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THE EFFECT OF HOSPITAL BREASTFEEDING POLICIES ON INFANT HEALTH

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ABSTRACT

We study the effects of state hospital regulations intended to increase breastfeeding by requiring certain care standards during the postpartum hospital stay. Policy adoption increased breastfeeding initiation by 3.3–4.1 percentage points (4.2–5.2 percent) and breastfeeding at 3 months postpartum by 6–9 percent. Further, following adoption, infant mortality declined by 0.2 deaths per 1,000 live births (3.5 percent), and infant hospitalization charges fell. Declines in mortality and charges primarily occurred among medically vulnerable infants, consistent with evidence that breast milk supports immune development. Additional evidence suggests that improvements in infant sleep practices also played a role in reducing mortality.

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A data appendix is available at http://www.nber.org/data-appendix/w34032

1 Introduction

In the United States, breastfeeding is heavily promoted as the best method of infant feeding and increasing breastfeeding rates has long been a public health priority. However, causal evidence of the health effects of breastfeeding or breastfeeding-promoting policies, particularly in the context of a developed country, is extremely sparse. The purported health benefits of breastfeeding are based primarily on studies that rely on cross-sectional variation in breastfeeding across mothers (Ip et al., 2007). Results from these studies should be interpreted with caution as they cannot account for important confounders that may drive both breastfeeding behavior and improved child outcomes (Raissian and Su, 2018).

In this paper we provide novel evidence of the effects of breastfeeding-promoting policies on infant health. We focus on the adoption of state hospital regulations which are intended to increase breastfeeding by requiring certain care standards for new mothers and their babies during their delivery hospital stay. Over the past several decades these regulations have gained in popularity, with 16 states having implemented one as of 2022. Although the specifics of the regulations vary across states, frequent requirements include that mothers be informed of the benefits of breastfeeding, that hospital staff be regularly trained on initiation and support of lactation, and that there be a lactation consultant on staff. Lawler and Yewell (2023) show that these policies are successful at increasing breastfeeding. However, their effects on infant health have not been examined.

Breastfeeding-promoting policies may impact infant health through several channels. There may be direct health benefits of increased breast milk consumption (e.g., nutrition benefits or receipt of maternal antibodies). Infant health may also change if breastfeeding impacts parental preferences about complementary household behaviors, such as child care provision or infant sleep practices. Notably, Lawler and Yewell (2023) find that maternal time allocation significantly changed in response to these policies, with mothers increasing time spent on child care and decreasing time spent on formal work. Finally, infant health may improve if the additional in-hospital support provided to mothers increases information about other recommended parenting behaviors.

Using self-reported breastfeeding outcomes from the restricted-use National Immunization

Survey-Child (NIS-Child), we first estimate the effects of state hospital breastfeeding support policies on breastfeeding initiation and duration. We then consider the impacts of the policies on several measures of infant health. For our primary measure, we focus on mortality during the first year of life using the restricted National Vital Statistics System's linked birth and infant mortality files. We also examine changes in infant hospitalization rates and associated charges using Healthcare Cost and Utilization Project (HCUP) state hospital inpatient discharge records.

Finally, we consider whether state adoption of hospital breastfeeding policies impacted other household behaviors, namely infant sleep practices and infant maltreatment. Policy adoption may change these behaviors if breastfeeding changes household preferences regarding sleeping arrangements or child care provision, or if exposure in the hospital to information about recommended infant care practices, such as safe sleep, increases. In the case of maltreatment, we may also expect changes given claims that breastfeeding improves maternal-infant bonding.

To identify the effects of these hospital breastfeeding interventions, we estimate models that leverage plausibly exogenous variation in state policy adoption. Our empirical setting features staggered policy adoption and heterogeneous state policies, which may bias standard two-way fixed effects (TWFE) estimators (de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021); thus, we implement the Callaway and Sant'Anna (2021) (CS) estimator.[1] For some outcomes, the CS event-study estimates suggest differential pre-trends between the treatment and control states. Although in most cases, these differential trends would lead us to understate the policy effects, we supplement the CS analyses with estimates from synthetic difference-in-differences (SDID) models, following Arkhangelsky et al. (2021).

Our results show that the hospital breastfeeding support regulations were successful at increasing breastfeeding, consistent with the findings in Lawler and Yewell (2023). After the adoption of a regulation, the share of mothers reporting that they initiated breastfeeding increased by 3.3–4.1 percentage points (4.2–5.2% increase relative to the pre-treatment mean). We also find

[1] In our setting, the Callaway and Sant'Anna (2021) estimator represents a more generalized estimator that nests several of the similarly robust alternative estimators (e.g., de Chaisemartin and D'Haultfœuille, 2020a; Sun and Abraham, 2021), and it requires weaker identifying assumptions that result in less bias if parallel trends only holds approximately (Roth et al., 2023).

a statistically significant and robust 3.7–5.7 percentage point increase in the share of infants breastfed at 3 months, representing a 6–9% increase relative to the pre-treatment mean. The share breastfed at 6 months increased by 0.9–3.0 percentage points (2–6.5%), although the statistical significance is sensitive to specification choices, and effects largely fade out by one year postpartum. Heterogeneity analyses show that the breastfeeding increases were largest for non-white infants.

We also find that the adoption of state hospital policies improved infant health. 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, or by approximately 3.5% relative to the pre-treatment mean. The most robust declines occur for postneonatal mortality (deaths between 28 and 364 days of life), although we also find suggestive evidence of declines in neonatal mortality (deaths within the first 28 days of life). We explore effect heterogeneity by race and ethnicity and find the mortality declines were primarily driven by non-white infants, consistent with our breastfeeding results. Analyses by health at birth and by underlying cause indicate 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 results align with evidence from the medical literature showing that breast milk can support immune development (Ballard and Morrow, 2013; Jakaitis and Denning, 2014). We also find significant declines in mortality due to external injuries and sleep-related causes, often referred to as Sudden Unexpected Infant Death (SUID),[2] suggesting that changes in complementary household behaviors may be an important mechanism underlying these results.

Analyses using HCUP inpatient hospitalization discharge data similarly suggest that infant health improved following state policy adoption. Our results show that there were significant reductions in average charges for non-delivery hospitalizations, with the most robust reductions occurring for hospitalizations with digestive system diseases, diseases related to environmental exposures and immune strength, causes originating in the perinatal period, or injuries and other

[2]We define SUID as 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).

external causes as the primary diagnosis.[3] We also find evidence of reductions in the infant hospitalization *rate* for digestive-related diagnoses.

Finally, our analyses examining other household behaviors suggest that infant sleep practices improved following policy adoption, consistent with additional in-hospital support increasing mothers' exposure to recommended behaviors. Specifically, we find a 1.3 percentage point increase in the probability of reporting that the infant usually sleeps on their back and suggestive evidence of reductions in bed-sharing. These findings, combined with the observed declines in sleep-related mortality, point to improved infant sleep practices as a potentially important factor underlying the overall reduction in infant mortality. Additional analyses investigating policy effects on infant maltreatment suggest declines, but they are seldom statistically significant.

This paper makes a number of contributions to the literature on the causal effects of breastfeeding-promoting policies on child health. 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 the duration of breastfeeding, 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., 2022). 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).[4]

[3]See Section 3.2 for detailed definitions of these diagnostic groups.

[4]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: 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

By contrast, we provide causal evidence of the infant health effects of breastfeeding support policies in a developed country context. Although PROBIT was a high-quality study, effects from Belarus may not generalize for a number of reasons, including differences in the availability of alternatives to breast milk, water quality, maternity leave policies, and the childcare environment.[5] 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. Our data allow us to study effects among medically vulnerable infants, such as those born low weight or prematurely, and to identify average treatment effects among all mothers, not just those intending to breastfeed.

A small set of studies exploit quasi-experimental variation in breastfeeding support to examine impacts on child health and other outcomes. Fitzsimons and Vera-Hernández (2022) leverage variation in access to hospital lactation support generated by staff scheduling in the UK. 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.[6] In another study in the UK context, Del Bono and Rabe (2012) exploit distance from the mother's residence to the closest hospital that implemented the Baby-Friendly Hospital Initiative (BFHI) program, which outlines best practices for breastfeeding support.[7] They too 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. Relative to these studies, we exploit a distinct source of variation—the staggered implementation of state-level hospital breastfeeding support policies. Furthermore, we consider a comprehensive set of administrative measures of infant health that allow us to detect small but economically meaningful effects and do not suffer from the measurement error concerns of maternal-reported child health measures.[8]

of iron deficiency (Dewey et al., 1998; Jonsdottir et al., 2012).

[5] 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., 2022). Additionally, mothers had three years of maternity leave.

[6] 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.

