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

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Abstract

We study an intervention that reduced cesarean deliveries among low-risk first-time mothers, using California birth records linked to earnings data. Exposed mothers were 8% less likely to have a c-section, with no adverse health effects. We find suggestive evidence that they were more likely to return to their pre-birth employer and had higher within-firm earnings rankings in the quarter post-birth. These labor market gains fade over time. However, mothers who had a second child were less likely to have a c-section or preterm delivery, suggesting our estimated effects from avoiding a first c-section may be lower bounds on total gains.

JEL classification: I14, I15, J13

Keywords: c-section, maternal health, child penalty

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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 goals 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] While the intervention's main goal was supporting vaginal births and reducing unnecessary c-sections, it also could plausibly affect maternal and infant health through other pathways. For example, the program sought to improve team and patient communication and standardize protocols for managing complications during labor. 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 2.3 percentage points (7.9 percent compared to the pre-treatment mean). This estimate is in

[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).

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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 inform other efforts across the country (Agency for Healthcare Research and Quality, 2022).[7] We do not find any statistically significant changes in maternal morbidity,[8] or rates of low birth weight (less than 2,500 grams) or early-term (between 37 and 39 weeks gestation) births—outcomes that could have been affected if physicians altered their protocols regarding elective c-sections.[9] The absence of significant changes in maternal or infant health outcomes is consistent with prior descriptive evidence documenting that hospitals participating in the intervention did not experience any changes in maternal or neonatal safety measures over a two-year post-intervention period (Main et al., 2019).

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 2.6 percentage points (8.5 percent at the pre-treatment mean). In the quarter after birth, we find suggestive evidence that treated mothers have stronger labor market attachment—they are 1.5 percentage points (2.1 percent) more likely to return to any employment, 2.1 percentage points (3.0 percent) more likely to return to their pre-birth employer, and rank 0.8 percentiles (2.1 percent) higher in their within-firm earnings distribution.[10] These results warrant some caution, however: the pre-intervention event-study coefficients are imprecisely estimated, and when we allow for differential linear trends using the Rambachan and Roth (2023) method, the 95% confidence intervals around the treatment effects narrowly include zero. The estimated coefficient for the mother's total earnings in the quarter after birth is also positive, but not statistically significant.

When we consider labor market outcomes over longer follow-up windows after childbirth, the effects become smaller in magnitude and lose statistical significance. Our results indicate that the CMQCC intervention did not significantly change the probability that a mother is employed, her average earnings, or her probability of being employed at a pre-birth employer over the period of

[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.

[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]Survey evidence from CMQCC intervention participants indicates that successful hospitals adopted peer review of all elective c-sections (VanGompel et al., 2021), suggesting these outcomes could plausibly have been affected. We also considered analyzing five- and one-minute APGAR scores as outcomes, but were unable to disclose these estimates in time to meet the journal's publication deadline.

[10]Mothers with zero earnings are assigned to the lowest rank in the within-firm earnings distribution.

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three to five quarters following childbirth. While we still detect an increase in the employer-specific earnings decile three to five quarters after childbirth, it is smaller 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.

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 2.5 percentage point (8.3 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 avoided at the first birth result in at least 30 fewer c-sections in the future.[11] Additionally, we find a 0.9 percentage point (15.2 percent) reduction in the likelihood of a preterm delivery (less than 37 weeks gestation) among subsequent births. This result 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, our analysis of an intervention that aims to lower c-section rates adds to the broader research on the health consequences of c-section deliveries. Correlational analyses tend to find that c-sections are associated with increased risks of long-term health complications for the mother, mostly operating through 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

[11] We estimate this as 100 $\times$ subsequent birth rate in the 10 quarters following initial birth (0.282) $\times$ implied reduction in subsequent c-sections among those with averted first-birth c-sections ($\frac{0.0253}{0.0234} = 1.08$). 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.

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associated with reductions in the likelihood of a low APGAR score and of infant readmission to the hospital during the neonatal period.[12] 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 and 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.[13] Kissel and Roy (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.[14] 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 the effect is increasing in magnitude as predicted risk increases. 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

[12] 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.

[13] 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 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).

[14] 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.

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NTSV births in any of the predicted c-section risk categories, suggesting that the marginal c-sections avoided in our context were likely medically unnecessary.[15] 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.

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 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

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

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is to end "preventable morbidity, mortality and racial disparities in California maternity care."[16] 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 cohorts. 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; 41 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. Figure 2 provides a map of California, with the locations of hospitals in each of the three cohorts shown in different colors (cohort 1 in blue circles, cohort 2 in orange squares, and cohort 3 in green triangles), and with gray hollow circles denoting the never treated hospitals.[17] The map shows that most of the treated hospitals are located in relatively populous areas of the state (in and around Los Angeles and throughout the Bay Area), with cohort 2 hospitals more spread out.

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

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

[17] In the map and in all of the analyses that follow, we exclude 57 of the 204 never treated hospitals at which any deliveries occur during our study period 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.

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Gynecologists' Council on Patient Safety in Women's Health Care, which was established in 2011 and later dissolved in 2021.[18]

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 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 3 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.[19] 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. While we include cohort 2 hospitals in our main empirical analyses, we also show that our results are robust to their exclusion (see Appendix Figures A14–A17). Additionally, we study the broader 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.

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

[19]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).

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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. More information on this process is available in Appendix A of 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 four groups of hospitals: cohort 1, cohort 2, cohort 3, and never treated.[20] All in all, we include births at 237 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). Therefore, 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 included in our sample. Across all study cohorts, the largest share of births occur to mothers aged 25–34, with Hispanic and non-Hispanic white mothers comprising the most prevalent racial and ethnic groups. 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 the never treated hospitals. Maternal characteristics in cohort 2 hospitals 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

[20] As mentioned earlier, we exclude hospitals that ran their own separate interventions.

