early-term (between 37 and 39 weeks gestation) births.[8] This means that the intervention-induced reduction in c-section deliveries did not come at the cost of any worse health outcomes for either mothers or infants.

We next study mothers' labor market outcomes post-childbirth using a sample of NTSV births to mothers who had positive earnings in the quarter before childbirth. Among these mothers, the intervention reduced c-section rates by 3.3 percentage points (10.2 percent at the pre-treatment mean). We find that treated mothers are 1.7 percentage points (2.4 percent) more likely to return to any employment in the quarter following birth, and 2.2 percentage points (3.2 percent) more likely to return to their pre-birth employer in the same quarter, although we interpret these estimates with caution given that the pre-intervention event study coefficients tend to be noisily estimated and not always close to zero. The estimated coefficients for the mother's total earnings and relative earnings rank at her employer in the quarter after birth are also positive, but not statistically significant.[9]

When we consider labor market outcomes over longer follow-up windows after childbirth, the effects become smaller and mostly insignificant. Our results indicate that the CMQCC intervention did not significantly change the probability that a mother is employed, her average earnings, or her employer-specific earnings decile over the period of three to five quarters following childbirth. While we still detect an increase in the probability of being employed at a pre-birth employer three to five quarters after childbirth, it is smaller (1.5 percentage points, or 2.3 percent) and only marginally significant at the 10% level. Overall, these results suggest that the primary labor market benefit of avoiding a c-section consists of accelerating a woman's return to work, especially at her pre-birth employer.

Lastly, we study mothers' subsequent fertility and birth outcomes. While we find no significant effect on the likelihood of having a subsequent birth in the ten quarters (i.e., 2.5 years) following the initial birth that we can observe in our data, we do find a 2.2 percentage point (7.1 percent) reduction in the likelihood of a subsequent c-section delivery among mothers who go on to have another birth in this window. Therefore, avoiding a c-section at the first delivery can prevent women from experiencing the compounding risks of multiple c-sections. Our estimates suggest that every 100 c-sections

[7] Examples of press coverage include Stenson (2021) and Belluz (2017).

[8] There are important data quality concerns regarding measuring maternal morbidity in birth certificates data, and these measures are known to be under-reported (Gemmill et al., 2022). Therefore, the result on maternal morbidity should be interpreted with caution.

[9] Mothers with zero earnings are assigned to the lowest decile in this measure.

avoided at the first birth result in at least 20 fewer c-sections in the future.[10] Additionally, we find a 0.8 percentage point (13.4 percent) reduction in the likelihood of a preterm delivery (less than 37 weeks gestation) among subsequent births. This effect is consistent with a medical literature that documents an association between an initial c-section delivery and the risk of complications in future pregnancies, including placenta praevia and placental abruption, which could in turn lead to preterm birth (Yang et al., 2007; Williams et al., 2018). Thus, the health benefits of avoiding an initial c-section seem to amplify with additional births. While we do not have enough statistical power to study effects on maternal labor market outcomes following a subsequent birth, our results suggest that the estimates of impacts after the first birth could be lower bounds of the total effects that incorporate women's lifetime fertility.

Our paper adds to several strands of literature. First, we build on studies that have examined the health consequences of c-section deliveries. Correlational analyses tend to find increased risks of long-term health complications for the mother, mostly driven by subsequent pregnancies and repeat c-sections (Keag et al., 2018; Antoine and Young, 2021), and higher rates of respiratory infections, asthma, and obesity among children (Słabuszewska-Jóźwiak et al., 2020). Quasi-experimental studies, however, have arrived at somewhat mixed conclusions. Card et al. (2023) leverage variation in relative distance from a mother's ZIP code to the nearest higher versus lower c-section rate hospital in an instrumental variables design, and find that delivering in a high c-section rate hospital is associated with reductions in the likelihood of a low APGAR score and of infant readmission to the hospital during the neonatal period.[11] Fischer et al. (2024) study closures of hospital-based obstetric units, and find that mothers who are affected by them are more likely to have a c-section delivery. They find no evidence of adverse impacts on birth outcomes; if anything, there are reductions in some measures of maternal morbidity, including transfusions and third/fourth degree perineal lacerations. Corredor-Waldron et al. (2024) use variation in hospital capacity, which they find predicts the likelihood of an unscheduled c-section delivery, and show that a capacity-induced reduction in the likelihood of a c-section decreases the probability of infant admission to the neonatal intensive care unit (NICU) and the incidence of postpartum complications among low-risk white and Black mothers.[12] Kissel and Roy

[10] We estimate this as 100 × subsequent birth rate in the 10 quarters following initial birth (0.282) × implied reduction in subsequent c-sections among those with averted first-birth c-sections ($\frac{0.0218}{0.0303} = 0.719$). Note that this is likely a lower bound for the number of c-sections avoided in the future since we do not observe completed fertility for the mothers in our data.

[11] Consistent with the correlational evidence, Card et al. (2023) also find an increased likelihood in emergency department visits for respiratory conditions in the first year of life.

[12] Two other studies have used variation in the likelihood of c-section delivery from time of day or day of week of the birth in Spain and Finland, respectively, finding adverse impacts of c-sections on APGAR scores and later asthma diagnoses

(2025) use data on Medicaid beneficiaries who go into spontaneous labor and exploit quasi-random assignment to physicians with different c-section propensities, finding that delivery by a high c-section physician leads to higher rates of maternal physical health complications and mental healthcare use.

The differences in findings across these studies likely reflect different types of identifying variation and samples used. Moreover, as pointed out by Currie and MacLeod (2017), the consequences of a c-section critically depend on the risk profile of the woman, and physicians with better decision-making skills achieve improved health outcomes by reallocating procedures from lower to higher-risk cases.[13] In contrast to the work cited above, our paper examines an intervention that explicitly targeted the reduction of c-sections among NTSV births by providing resources and direct incentives to labor & delivery teams at hospitals, thereby effectively standardizing the decision-making aspect of the process. In a heterogeneity analysis, we find that the intervention-driven reduction in c-sections exists throughout the predicted c-section risk distribution among NTSV births, but is largest in both absolute and relative terms among those in the top tercile. This finding is consistent with the idea that the NTSV births that would have otherwise been most likely to end up as c-sections were the most impacted. Importantly, we find no indication of any adverse maternal or infant health outcomes for NTSV births in any of the three predicted c-section risk terciles, suggesting that the marginal c-sections avoided in our context were likely medically unnecessary.[14] At the same time, we do not find any evidence of spillover effects on (riskier) non-NTSV births, suggesting that the intervention was effective at targeting deliveries in the appropriate risk categories. Overall, our findings underscore the cost-effectiveness of this type of intervention, since the healthcare savings resulting from the lower c-section rate—the average c-section delivery costs $26,280, while the average vaginal birth costs $14,768 (Rae et al., 2022)—do not come at the expense of worse outcomes.

Second, by measuring impacts on maternal labor market outcomes, we build on the burgeoning "child penalty" literature. While some studies have considered the role of the physical experience of childbirth in driving earnings losses among mothers by comparing birth parents to adoptive parents (Kleven et al., 2021) or heterosexual to same-sex couples (Andresen and Nix, 2022), research on the

among children (Costa-Ramón et al., 2018, 2022). Additionally, two studies have examined the dissemination of new medical evidence from the Term Breech Trial, which concerned the use of c-sections among breech births, finding that c-sections tend to improve health outcomes in this high-risk group in Denmark, Norway, and Sweden (Jensen and Wüst, 2015; Rogvi et al., 2025).

[13] Related, Singh (2021) shows that physicians rely on heuristics when making decisions regarding delivery mode. Specifically, if a physician's prior patient has complications in one delivery mode, they are more likely to switch to the other delivery mode for the next patient, regardless of that patient's clinical indications.

[14] We also do not detect any statistically significant differences across the three terciles of the predicted c-section risk distribution in the effects on maternal labor market outcomes or on subsequent birth outcomes.

causal effects of c-sections on maternal labor market trajectories is more rare. Halla et al. (2020) use data from Austria and leverage the timing of deliveries on "leisure days" (such as weekends or holidays) as an instrument for a c-section, finding short-term negative effects on maternal labor supply, but positive long-term effects, as women who deliver via c-section have lower subsequent fertility and thus less time away from work in their setting. Mühlrad (2022) examines the increased use of c-sections among high-risk births that resulted from new medical information in Sweden and finds no effects on maternal fertility or earnings. We build on this work by examining intervention-driven changes in c-section rates among low-risk births to provide insight on whether policy can play a role in effectively changing both delivery and labor market outcomes for mothers. And, our work speaks directly to the U.S. context, where the high baseline c-section rate implies that a larger share of women may be having medically unnecessary procedures than in other countries with lower rates.

Third, we relate to a broader literature on the effects of hospitals and physicians on patient outcomes. Outside of childbirth, studies have examined the "place-based" effects of healthcare mostly in the context of mortality among elderly patients (McClellan, 1994; Cutler, 2007; Doyle et al., 2015; Finkelstein et al., 2016, 2021; Deryugina and Molitor, 2021), or in specific specialties, including cardiology (Mourot, 2024), opioid treatment (Eichmeyer and Zhang, 2022), and mental healthcare (Ding, Forthcoming). Our work shows that hospital-specific factors have the potential to shape not just patients' health outcomes, but also their downstream economic well-being.

2 Background

The California Maternal Quality Care Collaborative was founded in 2006 as a collaboration between Stanford University School of Medicine and multiple stakeholders in California. Its central mission is to end "preventable morbidity, mortality and racial disparities in California maternity care."[15] The CMQCC has led several interventions targeting various maternal health issues, including hypertension, hemorrhage, maternal venous thromboembolism, preeclampsia, and c-sections.

We focus on CMQCC's c-section intervention, which was multifaceted and involved initiatives aimed at individual hospitals, as well as the entire state. All California hospitals with a 2015 NTSV c-section delivery rate greater than 23.9 percent were invited to join the "Collaborative to Support Vaginal Birth and Reduce Primary C-sections" (hereafter, "collaborative") at no cost. However, to accommodate finite financial and coordination resources, hospitals participated in three separate co-

[15] See: https://www.cmqcc.org/about/what-we-do.

horts. Cohort 1 was launched in June 2016, Cohort 2 was launched in January 2017, and Cohort 3 was launched in January 2018. Hospitals that declined to participate in the first round were re-invited in subsequent rounds. Figure 1 provides a schematic of hospital participation in the collaborative: 24 hospitals were included in cohort 1; 42 hospitals were included in cohort 2; and 25 hospitals were included in cohort 3. Among never treated hospitals, 58 hospitals were eligible but declined to participate, while 89 hospitals were ineligible because their NTSV c-section delivery rates were no higher than 23.9 percent in 2015.[16]

The collaborative followed a mentorship model previously developed by the CMQCC (Main et al., 2018). Specifically, each cohort was divided into groups of six to eight hospitals, with multidisciplinary teams from each hospital (including physicians, nurses, and quality improvement professionals) led by a physician and nurse mentor pair. The mentorship groups met monthly over an 18-month period, in addition to regional all-day kickoff and closing meetings. Mentors conducted regular site visits, which included presentations and peer-to-peer education. The intervention relied on two key written documents: the *CMQCC Toolkit to Support Vaginal Birth and Reduce Primary Cesareans* (Smith et al., 2016), which is a 159-page document that includes a variety of evidence-based tools, algorithms, and guidelines, and a *Safety Bundle* document put together by the American College of Obstetricians and Gynecologists' Council on Patient Safety in Women's Health Care, which was established in 2011 and later dissolved in 2021.[17]

Additionally, participating hospitals were encouraged to use the CMQCC Maternal Data Center, which provides real-time data on c-section delivery rates (along with other metrics). This allowed hospitals to compare their c-section rates with those of other hospitals. Collaborative participants also conducted formal analyses of barriers to reducing c-section rates, and were encouraged to use approaches from the CMQCC toolkit that were most appropriate to them, and to track various process measures.

At the state level, the CMQCC partnered with several organizations, including the California Health Care Foundation, Smart Care California, Covered California, and the California Department of Health Care Services, to implement informational and financial incentives to reduce NTSV c-section rates. These included an annual "honor roll" released by the California Health and Human Services

[16] In the analyses that follow, we exclude 60 of the 147 untreated hospitals that are part of the major systems Dignity Health, Sutter Health, Kaiser Permanente, and University of California, given information in CMQCC materials that they ran their own similar system-wide interventions (Main, 2023). We do not have specifics regarding the timing of these interventions, nor whether they included all of the same components as the CMQCC-led intervention.

[17] The document's full title is *National Patient Safety Bundle: Safe Reduction of Primary Cesarean Births*.

Agency highlighting hospitals with NTSV c-section delivery rates of 23.9 percent or less; publicly available information on NTSV c-section delivery rates on the http://www.CalHospitalCompare.org website and on each hospital's Yelp.com landing page; and tracking of c-section rates by public and private health plans with performance-based financial incentives for meeting targets.

Figure 2 plots NTSV c-section rates among each of the three cohorts of hospitals over the period of 2013 to 2019, as well as among the never treated hospitals.[18] The dotted vertical lines indicate implementation dates for each cohort. The trends among hospitals in cohorts 1 and 3 evolve in parallel to those among the never treated hospitals before the intervention. However, c-section rates in cohort 2 hospitals appear to decline substantially in the months *preceding* the start of cohort 2's intervention, perhaps indicating the indirect effects of the state-wide efforts on this group. Therefore, in our main empirical analysis, we drop births in cohort 2 hospitals, and only use variation from the timing of intervention implementation in cohorts 1 and 3. That said, if the broader state-wide efforts also impacted c-section rates among never-treated and not-yet treated cohort 3 hospitals, we expect an attenuation of our estimates of the intervention's effects. In a robustness check, we study the statewide effects of the CMQCC intervention on all NTSV births (i.e., regardless of treatment status) by comparing them to a control group of high-risk non-NTSV births that were not targeted by the intervention.