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

[8] Other studies exploit within-mother (i.e., between-sibling) variation in breastfeeding and find that the breastfed

We also contribute to the broader literature that examines the impact of public policies and the early childhood environment on infant health. Much of this literature has examined the effects of more extensive 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., 2022),[9] or Medicaid access (Goodman-Bacon, 2018). In contrast, the literature examining the 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 build on this literature by showing that information and support-based interventions during the initial postpartum hospital stay may have large returns in terms of infant health improvements. This finding is in line with Hirani et al. (2022), 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)[10] or for which the greatest returns are realized in the first weeks or months of life (e.g., safe sleep practices).

The rest of the paper proceeds as follows: Section 2 provides background information on potential channels linking breastfeeding policies and infant health, as well as discussion of the hospital breastfeeding policies we study. In Sections 3 and 4, we describe our data sources and empirical strategy, respectively. Our main results on breastfeeding and infant health are presented in Section 5; supplemental analyses are presented in Section 6. Finally, Section 7 concludes.

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.

[9]Particularly relevant is the finding in Altindağ et al. (2022) that information on safe infant sleep practices, conveyed to households during nurse home visits in Denmark, led to significant reductions in infant mortality.

[10]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).

2 Background

2.1 Channels Linking Breastfeeding Policies and Infant Health

There are several key ways in which breastfeeding-promoting policies may impact infant health. First, there may be direct effects of increased breast milk consumption on infant nutrition, although the magnitude and direction of this effect depends on the counterfactual infant food. 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., 2022). Survey evidence from the US, 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.[11] Notably, evidence from several small randomized controlled trials suggests that, compared to high-quality formula, breastfeeding may *inhibit* infant weight gain and growth and may increase the risk of iron deficiency (Dewey et al., 1998; Jonsdottir et al., 2012; Quigley et al., 2019).

Another key mechanism through which breast milk may affect infant health is by supporting immune system development. At birth, infants have immature immune systems. Breast milk contains a broad set of 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). 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]

Infant health may also change if breastfeeding-promoting policies indirectly lead to changes in complementary household behaviors. Lawler and Yewell (2023) show that state adoption of hospital breastfeeding support policies significantly increased maternal time spent on childcare.

[11] Author calculations using data from the Infant Feeding Practices Survey II, a national survey fielded by the FDA in collaboration with the CDC from 2005–2007, show that only 0.07% of infants (2 out of 2,842) were not fed formula or breast milk during the first two months of life (Centers for Disease Control and Prevention, 2021). Moreover, among formula-fed infants, over 80% were fed milk- or soy-based formula containing fatty acids that are essential for brain development (and found naturally in breast milk), and over 97% were fed iron-fortified formula.

[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).

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). Additionally, breastfeeding may increase maternal-infant bonding, due to potential increased skin-to-skin contact and because breastfeeding stimulates the release of the hormone oxytocin (Hurst, 2007). Changes in bonding, in turn, may impact the quality of childcare provided in the household.

Policy adoption may also impact household behaviors such as infant sleep practices, which directly affect the risk of Sudden Infant Death Syndrome (SIDS) and infant suffocation (Altindağ et al., 2022). Changes in sleep practices may occur if breastfeeding changes parental preferences or if additional in-hospital support increases exposure to information on recommended parenting behaviors. Notably, safe sleep and the provision of "information and strategies to minimise 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.[13] As an example, Appendix Figure A1 displays a Michigan Department of Health flyer that provides information on breastfeeding and safe sleep practices.

The overall expected empirical effect on sleep practices is ambiguous in our context. 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.[14] On the other hand, prominent breastfeeding support organizations promote bed-sharing as a tool to increase breastfeeding success and maternal-infant bonding.[15] Survey evidence

[13] For similar reasons, we may expect maternal smoking or other substance use behavior to change following policy adoption. Both the CDC and American Academy of Pediatrics (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. Unfortunately, small sample sizes in available data sources have prevented rigorous analysis of maternal substance use.

[14] The AAP has recommended since 1996 that it is safest for infants to 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).

[15] 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).

from the US 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).

2.2 State Hospital Breastfeeding Regulations

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 a broad set of policies that aim to increase breastfeeding, including provision of workplace accommodations, insurance coverage of lactation-related services, and information-based interventions.

In this study, we focus on the effects of state-level hospital policies intended to increase breastfeeding by regulating the postpartum care that women receive during their hospital stay. To date, these policies have been adopted by 16 states, 13 of which adopt during our sample period. Appendix Figure A2 shows the timing of policy adoption across states. Although the specific regulations vary, states frequently require the following: (1) hospitals must have a lactation consultant on staff, (2) patients must be informed about the benefits of breastfeeding, (3) obstetric staff must receive regular lactation training, (4) hospitals must develop a written policy promoting breastfeeding, and (5) patients must 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.

Existing evidence demonstrates that the adoption of these state hospital policies causally changed measures of lactation support in adopting states. 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 US during this period. Importantly, 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.

3 Data Description

3.1 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 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.[16] Given the survey question asked, 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.

[16]The NIS-Child includes questions about exclusive breastfeeding. However, we do not examine exclusive breastfeeding due to a significant survey question redesign in 2006 and variable coding inconsistencies in later survey waves.

3.2 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 US each year as well as information from the birth certificate, such as child and maternal demographics and the infant's health at birth.[17] For infants who die before their 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 do not consider mortality among 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.[18] Additionally, we consider the one-year mortality rate among infants born prematurely (before 37 weeks of gestation) and those born low weight (less than 2500 grams).

We also examine mortality rates separately by underlying cause of death.[19] 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

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

[18]Neonatal mortality accounts for about two-thirds of infant mortality in the US 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).

[19]Effective with deaths occurring in 1999, causes of death were classified using ICD-10 codes rather than ICD-9 codes. Our results are nearly identical when we restrict the cause-of-death analysis to cohorts born in 1999 and after. Those results are available by request.

strength;[20] nutritional, metabolic, and related disorders;[21] injuries and other external causes (e.g., accidents, homicides, complications from surgical procedures); 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 relating 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 childcare environment or household behaviors, including infant sleep practices and infant maltreatment, may also impact exposure to infectious disease (environmental exposure and immune strength) and the incidence of injury or ill-defined conditions.

In light of the hypothesis that breastfeeding and breastfeeding-promoting policies may impact infant sleep practices, we also examine mortality due to sleep-related deaths, often referred to as Sudden Unexpected Infant Death (SUID). We follow the literature (e.g., Li et al., 2022; Moon et al., 2022) and define SUID to include SIDS (ICD-10 code R95), accidental suffocation and strangulation in bed (ICD-10 code W75) and other ill-defined and unspecified causes of mortality (ICD-10 code R99). This SUID grouping combines subcategories from the injuries and other external causes (accidental suffocation and strangulation in bed) and the ill-defined conditions (SIDS; other ill-defined and unspecified causes) categories discussed above.

[20] 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., otis media/ear infection, meningitis), and diseases of skin and subcutaneous tissue (e.g., dermatitis and eczema).

[21] 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 this aggregation 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."

3.3 HCUP State Inpatient Discharge Records

To further examine the impact of state adoption of hospital breastfeeding support policies on infant health, we use hospital inpatient discharge data obtained 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 for hospitals in nine states (Arizona, California, Florida, Kentucky, Maryland, New Jersey, New York, Rhode Island, and South Carolina) from 2000–2019.[22] 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).[23]

In these data each observation is at the discharge record level and includes information on the year of discharge, hospital state, patient state of residence, diagnostic codes,[24] 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).[25] We are unable to link patients over time.[26]

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.[27] We collapse the data to the patient state-of-residence/hospital-

[22]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.

[23]We only have HCUP data for California prior to their policy adoption.

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

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

[26]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.

[27]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.

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.[28] Although hospital charges are not a direct measure of health, they may measure a dimension of 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.

3.4 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 provide supplemental evidence on the effects of the state hospital policies. 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. 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.[29] Second, the 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 state-years the PRAMS data are available, as well as how that

[28]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.

[29]This variation is due both to states choosing not to participate 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 to participate has increased over time, from 20 states in 2000 to 48 states in 2018. The response rate threshold that must be met for the data to be publicly released has also changed over time, decreasing from 70% for 2000–2006, to 65% for 2007–2011, to 60% for 2012–2014, and to 55% from 2015 to present.

coincides with state policy implementation.

3.5 National Child Abuse and Neglect Data System Child Files

We also explore how the state-level hospital policies impact infant maltreatment using data from the National Child Abuse and Neglect Data System (NCANDS) Child Files (Children's Bureau, 2002–2021). The NCANDS data contain all referrals to state child protective services (CPS) agencies that received a response from those agencies (referrals that are "screened in").[30] The data contain case-level information, where a case is a report-child pair. Each report contains the calendar year in which the case was reported to the state CPS agency as well as information about the child involved (e.g., their age in years) and the alleged maltreatment (e.g., neglect, physical abuse). Each allegation of maltreatment has its own disposition (or determination), namely whether the allegation has been substantiated or not.