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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.[21] 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.[22]

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.[23] 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 cohort 2 (28 percent) and 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

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

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

[23] Due to data quality concerns related to over-reporting of third or fourth degree perineal lacerations in one hospital in the never treated group, we exclude perineal laceration outcomes for all births in this hospital.

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early-term birth (between 37 and 39 weeks gestation).[24]

Labor market outcomes. We next examine labor market outcomes for the sub-sample of mothers with positive earnings in the quarter before childbirth (57 percent of our main analysis sample). We measure 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;[25] (3) the decile rank in the employer-specific earnings distribution (where zeros are assigned the lowest ranking);[26] and (4) total earnings (including zeros) in $2019 dollars. We measure return to any employment and return to the pre-birth employer as distinct outcomes, since maintaining an existing employment relationship may help women preserve firm-specific skills that could be important for longer-term labor market trajectories. Loss of such skills has been proposed as a key mechanism behind the "child penalty" women face after motherhood (e.g., Nielsen et al., 2004; Gangl and Ziefle, 2009; Anderson et al., 2003; Casarico and Lattanzio, 2023), and recent empirical work suggests that much of this penalty stems from mothers moving to lower-paying firms (Jack et al., 2025). Panel B of Table 2 shows averages for these outcomes, as well as the share of mothers in this subsample, by hospital group. Approximately three-quarters of women return to employment in the quarter following birth, with the 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 over quarters three, four, and five following delivery (i.e., from nine months to one-and-a-half years later) to capture medium-term maternal labor market outcomes.[27] As seen in Panel C of Table 2, rates of employment exceed 80 percent during this window, implying that some women who are not observed working in the quarter following childbirth do eventually return to employment. 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

[24] 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).

[25] 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.

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

[27] Specifically, we create binary indicators capturing any employment and any employment at the pre-birth employer in quarters three, four or five after delivery, as well as the average within-firm earnings decile and average quarterly earnings over these three quarters. 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 30 below for more details.

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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 low at around or less than one percent, while roughly five to six percent of the subsequent births are preterm.

Lastly, to motivate our empirical design discussed below in Section 4, we present raw trends for each of our outcome variables for each cohort and for the never treated hospitals in Appendix Figures A1–A3, analogous to our raw trend plot for c-section rates in Figure 3.[28]

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, 2, 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:

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

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

[28] In Appendix Figure A4, we do not show the cohort-specific trends in subsequent maternal morbidity due to small cell sizes and Census disclosure rules.

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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.[29] 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.[30] 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 and not-yet treated cohort 3 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).[31] We do not have sufficient follow-up data to study effects on subsequent fertility among mothers who deliver at the cohort 2 and 3 hospitals.

Causal identification of the effects of the CMQCC intervention relies on the assumption that out-

[29] 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 all cohorts can be observed in every event-time period.

[30] 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.

[31] We exclude births in cohort 2 hospitals from this analysis because they are treated partway through cohort 1's post-intervention window and therefore cannot serve as a control group.

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comes among women delivering in the three cohorts of hospitals would have evolved similarly to those among women delivering in the never treated and not-yet-treated hospitals in the absence of the 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. We also implement an analysis proposed by Rambachan and Roth (2023) to assess the sensitivity of our results to possible differential linear trends. 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 additionally 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.

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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. We then discuss some additional heterogeneity analyses, alternative specifications that examine effects on all NTSV births in the state, and spillover effects on non-NTSV births. Lastly, we present our analysis of the sensitivity of our results to differential pre-trends.

C-section deliveries and maternal and infant health outcomes. Figure 4(a) plots the CS event-study estimates of the impact of the CMQCC intervention on c-section rates in our sample of NTSV births. We do not observe any statistically significant event-study coefficients in the pre-period, implying that c-section rates in treated and not treated hospitals evolved similarly before the intervention. That said, although individually not significant, the overall pattern of the pre-period coefficients does indicate a slight downward trend, which could reflect that hospitals that were already making efforts to reduce their c-section rates were more likely to accept an invitation to participate in the intervention. Following the intervention, c-section rates deviate from this pre-existing trend and fall significantly in the

treated hospitals. Specifically, the estimated DD coefficient indicates a 2.3 percentage point reduction in the probability of a c-section delivery, which is a 7.9 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 (2.6 percentage points, or 8.5 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 5(a). We do not find any statistically significant changes in maternal morbidity. That said, the estimated DD coefficient is positive, and the pattern of event-study coefficients indicates an increasing pre-trend that continues into the post period. Also, 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 5(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 5(c) and (d), which could have been impacted if physicians changed their protocols regarding scheduling elective c-sections.[32] The DD estimates for both of these outcomes are statistically insignificant and close to zero. In addition, the direction of estimates is not consistent with a negative coefficient for low birth weight and a positive coefficient for early term. Overall, we interpret these results as showing neither evidence of health benefits nor harm to mothers and their children accompanying the reduction in c-sections.

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. Figures 6(a) and (b) present suggestive evidence that women exposed to the CMQCC intervention are 1.5 percentage points (2.1 percent) more likely to return to employment in the quarter following the birth and 2.1 percentage points (3.0 percent) more likely to return to their pre-birth em-

[32] 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.

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ployer, respectively. However, these effects should be interpreted with caution given we estimate some large and imprecise pre-period event study coefficients, and as discussed further below, are not robust to allowing for differential linear trends using the Rambachan and Roth (2023) approach. We also examine the impact of the intervention on the mother's earnings decile within her firm (capturing her rank among coworkers) in Figure 6(c), finding an increase in her relative earnings decile of 0.08 deciles, or 0.8 percentiles (2.1 percent relative to the pre-treatment mean). The effects on her total earnings in the quarter after birth, shown in Figure 6(d), show some positive coefficients, but a statistically insignificant average effect.