3 Data and Analysis Sample

We use a novel dataset created by linking confidential birth certificate data for 3.4 million children born in California between 2013 and 2019 to maternal earnings records contained in the Longitudinal Employer Household Dynamics (LEHD) database. This linkage was facilitated by the U.S. Census Bureau, whose staff used the Personal Identification Validation System (PVS) to assign mothers unique anonymized identifiers—called Protected Identification Keys, or PIKs—based on maternal identifying information provided on the birth record. Our study team then uses the PIKs to link the birth records data to other data sources in the Census data infrastructure that have undergone a similar process. In a small share of cases when the mother's identifying information is incomplete on the birth record, a PIK is not able to be assigned to the mother. If the child's PIK is available, then we use additional Census datasets, including the Census Household Composition Key and the American Community Survey, to fill in missing maternal PIKs. Additional information on this process is available in Appendix A of

[18] To construct the never treated group, we combine both ineligible hospitals and those that were eligible but declined to participate because the c-section trends across these two groups evolve in parallel over the time period of analysis (although their c-section levels are different).

Kennedy-Moulton et al. (Forthcoming). Overall, we are able to assign a maternal PIK to 90.8 percent of all birth certificate records.

We then use information on the birth records to create a sample of NTSV births. That is, we restrict to first-time singleton births born at 37 weeks of gestation or more and in vertex position. Next, we use the hospital identifiers to select NTSV births in three groups of hospitals: cohort 1, cohort 3, and never treated.[19] All in all, we include births at 94 hospitals in California. As mentioned in Section 2, cohort 1's intervention period began in June 2016 (2016q2), while cohort 3's began in January 2018 (2018q1). For our primary analysis sample, we consider NTSV births that occurred from 12 quarters before cohort 1's intervention to six quarters after cohort 3's intervention: that is, births between April 2013 (2013q2) and September 2019 (2019q3).

Table 1 provides the means of maternal characteristics for the births observed in our sample. Across study groups, the largest share of births occur to mothers aged 25–34 years old, and the most prevalent maternal race and ethnicity groups are Hispanic and non-Hispanic white. We observe some variation across hospital groups in maternal education level, with a larger fraction of women having four years of college or more in Cohort 1 and the smallest in never treated hospitals. Maternal characteristics in cohort 2 hospitals—which we drop from our main analysis—are mostly in the middle between those in cohort 1 and cohort 3 (e.g., the share of mothers who are Hispanic is 0.34 in cohort 1, 0.37 in cohort 2, and 0.48 in cohort 3). More generally, these differences highlight the importance of using a staggered DD design that measures within-hospital changes in outcomes in treated cohorts relative to never and not-yet treated ones.

When measuring labor market outcomes, we restrict our attention to NTSV births by mothers who had any positive earnings in the quarter before birth. The earnings data are contained in the LEHD files for California and 11 other states.[20] These records have information on quarterly earnings, as reported to the Census Bureau by state unemployment insurance (UI) agencies. Therefore, we only have information on earnings at jobs that are covered by the UI system, which is estimated to include more than 90 percent of the workforce (Isen et al., 2017), but excludes some categories such as "gig" workers and self-employed individuals. The LEHD also contains an anonymized employer identifier. We use LEHD data covering the period 2010q2 through 2020q1 to measure maternal labor market outcomes.[21]

[19] As mentioned earlier, we exclude hospitals that ran their own separate interventions, as well as cohort 2 hospitals.

[20] The other states are: AZ, DC, DE, KS, MD, ME, ND, NV, OK, TN, and WI.

[21] We purposely do not include LEHD data beyond 2020q1 to avoid any potentially confounding impacts of the COVID-19 pandemic. This means we restrict our main sample to births from 2013q2 to 2018q4 when studying medium-term labor

Delivery and birth outcomes. The birth certificate records include information on the method of delivery, and several measures of maternal and infant health. Our first outcome of interest is a binary indicator equal to one if a birth is delivered via a c-section. Additionally, we create an indicator capturing maternal morbidity, which is set to one if any of the following complications are checked on the birth record: blood transfusion, third or fourth degree perineal laceration, ruptured uterus, unplanned hysterectomy, admission to the intensive care unit, and unplanned operating room procedure following delivery. Panel A of Table 2 reports average c-section rates and maternal morbidity prevalence for each hospital group during our study period. Cohorts 1 and 3 have similar c-section rates of around 30 percent, while the rate is lower in never treated hospitals (26 percent). Average rates of maternal morbidity range from one to two percent across hospital groups. In addition to these focal delivery outcomes, we also construct a binary indicator for induction of labor—i.e., the use of medical interventions, such as administering a hormonal drug called Pitocin, to stimulate uterine contractions—to measure any other changes in the method of delivery. Lastly, we create an indicator for low birth weight (less than 2,500 grams, or LBW) and an indicator for an early-term birth (between 37 and 39 weeks gestation).[22]

Labor market outcomes. For the sub-sample of mothers with positive earnings in the quarter before childbirth, we measure the following outcomes in the quarter following delivery: (1) an indicator for having positive earnings to capture return to employment, (2) an indicator for having positive earnings from their pre-birth employer to capture return to the pre-birth employer,[23] (3) the decile rank in the employer-specific earnings distribution (where zeros are assigned the lowest ranking),[24] and (4) total earnings (including zeros) in $2019 dollars. Panel B of Table 2 shows averages for these outcomes by hospital group. Approximately three-quarters of women return to employment in the quarter following birth, with the vast majority of them returning to their pre-birth employer. Women also tend to be about midway between the third and fourth earnings deciles at their employers.

Analogously, we measure these same outcomes as averages over quarters three, four, and five following delivery (i.e., from nine months to 1.5 years later) to capture medium-term maternal labor

market outcomes that are measured as averages over quarters three to five after birth, as described below.

[22] Since we focus on NTSV births, we only include term births at 37 weeks gestation or higher, and therefore have virtually no observations that are classified as very low birth weight (below 1,500 grams).

[23] For cases in which a woman has more than one employer in the quarter before childbirth, we set this indicator equal to one if she has positive earnings from any of her pre-birth employers in the quarter following delivery.

[24] For mothers with multiple employers in the same quarter, we use the one with the highest earnings.

market outcomes.[25] As seen in Panel C of Table 2, rates of employment exceed 80 percent during this window. Meanwhile, rates of employment at pre-birth employers are slightly lower than those observed in the quarter following birth. Average earnings rank is higher when measured over quarters three to five after delivery than in the quarter following it, which may reflect either differences in the composition of women who are observed in the labor market over the medium-term rather than in the immediate post-birth window or upward job transitions.

Subsequent fertility and delivery outcomes. We also use the birth certificate records to create a binary indicator if a mother has any subsequent birth in the ten quarters (i.e., 2.5 years) following the initial delivery. Among mothers with a subsequent birth, we examine the method of delivery, creating an indicator for a c-section delivery at the second birth. Further, we study the same maternal and infant health outcomes for the second birth as we do for the initial birth. We also examine preterm births (less than 37 weeks gestational age), the prevalence of which may be affected by prior c-sections (Visser et al., 2020; Woolner et al., 2024). Outcome means for this analysis are reported in Panel D of Table 2. Just under 30 percent of women have another birth within this ten quarter window and the c-section rate among those subsequent births ranges from 26 to 31 percent. Rates of maternal morbidity are very low, while roughly five to six percent of the subsequent births are preterm.

4 Empirical Methods

For all outcomes except for those capturing subsequent births, we adopt a staggered difference-in-differences design that uses never treated and not-yet-treated hospitals as the control group for treated hospitals in cohorts 1 and 3, following the methods in Callaway and Sant'Anna (2021) (CS). We aggregate the group and time estimates to the "event-time" level to capture effects relative to the quarter of the intervention.

Concretely, our event-study model is specified as:

$$y_{iht} = \alpha + \sum_{k=-12, k\neq -1}^{k=6} \beta_k \mathbf{1}[t - CMQCC^*_{ht} = k] + \psi' X_i + \eta_t + \gamma_h + \varepsilon_{iht} \tag{1}$$

for each NTSV birth i in hospital h delivered in quarter t. y_{iht} is an outcome of interest, such as an indicator for a c-section delivery. The event-time indicators, $\mathbf{1}[t - CMQCC^*_{ht} = k]$, reflect the quarter

[25] We do not consider labor market outcomes over a longer period following childbirth because the measurement of those outcomes would coincide with the COVID-19 pandemic. See footnote 27 below for more details.

of birth relative to the start of the CMQCC c-section intervention. When studying focal delivery and birth outcomes, as well as maternal labor market outcomes measured in the quarter after birth, we use a sample of NTSV births occurring in the period of 12 quarters before to six quarters after the intervention.[26] When studying medium-term labor market outcomes, we restrict the post-period to births occurring up to three quarters after the intervention to avoid any confounding with the COVID-19 pandemic.[27] The following individual-level characteristics are included in vector X_i: indicators for maternal race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, non-Hispanic Asian, other/missing); indicators for maternal age categories (less than 20, 20-24, 25-34, 35+, missing); indicators for maternal education level (less than high school, high school or some college, 4-year college or more, missing), and an indicator for whether the mother is foreign-born. We control for calendar year-by-quarter fixed effects, η_t, to account for statewide trends, and hospital fixed effects, γ_h, to account for time-invariant differences across hospitals. We cluster standard errors on the hospital level.

The CS estimator computes each feasible 2×2 DD combination to obtain the average treatment effect $ATT(g, t)$ for each treated cohort that begins the intervention in quarter g for every calendar quarter t. The $ATTs$ in both the pre- and post-intervention period use $g - 1$ as the base period (i.e., the quarter just prior to treatment). For this reason, the $g - 1$ term is omitted. We report the dynamic treatment effects using CS event-study plots, along with a single DD estimate that aggregates the post-treatment $ATTs$. Since we include covariates in equation (1), we use the doubly robust estimator proposed by Sant'Anna and Zhao (2020).

For the analysis of subsequent fertility and delivery outcomes, we only compare mothers who deliver at cohort 1 hospitals to those who deliver at the never-treated hospitals, before and after cohort 1's intervention implementation, with standard event-study and DD models (i.e., we do not have staggered treatment timing). We do not have sufficient follow-up data to study subsequent fertility among mothers who deliver at the cohort 3 hospitals.

Causal identification of the effects of the CMQCC intervention relies on the assumption that outcomes among women delivering in cohort 1 and cohort 3 hospitals would have evolved similarly to those among women delivering in the untreated and not-yet-treated hospitals in the absence of the

[26]Note that while our data are cross-sectional (i.e., we only have one observation per birth), the sample period reflects a balanced cohort×quarter panel, where both cohorts can be observed in every event-time period.

[27]As noted in Section 3, our medium-term labor market outcomes are measured three to five quarters after birth. Therefore, women who gave birth four or more quarters after the intervention in cohort 3 (i.e., from 2019q1 onward) have these labor market outcomes measured at least partially during the pandemic.

intervention (i.e., the "parallel trends" assumption). Although this assumption is not directly testable, we examine estimates of the event-time coefficients in the pre-period to assess its credibility. Moreover, to address the concern that the timing of the intervention could be correlated with other determinants of maternal economic trajectories, we study maternal labor market outcomes observed *before* childbirth as placebo outcomes that should not be affected by the intervention. This placebo analysis also allows us to examine whether higher earning mothers selected into treated hospitals after the CMQCC intervention was implemented (e.g., due to a change in the perception of the hospital's quality) in ways that could be mistaken for the treatment effect of the program itself.

5 Results

This section presents our results, starting with the effects on c-section rates and maternal and infant health outcomes at the focal delivery. We then describe our results on maternal labor market outcomes, both in the short- and medium-term following childbirth, and present our results testing selection into treated hospitals based on maternal labor market outcomes observed *before* the birth. Next, we discuss the impacts on subsequent fertility and delivery outcomes. Lastly, we discuss some additional heterogeneity analyses, alternative specifications that examine effects on all NTSV births in the state, and spillover effects on non-NTSV births.

C-section deliveries and maternal and infant health outcomes. Figure 3(a) plots the CS event-study estimates of the impact of the CMQCC intervention on c-section rates in our sample of NTSV births. Prior to the intervention, the graph shows that the c-section rates in treated and not treated hospitals evolved similarly. However, following the intervention, c-section rates in treated hospitals fell significantly. Specifically, the estimated DD coefficient indicates a 3.0 percentage point reduction in the probability of a c-section delivery, which is a 9.7 percent effect size relative to the mean c-section rate in the treated cohorts in the quarter before the intervention. In sub-figure (b), when we restrict our sample to women who were employed in the quarter prior to the birth, we observe a decline of similar magnitude (3.2 percentage points, or 10.2 percent). Both coefficients are statistically significant at the 0.001 level. Our estimates are in the ballpark of the 13 percent decline reported by CMQCC's research team, who used a slightly different sample period and estimation strategy (Rosenstein et al., 2021). These results confirm that the CMQCC intervention was indeed effective at reducing c-section rates among participating hospitals.