The Child Files are organized by the fiscal year in which the case received a disposition, which often differs from the report year. Given that nearly all cases receive a disposition within two years of being reported, we use data from fiscal years 2002–2021 to construct substantiation rate measures at the state-report year level from 2004–2019.[31] We focus on allegations of maltreatment—the number of children less than one year old with at least one report of neglect or physical abuse in the state-report year per 1,000 children less than one year old in a given state-report year—as well as substantiations—the number of children with at least one substantiated report of neglect or physical abuse in the state-report year per 1,000 children less than one year old in that state-report year. We also consider allegations and substantiations for neglect and physical abuse separately.[32] Data on state child population counts come from the US Census.

[30]We obtained these data via a restricted data agreement with the National Data Archive on Child Abuse and Neglect.

[31]States voluntarily submit data to NCANDS. Most states consistently report during the sample period. However, some states have missing data for part of the period. Appendix Table A3 provides information on the set of state-report years the NCANDS data are available, as well as how that coincides with state policy implementation.

[32]We focus on substantiations related to neglect and physical abuse as they are the two most common types of maltreatment, and information about these maltreatment types is available consistently throughout the sample period.

3.6 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 A2. While there is substantial variation in policy adoption across space and time, there is some clustering of adoption in the Northeast and South, and notably only one state in the Western census region (California) ever implements a state hospital policy.

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 breastfeeding, infant health, and other infant outcomes. 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).

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.[33] Once estimated, the individual $\widehat{ATT}^{CS}(g,t)$ can be aggregated to economically-relevant parameters by taking their weighted average. Relative to both standard TWFE estimates and other new estimators that are similarly robust to treatment effect heterogeneity (e.g., the de Chaisemartin and D'Haultfœuille (2020a) estimator, stacked difference-in-differences estimator), Callaway and Sant'Anna (2021) has the advantage that it allows for researcher choice in the weighting and aggregation of the $ATT^{CS}(g,t)$ estimates into summary treatment effect parameters.

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, in which we report 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 ever observed to participate in treatment for 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

[33]The composition of the always-treated group changes depending on the data set used and outcome under consideration.

control states prior to policy adoption. Treatment exposure is assigned based on an infant's state of residence at the time of birth,[34] 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.[35]

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 data sets. That is, since $ATT_k^{CS,ES}$ is the average treatment effect at k years of exposure to the treatment (among those groups that ever 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.[36] Following Callaway and Sant'Anna (2021), we report standard errors from a multiplicative bootstrap procedure clustered at the state level. Depending on the dataset being used, observations are weighted by relevant sample weights or by the number of observed births in the state-year cell.

There are two key identifying assumptions in this framework. 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,

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

[35]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.

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

the CS estimator will be less biased than alternative estimators that require a stronger assumption. The second 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, we examine the extent to which treatment and control states had parallel trends in the pre-treatment period.

4.2 Synthetic Difference-in-Differences Estimator

In some cases, the results of our Callaway and Sant'Anna (2021) analyses 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.[37] 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 and control states in period $g+k$ to the difference in the pre-treatment weighted average of the outcome for those same states:

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

In this specification, t_{min} is the earliest (pre-treatment) period observed in the relevant dataset,

[37] 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.

$\omega_{s,g}$ 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 from all specifications. 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 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 Breastfeeding Results

We begin by investigating 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 1. 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, 76.3% of infants are ever breastfed. By 3 months postpartum, only 58.9% are still breastfed, and this share declines to 44.4% by 6 months and 22.6% by one year after birth.[38] 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, infants and mothers look relatively similar in the ever- and never-treated states for the characteristics we observe. However, infants in never-treated states are relatively more likely to be non-Hispanic white and less likely to be Hispanic, and mothers in never-treated states are relatively older.

The event-study estimates from the CS and SDID estimators are presented in Figure 1. For both estimators, these figures show a significant and sustained increase in the share of mothers initiating breastfeeding after policy adoption, as well as the share breastfeeding at 3 months after birth. The figures also provide some evidence of dynamic treatment effects, with the share ever breastfeeding and breastfeeding at 3 months gradually increasing across the first several years following policy adoption. These dynamics are consistent with the idea that program implementation (e.g., training or hiring new staff) takes time. For the SDID estimator, the pre-treatment trends for all outcomes are similar between the treated and weighted set of never-treated states. However, the CS event-study estimates show that breastfeeding rates were differentially decreasing in treatment states relative to the full set of never-treated states prior to policy adoption. Thus, for breastfeeding outcomes, we present and discuss both the CS and SDID results, but we put more weight on the SDID estimates.[39]

In Table 2 we report a single summary estimate of the medium-term treatment effect of the policy, which is the simple average of the event-study estimates for post-treatment periods 0 through 4. The first and second rows present the results from the CS and SDID estimators, respectively, and show that policy adoption resulted in significant increases in breastfeeding initiation and duration

[38]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.

[39]Notably, 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 CS estimates are an underestimate of the true treatment effect.

in the medium term. Specifically, the share of infants ever breastfed increased by 3.3–4.1 percentage points (column 1). If we scale these estimates by the mean in the period prior to policy adoption, this translates to a 4.2–5.2% increase. We also find that the share breastfeeding at 3 months and 6 months postpartum significantly 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).

To help interpret these findings, we scale our estimates by the total number of births during our sample period in treated states in the 0–4 years after policy adoption, which is 9.63 million. 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. Notably, these increases only reflect changes in extensive margin breastfeeding. They do not capture changes in breastfeeding intensity (e.g., mix of breast milk and formula) that may have resulted from policy adoption.

Effect Heterogeneity by Race/Ethnicity In Table 3, 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 non-Hispanic white infants, effects are significantly larger for non-white infants (i.e., Black, other race, or Hispanic).[40] There is a 4.3–6.1 percentage point increase in breastfeeding initiation and a 5.3–7.7 percentage point increase in the share breastfed at 3 months among non-white infants, representing a 5.5–12.7% increase relative to the period before policy adoption. The event-study estimates for breastfeeding initiation and duration through 3 months postpartum are shown in Figure 2; Appendix Figure A3 displays them for duration through 6 months and one year postpartum.

[40] We do not explore effect heterogeneity using more granular race/ethnicity categories as the resulting subsamples become unbalanced, which prevents us from implementing the SDID estimator.

5.2 Infant Mortality Results

We next turn to 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 4 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. Similar to the patterns in the NIS-Child data, mothers in never-treated states are less likely to be Hispanic and more likely to be non-Hispanic white.

In Figure 3, we present the event-study estimates from the CS and SDID estimators. The figures generally show evidence of declines across the mortality outcomes following state policy adoption, with particularly striking and sustained decreases in one-year and postneonatal mortality rates. The figures also show no evidence of differential pre-trends across the mortality outcomes.

Averages of the event-study effects for 0 through 4 years post-policy adoption are presented in Table 5. We find 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). Columns 2 and 3 show that this reduction is driven by decreases in both the neonatal and postneonatal periods. Specifically, hospital breastfeeding regulations lead to a 0.10–0.13 decrease in 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 only the SDID estimate is statistically significant. Mortality in the 28–364 days after birth significantly declined by 0.11–0.12 deaths per 1,000 births, representing a 5.7% decline relative to the pre-treatment mean.

Effect Heterogeneity by Race/Ethnicity In Table 6, we show the medium-term effects of state-level hospital breastfeeding support policies on infant mortality separately by maternal race and ethnicity, with corresponding event-study estimates presented in Figure 4. Consistent with the breastfeeding results, the infant mortality declines are primarily driven by infants of non-white mothers (i.e., Black, other race, or Hispanic). There is a 0.43–0.52 decrease in deaths in the first year of life per 1,000 live births to non-white mothers, a 6.0–7.3% decline relative to the period before policy adoption. These declines occur during both the neonatal and postneonatal periods. There are no significant effects for infants of non-Hispanic white mothers.[41]

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.[42] We present the medium-term effects in Table 7 and the event-study estimates in Appendix Figure A4. A large and significant mortality decline occurred among infants born preterm. This finding is consistent with the notion that the direct immune-boosting benefits of breast milk may be particularly large for premature infants, as their immune systems are less developed at birth (Jakaitis and Denning, 2014). 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. There is weak evidence that mortality declined among infants born low weight, as the SDID estimate suggests a marginally significant decrease of 1.1 deaths per 1,000 low weight births after policy adoption. While the CS estimate is similar in magnitude, it is not precisely estimated.

Analyses by Cause of Death We graphically present estimates of the effects on infant mortality rates separately by primary cause of death in Figure 5. We also report the point estimates and standard errors in Appendix Table A4. 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

[41]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. Analyses of breastfeeding outcomes by maternal education show no evidence of heterogeneous effects (results available upon request).

[42]We do not explore breastfeeding effect heterogeneity by health at birth as the NIS-Child data does not include information about whether the infant was born preterm or low weight.

