The Effect of Hospital Postpartum Support Interventions on Infant Mortality

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Abstract

We study the infant mortality effects of a low-complexity intervention targeting the postpartum hospital stay: state hospital regulations designed to promote breastfeeding. Policy adoption significantly reduced infant mortality by 0.2 deaths per 1,000 live births (3.5%). The mortality declines were concentrated among non-white infants and were particularly large among those who were medically vulnerable. We also find policy adoption increased breastfeeding initiation and duration and improved infant sleep practices. The evidence suggests that both increased breast milk consumption and broader household behavioral responses played a role in reducing mortality.

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1 Introduction

Infant mortality rates in the United States are persistently higher than in other comparably developed countries. In 2021, the U.S. infant mortality rate was 5.4 deaths per 1,000 live births—translating to more than 20,500 infant deaths—compared with an OECD average of 4.1 deaths per 1,000 live births (Gunja et al., 2023). Substantial disparities also exist within the United States, with Black infants more than twice as likely as white infants to die in the first year of life (Centers for Disease Control and Prevention, 2024). Although reducing infant mortality has long been a policy priority, the drivers of the U.S. disadvantage are not well understood (Chen et al., 2016), and there is limited evidence on which policy levers can meaningfully improve infant health.

In this paper, we provide novel evidence of the infant mortality effects of a low-complexity hospital-based policy intervention: state regulations designed to promote breastfeeding during the postpartum hospital stay. These regulations involve providing breastfeeding-related information and support to mothers during their delivery hospitalization. Although the specifics of the policies vary, states frequently require that there be a lactation consultant on staff, that mothers be informed of the benefits of breastfeeding, and that hospital staff be regularly trained on initiation and support of lactation. Over the past several decades, these regulations have become increasingly common, with 16 states adopting them as of 2022.

These policies may impact infant mortality through two broad channels: increased breastfeeding and changes in other household behaviors. First, greater consumption of breast milk may have health benefits through nutritional changes and the transfer of maternal antibodies. Second, the policies may alter parental behaviors beyond breastfeeding. Additional in-hospital support likely increases parental exposure to information about recommended infant-care practices, while breastfeeding itself may shift parental preferences over other dimensions of infant care. As a result, households may change behaviors such as childcare provision or infant sleep practices, which in turn can affect infant health. For example, earlier work has shown that these policies increased maternal time spent on child care (Lawler and Yewell, 2023), which may alter exposure to infectious disease or injury risk (Currie and Hotz, 2004). Similarly, safe infant sleep practices have been shown to reduce

the risk of Sudden Infant Death Syndrome (SIDS) and accidental suffocation (Altindağ et al., 2024).

To estimate the effects of these state hospital breastfeeding support regulations on infant mortality, we use the restricted National Vital Statistics System's linked birth and infant mortality files and leverage plausibly exogenous variation in state policy adoption. As our empirical setting features staggered policy adoption and heterogeneous state policies, we provide estimates from both the Callaway and Sant'Anna (2021) (CS) and synthetic difference-in-differences (SDID) estimators. Our results show that policy adoption reduced infant mortality, with improvements concentrated among infants born to non-white mothers. Following the implementation of these policies, mortality in the first year of life significantly declined by 0.22–0.23 deaths per 1,000 births, about a 3.5% reduction relative to the year before policy adoption. For infants of non-white mothers, one-year mortality significantly declined by 0.43–0.46 deaths per 1,000 births, a 6.0–6.4% decline. We detect no statistically significant reductions among infants born to white mothers.

We next explore suggestive mechanisms through which these policies improve infant health, using a range of administrative and survey datasets. Analyses by health at birth and underlying cause of death show that mortality improvements were particularly large among infants born prematurely and due to causes originating in the perinatal period (e.g., disorders related to short gestation and low birth weight). These findings are consistent with evidence that breast milk supports immune development, with larger expected benefits for preterm infants given their immune systems are relatively less developed at birth (Ballard and Morrow, 2013; Jakaitis and Denning, 2014). Complementing these estimates, using Healthcare Cost and Utilization Project (HCUP) inpatient discharge records, we find suggestive evidence that policy adoption led to declines in digestive-related infant hospitalizations and charges, as well as reductions in charges due to immune- and perinatal-related causes. Together, these results suggest that breast milk's documented benefits for digestive health and immune system development may play an important role in explaining the mortality reductions.

Our results also point to the importance of broader household behavioral responses. We find significant declines in mortality due to external and sleep-related causes, often referred to as

Sudden Unexpected Infant Death (SUID),[1] as well as declines in infant hospitalization charges due to external causes. These findings suggest that channels beyond the direct health benefits of breast milk also contribute to the observed reductions in infant mortality.

We then investigate whether the policies changed breastfeeding and infant sleep practices using self-reported measures from the restricted-use National Immunization Survey-Child (NIS-Child) and the CDC's Pregnancy Risk Assessment Monitoring System (PRAMS), respectively. Our results show that the hospital breastfeeding support regulations substantially increased breastfeeding, with the largest and most persistent effects occurring among non-white infants. After policy adoption, the share of mothers of non-white infants reporting that they initiated breastfeeding increased by 4.3–6.1 percentage points, a 5.5–7.9% increase relative to the pre-treatment mean, while the share reporting that they breastfed at 3 months increased by 5.3–7.7 percentage points (8.7–12.7% increases). Effects among white infants are smaller in both absolute and relative terms, ranging from 0.9–3.3 percentage point increases (1.4–4.1% increases). These heterogeneous breastfeeding effects mirror the pattern of results observed in the mortality data and are consistent with policy impacts being concentrated among non-white infants.

Finally, our analyses suggest that infant sleep practices improved following policy adoption, consistent with additional in-hospital support increasing mothers' exposure to recommended behaviors. The largest improvements are observed among infants of non-white mothers. For this subgroup, we find a robust 2.6 percentage point increase in the probability of back sleeping (as is recommended). We find no evidence of changes in usual sleep position for infants of white mothers. For infants of both white and non-white mothers, we find suggestive evidence that they are more likely to never bed-share, although statistical significance is sensitive to the inference procedure. Combined with prior evidence that these policies increased maternal time spent on

[1]External-cause mortality and SUID are not mutually exclusive categories. External-cause mortality includes accidental suffocation and strangulation in bed, motor vehicle crashes, drowning, poisoning, and other trauma, with deaths due to accidental suffocation and strangulation in bed making up the largest share of this category during our sample period. SUID is defined as mortality due to Sudden Infant Death Syndrome (SIDS), accidental suffocation and strangulation in bed, and other ill-defined and unspecified causes of mortality, following the literature (e.g., Li et al., 2022; Moon et al., 2022).

child care (Lawler and Yewell, 2023),[2] our findings suggest that they changed parental investments along multiple dimensions. Together with our analyses by cause of death, the results point to both the direct effects of breast milk and broader changes in household behaviors as potential mechanisms underlying the declines in infant mortality.

Our paper contributes to two strands of literature. The first is the literature on the causal effects of public policies and the early childhood environment on infant health. Much of this literature has examined the effects of more intensive early-life interventions such as family leave (e.g., Ruhm, 2000; Tanaka, 2005; Baker and Milligan, 2008; Rossin, 2011; Stearns, 2015; Beuchert et al., 2016), nurse home-visiting programs (e.g., Wüst, 2012; Moehling and Thomasson, 2014; Bhalotra et al., 2017; Altindağ et al., 2024),[3] or Medicaid access (Goodman-Bacon, 2018). In contrast, the literature examining the health effects of interventions during the initial delivery hospital stay is sparse and has primarily focused on the returns to medical care (e.g., Almond et al., 2010; Miller and Tucker, 2011; Bharadwaj et al., 2013; Jensen and Wüst, 2015). Our results show that low-complexity information and support-based interventions during the initial postpartum hospital stay can generate meaningful improvements in infant health. This finding is in line with Hirani et al. (2025), which demonstrates that nurse home visits during the first several months of life have greater infant health benefits than visits later in the first year. The timing of early-life policy interventions may be particularly important for investments in which initiation is either time-sensitive (e.g., breastfeeding)[4] or for which the greatest returns are realized in the first weeks or months of life (e.g., safe sleep practices).

This paper also contributes to the literature on the causal effects of breastfeeding-support interventions on breastfeeding and child well-being. The limited experimental evidence largely comes from the Promotion of Breastfeeding Intervention Trial (PROBIT) conducted in Belarus in the 1990s, where breastfeeding support and encouragement from hospital staff was randomized across 31 hospitals. The intervention increased exclusive breastfeeding and breastfeeding duration,

[2]Notably, the changes in maternal time use documented in Lawler and Yewell (2023) were largest among non-white mothers.

[3]Particularly relevant is the finding in Altindağ et al. (2024) that information on safe infant sleep practices, conveyed to households during nurse home visits in Denmark, led to significant reductions in infant mortality.

[4]Early initiation of breastfeeding is important because if milk is not removed from the breasts soon after birth then biological mechanisms cause the cells to stop producing milk (Neville and Morton, 2001; Hurst, 2007).

and infants of treated mothers had fewer gastrointestinal infections, lower rates of skin rashes, and higher weight-for-age (Kramer et al., 2001, 2007a,b, 2008; Brenøe et al., 2026). However, there was no evidence of benefits across other outcomes, including infant mortality, respiratory infections during the first year of life, allergies, asthma, height, body mass index (BMI), and blood pressure in early childhood, measures of child behavior, cognitive development, and mother-child bonding (Kramer et al., 2001, 2007a,b, 2008; Oken et al., 2013; Yang et al., 2018).[5]

Although PROBIT was a high-quality study, effects from Belarus may not generalize to a developed country context for a number of reasons, including differences in the availability of alternatives to breast milk, water quality, maternity leave policies, and the childcare environment.[6] Furthermore, the results from the PROBIT study are based on a sample of full-term infants (born at a gestational age of at least 37 weeks) who weighed at least 2500 grams and whose healthy mothers expressed an intention to breastfeed.

Within this strand of literature, two recent studies exploit quasi-experimental variation in breastfeeding support in the United Kingdom and examine impacts on breastfeeding and child development. Fitzsimons and Vera-Hernández (2022) leverage variation in access to hospital lactation support generated by staff scheduling. They focus on children of low-educated mothers and find large effects of lactation support on breastfeeding and children's cognitive development, but no effects on maternal-reported measures of children's health.[7] Del Bono and Rabe (2012) exploit distance from

[5]In addition to PROBIT, a number of small-scale randomized controlled trials examine more limited dimensions of infant health. These studies leverage random assignment to either (1) peer counselors providing breastfeeding support (Anderson et al., 2005), (2) recommendations on the timing of introduction of complementary foods (Cohen et al., 1994, 1995; Dewey et al., 1999; Jonsdottir et al., 2012), or (3) for low birth weight or preterm infants, in-hospital feeding of either donor breast milk or formula (see Colaizy et al. (2024) and the review in Quigley et al., 2019). Results generally align with those from PROBIT: among those in treatment arms promoting (exclusive) breastfeeding or breast milk consumption, there is evidence of reductions in gastrointestinal conditions (Anderson et al., 2005; Quigley et al., 2019; Colaizy et al., 2024), and limited to no evidence of changes in other dimensions, including infant mortality (Quigley et al., 2019; Colaizy et al., 2024) and ear infections (Anderson et al., 2005). Evidence on weight gain and infant growth is mixed (Cohen et al., 1994, 1995; Dewey et al., 1999; Jonsdottir et al., 2012; Quigley et al., 2019; Colaizy et al., 2024). Two studies additionally examine infant iron status, and find that introducing complementary foods at 4 versus 6 months of age decreases incidence of iron deficiency (Dewey et al., 1998; Jonsdottir et al., 2012).

[6]In Belarus during the period of the PROBIT study, the price of infant formula was high and the primary alternative to breast milk was water or juice (Brenøe et al., 2026). Additionally, mothers had three years of maternity leave.

[7]Fitzsimons and Vera-Hernández (2022) observe children's health at ages 9 months as well as 3, 5, and 7 years old. They combine seven indicators of maternal-reported child health including asthma, hay fever, eczema, wheezing, ear infections, obesity, and long-standing conditions.

the mother's residence to the closest hospital that implemented the Baby-Friendly Hospital Initiative (BFHI) program, which outlines best practices for breastfeeding support.[8] They likewise find that increased exposure to breastfeeding support policies improves child cognitive development, but has no significant impact on a similar set of maternal-reported child health outcomes. Lawler and Yewell (2023) study the same state hospital breastfeeding support regulations we do and find policy adoption increased breastfeeding rates and altered maternal time allocation, with mothers spending more time on child care and less time on formal work. They do not consider effects on infant health.[9]

We make several contributions to this strand of literature on in-hospital breastfeeding support. First, we consider policy effects on a comprehensive set of administrative measures of infant health, including infant mortality and infant hospitalizations. Beyond being important measures of child well-being, these outcomes do not suffer from the measurement error concerns associated with maternal-reported child health measures. Moreover, the large scale of our administrative data provides greater statistical power than prior studies. This allows us to detect small but economically meaningful effects on rare outcomes like infant mortality and to estimate effects for policy-relevant subpopulations. Second, unlike the PROBIT study, which excluded premature and low birth weight infants, we include medically vulnerable infants. In fact, this group experiences particularly large mortality improvements. Third, while much of this literature has focused on the effects of in-hospital breastfeeding support on breastfeeding behavior, we build on Lawler and Yewell (2023)'s findings that state hospital breastfeeding regulations affect maternal time use, and provide further evidence that these interventions alter broader household behaviors, including infant sleep practices, which may independently affect infant health.

The rest of the paper proceeds as follows: Section 2 provides background information on the hospital postpartum support policies we study, as well as discussion of the potential channels linking the policies and infant health. In Sections 3 and 4, we describe our data sources and

[8] We provide more details about the BFHI program in Appendix Section B.

[9] Other studies exploit within-mother (i.e., between-sibling) variation in breastfeeding and find that the breastfed sibling has lower risk of child disability (Wehby, 2014) and increased cognitive skill and educational achievement (Evenhouse and Reilly, 2005; Rees and Sabia, 2009). Furthermore, Haider et al. (2014) exploit quasi-random enrollment in peer counselor breastfeeding support in Michigan generated by excess demand for services. In their sample of about 850 low-income women, they find program participation reduced the share of infants with gastrointestinal disorders.

empirical strategy, respectively. The infant mortality results are presented in Section 5; we shed light on suggestive mechanisms in Section 6. Finally, Section 7 concludes.

2 Background

2.1 State Hospital Breastfeeding Support Policies

Both the World Health Organization (2011) and American Academy of Pediatrics (2012) recommend that, unless medically contraindicated, infants should be exclusively breastfed for the first 6 months of life, with continued breastfeeding through at least 1 year of age. In light of these recommendations, states have implemented various policies to increase breastfeeding, including provision of workplace accommodations, insurance coverage of lactation-related services, and information-based interventions.

We focus on the effects of state-level hospital policies intended to increase breastfeeding by increasing access to breastfeeding information and support during the delivery-hospital stay. To date, these policies have been adopted by 16 states, 13 of which adopt during our sample period. Appendix Figure A1 shows the timing of policy adoption across states. Although the specific regulations vary, states frequently require that: (1) hospitals have a lactation consultant on staff, (2) patients be informed about the benefits of breastfeeding, (3) obstetric staff receive regular lactation training, (4) hospitals develop a written policy promoting breastfeeding, and (5) patients be permitted to have their baby stay with them 24 hours a day ("rooming in"). We provide detail on the provisions of each state policy in Appendix Table A1.

The adoption of these state hospital policies causally changed lactation support. Lawler and Yewell (2023) show that policy adoption significantly increased the number of International Board Certified Lactation Consultants (IBCLCs) in a state, with the increases driven by states that explicitly require lactation consultants in their regulation. They also find that after policy adoption, women that initiate breastfeeding are significantly more likely to report that they received breastfeeding information from hospital staff or that hospital staff helped with breastfeeding.

In Appendix Section B we present more detail on the broader breastfeeding policy landscape in the U.S. during this period. We also provide evidence that adoption of state hospital breastfeeding policies did not occur at the same time as related state policies, such as requirements that hospitals provide parents information about safe sleep and SUID prevention, implementation of Perinatal Quality Collaboratives, paid family leave laws, and other breastfeeding policies.

2.2 Channels Linking Postpartum Care Policies and Infant Health

There are two key channels through which the postpartum care policies we study may reduce infant mortality and improve infant health: the direct effects of breast milk consumption and changes in household behaviors beyond breastfeeding. These behavioral changes may occur either because additional in-hospital support increases parental exposure to information about recommended infant-care practices or because breastfeeding itself shifts parental preferences over other dimensions of infant care.

First, if the policies have their intended effect of increasing breastfeeding, there may be direct health impacts of increased breast milk consumption. These may occur due to changes in infant nutrition, although the magnitude and direction of this effect depends on the counterfactual infant food,[10] or because breast milk supports immune system development.[11] The immune-supporting benefits of breast milk may be particularly large for infants born prematurely, as their immune systems are less developed relative to full-term infants (Jakaitis and Denning, 2014).[12]

Second, these policies may impact infant health due to changes in household behaviors beyond breastfeeding. One mechanism is increased parental exposure to information about recommended infant-care practices. As mentioned above, Lawler and Yewell (2023) find that after policy adoption,

[10]This channel has likely been important in other contexts, including the PROBIT study in Belarus, where the primary alternative to breast milk was water or juice (Brenøe et al., 2026). Survey evidence from the U.S., however, suggests that during our study period, high-quality infant formula was the most likely counterfactual infant food during the first two months of life (Centers for Disease Control and Prevention, 2021).

[11]At birth, infants have immature immune systems. Breast milk contains unique bioactive factors that may provide protection against pathogens, stimulate maturation of the immune system, and contribute to establishment of a healthy microbiome (Lawrence and Pane, 2007; Ballard and Morrow, 2013; Jakaitis and Denning, 2014).

[12]In particular, randomized controlled trials show that breast milk reduces the incidence of necrotizing enterocolitis, a severe gastrointestinal illness that primarily impacts premature infants (Quigley et al., 2019).

the number of certified lactation consultants increased and mothers that initiated breastfeeding were more likely to report receiving breastfeeding information and assistance from hospital staff. Greater in-hospital support and contact with specialized providers may increase mothers' exposure to pediatric health recommendations and guidance on infant-care practices more generally.

This informational pathway may be particularly relevant for safe sleep practices, which directly affect the risk of Sudden Infant Death Syndrome (SIDS) and infant suffocation (Altindağ et al., 2024). Notably, safe sleep and the provision of "information and strategies to minimize the risk of Sudden Infant Death Syndrome" represent core competencies for International Board Certified Lactation Consultants (IBCLCs) (IBCLC Commission, 2023), and anecdotal evidence suggests that information on safe infant sleep is frequently provided in tandem with information on breastfeeding. As an example, Appendix Figure A2 displays a Michigan Department of Health flyer that provides information on breastfeeding and safe sleep practices. Throughout our sample period, both the CDC and American Academy of Pediatrics (AAP) recommended that infants be placed to sleep on their backs on a firm sleep surface free of soft bedding, and that they not bed-share.[13]

Beyond sleep practices, the informational channel may impact maternal smoking or other substance use. Both the CDC and AAP currently recommend that mothers not smoke when there is an infant in the household, particularly if the infant is being breastfed, and IBCLC certification requirements emphasize provision of evidence-based information about the use of alcohol, tobacco, and illicit drugs while breastfeeding. Changes in smoking may impact health given that infant exposure to secondhand smoke has been linked to SIDS, increased respiratory illness, and impaired lung function (Sachs and Committee on Drugs, 2013; CDC, 2019). While postpartum maternal substance use is a potential channel through which infant health may be affected, small sample sizes in available data sources unfortunately prevent rigorous analysis of such changes.

Broader changes in parental behaviors may also occur if policy-induced increases in breastfeeding change household preferences over other dimensions of infant care. Lawler and Yewell (2023)

[13]The AAP has recommended since 1996 that infants be placed to sleep on their backs, and that they sleep on firm sleep surfaces free of soft bedding (Kattwinkel et al., 1996). In 2000, the AAP additionally emphasized that bed-sharing can increase the risk of infant suffocation or death due to unexplained causes (Kattwinkel et al., 2000).

show that state adoption of these postpartum care policies significantly increased maternal time spent on child care. Changes in childcare environments may, in turn, impact infant health due to changes in exposure to infectious disease or infant safety (Currie and Hotz, 2004). Furthermore, the physiological effects of breastfeeding, namely the release of the hormone oxytocin as well as increased skin-to-skin contact, may increase maternal-infant bonding (Hurst, 2007). Changes in bonding may impact the quality of child care provided in the household.

Changes in breastfeeding could also impact sleep practices. Survey evidence from the U.S. suggests that decisions about sleeping arrangements are closely linked to breastfeeding, with approximately 35% of mothers reporting at 3 months postpartum that they bring their baby to bed with them to facilitate breastfeeding (Centers for Disease Control and Prevention, 2021). While the CDC and AAP discourage bed-sharing, prominent breastfeeding support organizations promote it as a tool to increase breastfeeding success and maternal-infant bonding.[14] Thus, the overall expected effect on sleep practices is ambiguous in our context, as they are influenced by both the informational channel and the extent to which increases in breastfeeding change preferences.

3 Data Description

3.1 Cohort Linked Birth-Infant Death Files

To examine infant mortality, we use the 1995–2018 Birth Cohort Linked Birth-Infant Death files compiled by the National Vital Statistics System of the National Center for Health Statistics (National Center for Health Statistics, 1995–2018). These data contain the complete census of births that occur in the U.S. each year, as well as information from the birth certificate, such as child and maternal demographics and the infant's health at birth.[15] For infants who die before their

[14]For example, a La Leche League publication states, "Bedsharing when breastfeeding is a traditional way of caring for a baby at night—breastfeeding at night can be a whole lot easier when you take your baby into bed with you and feed lying down. Breastfeeding mothers who bedshare get more sleep than bottlefeeding mothers and breastfeed for longer" (Cardus et al., 2022).

[15]Information about whether the infant was breastfed before discharge is available starting with the 2011 birth cohort and only for states that had implemented the 2003 revision to the birth certificate. The revised certificate was implemented across (and within) states on a rolling basis from 2003 to 2015. The limited availability of this

first birthday, the data contain information from the death certificate, such as age of death and cause of death. We collapse the data into state-of-residence (at birth)/birth-year cells and calculate the infant mortality rate by dividing the number of deaths in each state-of-residence/birth-year cell by the number of live births in that cell (measured in thousands). We examine mortality among infants born between 1995 and 2018; we exclude those born after 2018 to avoid including births and deaths that occurred during the COVID-19 pandemic.

Our primary measures of infant mortality are the overall one-year mortality rate as well as the neonatal and postneonatal mortality rates. Neonatal deaths are those that occur before the 28th day of life; postneonatal deaths are those that occur between 28 and 364 days of life.[16] Additionally, we consider mortality among infants born prematurely (before 37 weeks of gestation) and those born low weight (less than 2500 grams), and separately by underlying cause of death.

3.2 HCUP State Inpatient Discharge Records

To further examine the impact of hospital breastfeeding support policies on infant health, we use hospital inpatient discharge data from the Healthcare Cost and Utilization Project (HCUP) (Healthcare Cost and Utilization Project, 2000–2019). Our data consist of the universe of hospital inpatient discharges in nine states (Arizona, California, Florida, Kentucky, Maryland, New Jersey, New York, Rhode Island, and South Carolina) from 2000–2019.[17] Of these states, four adopted a hospital breastfeeding support policy during our sample years: Maryland (June 2005), New Jersey (January 2014), New York (September 2005), and South Carolina (June 2015).

Each observation is at the discharge record level and includes information on the year of

breastfeeding measure combined with the changing composition of states that implemented the revised certificate prevent us from using this information in our analysis.

[16]Neonatal mortality accounts for about two-thirds of infant mortality in the U.S. during our sample period and is typically due to poor health at birth (e.g., low weight) or delivery-related causes. Postneonatal mortality, on the other hand, is most commonly due to congenital malformations (birth defects), Sudden Infant Death Syndrome (SIDS), or external causes (Ely et al., 2018).

[17]We have the following years for each state: AZ: 2000–2018; CA: 2003–2011; FL: 2000–2019; KY: 2000–2019; MD: 2000-2019; NJ: 2000–2019; NY: 2000–2018; RI: 2002–2019; SC: 2000–2019. These state-years were selected based on data availability (CA, NY, RI) and budgetary constraints. Note that California data is only available prior to their policy adoption (2014).

discharge, hospital state, patient state of residence, diagnostic codes,[18] and associated charges. Each discharge can include at most 25 diagnostic codes, with one code designated as the primary diagnosis. The data also include limited patient and visit characteristics (e.g., sex, age in years at time of discharge, weekend admission).[19] We are unable to link patients over time.[20]

We focus on discharges for infants that were less than one year of age at the time of discharge. As the data are provided at the hospital-state level, they include discharges for out-of-state residents who receive care at one of our sample-state hospitals. We do not observe discharges for sample-state residents who travel to a non-sample state for hospital care. Therefore, in our sample we include only patients who reside in a state that has a hospital service area overlapping with a state for which we have discharge records.[21] We collapse the data to the patient state-of-residence/hospital-state/discharge-year level and assign treatment exposure based on patient state of residence.

For outcomes, we consider the inpatient hospitalization rate and average total charges, each measured for non-delivery hospitalizations.[22] Although hospital charges are not a direct measure of health, they may measure medical service intensity. Thus, changes in charges may reflect changes in underlying health. We also separately examine the hospitalization rate and average charges by primary diagnosis. Across all outcome variables, the denominator is a measure of the number of observed deliveries in the state-of-residence/hospital-state/discharge-year cell.

3.3 NIS-Child

To study how these regulations impact breastfeeding, we use the restricted-use version of the CDC's National Immunization Survey-Child (NIS-Child) from 2003–2017 (U.S. Department of Health and Human Services, 2004–2010, 2011–2015, 2016–2018). We accessed these data through

[18] These codes are ICD-9 until 2015, and then transition to ICD-10.

[19] The data contain information about patient race and expected primary payer. However, the collection and coding of these variables change substantially within states over time, so we do not use them in our analyses.