While the reduced form effect of the intervention on maternal employment may appear modest in size, it is large when compared to the approximately 2.6 percentage point change in the c-section rate reported in Figure 4(b), suggesting that labor supply effects among women whose delivery method changed as a result of the program (i.e., the "compliers") could be large. However, as already noted, the CMQCC intervention could have impacted other aspects of healthcare received by women, and we therefore do not estimate an instrumental variables model to interpret our findings as the local average treatment effect of a c-section delivery due to a likely violation of an exclusion restriction in that model.

We also study maternal labor market outcomes observed between nine months and one-and-a-half years (i.e., three to five quarters) following childbirth, which we consider "medium-term" effects, in Figure 7. 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 overall employment or the probability that a mother is employed at her pre-birth employer, we do see a marginally significant increase in average relative earnings decile within mother's firms of 0.07 deciles, or 1.5 percent shown in sub-figure (c). We also observe a positive but not statistically significant effect on the mother's average quarterly earnings in levels over quarters three to five following childbirth, as reported in sub-figure (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 A5 shows that there is no significant relationship between the CMQCC intervention and the mother's total earnings or her earnings

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decile in the quarter before birth. Similarly, in Appendix Figures A6, we find no effects on the mother's employment, average earnings decile, or average quarterly earnings measured 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 and the not-yet-treated cohort 3 in a standard event-study framework.

About 28 percent of mothers in our NTSV sample are observed having another child within this time frame, and Figure 8(a) shows that there is no change in the likelihood of a subsequent birth associated with the intervention. However, Figure 8(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 2.5 percentage points (8.3 percent). Notably, the magnitude of this effect is similar in magnitude as that observed at the initial birth (a 2.3 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 1.3 c-sections in total.[33] 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 8(d). In particular, exposure to the CMQCC intervention at the first birth reduces the probability that a subsequent birth is preterm by 0.9 percentage points, or about 15.2 percent relative to the baseline mean in the

[33] We calculate this as the ratio of treatment effects on c-sections in the initial and subsequent birth ($\frac{0.0253}{0.0234}$) times the probability a second birth is observed (0.282).

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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 also find a marginally significant decrease in the likelihood of subsequent low birth weight birth, though we have less confidence in this finding because the event-study coefficients do not show a clear pattern and are estimated with considerable imprecision. Finally, we do not find significant effects on the prevalence of 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.

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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 certificates data: indicators for various pregnancy complications,[34] 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 A7 presents the event-study graphs for these three sub-groups, using the c-section indicator as the outcome. It appears that the reduction in c-sections exists in all three groups, with effect sizes indicating decreases of between seven and eight percent relative to baseline. In absolute terms, the estimated coefficients are largest for the medium- and high-risk groups, indicating a larger total number of c-sections avoided for these groups. 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

[34] 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.

births in any of the three categories of predicted c-section risk in Appendix Figure A8. 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 A9 and A10, respectively) or for subsequent birth outcomes (Appendix Figure A11). 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.[35] 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.

Appendix Figure A12 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 A13, where we present results from estimating event-study model (1), except we use non-NTSV births in cohort 1, cohort 2, 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 distri-

[35] 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.

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bution 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.

Robustness to differential trends. In some cases, the event-study results discussed above show evidence of either mild but not statistically significant pre-trends (as for the c-section outcome in the full sample of NSTV births in Figure 4(a)), or of pre-period event-study coefficients that are imprecisely estimated with no systematic upward or downward patterns (as for the return to employment and return to the pre-birth employer outcomes in Figures 6(a) and (b), respectively). We therefore assess whether the treatment effects that we find can be explained by a pre-existing linear pre-trend using the method described in Rambachan and Roth (2023). We implement this method using the "smoothness restriction" version, which allows for violations of parallel trends that are approximately linear, to provide updated confidence intervals. Then, the method allows one to further assess robustness to *non-linear* deviations from the differential linear trends.[36]

Appendix Table A1 reports the results from this exercise, using the average value of the post-treatment event-study coefficients as the key parameter. For each of our outcomes, we report the original estimate (i.e., the average of the event-study coefficients over the post-period for Figures 4, 5, 6, 7, and 8), the original 95% confidence interval around this estimate, and the 95% confidence interval that allows for differential linear trends between the treatment and comparison groups after implementing the Rambachan and Roth (2023) method. We find that even if we allow for differential trends, the treatment effect of the CMQCC intervention continues to be statistically significant at the 95% level for the c-section outcome. We also continue to see significant effects on subsequent c-sections and subsequent preterm births. Notably, the effect on the likelihood of a subsequent c-section remains significant at the 5% level even after allowing non-linear deviations in these trends up to the size of about 10 percent of the baseline mean of the variable (not reported in Table A1).

When it comes to our key labor market outcomes, we find that, after allowing for differential

[36]We implement this using the Stata command *honestdid* with the *delta(sd)* option. We assess robustness to differential linear trends by setting $M = 0$ and to non-linear deviations from these trends by selecting values of $M > 0$.

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linear trends, the 95% confidence intervals around the treatment effects of the intervention on return to employment and the pre-birth employer narrowly include zero. Thus, we cannot fully rule out that pre-existing differential trends in these outcomes may be contributing to the results, and view them as more suggestive than the results on initial c-sections, subsequent c-sections, and subsequent preterm births.