(Panel A). The declines in mortality due to perinatal causes are driven by decreases in the neonatal period (Panels B and C). Deaths in the first 28 days due to perinatal causes fell by 0.10–0.15 per 1,000 live births, although only the SDID estimate is statistically significant.[43]

There are also significant reductions in one-year mortality due to injury and external causes. These declines occur entirely during the postneonatal period, for which we estimate a statistically significant 0.04 decrease in deaths per 1,000 live births. Relative to the pre-treatment mean, this represents about a 13% decline in postneonatal injury-related mortality. We also find suggestive evidence of declines in one-year and postneonatal mortality due to ill-defined conditions and congenital abnormalities, although statistical significance is sensitive to estimator choice. The event-study estimates from the CS and SDID estimators for mortality by cause of death are presented in Appendix Figures A5–A7. For both perinatal and injury-related causes of death (the causes with robust declines across the CS and SDID estimators), we see little to no evidence of differential pre-trends.

Finally, given the hypothesis that breastfeeding and breastfeeding-promoting policies may impact infant sleep practices, we examine sleep-related mortality, often referred to as Sudden Unexpected Infant Death (SUID).[44] The medium-term effects are shown in Table 8, and corresponding event-study results are shown in Appendix Figure A8. We find statistically significant declines in sleep-related deaths, driven by decreases in the postneonatal period. Specifically, over the medium-term, policy adoption decreases postneonatal deaths due to SUID by 0.04 per 1,000 live births, about a 6% decline from the pre-treatment mean.

Scaling our estimates by the number of 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,

[43] We also investigated mortality by cause of death among infants born premature. The mortality declines among this group are largely driven by decreases due to perinatal causes. Results are presented in Appendix Table A5.

[44] We again note that SUID includes SIDS, accidental suffocation and strangulation in bed, and other ill-defined and unspecified causes of mortality. Thus, it consists of subcategories from the "injuries and other external causes" and the "other ill-defined and unspecified causes" groups that are presented in Figure 5 and Appendix Table A4.

356–375 of the averted deaths would have occurred due to external causes and injury (about 0.04 fewer deaths due to external causes per 1,000 births $\times$ 9.63 million births).

We caution against combining the mortality declines with the breastfeeding increases from Section 5.1 to calculate an implied two-stage least squares estimate. These policies likely impact infant mortality through multiple mechanisms, including via changes in complementary household behaviors. Lawler and Yewell (2023) find these policies increase the amount of time mothers spend providing child care, and we show in Section 6.2 that infant sleep practices change as well. We also note that 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.

5.3 Robustness Checks

We conduct a number of analyses to probe the robustness of the breastfeeding and infant mortality results. Appendix Tables A6 and A7 show that our main results are consistently 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 coding treatment as starting in the calendar year of adoption for all states (as opposed to calendar year of adoption + 1 for states adopting in July or later). In the case of infant mortality, the CS and SDID estimates generally align quite closely. However, for neonatal mortality, the SDID estimates tend be estimated more precisely. As an additional specification check, we verify that our CS breastfeeding results are not sensitive to omitting sample weights (Appendix Table A6, column 8), given that the sampling frame changed in the NIS-Child during our sample period. 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 data are available (2000–2015) (Appendix Table A7, column 7).[45]

To investigate the extent to which our medium-term effects may be driven by one individual treated state, we conduct a "leave-one-out" exercise, where we sequentially drop states that

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

adopted a hospital breastfeeding support policy during the sample period. The results from both the CS and SDID estimators are presented in Appendix Figures A9–A12 and generally suggest that no single state has an outsized impact on our estimates.

Maternal Selection As a falsification test, we examine if state policy adoption was associated with changes in measures of maternal demographic characteristics, the receipt of appropriate prenatal care, infant health at birth, or characteristics of the birth, as observed in the NIS-Child and Vital Statistics data. Given that the state hospital policies 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 affect fertility or perinatal health care.

The results for maternal demographic measures are presented in Appendix Tables A8 and A9 for analyses using the NIS-Child and the Vital Statistics data, respectively.[46] Across the demographic measures, the averages of the event-study estimates from 0–4 years after policy adoption are generally small in magnitude and none are statistically different from zero.

The results in Appendix Table A10 show that there are no significant changes in prenatal care, infant health at birth, or characteristics of the birth, with one exception. There is a marginally significant 0.001 decline in the share of infants born low weight. Relative to the pre-treatment mean, this represents only a 1% decrease. Taking the decline at face value, it is too small to explain much of the estimated decrease in infant mortality.[47] Furthermore, it is reassuring that the effect of policy adoption on the share of infants born very low weight is very small in magnitude and not statistically significant, given that deaths among very low birth weight infants (i.e., less than 1500 grams) make up about 80% of low birth weight (i.e., less than 2500 grams) infant mortality during

[46] In Appendix Tables A9 and A10, 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. This is due to: (i) variables being missing 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.

[47] To fully explain the decrease in one-year mortality, infants born low weight would have to have a one-year mortality risk that is 20 percentage points higher than those not born low weight (0.0002/0.001), which is implausibly large. For the cohorts we study, the average unconditional one-year mortality rate among low birth weight infants is 5.2 percentage points higher than that of infants not born low weight. Thus, the most pessimistic case is that the decline in the share of infants born low weight explains at most 25% of the infant mortality declines.

our sample period. Overall, the results of these exercises mitigate the concern that there were other factors concurrent with policy adoption that might drive our breastfeeding and mortality results.

6 Supplemental Results

6.1 Inpatient Hospitalization Results

To further explore the extent to which state adoption of hospital breastfeeding policies impacted infant health, we examine their 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. Descriptive statistics are presented in Appendix Table A11. 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.

Before presenting results, we first verify that our main breastfeeding estimates are robust to using the restricted set of states and years that are available in the HCUP data. Results are presented in Appendix Table A6, column 7. The estimated effects of policy adoption on breastfeeding initiation and duration are generally similar in magnitude for these state-years relative to the estimates from the full NIS-Child sample, although only the CS estimates are statistically significant.

We present the CS and SDID event-study estimates for non-delivery infant hospitalization rates and charges in Figure 6. 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.[48] The results show that there was no significant change in the rate of non-delivery hospitalizations among infants following state adoption of a hospital breastfeeding support policy (Figure 6, Panel A). We do find,

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

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 corresponding medium-term effects are presented in Table 9, column 1. Given the small number of states and small number of treated units in the HCUP sample, the bootstrap procedure may not perform well (Callaway and Sant'Anna, 2021). Therefore, for the CS estimator, we also report robust asymptotic normal standard errors.[49] Across both the CS and SDID estimators, we find no significant reduction in the inpatient hospitalization rate (Panel A, column 1). The SDID estimate in 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.

As with our main results, we examine the robustness of these findings to a number of alternative specification choices. The results are presented in Appendix Table A12 and show a robust reduction in average charges across the specifications we consider.

Analyses by Primary Diagnosis We examine effects on hospitalization rates and charges separately by primary diagnosis and present the results in Table 9, columns 2–9. The associated event-study estimates are in Appendix Figures A14 and A15. There were significant reductions in the hospitalization rate and charges for digestive-related causes (column 2).[50] For both the CS and SDID

[49] For the SDID estimator, when there are a small number of treated states, placebo-based inference may be preferable. With the balanced panel of state-years that the SDID estimator requires, we have exactly as many treated states (MD, NJ, NY, and SC) as control states (AZ, FL, KY, RI). We are therefore unable to implement the placebo-based procedure on the full sample as it requires strictly more control states than treated states. We explore the robustness of the decline in hospitalization charges to placebo-based inference by separately estimating the effect for each treated state and showing confidence intervals based on bootstrapped and placebo-based standard errors in Appendix Figure A13.

[50] 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,

estimators, we find reductions in charges for immune-related causes (Panel B, column 3), external causes and injury (Panel B, column 5), and causes originating in the perinatal period (Panel B, column 8). These findings align with the literature showing causal benefits of breast milk on digestive-related conditions and immune system development, as well as the declines in mortality among medically vulnerable infants and external cause and SUID mortality that we estimate in Section 5.2.

6.2 Infant Sleep Practices Results

We next examine the extent to which state adoption of hospital breastfeeding support policies impacts infant sleep practices. Although these policies do not explicitly target sleep practices, increased breastfeeding may change household preferences about sleeping arrangements. Additionally, information on recommended sleep behavior is frequently conveyed in tandem with information on breastfeeding (see for example, Appendix Figure A1), 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 hospital breastfeeding support policies may increase exposure to information about safe sleep.

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, sometimes, often, or always). The bed-sharing question was asked in an optional module for a limited set of state-years, and only two states provide identifying variation: New York (policy adopted in September 2005) and New Jersey (policy adopted in January 2014).[51] To prevent changes in sample composition from impacting our results, we use a restricted sample that is 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.[52] Furthermore, it is not possible to construct a sample that includes

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.