[20] Patients can be linked over time in a subset of state-years (CA: 2003–2011; FL: 2005–2019; MD: 2013–2019; NY: 2005–2018), but unfortunately we have no identifying variation in this subsample.

[21] For example, the New York hospital service areas include regions of PA, VT, and MA. Therefore, in the NY discharge records we keep only observations for patients whose state of residence is NY, PA, VT, or MA. Our findings are robust to limiting the sample to discharges for infants for which the hospital state and state of residence are the same.

[22] Each infant discharge record is classified as either a delivery or non-delivery stay based on diagnostic codes. For ICD-9, we use codes V30.0–V39.0 to identify deliveries; for ICD-10: Z38.

a Federal Statistical Research Data Center.

The NIS-Child is an annual state-representative survey that targets households with children aged 19–35 months. Breastfeeding outcomes are self-reported, and include information on both initiation and duration. The duration measures focus on breastfeeding along the *extensive* margin; an infant is considered breastfed if they are fed any breast milk.[23] We cannot distinguish between breastfeeding and bottle feeding breast milk.

In the restricted-use data, we observe state of residence at time of birth, as well as the month and year of birth. These restricted geographic and date variables allow us to assign policy exposure to the survey respondents. To conduct our analyses, we collapse the data into state-of-residence (at birth)/birth-year cells using the NIS-Child sampling weights.

3.4 PRAMS

We use data from the CDC's Pregnancy Risk Assessment Monitoring System (PRAMS) from 2000–2018 (Centers for Disease Control and Prevention, 2000–2018) to examine policy effects on infant sleep practices. The PRAMS surveys women who had a live birth in the past 2–4 months, drawn from a sample of state birth certificate records. We use self-reported data on infant sleep position and infant bed-sharing.[24] We construct three indicator variables related to these sleep practices: if the infant's usual sleep position is on their back (the recommended position); if the infant *never* bed-shares (as is recommended); and if the infant often or always bed-shares (which is recommended against). We collapse the data to the state-of-residence (at birth)/birth-year level using PRAMS sample weights.

This data set has two notable limitations. First, the set of states with available data varies substantially across years, with between 19 and 36 states reporting in a given year.[25] Second, the

[23] Although the NIS-Child includes questions about exclusive breastfeeding, we do not examine this outcome due to a significant survey question redesign in 2006 and variable coding inconsistencies in later survey waves.

[24] Possible responses to the question about the infant's usual sleep position are side, back, stomach, and all combinations of those three. Regarding bed-sharing, the respondent is asked how often the infant sleeps in the same bed as the mom or anyone else, with possible responses of always, often, sometimes, rarely, and never.

[25] This variation is due both to states not participating in the survey in a given year and data not being released for a given state-year if response rates did not meet a pre-specified threshold. The number of states choosing

survey question pertaining to infant bed-sharing was part of an optional module, which further restricts the set of included state-years when examining these outcomes. Appendix Table A2 provides information on the availability of the PRAMS data across state-years, as well as how that coincides with state policy implementation.

3.5 Policy Data

We obtained information on state adoption of hospital breastfeeding regulations from the LawAtlas Policy Surveillance Program database (ChangeLab Solutions, 2018); adoption dates were identified through independent review of state statutes and state administrative codes. We graphically present the timing of policy adoption across states in Appendix Figure A1. While there is substantial variation in policy adoption across space and time, there is some clustering of adoption in the Northeast and South, and only one state in the Western census region (California) ever adopts.

4 Empirical Strategy

4.1 Callaway and Sant'Anna (2021) Estimator

We aim to identify the causal effects of state adoption of hospital regulations on infant mortality and other measures of infant health, breastfeeding, and infant sleep practices. Our empirical setting presents a number of challenges: policy adoption is staggered across time, state policies vary in their relative strength, and we expect time-varying treatment effects due to, for example, time needed for staff training and program implementation. The recent econometric literature establishes that in such settings where treatment effect heterogeneity is likely, two-way fixed effects (TWFE) estimates may be biased (see for example, de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

to participate has increased over time, from 20 states in 2000 to 48 states in 2018. The response rate threshold for the data to be publicly released has also changed, decreasing from 70% for 2000–2006, to 65% for 2007–2011, to 60% for 2012–2014, and to 55% from 2015 to present.

In light of these econometric challenges, we use the Callaway and Sant'Anna (2021) (CS) estimator as it does not suffer from bias due to time-varying or cohort-specific treatment effects. The CS estimator separately identifies the average treatment effect on the treated for each treatment cohort g, where g represents the year of policy adoption, and calendar year t, $ATT^{CS}(g,t)$. Each $ATT^{CS}(g,t)$ is estimated by comparing the average change in the outcome between periods $g-1$ and t for cohort g to the average change over this same period for the control group:

$$\widehat{ATT}^{CS}(g,t) = \frac{1}{n_g}\sum_{s=1}^{n} \mathbf{1}\{G_s = g\}(Y_{s,t} - Y_{s,g-1}) - \frac{1}{n_U}\sum_{s=1}^{n} \mathbf{1}\{U_s = 1\}(Y_{s,t} - Y_{s,g-1}). \quad (1)$$

$Y_{s,t}$ is the outcome for state s in year t. G_s represents state s's group, defined by the time period that the state adopted a policy; n_g is the number of states in treatment cohort g; n_U is the number of states in the control group; and U_s is an indicator for whether the state is in the control group. For our main results we use the never-treated states as the control group; in robustness checks we use the never- *and* not-yet-treated states. Always-treated states (i.e., those that had already adopted the regulation by the start of the sample period) are excluded from the sample throughout.[26] Once estimated, the individual $\widehat{ATT}^{CS}(g,t)$ can be aggregated into economically-relevant parameters by taking their weighted average.

For our main analyses, we present two different aggregations of the individual $ATT^{CS}(g,t)$. First, we present aggregations analogous to classic event-study parameters, which are the weighted average of all cohorts' treatment effects k years relative to policy adoption, for $k \in \{-4, ..., -2, 0, ..., 3, 4\}$,

$$ATT_k^{CS,ES} = \sum_g w(g,k) ATT^{CS}(g, g+k), \quad (2)$$

where $w(g,k)$ is a weight that depends on the relative size of group g among groups that are treated for at least k periods. This aggregation allows us to test for dynamic policy effects and examine the extent to which outcomes were changing similarly in treatment and control states prior to policy adoption. Treatment exposure is assigned based on an infant's state of residence

[26] The composition of the always-treated group changes depending on the dataset and outcome under consideration.

at the time of birth,[27] and we define the year of policy adoption as follows. A state is considered to have implemented a hospital postpartum care regulation in a given calendar year if they had done so by June of that year. For states that implement these policies in the latter half of the calendar year, we define the year of policy adoption as the following calendar year.[28]

To summarize the $ATT^{CS}(g,t)$ into a single parameter, we also report the simple average of all post-treatment $ATT_k^{CS,ES}$ specified above (i.e., for $k \in \{0, ..., 3, 4\}$). As this parameter captures an average of the event-time effects through 4 years after policy adoption, we consider it an estimate of the medium-term effect of policy adoption. The decision to focus on treatment through 4 years post-adoption was motivated by the distribution of state-years that provide identification across the different datasets. That is, since $ATT_k^{CS,ES}$ is the average treatment effect at k years of exposure to the treatment (among those groups that experience k periods of treatment), we focus on a post-treatment horizon where the composition of groups that identify $ATT_k^{CS,ES}$ is relatively stable across the values of k. In the appendix, we report estimates that represent the average of the event-time effects for post-treatment years 0 through 3, 0 through 5, and 0 through 12.[29] Following Callaway and Sant'Anna (2021), we report standard errors from a multiplier bootstrap procedure clustered at the state level. Depending on the dataset, observations are weighted by the sum of the relevant sampling weights or by the number of observed births in the state-year cell.

There are two key identifying assumptions. First, in the absence of treatment, outcomes across treated and control states would have evolved in parallel *during the post-treatment period* (Roth et al., 2023). Notably, this parallel trends assumption is weaker relative to that needed by imputation estimators that are similarly robust to treatment effect heterogeneity (e.g., Borusyak et al., 2024), as they require parallel trends across treated and control states and *all time periods*. In practice, this means that if the parallel trends assumption only holds approximately, the CS estimator will be less biased than alternative estimators that require a stronger assumption. The second

[27] In HCUP data, however, we know only the state of residence at the time of hospitalization.

[28] In the appendix we show that our estimates are generally robust to alternatively defining the calendar year of policy adoption to be the first treated year, regardless of the month of adoption.

[29] We report the estimate for 0 through 12 years after policy adoption because, across all datasets, 12 years is the most post-treatment periods we always observe.

assumption is that there are no anticipation effects, or that the treatment has no causal effect prior to being implemented. To provide evidence in support of these assumptions, in Section 5.1 we examine the extent to which outcomes in treatment and control states exhibited parallel trends in the pre-treatment period. We also investigate, in Section 5.3, whether baseline measures of breastfeeding, infant mortality, and maternal characteristics, as well as changes in those measures, were related to policy adoption.

4.2 Synthetic Difference-in-Differences Estimator

In some cases, the results from the CS estimator suggest differential pre-trends between the treatment and control states. To address this limitation, we also estimate synthetic difference-in-differences (SDID) models, following Arkhangelsky et al. (2021). Similar to a standard synthetic control (SC) model, the SDID estimator re-weights control (never-treated) states to match pre-treatment trends in the outcome among treated states.[30] To improve precision and reduce bias, the SDID estimator also employs time weights, which put relatively less weight on pre-treatment time periods that are very different from the post-treatment period.

Given that our empirical setting features staggered policy adoption, we follow Clarke et al. (2024) and Ciccia (2024) and calculate the SDID estimate of the average treatment effect on the treated for each treatment cohort g and event time k, $ATT^{SDID}(g,k)$. For each cohort g, $ATT^{SDID}(g,k)$ is calculated by comparing the average difference in the outcome between treated states and the synthetic control group in period $g+k$ to the difference in the pre-treatment weighted average of the outcome for those same states:

$$\widehat{ATT}^{SDID}(g,k) = \frac{1}{n_g}\sum_{s=1}^{n}\mathbf{1}\{G_s = g\}Y_{s,g+k} - \sum_{s=1}^{n}\mathbf{1}\{U_s = 1\}\omega_{s,g}Y_{s,g+k} - \sum_{t=t_{min}}^{g-1}\left(\frac{1}{n_g}\sum_{s=1}^{n}\mathbf{1}\{G_s = g\}\lambda_{g,t}Y_{s,t} - \sum_{s=1}^{n}\mathbf{1}\{U_s = 1\}\omega_{s,g}\lambda_{g,t}Y_{s,t}\right). \quad (3)$$

[30] As with the demeaned SC estimator proposed by Ferman and Pinto (2021), SDID allows for time-invariant level differences across units. Therefore, SDID unit weights are selected only to match pre-treatment trends, as opposed to matching on pre-treatment levels *and* trends.

In this specification, t_{min} is the earliest (pre-treatment) period observed in the relevant dataset, $\omega_{s,g}$ represents the optimal unit weight for never-treated state s when serving as a control unit for treatment cohort g, and $\lambda_{g,t}$ represents the optimal time weights assigned to each pre-treatment period t. All other variables are as defined in equation 1.

Prior to the estimation of equation 3, optimal unit weights are chosen to match pre-treatment trends in the outcome between never-treated states and states in treatment cohort g. Always-treated states are excluded. Time weights are then selected for the pre-treatment periods such that the difference between the post-treatment average and the pre-treatment *weighted* average for each control unit is a common constant. See Arkhangelsky et al. (2021) for a detailed discussion of the algorithm used to identify the optimal unit and time weights.

Once estimated, we can aggregate the individual $\widehat{ATT}^{SDID}(g,k)$ into economically-relevant parameters by taking their weighted average. As with the CS estimator, we present event-study parameters, $ATT_k^{SDID,ES}$, which are the weighted average of all cohorts' treatment effects k years relative to policy adoption, for $k \in \{-4,-3,...,3,4\}$, using the same weighting scheme as in equation 2. We also report the simple average of the post-treatment $ATT_k^{SDID,ES}$ for $k \in \{0,...,3,4\}$. Standard errors are obtained from a block bootstrap procedure clustered at the state level. Aside from the optimal unit and time weights, we do not employ sample weights for the SDID analyses.

One important consideration is that the SDID estimator requires a balanced panel. For several outcomes and datasets, our sample is unbalanced. We discuss how we address this limitation for each impacted outcome and dataset when we present the relevant set of results.

5 Main Results

5.1 Infant Mortality Results

We begin by investigating the effects of state-level hospital breastfeeding regulations on infant mortality rates using the Vital Statistics Cohort Linked Birth-Infant Death files, collapsed to the state-of-residence (at birth)/birth-year level. Descriptive statistics are shown in Table 1 for the full

sample and separately for states that are ever treated or never treated during the sample period. On average in the full sample, there are 6.5 deaths within the first year of life, 4.3 deaths within the first 28 days of life, and 2.2 deaths between 28–364 days of life per 1,000 live births. Across the mortality measures, rates are lower in ever-treated states compared to never-treated states. Infant health at birth, on average, is nearly identical across the two groups of states. However, mothers in never-treated states are more likely to be non-Hispanic white (versus Black, other race, or Hispanic).

In Figure 1, we present the event-study estimates from the CS and SDID estimators for our three main mortality measures: one-year, neonatal (within the first 28 days), and postneonatal (28–364 days) deaths per 1,000 live births. The figures generally show evidence of declines following state policy adoption, with particularly striking and sustained decreases in one-year and postneonatal mortality rates. They also show no evidence of differential pre-trends across the mortality outcomes.

In Table 2, Panel A, we report a single summary estimate of the medium-term treatment effect of the policy, which is the simple average of the event-study effects for 0 through 4 years post-policy adoption. The first and second rows present the results from the CS and SDID estimators, respectively, and show a statistically significant 0.22–0.23 decrease in deaths in the first year of life per 1,000 live births, a 3.5–3.7% decline relative to the period before policy adoption (column 1). This reduction is driven by decreases in both the neonatal and postneonatal periods (columns 2 and 3). Specifically, hospital postpartum care regulations lead to 0.10–0.13 fewer deaths per 1,000 births in the first 28 days of life, a 2.4–3.1% decline relative to the pre-treatment mean, though these estimates are not statistically significant. Deaths in the 28–364 days after birth significantly declined by 0.11–0.12 per 1,000 births, representing a 5.7% decline relative to the pre-treatment mean.

Effect Heterogeneity by Race/Ethnicity In Table 2, Panels B and C, we show the medium-term effects of state-level hospital postpartum care policies on infant mortality separately by maternal race and ethnicity.[31] Corresponding event-study estimates for one-year, neonatal, and postneonatal mortality rates are presented in Figure 2. These results show that the mortality

[31]Maternal race is always observed in the natality data. However, Hispanic origin is missing for 0.8% of infants in our sample. For these analyses, infants born to white mothers with missing Hispanic origin cannot be classified as non-Hispanic white or as Black, other race, or Hispanic, and are therefore excluded.

declines are primarily driven by infants of Black, other race, or Hispanic mothers (hereafter referred to as non-white). There is a 0.43–0.46 decrease in deaths in the first year of life per 1,000 live births to non-white mothers, a 6.0–6.4% decline relative to the period before policy adoption. These declines occur during both the neonatal and postneonatal periods, although the statistical significance of the neonatal mortality effect is sensitive to the choice of estimator. In contrast, there are no significant changes for infants of non-Hispanic white mothers (hereafter referred to as white) and the estimates are small in magnitude.[32]

5.2 Sensitivity Analyses

We conduct a number of analyses to probe the robustness of the main infant mortality results. Appendix Tables A3–A5 show that our results are generally robust to alternative aggregations of the event-study effects, to using never- and not-yet-treated states as the control group (in the case of the CS estimator), and to defining treatment as starting in the calendar year of adoption for all states (rather than the year following adoption for states adopting in July or later). Across the specification choices, the CS and SDID estimates align quite closely. However, for neonatal mortality among infants born to non-white mothers, the SDID estimates tend to be estimated more precisely. We also demonstrate that the infant mortality results are similar, and if anything larger in magnitude, if we limit the sample to the birth cohorts for which the NIS-Child breastfeeding data are available (2000–2015) (Appendix Tables A3–A5, column 7).[33] Last, since Hispanic origin is missing for a small fraction of the sample, we show that our conclusions are unchanged if we use an alternative race classification that ignores Hispanic origin, defining infants born to white mothers as white (regardless of maternal ethnicity) and those born to Black or other-race mothers as non-white (Appendix Tables A4 and A5, column 8).

To investigate the extent to which our medium-term effects may be driven by one individual

[32]Unfortunately, we cannot examine heterogeneous effects on infant mortality by maternal education. From 2011–2015, maternal education is missing from the natality data if the mother gave birth in a state that had not yet adopted the 2003 revision to the birth certificate, which is a non-trivial share of mothers.

[33]In this robustness exercise, Mississippi, Missouri, Pennsylvania, South Carolina, and Texas no longer contribute identifying variation.

treated state, we conduct a "leave-one-out" exercise, where we sequentially drop states that adopted a hospital breastfeeding support policy during the sample period. The results from both the CS and SDID estimators are presented in Appendix Figures A3 and A4 and generally suggest that no single state has an outsized impact on our estimates.

Finally, given evidence that bootstrapped standard errors may result in over-rejection in settings with a small number of treated clusters (MacKinnon and Webb, 2017, 2018), we test the sensitivity of our results to t-statistic-based randomization inference, following the procedure outlined in MacKinnon and Webb (2020) and Roth and Sant'Anna (2023). Except in settings with very small numbers of treated clusters (i.e., 1–2 clusters), t-statistic-based randomization inference typically under-rejects the null hypothesis (MacKinnon and Webb, 2020).[34] It also generally outperforms coefficient-based randomization inference, especially in settings such as ours with variation in cluster sizes.

The randomization inference tests the sharp null hypothesis of no treatment effect. To implement the procedure, we randomly reassign treatment dates across the states in our sample while preserving the empirical distribution of treatment timing. For each randomized treatment reassignment, we estimate the medium-term treatment effects and associated t-statistics using the CS and SDID estimators. We repeat this procedure 1,000 times and calculate one-sided p-values based on the share of placebo t-statistics that are smaller (i.e., more negative) than the t-statistic obtained using the true assignment of treatment dates. The one-sided test is informative because our hypothesis of interest is whether the policies reduced infant mortality. We also report two-sided p-values based on the share of placebo t-statistics that are larger in absolute value than the absolute value of the t-statistic obtained using the true treatment assignment.

We report the p-values from this randomization inference procedure in Appendix Table A6. For the CS estimator, we report the multiplier bootstrap p-value from our main specification in column (1) and the one- and two-sided randomization inference p-values in columns (2) and (3). For the SDID estimator, construction of the t-statistics relies on block bootstrap standard errors. In

[34] Our setting is *not* one with very few treated clusters. Our mortality analyses include 48 total clusters (including the District of Columbia), 13 of which are treated. However, the finite-sample properties of the multiplier and block bootstrap procedures used for inference with the CS and SDID estimators, respectively, are less established than inference with TWFE. We therefore view the randomization inference exercises as a useful conservative benchmark.

order to make this exercise computationally feasible, within the randomization inference procedure these standard errors are computed using 50 block bootstrap replications; all other SDID results reported in the paper, including the main ones, are based on 200 replications. Columns (4) and (5) show that using 50 rather than 200 bootstrap replications has little effect on the block bootstrap p-values. We report the one- and two-sided randomization inference p-values in columns (6) and (7). For the CS estimator, the p-values obtained from the randomization inference procedure tend to be slightly more conservative, whereas for the SDID estimator, the p-values tend to be similar, and in several cases, slightly smaller. Crucially, our core conclusions are unaffected, as we continue to find that reductions in one-year and postneonatal mortality in the full sample and among infants born to non-white mothers are statistically significant at conventional levels.[35]

5.3 Support for Identifying Assumptions

Our empirical strategy relies on two key identifying assumptions: (1) in the absence of treatment, outcomes across treated and control states would have evolved in parallel during the post-treatment period (Roth et al., 2023), and (2) no anticipation effects. Our event-study estimates, presented in Figures 1 and 2, provide support for these assumptions as there is no evidence of pre-trends or anticipatory changes in mortality outcomes. In Appendix Table B2, we also show that adoption of state hospital breastfeeding policies did not occur at the same time as related state policies, such as paid family leave, requirements that hospitals provide parents information about SUID prevention and safe sleep, implementation of state Perinatal Quality Collaboratives, and other breastfeeding support policies. In this section, we further assess the validity of the identifying assumptions.

We first conduct a series of falsification tests to examine whether policy adoption predicts changes in maternal demographic characteristics, prenatal care use, infant health at birth, or characteristics of the birth, as observed in the Vital Statistics data. Given that the policies we study should only impact postpartum care, changes in these measures concurrent with policy adoption could suggest that our main results are driven by other unobserved changes that broadly

[35] We do not report randomization inference results for infants born to white mothers, as none of the estimated mortality effects for this subgroup are statistically significant under our bootstrap inference procedures.

affect fertility or perinatal health care.

The results for maternal demographic measures from the full sample and separately by race/ethnicity are presented in Appendix Table A7.[36] Overall, we find little evidence that policy adoption meaningfully changed the composition of mothers giving birth. Although some maternal age estimates are statistically significant, the effects are small in magnitude and do not systematically imply improvements or deterioration in infant health.[37]

The results in Appendix Table A9 provide little evidence that policy adoption meaningfully changed prenatal care, infant health at birth, or characteristics of the birth. The primary exception is the share of infants born low weight, which declines by roughly 0.001 in the full sample and among infants born to white mothers, and by about 0.002 among infants born to non-white mothers. Relative to the period prior to policy adoption, these estimates represent only a 1–2.5% decrease. Taking these declines at face value, however, they are too small to explain much of the estimated decrease in infant mortality.[38] Moreover, declines in low birth weight are observed for both infants born to white and non-white mothers, whereas the mortality improvements are concentrated among infants born to non-white mothers. This pattern further suggests that changes in low birth weight are unlikely to be the primary driver of the mortality results.

We next examine whether baseline measures of breastfeeding, infant mortality, and maternal characteristics, as well as changes in these measures, were related to state policy adoption. To

[36]In Appendix Tables A7 and A9, there are instances where we cannot show SDID estimates because the panel for that outcome is not balanced around calendar time, which the SDID estimator requires, or there are many missing values for that outcome in a state-year cell. This is due to: (i) missing variables in certain years if states (or areas of states) had not yet adopted the 2003 revision to the birth certificate (e.g., maternal education, prenatal care); (ii) California not reporting mothers' marital status after 2016; (iii) NICU admission becoming available starting with the 2005 birth cohort and only on the 2003 revision to the birth certificate. For the CS estimator, we address these missingness issues by weighting observations by the number of births in that cell with non-missing information on that characteristic. As SDID does not accommodate sample weights, we omit estimates for these outcomes.

[37]We also evaluate changes in maternal demographic characteristics using the NIS-Child data that we use for the breastfeeding analysis. The estimates are presented in Appendix Table A8 and are likewise small in magnitude and not statistically significant.

[38]To fully explain the decrease in one-year mortality, infants born low weight would need to have a one-year mortality risk 20 percentage points higher than those not born low weight (0.0002/0.001 in the full sample and 0.0004/0.002 for infants born to non-white mothers), which is implausibly large. For the cohorts we study, the observed difference in one-year mortality between infants born low weight and all other infants is 5.2 percentage points in the full sample (5.7 percentage points among infants born to non-white mothers). Thus, even if this mortality differential were entirely causal, the most pessimistic case is that declines in the share of infants born low weight could explain at most 26% of the mortality decline in the full sample and 29% of the decline among infants born to non-white mothers.

overcome substantial missingness in maternal education and marital status across states and years in the Vital Statistics data, we additionally use data from the American Community Survey (ACS) from 2000–2019. To test for trends prior to policy adoption, we estimate modified TWFE event-study models, where the pre-policy adoption indicators are replaced with a linear trend. We use TWFE for this exercise because our objective is to test whether treated states exhibited differential linear trends prior to policy adoption, rather than to estimate dynamic treatment effects. We also estimate cross-sectional regressions to examine whether baseline state characteristics, or changes in those characteristics, predict policy adoption. To ensure sufficient pre-policy adoption data, we restrict these analyses to states in which a policy took effect in 2003 or later. Details on the data and specifications are provided in Appendix C.

The results are presented in Appendix Table A10. We find no evidence of linear pre-trends in any of the maternal characteristics or outcomes we consider (column 1); all estimates are relatively small in magnitude and none are statistically significant. Similarly, neither baseline characteristics nor pre-adoption changes in those characteristics predict whether a state ever adopted a policy or was an early adopter (adopting prior to 2010), with one exception. Consistent with the racial differences observed in the descriptive statistics (see Table 1), states in which a relatively higher share of women with past-year births are white were significantly less likely to ever adopt a hospital breastfeeding support policy (column 2). Importantly, however, pre-adoption *changes* over time (2000 to 2002) in the share of mothers that are white do not predict either ever adopting or early adoption (columns 4 and 5). Furthermore, across alternative specifications, none of the observed characteristics are significant predictors of adoption, and the joint F-test of the characteristics always yields a p-value greater than 0.5.[39] Taken together, these results provide additional support for the identifying assumptions and suggest that the estimated mortality effects are unlikely to be driven by pre-existing differential trends or other unobserved changes in perinatal care.

[39] Alternative specifications that control for other measures of breastfeeding and infant mortality by race/ethnicity similarly yield joint F-test p-values that are not statistically significant. Results available upon request.

6 Potential Mechanisms

In this section, we shed light on potential channels underlying the reductions in infant mortality. First, we examine whether the policy effects vary by infant health at birth and across primary cause of death. We then explore effects on infant hospitalizations and charges, overall and by primary diagnosis. Last, we investigate whether the policies induced changes in breastfeeding and infant sleep practices. Given that the main infant mortality reductions occur among infants born to non-white mothers, we also explore heterogeneous effects on these outcomes by race/ethnicity when the data allow. While disentangling the exact mechanisms is practically very difficult, results from these exercises provide insight on whether direct effects of breast milk consumption or broader changes in household behaviors due to informational pathways and/or policy-induced changes in breastfeeding are more or less likely to be at play.