Robustness to excluding cohort 2. As discussed in Section 2 and shown in Figure 3, cohort 2 hospitals exhibit a decline in c-section rates in the months preceding their intervention implementation, potentially reflecting indirect effects from the statewide initiative. To ensure that our results are not driven by this cohort, we re-estimate all main specifications excluding cohort 2 hospitals. Appendix Figures A14–A17 show that our findings are robust to this restriction. If anything, the treatment effects on c-sections and maternal labor market outcomes are somewhat larger in magnitude when excluding cohort 2, though these differences in point estimates are not statistically significant. This pattern suggests that the pre-trend in cohort 2 may be attenuating our main estimates, and that our main specification is therefore conservative.

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 suggestive evidence 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, more likely to return to their pre-birth employer, and have a higher earnings ranking within their firm. However, these effects are not robust to different assumptions about the presence of a differential trend using methods described in Rambachan and Roth (2023), so

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they should be interpreted with some caution. Employment effects of exposure to the intervention do not persist over a longer time frame, implying that any benefit from improved retention of firm- or occupation-specific human capital derived from returning to the same employer more quickly may have been too small for us to detect in the long-term average. The main economic benefit from exposure to the c-section reduction program therefore appears to be an accelerated return to work.

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 without adversely impacting the health of their firstborn children, while also sparing these mothers and their families from the downstream costs of repeat c-sections.

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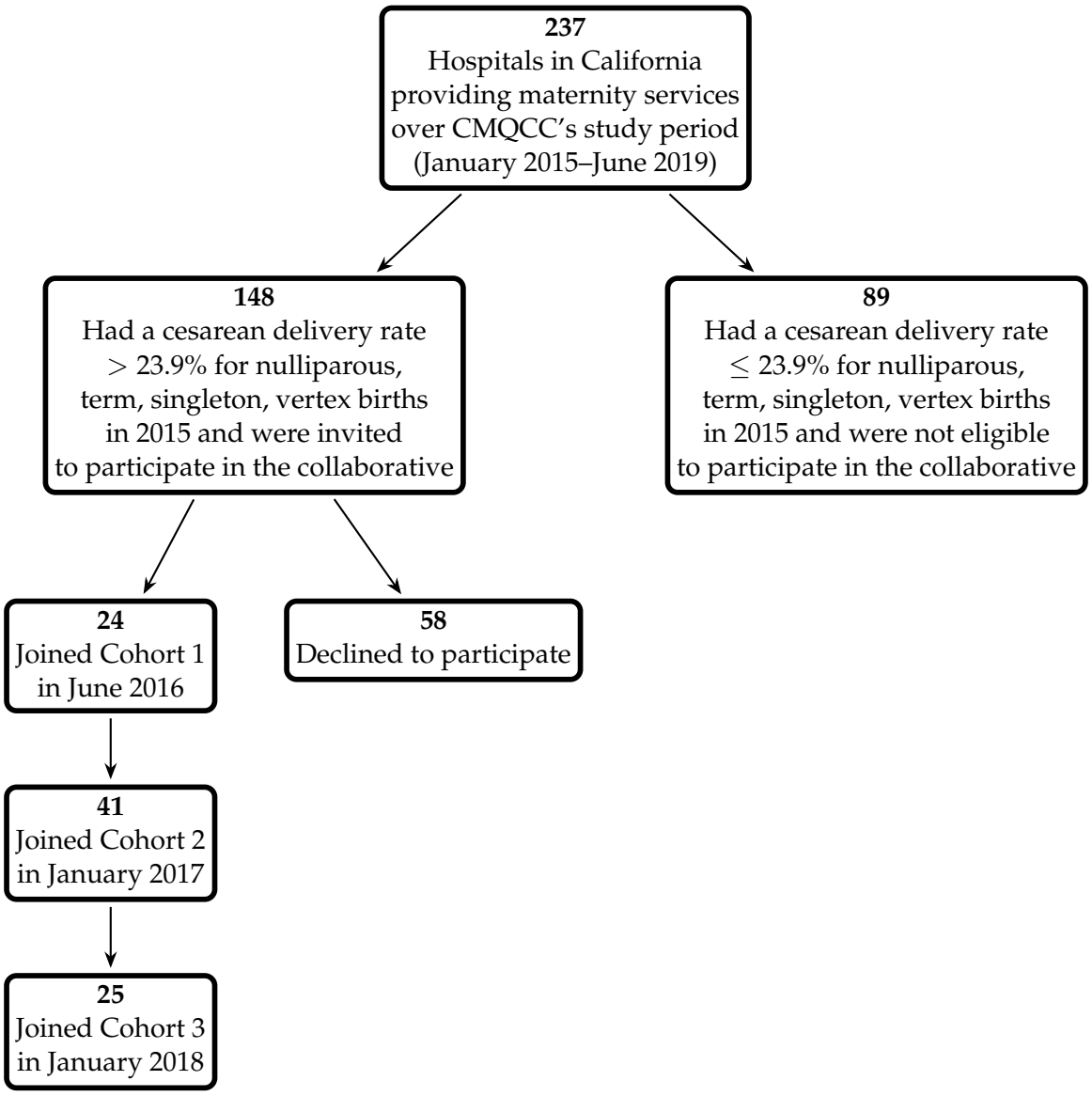
Figure 1: Schematic of California Hospitals Included and Excluded in CMQCC's Collaborative to Support Vaginal Birth and Reduce Primary C-sections



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 based on CMQCC-provided documentation and is similar to Figure 1 in Rosenstein et al. (2021).