[51] 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.

[52] For New York City, the bed-sharing question is only available from 2004–2007 (two years prior to one year

both treated states and is balanced around *calendar* time (a requirement for the SDID estimator). Therefore, we report separate CS and SDID estimates of the treatment effect for New York and New Jersey. We also report the CS estimate using the pooled sample.[53] As with the HCUP analyses, we have a small number of states and small number of treated units, so the bootstrap procedure may not perform well. Therefore, we additionally report robust asymptotic normal standard errors for the CS estimator and placebo-based standard errors for the SDID estimator.

The results are presented in Table 10, and provide suggestive evidence that following adoption of a hospital policy, infant sleep practices improve. In the pooled sample, the probability of reporting that an infant usually sleeps on their back significantly increases by 1.3 percentage points (Panel A). We also find a 2.7 percentage point increase in the share reporting that the infant never bed-shares, although it is not statistically significant when using robust standard errors.

The results in Panels B and C of Table 10 show that the change in infant sleep position is driven by New York City, whereas changes in bed-sharing are driven by New Jersey. These heterogeneous effects across states may be due to the evolving nature of infant sleep recommendations. In particular, the American Academy of Pediatrics (AAP) took a much stronger stance against bed-sharing at 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). Event-study figures, presented in Appendix Figure A16, show no evidence of pre-existing differential trends. We again note, however, that these estimates are only identified for a limited set of post-treatment periods (i.e., 0 and 1 year after policy adoption) and adopting states (New York and New Jersey). We therefore interpret these results as suggestive. Nevertheless, together with our finding that policy adoption significantly reduced SUID mortality, these results suggest that increases in safe infant sleep practices may explain, in part, the observed improvements in infant health.

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.

[53]Results using the CS estimator and the full set of state-years that include these questions are similar and available upon request.

6.3 Infant Maltreatment Results

Motivated both by the estimated declines in infant mortality and hospital charges, especially those related to injuries, and that improved maternal-child bonding is one of the purported benefits of breastfeeding, we examine the effect of hospital breastfeeding regulations on infant maltreatment using the NCANDS data. We consider effects on allegations as well as substantiations of neglect and physical abuse of children under the age of one year. Results from the CS and SDID estimators are presented in Appendix Table A13 and Appendix Figure A17.[54] For the most part, we find negative average treatment effects over the medium-term, but they are seldom statistically significant. Thus, the pattern of point estimates is consistent with a decline in infant maltreatment following policy adoption, but the wide confidence intervals prevent us from drawing strong conclusions.

7 Conclusion

We examine how state-level hospital regulations intended to promote breastfeeding impact breastfeeding, infant health, and complementary household behaviors. Focusing on effects in the 0–4 years after policy adoption, we find that these policies increased breastfeeding, particularly initiation and duration through 3 months postpartum, and improved infant health. We find declines in infant mortality, with robust declines in the postneonatal period and among infants born preterm. Analyses by cause of death show pronounced reductions due to causes originating in the perinatal period as well as injuries and external causes. Supplemental results from inpatient hospitalization data similarly imply improvements in health among infants with digestive, immune-related, and external cause diagnoses as well as conditions originating in the perinatal period.

This collection of results suggests several mechanisms underlie our findings. We generally find larger health improvements among medically fragile infants, consistent with there being direct health benefits of breast milk consumption for these infants. Indirect policy effects are also likely

[54]States began reliably contributing maltreatment data at different times. Therefore, the sample used for the CS estimator is not balanced around calendar time. However, the SDID estimator requires a balanced sample. We have estimated the CS models on a balanced panel and our conclusions are unchanged. Those results are available by request.

at play as we observe an increase in safe infant sleep practices following policy adoption. The changes in sleep practices may mediate the declines in infant mortality and hospital charges, particularly those related to injuries and external causes.

Our results also show that the mortality improvements and increases in breastfeeding were stronger among non-white infants. There are several potential explanations for the larger effects among this group. For example, the information shock generated by these policies may have been greater for non-white mothers, or hospitals serving primarily white patients may have already been providing breastfeeding support before these policies were adopted. Disentangling the channels underlying the effect heterogeneity is beyond the scope of this paper and is an interesting avenue for future work.

Prior studies that investigate the effects of breastfeeding support policies have not found significant infant mortality benefits, but these studies have generally been underpowered and/or systematically excluded medically vulnerable infants (Kramer et al., 2001; Quigley et al., 2019).[55] By contrast, we use the universe of infant death records in the United States to examine an intervention adopted by 13 states, in which approximately 50% of all US-born infants reside. This allows us to detect small but economically meaningful declines in infant mortality, and to investigate effects among medically vulnerable infants.

Our core finding is that the adoption of state hospital breastfeeding support policies resulted in 0.22–0.23 fewer infant deaths per 1,000 births. This result translates to 2,090–2,234 averted infant deaths among infants born 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

[55] For example, 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. They also find fewer SIDS deaths among the treatment group, but the difference was not statistically significant. Several of the studies included in the Quigley et al. (2019) meta-analysis examine impacts on all-cause mortality and find no significant effect of formula relative to donor breast milk. However, most of the studies focused on short-run 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).

and injury.[56] These figures provide some rough bounds on the role that changes in complementary household behaviors, such as safe sleep and maternal time allocated to child care (Lawler and Yewell, 2023), play in the overall mortality decline as deaths due to external causes and injury are plausibly more strongly influenced by such behaviors than by the consumption of breast milk.

The estimated infant mortality improvements are similar or smaller in magnitude than estimated effects of other recent policy interventions targeting the immediate postpartum period. For example, Altindağ et al. (2022) study a nationwide information campaign that occurred in Denmark in 1991 that encouraged parents to put infants to sleep on their back or side (instead of their stomach). Relative to our estimates, they find much larger mortality reductions—the information campaign 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 was over ten times larger for infants born low weight or premature. More closely in line with our estimates, 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.

Our findings are not without limitations. We provide suggestive evidence that the direct effects of breast milk and changes in infant sleep practices may be important drivers of the effects we find, but are unable to examine other potentially relevant behaviors (e.g., intensive margin changes in breastfeeding, maternal substance use) or precisely identify the mechanisms underlying the infant health improvements. Furthermore, as hospitals increasingly provide lactation support independently of state requirements and as knowledge of safe infant sleep practices becomes more widespread, we may expect smaller effects of future state hospital breastfeeding support policies on infant health. Our analyses and results also open up several promising avenues for future work, including investigating the effects of these policies on mothers' outcomes as well as the long-term effects on children's outcomes.

[56] See Section 5.2 for details on these calculations.

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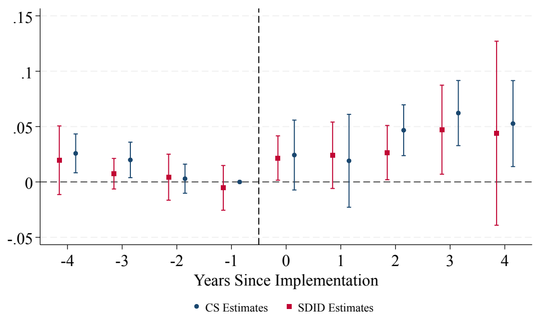
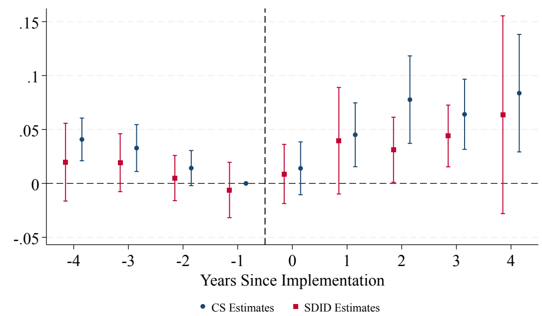
Figures

Figure 1: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes, NIS-Child (2003-2017)