The reduction in c-sections could have downstream impacts on maternal health if it changes the risk of complications during and after childbirth. We investigate this possibility by estimating the effect of the CMQCC intervention on the maternal morbidity outcome in Figure 4(a). We do not find any statistically significant change in maternal morbidity, although, as previously noted, this result should be interpreted with caution since maternal morbidity is known to be under-reported on the birth certificate (Gemmill et al., 2022). In Figure 4(b), we report results for induction of labor, similarly finding no significant effect. We also find no evidence of impacts on either rates of low birth weight or early-term births in Figures 4(c) and (d), which could have been impacted if physicians changed their protocols regarding scheduling elective c-sections.[28]

Maternal labor market outcomes. Next, we consider changes in mothers' labor market outcomes immediately following the birth, using our sample of women who were employed in the quarter prior to the birth. In Figures 5(a) and (b), we show that women exposed to the CMQCC intervention are 1.7 percentage points (2.4 percent) more likely to return to employment in the quarter following the birth and 2.2 percentage points (3.2 percent) more likely to return to their pre-birth employer, respectively. However, these effects should be interpreted with caution given we estimate some large and imprecise pre-period event study coefficients. We also examine the impact of the intervention on the mother's earnings decile within her firm (capturing her rank among coworkers) and her total earnings in the quarter after birth in Figure 5(c) and (d), respectively, finding positive, but statistically insignificant, coefficients.

While the reduced form effect of the intervention on maternal employment may appear modest in size, it is large when compared to the approximately 3 percentage point change in the c-section rate reported in Figure 3(b), suggesting that labor supply effects among women whose delivery method changed as a result of the program (i.e., the "compliers") are potentially quite substantial. However, we do not estimate an instrumental variables model to interpret our findings as the local average treatment effect of a c-section delivery, since the CMQCC intervention could have impacted other aspects of healthcare received by women. For example, it is possible that women who ended up with c-sections in treated hospitals—i.e., the intervention did not change their likelihood of a c-section delivery—experienced changes in their care before, during, or after the delivery.

We also study maternal labor market outcomes observed between nine months and 1.5 years

[28]A survey of participants in the CMQCC intervention suggests that successful hospitals endorsed peer review of all elective c-sections (VanGompel et al., 2021), implying that these outcomes could have in principle been impacted.

(i.e., three to five quarters) following childbirth, which we consider "medium-term" effects, in Figure 6. In sub-figures (a) and (b), we examine whether the mother is employed and employed at her previous employer during this window, respectively. While we do not detect any significant effect on employment overall, we do see a marginally significant increase in the probability that a mother is employed at her pre-birth employer of 1.5 percentage points, or 2.3 percent. We also observe positive but not statistically significant effects on the mother's average earnings decile and quarterly earnings in levels over quarters three to five following childbirth, as reported in sub-figures (c) and (d).

We next conduct a placebo analysis to assess whether there may have been selection into the hospitals that adopted the CMQCC intervention. If, for example, women who were more connected to the labor market prior to childbirth were more likely to choose treated hospitals once the intervention was implemented, then our labor market effects may be biased upward. To assess this possibility, we study mothers' labor market outcomes *before* birth. Appendix Figure A1 shows that there is no significant relationship between the CMQCC intervention and the mother's total earnings or her earnings decile in the quarter before birth. Similarly, in Appendix Figures A2(b) and (c), we find no effects on the mother's average earnings decile or average quarterly earnings measured three to five quarters before birth. Appendix Figure A2(a) shows a very small (less than one percent of the pre-treatment mean) negative coefficient for employment three to five quarters before birth. Taken together, these placebo analyses give us confidence that positive selection into hospitals around the time of the CMQCC is unlikely to be driving our results.

Subsequent fertility and delivery outcomes. Nationally, about 85 percent of women who have an initial c-section delivery and go on to have another child end up with a repeat c-section (Hamilton et al., 2024). The presence of scar tissue from an initial c-section makes each subsequent c-section more risky, implying that some of the largest health benefits of avoiding c-sections are likely to materialize due to the avoidance of future ones (Keag et al., 2018).

To explore these effects, we examine how exposure to the CMQCC intervention affected the probability of having a second birth and the health and delivery characteristics of that birth if it occurred. Because our birth certificates data end in 2019, we restrict our attention to the 2.5-year period following a first birth in this analysis, and, as noted previously, compare cohort 1 to the never treated group in a standard event-study framework.

About 28.2 percent of mothers in our NTSV sample are observed having another child within this time frame, and Figure 7(a) shows that there is no change in the likelihood of a subsequent birth associated with the intervention. However, Figure 7(b) shows that among those with a subsequent birth, mothers exposed to the CMQCC intervention at the time of the first birth are significantly less likely to have a c-section at the next one. Specifically, the probability of a c-section at the second birth falls by about 2.2 percentage points (7.1 percent). Notably, the magnitude of this effect is nearly as large as the magnitude at the initial birth (a 3.0 percentage point reduction in the likelihood of a c-section), reflecting the fact that relatively few women give birth vaginally after having a first c-section. Combined with the second birth rate of 28.2 percent, we therefore estimate that each initially avoided c-section averts at least 1.2 c-sections in total.[29] Incorporating longer birth spacings and higher-order births, which we are not able to do given our limited follow-up window, would likely yield an even larger number of averted c-sections.

We also find that subsequent births are less likely to be delivered preterm in Figure 7(d). In particular, exposure to the CMQCC intervention at the first birth reduces the probability that a subsequent birth is preterm by 0.8 percentage points, or about 13.4 percent relative to the baseline mean in the treated cohort. This result is supported by a medical literature that finds that subsequent births following a c-section have a higher risk of complications such as placenta praevia and placental abruption, which can result in a preterm delivery (Yang et al., 2007; Williams et al., 2018). We do not find significant effects on the prevalence of low birth weight or maternal morbidity among subsequent births.

Overall, our results demonstrate that there are no adverse health impacts of reducing c-section deliveries. If anything, there may be health benefits, driven by subsequent deliveries that are less likely to occur preterm. Moreover, if subsequent c-sections have similar impacts on maternal labor market outcomes as an initial one, then we may expect that the total economic benefits of reduced c-sections may increase with birth order.

Heterogeneity by predicted c-section risk. While the CMQCC intervention targeted all NTSV births in participating hospitals, it is possible that some sub-groups were more impacted than others. In particular, deliveries that would have been the most likely to end up as c-sections in the absence of the intervention may be the most impacted. To assess this conjecture, we predict the likelihood of a c-section for each delivery using the following pre-delivery characteristics available in the birth

[29] We calculate this as the ratio of treatment effects on c-sections in the initial and subsequent birth ($\frac{0.0218}{0.030}$) times the probability a second birth is observed (0.282).

certificates data: indicators for various pregnancy complications,[30] an indicator for maternal age over 40, the number of prenatal care visits, and an indicator for at least one prenatal care visit. We then split our baseline analysis sample into low-, medium- and high-risk sub-groups according to terciles of the predicted c-section distribution.

Appendix Figure A3 presents the event-study graphs for these three sub-groups, using the c-section indicator as the outcome. While it appears that the reduction in c-sections exists in all three groups, the effect size is largest in both absolute and relative terms for NTSV births in the top tercile of the predicted c-section distribution (Appendix Figure A3(c)). This result is consistent with the conjecture that the intervention was most successful at changing the delivery mode of NTSV births that would have otherwise been most likely to end up as c-sections.

At the same time, we find no significant effects on maternal and infant health outcomes among births in any of the three categories of predicted c-section risk in Appendix Figure A4. The lack of any adverse consequences suggests that the marginal c-sections avoided due to the intervention—i.e., those that had the highest *a priori* risk of a c-section among all NTSV births—were likely medically unnecessary. We also find no indication of significant differences in effects by predicted c-section risk for our immediate and medium-term labor market outcomes (Appendix Figures A5 and A6, respectively) or for subsequent birth outcomes (Appendix Figure A7). That said, the confidence intervals for some outcomes are fairly wide, suggesting we may be underpowered in these sub-group analyses.

Overall effects on all NTSV births. As noted in Section 2, the CMQCC initiative included a number of statewide efforts that could have impacted c-section deliveries among NTSV births in all hospitals and not just those that directly participated in the collaborative. We explore these potential broader effects by estimating an event-study model, in which the treatment group consists of all NTSV births in California over our analysis period, while the control group includes *non*-NTSV births that have a predicted c-section likelihood in the top tercile of the distribution.[31] We use this control group as a proxy for births that were highly unlikely to be targeted by any CMQCC c-section-related efforts, since these deliveries are the most likely to include clinically-indicated c-sections. We use the start of cohort 1's intervention as the beginning of treatment period.

[30] We include over 20 pregnancy complication indicators, such as: diabetes, hypertension, previous poor pregnancy outcomes, pre-delivery obstetric procedures, infections during pregnancy, and pregnancy resulting from infertility treatment.

[31] To predict c-section risk among non-NTSV births, we use the same predictors as those used in predicting risk among NTSV births described above, along with parity indicators (first, second, or third or more birth), and an indicator for a non-singleton birth.

Appendix Figure A8 presents the results from this exercise. We find that the probability of a c-section decreases by 1.8 percentage points, or 6.3 percent relative to the pre-treatment mean. Thus, it appears that there is an overall effect on the likelihood of a c-section delivery among NTSV births in California, although it is smaller in magnitude than when we directly focus on births in treated hospitals.

Spillover effects on non-NTSV births. It is also possible that the CMQCC intervention had spillover effects on non-targeted births due to changes in hospital-wide protocols or practices. We investigate this possibility in Appendix Figure A9, where we present results from estimating event-study model (1), except we use non-NTSV births in Cohort 1, Cohort 3, and never-treated hospitals in the analysis sample and add controls for parity indicators and a non-singleton birth indicator. We use all non-NTSV births in sub-figure (a), and further split into terciles of the predicted c-section risk distribution in sub-figures (b), (c), and (d). We do not find any evidence of statistically significant changes in the likelihood of a c-section delivery among these non-NTSV births. Interestingly, if anything, the likelihood of a c-section delivery appears to increase among the highest-risk non-NTSV births in sub-figure (d), which could reflect better decision-making that results in a reallocation of procedures from lower to higher-risk cases as discussed by Currie and MacLeod (2017). However, this interpretation should be perceived with caution since the positive DD coefficient in sub-figure (d) is not statistically significant, and there is some indication of a pre-trend for this high-risk non-NTSV sub-group.

6 Conclusion

The United States has a higher c-section rate and worse maternal and infant health outcomes compared to many other high-income countries. These facts—while not necessarily causally linked through simple cross-country comparisons—could suggest that many American women have medically unnecessary surgeries. Correspondingly, major national public health initiatives, such as Healthy People 2030, have set objectives regarding reducing c-section deliveries among low-risk births.

In this paper, we document that efforts to reduce c-section deliveries have benefits for mothers and their families that extend beyond the current birth. We examine a large state-level intervention targeting hospitals with high c-section rates among low-risk first-time births in California. We find a significant almost 10 percent reduction in the use of the procedure, and no changes in maternal or infant health as a result. At the same time, we find that among women working before childbirth, those exposed to the intervention appear to be more likely to return to employment in the quarter after birth, and also more likely to return to their pre-birth employer. While the overall employment effects fade over time, the estimated effect of the intervention on attachment to the same employer remains positive and marginally statistically significant three to five quarters after childbirth.

We also find a substantial reduction in future c-sections among women who go on to have another child. While we are unable to trace out the longer-term labor market outcomes for the women affected by the intervention, this finding suggests there may be even larger economic impacts of avoiding the initial c-section if effects across multiple births for a mother are incorporated. Finally, we find evidence of improved health for the mother's next child with a reduction in the likelihood of preterm birth, indicating that the effects of the intervention also spillover to younger siblings.

Taken together, our findings suggest that reducing the c-section rate among low-risk first-time mothers—and thus bringing the c-section rate closer to that of many high-income countries—could improve the economic outcomes for new mothers at no cost for their firstborn children, while also sparing these mothers and their families from the downstream costs of repeat c-sections.

References

Agency for Healthcare Research and Quality, "A statewide collaborative to support vaginal birth and reduce unnecessary cesarean deliveries," *Patient Safety Network*, 2022.

Andresen, Martin Eckhoff and Emily Nix, "What causes the child penalty? Evidence from adopting and same-sex couples," *Journal of labor economics*, 2022, *40* (4), 971–1004.

Angolile, Cornel M, Baraka L Max, Justice Mushemba, and Harold L Mashauri, "Global increased cesarean section rates and public health implications: A call to action," *Health science reports*, 2023, *6* (5), e1274.

Antoine, Clarel and Bruce K Young, "Cesarean section one hundred years 1920–2020: the Good, the Bad and the Ugly," *Journal of perinatal Medicine*, 2021, *49* (1), 5–16.

Assistant Secretary for Planning and Evaluation, "Addressing the Maternal Health Crisis in the United States: An Update from the U.S. Department of Health and Human Services," July 2024.

Athey, Susan and Guido W Imbens, "Design-based analysis in difference-in-differences settings with staggered adoption," *Journal of Econometrics*, 2022, *226* (1), 62–79.

Baicker, Katherine, Kasey S Buckles, and Amitabh Chandra, "Geographic variation in the appropriate use of cesarean delivery: do higher usage rates reflect medically inappropriate use of this procedure?," *Health Affairs*, 2006, *25* (Suppl1), W355–W367.

Belluz, Julia, "California decided it was tired of women bleeding to death in childbirth," *Vox*, December 2017.

Bögl, Sarah, Jasmin Moshfegh, Petra Persson, and Maria Polyakova, "The economics of infertility: Evidence from reproductive medicine," Working Paper w32445, National Bureau of Economic Research 2024.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event study designs: Robust and efficient estimation," *arXiv preprint arXiv:2108.12419*, 2021.

Callaway, Brantly and Pedro H. C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Card, David, Alessandra Fenizia, and David Silver, "The health impacts of hospital delivery practices," *American Economic Journal: Economic Policy*, 2023, *15* (2), 42–81.