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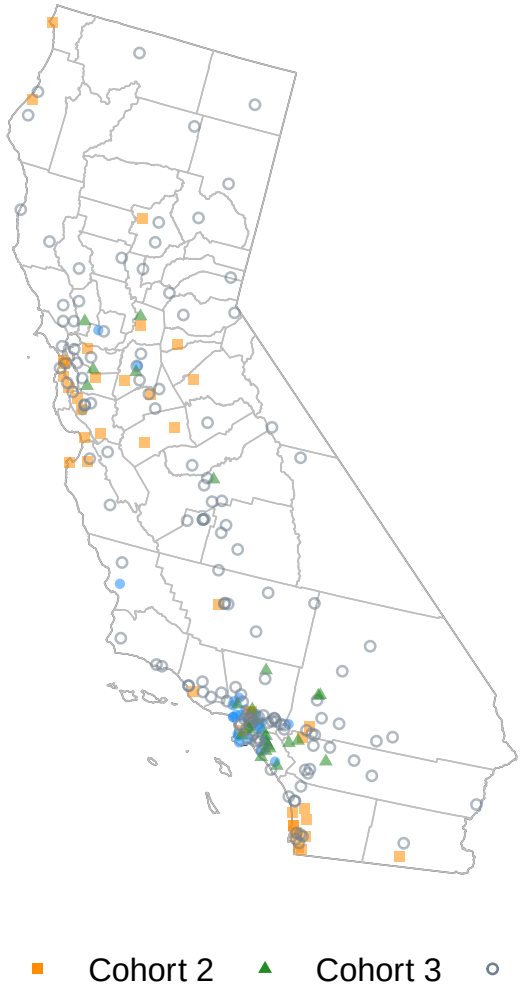
Figure 2: Map of California Hospitals Included in the Analysis



Notes: Map of CA and its counties shows the locations of hospitals included in each of the three cohorts of the CMQCC c-section intervention, along with the never treated group of hospitals that includes both ineligible hospitals and those that declined participation. System and UC hospitals that conducted their own interventions are excluded.

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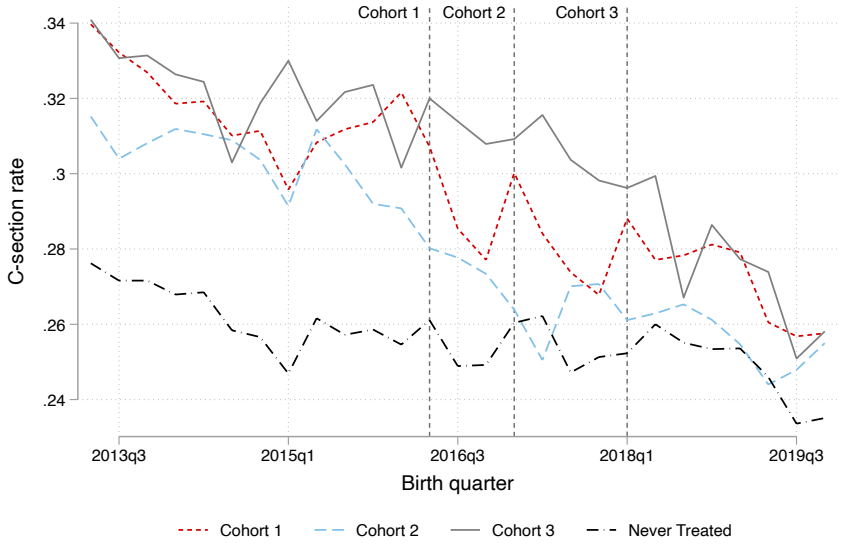
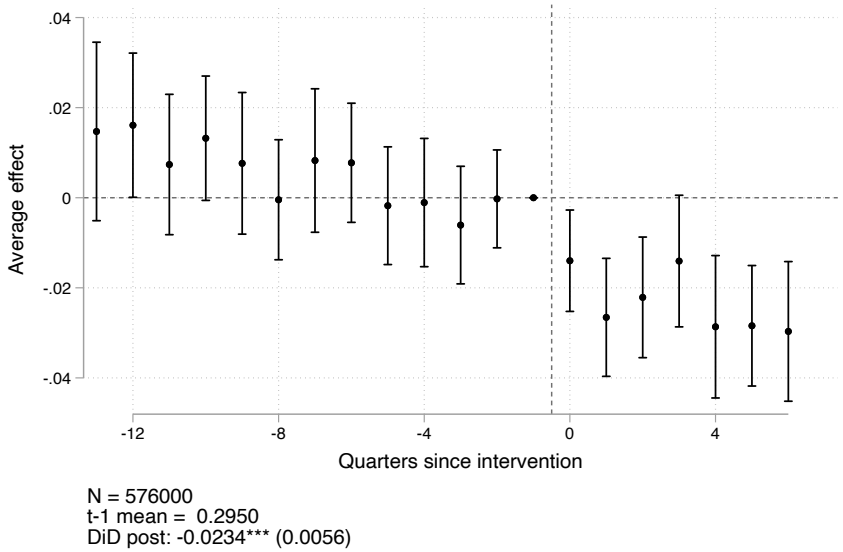
Figure 3: Raw NTSV C-section Rates by Intervention Cohort and Year