6.1 Mortality Analyses by Infant Health at Birth

We explore the policy effects on one-year mortality rates among medically vulnerable infants, namely those born premature and low weight.[40] We present the medium-term effects in Panel A of Table 3 and the event-study estimates in Panel A of Appendix Figure A5.

A large and significant mortality decline occurred among infants born preterm. In the medium term, hospital breastfeeding regulations resulted in 1.5–1.6 fewer deaths in the first year of life per 1,000 infants born premature, a 4.1–4.5% decline relative to the pre-treatment mean. While both the CS and SDID estimates suggest declines in mortality among infants born low weight, they are not precisely estimated. When we consider effect heterogeneity by maternal race and ethnicity, we find that the declines in mortality among premature infants are driven by those born to non-white mothers—they experience 2.1–2.9 fewer deaths per 1,000 births, a 5.2–7.2% reduction relative to the period before policy adoption (Table 3, Panel C). These findings are consistent with direct effects of breast milk playing a role, as the immune-boosting benefits of breast milk

[40] We do not explore effect heterogeneity by health at birth in our other datasets, as the HCUP and NIS-Child data do not include information about whether the infant was born preterm or low weight, while the sample sizes for these subgroups in the PRAMS data are too small to yield reliable estimates.

may be particularly large for premature infants whose immune systems are less developed at birth (Jakaitis and Denning, 2014).

6.2 Mortality Analyses by Cause of Death

We next consider effects on infant mortality by primary cause of death.[41] We group all possible diagnostic codes for underlying cause of death into the following eight mutually-exclusive groups: diseases of the digestive system; diseases related to environmental exposures and immune strength;[42] nutritional, metabolic, and related disorders;[43] external causes (e.g., accidental suffocation in bed, motor vehicle crashes, drowning, poisoning, other trauma); ill-defined causes (e.g., Sudden Infant Death Syndrome (SIDS)); congenital abnormalities (e.g., cleft lip or palate, congenital heart defects); conditions originating in the perinatal period (e.g., disorders related to short gestation and low birth weight, feeding problems of newborn); and all other diagnoses.

Our groupings are motivated by the key pathways through which breastfeeding support policies may change infant health. Consumption of breast milk may reduce the incidence of gastrointestinal infection (digestive system), improve immune functioning (environmental exposure and immune strength), or change nutritional intake (nutrition-related). The effects of these pathways may be relatively larger for infants with congenital abnormalities or conditions originating in the perinatal period, as they are typically more medically vulnerable, at higher risk of severe gastrointestinal disease, and have higher rates of feeding difficulties (Davis and Spatz, 2019). Changes in child-care environment or household behaviors arising from informational channels and/or changes in breastfeeding, including infant sleep practices, may also impact exposure to infectious disease (environmental exposure and immune strength) and the incidence of injury or ill-defined conditions.

[41] Effective in 1999, causes of death were classified using ICD-10 codes rather than ICD-9 codes. Results are nearly identical when we restrict the analysis to cohorts born in 1999 and after and are available by request.

[42] This group aggregates infectious and parasitic diseases (e.g., pertussis/"whooping cough," streptococcal sore throat), diseases of the respiratory system (e.g., influenza, asthma), diseases of the nervous system and sense organs (e.g., otitis media/ear infection, meningitis), and diseases of skin and subcutaneous tissue (e.g., dermatitis and eczema).

[43] This group combines endocrine, nutritional, metabolic diseases, and immunity disorders (which include diabetes, dehydration, and nutritional deficiencies) together with diseases of blood and blood-forming organs (e.g., anemia). Although it contains some disorders that are unlikely affected by nutrition (e.g., genetic disorders), for simplicity we refer to this group of causes as "nutrition-related."

We present estimates of the medium-term effects on one-year mortality by cause of death graphically in Figure 3, with point estimates and standard errors reported in Appendix Table A11. There is a statistically significant reduction in one-year mortality due to conditions originating in the perinatal period, amounting to 0.13–0.15 fewer deaths per 1,000 live births, or a 3.9–4.6% decline relative to the period before policy adoption (Panel A). The declines in mortality due to perinatal causes are driven by infants born to non-white mothers (Panels B and C). Disorders related to gestation length and fetal malnutrition are the leading contributors to deaths due to perinatal causes. Thus, the decline in these deaths is consistent with the mortality reductions we find among premature infants. In fact, when we investigate mortality by cause of death among premature infants, declines due to perinatal causes are the primary driver (Appendix Table A12). These results again suggest that direct effects of breast milk are at play.

There are also reductions in one-year mortality due to external causes, though only the CS estimates are statistically significant. We estimate a 0.04 decrease in deaths per 1,000 live births. Relative to the pre-treatment mean, this represents about a 12% decline in external cause infant mortality. During our sample period, deaths due to accidental suffocation and strangulation in bed make up the largest share of this category, which also includes mortality due to motor vehicle crashes, drowning, poisoning, and other trauma. Thus, the reductions in mortality due to external causes are less likely attributed to direct effects of breast milk and instead to in-hospital exposure to information about recommended parenting behaviors and/or changes in household behaviors arising from policy-induced changes in breastfeeding.

The event-study estimates from the CS and SDID estimators for one-year mortality by cause of death are presented in Appendix Figures A6–A8. For both perinatal and external causes of death, we see little to no evidence of differential pre-trends.

The suggestive reduction in external cause mortality, for which deaths due to accidental suffocation and strangulation in bed make up the largest share, indicates that another cause of death grouping may be important to explore: sleep-related mortality, often referred to as Sudden Unexpected Infant Death (SUID). SUID is defined in the literature to include accidental suffocation

and strangulation in bed, SIDS, and other ill-defined and unspecified causes of mortality (Li et al., 2022; Moon et al., 2022); therefore, it represents a combination of subgroups from the "external" and "ill-defined" causes categories discussed above.

The medium-term effects on one-year sleep-related mortality are shown in Table 4, with corresponding event-study results in Appendix Figure A9. In the full sample and among infants born to non-white mothers, the estimates suggest declines, though the statistical significance is sensitive to estimator choice (Table 4, column 1). Specifically, over the medium-term, policy adoption decreases deaths in the first year of life due to SUID among infants born to non-white mothers by 0.06–0.1 per 1,000 live births, about a 7–11% decline from the pre-treatment mean.

Given the substantial declines in one-year mortality we observe among infants born preterm, and the fact that preterm infants have significantly higher rates of SUID (Horne et al., 2024), we also examine sleep-related mortality effects for this subgroup. Preterm infants may be particularly vulnerable to SUID due to less developed cardiorespiratory control and impaired arousal response from sleep, which can impact their ability to respond to external stressors, including prone sleeping, head coverings, and bed-sharing. The results are reported in Table 4, column 2, and show robust reductions among the full sample of premature infants (Panel A) and premature infants born to non-white mothers (Panel C).[44]

The reductions in sleep-related mortality are unlikely to arise primarily from direct effects of breast milk. Instead, it seems more likely that additional in-hospital interactions with specialized providers increased exposure to information about safe sleep practices. We directly explore changes in sleep practices in Section 6.5, and find suggestive evidence of improvements following policy adoption. We note, however, that for premature infants in particular, we cannot fully rule out a role for breast milk consumption in the observed reduction in sleep-related mortality. Epidemiological work documents an association between breastfeeding and SUID risk (Hauck et al., 2011), though the exact biological mechanisms and whether these associations reflect a causal relationship are

[44]Consistent with these results, Appendix Table A12, Panel C shows robust declines in one-year mortality due to ill-defined causes among premature infants born to non-white mothers. SIDS and "other ill-defined and unspecified causes" make up over 95% of deaths in the "ill-defined" causes category.

widely debated.

Scaling our estimates by the number of observed births in treated states in the 0–4 years after policy adoption (9.63 million) suggests that 2,090–2,234 infant deaths were averted as a result of these policies. Of the averted deaths, 1,223–1,444 would have occurred due to causes originating in the perinatal period (0.127–0.150 fewer deaths due to perinatal causes per 1,000 births $\times$ 9.63 million births), which is about 60% of the overall decrease in one-year infant mortality. Furthermore, 356–375 of the averted deaths would have occurred due to external causes (about 0.04 fewer deaths due to external causes per 1,000 births $\times$ 9.63 million births).

6.3 Inpatient Hospitalization Results

We next examine effects on infant inpatient hospitalizations using HCUP data from nine states (Arizona, California, Florida, Kentucky, Maryland, New Jersey, New York, Rhode Island, and South Carolina) for 2000–2019. These data provide additional measures of infant health and offer further insight on potential mechanisms underlying the reductions in infant mortality. Due to frequent changes in the race and ethnicity variables within states over time, we are unable to examine heterogeneous effects on hospitalization outcomes by race/ethnicity.

Descriptive statistics from the HCUP data are presented in Appendix Table A13. In the first year of life there is an average of 137.8 non-delivery hospitalizations per 1,000 observed births, and average charges for these hospitalizations are $5,086 per observed birth. For both outcomes, the averages are higher in never-treated relative to ever-treated states.

We present the CS and SDID event-study estimates for non-delivery infant hospitalization rates and charges in Figure 4. The corresponding medium-term effects are presented in Table 5, column 1. For the CS estimator we use the full sample of state-of-residence/hospital-state/year observations; to achieve a balanced sample for the SDID estimator we drop discharges from California (as they are available only for 2003–2011), limit the sample years to 2002–2018 for all other states, and drop all discharges of non-hospital state residents.[45] The results show that there was no significant

[45]We have estimated the CS models on this balanced panel and our conclusions are unchanged. Those results

change in the rate of non-delivery hospitalizations among infants following state adoption of a hospital breastfeeding support policy (Figure 4, Panel A). We do find, however, a significant reduction in average charges (Panel B). Although pre-treatment estimates are consistently small in magnitude and not statistically different from zero across both outcomes and estimators, the CS estimates suggest a modest pre-existing downward trend in charges. We therefore put more weight on the SDID estimate for this outcome.

The SDID estimate in Table 5, Panel B suggests that inpatient charges for non-delivery stays in the first year of life fall by $583 per birth. This effect represents around a 17% reduction relative to the period prior to policy adoption, or 11% relative to the full sample mean. Changes in hospital charges may occur due to changes in quantities *or* prices of services. For this finding to reflect price changes, prices would have to systematically change at the same time as policy adoption, which is unlikely. Therefore, we interpret this result as suggestive evidence of a reduction in treatment intensity.

Given the small number of states and small number of treated units in the HCUP sample, in Appendix Table A14 we present the p-values from the t-statistic-based randomization inference procedure discussed in Section 5.2. The reduction in inpatient charges continues to be statistically significant using this alternate inference method.

Analyses by Primary Diagnosis We next examine effects on hospitalization rates and charges separately by primary diagnosis and present the results in Table 5, columns 2–9. The associated event-study estimates are in Appendix Figures A11 and A12. We find suggestive evidence of reductions in the hospitalization rate and charges for digestive-related causes (column 2), consistent with the literature documenting causal benefits of breast milk on digestive-related conditions.[46] We also find suggestive evidence of large declines in charges for causes originating in the perinatal period (Panel B, column 8) and congenital abnormalities (Panel B, column 7). For both the CS and SDID estimators, we find reductions in charges for immune-related causes (Panel B, column

are available by request.

[46] If anything, the effects on hospitalizations are likely conservative given the estimated declines in infant mortality. If the infants whose deaths were averted due to the policies have relatively worse underlying health, they may be at increased risk of hospitalization. This is likely why, in some specifications, we observe a significant increase in hospitalizations due to causes originating in the perinatal period.

3). These findings complement the estimated declines in mortality from perinatal causes and among premature infants and provide further suggestive evidence that breast milk's benefits for immune system development may be an important mechanism.

Finally, we find reductions in charges due to external causes (Panel B, column 5), consistent with the estimated declines in mortality from those same causes and suggesting that household behavioral changes are also at play. The p-values associated with these estimates from the t-statistic-based randomization inference procedure are presented in Appendix Table A14. Overall, the randomization inference results are broadly consistent with the baseline inference and continue to support evidence of reductions in charges across several diagnosis categories.

6.4 Breastfeeding Results

We investigate the impacts of state hospital breastfeeding support policies on self-reported breastfeeding outcomes using data from the NIS-Child, collapsed to the state-of-residence (at birth)/birth-year level. Descriptive statistics are presented in Table 6. Column (1) presents means and standard deviations for the full sample; columns (2) and (3) present statistics for the set of states that are ever treated or never treated during the NIS-Child sample period, respectively. On average across our sample period, 76% of infants are ever breastfed. By 3 months postpartum, only 59% are still breastfed, and this share declines to 44% by 6 months and 23% by one year after birth. Across all four breastfeeding measures, a slightly higher share of infants are breastfed in ever-treated states relative to infants in never-treated states. In general, observed infant and maternal characteristics are similar in the ever- and never-treated states. However, as in the Vital Statistics data, infants in never-treated states are relatively more likely to be non-Hispanic white; mothers in never-treated states are also relatively older.

The event-study estimates from the CS and SDID estimators are presented in Figure 5. For both estimators, there is a significant and sustained increase in breastfeeding initiation and breastfeeding at 3 months after policy adoption. The CS event-study estimates show that breastfeeding rates were differentially decreasing in treatment states relative to never-treated states prior to

policy adoption. These differential pre-treatment trends are substantially attenuated using the SDID estimator. Thus, for breastfeeding outcomes, we discuss both the CS and SDID results, but we put more weight on the SDID estimates.[47]

Table 7 shows that policy adoption resulted in significant increases in breastfeeding initiation and duration in the medium term. Specifically, the share of infants ever breastfed increased by 3.3–4.1 percentage points (column 1), a 4.2–5.2% increase relative to the mean in the period prior to policy adoption. We also find that the share breastfeeding at 3 months and 6 months postpartum increased by 4–6 and 1–3 percentage points (columns 2 and 3), respectively, or by 6–9% and 2–6.5% relative to the relevant pre-treatment period means. However, the effect on breastfeeding at 6 months is only significant for the CS estimator, and both estimators suggest that effects largely fade out by one year postpartum (column 4). In Appendix Table A15 we show that these conclusions are unchanged if we alternatively use the t-statistic-based randomization inference procedure.

To interpret these findings, we scale our estimates by the 9.63 million observed births in treated states in the 0–4 years after policy adoption. Our estimates conservatively imply that state-level hospital breastfeeding policies led to about 315,000 additional infants from those cohorts ever breastfed and 360,000 additional infants breastfed at 3 months. We also compare the dynamics of the breastfeeding effects with those for infant mortality. Although the point estimates for breastfeeding initiation are visually suggestive of increasing effects over time, we cannot reject an effect equal to the corresponding medium-term average treatment effect (3.3–4.1 percentage points) in any single post-treatment period. For breastfeeding at 3 months, the estimated effect in event-year 0 is modestly smaller than the medium-term effect, although we cannot rule out meaningful increases in the range of 3.6–3.9 percentage points. In event-years 1–4, we find relatively constant and sustained increases in the share of infants breastfed at 3 months. Taken together, these patterns are broadly consistent with the immediate and sustained declines in infant mortality that we observe over the first 5 years after policy adoption.

The results underscore that these regulations achieved their intended goal of promoting breast-

[47] If we assume that in the absence of policy adoption breastfeeding rates in the treated states would have continued to decrease relative to the control states, our estimates would tend to underestimate the true treatment effect.

feeding and support the idea that direct health benefits of breast milk consumption contribute to the infant mortality declines. However, we caution against combining the mortality declines with the breastfeeding increases to calculate an implied two-stage least squares estimate. As previously mentioned, these policies may impact infant mortality through multiple mechanisms, including changes in other household behaviors. Lawler and Yewell (2023) find these policies increase the amount of time mothers spend providing child care, and we show in Section 6.5 that infant sleep practices change as well. Behaviors that we are unable to directly examine may have also changed, such as maternal substance use. Furthermore, the estimated increases in breastfeeding only reflect changes in *extensive* margin breastfeeding. They do not capture changes in breastfeeding intensity (e.g., mix of breast milk and formula), which may also be important for infant health.

Effect Heterogeneity by Race/Ethnicity In Table 7, Panels B and C, we show the medium-term effects of hospital breastfeeding support policies on breastfeeding outcomes separately by infant race and ethnicity. While we find evidence of modest increases in breastfeeding initiation and duration among white infants, effects are substantially larger for non-white infants. Among non-white infants, breastfeeding initiation increases by 4.3–6.1 percentage points and breastfeeding at 3 months increases by 5.3–7.7 percentage points, representing increases of 5.5–7.9% and 8.7–12.7%, respectively, relative to the period before policy adoption. The randomization inference results also provide stronger evidence of these increases among non-white than white infants (Appendix Table A15). Thus, as with infant mortality, the breastfeeding effects are concentrated among non-white infants. The event-study estimates for breastfeeding initiation and duration through 3 months postpartum are shown in Figure 6; Appendix Figure A10 displays them for duration through 6 months and one year postpartum.

6.5 Infant Sleep Practices Results

We next examine the effects of state adoption of hospital breastfeeding support policies on infant sleep practices. Although the regulations do not explicitly target sleep practices, information on

recommended sleep behavior is frequently conveyed in tandem with information on breastfeeding (see for example, Appendix Figure A2), and International Board Certified Lactation Consultants (IBCLCs) are expected to have competency in safe sleep and minimizing the risk of SIDS (IBCLC Commission, 2023). Thus, the policies may increase exposure to information about safe sleep. Additionally, increased breastfeeding may change household preferences about sleeping arrangements.

To investigate this question, we use self-reported data from the PRAMS on the infant's usual sleep position and the frequency with which the infant shares a bed with another individual (recorded as never, rarely, sometimes, often, or always). The bed-sharing question was included in an optional module for a limited set of state-years, leaving only two states that provide identifying variation: New York (policy adopted in September 2005) and New Jersey (policy adopted in January 2014).[48] To prevent changes in sample composition from impacting our results, we restrict the sample to be balanced around event time for each treated unit. The limited set of available state-years also means that we can only identify effects for a short post-treatment period that includes the year of and the year following policy adoption.[49] Furthermore, it is not possible to construct a sample that includes both treated states and is balanced around *calendar* time (a requirement for the SDID estimator). Therefore, in the main text we report only CS estimates; in the appendix we report separate CS and SDID estimates of the treatment effect for New York and New Jersey.

The results in Table 8, Panel A provide suggestive evidence of improvements in infant sleep practices following policy adoption. In the full sample, the share of infants who usually sleep on their back increases by 1.3 percentage points, although the estimate is not statistically significant, while the share who never bed-share increases by a statistically significant 2.7 percentage points. We find little change in often or always bed-sharing, implying that the increase in never bed-sharing

[48]New York City and New York State (excluding New York City) run two separate PRAMS surveys. The bed-sharing questions are only asked in the New York City PRAMS questionnaire.

[49]For New York City, the bed-sharing question is only available from 2004–2007 (two years prior to one year after policy adoption). The control states for NYC are therefore AK, ME, MI, MN, NE, OR, UT, VT, WA, and WV. For New Jersey, the question is available from 2002–2015 (more than four years prior to one year after policy adoption). As there is a trade-off between having more control states versus more pre-treatment periods in our balanced sample, we require control states to have data for at least 2011–2015 (three years prior to one year after policy adoption). Therefore, DE, NE, VT, WA, WV, and WI serve as control states for NJ. See Appendix Table A2 for information on the full set of state-years available in the PRAMS sample.

primarily reflects reductions in the omitted rarely or sometimes categories.

Appendix Table A16 shows that the increase in back sleeping is driven by New York City, whereas changes in bed-sharing are driven by New Jersey. These heterogeneous effects across states may reflect the evolving nature of infant sleep recommendations. The American Academy of Pediatrics (AAP) took a much stronger stance against bed-sharing by the time of New Jersey's policy adoption (January 2014) than at the time of New York's (September 2005) (Kattwinkel et al., 2000, 2005; Moon et al., 2011).

In Table 8, Panels B and C, we show that the increase in back sleeping is concentrated among infants born to non-white mothers. Reductions in bed-sharing are observed for both white and non-white mothers. Notably, non-Hispanic white mothers report higher rates of safe sleep practices during the pre-treatment period. Approximately 70% of white mothers report that their infant usually sleeps on their back, and 47% report that their infant never bed-shares, compared with 52% and 28%, respectively, among non-white mothers. We report p-values from t-statistic-based randomization inference in Appendix Table A17. These results have relatively low power given the very small number of treated states. Nevertheless, the increase in back sleeping among infants born to non-white mothers remains statistically significant using a one-sided test.

Event-study figures, presented in Appendix Figure A13 for the pooled sample and separately by treatment state in Appendix Figure A14, do not suggest obvious pre-treatment differential trends for back sleeping, although the limited number of available pre-treatment periods makes it difficult to assess such trends. Furthermore, the estimates are identified from only two adopting states (New York and New Jersey) and for a limited set of post-treatment periods (i.e., year of and one year after policy adoption). We therefore interpret these results as suggestive. Together with the estimated decline in SUID, these results nevertheless suggest that improvements in safe infant sleep practices may have contributed to the improvements in infant health.

7 Conclusion

We examine how state-level hospital regulations intended to promote breastfeeding impact infant mortality. Focusing on effects in the 0–4 years after policy adoption, we find mortality in the first year of life decreased by about 0.2 deaths per 1,000 live births, a 3.5% decline relative to the pre-treatment period. The mortality declines were concentrated among infants born to non-white mothers and among medically vulnerable infants.

Our findings indicate that these low-complexity hospital-based interventions improved infant health through several channels. Mortality declines were particularly large among premature infants and due to causes originating in the perinatal period, and we find suggestive evidence that policy adoption reduced infant hospitalization charges associated with digestive, immune-related, and perinatal diagnoses. These patterns point to a potentially important role for the direct health benefits of breast milk. Supporting this interpretation, we find that policy adoption increased breastfeeding initiation and duration, with the largest and most persistent increases occurring among non-white infants.

At the same time, mortality due to external causes and sleep-related causes declined following policy adoption, and we find suggestive evidence that infant sleep practices improved, particularly among infants born to non-white mothers. Together with prior evidence showing that policy adoption increased maternal child care provision (Lawler and Yewell, 2023), these findings are consistent with broader household behavioral responses contributing to the observed improvements in infant health.

While we cannot formally decompose the mechanisms underlying the mortality declines, we provide suggestive evidence on their relative importance. Our estimates imply that 2,090–2,234 infant deaths were averted in treated states in the 0–4 years after policy adoption, of which 1,223–1,444 would have occurred due to causes originating in the perinatal period (e.g., disorders relating to short gestation and low birth weight) and 356–375 would have occurred due to external causes.[50] As deaths due to external causes are plausibly more influenced by household behaviors

[50] See Section 5.1 for details on these calculations.

than by breast milk consumption, these figures provide some sense of the potential contribution of changes in household behaviors, such as safe sleep practices and maternal time allocated to child care (Lawler and Yewell, 2023), to the overall mortality decline.

Our results complement and extend the causal literature on breastfeeding-support interventions. Prior studies have often reported sizable but imprecise reductions in infant mortality from such interventions. Many of these studies were underpowered to detect statistically significant changes in rare outcomes and/or systematically excluded medically vulnerable infants (Kramer et al., 2001; Quigley et al., 2019).[51] By contrast, we use U.S. administrative infant death records to examine an intervention adopted by 13 states, in which approximately 50% of all U.S.-born infants reside. This allows us to detect economically meaningful declines in infant mortality and show that some of the largest improvements occur among medically vulnerable infants.

The magnitude of our estimated infant mortality effects is comparable to those of other interventions targeting the immediate postpartum period. For example, Rossin (2011) finds that the 1993 Family and Medical Leave Act (FMLA) reduced infant mortality by 0.20 deaths per 1,000 births among likely-eligible women, with declines in mortality due to ill-defined causes accounting for the vast majority of the reduction (0.16 deaths per 1,000 births). Similarly, Chen (2023) finds that California's adoption of paid family leave in 2004 reduced postneonatal mortality by 0.14 deaths per 1,000 births. Larger effects have been documented for interventions that directly target infant sleep practices. For example, Altindağ et al. (2024) find that a nationwide Danish information campaign in 1991 that encouraged parents to put infants to sleep on their back or side (instead of their stomach) lowered infant mortality by 1.1 to 1.4 deaths per 1,000 births. This effect was entirely driven by reductions in mortality due to SIDS and other unknown causes, and

[51] In analyses of the Belarusian PROBIT study (N =16,491), which excluded premature and low birth weight infants, Kramer et al. (2001) find that the overall infant mortality rate per 1,000 live births was 2.3 among treatment group infants versus 3.7 among control group infants, but the difference was not statistically significant. The Quigley et al. (2019) meta-analysis examines RCTs comparing in-hospital feeding with donor breast milk versus formula among premature or low weight infants with insufficient maternal breast milk. They report 85.6 deaths per 1,000 births among infants fed donor breast milk and 93.7 deaths per 1,000 births among infants fed formula, but the difference was not statistically significant. However, five of the seven studies included in that meta-analysis focused on mortality prior to the initial hospital discharge (with a pooled sample size of 1,025 infants). Only two of the studies examined mortality at 9 months postpartum (with a pooled sample of slightly over 500 infants).

was over ten times larger for infants born low weight or premature.

Our findings are not without limitations. As discussed, we provide suggestive evidence that both the direct health benefits of breast milk and broader household behavioral responses contribute to the observed improvements in infant health, but we cannot precisely quantify the relative importance of the channels or examine all potentially relevant behaviors (e.g., intensive margin changes in breastfeeding, maternal substance use). Furthermore, as hospitals increasingly provide lactation support independent of state requirements and as knowledge of safe infant sleep practices becomes more widespread, the effects of future postpartum support policies may differ from those we estimate here.

Despite these limitations, our results highlight that low-complexity hospital-based interventions during the immediate postpartum period can have meaningful effects on infant health. The delivery hospitalization may be a particularly valuable opportunity to influence parental behaviors and improve infant outcomes. Several important questions remain. In particular, understanding why the mortality declines, increases in breastfeeding, and improvements in sleep practices were concentrated among non-white infants is an important area for future research. One possibility is that these regulations represented a larger change in the information and support available to non-white mothers, especially if hospitals serving primarily white patients already offered this support. Another is that households differed in how they responded to the additional breastfeeding support and infant-care guidance received during the delivery stay. Understanding the sources of this heterogeneity as well as the effects of these policies on maternal outcomes and long-run child outcomes are important avenues for future work.