Chaisemartin, Clément De and Xavier d'Haultfœuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 2020, *110* (9), 2964–96.

Clark, Steven L, Michael A Belfort, Gary DV Hankins, Janet A Meyers, and Frank M Houser, "Variation in the rates of operative delivery in the United States," *American journal of obstetrics and gynecology*, 2007, *196* (6), 526–e1.

Corredor-Waldron, Adriana, Janet Currie, and Molly Schnell, "Drivers of racial differences in C-sections," Working Paper w32891, National Bureau of Economic Research 2024.

Costa-Ramón, Ana María, Ana Rodríguez-González, Miquel Serra-Burriel, and Carlos Campillo-Artero, "It's about time: Cesarean sections and neonatal health," *Journal of health economics*, 2018, *59*, 46–59.

Costa-Ramón, Ana, Mika Kortelainen, Ana Rodríguez-González, and Lauri Sääksvuori, "The long-run effects of cesarean sections," *Journal of Human Resources*, 2022, *57* (6), 2048–2085.

Currie, Janet and W Bentley MacLeod, "Diagnosing expertise: Human capital, decision making, and performance among physicians," *Journal of labor economics*, 2017, *35* (1), 1–43.

Cutler, David M., "The lifetime costs and benefits of medical technology," *Journal of Health Economics*, December 2007, *26* (6), 1081–1100.

Deryugina, Tatyana and David Molitor, "The causal effects of place on health and longevity," *Journal of Economic Perspectives*, 2021, *35* (4), 147–170.

Ding, Hui, "Geographic Variation in Mental Health Treatment Utilization: Evidence from Migration," *American Economic Journal: Economic Policy*, Forthcoming.

Doyle, Joseph J., John A. Graves, Jonathan Gruber, and Samuel A. Kleiner, "Measuring Returns to Hospital Care: Evidence from Ambulance Referral Patterns," *Journal of Political Economy*, February 2015, *123* (1), 170–214.

Eichmeyer, Sarah and Jonathan Zhang, "Pathways into opioid dependence: Evidence from practice variation in emergency departments," *American Economic Journal: Applied Economics*, 2022, *14* (4), 271–300.

Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams, "Sources of geographic variation in health care: Evidence from patient migration," *The quarterly journal of economics*, 2016, *131* (4), 1681–1726.

_ , _ , and _ , "Place-based drivers of mortality: Evidence from migration," *American Economic Review*, 2021, *111* (8), 2697–2735.

Fischer, Stefanie, Heather Royer, and Corey White, "Health Care Centralization: The Health Impacts of Obstetric Unit Closures in the United States," *American Economic Journal: Applied Economics*, 2024, *16* (3), 113–141.

Gallen, Yana, Juanna Schrøter Joensen, Eva Rye Johansen, and Gregory F Veramendi, "The Labor Market Returns to Delaying Pregnancy," *Available at SSRN 4554407*, 2023.

Gemmill, Alison, Molly Passarella, Ciaran S Phibbs, Elliott K Main, Scott A Lorch, Katy B Kozhimannil, Suzan L Carmichael, and Stephanie A Leonard, "Validity of birth certificate data compared with hospital discharge data in reporting maternal morbidity and disparities," *Obstetrics & Gynecology*, 2022, pp. 10–1097.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Gunja, Munira Z., Relebohile Gumas Evan D.and Masitha, and Laurie C. Zephyrin, "Insights into the U.S. Maternal Mortality Crisis: An International Comparison," Issue Brief, The Commonwealth Fund June 2024.

Halla, Martin, Harald Mayr, Gerald J. Pruckner, and Pilar García-Gómez, "Cutting fertility? Effects of cesarean deliveries on subsequent fertility and maternal labor supply," *Journal of Health Economics*, 2020, *72*, 102325.

Hamilton, Brady E, Joyce A Martin, and Michelle JK Osterman, "Births: Provisional Data for 2023," Technical Report 35, Centers for Disease Control and Prevention, NVSS Vital Statistics Rapid Release April 2024.

Isen, Adam, Maya Rossin-Slater, and W Reed Walker, "Every Breath You Take—Every Dollar You'll Make: The Long-Term Consequences of the Clean Air Act of 1970," *Journal of Political Economy*, 2017, *125* (3), 848–902.

Jensen, Vibeke Myrup and Miriam Wüst, "Can Caesarean section improve child and maternal health? The case of breech babies," *Journal of Health Economics*, January 2015, *39*, 289–302. Publisher: Elsevier BV.

Kassebaum, Nicholas J, Ryan M Barber, Zulfiqar A Bhutta, Lalit Dandona, Peter W Gething, Simon I Hay, Yohannes Kinfu, Heidi J Larson, Xiaofeng Liang, Stephen S Lim et al., "Global, regional, and national levels of maternal mortality, 1990–2015: a systematic analysis for the Global Burden of Disease Study 2015," *The Lancet*, 2016, *388* (10053), 1775–1812.

Keag, Oonagh E, Jane E Norman, and Sarah J Stock, "Long-term risks and benefits associated with cesarean delivery for mother, baby, and subsequent pregnancies: Systematic review and meta-analysis," *PLoS medicine*, 2018, *15* (1), e1002494.

Kennedy-Moulton, Kate, Sarah Miller, Petra Persson, Maya Rossin-Slater, Laura R. Wherry, and Gloria Aldana, "Maternal and infant health inequality: New evidence from linked administrative data," *Review of Economics and Statistics*, Forthcoming.

Kissel, Helen and Helena Roy, "The Role of Providers in Variation in Cesarean Section Use," 2025. Stanford University, unpublished manuscript.

Kleven, Henrik, Camille Landais, and Gabriel Leite-Mariante, "The child penalty atlas," *Review of Economic Studies*, 2024, p. rdae104.

_ , _ , and Jakob Egholt Søgaard, "Does biology drive child penalties? Evidence from biological and adoptive families," *American Economic Review: Insights*, 2021, *3* (2), 183–198.

Kozhimannil, Katy Backes, Michael R Law, and Beth A Virnig, "Cesarean delivery rates vary tenfold among US hospitals; reducing variation may address quality and cost issues," *Health Affairs*, 2013, *32* (3), 527–535.

Lundborg, Petter, Erik Plug, and Astrid Würtz Rasmussen, "Can women have children and a career? IV evidence from IVF treatments," *American Economic Review*, 2017, *107* (6), 1611–1637.

Main, Elliott, January 2023. Personal Communication.

Main, Elliott K, Ravi Dhurjati, Valerie Cape, Julie Vasher, Anisha Abreo, Shen-Chih Chang, and Jeffrey B Gould, "Improving maternal safety at scale with the mentor model of collaborative improvement," *The Joint Commission Journal on Quality and Patient Safety*, 2018, *44* (5), 250–259.

McClellan, Mark, "Does More Intensive Treatment of Acute Myocardial Infarction in the Elderly Reduce Mortality?: Analysis Using Instrumental Variables," *JAMA*, September 1994, *272* (11), 859.

Mourot, Pauline, "Should Top Surgeons Practice at Top Hospitals? Sorting and Complementarities in Healthcare," 2024. University of Chicago, unpublished manuscript.

Mühlrad, Hanna, "Cesarean sections for high-risk births: health, fertility, and labor market outcomes*," *The Scandinavian Journal of Economics*, October 2022, *124* (4), 1056–1086. Publisher: Wiley.

Rae, Matthew, Cynthia Cox, and Hanna Dingel, "Health costs associated with pregnancy, childbirth, and postpartum care," Brief, Peterson-KFF Health System Tracker July 2022.

Robinson, Sarah, Heather Royer, and David Silver, "Geographic variation in cesarean sections in the United States: Trends, correlates, and other interesting facts," *Journal of Labor Economics*, 2024, *42* (S1), S219–S259.

Rogvi, Jessica Á, Aline Bütikofer, Lone Krebs, Hanna Mühlrad, and Miriam Wüst, "Cesarean Section, Childhood Health, and Schooling: Quasi-Experimental Evidence From Denmark, Norway and Sweden," *Health Economics*, March 2025, *34* (3), 431–441. Publisher: Wiley.

Rosenstein, Melissa G, Shen-Chih Chang, Christa Sakowski, Cathie Markow, Stephanie Teleki, Lance Lang, Julia Logan, Valerie Cape, and Elliott K Main, "Hospital quality improvement interventions, statewide policy initiatives, and rates of cesarean delivery for nulliparous, term, singleton, vertex births in California," *Jama*, 2021, *325* (16), 1631–1639.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe, "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 2023, *235* (2), 2218–2244.

Sant'Anna, Pedro H.C. and Jun Zhao, "Doubly Robust Difference-in-Differences Estimators," *Journal of Econometrics*, 2020, *219* (1), 101–22.

Singh, Manasvini, "Heuristics in the delivery room," *Science*, 2021, *374* (6565), 324–329.

Słabuszewska-Jóźwiak, Aneta, Jacek Krzysztof Szymański, Michał Ciebiera, Beata Sarecka-Hujar, and Grzegorz Jakiel, "Pediatrics consequences of caesarean section—a systematic review and meta-analysis," *International journal of environmental research and public health*, 2020, *17* (21), 8031.

Smith, Holly, Nancy Peterson, David Lagrew, and Elliott Main, "Toolkit to Support Vaginal Birth and Reduce Primary Cesareans: A Quality Improvement Toolkit.," Technical Report, California Maternal Quality Care Collaborative 2016.

Stenson, Jacqueline, "How hospitals in California lowered C-section rates for new mothers," *NBC News*, May 2021.

Stephenson, Joan, "Rate of first-time cesarean deliveries on the rise in the US," in "JAMA Health Forum," Vol. 3 American Medical Association 2022, pp. e222824–e222824.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Tikkanen, Roosa, Munira Z Gunja, Molly FitzGerald, and Laurie Zephyrin, "Maternal mortality and maternity care in the United States compared to 10 other developed countries," *Issue briefs, Commonwealth Fund*, 2020.

U.S. Department of Health and Human Services, "Healthy People 2020 Midcourse Review," 2017.

VanGompel, Emily C White, Susan L Perez, Avisek Datta, Francesca R Carlock, Valerie Cape, and Elliott K Main, "Culture that facilitates change: a mixed methods study of hospitals engaged in reducing cesarean deliveries," *The Annals of Family Medicine*, 2021, *19* (3), 249–257.

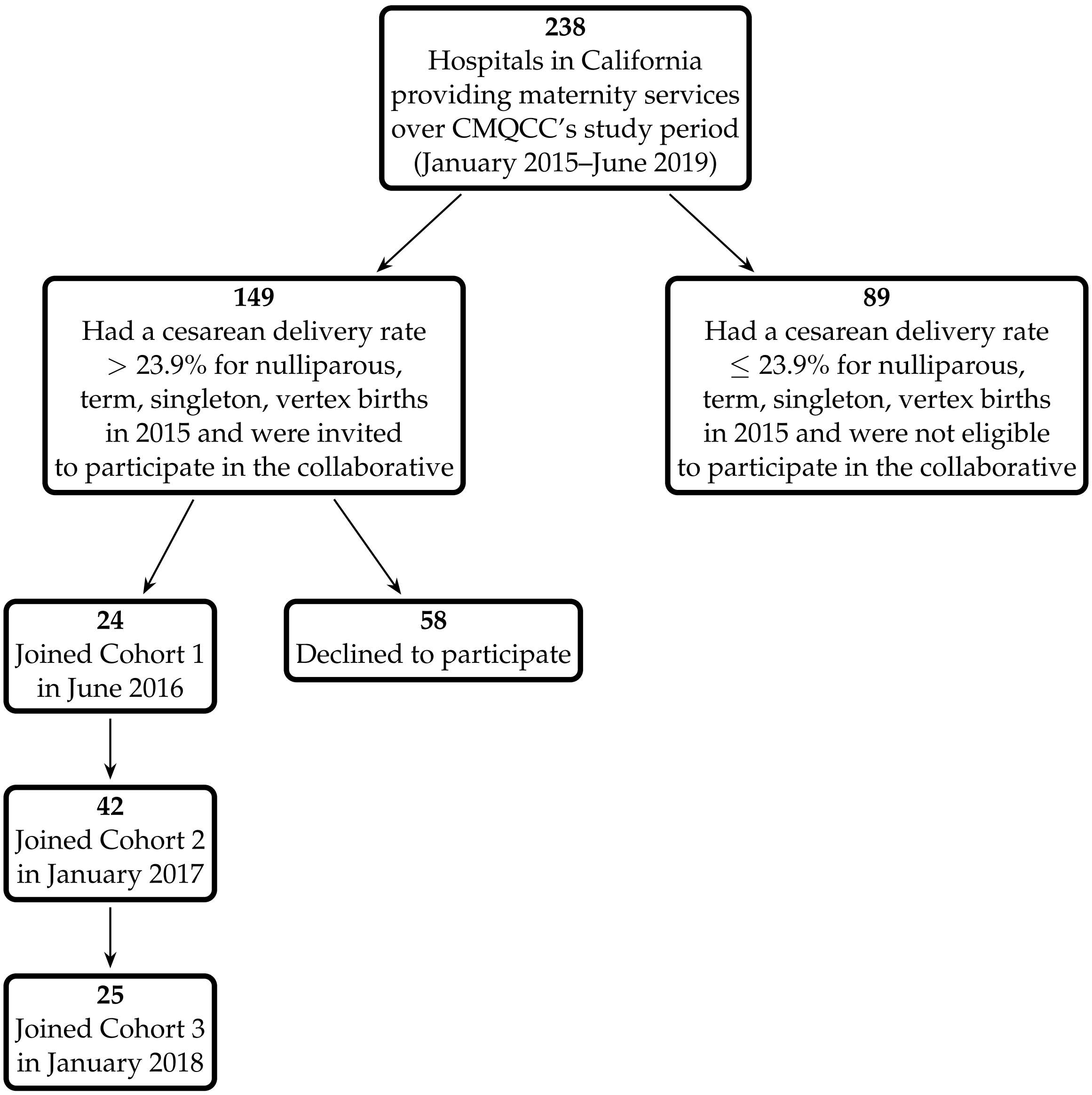
Visser, L, C Slaager, BM Kazemier, AL Rietveld, MA Oudijk, CJM de Groot, BW Mol, and de Boer MA, "Risk of preterm birth after prior term cesarean," *BJOG: An International Journal of Obstetrics and Gynaecology*, 2020, *127*, 610–617.