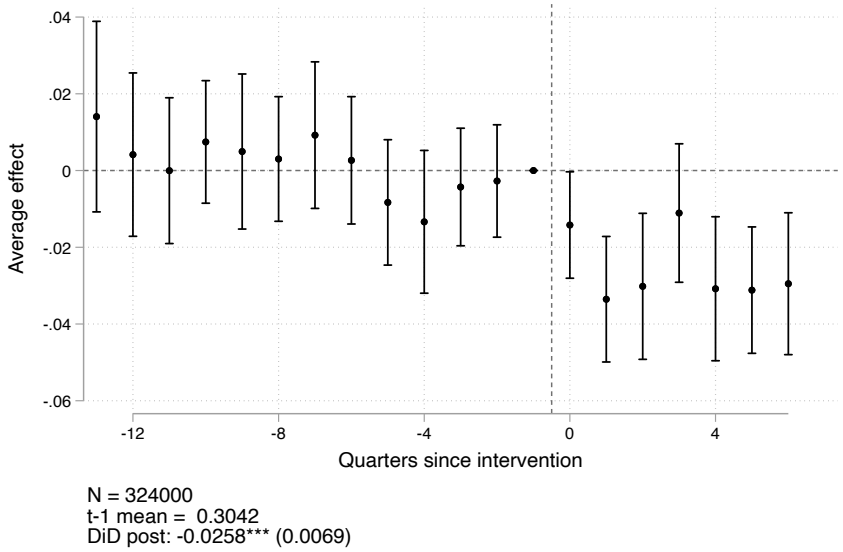


Notes: Figure plots c-section rates among NTSV births by intervention cohort over our sample period, 2013–2019. The never treated group includes both ineligible hospitals and hospitals that declined participation. Vertical dashed lines mark the first treatment quarter for Cohort 1, Cohort 2, and Cohort 3. 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 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, 2, 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-FY26-0178.

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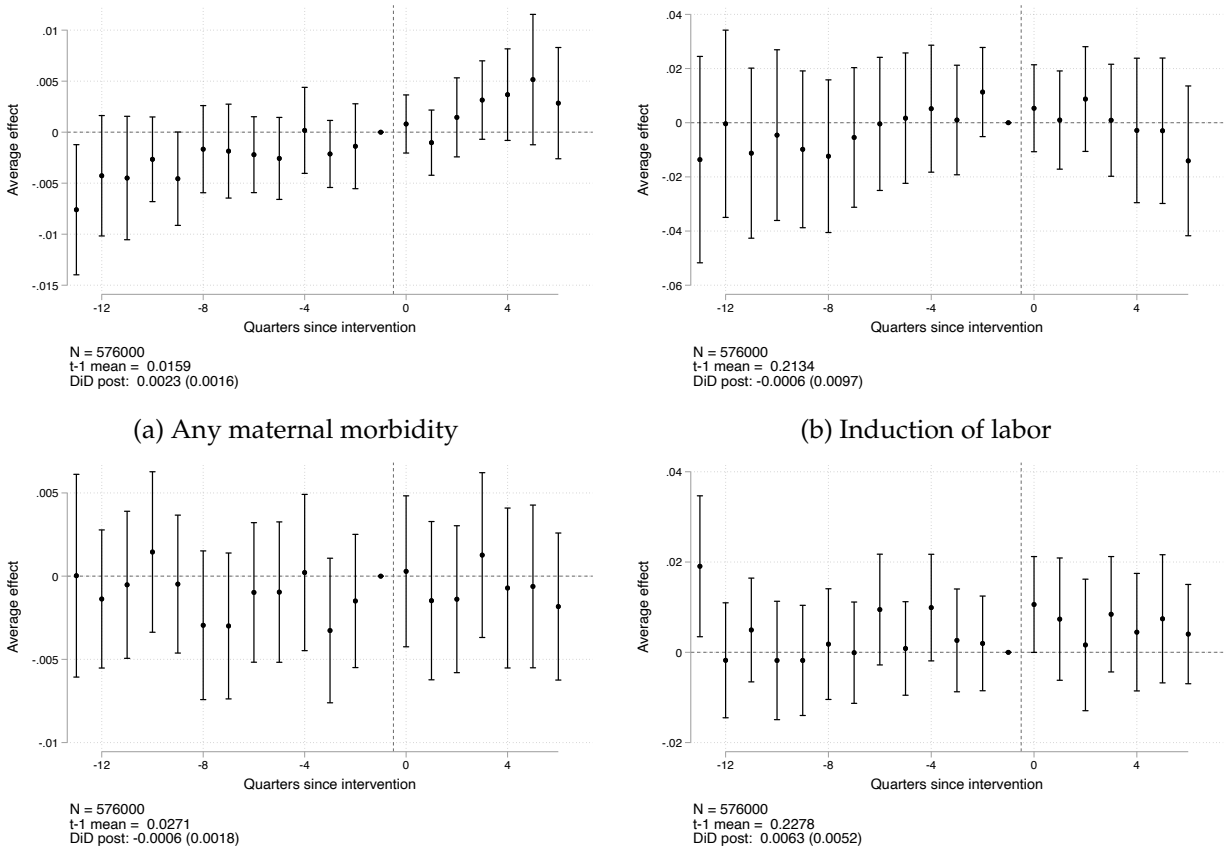
Figure 5: Effects of CMQCC's C-section Intervention on Maternal and Infant Health Outcomes at the Focal Delivery



(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, 2, 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 4 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-FY26-0178.