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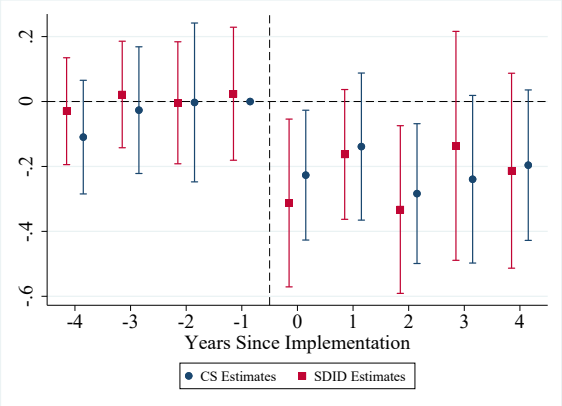
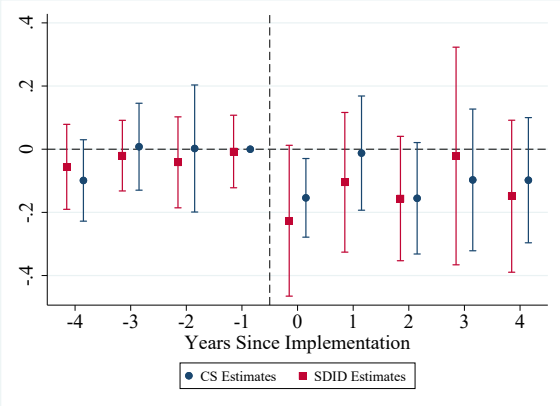
Figures

Figure 1: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

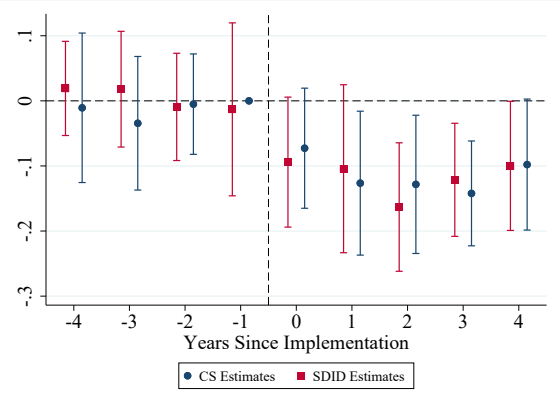
(a) One-Year Mortality Rate



(b) Neonatal Mortality Rate



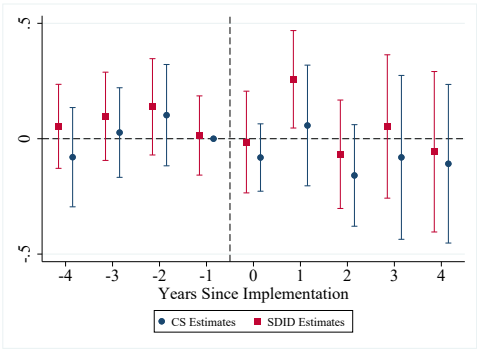
(c) Postneonatal Mortality Rate



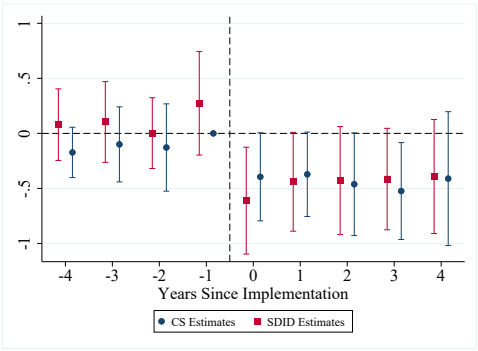
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths per 1,000 live births within the first year of life in panel (a), the first 28 days in panel (b), and days 28–364 in panel (c). For the CS estimator, observations are weighted by the number of births in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure 2: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Infant Mortality by Maternal Race/Ethnicity, Cohort Linked Birth-Infant Death Data (1995-2018)

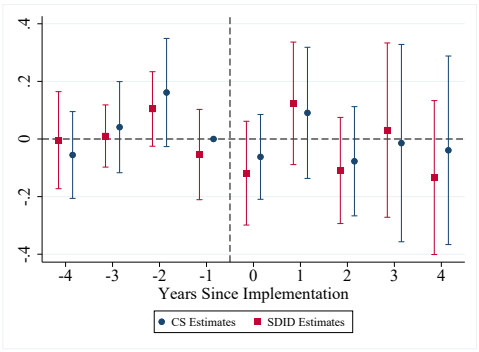
(a) One-Year Mortality Rate, Infants of White Mothers



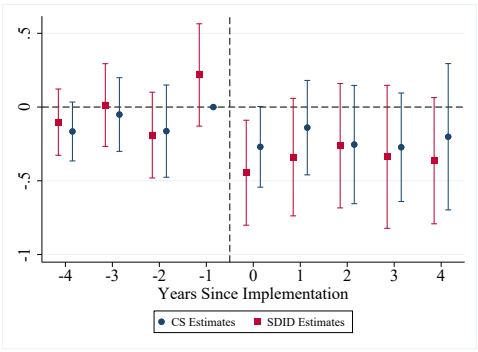
(b) One-Year Mortality Rate, Infants of Non-White Mothers



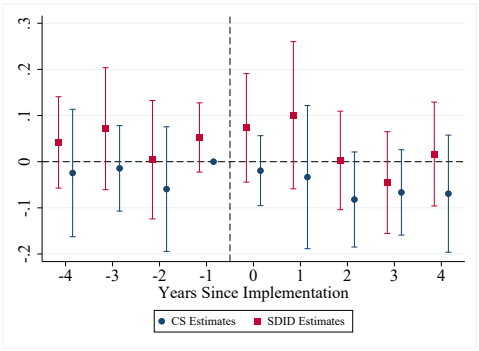
(c) Neonatal Mortality Rate, Infants of White Mothers



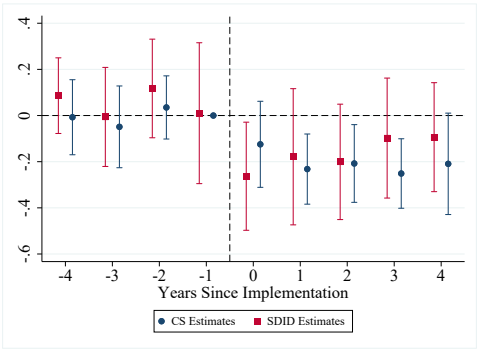
(d) Neonatal Mortality Rate, Infants of Non-White Mothers



(e) Postneonatal Mortality Rate, Infants of White Mothers



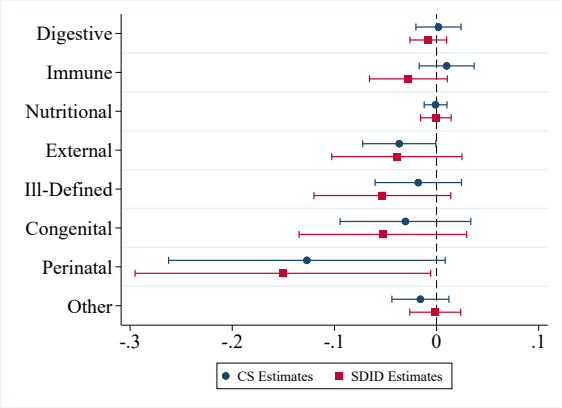
(f) Postneonatal Mortality Rate, Infants of Non-White Mothers



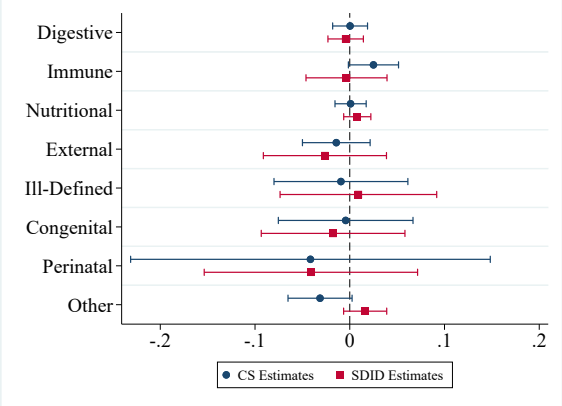
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Panels (a), (c), and (e) show one-year, neonatal, and postneonatal mortality, respectively, among infants born to non-Hispanic white mothers; panels (b), (d), and (f) show the corresponding outcomes among infants born to non-white mothers (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the number of births in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure 3: Effects of Hospital Breastfeeding Support Policies on One-Year Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995-2018)

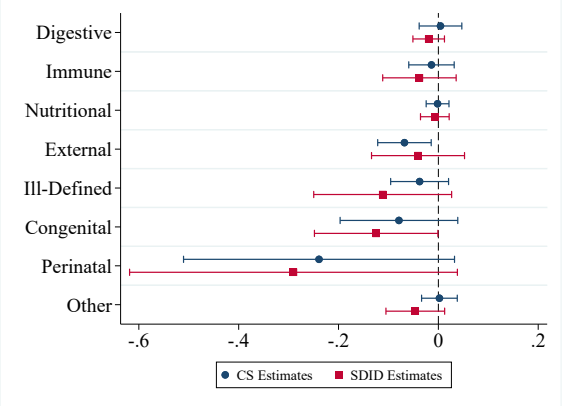
(a) Full Sample



(b) Infants of White Mothers



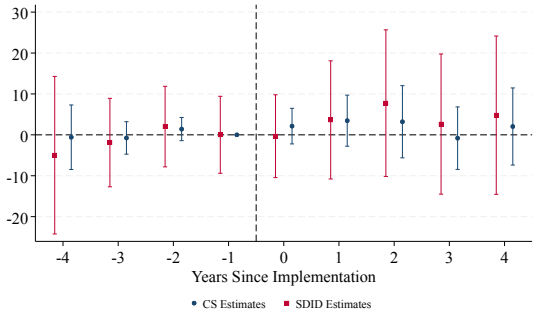
(c) Infants of Non-White Mothers



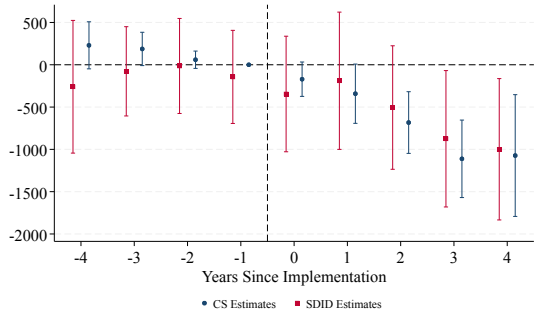
Note: Each point within a panel presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS (blue circles) and SDID (red squares) estimators, with horizontal lines showing 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life by underlying cause per 1,000 live births. Panel (a) includes the full sample; panels (b) and (c) include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. For the CS estimator, observations are weighted by the number of births in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure 4: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Inpatient Hospitalizations, HCUP (2000-2019)

(a) Non-Delivery Stays, Per 1,000 Births

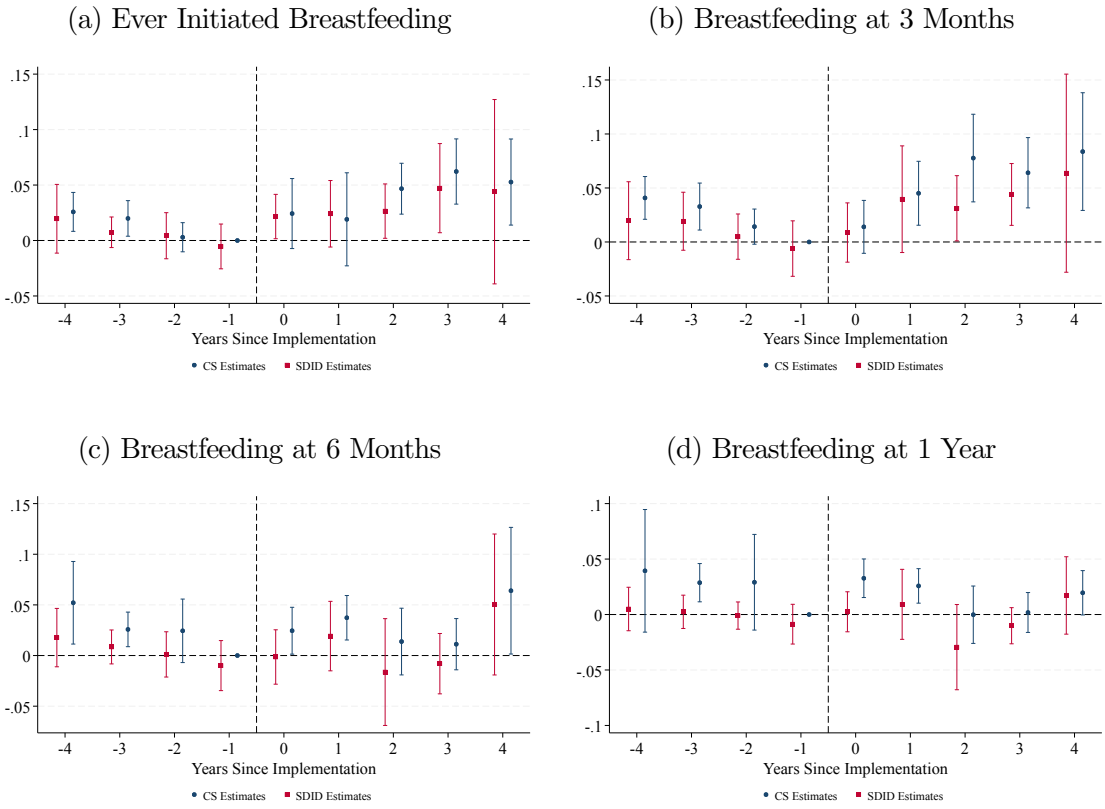


(b) Charges Per Birth, Non-Delivery Stays



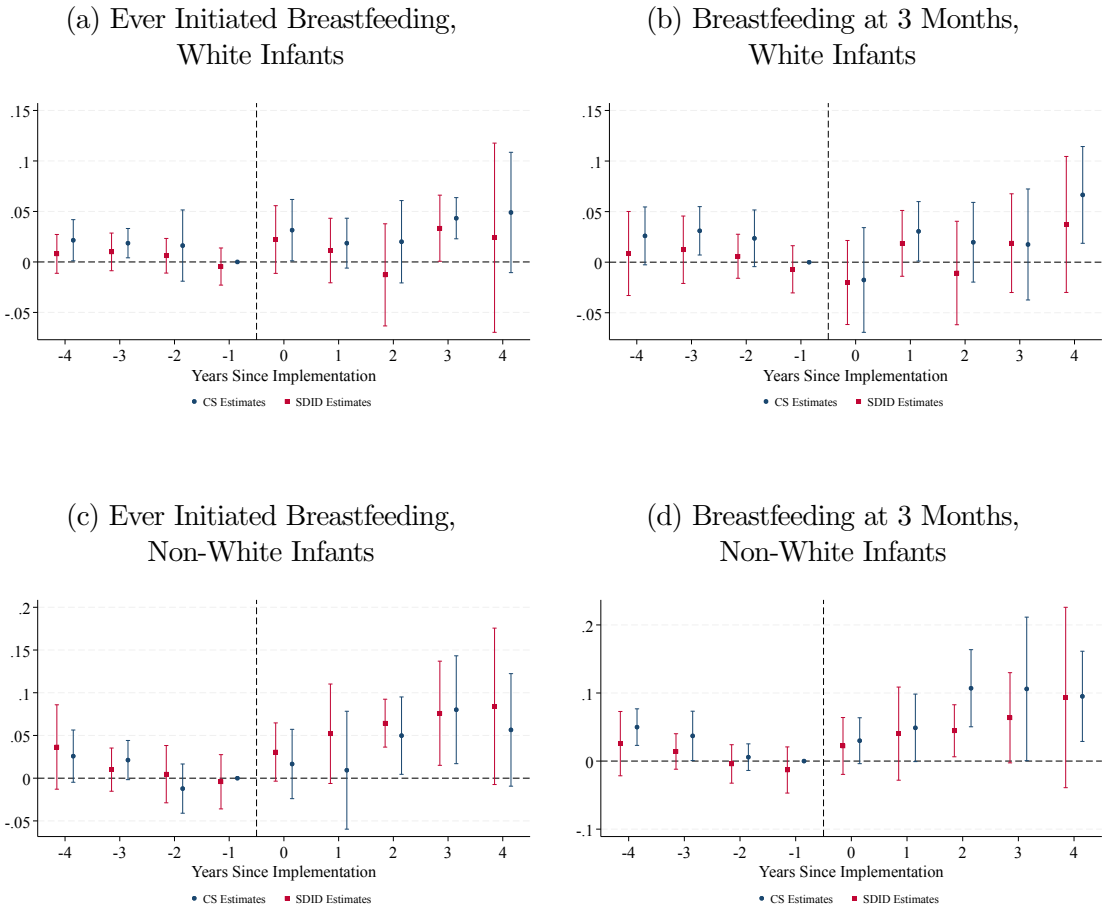
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence/hospital-state/discharge-year cell, and never-treated states are the control group. Outcomes are non-delivery inpatient stays per 1,000 births in panel (a) and non-delivery inpatient charges per birth in panel (b). For the CS estimator, observations are weighted by the number of deliveries in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence/hospital-state level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure 5: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes, NIS-Child (2003-2017)



Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the shares of infants ever breastfed in panel (a) and breastfed at 3 months, 6 months, and 1 year in panels (b), (c), and (d), respectively. For the CS estimator, observations are weighted by the sum of the NIS-Child sampling weights in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure 6: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes by Race/Ethnicity, NIS-Child (2003-2017)



Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the shares of infants ever breastfed in panels (a) and (c) and breastfed at 3 months in panels (b) and (d). Panels (a) and (b) include non-Hispanic white infants, and panels (c) and (d) include non-white infants (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the sum of the NIS-Child sampling weights in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Tables

Table 1: Descriptive Statistics, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) Full Sample	(2) Ever-Treated States	(3) Never-Treated States
Infant Mortality Rates (Per 1,000 Live Births)			
One-Year	6.469	6.302	6.681
	(1.344)	(1.382)	(1.263)
Neonatal	4.297	4.215	4.402
	(0.904)	(0.915)	(0.880)
Postneonatal	2.172	2.087	2.279
	(0.560)	(0.559)	(0.544)
Infant Health at Birth			
Share Born Premature	0.119	0.119	0.119
	(0.016)	(0.017)	(0.015)
Share Born Low Birth Weight	0.080	0.080	0.079
	(0.011)	(0.011)	(0.011)
Mothers' Characteristics			
Share Non-Hispanic White	0.556	0.482	0.650
	(0.175)	(0.158)	(0.148)
Share Less than High School	0.195	0.212	0.174
	(0.062)	(0.065)	(0.049)
Share High School	0.288	0.284	0.295
	(0.044)	(0.038)	(0.050)
Share Some College	0.249	0.236	0.266
	(0.044)	(0.039)	(0.045)
Share College Degree	0.267	0.268	0.266
	(0.063)	(0.063)	(0.062)
Share Under Age 20	0.096	0.097	0.095
	(0.034)	(0.035)	(0.033)
Share Age 20–29	0.517	0.503	0.535
	(0.048)	(0.047)	(0.044)
Share Age 30–39	0.360	0.372	0.346
	(0.065)	(0.067)	(0.060)
Share Over Age 39	0.026	0.029	0.023
	(0.009)	(0.010)	(0.007)
Share Married	0.626	0.618	0.637
	(0.062)	(0.050)	(0.072)
Share Giving Birth Out of State	0.021	0.019	0.025
	(0.022)	(0.021)	(0.023)
State-Birth Year Observations	1,152	312	840

Note: The unit of observation is a state-of-residence (at birth)/year-of-birth cell. Each cell reports a weighted mean, with the standard deviation in parentheses. Means are computed using observations with non-missing values and weighted by the number of live births with non-missing information on the corresponding measure. Always-treated states are dropped from the sample and include Arkansas, Kansas, and Massachusetts. Ever-treated states are California, Georgia, Illinois, Louisiana, Maryland, Mississippi, Missouri, New Jersey, New York, Ohio, Pennsylvania, South Carolina, and Texas.

Table 2: Hospital Breastfeeding Support Policy Effects on Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) One-Year	(2) Neonatal	(3) Postneonatal
Panel A: Full Sample			
CS ATT	-0.217**	-0.103	-0.114***
	(0.093)	(0.068)	(0.043)
SDID ATT	-0.232**	-0.132	-0.117***
	(0.105)	(0.094)	(0.035)
Pre-Treatment Mean	6.216	4.213	2.003
Panel B: Infants of White Mothers			
CS ATT	-0.075	-0.020	-0.054
	(0.114)	(0.103)	(0.050)
SDID ATT	0.034	-0.041	0.030
	(0.083)	(0.072)	(0.043)
Pre-Treatment Mean	5.086	3.365	1.721
Panel C: Infants of Non-White Mothers			
CS ATT	-0.433**	-0.228	-0.205***
	(0.201)	(0.161)	(0.071)
SDID ATT	-0.457**	-0.350**	-0.167**
	(0.179)	(0.156)	(0.082)
Pre-Treatment Mean	7.182	4.920	2.262

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths per 1,000 live births within the first year of life in column (1), the first 28 days in column (2), and days 28–364 in column (3). Panel A includes the full sample; Panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. For the CS estimator, observations are weighted by the number of births in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table 3: Hospital Breastfeeding Support Policy Effects on One-Year Infant Mortality by Health at Birth, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) Premature	(2) Low Birth Weight
Panel A: Full Sample		
CS ATT	-1.656**	-1.217
	(0.653)	(0.863)
SDID ATT	-1.487**	-1.114
	(0.749)	(0.843)
Pre-Treatment Mean	36.552	52.261
Panel B: Infants of White Mothers		
CS ATT	-0.432	0.048
	(0.789)	(1.251)
SDID ATT	0.278	0.418
	(0.638)	(0.901)
Pre-Treatment Mean	30.725	46.194
Panel C: Infants of Non-White Mothers		
CS ATT	-2.909**	-2.235
	(1.224)	(1.412)
SDID ATT	-2.076*	-2.675*
	(1.152)	(1.529)
Pre-Treatment Mean	40.523	55.894

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life per 1,000 premature births (less than 37 weeks gestation) in column (1) and per 1,000 low-birth-weight births (less than 2500 grams) in column (2). Panel A includes the full sample; Panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. For the CS estimator, observations are weighted by the number of premature or low-birth-weight births in each cell, as appropriate. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table 4: Hospital Breastfeeding Support Policy Effects on One-Year Sleep-Related Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) All	(2) Premature
Panel A: Full Sample		
CS ATT	-0.040	-0.244**
	(0.029)	(0.106)
SDID ATT	-0.058*	-0.212**
	(0.032)	(0.099)
Pre-Treatment Mean	0.848	1.849
Panel B: Infants of White Mothers		
CS ATT	-0.032	-0.205
	(0.041)	(0.140)
SDID ATT	0.017	-0.144
	(0.040)	(0.115)
Pre-Treatment Mean	0.794	1.697
Panel C: Infants of Non-White Mothers		
CS ATT	-0.064*	-0.301**
	(0.038)	(0.122)
SDID ATT	-0.095	-0.426**
	(0.074)	(0.189)
Pre-Treatment Mean	0.898	1.978

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths due to SUID per 1,000 live births in column (1) and per 1,000 premature births (less than 37 weeks gestation) in column (2). Panel A includes the full sample; Panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. For the CS estimator, observations are weighted by the corresponding number of births in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table 5: Hospital Breastfeeding Support Policy Effects on Inpatient Hospitalizations, HCUP (2000-2019)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		By Primary Diagnosis							
	Overall	Digestive	Immune-related	Nutrition-related	External Cause	Ill-defined	Congenital	Perinatal	Other
Panel A: Hospitalization Rate per 1,000 Births									
CS ATT	2.005	-1.590**	1.148	0.602	-0.266	-0.114	-0.00346	1.877***	0.0114
	(2.848)	(0.794)	(2.382)	(0.547)	(0.187)	(0.438)	(0.257)	(0.711)	(0.354)
SDID ATT	3.702	-0.768	1.633	0.158	-0.053	-0.029	0.480	0.648	-0.027
	(7.344)	(0.875)	(3.529)	(0.968)	(0.162)	(0.553)	(0.513)	(2.537)	(0.418)
Pre-Treatment Mean	134.7	9.571	55.10	6.409	4.496	11.98	9.549	27.74	8.298
Panel B: Average Charges per Birth									
CS ATT	-676.1***	-49.74***	-137.1**	-15.59	-30.52**	-13.46	-199.2**	-206.1**	-28.99*
	(189.1)	(10.19)	(53.62)	(15.82)	(12.88)	(11.68)	(78.24)	(85.38)	(15.33)
SDID ATT	-582.7*	-28.55	-161.7**	-6.555	-32.25*	-33.22	-96.34	-198.9	-20.09
	(340.259)	(20.471)	(69.355)	(18.114)	(16.671)	(23.107)	(167.752)	(240.683)	(36.096)
Pre-Treatment Mean	3,491.8	174.6	992.4	105.6	123.4	164.1	580.3	1,131.3	177.3

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence/hospital-state/discharge-year cell, and never-treated states are the control group. Outcomes are non-delivery inpatient hospitalizations per 1,000 births in Panel A and non-delivery inpatient charges per birth in Panel B, overall in column (1) and by primary diagnosis in columns (2)–(9). For the CS estimator, observations are weighted by the number of deliveries in each cell. Standard errors are clustered at the state-of-residence/hospital-state level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table 6: Descriptive Statistics, NIS-Child (2003-2017)

	(1) Full Sample	(2) Ever-Treated States	(3) Never-Treated States
Breastfeeding Outcomes			
Share Ever Breastfed	0.763	0.766	0.760
	(0.095)	(0.098)	(0.092)
Share Breastfed, 3 Months	0.589	0.597	0.579
	(0.113)	(0.115)	(0.111)
Share Breastfed, 6 Months	0.444	0.448	0.438
	(0.104)	(0.103)	(0.105)
Share Breastfed, 1 Year	0.226	0.229	0.223
	(0.074)	(0.073)	(0.075)
Child Characteristics			
Share Female	0.488	0.488	0.487
	(0.025)	(0.021)	(0.028)
Share Firstborn	0.419	0.417	0.421
	(0.051)	(0.050)	(0.053)
Share Ever Received WIC	0.553	0.576	0.527
	(0.088)	(0.077)	(0.092)
Share Non-Hispanic White	0.480	0.394	0.577
	(0.166)	(0.129)	(0.149)
Mothers' Characteristics			
Share High School or Less	0.482	0.496	0.466
	(0.081)	(0.077)	(0.082)
Share At Least Some College	0.518	0.504	0.534
	(0.081)	(0.077)	(0.082)
Share Married	0.652	0.643	0.663
	(0.077)	(0.064)	(0.088)
Share Age: <29 Years	0.435	0.419	0.452
	(0.076)	(0.075)	(0.074)
State-Birth Year Observations	782	187	595

Note: The unit of observation is a state-of-residence (at birth)/year-of-birth cell. Each cell reports a weighted mean, with the standard deviation in parentheses. Means are weighted using the sum of the NIS-Child sampling weights in each cell (landline only weights for 2003-2011, dual-frame weights for 2012-2017). Always-treated states are dropped from the sample and include Arkansas, Kansas, Massachusetts, Missouri, and Pennsylvania. Ever-treated states are California, Georgia, Illinois, Louisiana, Maryland, New Jersey, New York, and Ohio.