Williams, Corrine M, Ibitola Asaolu, Niraj R Chavan, Lucy H Williamson, Alysha M Lewis, Lauren Beaven, and Kristin B Ashford, "Previous cesarean delivery associated with subsequent preterm birth in the United States," *European Journal of Obstetrics & Gynecology and Reproductive Biology*, 2018, *229*, 88–93.

Woolner, Andrea MF, Edwin Amalraj Raja, Sohinee Bhattacharya, and Mairead E. Black, "Risk of spontaneous preterm birth elevated after first cesarean delivery at full dilation: a retrospective cohort study of over 30,000 women," *American Journal of Obstetrics & Gynecology*, 2024, *230* (3), 358.E1–358.E13.

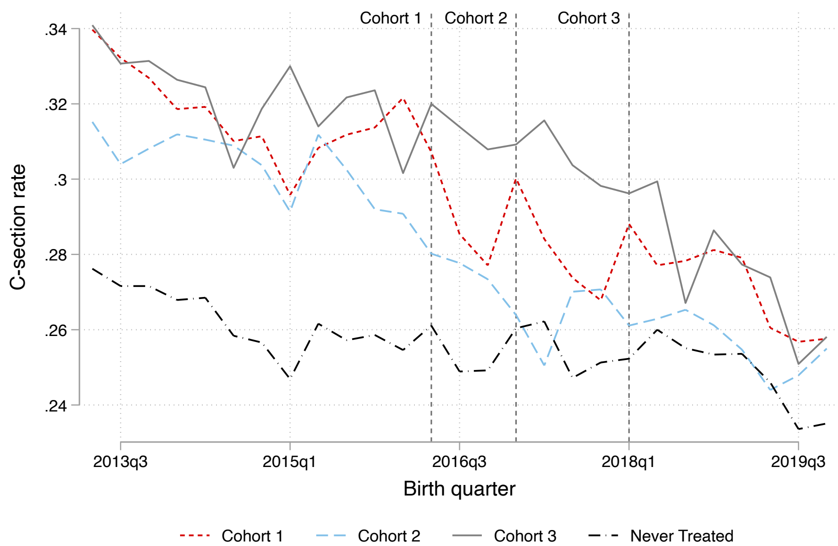
Yang, Q, SW Wen, L Oppenheimer, XK Chen, D Black, J Gao, and MC Walker, "Association of caesarean delivery for first birth with placenta praevia and placental abruption in second pregnancy," *BJOG: An International Journal of Obstetrics & Gynaecology*, 2007, *114* (5), 609–613.

Figure 1: Schematic of California Hospitals Included and Excluded in CMQCC's Collaborative to Support Vaginal Birth and Reduce Primary C-sections



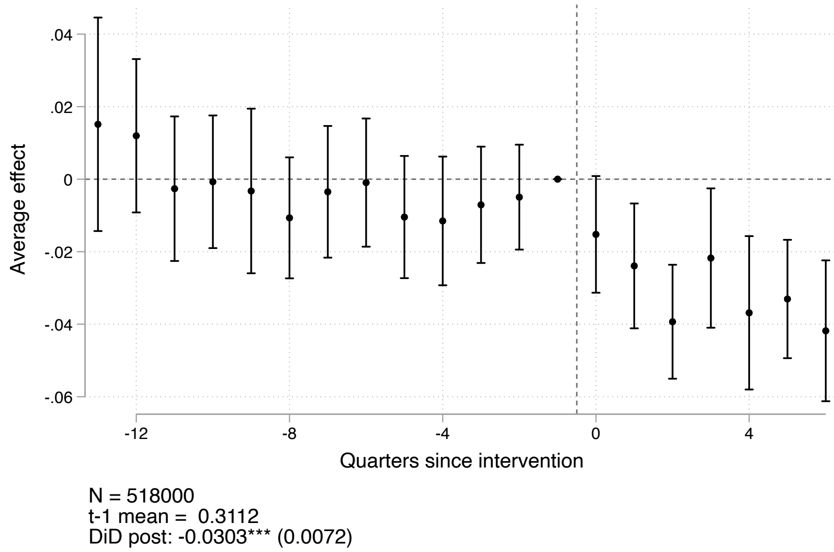
Notes: Figure depicts a schematic showing how many hospitals were included and excluded in the CMQCC c-section Intervention called the *Collaborative to Support Vaginal Birth and Reduce Primary C-sections*. It was created by the authors and is similar to Figure 1 in Rosenstein et al. (2021).

Figure 2: Raw NTSV C-section Rates by Intervention Cohort and Year

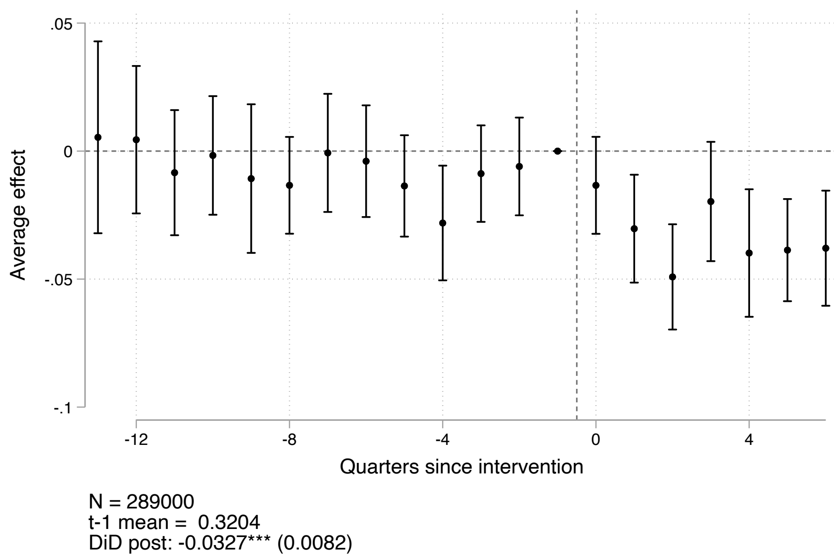


Notes: Figure plots c-section rates among NTSV births by intervention cohort over our sample period, 2013–2019. The never treated group includes both ineligible hospitals and hospitals that declined participation. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Figure 3: Effects of CMQCC's C-section Intervention on the Probability of a C-section Delivery Among NTSV Births



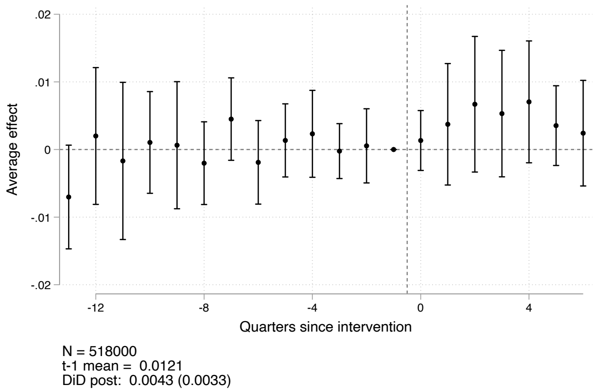
(a) All NTSV births



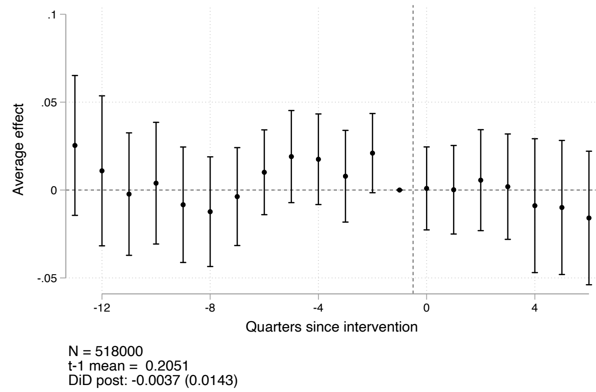
(b) NTSV births to women employed in the quarter before childbirth

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3. The outcome is a binary indicator for a c-section delivery. The figures also show the estimated difference-in-differences coefficients using the same pre- and post-intervention periods, standard errors in parentheses, and the sample means of the dependent variable in the treatment group at event-time$= -1$. Sub-figure (a) uses our sample of all NTSV births, while sub-figure (b) uses the sample of NTSV births by mothers with positive earnings in the quarter before birth. The regression models control for maternal race and ethnicity, age group, education level, and foreign-born status indicators, as well as year-by-quarter and hospital fixed effects. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

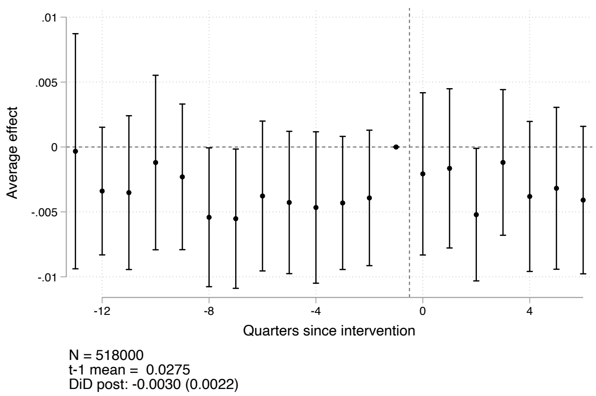
Figure 4: Effects of CMQCC's C-section Intervention on Maternal and Infant Health Outcomes at the Focal Delivery



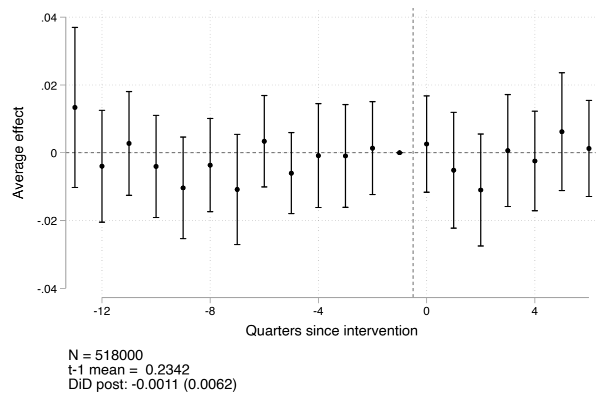
(a) Any maternal morbidity



(b) Induction of labor



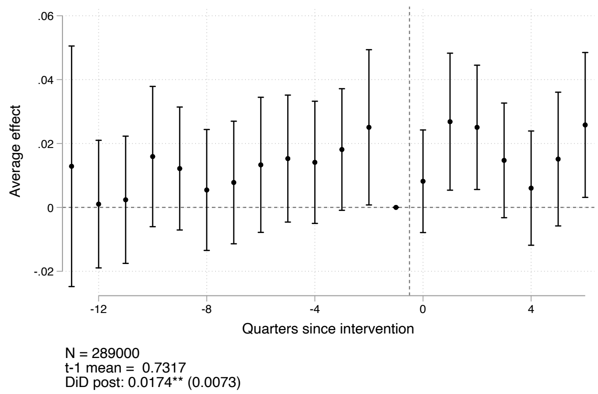
(c) Low birth weight



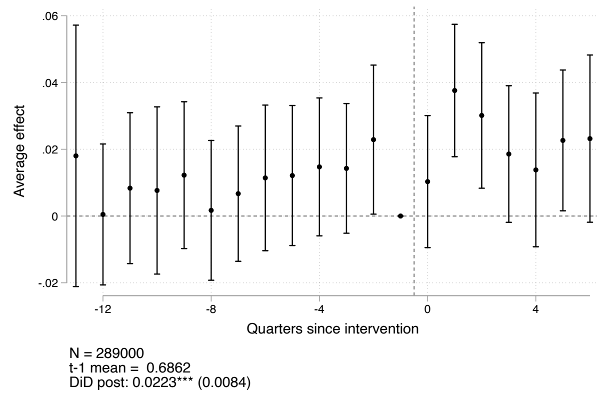
(d) Early term

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3. The outcomes are: (a) a binary indicator for maternal morbidity that equals 1 if any of the following complications are checked on the birth record: blood transfusion, third or fourth degree perineal laceration, ruptured uterus, unplanned hysterectomy, admission to the intensive care unit, and unplanned operating room procedure following delivery, (b) a binary indicator for induction of labor, (c) a binary indicator for low birth weight (less than 2,500 grams), (d) a binary indicator for an early-term delivery (between 37 and 39 weeks gestation). See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

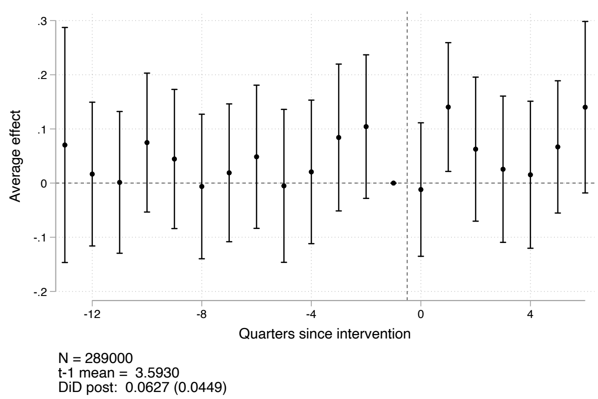
Figure 5: Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes in the Quarter Following Birth