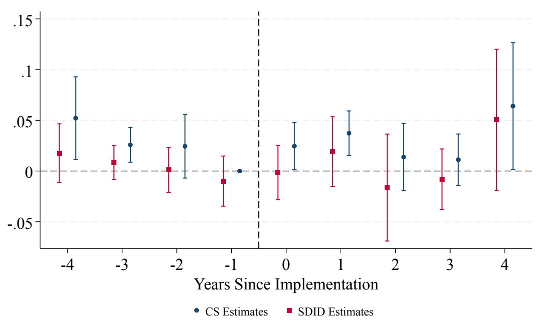
(a) Ever Initiated Breastfeeding



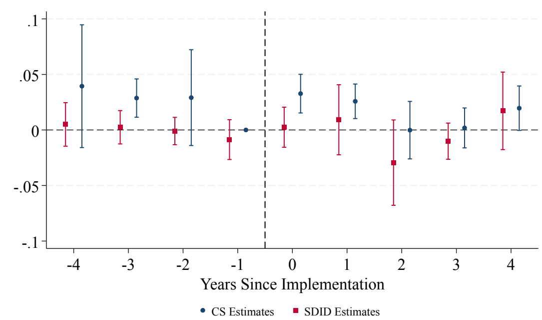
(b) Breastfeeding at 3 Months



(c) Breastfeeding at 6 Months



(d) Breastfeeding at 1 Year

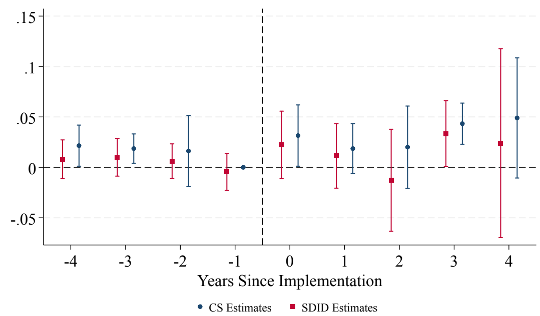


Note: Each figure presents the event-time effects obtained using the CS (blue circles) and SDID (red squares) estimators and their corresponding 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. For the CS estimator, observations are weighted by NIS-Child sampling weights. The outcome variable is described in the panel label. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy. For the CS estimator, estimated effects are relative to the year prior to policy adoption; SDID estimates are calculated relative to a weighted average of the pre-treatment period. Bootstrap standard errors are clustered at the state-of-birth level.

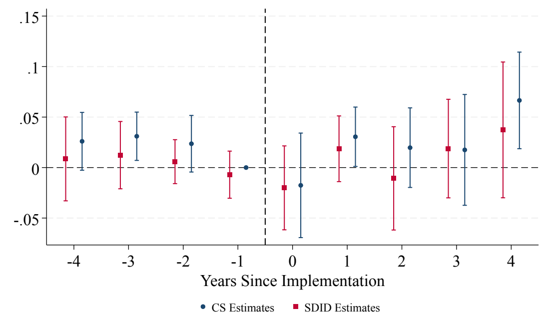
Figure 2: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes by Race/Ethnicity, NIS-Child (2003-2017)

Panel A: Non-Hispanic White Infants

(a) Ever Initiated Breastfeeding

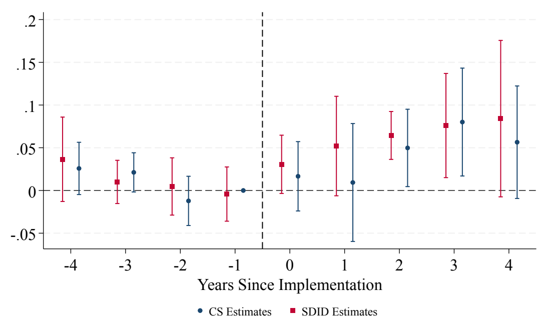


(b) Breastfeeding at 3 Months

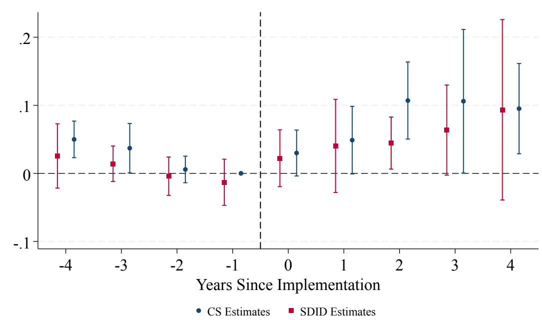


Panel B: Black, Hispanic, and Other Race/Ethnicity Infants

(c) Ever Initiated Breastfeeding



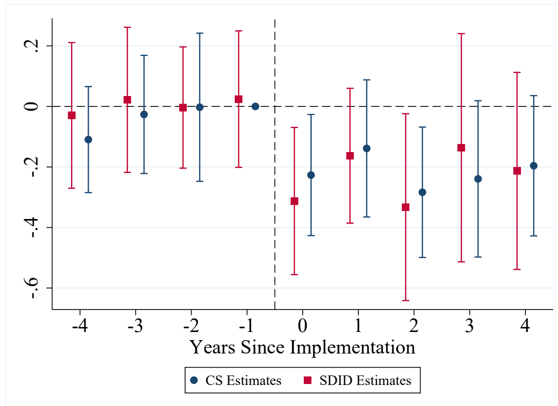
(d) Breastfeeding at 3 Months



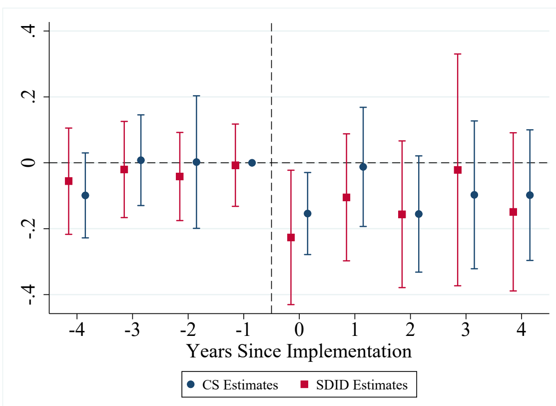
Note: Each figure presents the event-time effects obtained using the CS (blue circles) and SDID (red squares) estimators and their corresponding 95% confidence intervals. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and never-treated states are the control group. For the CS estimator, observations are weighted by NIS-Child sampling weights. The outcome variable is described in the panel label. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy. For the CS estimator, estimated effects are relative to the year prior to policy adoption; SDID estimates are calculated relative to a weighted average of the pre-treatment period. Bootstrap standard errors are clustered at the state-of-birth level.

Figure 3: Event-Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

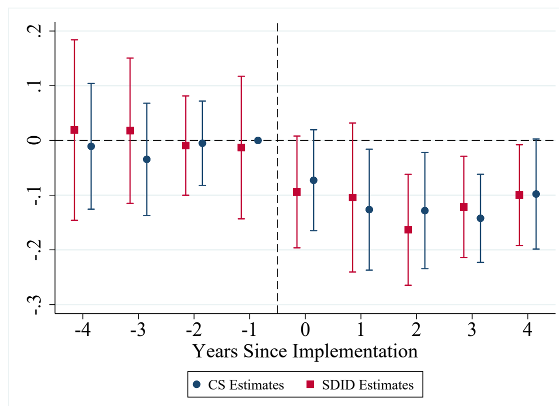
(a) One-Year Mortality Rate



(b) Neonatal Mortality Rate



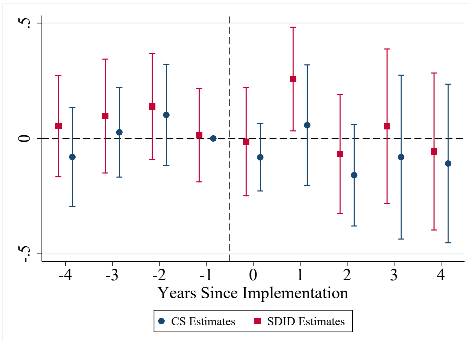
(c) Postneonatal Mortality Rate



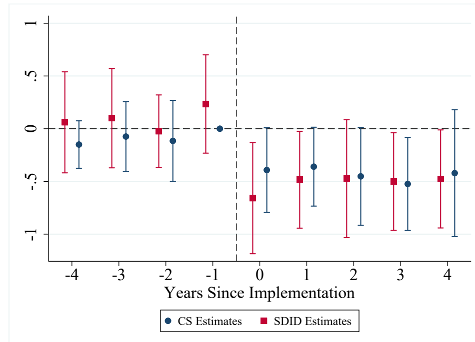
Note: Each figure presents the event-time effects obtained using the CS (blue circles) and SDID (red squares) estimators and their corresponding 95% confidence intervals. The unit of observation is a state-of-residence/year-of-birth cell, and never-treated states are the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcome in panel (a) is the number of deaths within the first year of life per 1,000 live births in that cell. The outcome in panel (b) is the number of deaths within the first 28 days of life per 1,000 live births in that cell. The outcome in panel (c) is the number of deaths between 28 and 364 days of life per 1,000 live births in that cell. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy. For the CS estimator, estimated effects are relative to the year prior to policy adoption; SDID estimates are calculated relative to a weighted average of the pre-treatment period. Bootstrap standard errors are clustered at the state-of-residence level.