Table 7: Hospital Breastfeeding Support Policy Effects on Breastfeeding Initiation and Duration, NIS-Child (2003-2017)

	(1) Breastfeeding Initiation	(2) Breastfeeding, 3 Months	(3) Breastfeeding, 6 Months	(4) Breastfeeding, 1 Year
Panel A: Full Sample				
CS ATT	0.0410***	0.0569***	0.0302***	0.0160***
	(0.0118)	(0.0084)	(0.0076)	(0.0046)
SDID ATT	0.033**	0.037***	0.009	-0.002
	(0.013)	(0.013)	(0.009)	(0.007)
Pre-Treatment Mean	0.781	0.617	0.463	0.241
Panel B: White Infants				
CS ATT	0.0325***	0.0233*	0.0002	-0.0126
	(0.0124)	(0.0135)	(0.0206)	(0.0139)
SDID ATT	0.016	0.009	-0.007	-0.016
	(0.018)	(0.015)	(0.014)	(0.012)
Pre-Treatment Mean	0.789	0.633	0.510	0.283
Panel C: Non-White Infants				
CS ATT	0.0425**	0.0773***	0.0481***	0.0367***
	(0.0213)	(0.0176)	(0.0162)	(0.0139)
SDID ATT	0.061***	0.053**	0.018	0.016
	(0.020)	(0.021)	(0.016)	(0.013)
Pre-Treatment Mean	0.776	0.608	0.432	0.214

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the shares of infants ever breastfed in column (1) and breastfed at 3 months, 6 months, and 1 year in columns (2), (3), and (4), respectively. Panel A includes the full sample; Panels B and C include non-Hispanic white and non-white infants (i.e., Black, other race, or Hispanic), respectively. For the CS estimator, observations are weighted by the sum of the NIS-Child sampling weights in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table 8: Hospital Breastfeeding Support Policy Effects on Infant Sleep Practices, PRAMS (2000-2018)

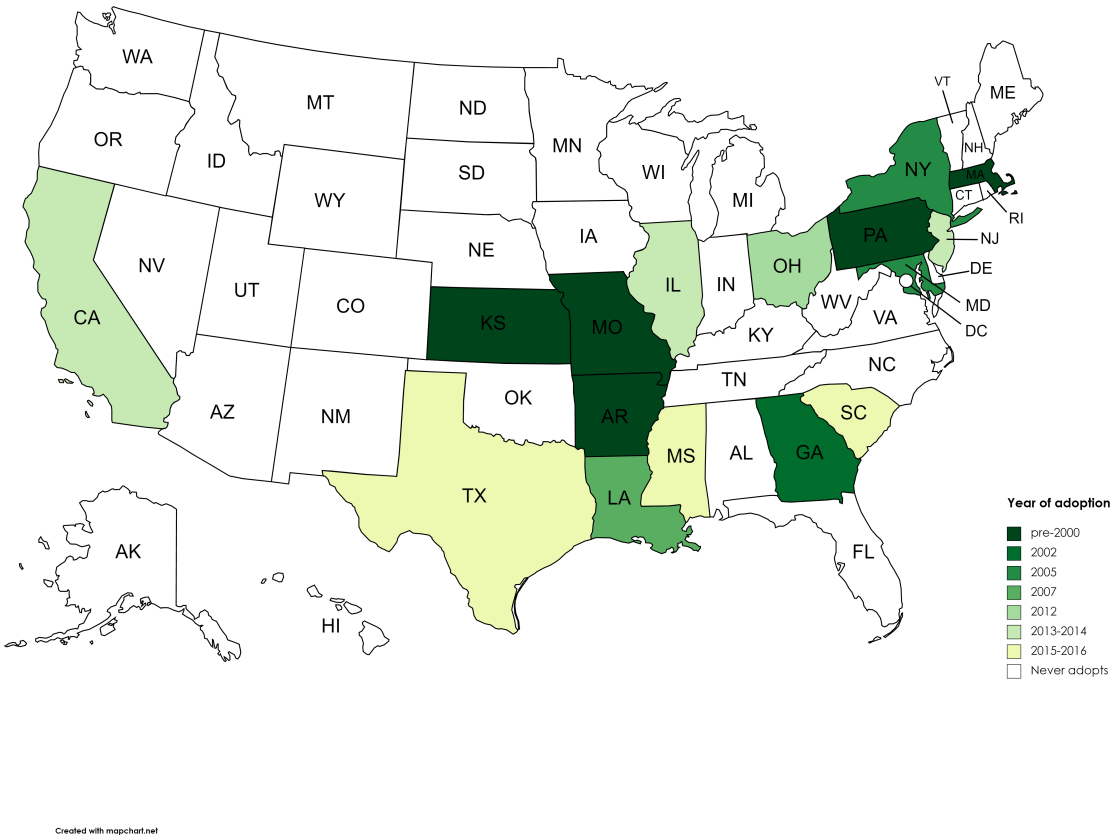
	(1) Usual Infant Sleep Position: Back	(2) Infant Bedshares, Never	(3) Infant Bedshares, Often or Always
Panel A: Full Sample			
CS ATT	0.0131	0.0272***	-0.00713
	(0.00845)	(0.00705)	(0.00670)
Pre-Treatment Mean	0.588	0.351	0.222
Panel B: White Mothers			
CS ATT	0.00435	0.0243**	0.00474
	(0.00521)	(0.0101)	(0.00716)
Pre-Treatment Mean	0.699	0.467	0.137
Panel C: Non-White Mothers			
CS ATT	0.0263***	0.0304***	0.000289
	(0.00734)	(0.0111)	(0.00638)
Pre-Treatment Mean	0.522	0.284	0.271

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 and 1 (inclusive) from the CS estimator, which represents the full set of post-treatment periods for which effects are identified. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the shares of infants whose usual sleep position is on their back in column (1), who never bed-share in column (2), and who often or always bed-share in column (3). Panel A includes the full sample; Panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. Observations are weighted by the sum of the PRAMS sampling weights in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap. SDID estimates are not reported because the sample is unbalanced in calendar time. See Appendix Table A2 for the full set of state-years available in the PRAMS sample.

Appendix A Appendix Figures and Tables

Figure A1: Timing of Adoption of State Hospital Breastfeeding Policies



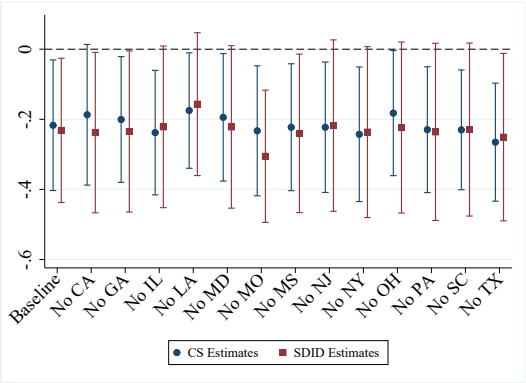
Note: The map shows the timing of adoption of state-level hospital breastfeeding policies. See Appendix Table A1 for exact month and year of adoption. For the empirical analysis, the year a policy is considered to be in effect may differ from the legal adoption year and is defined as follows. Policies adopted by June are coded as effective in that calendar year, while policies adopted after June are coded as effective in the following year. Thus, Missouri's effective policy year is 2000, Georgia's is 2003, New York's is 2006, and Mississippi's is 2017.

Figure A2: Michigan Department of Health Flyer for Breastfeeding and Safe Sleep

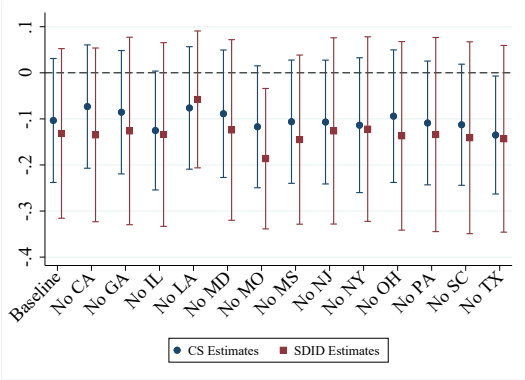
Source: https://www.michigan.gov/mdhhs/keep-mi-healthy/maternal-and-infant-health/breastfeeding/breastfeeding-and-safe-sleep. Last accessed: 09/10/2023.

Figure A3: Robustness of Hospital Breastfeeding Support Policy Effects on Infant Mortality from Leave-One-Out Exercise, Cohort Linked Birth-Infant Death Data (1995-2018)

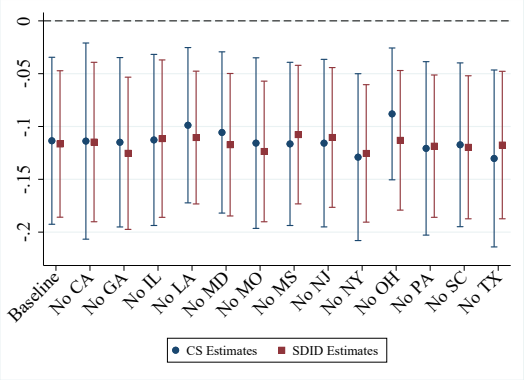
(a) One-Year Mortality Rate



(b) Neonatal Mortality Rate

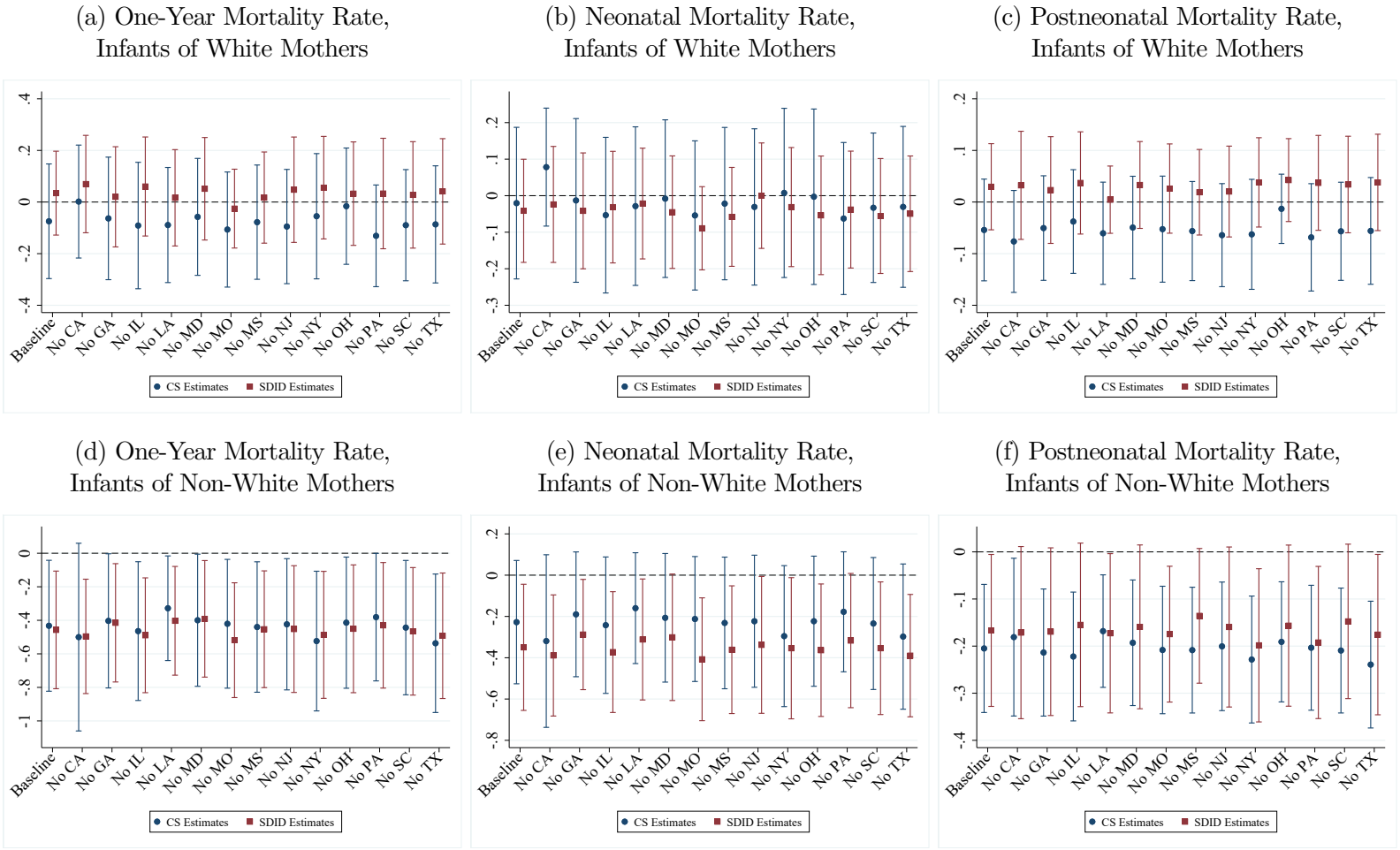


(c) Postneonatal Mortality Rate



Note: Each panel presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS (blue circles) and SDID (red squares) estimators for the baseline specification and when iteratively dropping each treated state. Vertical lines show 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths per 1,000 live births within the first year of life in panel (a), the first 28 days in panel (b), and days 28–364 in panel (c). For the CS estimator, observations are weighted by the number of births in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

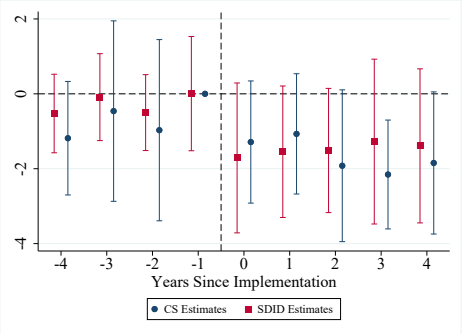
Figure A4: Robustness of Hospital Breastfeeding Support Policy Effects on Infant Mortality by Maternal Race/Ethnicity from Leave-One-Out Exercise, Cohort Linked Birth-Infant Death Data (1995-2018)



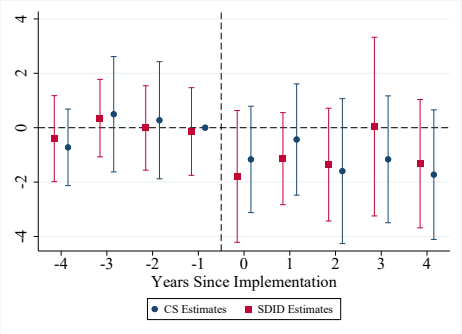
Note: Each panel presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS (blue circles) and SDID (red squares) estimators for the baseline specification and when iteratively dropping each treated state. Vertical lines show 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths per 1,000 live births within the first year of life, the first 28 days, and days 28–364 in panels (a)–(c), respectively, and again in panels (d)–(f), respectively. Panels (a)–(c) include infants born to non-Hispanic white mothers; panels (d)–(f) include infants born to non-white mothers (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the number of births in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A5: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on One-Year Mortality by Infant Health at Birth, Cohort Linked Birth-Infant Death Data (1995-2018)

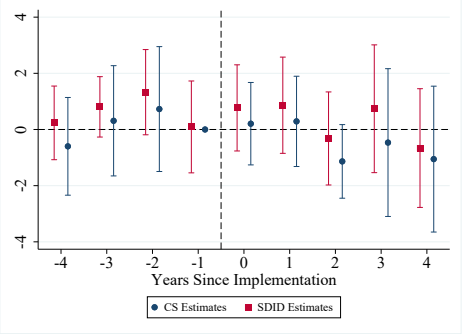
(a) Premature Mortality, Full Sample



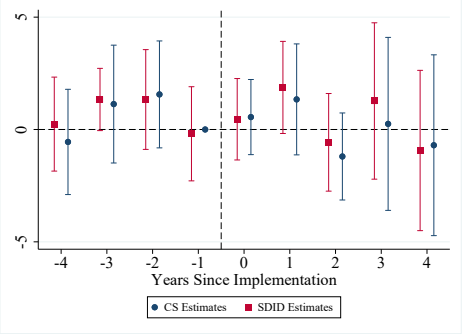
(b) Low Birth Weight Mortality, Full Sample



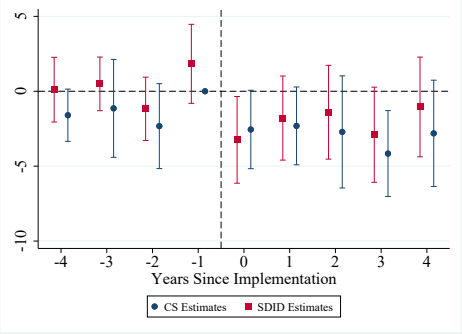
(c) Premature Mortality, Infants of White Mothers



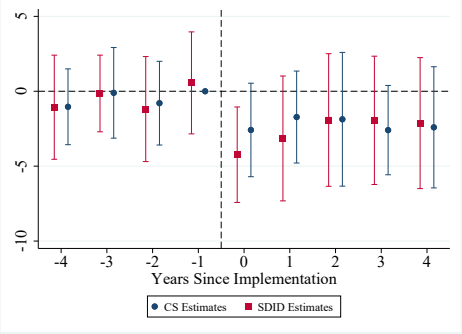
(d) Low Birth Weight Mortality, Infants of White Mothers



(e) Premature Mortality, Infants of Non-White Mothers

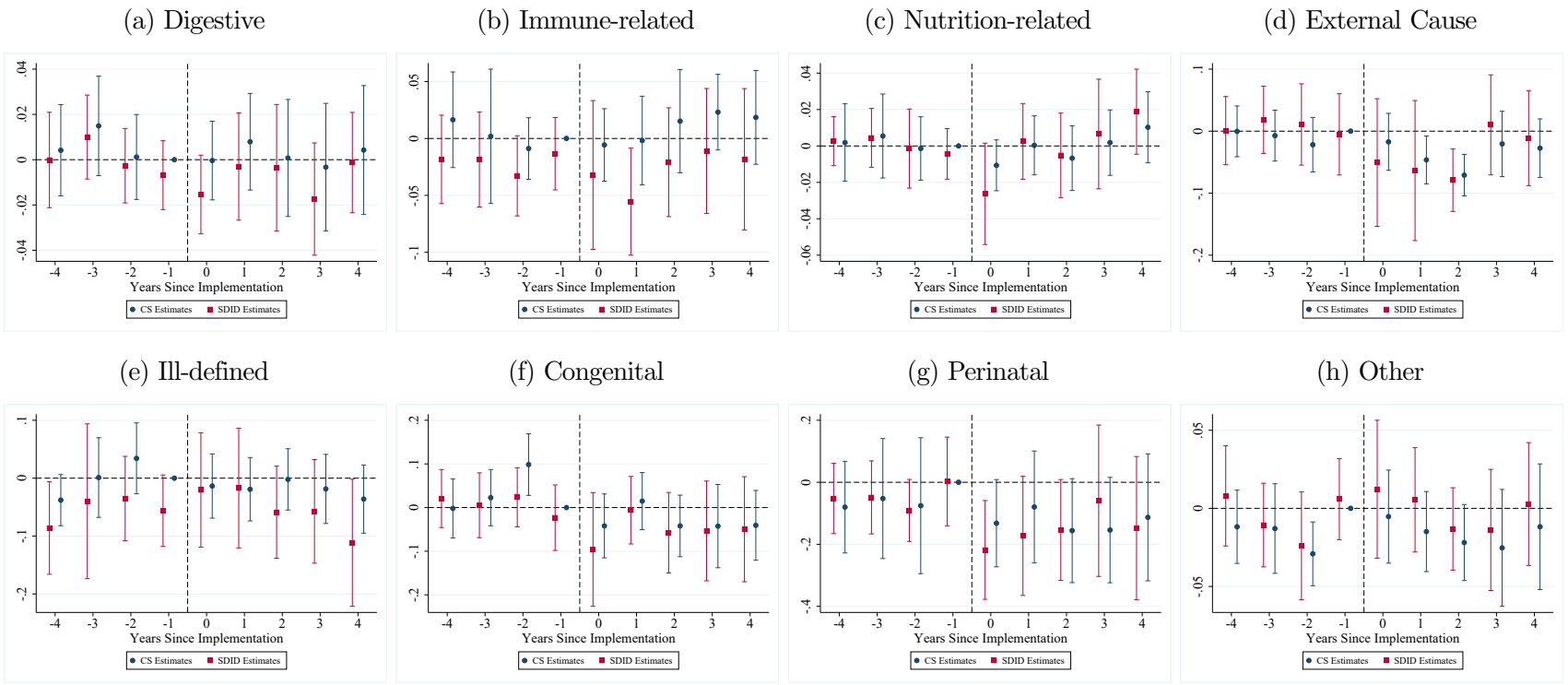


(f) Low Birth Weight Mortality, Infants of Non-White Mothers



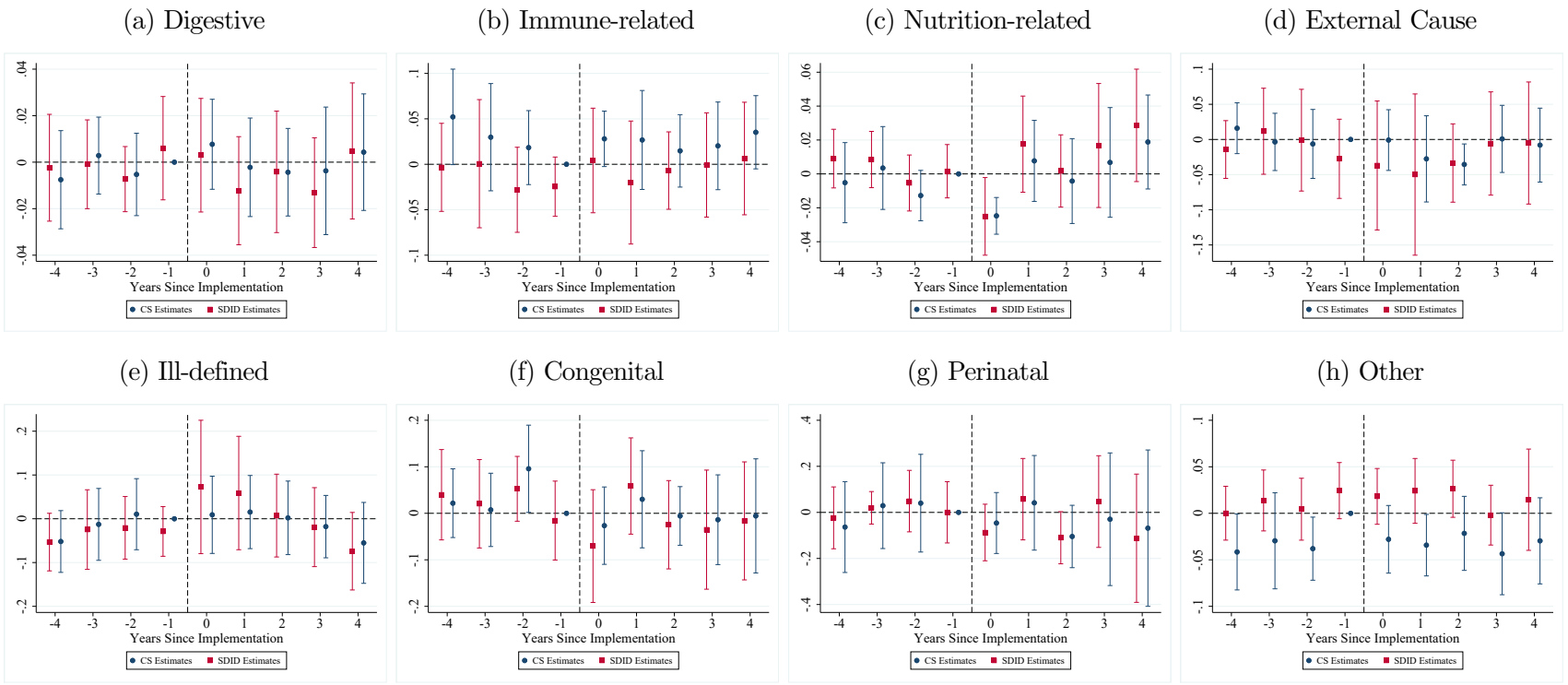
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life per 1,000 premature births in panels (a), (c), and (e) and per 1,000 low-birth-weight births in panels (b), (d), and (f). Panels (a) and (b) include the full sample; panels (c) and (d) include infants born to non-Hispanic white mothers; panels (e) and (f) include infants born to non-white mothers (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the number of births in the corresponding subgroup in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A6: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on One-Year Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995–2018)