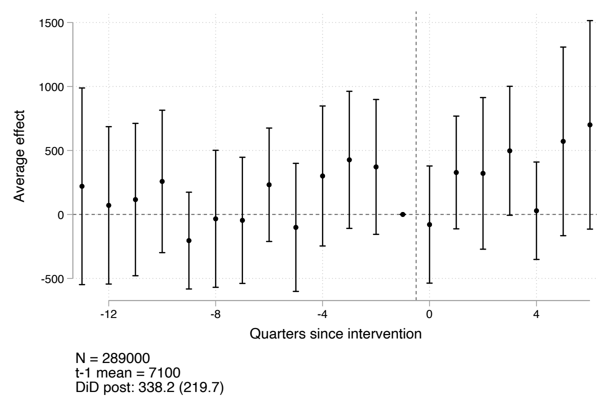
(a) Any positive earnings (return to employment)



(b) Any positive earnings at pre-birth employer



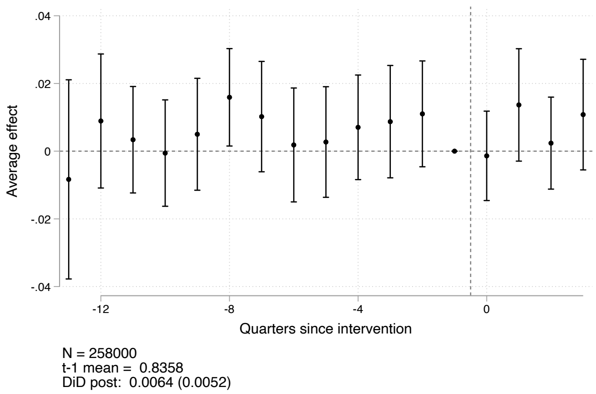
(c) Within-firm earnings decile



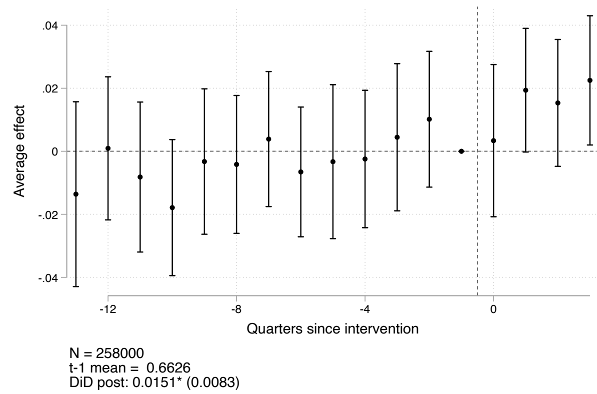
(d) Total earnings (in levels)

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3 to mothers who had positive earnings in the quarter before birth. The outcomes are all measured in the quarter following birth and are: (a) binary indicator for having any positive earnings (i.e., "return to employment"), (b) binary indicator for having any positive earnings in any of one's pre-birth employers (i.e., "return to pre-birth employer"), (c) the earnings decile in one's firm (with the lowest decile assigned to those who have no earnings), and (d) quarterly earnings (including zeros). See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

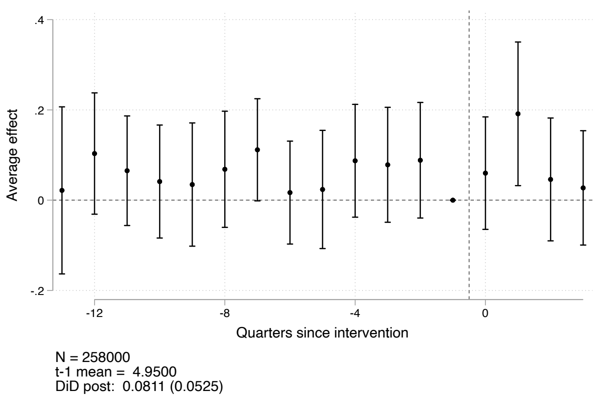
Figure 6: Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes Averaged Over Quarters 3–5 Following Childbirth



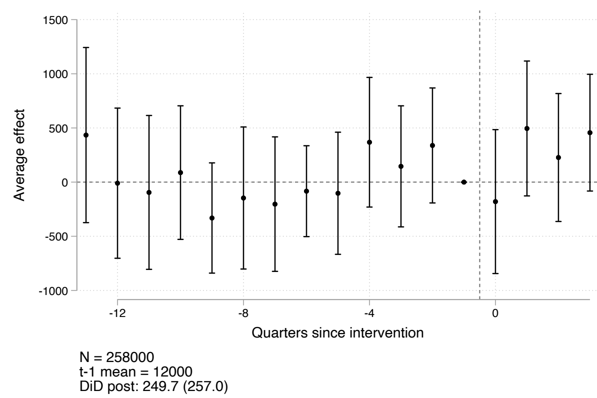
(a) Any positive earnings



(b) Any positive earnings at pre-birth employer



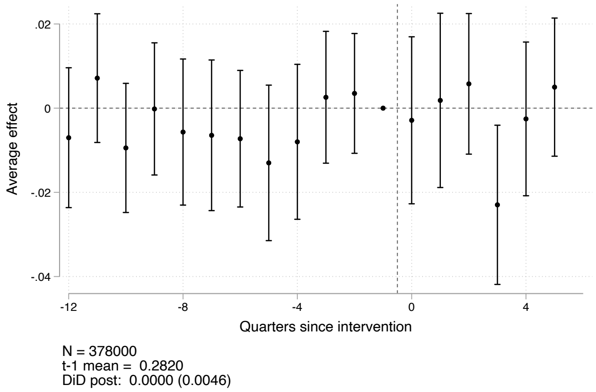
(c) Within-firm earnings decile



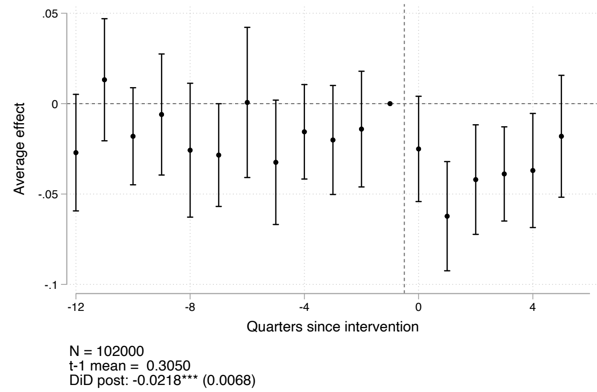
(d) Quarterly earnings (in levels)

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2018q4 to mothers who had positive earnings in the quarter before birth. The outcomes are all measured as averages over quarters 3–5 following birth and are: (a) binary indicator for having any positive earnings (i.e., "employment"), (b) binary indicator for having any positive earnings in any of one's pre-birth employers (i.e., "employment at pre-birth employer"), (c) the earnings decile in one's firm (with the lowest decile assigned to those who have no earnings), and (d) quarterly earnings, including zeros. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435 and CBDRB-FY26-002.

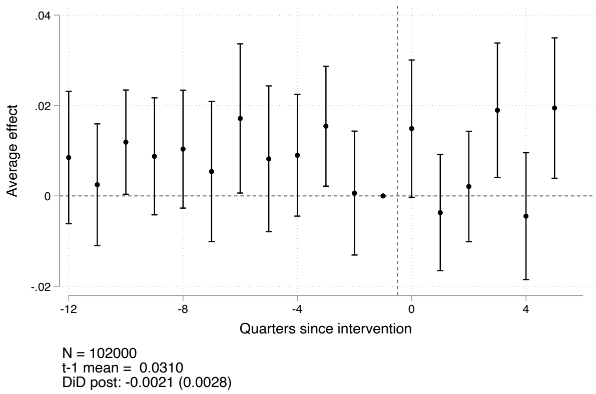
Figure 7: Effects of CMQCC's C-section Intervention on Subsequent Birth and Delivery Outcomes



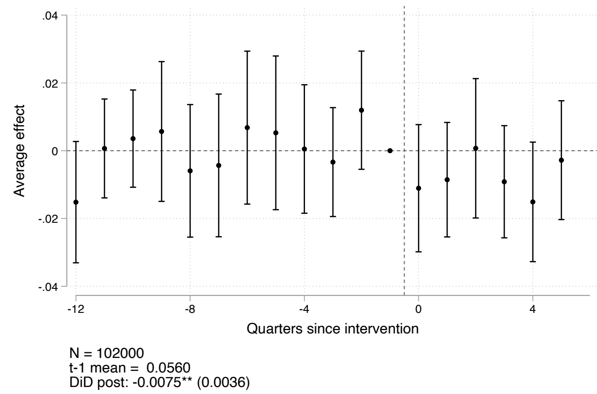
(a) Any subsequent birth



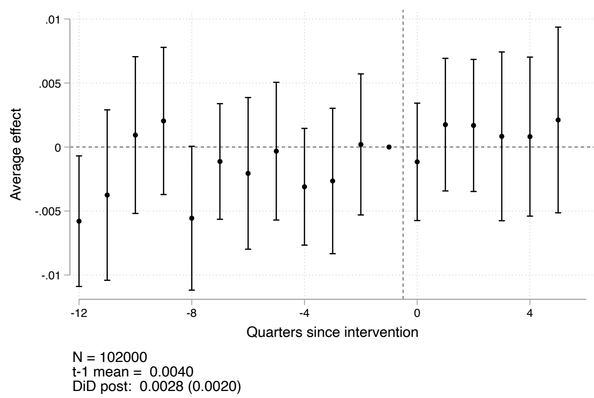
(b) C-section (among subsequent births)



(c) Low birth weight (among subsequent births)



(d) Preterm (among subsequent births)



(e) Maternal morbidity (among subsequent births)

Notes: These figures plot event-study coefficients and 95% confidence intervals from estimating an event-study model that compares Cohort 1 to never treated NTSV births over 2013q2–2017q3. Sub-figure (a) uses all NTSV births in this analytic sample, while sub-figures (b)–(e) restrict the sample to mothers who have a subsequent birth within 10 quarters of the initial focal birth, and measure outcomes at the time of the subsequent birth. The outcomes are: (a) a binary indicator for having any subsequent birth within 10 quarters of the focal birth, (b) a binary indicator for a c-section delivery at the subsequent birth, (c) a binary indicator for the subsequent birth having low birth weight (<2500 grams), (d) a binary indicator for the subsequent birth being delivered preterm (<37 weeks gestation), and (e) a binary indicator for the subsequent birth having any maternal morbidity diagnoses. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Table 1: Means of Maternal Characteristics among NTSV Births

	Cohort 1	Cohort 2	Cohort 3	Not treated
Maternal characteristics				
Under 20 years old	0.06	0.09	0.13	0.15
	(0.23)	(0.29)	(0.34)	(0.36)
20–24 years old	0.16	0.22	0.28	0.32
	(0.37)	(0.42)	(0.45)	(0.47)
25–34 years old	0.57	0.54	0.48	0.44
	(0.49)	(0.50)	(0.50)	(0.50)
35 years old and over	0.21	0.14	0.10	0.08
	(0.41)	(0.35)	(0.30)	(0.28)
Foreign born	0.31	0.32	0.26	0.27
	(0.46)	(0.46)	(0.44)	(0.45)
Hispanic	0.34	0.37	0.48	0.52
	(0.47)	(0.48)	(0.50)	(0.50)
Non-Hispanic Asian	0.20	0.19	0.15	0.13
	(0.40)	(0.39)	(0.35)	(0.34)
Non-Hispanic Black	0.05	0.03	0.06	0.04
	(0.22)	(0.18)	(0.23)	(0.20)
Non-Hispanic White	0.36	0.37	0.28	0.27
	(0.48)	(0.48)	(0.45)	(0.45)
Other race or missing	0.07	0.09	0.05	0.06
	(0.26)	(0.29)	(0.21)	(0.23)
Less than high school	0.05	0.08	0.11	0.12
	(0.21)	(0.27)	(0.32)	(0.33)
High school or some college	0.42	0.46	0.56	0.63
	(0.49)	(0.50)	(0.50)	(0.48)
4 year college or more	0.54	0.46	0.33	0.25
	(0.50)	(0.50)	(0.47)	(0.43)
Missing education	0.04	0.07	0.03	0.03
	(0.19)	(0.26)	(0.16)	(0.18)
Number of sample NTSV births	138,000	168,000	108,000	272,000

Notes: The sample is restricted to nulliparous, term, singleton, vertex (NTSV) births occurring to mothers residing in California over our sample period from 2013q2 to 2019q3. Average maternal characteristics are presented separately for births at hospitals in cohort 1, cohort 2, cohort 3, and the never-treated group. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Table 2: Selected Outcome Means