Figure 4: 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)

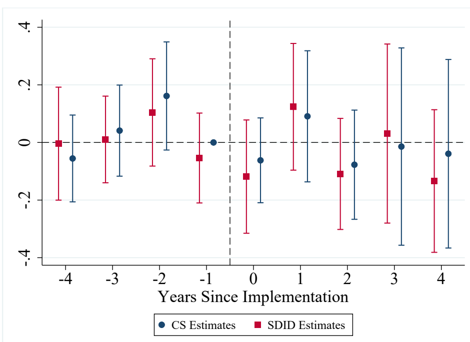
(a) One-Year Mortality Rate,
Infants of Non-Hispanic White Mothers



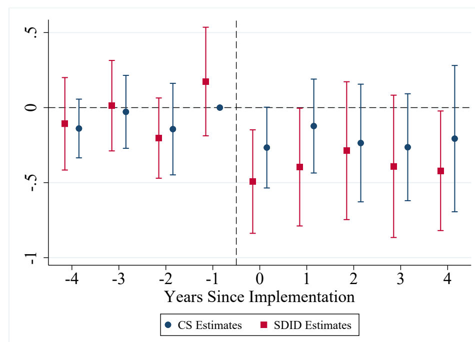
(b) One-Year Mortality Rate,
Infants of Non-White Mothers



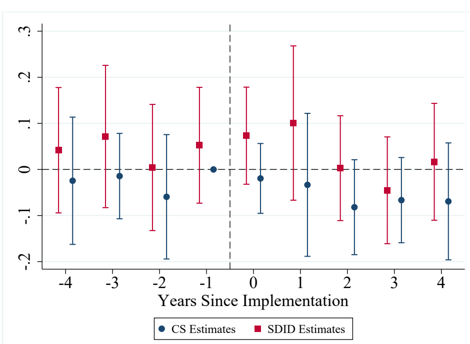
(c) Neonatal Mortality Rate,
Infants of Non-Hispanic White Mothers



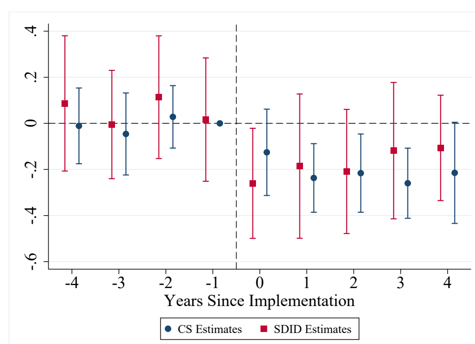
(d) Neonatal Mortality Rate,
Infants of Non-White Mothers



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



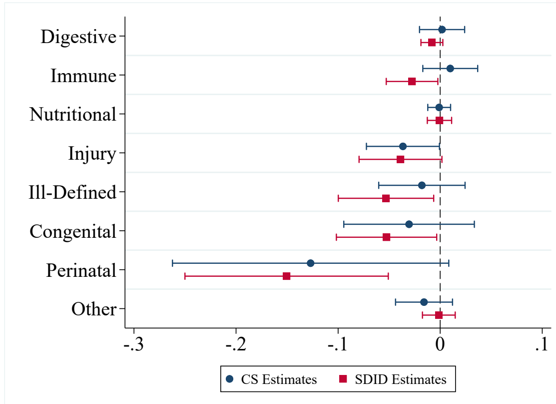
(f) Postneonatal Mortality Rate,
Infants of Non-White Mothers



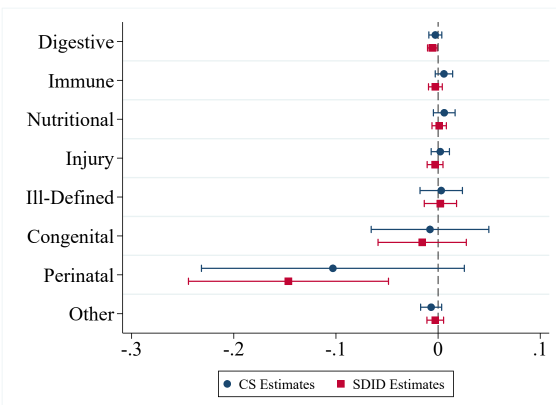
Note: Each figure presents the event-time effects obtained using the CS (blue circles) and the SDID (red squares) estimators and their corresponding 95% confidence intervals. The unit of observation is a state-of-residence/year-of-birth cell, and never-treated states are the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcomes in panels (a), (c), and (e) are the one-year, neonatal, and postneonatal mortality rates among infants of non-Hispanic white mothers. The outcomes in panels (b), (d), and (f) are the one-year, neonatal, and postneonatal mortality rates among infants of non-white mothers (i.e., Black, other race, and Hispanic), respectively. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy. For the CS estimator, estimated effects are relative to the year prior to policy adoption; SDID estimates are calculated relative to a weighted average of the pre-treatment period. Bootstrap standard errors are clustered at the state-of-residence level.

Figure 5: Effect of Hospital Breastfeeding Support Policies on Infant Mortality by Cause of Death, Cohort Linked Birth-Infant Death Data (1995-2018)

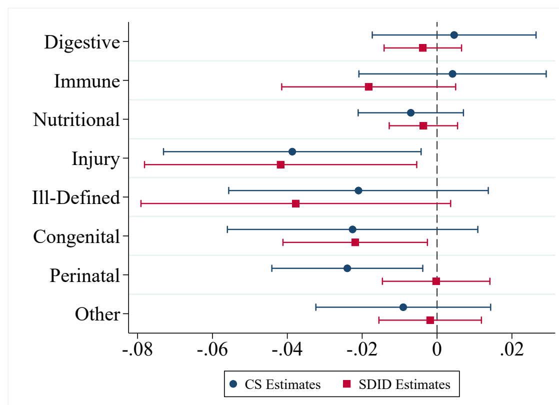
(a) One-Year Mortality Rate



(b) Neonatal Mortality Rate



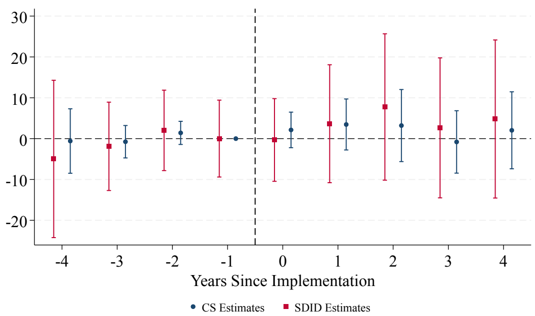
(c) Postneonatal Mortality Rate



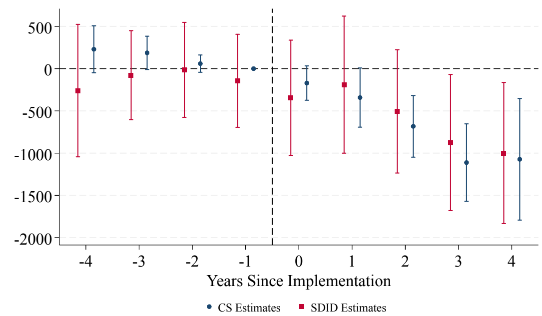
Note: Each point within a subfigure represents the simple average of the event-time effects over event periods 0 through 4 (inclusive) obtained using the CS (blue circles) and SDID (red squares) estimators and horizontal lines represent the corresponding 95% confidence intervals. The unit of observation is a state-of-residence/year-of-birth cell, and never-treated states are the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcomes in panels (a), (b), and (c) are the one-year, neonatal, and postneonatal mortality rates, respectively, by underlying cause of death and scaled per 1,000 live births in that cell. Bootstrap standard errors are clustered at the state-of-residence level.

Figure 6: 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 figure presents the event-time effects obtained using the CS (blue circles) and SDID (red squares) estimators and their corresponding 95% confidence intervals. The unit of observation is a state-of-residence/hospital-state/discharge-year cell and the outcome variable is described in the panel label. Never-treated states are the control group. For the CS estimator, observations are weighted by the number of deliveries in that cell. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy. For the CS estimator, estimated effects are relative to the year prior to policy adoption; SDID estimates are calculated relative to a weighted average of the pre-treatment period. Bootstrap standard errors are clustered at the state-of-residence/hospital-state level.

Tables

Table 1: 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)
Share Hispanic	0.284	0.355	0.203
	(0.180)	(0.189)	(0.127)
Share Non-Hispanic Black	0.132	0.148	0.115
	(0.097)	(0.104)	(0.086)
Share Other Ethnicity	0.104	0.103	0.106
	(0.057)	(0.038)	(0.073)
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 standard deviations in parentheses, where each observation is weighted using provided sample weights (landline only for 2003-2011, dual weights for 2012-2017). Always-treated states are dropped from the sample and include Arkansas, Kansas, Massachusetts, Missouri and Pennsylvania. Ever-treated states include California, Georgia, Illinois, Louisiana, Maryland, New Jersey, New York, and Ohio.

Table 2: 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
CS ATT	0.0410***	0.0569***	0.0302***	0.0160***
	(0.0118)	(0.0084)	(0.0076)	(0.0046)
SDID ATT	0.0327***	0.0374***	0.0088	-0.0021
	(0.0104)	(0.0118)	(0.0104)	(0.0067)
Pre-Treatment Mean	0.781	0.617	0.463	0.241

* 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). The first row reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the NIS-Child sampling weights. Infants are observed at ages 19–35 months, between 2003 and 2017. The outcome is described in the column header. Bootstrap standard errors are clustered at the state-of-birth level.