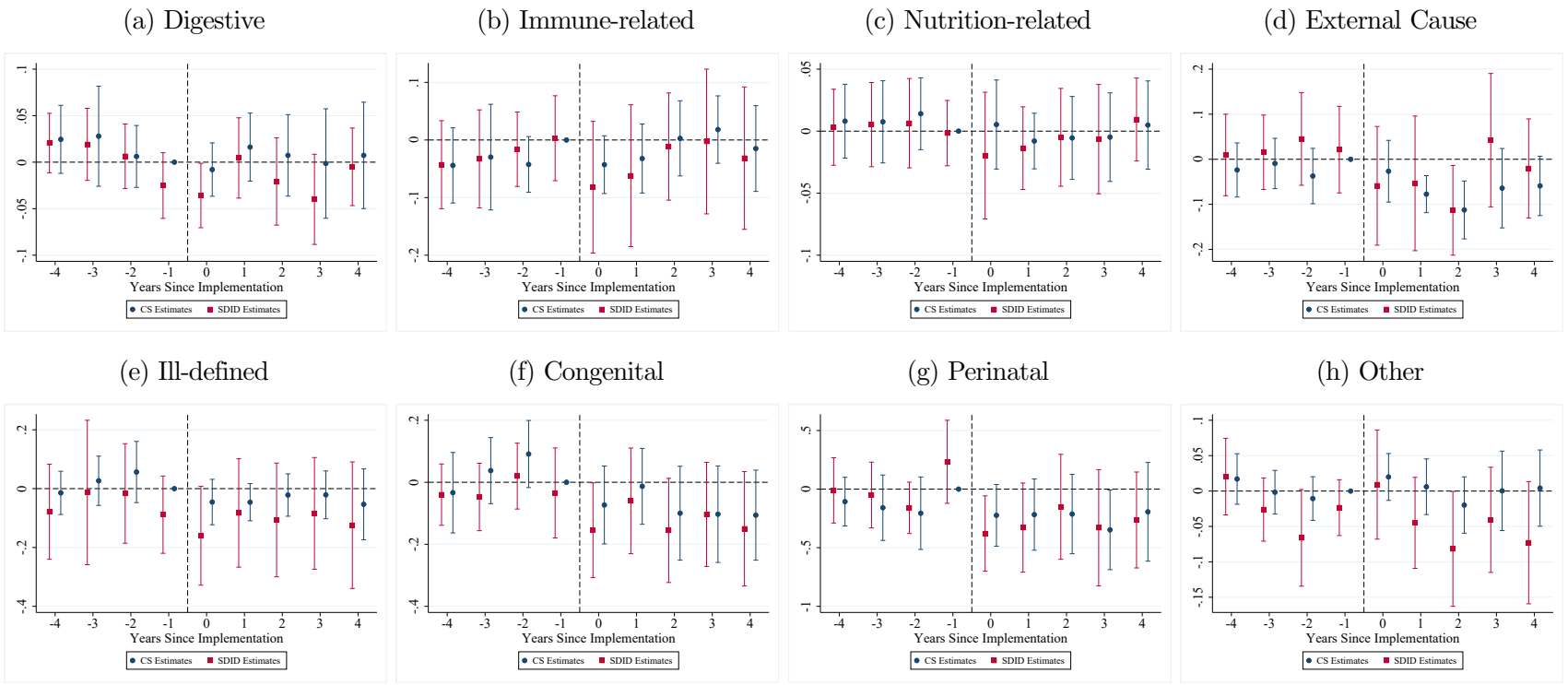
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life due to the cause indicated in each panel, per 1,000 live births. For the CS estimator, observations are weighted by the number of births in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A7: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on One-Year White Infant Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995–2018)



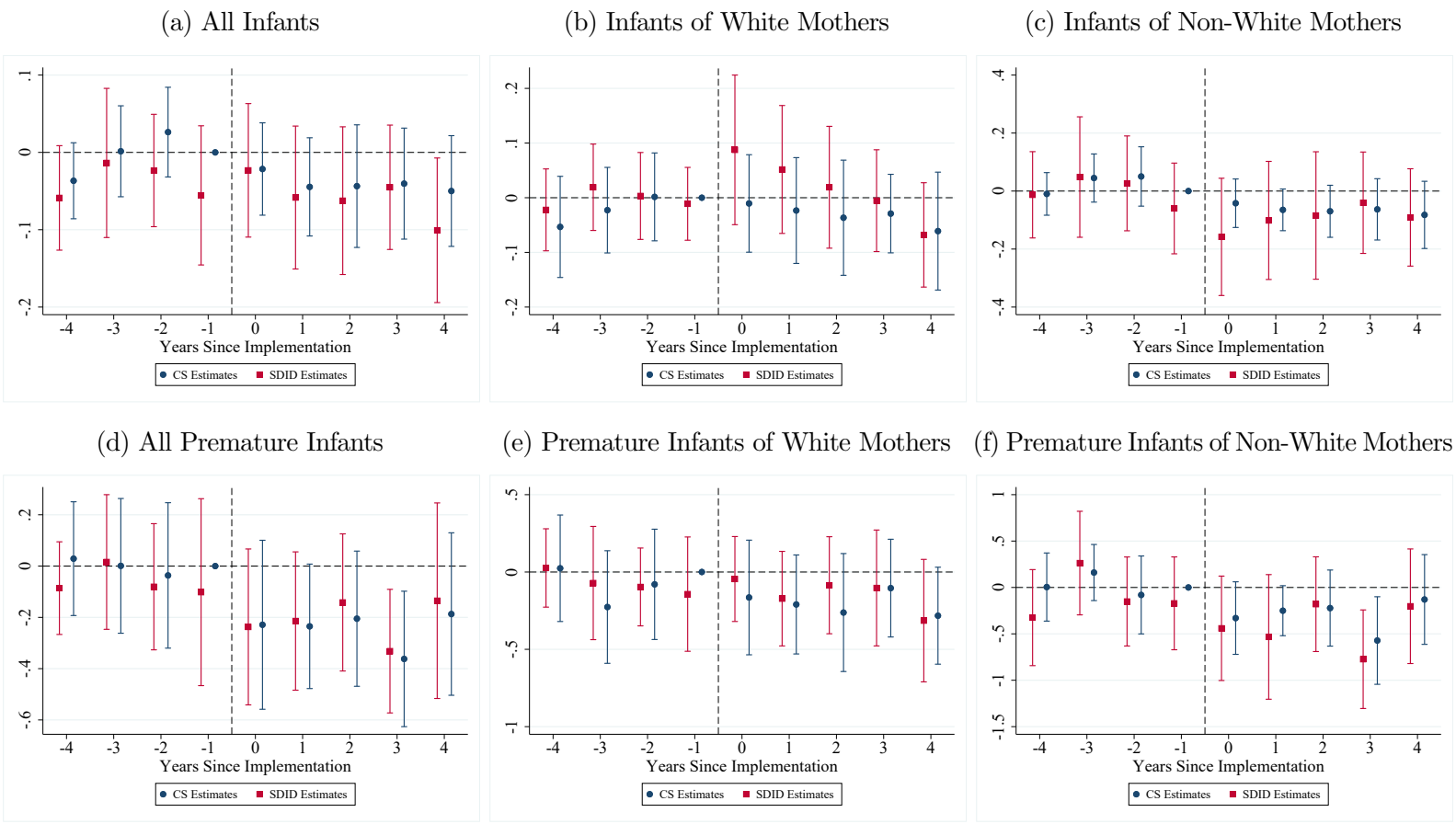
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life due to the cause indicated in each panel, per 1,000 live births. The sample includes infants born to non-Hispanic white mothers. For the CS estimator, observations are weighted by the number of births in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A8: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on One-Year Non-White Infant Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995–2018)



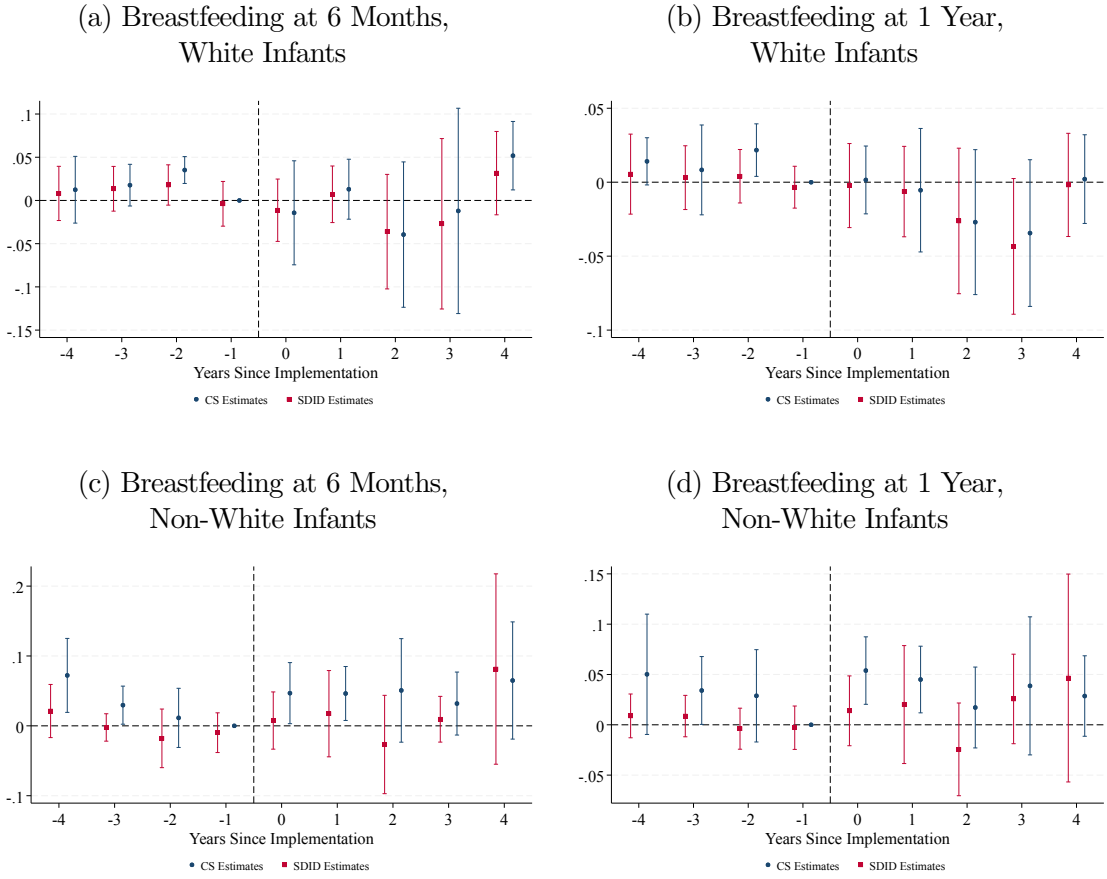
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life due to the cause indicated in each panel, per 1,000 live births. The sample includes infants born to non-white mothers (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the number of births in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A9: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on One-Year Sleep-Related Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)



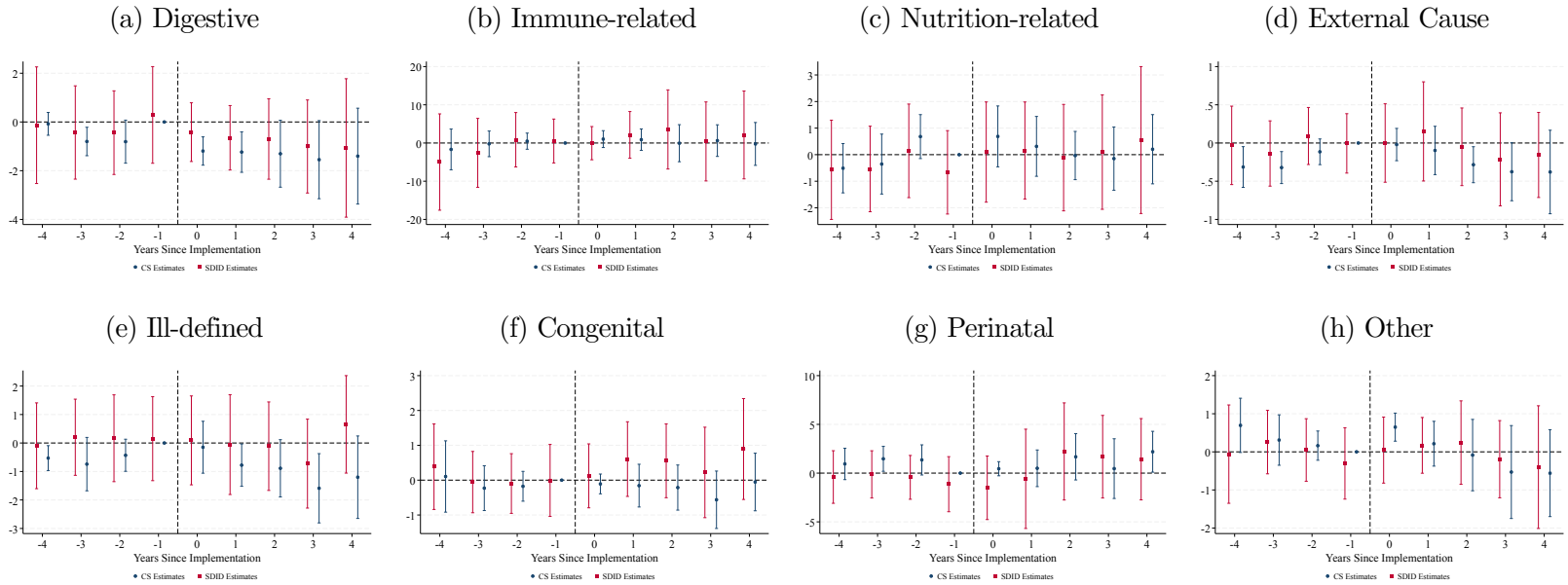
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. The outcome is the number of deaths due to Sudden Unexpected Infant Death (SUID) per 1,000 births in the corresponding sample. Panels (a)–(c) include all infants, infants born to non-Hispanic white mothers, and infants born to non-white mothers (i.e., Black, other race, or Hispanic), respectively; panels (d)–(f) include the corresponding samples of premature infants. For the CS estimator, observations are weighted by the number of births in the corresponding sample in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A10: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes by Race/Ethnicity, NIS-Child (2003-2017)



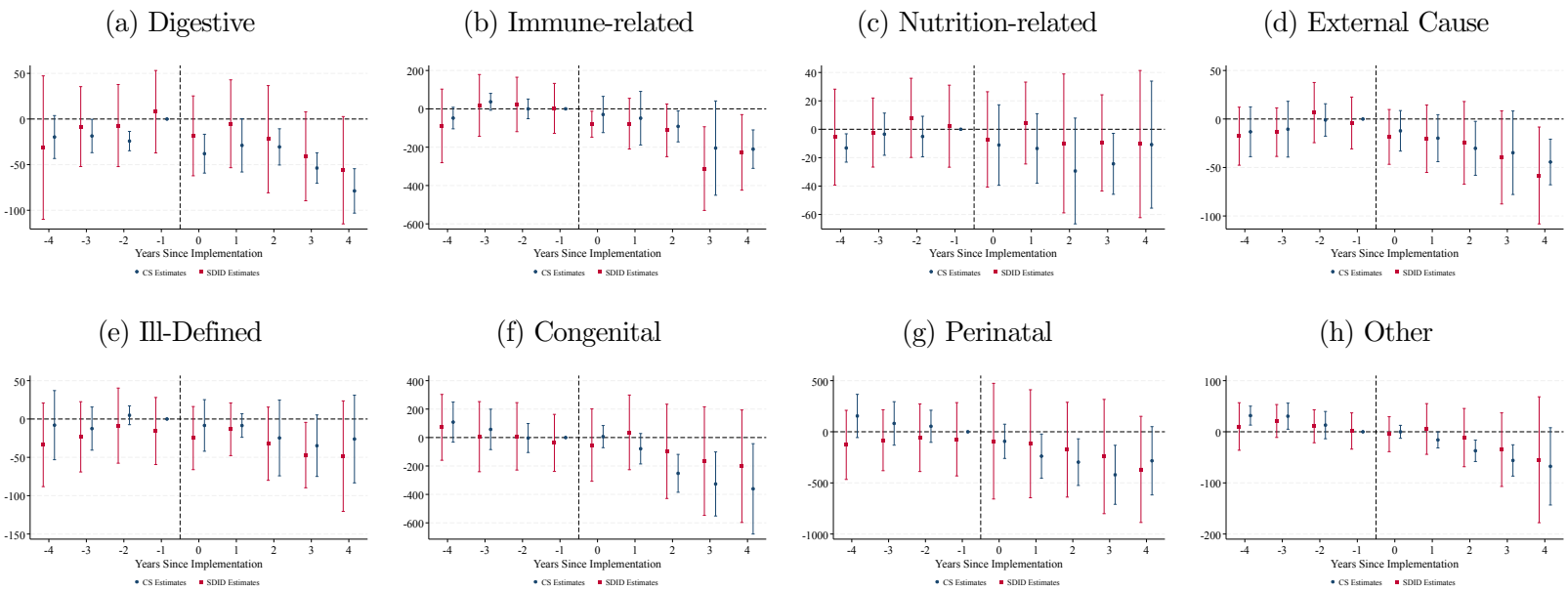
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the shares of infants breastfed at 6 months in panels (a) and (c) and at 1 year in panels (b) and (d). Panels (a) and (b) include non-Hispanic white infants; panels (c) and (d) include non-white infants (i.e., Black, other race, or Hispanic). For the CS estimator, observations are weighted by the sum of the NIS-Child sampling weights in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A11: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Inpatient Hospitalization Rates, HCUP (2000–2019)



Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence/hospital-state/discharge-year cell, and never-treated states are the control group. Outcomes are non-delivery inpatient hospitalizations per 1,000 births for the primary diagnosis indicated in each panel. For the CS estimator, observations are weighted by the number of deliveries in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence/hospital-state level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

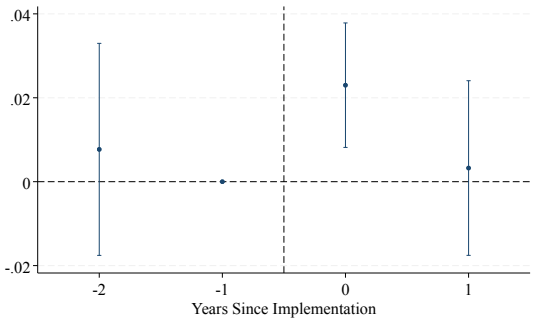
Figure A12: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Inpatient Charges, HCUP (2000–2019)



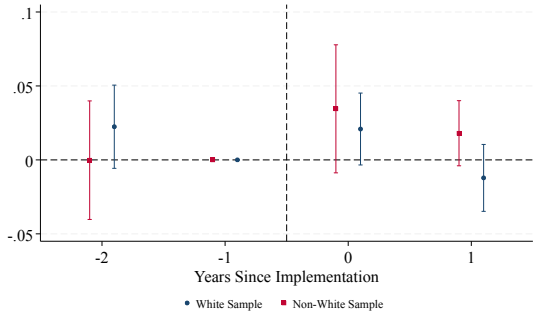
Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence/hospital-state/discharge-year cell, and never-treated states are the control group. Outcomes are non-delivery inpatient charges per birth for the primary diagnosis indicated in each panel. For the CS estimator, observations are weighted by the number of deliveries in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence/hospital-state level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Figure A13: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Infant Sleep Practices, PRAMS (2000-2018)

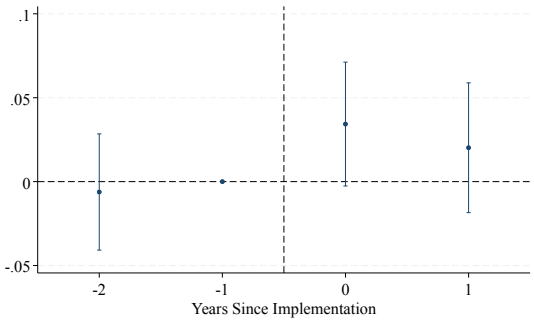
(a) *Full Sample*: Usual Sleep Position, Back



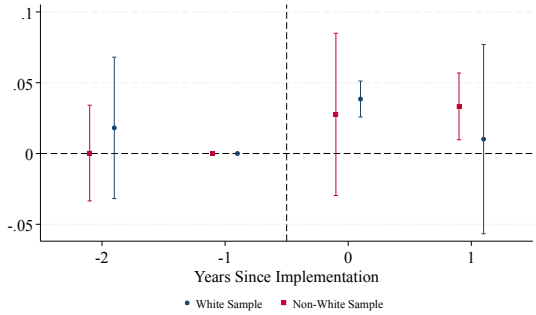
(b) *By Race/Ethnicity*: Usual Sleep Position, Back



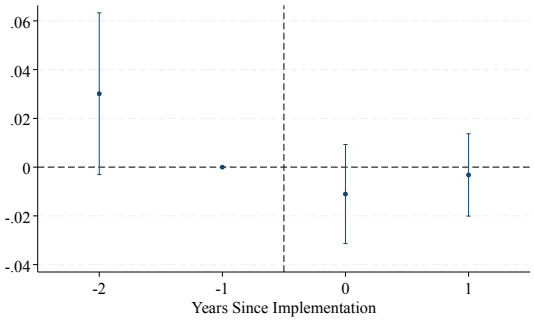
(c) *Full Sample*: Infant Bedshares, Never



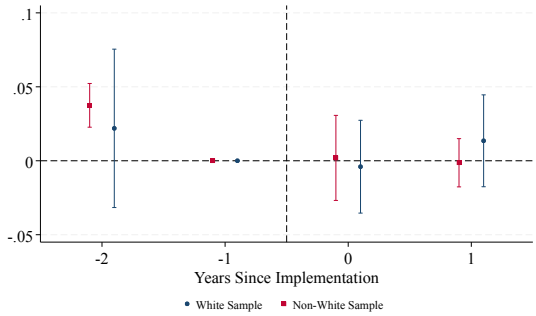
(d) *By Race/Ethnicity*: Infant Bedshares, Never



(e) *Full Sample*: Infant Bedshares, Often or Always

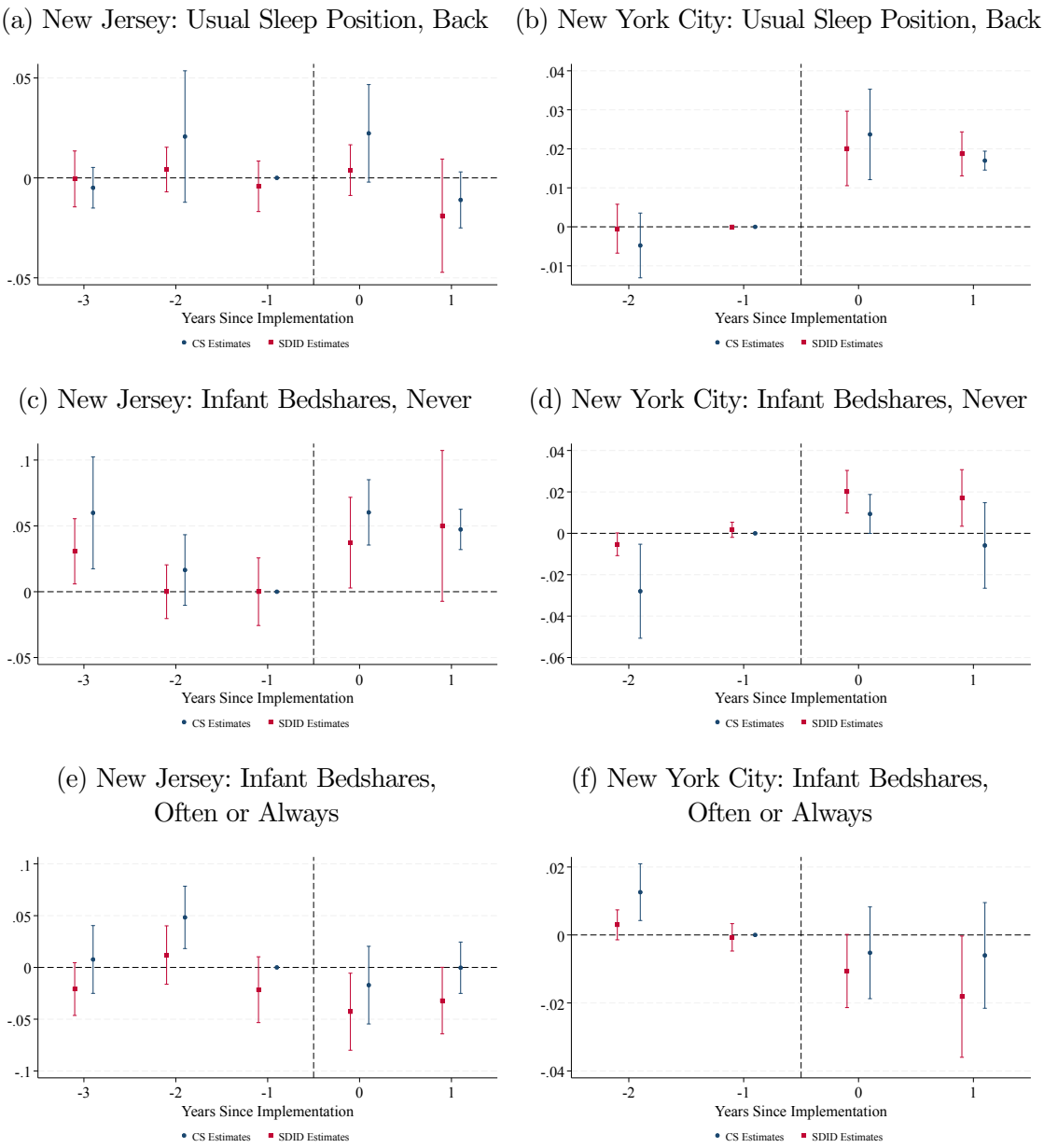


(f) *By Race/Ethnicity*: Infant Bedshares, Often or Always



Note: Each panel presents the event-time effects from the CS estimator with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Panels (a), (c), and (e) present estimates for the full sample; panels (b), (d), and (f) present estimates separately for infants of non-Hispanic white mothers (blue circles) and non-white mothers (red squares), where non-white includes Black, other race, or Hispanic mothers. Outcomes are the share of infants whose usual sleep position is on their back in panels (a) and (b), the share who never bed-share in panels (c) and (d), and the share who often or always bed-share in panels (e) and (f). Observations are weighted by the sum of the PRAMS sampling weights in each cell. The x-axis measures event time relative to policy adoption, with effects relative to the year prior to adoption. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap. We report the full set of event-time effects identified by both treated units (New York and New Jersey). See Appendix Table A2 for the state-years available in the PRAMS sample.