	Cohort 1	Cohort 2	Cohort 3	Not treated
A. Focal delivery outcomes				
C–section	0.30	0.28	0.31	0.26
	(0.46)	(0.45)	(0.46)	(0.44)
Any maternal morbidity	0.02	0.02	0.01	0.02
	(0.12)	(0.14)	(0.10)	(0.15)
Number of sample NTSV births	138,000	168,000	108,000	272,000
B. Economic outcomes in the quarter following birth				
Total earnings ($)	8537	8432	5931	4925
	(12410)	(12830)	(9216)	(8614)
Earnings decile	3.77	3.61	3.53	3.38
	(3.12)	(2.99)	(3.02)	(3.01)
Employed	0.77	0.76	0.74	0.70
	(0.42)	(0.43)	(0.44)	(0.46)
Employed at pre–birth employer	0.73	0.72	0.69	0.65
	(0.44)	(0.45)	(0.46)	(0.48)
C. Economic outcomes over quarters 3–5 following birth				
Average earnings ($)	13690	13260	9966	8327
	(14290)	(15010)	(11400)	(10620)
Earnings decile	5.07	4.86	4.76	4.58
	(3.28)	(3.20)	(3.20)	(3.18)
Employed	0.84	0.83	0.83	0.81
	(0.36)	(0.37)	(0.38)	(0.39)
Employed at pre–birth employer	0.69	0.67	0.64	0.61
	(0.46)	(0.47)	(0.48)	(0.49)
Number of NTSV births to mothers employed one quarter pre– birth	90500	103000	60500	138000
D. Subsequent birth outcomes within 10 quarters following focal birth				
Any subsequent birth	0.28	0.28	0.28	0.26
	(0.45)	(0.45)	(0.45)	(0.44)
Among the subsequent births within 10 quarters				
C–section	0.29	0.28	0.31	0.26
	(0.45)	(0.45)	(0.46)	(0.44)
Any maternal morbidity	0.01	0.01	0.00	0.01
	(0.08)	(0.08)	(0.06)	(0.10)
Preterm (< 37 weeks)	0.06	0.05	0.06	0.06
	(0.23)	(0.22)	(0.24)	(0.24)
Number of NTSV focal births with subsequent births within 10 quarters	27500	33500	23500	52500

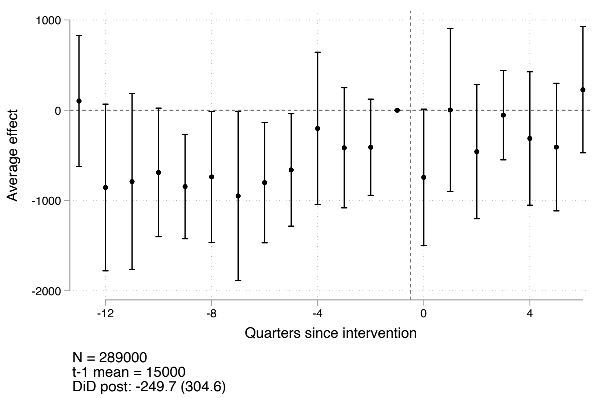
Notes: The sample is restricted to nulliparous, term, singleton, vertex (NTSV) births occurring to mothers residing in California over our sample period from 2013q2 to 2019q3. This table presents means of selected outcomes used in our analysis separately for mothers giving birth in cohort 1, cohort 2, cohort 3, and never-treated hospitals. Panels B and C further restrict the sample to NTSV births to mothers who have positive earnings in the quarter before birth. The first row of Panel D (Any subsequent birth) is averaged over all sample NTSV births from 2013q2–2017q3 while the remaining rows only average over those observations with a subsequent birth within 10 quarters of their focal birth. Any maternal morbidity is a binary indicator for if any of the following were reported: maternal blood transfusion, third or fourth degree perineal laceration, ruptured uterus, unplanned hysterectomy, admission to the ICU, and unplanned operating room procedure following delivery. Sample sizes are rounded according to Census Bureau rounding rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

ONLINE APPENDIX

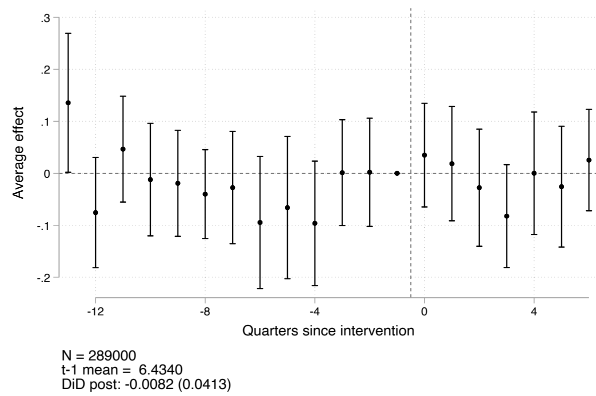
Manuscript Title: The Labor Market and Health Impacts of Reducing Cesarean Section

Author Names: Sarah Miller, Petra Persson, Maya Rossin-Slater, and Laura R. Wherry

Figure A1: The Relationship Between the Timing of CMQCC's C-section Intervention and Maternal Labor Market Outcomes in the Quarter Before Childbirth



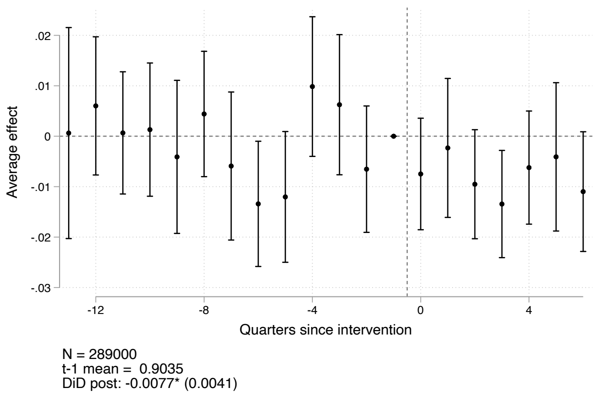
(a) Total earnings in quarter before birth



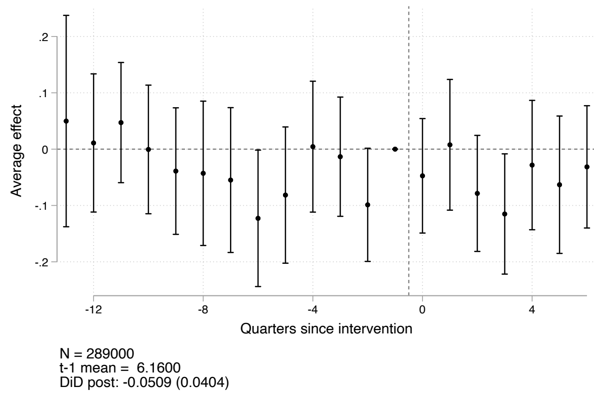
(b) Within-firm earnings decile in quarter before birth

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3 to mothers who had positive earnings in the quarter before birth. The outcomes are: (a) total earnings in the quarter before birth and (b) within-firm earnings decile in the quarter before birth. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

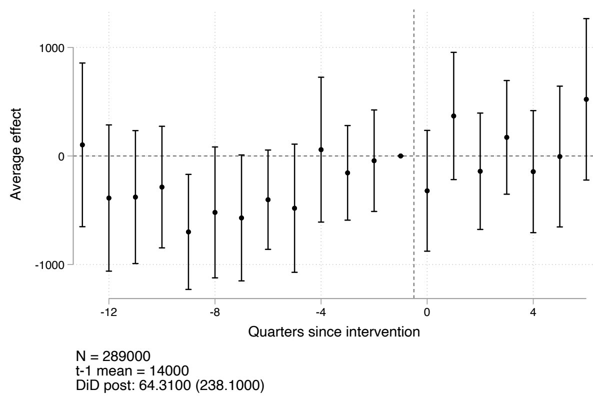
Figure A2: The Relationship Between the Timing of CMQCC's C-section Intervention and Maternal Labor Market Outcomes Averaged Over Quarters 3–5 Before Childbirth



(a) Any positive earnings 3–5 quarters before birth



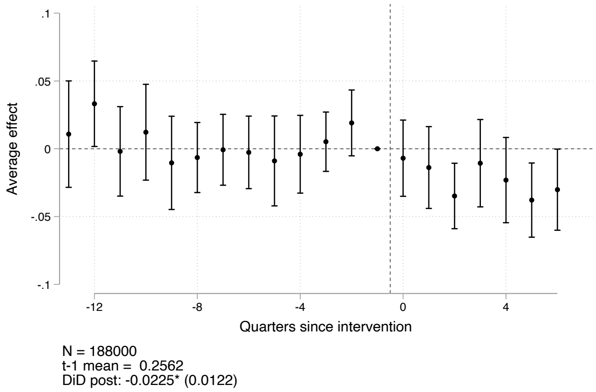
(b) Average earnings decile over quarters 3–5 pre-birth



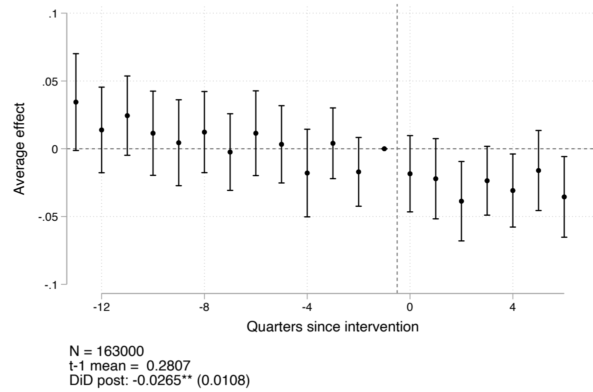
(c) Average quarterly earnings over quarters 3–5 pre-birth

Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3 to mothers who had positive earnings in the quarter before birth. The outcomes are are all measured as averages over quarters 3–5 before birth and are: (a) a binary indicator for having any positive earnings, (b) the earnings decile in one's firm (with the lowest decile assigned to those with no earnings), and (c) quarterly earnings, including zeros. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

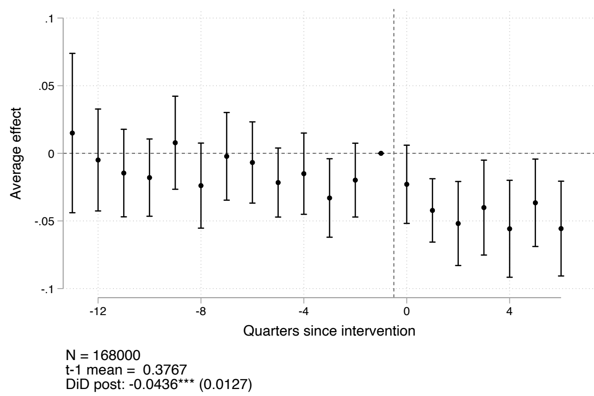
Figure A3: Heterogeneous Effects of CMQCC's C-section Intervention on the Probability of a C-section Delivery by Predicted C-section Risk



(a) Low risk



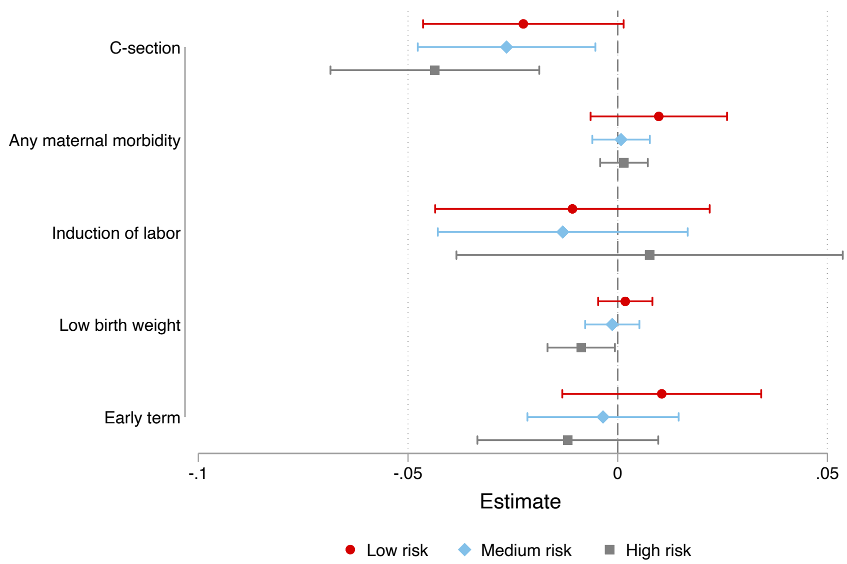
(b) Medium risk



(c) High risk

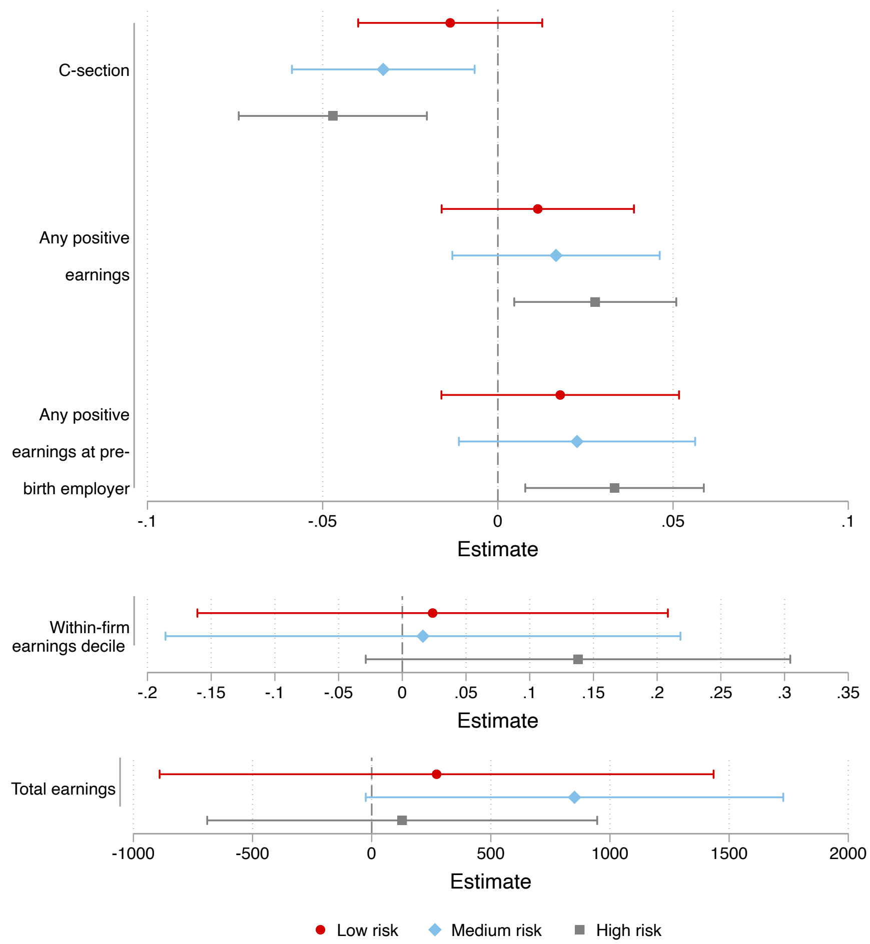
Notes: These figures plot the Callaway and Sant'Anna (2021) event-study coefficients and 95% confidence intervals from estimating equation (1) on Cohort 1, 3, and never treated NTSV births over 2013q2–2019q3. The outcome is a binary indicator for a c-section delivery. The sample of all NTSV births is split into terciles of the predicted risk of c-section in panels (a), (b), and (c), respectively, where the prediction model regresses the c-section indicator on indicators for reported pregnancy complications, maternal age over 40, number of prenatal care visits, and a binary indicator of at least one prenatal care visit. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Figure A4: Heterogeneous Effects of CMQCC's C-section Intervention on Maternal and Infant Health Outcomes at the Focal Delivery by Predicted C-section Risk