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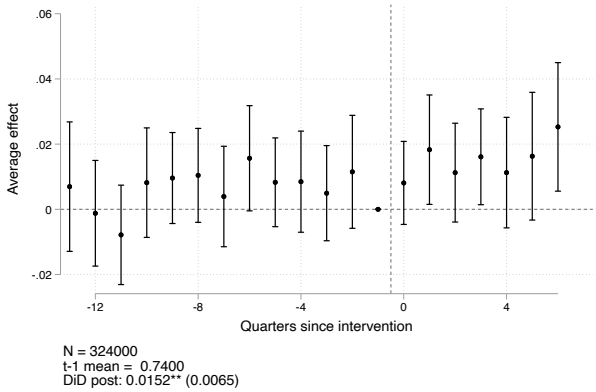
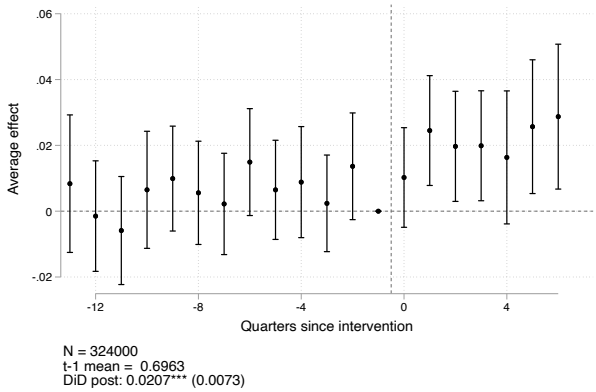
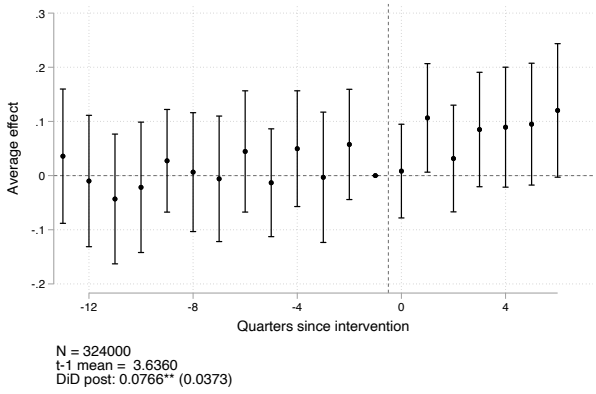
Figure 6: Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes in the Quarter Following Birth



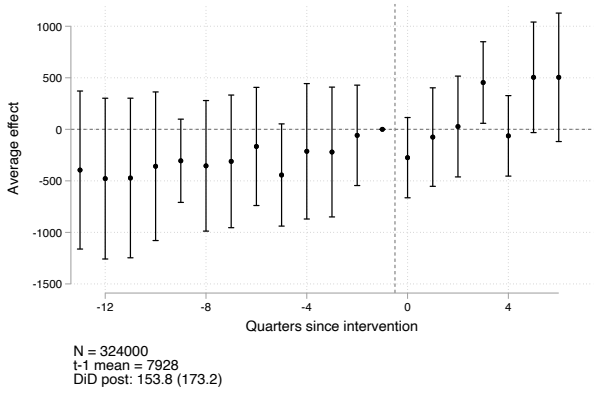
(a) Any positive earnings (return to employment)



(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, 2, 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 4 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-FY26-0178.

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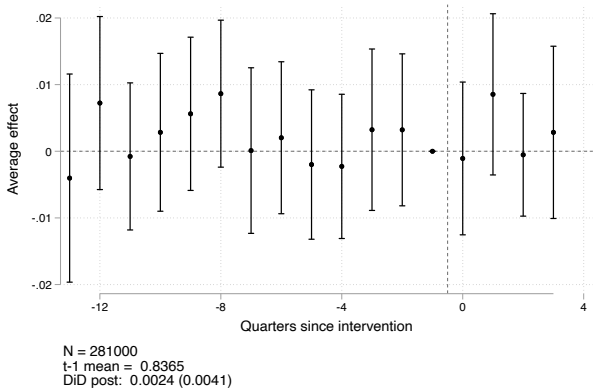
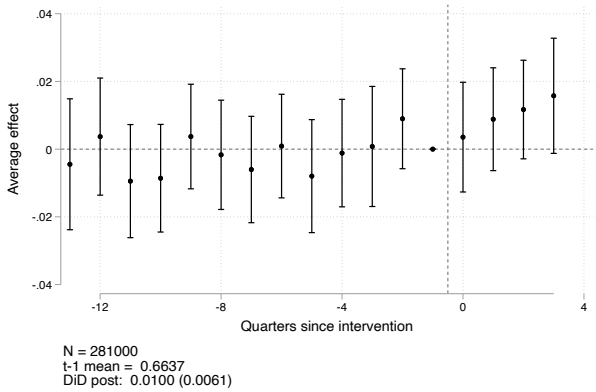
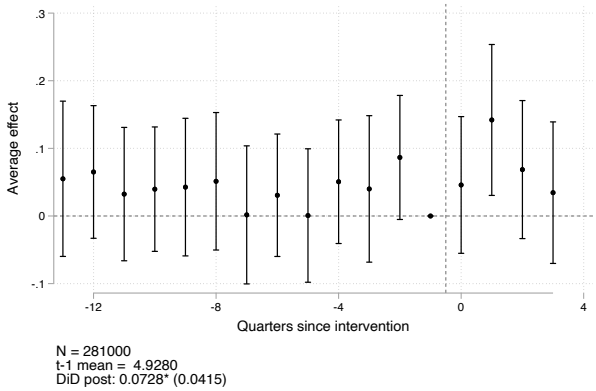
Figure 7: Effects of CMQCC's C-section Intervention on Maternal Labor Market Outcomes in Quarters 3–5 Following Childbirth



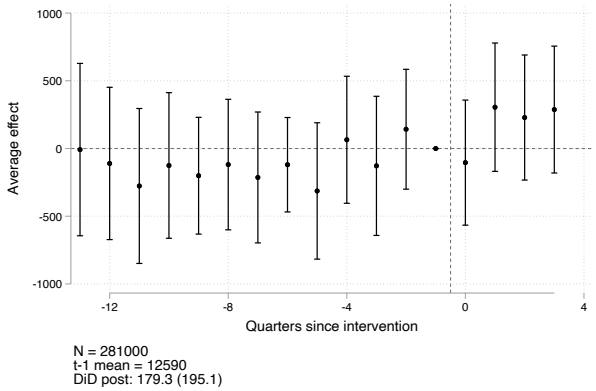
(a) Any positive earnings



(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, 2, 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 in 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 average earnings decile in one's firm (with the lowest decile assigned to those who have no earnings), and (d) average quarterly earnings, including zeros. See notes under Figure 4 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-FY26-0178.