Figure A14: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Infant Sleep Practices by State, PRAMS (2000-2018)



Note: Each panel presents the event-time effects from the CS (blue circles) and SDID (red squares) estimators with 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Panels (a), (c), and (e) present estimates for New Jersey; panels (b), (d), and (f) present estimates for New York City. Outcomes are the share of infants whose usual sleep position is on their back in panels (a) and (b), the share who never bed-share in panels (c) and (d), and the share who often or always bed-share in panels (e) and (f). Observations are weighted by the sum of the PRAMS sampling weights in each cell. The x-axis measures event time relative to policy adoption. CS effects are relative to the year prior to policy adoption; SDID effects are relative to a weighted average of the pre-treatment period. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID. Because the sleep-related questions are available for only a limited set of state-years, we report a restricted set of event-time effects. For analyses in which New Jersey (New York City) is the treated unit, we restrict the sample to be balanced around an event window from three (two) years prior to one year after policy adoption. See Appendix Table A2 for the state-years available in the PRAMS sample.

Table A1: Components of State Breastfeeding Policies

State	Month and Year of Hospital Policy Adoption	Lactation Consultant	Staff Training	Inform Patients	Written/ Communicated	Rooming In	Non Breastmilk	Group/ Resources Info	Initiate BF	How to BF	On Demand BF	No Pacifiers	Total Components (out of 11)
Pennsylvania	06/1998				X								1
Missouri	08/1999	X		X				X					3
Georgia	12/2002					X							1
Maryland	06/2005	X	X										2
New York	09/2005	X	X	X	X	X	X	X	X	X	X	X	11
Louisiana	02/2007	X											1
Ohio	01/2012	X		X									2
Illinois	01/2013	X	X	X	X		X	X	X				7
California	01/2014	X		X	X	X		X					5
New Jersey	01/2014	X	X	X	X	X	X	X	X	X			9
South Carolina	06/2015	X											1
Texas	06/2016	X											1
Mississippi	07/2016		X	X	X		X						4
Total States (out of 13)		10	5	7	6	4	4	5	3	2	1	1	

Note: In the empirical analysis, the year a policy is considered to be in effect may differ from the legal adoption year and is defined as follows. A state is considered to have an effective hospital policy in a given calendar year if they adopted the policy by June of that year. For states that adopt policies in the latter half of the calendar year, the effective year is defined as the following calendar year. Therefore, in the empirical analysis, Missouri's policy effective year is coded as 2000, Georgia's as 2003, New York's as 2006, and Mississippi's as 2017. States that adopted a hospital policy before June 1998 are excluded. We use the policy component categorizations developed by the LawAtlas Policy Surveillance Program database, detailed below. Lactation consultant: state policy requires that hospitals must make a breastfeeding consultant available to maternity patients. Staff training: state policy requires that healthcare staff be trained in the skills necessary to implement practices that support breastfeeding among maternity patients. Inform patients: state policy requires hospitals to inform patients about breastfeeding (whether it be general, about the benefits and/or disadvantages, about initiation, or management). Written/communicated: state policy requires hospitals' breastfeeding policy be written and/or communicated (whether it be to staff, to patients, posted, or provided directly). Rooming in: state policy requires hospitals to permit rooming-in, where the baby's crib is kept by the side of the mother's bed. Non-breastmilk: state policy includes requirements about when infants may be given food or drink other than breast milk. Group/resources info: state policy requires hospitals to foster the establishment of breastfeeding groups and/or refer mothers to them. Initiate BF: state policy requires hospitals to help mothers initiate breastfeeding within one hour of birth. How to BF: state policy requires hospitals to provide mothers with instruction on how to breastfeed, and how to maintain lactation. On demand BF: state policy requires that hospitals allow mothers to breastfeed on demand. No pacifiers: state policy prohibits hospitals from giving pacifiers or artificial nipples (e.g., bottle feeding) to breastfeeding infants.

Table A2: PRAMS Data Availability

Site	2018	2017	2016	2015	2014	2013	2012	2011	2010	2009	2008	2007	2006	2005	2004	2003	2002	2001	2000
Alabama		○		○	○											●	●	●	●
Alaska	○	○	○	●	●	●	●		○	○	●	●	●	●	●	●	●	●	●
Colorado	○	○	○	○		○	○	○	○	○	○	○	○	○	○	○	○	○	○
Connecticut	○	○	○	●	●														
Delaware	○	○	○	●	●	●	●	●	●	●	●	●							
Florida														●	●	○	○	○	○
Georgia	○	○				●	●	●	●	●	●	●	●	●	●				
Hawaii			○	○	○	○	○	●	●	●	○	○	○	○	○	○	○	○	○
Illinois		○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○
Iowa		○	○	○	○	○													
Kentucky	○	○																	
Louisiana	○	○	○	●											●	●	●	●	●
Maine		○	○	●	●	●	●	○	○	○	●	●	●	●	●	●	●	●	●
Maryland		○	○	○	○	○	○	●	●	●	○	○	○	○	○	○	○	○	
Michigan	○	○	○	○		○	○	○	○	○	●	●	●	●	●	●	●	●	
Minnesota						○	○	●	●	●	●	●	●	●	●	●	●		
Mississippi										○	○		○		○	○			
Montana		○																	
Nebraska	○		○	●	●	●	●	●	●	●	●	●	●	●	●	○	○	○	○
New Hampshire		○	○	○	○	○													
New Jersey	○	○	○	●	●	●	●	●	●	●	●	●	●	●	●	●	●		
New Mexico	○	○	○	○	○	○	○	○						○	○	○	○	○	○
New York		○	○	○	○	○		○	○		○	○	○	○	○	○	○	○	○
New York City	○	○	○	○	○	○	○	○	○			●	●	●	●				
North Carolina		○									○	○		○	○	○	○	○	○
North Dakota		○																	
Ohio				○	○		○		○	○	●	●	●	●		●	●	●	●
Oklahoma		○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○	○
Oregon				○		○	○	○	○	○	●	●	●	●	●	○			
Rhode Island	○	○	○		●	●	●	○	○	○	○	○	○	○	○	○	○		
South Carolina												●	●	●	●	○	○	○	○
South Dakota	○	○																	
Tennessee				●	●	●	●			●	●								
Texas			○	●					●	●									
Utah	○	○	○	○	○	○	○	○	○	○	●	●	●	●	●	○	○	○	○
Vermont	○	○	○	●	●	●	●	●	●	●	●	●	●	●	●	●	●	●	
Virginia	○	○	○	●															
Washington	○	○	○	●	●	●	●	●	●	●	●	●	●	●	●	●	●	●	●
West Virginia	○	○	○	●	●	●	●	●	●	●	●	●	●	●	●	●	●	●	●
Wisconsin	○	○	○	●	●	●	●	●		●	●	●							
Wyoming	○	○	○	○	○	○	○	○	○	○	●	●							

Note: ● indicates that data are available and the survey includes bed-sharing questions in that year; ○ indicates that data are available but bed-sharing questions are unavailable. A blank cell indicates that data are unavailable for that state-year. States are not listed if data are unavailable for all years or they had adopted a hospital breastfeeding support policy prior to 2000 (Arkansas, Kansas, Massachusetts, Missouri, and Pennsylvania). Gray shaded cells indicate state-years in which a hospital breastfeeding support policy is considered to be in effect.

Table A3: Robustness of Infant Mortality Estimates to Specification Choices, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: One-Year Mortality							
CS ATT	-0.217**	-0.215**	-0.222**	-0.217**	-0.313*	-0.252***	-0.392**
	(0.093)	(0.098)	(0.097)	(0.108)	(0.167)	(0.097)	(0.154)
SDID ATT	-0.232**		-0.236**	-0.230**	-0.265	-0.225**	-0.481***
	(0.105)		(0.106)	(0.115)	(0.180)	(0.108)	(0.163)
Panel B: Neonatal Mortality							
CS ATT	-0.103	-0.109*	-0.105	-0.093	-0.222*	-0.130*	-0.215**
	(0.068)	(0.065)	(0.072)	(0.078)	(0.126)	(0.077)	(0.102)
SDID ATT	-0.132		-0.127	-0.137	-0.264*	-0.107	-0.347***
	(0.094)		(0.100)	(0.102)	(0.140)	(0.099)	(0.125)
Panel C: Postneonatal Mortality							
CS ATT	-0.114***	-0.106**	-0.117***	-0.124***	-0.092*	-0.122***	-0.178**
	(0.043)	(0.045)	(0.041)	(0.045)	(0.054)	(0.040)	(0.075)
SDID ATT	-0.117***		-0.121***	-0.113***	-0.055	-0.121***	-0.095*
	(0.035)		(0.035)	(0.039)	(0.045)	(0.046)	(0.051)
Post-treatment aggregation?	0-4	0-4	0-3	0-5	0-12	0-4	0-4
Use never and not-yet treated?	No	Yes	No	No	No	No	No
Alternative treatment timing?	No	No	No	No	No	Yes	No
NIS-Child birth cohorts only?	No	No	No	No	No	No	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects for the full sample over the event periods indicated at the bottom of the table. Column (1) presents the baseline specification, which averages effects over event periods 0 through 4 (inclusive), uses never-treated states as the control group, and defines the treatment year based on the month of policy adoption. Column (2) instead uses never-treated and not-yet-treated states as the control group; columns (3)–(5) vary the event periods over which the event-time effects are averaged; column (6) defines treatment as beginning in the year of policy adoption regardless of the month of adoption; and column (7) restricts the sample to birth cohorts 2000–2015, corresponding to those covered by NIS-Child. The unit of observation is a state-of-residence (at birth)/year-of-birth cell. For the CS estimator, observations are weighted by the number of births in each cell. Outcomes are described in the note to Figure 1. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A4: Robustness of White Infant Mortality Estimates to Specification Choices, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: One-Year Mortality								
CS ATT	-0.075	-0.080	-0.066	-0.069	-0.052	-0.111	-0.230**	-0.047
	(0.114)	(0.114)	(0.111)	(0.125)	(0.151)	(0.129)	(0.101)	(0.108)
SDID ATT	0.034		0.057	0.024	0.031	0.044	-0.136	0.052
	(0.083)		(0.080)	(0.091)	(0.121)	(0.085)	(0.095)	(0.079)
Panel B: Neonatal Mortality								
CS ATT	-0.020	-0.027	-0.016	0.009	-0.041	-0.081	-0.133*	0.008
	(0.103)	(0.104)	(0.100)	(0.107)	(0.112)	(0.119)	(0.076)	(0.088)
SDID ATT	-0.041		-0.018	-0.028	-0.099	-0.033	-0.144**	-0.005
	(0.072)		(0.075)	(0.070)	(0.081)	(0.074)	(0.061)	(0.073)
Panel C: Postneonatal Mortality								
CS ATT	-0.054	-0.053	-0.050	-0.078	-0.011	-0.030	-0.097	-0.055
	(0.050)	(0.050)	(0.048)	(0.054)	(0.061)	(0.058)	(0.059)	(0.040)
SDID ATT	0.030		0.033	-0.001	0.045	0.047	0.002	0.007
	(0.043)		(0.045)	(0.043)	(0.049)	(0.046)	(0.055)	(0.035)
Post-treatment aggregation?	0-4	0-4	0-3	0-5	0-12	0-4	0-4	0-4
Use never and not-yet treated?	No	Yes	No	No	No	No	No	No
Alternative treatment timing?	No	No	No	No	No	Yes	No	No
NIS-Child birth cohorts only?	No	No	No	No	No	No	Yes	No
Alternative race definition?	No	No	No	No	No	No	No	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects for infants born to non-Hispanic white mothers over the event periods indicated at the bottom of the table. Column (1) presents the baseline specification, which averages effects over event periods 0 through 4 (inclusive), uses never-treated states as the control group, and defines the treatment year based on the month of policy adoption. Column (2) instead uses never-treated and not-yet-treated states as the control group; columns (3)–(5) vary the event periods over which the event-time effects are averaged; column (6) defines treatment as beginning in the year of policy adoption regardless of the month of adoption; column (7) restricts the sample to birth cohorts 2000–2015, corresponding to those covered by NIS-Child; and column (8) defines white infants based on race regardless of Hispanic ethnicity. The unit of observation is a state-of-residence (at birth)/year-of-birth cell. For the CS estimator, observations are weighted by the number of births in each cell. Outcomes are described in the note to Figure 1. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A5: Robustness of Non-White Infant Mortality Estimates to Specification Choices, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: One-Year Mortality								
CS ATT	-0.433**	-0.448**	-0.438**	-0.476**	-0.813**	-0.458***	-0.619**	-0.662**
	(0.201)	(0.187)	(0.187)	(0.217)	(0.354)	(0.178)	(0.298)	(0.294)
SDID ATT	-0.457**		-0.474***	-0.452**	-0.742***	-0.455**	-0.726***	-0.454**
	(0.179)		(0.179)	(0.180)	(0.211)	(0.183)	(0.223)	(0.195)
Panel B: Neonatal Mortality								
CS ATT	-0.228	-0.253*	-0.234	-0.276	-0.582*	-0.215	-0.336	-0.390*
	(0.161)	(0.141)	(0.149)	(0.178)	(0.313)	(0.143)	(0.295)	(0.214)
SDID ATT	-0.350**		-0.346**	-0.384**	-0.646***	-0.281	-0.541***	-0.372**
	(0.156)		(0.161)	(0.164)	(0.173)	(0.180)	(0.205)	(0.169)
Panel C: Postneonatal Mortality								
CS ATT	-0.205***	-0.195***	-0.204***	-0.200***	-0.231**	-0.243***	-0.283**	-0.272***
	(0.071)	(0.069)	(0.063)	(0.066)	(0.090)	(0.066)	(0.116)	(0.094)
SDID ATT	-0.167**		-0.185**	-0.117	-0.092	-0.195**	-0.204**	-0.188*
	(0.082)		(0.088)	(0.087)	(0.098)	(0.089)	(0.102)	(0.107)
Post-treatment aggregation?	0-4	0-4	0-3	0-5	0-12	0-4	0-4	0-4
Use never and not-yet treated?	No	Yes	No	No	No	No	No	No
Alternative treatment timing?	No	No	No	No	No	Yes	No	No
NIS-Child birth cohorts only?	No	No	No	No	No	No	Yes	No
Alternative race definition?	No	No	No	No	No	No	No	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects for infants born to Black, other race, or Hispanic mothers over the event periods indicated at the bottom of the table. Column (1) presents the baseline specification, which averages effects over event periods 0 through 4 (inclusive), uses never-treated states as the control group, and defines the treatment year based on the month of policy adoption. Column (2) instead uses never-treated and not-yet-treated states as the control group; columns (3)–(5) vary the event periods over which the event-time effects are averaged; column (6) defines treatment as beginning in the year of policy adoption regardless of the month of adoption; column (7) restricts the sample to birth cohorts 2000–2015, corresponding to those covered by NIS-Child; and column (8) defines non-white infants as those who are Black or other race, regardless of Hispanic ethnicity. The unit of observation is a state-of-residence (at birth)/year-of-birth cell. For the CS estimator, observations are weighted by the number of births in each cell. Outcomes are described in the note to Figure 1. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A6: p-Values on Infant Mortality Estimates from Different Inference Procedures, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Callaway and Sant'Anna (CS)			Synthetic Difference-in-Differences (SDID)			
	Multiplier Bootstrap	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided	Block Bootstrap, 200 Reps	Block Bootstrap, 50 Reps	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided
Panel A: Full Sample							
One-Year	0.020	0.018	0.058	0.027	0.017	0.005	0.014
Neonatal	0.128	0.105	0.221	0.161	0.184	0.098	0.170
Postneonatal	0.008	0.015	0.034	0.001	0.001	0.001	0.001
Panel B: Infants of Non-White Mothers							
One-Year	0.032	0.048	0.098	0.011	0.007	0.003	0.009
Neonatal	0.157	0.127	0.256	0.025	0.036	0.010	0.027
Postneonatal	0.004	0.018	0.036	0.043	0.039	0.005	0.019

Note: Each cell reports the p-value for the simple average of the event-time effects over event periods 0 through 4 (inclusive) from different inference procedures and estimators. Column (1) reports multiplier (wild) bootstrap p-values for the observed treatment assignment using the CS estimator. Columns (2) and (3) report the corresponding one- and two-sided t-statistic-based randomization inference (RI) p-values for CS using 1,000 randomized treatment reassignments. Columns (4) and (5) report block bootstrap p-values for the observed treatment assignment using the SDID estimator with 200 and 50 bootstrap replications, respectively. Columns (6) and (7) report the corresponding one- and two-sided t-statistic-based RI p-values for SDID based on 1,000 randomized treatment reassignments, with the t-statistic for each treatment reassignment computed using 50 block bootstrap replications. Bootstrap inference is clustered at the state-of-residence (at birth) level. Outcomes are described in the note to Figure 1. Panel A includes the full sample and Panel B includes infants born to non-white mothers (i.e., Black, other race, or Hispanic).

Table A7: Hospital Breastfeeding Support Policy Effects on Maternal Characteristics in Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample			White Sample			Non-White Sample		
Characteristic	CS ATT	SDID ATT	Pre-Treatment Mean	CS ATT	SDID ATT	Pre-Treatment Mean	CS ATT	SDID ATT	Pre-Treatment Mean
Share Non-Hispanic White	0.0027	-0.0027	0.4791						
	(0.0032)	(0.0036)							
Share Under 20	-0.0008	-0.0002	0.0821	0.0022**	0.0014*	0.0591	-0.0015	-0.0022	0.1035
	(0.0009)	(0.0010)		(0.0009)	(0.0008)		(0.0010)	(0.0017)	
Share Aged 20-29	0.0001	0.0016	0.4977	-0.0013	0.0018	0.4748	0.0004	0.0017	0.5195
	(0.0021)	(0.0019)		(0.0035)	(0.0023)		(0.0027)	(0.0028)	
Share Aged 30-39	0.0000	-0.0012	0.3888	-0.0010	-0.0029	0.4340	0.0007	0.0001	0.3466
	(0.0020)	(0.0022)		(0.0029)	(0.0027)		(0.0017)	(0.0022)	
Share 40 and Older	0.0007*	0.0005	0.0314	0.0001	0.0009**	0.0321	0.0005	0.0004	0.0304
	(0.0004)	(0.0004)		(0.0002)	(0.0004)		(0.0004)	(0.0006)	
Share < High School	-0.0052		0.1825	0.0024		0.0968	-0.0030		0.2623
	(0.0050)			(0.0015)			(0.0067)		
Share High School	0.0004		0.2725	0.0002		0.2427	-0.0031		0.3004
	(0.0034)			(0.0037)			(0.0054)		
Share Some College	-0.0003		0.2620	-0.0056*		0.2739	-0.0002		0.2511
	(0.0042)			(0.0032)			(0.0035)		
Share ≥ College	0.0050		0.2830	0.0030		0.3865	0.0063		0.1862
	(0.0034)			(0.0025)			(0.0045)		
Share Married	0.0030		0.6016	-0.0043		0.7436	-0.0007		0.4703
	(0.0019)			(0.0035)			(0.0043)		
Share Deliver Out of State	-0.0001	-0.0004	0.0187	0.0006	0.0005	0.0269	-0.0008	-0.0002	0.0111
	(0.0008)	(0.0015)		(0.0007)	(0.0013)		(0.0010)	(0.0024)	

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive). Columns (1), (4), and (7) report estimates using the CS estimator. Columns (2), (5), and (8) use the SDID estimator. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the characteristics listed in each row and are calculated using births with non-missing information on that measure. For the CS estimator, observations are weighted by the number of births in each cell with non-missing information for that measure. The white sample includes non-Hispanic white mothers; the non-white sample includes Black, other race, or Hispanic mothers. Standard errors are clustered at the state-of-residence level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A8: Hospital Breastfeeding Support Policy Effects on Maternal Characteristics in NIS-Child (2003-2017)

	(1) At Least Some College	(2) Non-Hispanic White	(3) Married	(4) ≤ 29 Years Old
CS ATT	0.0060	0.0058	-0.0055	-0.0009
	(0.0067)	(0.0097)	(0.0119)	(0.0080)
SDID ATT	0.002	-0.002	-0.014	0.012
	(0.010)	(0.011)	(0.012)	(0.014)
Pre-Treatment Mean	0.551	0.393	0.622	0.415

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the maternal characteristics listed in the column headers. For the CS estimator, observations are weighted by the sum of the NIS-Child sampling weights in each cell. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A9: Hospital Breastfeeding Support Policy Effects on Prenatal Care and Birth Characteristics in Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample			White Sample			Non-White Sample		
Characteristic	CS ATT	SDID ATT	Pre-Treatment Mean	CS ATT	SDID ATT	Pre-Treatment Mean	CS ATT	SDID ATT	Pre-Treatment Mean
Share Late Prenatal Care (PNC)	-0.0011		0.0499	-0.0012		0.0331	-0.0028		0.0655
	(0.0037)			(0.0021)			(0.0074)		
Share PNC in First Trimester	-0.0222		0.7463	-0.0090		0.7996	-0.0384		0.6963
	(0.0434)			(0.0418)			(0.0524)		
Share PNC Initiation Missing	0.039		0.047	0.020		0.046	0.064		0.048
	(0.085)			(0.086)			(0.098)		
Share Premature	0.0001	0.0003	0.1154	-0.0005	-0.0007	0.1039	0.0003	-0.0020	0.1259
	(0.0009)	(0.0012)		(0.0010)	(0.0013)		(0.0013)	(0.0017)	
Share Low Birth Weight	-0.0009*	-0.0007	0.0816	-0.0009*	-0.0009	0.0695	-0.0020***	-0.0024***	0.0926
	(0.0005)	(0.0007)		(0.0005)	(0.0007)		(0.0007)	(0.0009)	
Share Very Low Birth Weight	-0.0001	-0.0001	0.0150	-0.0001	-0.0001	0.0115	-0.0003	-0.0006**	0.0182
	(0.0001)	(0.0002)		(0.0002)	(0.0002)		(0.0002)	(0.0003)	
Share Macrosomia	-0.0001	-0.0001	0.0111	-0.0003	-0.0003	0.0141	0.0001	0.0000	0.0083
	(0.0002)	(0.0001)		(0.0002)	(0.0002)		(0.0002)	(0.0002)	
Share Born in Hospital	0.0004	0.0001	0.9899	0.0006	0.0005	0.9843	0.0003	0.0004	0.9954
	(0.0003)	(0.0004)		(0.0005)	(0.0005)		(0.0002)	(0.0003)	
Share Born via C-Section	-0.0017	0.0015	0.3156	-0.0022	-0.0013	0.3053	-0.0028	0.0025	0.3251
	(0.0025)	(0.0029)		(0.0023)	(0.0036)		(0.0035)	(0.0028)	
Share Multiple Births	-0.0007	-0.0004	0.0345	-0.0014*	-0.0010	0.0393	-0.0006*	0.0004	0.0299
	(0.0004)	(0.0006)		(0.0008)	(0.0009)		(0.0004)	(0.0004)	
Share Admitted to NICU	-0.0002		0.0777	-0.0024		0.0754	-0.0008		0.0793
	(0.0014)			(0.0017)			(0.0024)		

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive). Columns (1), (4), and (7) report estimates using the CS estimator. Columns (2), (5), and (8) report estimates using the SDID estimator. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the prenatal care and birth characteristics listed in each row and are calculated using births with non-missing information for that measure. For the CS estimator, observations are weighted by the number of births in each cell with non-missing information for that measure. The white sample includes infants born to non-Hispanic white mothers; the non-white sample includes infants born to Black, other race, or Hispanic mothers. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A10: Pre-Trend Estimates and Analyses Predicting Policy Adoption

	(1)	(2)	(3)	(4)	(5)
		Characteristics in Levels, (2000 Birth Cohort)		Changes in Characteristics, (2002-2000 Birth Cohorts)	
	Pretrend	Ever Adopt	Early Adopt	Ever Adopt	Early Adopt
Share Non-Hispanic White	0.002	-2.114***	-0.111	-0.686	1.390
	(0.001)	(0.587)	(0.276)	(2.473)	(1.121)
Share Married	0.001	1.094	-0.471	4.345	-0.523
	(0.001)	(2.264)	(1.809)	(2.710)	(1.647)
Share HS or Less	0.001	1.949	0.085	-0.894	-0.788
	(0.001)	(1.670)	(1.153)	(2.171)	(0.904)
Share Age $\leq$ 29	-0.000	-0.642	-0.627	2.072	-0.207
	(0.001)	(1.341)	(1.008)	(2.007)	(0.969)
Share Ever Breastfed	0.002	-0.924	-0.610	0.579	0.817
	(0.002)	(1.071)	(0.830)	(1.486)	(1.089)
Infant Mortality Rate	0.002	-0.019	0.035	0.008	0.077
	(0.022)	(0.062)	(0.074)	(0.184)	(0.068)
Observations	861	46	46	46	46
F-Test p-value		0.000	0.654	0.506	0.560

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each estimate in column (1) is from a separate regression in which the outcome is the state-level characteristic listed in that row. The reported estimate is the coefficient on a linear trend variable measured relative to the year of policy adoption. Each regression includes state and birth-year fixed effects and indicators for each year post-policy adoption. Standard errors in column (1) are clustered at the state-level and are reported in parentheses. Columns (2)–(5) report estimates from regressions examining whether baseline characteristics predict ever adopting a hospital breastfeeding support policy in columns (2) and (4) or early policy adoption in columns (3) and (5). Baseline characteristics are measured in levels for the 2000 birth cohort in columns (2) and (3) and as changes between the 2000 and 2002 birth cohorts in columns (4) and (5). The regressions in columns (2)–(5) include census region fixed effects, with heteroskedasticity-robust standard errors reported in parentheses. The F-test p-value tests whether the reported coefficients are jointly equal to zero. See Appendix Section C for more details on the underlying data and empirical specifications.