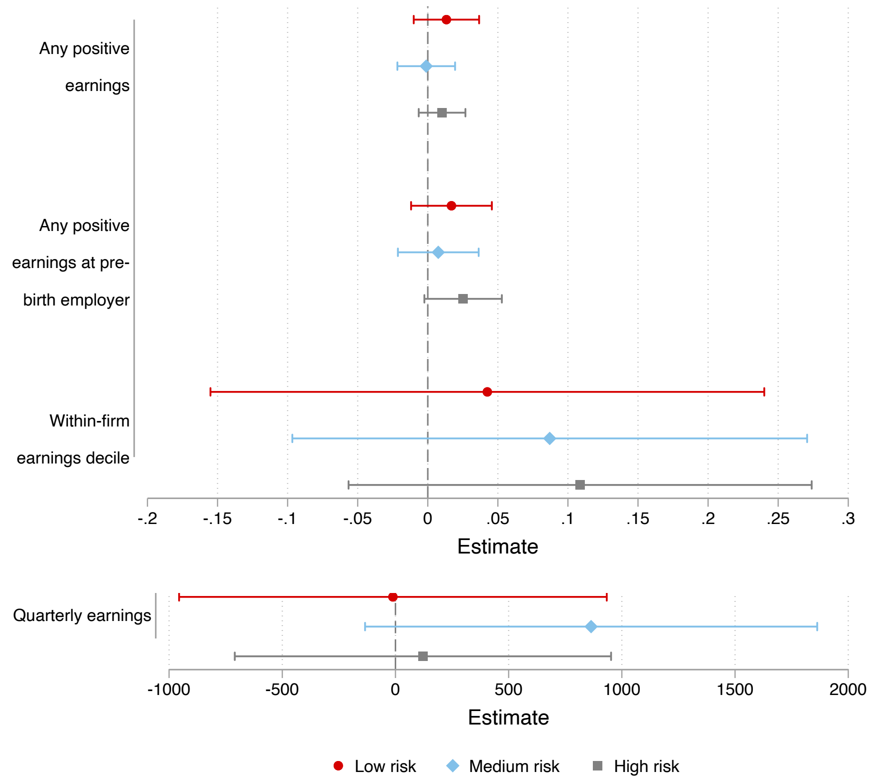
Notes: This figure plots the difference-in-differences coefficients and 95% confidence intervals from our baseline Callaway and Sant'Anna (2021) model in which we compare Cohort 1 and Cohort 3 to never treated NTSV births over 2013q2–2019q3. We separately estimate these regressions for sub-samples based on terciles of the predicted c-section risk. See notes under Figure A3 for details on predictors included in the prediction regression and notes under Figure 4 for more details on the outcomes. The results for the c-section indicator outcome are presented at the top for reference. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Figure A5: Heterogeneous Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes in the Quarter Following Birth by Predicted C-section Risk



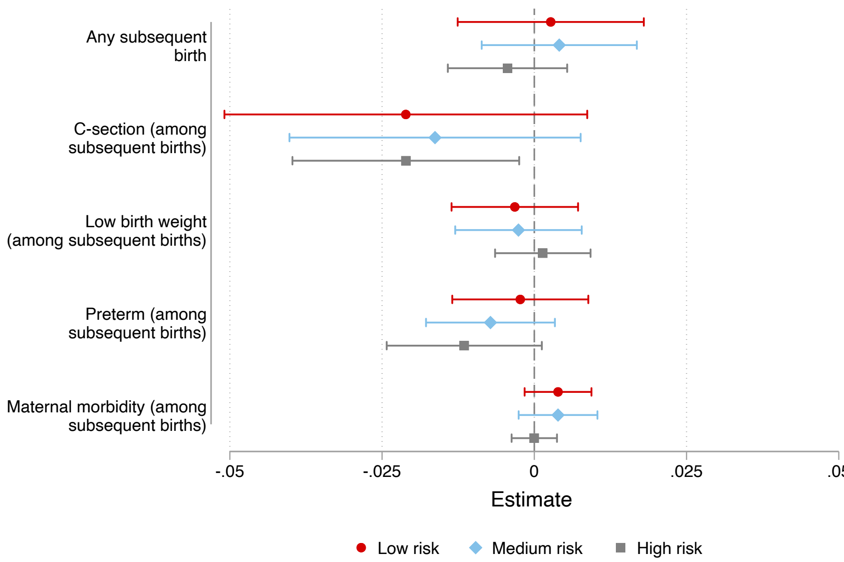
Notes: This figure plots the difference-in-differences coefficients and 95% confidence intervals from our baseline Callaway and Sant'Anna (2021) model in which we compare Cohort 1 and Cohort 3 to never treated NTSV births over 2013q2–2019q3 to mothers who had positive earnings in the quarter before birth. We separately estimate these regressions for sub-samples based on terciles of the predicted c-section risk. See notes under Figure A3 for details on predictors included in the prediction regression and notes under Figure 5 for more details on the outcomes. The results for the c-section indicator outcome estimated on the sample of women with positive earnings in the quarter before birth are presented at the top for reference.See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

Figure A6: Heterogeneous Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes Averaged Over Quarters 3–5 Following Childbirth by Predicted C-section Risk



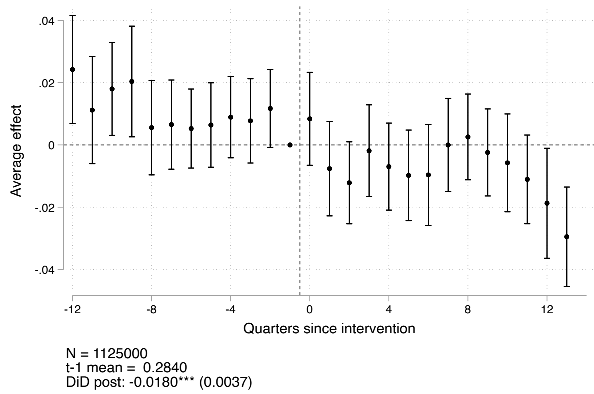
Notes: This figure plots the difference-in-differences coefficients and 95% confidence intervals from our baseline Callaway and Sant'Anna (2021) model in which we compare Cohort 1 and Cohort 3 to never treated NTSV births over 2013q2–2018q4 to mothers who had positive earnings in the quarter before birth. We separately estimate these regressions for sub-samples based on terciles of the predicted c-section risk. See notes under Figure A3 for details on predictors included in the prediction regression and notes under Figure 6 for more details on the outcomes. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-002.

Figure A7: Heterogeneous Effects of CMQCC's C-section Intervention on Subsequent Birth Outcomes by Predicted C-section Risk



Notes: This figure plots difference-in-differences coefficients and 95% confidence intervals from estimating a DD model comparing Cohort 1 to never treated NTSV births over 2013q2–2017q3. The sample of all NTSV births is split in to terciles of predicted risk of c-section. The sample used for the first outcome, any subsequent birth, includes all NTSV births, while for the other outcomes, the sample is restricted to mothers who have a subsequent birth within 10 quarters of the initial focal birth, and measure outcomes at the time of the subsequent birth. See notes under Figure A3 for details on predictors included in the prediction regression and notes under Figure 7 for more details on the outcomes. See notes under Figure 3 for more details about the specifications and controls. Standard errors are clustered at the hospital level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

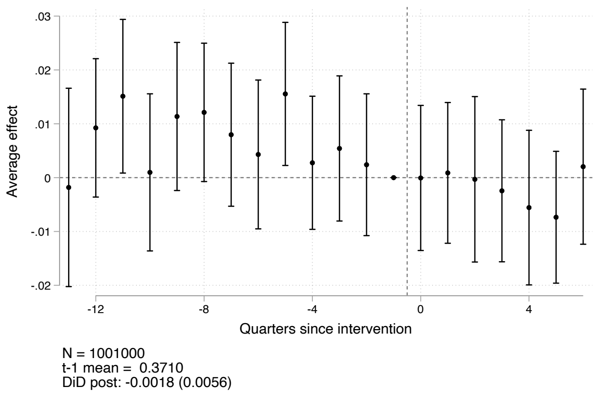
Figure A8: Effects of CMQCC's C-section Intervention on C-sections for all NTSV births



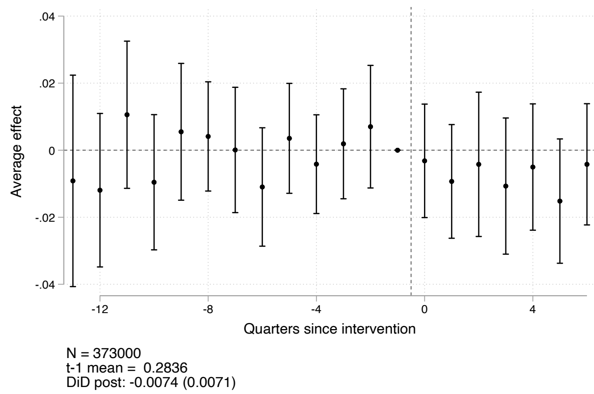
(a) All NTSV births

Notes: This figure plots event-study coefficients and 95% confidence intervals from estimating an event-study model that compares all NTSV births regardless of treatment status to the highest-risk non-NTSV births over 2013q2–2019q3. The outcome is a binary indicator for a c-section delivery. To predict risk, we estimate a prediction model similar to the one described in the notes under Figure A3, and additionally include indicators for birth parity (first, second, or third birth or more) and an indicator for a non-singleton birth. See notes under Figure 3 for more details about the controls included in the event-study model. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.

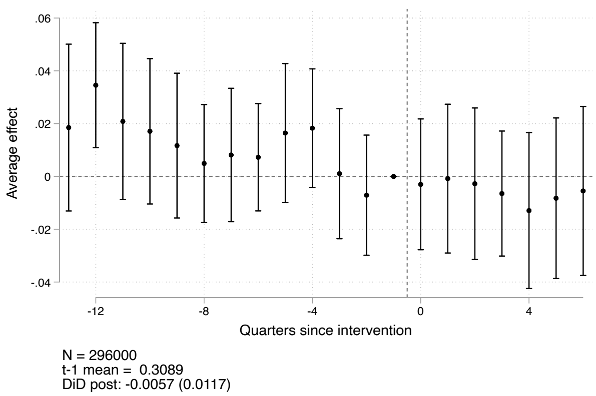
Figure A9: Spillover Effects of CMQCC's C-section Intervention on C-sections for non-NTSV Births



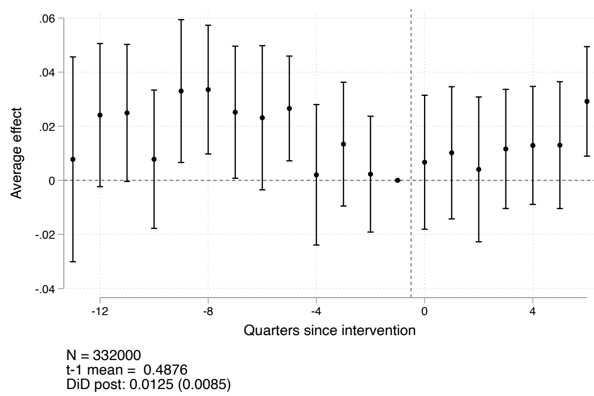
(a) All non-NTSV births



(b) Low risk, non-NTSV births



(c) Medium risk, non-NTSV births



(d) High risk, non-NTSV births

Notes: These figures plot event-study coefficients and 95% confidence intervals from estimating an event-study model that compares non-NTSV births in Cohort 1 and Cohort 3 hospitals to non-NTSV births in never treated hospitals over 2013q2–2019q3. The non-NTSV births are additionally split in to terciles of predicted risk of c-section. To predict risk, we estimate a prediction model similar to the one described in the notes under Figure A3, and additionally include indicators for birth parity (first, second, or third birth or more) and an indicator for a non-singleton birth. See notes under Figure 3 for more details about the controls included in the event-study models. Standard errors are clustered at the hospital level. Sample sizes are rounded to the nearest 1000 per Census Bureau rules. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0435.