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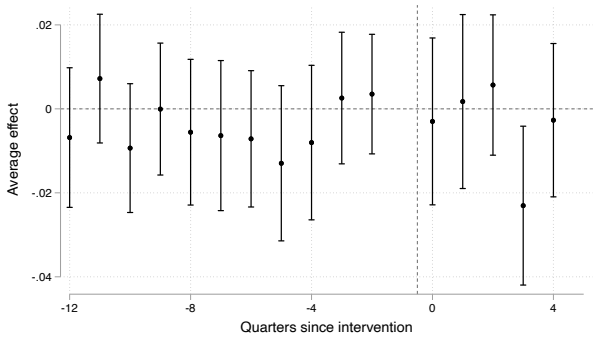
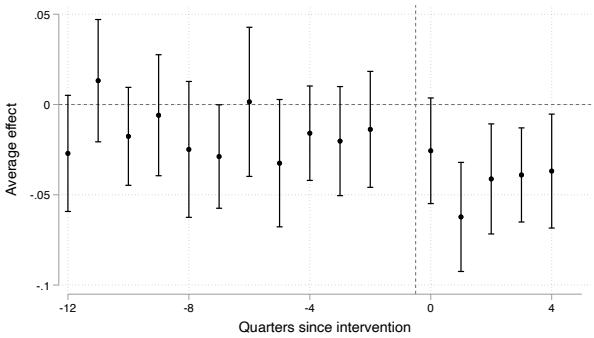
Figure 8: Effects of CMQCC's C-section Intervention on Subsequent Birth and Delivery Outcomes



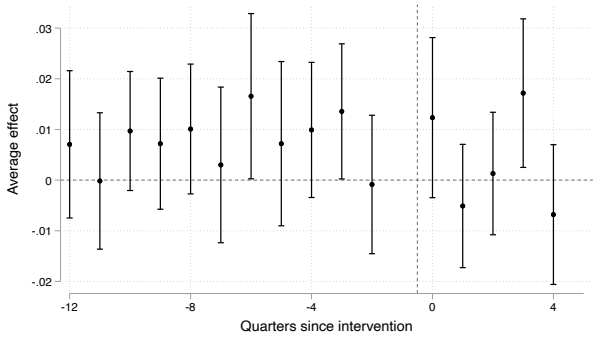
N = 358000
t-1 mean = 0.2820
DiD post: -0.0019 (0.0048)

(a) Any subsequent birth



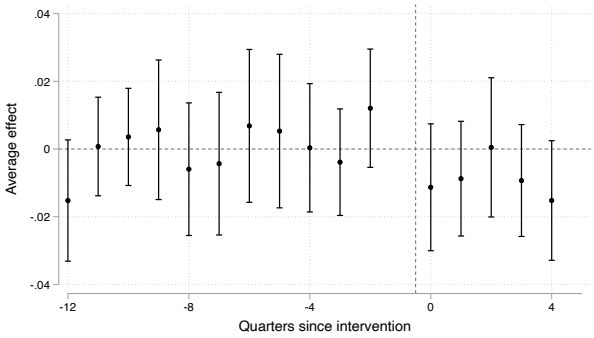
N = 98000
t-1 mean = 0.3050
DiD post: -0.0253*** (0.0069)

(b) C-section (among subsequent births)



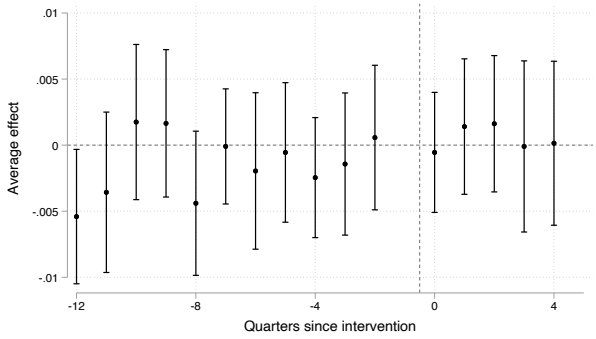
N = 98000
t-1 mean = 0.0310
DiD post: -0.0049* (0.0026)

(c) Low birth weight (among subsequent births)



N = 98000
t-1 mean = 0.0560
DiD post: -0.0085** (0.0035)

(d) Preterm (among subsequent births)



N = 98000
t-1 mean = 0.0040
DiD post: 0.0018 (0.0020)

(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 and not-yet-treated Cohort 3 NTSV births over 2013q2–2017q2. 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 4 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-FY26-0178.

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Table 1: Means of Maternal Characteristics among NTSV Births

	Cohort 1	Cohort 2	Cohort 3	Never 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.

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Table 2: Selected Outcome Means

	Cohort 1	Cohort 2	Cohort 3	Never 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)	(0.13)
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 (%)	90,500 (66%)	103,000 (61%)	60,500 (56%)	138,000 (51%)
D. Subsequent birth outcomes within 10 quarters following focal birth				
Any subsequent birth	0.28	0.28	0.29	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)	(0.09)
Preterm ($<$ 37 weeks)	0.06	0.05	0.06	0.06
	(0.23)	(0.22)	(0.23)	(0.23)
Number of NTSV focal births with subsequent births within 10 quarters	26,000	32,000	21,500	50,500

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 2013q2–2017q2 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 and CBDRB-FY26-0178.

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