Table A11: Hospital Breastfeeding Support Policy Effects on One-Year Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Digestive	Immune-related	Nutrition-related	External Cause	Ill-defined	Congenital	Perinatal	Other
Panel A: Full Sample								
CS ATT	0.002	0.010	-0.001	-0.037**	-0.018	-0.030	-0.127**	-0.016
	(0.012)	(0.014)	(0.006)	(0.018)	(0.022)	(0.033)	(0.064)	(0.013)
SDID ATT	-0.008	-0.028	-0.001	-0.039	-0.053	-0.053	-0.150**	-0.001
	(0.009)	(0.019)	(0.008)	(0.033)	(0.034)	(0.042)	(0.074)	(0.013)
Pre-Treatment Mean	0.072	0.343	0.077	0.330	0.719	1.237	3.231	0.205
Panel B: Infants of White Mothers								
CS ATT	0.000	0.025*	0.001	-0.014	-0.009	-0.004	-0.042	-0.031*
	(0.010)	(0.014)	(0.009)	(0.018)	(0.036)	(0.036)	(0.096)	(0.018)
SDID ATT	-0.004	-0.003	0.008	-0.026	0.009	-0.018	-0.041	0.016
	(0.010)	(0.022)	(0.007)	(0.033)	(0.042)	(0.039)	(0.057)	(0.012)
Pre-Treatment Mean	0.056	0.263	0.070	0.299	0.666	1.156	2.370	0.206
Panel C: Infants of Non-White Mothers								
CS ATT	0.004	-0.014	-0.002	-0.068**	-0.038	-0.079	-0.239*	0.002
	(0.022)	(0.023)	(0.012)	(0.027)	(0.030)	(0.067)	(0.138)	(0.019)
SDID ATT	-0.019	-0.038	-0.007	-0.041	-0.112	-0.125**	-0.290*	-0.046
	(0.016)	(0.038)	(0.015)	(0.048)	(0.070)	(0.063)	(0.168)	(0.030)
Pre-Treatment Mean	0.087	0.419	0.084	0.357	0.767	1.298	3.967	0.203

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life due to the cause listed in the column header per 1,000 live births. For the CS estimator, observations are weighted by the number of births in each cell. Panel A includes the full sample; panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A12: Hospital Breastfeeding Support Policy Effects on One-Year Infant Mortality among Premature Infants by Cause of Death, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Digestive	Immune-related	Nutrition-related	External Cause	Ill-defined	Congenital	Perinatal	Other
Panel A: Full Sample								
CS ATT	0.035	0.114	0.001	-0.109**	-0.184**	-0.207	-1.265**	-0.040
	(0.070)	(0.080)	(0.036)	(0.045)	(0.086)	(0.154)	(0.565)	(0.089)
SDID ATT	-0.028	-0.036	0.002	-0.043	-0.157	-0.146	-1.103*	-0.049
	(0.050)	(0.113)	(0.042)	(0.071)	(0.102)	(0.201)	(0.569)	(0.066)
Pre-Treatment Mean	0.419	1.576	0.281	0.639	1.629	5.513	25.562	0.932
Panel B: Infants of White Mothers								
CS ATT	0.016	0.274***	-0.021	-0.031	-0.087	-0.084	-0.443	-0.057
	(0.074)	(0.072)	(0.055)	(0.087)	(0.102)	(0.295)	(0.817)	(0.138)
SDID ATT	-0.056	0.037	0.002	-0.041	-0.041	-0.010	-0.160	0.032
	(0.063)	(0.089)	(0.047)	(0.076)	(0.112)	(0.298)	(0.441)	(0.096)
Pre-Treatment Mean	0.332	1.052	0.332	0.550	1.441	5.707	20.410	0.901
Panel C: Infants of Non-White Mothers								
CS ATT	0.067	-0.036	0.003	-0.181**	-0.260**	-0.324	-2.123**	-0.056
	(0.106)	(0.127)	(0.078)	(0.087)	(0.123)	(0.284)	(0.910)	(0.101)
SDID ATT	-0.093	-0.237	-0.098	-0.125	-0.437**	-0.740**	-1.515	-0.209*
	(0.090)	(0.184)	(0.100)	(0.127)	(0.198)	(0.372)	(1.130)	(0.116)
Pre-Treatment Mean	0.489	1.978	0.237	0.705	1.784	5.287	29.087	0.957

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 through 4 (inclusive) from the CS and SDID estimators. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are deaths within the first year of life among premature infants due to the cause listed in the column header per 1,000 premature births. For the CS estimator, observations are weighted by the number of premature births in each cell. Panel A includes the full sample; panels B and C include infants born to non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID.

Table A13: Descriptive Statistics, HCUP (2000-2019)

	(1) Full Sample	(2) Ever-Treated States	(3) Never-Treated States
Hospitalization Outcomes			
Non-Delivery Stays per 1,000 Births	137.806	131.651	150.437
	(33.878)	(26.482)	(42.826)
Non-Delivery Charges per Birth	5,085.832	4,647.992	5,984.342
	(2,670.269)	(2,073.804)	(3,432.625)
State-of-Residence/Hospital-State/Year Obs.	471	162	309

Note: The unit of observation is a state-of-residence/hospital-state/year-of-discharge cell. Each cell reports a weighted mean with standard deviation in parentheses, where observations are weighted by the number of deliveries in each cell. Discharges represent the universe of inpatient admissions for hospitals in the following state-years: Arizona: 2000–2018; California: 2003–2011; Florida: 2000–2019; Kentucky: 2000–2019; Maryland: 2000–2019; New Jersey: 2000–2019; New York: 2000–2018; Rhode Island: 2002–2019; South Carolina: 2000–2019. Residents of always-treated states are dropped from the sample. Ever-treated states with residents in the sample include California, Georgia, Illinois, Maryland, New Jersey, New York, Ohio, and South Carolina.

Table A14: p-Values on Inpatient Hospitalization Estimates from Different Inference Procedures, HCUP (2000-2019)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Callaway and Sant'Anna (CS)			Synthetic Difference-in-Differences (SDID)			
	Multiplier Bootstrap	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided	Block Bootstrap, 200 Reps	Block Bootstrap, 50 Reps	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided
Panel A: Hospitalization Rate per 1,000 Births							
Overall	0.417	0.244	0.552	0.614	0.652	0.242	0.622
Digestive	0.024	0.077	0.152	0.380	0.382	0.201	0.353
Immune-related	0.531	0.325	0.680	0.644	0.665	0.220	0.578
Nutrition-related	0.370	0.272	0.539	0.870	0.886	0.435	0.866
External	0.101	0.148	0.310	0.741	0.691	0.333	0.550
Ill-defined	0.865	0.476	0.898	0.958	0.955	0.582	0.955
Congenital	0.887	0.448	0.918	0.350	0.296	0.105	0.270
Perinatal	0.005	0.032	0.043	0.798	0.812	0.448	0.788
Other	0.977	0.462	0.989	0.949	0.942	0.596	0.943
Panel B: Average Charges per Birth							
Overall	0.000	0.018	0.039	0.087	0.107	0.040	0.085
Digestive	0.000	0.013	0.022	0.163	0.173	0.117	0.201
Immune-related	0.018	0.051	0.136	0.020	0.033	0.023	0.039
Nutrition-related	0.339	0.254	0.504	0.717	0.748	0.345	0.769
External	0.015	0.061	0.117	0.053	0.081	0.083	0.117
Ill-defined	0.263	0.287	0.522	0.151	0.165	0.119	0.173
Congenital	0.009	0.069	0.127	0.566	0.584	0.263	0.543
Perinatal	0.000	0.003	0.014	0.409	0.386	0.220	0.342
Other	0.074	0.133	0.220	0.578	0.587	0.267	0.497

Note: Each cell reports the p-value for the simple average of the event-time effects over event periods 0 through 4 (inclusive) from different inference procedures and estimators. Column (1) reports multiplier (wild) bootstrap p-values for the observed treatment assignment using the CS estimator. Columns (2) and (3) report the corresponding one- and two-sided t-statistic-based randomization inference (RI) p-values for CS using 1,000 randomized treatment reassignments. Columns (4) and (5) report block bootstrap p-values for the observed treatment assignment using the SDID estimator with 200 and 50 bootstrap replications, respectively. Columns (6) and (7) report the corresponding one- and two-sided t-statistic-based RI p-values for SDID based on 1,000 randomized treatment reassignments, with the t-statistic for each treatment reassignment computed using 50 block bootstrap replications. Bootstrap inference is clustered at the state-of-residence/hospital-state level. Outcomes are non-delivery inpatient hospitalizations per 1,000 births in Panel A and non-delivery inpatient charges per birth in Panel B, overall in the first row and by primary diagnosis in the remaining rows.

Table A15: p-values on Breastfeeding Estimates from Different Inference Procedures, NIS-Child (2003-2017)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Callaway and Sant'Anna (CS)			Synthetic Difference-in-Differences (SDID)			
	Multiplier Bootstrap	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided	Block Bootstrap, 200 Reps	Block Bootstrap, 50 Reps	t-Statistic RI, One-Sided	t-Statistic RI, Two-Sided
Panel A: Full Sample							
Ever Breastfed	0.001	0.013	0.035	0.015	0.004	0.003	0.005
Breastfed, 3 months	0.000	0.000	0.001	0.004	0.003	0.003	0.008
Breastfed, 6 months	0.000	0.012	0.023	0.304	0.269	0.127	0.242
Breastfed, 1 year	0.001	0.019	0.049	0.754	0.736	0.350	0.731
Panel B: White Infants							
Ever Breastfed	0.009	0.032	0.080	0.374	0.382	0.189	0.386
Breastfed, 3 months	0.084	0.111	0.211	0.558	0.450	0.244	0.496
Breastfed, 6 months	0.992	0.510	0.995	0.601	0.545	0.287	0.559
Breastfed, 1 year	0.364	0.768	0.498	0.178	0.123	0.069	0.137
Panel C: Non-White Infants							
Ever Breastfed	0.046	0.084	0.186	0.002	0.001	0.001	0.002
Breastfed, 3 months	0.000	0.006	0.011	0.011	0.006	0.006	0.011
Breastfed, 6 months	0.003	0.028	0.060	0.265	0.259	0.120	0.259
Breastfed, 1 year	0.008	0.048	0.112	0.201	0.251	0.104	0.252

Note: Each cell reports the p-value for the simple average of the event-time effects over event periods 0 through 4 (inclusive) from different inference procedures and estimators. Column (1) reports multiplier (wild) bootstrap p-values for the observed treatment assignment using the CS estimator. Columns (2) and (3) report the corresponding one- and two-sided t-statistic-based randomization inference (RI) p-values for CS using 1,000 randomized treatment reassignments. Columns (4) and (5) report block bootstrap p-values for the observed treatment assignment using the SDID estimator with 200 and 50 bootstrap replications, respectively. Columns (6) and (7) report the corresponding one- and two-sided t-statistic-based RI p-values for SDID based on 1,000 randomized treatment reassignments, with the t-statistic for each treatment reassignment computed using 50 block bootstrap replications. Bootstrap inference is clustered at the state-of-residence (at birth) level. Panel A includes the full sample; panels B and C include non-Hispanic white and non-white infants (i.e., Black, other race, or Hispanic), respectively.

Table A16: Hospital Breastfeeding Support Policy Effects on Infant Sleep Practices by Race/Ethnicity, PRAMS (2000-2018)

	New York City			New Jersey		
	(1)	(2)	(3)	(4)	(5)	(6)
	Usual Infant Sleep Position: Back	Infant Bedshares, Never	Infant Bedshares, Often or Always	Usual Infant Sleep Position: Back	Infant Bedshares, Never	Infant Bedshares, Often or Always
Panel A: Full Sample						
CS ATT	0.0203***	0.0017	-0.0056	0.00563	0.0538***	-0.00870***
	(0.00275)	(0.00691)	(0.00747)	(0.0133)	(0.00364)	(0.00164)
SDID ATT	0.0194***	0.0186***	-0.0144**	-0.00751	0.0436*	-0.0374**
	(0.00298)	(0.00505)	(0.00657)	(0.00991)	(0.0227)	(0.0172)
Pre-Treat Mean	0.517	0.310	0.256	0.674	0.402	0.181
Panel B: White Mothers						
CS ATT	0.00463	-0.00587	0.00935	0.00415	0.0450***	0.00157
	(0.00297)	(0.0125)	(0.00691)	(0.00924)	(0.00495)	(0.0110)
SDID ATT	-0.00392	0.0108	0.00783	-0.00710	0.0303	-0.0230*
	(0.00738)	(0.00828)	(0.00701)	(0.0121)	(0.0202)	(0.0128)
Pre-Treat Mean	0.628	0.398	0.193	0.756	0.519	0.0915
Panel C: Non-White Mothers						
CS ATT	0.0223***	0.00733	0.00577	0.0315**	0.0612***	-0.00699
	(0.00833)	(0.00930)	(0.00772)	(0.0134)	(0.00188)	(0.00466)
SDID ATT	0.0167	0.0275***	0.00580	0.0268	0.0129	-0.0633**
	(0.0198)	(0.00800)	(0.0189)	(0.0237)	(0.0324)	(0.0260)
Pre-Treat Mean	0.468	0.273	0.281	0.605	0.300	0.257

* significant at 10%; ** significant at 5%; *** significant at 1%.

Note: Each cell presents the simple average of the event-time effects over event periods 0 and 1 (inclusive) from the CS and SDID estimators, representing the full set of post-treatment periods for which effects are identified. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. Outcomes are the share of infants whose usual sleep position is on their back in columns (1) and (4), the share who never bed-share in columns (2) and (5), and the share who often or always bed-share in columns (3) and (6). For the CS estimator, observations are weighted by the sum of the PRAMS sampling weights in each cell. Panel A includes the full sample; panels B and C include non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively. Standard errors are clustered at the state-of-residence (at birth) level using a multiplier (wild) bootstrap for CS and a block bootstrap for SDID. For New York City estimates, we restrict the sample to be balanced around an event window from two years prior to one year after policy adoption; for New Jersey estimates, we restrict the sample to be balanced around an event window from three years prior to one year after policy adoption. See Appendix Table A2 for the state-years available in the PRAMS sample.

Table A17: p-Values on Sleep Practices Estimates from Different Inference Procedures, PRAMS (2000-2018)

	(1) CS Multiplier Bootstrap	(2) CS t-Statistic RI, One-Sided	(3) CS t-Statistic RI, Two-Sided
Panel A: Full Sample			
Usual Sleep Position: Back	0.101	0.197	0.395
Bedshares, Never	0.001	0.145	0.224
Bedshares, Often/Always	0.291	0.355	0.658
Panel B: White Mothers			
Usual Sleep Position: Back	0.381	0.408	0.697
Bedshares, Never	0.029	0.118	0.342
Bedshares, Often/Always	0.529	0.316	0.671
Panel C: Non-White Mothers			
Usual Sleep Position: Back	0.0005	0.013	0.211
Bedshares, Never	0.008	0.211	0.395
Bedshares, Often/Always	0.965	0.513	0.961

Note: Each cell reports the p-value for the simple average of the event-time effects over event periods 0 and 1 (inclusive) from the CS estimator using different inference procedures. Column (1) reports multiplier (wild) bootstrap p-values for the observed treatment assignment. Columns (2) and (3) report the corresponding one- and two-sided t-statistic-based randomization inference (RI) p-values using 1,000 randomized treatment reassignments. Bootstrap inference is clustered at the state-of-residence (at birth) level. Panel A includes the full sample; panels B and C include non-Hispanic white and non-white mothers (i.e., Black, other race, or Hispanic), respectively.

Appendix B Breastfeeding and Related Policies

Several national and state policies and initiatives in the U.S. aim to improve breastfeeding outcomes, such as the Affordable Care Act (ACA)[52] and laws protecting breastfeeding rights in the workplace and in particular public or private locations (Hawkins et al., 2013).[53] The U.S. has also seen a growth in hospitals participating in the Baby-Friendly Hospital Initiative's (BFHI) "Ten Steps to Successful Breastfeeding" program, launched by the World Health Organization (WHO) and UNICEF in 1991 (UNICEF, 2005). The BFHI program outlines ten hospital-level initiatives designed to increase breastfeeding, such as having a written breastfeeding policy and training healthcare staff to help women breastfeed (see Appendix Table B1 for all ten steps).[54] Hospitals that implement all of the recommended policies are designated as Baby-Friendly®. These ten

[52]For example, the ACA requires that employers provide adequate break time and space for employees to express milk, and that insurers cover lactation support and equipment rental with no cost sharing (Hawkins et al., 2015).

[53]Nearly all states currently allow breastfeeding in any public or private location; the majority also exempt breastfeeding mothers from public indecency laws.

[54]See https://www.babyfriendlyusa.org/about/

hospital-level initiatives closely overlap with the components of the state-level regulations we study, and during our sample period, the BFHI program became increasingly widespread. Between 2007 and 2019, the share of births occurring in a Baby-Friendly facility increased from less than 3% to nearly 28%. However, the majority of the state-level hospital regulations require implementation of only a relatively small subset of the BFHI ten steps, and many require that hospitals have a full-time lactation consultant on staff, which is not a requirement of the BFHI program.

Importantly, Lawler and Yewell (2023) show that the adoption of a state hospital breastfeeding policy is not significantly related to the share of that state's live births that occur in a Baby-Friendly facility or the number of Baby-Friendly certified facilities. In addition, Appendix Table B2 shows that state hospital breastfeeding policies were not systematically adopted alongside related state policies, such as paid family leave, requirements that hospitals provide parents information about SUID prevention and safe sleep, implementation of state Perinatal Quality Collaboratives, and other breastfeeding support policies. Together, this body of evidence mitigates concerns that our estimates reflect differential changes in the probability that hospitals achieve the Baby-Friendly designation or combined effects of related policies adopted in bundles.

Table B1: WHO/UNICEF "Ten Steps to Successful Breastfeeding"

1.	Have a written breastfeeding policy that is routinely communicated to all healthcare staff.
2.	Train all healthcare staff in skills necessary to implement this policy.
3.	Inform all pregnant women about the benefits and management of breastfeeding.
4.	Help mothers initiate breastfeeding within one half-hour of birth.
5.	Show mothers how to breastfeed and maintain lactation, even if they should be separated from their infants.
6.	Give newborn infants no food or drink other than breastmilk, unless medically indicated.
7.	Practice rooming in - that is, allow mothers and infants to remain together 24 hours a day.
8.	Encourage breastfeeding on demand.
9.	Give no artificial teats or pacifiers (also called dummies or soothers) to breastfeeding infants.
10.	Foster the establishment of breastfeeding support groups and refer mothers to them on discharge from the hospital or clinic.

Note: These represent the "Ten Steps to Successful Breastfeeding" as of 2017. WHO/UNICEF published a revised guide in 2018; however, our sample period corresponds to these earlier guidelines. Guidelines were obtained from https://www.who.int/nutrition/publications/infantfeeding/bfhi-national-implementation2017/en/

Table B2: Timing of Adoption of Parental Leave, Infant Health, and Breastfeeding Policies for Treated States

State	State hospital breastfeeding policy	Paid family leave+	Perinatal Quality Collab.±	SUID/Safe Sleep Info in Hospital‡	Provision of break time and private space by employers§	Employers prohibited from discriminating against breastfeeding employees	Breastfeeding permitted in any public/private location	Breastfeeding exempt from public indecency laws	Breastfeeding mothers exempt from jury duty
California	2014	2004	2006	1998	2002	2013	1997		2000
Georgia*	2002		2012		1999**		1999		
Illinois	2013		2012	2011, 2015	2001		2004	1995	2005
Louisiana	2007		2018	2016			2001	2001	
Maryland	2005		2006				2003		
Mississippi*	2016		2014			2006	2006	2006	2006
Missouri*	1999		2018				1999	2014	2014
New Jersey	2014	2009	2017			2018	1997		
New York*	2005	2018	2010	2017	2007	2007	1994	1994	
Ohio	2012		2007	2015			2005		
Pennsylvania	1998		2019	2010			2007	2007	
South Carolina	2015		2011	2018			2008	2008	
Texas	2016		2013	2009					1995

Note: *In the empirical analysis, the year a policy is considered to be in effect may differ from the legal adoption year. A state is considered to have an effective hospital policy in a given calendar year if it adopted the policy by June of that year. For states adopting policies in the latter half of the calendar year, the effective year is the following calendar year. Thus, in the empirical analysis, Missouri's policy effective year is coded as 2000, Georgia's as 2003, New York's as 2006, and Mississippi's as 2017.

+ Only 1 other state required PFL during our sample period: Rhode Island (2014). PFL requirements took effect in two additional states in 2020 (Washington, D.C. and Washington). Other state programs providing access to leave following childbirth (Temporary Disability Insurance and unpaid leave more generous than FMLA) did not change during our sample period.

± Perinatal Quality Collaboratives are initiatives aimed at improving maternal and infant health. State adoption dates generously shared by Jessica Kiser.

‡ These laws require hospitals to provide parents with information about SUID prevention and/or safe infant sleep prior to discharge from the hospital. Information obtained from Morcelle (2017) and author review of state statutes and regulations.

§ Under the Affordable Care Act, all employers with 50 or more employees are required to provide break time and private space for mothers, effective March 2010.

** Georgia law simply encourages employer provision.

Appendix C Policy Adoption Prediction Exercises

In this section, we examine whether baseline measures of breastfeeding, infant mortality, and maternal characteristics, as well as changes in these measures, were associated with state policy adoption.

C.1 Data

For these analyses, we draw on an additional data source, the American Community Survey (ACS), 2000-2019, obtained from IPUMS USA (Ruggles et al., 2025). The ACS is conducted by the U.S. Census Bureau and is designed to be representative at the county level. Unlike the Vital Statistics natality data, which has substantial missing information about maternal education and marital status across states and years,[55] the ACS allows us to construct a complete panel of maternal characteristics.

To construct the panel of maternal characteristics at the state-year level, we first restrict the sample to women ages 15–50 who report a birth in the last 12 months.[56] The survey reports respondents' current state of residence and state of residence one year prior; we use the latter to better approximate the state in which the birth occurred (only 3.3% of the sample reports moving between states in the past year). Women who report residing abroad one year prior are excluded from the sample (0.67% of the sample). We define the birth year as the survey year minus one, yielding birth cohorts spanning 1999–2018. The final sample consists of 621,863 women. Using ACS person-level weights, we collapse the data to the state-birth year level.[57]

For measures of breastfeeding, we use publicly-available state-level statistics published by the CDC for the 2000–2018 birth cohorts (Centers for Disease Control and Prevention, 2026). These data allow us to mirror the information available to policymakers, while facilitating linkage to other datasets. The measures include the share of infants in each birth cohort ever breastfed and the share breastfed at 6 months, derived from pooled waves of the National Immunization Survey (NIS). Notably, since these measures are constructed at the birth-cohort level, they closely

[55] This is due to (i) missing variables in certain years if states (or areas of states) had not yet adopted the 2003 revision to the birth certificate and (ii) California not reporting mothers' marital status after 2016.

[56] The survey question about past-year births is only asked of women ages 15–50.

[57] In 2012, there were problems in the collection of ACS data on women who gave birth in the past year, leading to the suppression of this variable in 59 Public Use Microdata Areas (PUMAs) within the states of Florida, Georgia, Kansas, Montana, North Carolina, Ohio and Texas. This affects approximately 0.1% of the sample of women of childbearing age; we drop these individuals from our sample.

correspond to those we construct using the restricted-access NIS-Child microdata.

C.2 Pretrend Test

To test for differential trends in outcomes or maternal characteristics prior to policy adoption, we estimate a modified two-way fixed effects event-study model in which the pre-policy adoption indicators are replaced with a linear trend in relative event time:

$$Y_{s,t-1} = \beta_0 + \beta_1 Pretrend_{st} + \sum_{k \in K} \beta_2^k HospitalPolicy_{st}^k + \delta_s + \tau_t + \varepsilon_{st}. \tag{4}$$

$Y_{s,t-1}$ denotes characteristics of the cohort born in state s in the year prior to year t. Birth-cohort characteristics are lagged by one year to approximate the information set available to policymakers in year t. That is, policy adoption in year t is assumed to respond to information about births occurring in year $t-1$.

In this specification, $Pretrend_{st}$ is a continuous measure of relative event time for ever-treated states and is set equal to zero in all years for never-treated states. Always-treated states are excluded from the sample. To align with our main event-study specifications, we restrict the pre-adoption period to the four years prior to policy adoption by setting $Pretrend$ to missing for relative event times earlier than -4. $HospitalPolicy_{st}^k$ is a vector of indicator variables equal to one if state s in year t has had a hospital breastfeeding support regulation in effect for k years, where $k \in K = \{0, ..., , 18\}$, and is zero otherwise. Including a full set of indicators for post-adoption years ensures that the coefficient on $Pretrend$ is identified only from trends prior to policy adoption. Thus, β_1 provides a test for linear pre-trends in observable maternal characteristics, breastfeeding initiation, or infant mortality.

The specification also includes a full set of state fixed effects and birth-year fixed effects, δ_s and τ_t, respectively. Observations are weighted by the sum of the ACS person-level weights in each cell and standard errors are clustered at the state-of-birth level.

C.3 Policy Adoption Prediction Regression

We next estimate a series of cross-sectional regressions to examine whether baseline state characteristics or changes in those characteristics predict subsequent adoption of hospital breastfeeding support policies.

We estimate the following specification:

$$Y_s = \beta_0 + \beta_1 X_s + \gamma_r + \varepsilon_s, \tag{5}$$

where Y_s is one of two measures of state policy adoption. First, $EverAdopt_s$ is an indicator variable equal to one if state s ever adopts a policy between 2001 and 2018, and is equal to zero otherwise. Second, $EarlyAdopt_s$ is an indicator variable equal to one if state s adopts a policy in the first half of the sample period (2001 to 2010), and is zero otherwise. Given that only 11 states adopt during this sample period, we do not examine predictors of early versus late adoption among adopting states. States that adopt in 2000 or earlier are omitted from all regressions. The earliest adoption observed in this estimation sample is Georgia, with a policy that was adopted in December 2002 and is considered effective in 2003.

X_s is a vector of the following state-level characteristics, as measured in the ACS: the shares of birthing women that are white, married, have a high school degree or less, and are 29 years of age or younger. We also include the share of infants ever breastfed, obtained from the public-use NIS data, and the one-year infant mortality rate, from the Vital Statistics data. In some specifications, these characteristics are measured for the baseline (year 2000) birth cohort. In alternative specifications, we instead include the *change* in each characteristic between the 2000 and 2002 birth cohorts to test whether short-run pre-policy trends predict subsequent adoption.

All regressions include census region fixed effects, γ_r, and we report heteroskedasticity-robust standard errors. Observations are weighted by the sum of the ACS person-level weights in each cell.