Table 3: Hospital Breastfeeding Support Policy Effects on Breastfeeding Initiation and Duration by Race/Ethnicity, NIS-Child (2003-2017)

	(1) Breastfeeding Initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Panel A: Non-Hispanic White Infants				
CS ATT	0.0325***	0.0233*	0.0002	-0.0126
	(0.0124)	(0.0135)	(0.0206)	(0.0139)
SDID ATT	0.0156	0.0088	-0.0071	-0.0160*
	(0.0123)	(0.0113)	(0.0140)	(0.0088)
Pre-Treatment Mean	0.789	0.633	0.510	0.283
Panel B: Black, Hispanic, and Other Race/Ethnicity Infants				
CS ATT	0.0425**	0.0773***	0.0481***	0.0367***
	(0.0213)	(0.0176)	(0.0162)	(0.0139)
SDID ATT	0.0614***	0.0528***	0.0178	0.0164
	(0.0135)	(0.0176)	(0.0177)	(0.0142)
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). The first row of each panel reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence (at birth)/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the NIS-Child sampling weights. Infants are observed at ages 19–35 months, between 2003 and 2017. The outcome is described in the column header for the race/ethnicity group described in each panel. Bootstrap standard errors are clustered at the state-of-birth level.

Table 4: 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	0.557	0.483	0.651
	(0.175)	(0.158)	(0.148)
Share Non-Hispanic Black	0.150	0.163	0.133
	(0.098)	(0.098)	(0.094)
Share Non-Hispanic Other Race	0.066	0.070	0.061
	(0.057)	(0.045)	(0.069)
Share Hispanic	0.227	0.283	0.156
	(0.171)	(0.185)	(0.116)
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 Education Missing	0.016	0.010	0.024
	(0.119)	(0.092)	(0.147)
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/year-of-birth cell. Each cell reports a weighted mean with standard deviations in parentheses, where each observation is weighted by the number of live births in the state-of-residence/year-of-birth cell. Always-treated states are dropped from the sample and include Arkansas, Kansas, and Massachusetts. Ever-treated states include California, Georgia, Illinois, Louisiana, Maryland, Mississippi, Missouri, New Jersey, New York, Ohio, Pennsylvania, South Carolina, and Texas.

Table 5: Hospital Breastfeeding Support Policy Effects on Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) One-Year Mortality	(2) Neonatal Mortality	(3) Postneonatal Mortality
CS ATT	-0.217**	-0.103	-0.114***
	(0.093)	(0.068)	(0.043)
SDID ATT	-0.232***	-0.132**	-0.117***
	(0.064)	(0.059)	(0.025)
Pre-Treatment Mean	6.216	4.213	2.003

* 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). The first row reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcomes are described in detail in the note to Figure 3. Bootstrap standard errors are clustered at the state-of-residence level.

Table 6: Hospital Breastfeeding Support Policy Effects on Infant Mortality by Maternal Race/Ethnicity, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) One-Year Mortality	(2) Neonatal Mortality	(3) Postneonatal Mortality
Panel A: Infants of Non-Hispanic White Mothers			
CS ATT	-0.075	-0.020	-0.054
	(0.114)	(0.103)	(0.050)
SDID ATT	0.034	-0.041	0.030
	(0.066)	(0.055)	(0.029)
Pre-Treatment Mean	5.086	3.365	1.721
Panel B: Infants of Black, Other Race, and Hispanic Mothers			
CS ATT	-0.430**	-0.219	-0.211***
	(0.195)	(0.157)	(0.068)
SDID ATT	-0.519***	-0.398***	-0.176***
	(0.111)	(0.088)	(0.057)
Pre-Treatment Mean	7.133	4.873	2.260

* 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). The first row in a panel reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcomes are described in detail in the note to Figure 3. Bootstrap standard errors are clustered at the state-of-residence level.

Table 7: Hospital Breastfeeding Support Policy Effects on One-Year Mortality by Health at Birth, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) One-Year Mortality, Premature	(2) One-Year Mortality, Low Birth Weight
CS ATT	-1.656**	-1.217
	(0.653)	(0.863)
SDID ATT	-1.487***	-1.114*
	(0.441)	(0.586)
Pre-Treatment Mean	36.552	52.261

* 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). The first row reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcome in column (1) is the one-year mortality rate among infants born preterm and the outcome in column (2) is the one-year mortality rate among infants born low weight. Bootstrap standard errors are clustered at the state-of-residence level.

Table 8: Hospital Breastfeeding Support Policy Effects on Sleep-Related Infant Mortality, Cohort Linked Birth-Infant Death Data (1995-2018)

	(1) One-Year	(2) Neonatal	(3) Postneonatal
CS ATT	-0.040	0.003	-0.043*
	(0.029)	(0.011)	(0.026)
SDID ATT	-0.058**	0.006	-0.043**
	(0.023)	(0.007)	(0.019)
Pre-Treatment Mean	0.848	0.100	0.749

* 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). The first row reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the number of births in that cell. The outcomes in columns (1), (2), and (3) are the one-year, neonatal, and postneonatal mortality rates, respectively, due to Sudden Unexpected Infant Death (SUID) and scaled per 1,000 live births in that cell. Bootstrap standard errors are clustered at the state-of-residence level.

Table 9: 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	Injury	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	-0.003	1.877**	0.011
	(2.914)	(0.761)	(2.519)	(0.634)	(0.183)	(0.433)	(0.259)	(0.743)	(0.373)
	[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
	[3.566]	[0.428]	[2.240]	[0.518]	[0.113]	[0.361]	[0.292]	[1.196]	[0.260]
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*
	(185.4)	(10.77)	(54.33)	(16.13)	(13.11)	(20.36)	(73.28)	(87.84)	(16.71)
	[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
	[193.6]	[12.51]	[36.01]	[9.962]	[9.588]	[11.96]	[76.67]	[112.7]	[15.92]
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 in the table presents the simple average of the event-time effects over event periods 0 through 4 (inclusive). The first row of each panel reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-residence/hospital-state/discharge-year cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by the number of deliveries in that cell. The outcome variable in Panel A is the hospitalization rate by the primary diagnosis described in the column header per 1,000 births in that cell. The outcome variable in Panel B is average charges by the primary diagnosis described in the column header per birth in that cell. For the CS estimator, robust and asymptotic standard errors clustered at the state-of-residence/hospital-state level are reported in parentheses. For both estimators, standard errors from a bootstrap procedure clustered at the state-of-residence/hospital-state level are reported in square brackets.

Table 10: 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: Aggregated Estimates			
CS ATT	0.0131*	0.0272	-0.0071
	(0.0078)	(0.0187)	(0.0086)
	[0.0084]	[0.0070]	[0.0067]
Pre-Treatment Mean	0.588	0.351	0.222
Panel B: New York City Estimates			
CS ATT	0.0203***	0.0017	-0.0056
	(0.0028)	(0.0067)	(0.0064)
	[0.0027]	[0.0069]	[0.0075]
SDID ATT	0.0194**	0.0186	-0.0144
	(0.0096)	(0.0186)	(0.0179)
	[0.0028]	[0.0073]	[0.0067]
Pre-Treatment Mean	0.517	0.310	0.256
Panel C: New Jersey Estimates			
CS ATT	0.0056	0.0538***	-0.0087
	(0.0098)	(0.0042)	(0.0185)
	[0.0133]	[0.0036]	[0.0016]
SDID ATT	-0.0075	0.0436*	-0.0374***
	(0.0202)	(0.0227)	(0.0010)
	[0.0077]	[0.0227]	[0.0171]
Pre-Treatment Mean	0.674	0.402	0.181

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

Note: Each cell in the table presents the simple average of the event-time effects over event periods 0 and 1 (inclusive), as this represents the full set of post-treatment periods where the effects are identified. The first row of each panel reports estimates using the Callaway and Sant'Anna (2021) (CS) estimator; the second row uses the synthetic difference-in-differences (SDID) estimator. The unit of observation is a state-of-birth/year-of-birth cell, and all specifications use never-treated states as the control group. For the CS estimator, observations are weighted by PRAMS sampling weights. The outcome is described in the column header. For the CS estimator, robust and asymptotic standard errors clustered at the state-of-birth level are reported in parentheses. For the SDID estimator, placebo-based standard errors are reported in parentheses. For both estimators, standard errors from a bootstrap procedure clustered at the state-of-birth level are reported in square brackets. In Panel B, we use a restricted sample that is balanced around an event window from two years prior to one year after New York's policy implementation. In Panel C, we use a restricted sample that is balanced around an event window from three years prior to one year after New Jersey's policy implementation. Panel A uses the full set of state-years included in either Panel B or Panel C. We do not report the SDID estimate in Panel A, as the sample is unbalanced in calendar time. See Appendix Table A2 for the full set of state-years that are available in the PRAMS sample.