The Effect of Hospital Postpartum Care Regulations on Breastfeeding and Maternal Time Allocation

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Abstract

We study the effects of state hospital regulations intended to increase breastfeeding by requiring certain standards of care during the immediate postpartum hospital stay. We find that these regulations significantly increased breastfeeding initiation by 3.8 percentage points (5.1 percent) and the probability of breastfeeding at 3 and 6 months postpartum by approximately 7 percent. We also provide evidence that these breastfeeding-promoting policies significantly increased maternal time spent on child care, crowding out time spent on formal work. Observed reductions in employment are concentrated among mothers with infants between 0 and 3 months of age.

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1 Introduction

Breastfeeding is widely considered to be an important parental investment in child health and development, as a large body of research in the medical literature shows breastfeeding is associated with positive infant and maternal outcomes (Eidelman and Schanler, 2012; Ip et al., 2007). In light of this research, both the American Academy of Pediatrics (AAP) and the World Health Organization (WHO) recommend that infants be breastfed for at least the first year of life (AAP, 2012; WHO, 2011). Policymakers in the United States have also responded by making breastfeeding a public health priority: improvements in breastfeeding rates are an explicit goal of the Department of Health and Human Services' "Healthy People 2030" initiative (US DHHS, 2020). Additionally, states have implemented a broad set of policies that target potential barriers to breastfeeding, including provision of workplace accommodations, insurance coverage of lactation-related services, and information-based interventions.

In this paper, we examine the effects of one such policy: state hospital regulations which are intended to increase breastfeeding by requiring certain standards of care during the immediate postpartum hospital stay. Over the past two decades, these regulations have been gaining popularity, and, as of 2019, sixteen states have implemented a hospital breastfeeding support policy. Although the specifics of the regulations vary across states, common requirements include that all new 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. In spite of their increasing popularity, very little is known about the effects of these policies.

Our analyses provide new evidence on both the direct and indirect effects of hospital postpartum care regulations that aim to support breastfeeding. Using self-reported breastfeeding outcomes from the National Immunization Survey-Child (NIS-Child), we first examine the effects on breastfeeding initiation and duration. We next consider potential impacts of the policies on maternal time allocation and employment, using data from the American Time Use Survey (ATUS) and the Current Population Survey (CPS). Breastfeeding-promoting policies may change maternal time use and employment both by imposing additional constraints on maternal time and by changing the relative costs of external versus in-home child care. To identify the effects of these hospital breastfeeding interventions, we estimate difference-in-differences models that leverage plausibly exogenous variation across states in the timing of policy adoption.

Our results show that the hospital breastfeeding support regulations were successful at increasing both the initiation and duration of breastfeeding. We find that after the adoption of a regulation, the probability of breastfeeding initiation increases by 3.8 percentage points and the probabilities of breastfeeding at 3 months, 6 months, and 1 year increase by 4.1, 2.8, and 1.2 percentage points, respectively. Across these different outcomes, estimated effects consistently represent a 5 to 7 percent increase relative to the respective outcome mean. Compared to other breastfeeding-promoting policies that have been studied, including mandated insurance coverage of lactation support services (Kapinos et al., 2017; Gurley-Calvez et al., 2018), paid family leave (Baker and Milligan, 2008; Kottwitz et al., 2016; Pac et al., 2019), and employment-based policies that require provision of break time and private spaces to express breast milk (Hawkins et al., 2013), our results suggest that hospital-level initiatives are substantially more effective at increasing breastfeeding *initiation*, while their estimated effects on duration are generally comparable or smaller.

Heterogeneity analyses show that these state hospital policies had the largest effect among non-Hispanic Black mothers. Notably, in our baseline year, non-Hispanic Black mothers are nearly 17 percentage points less likely to initiate breastfeeding than white mothers, and this gap persists for measures of breastfeeding duration. This finding suggests that hospital postpartum care regulations supporting breastfeeding may reduce disparities in breastfeeding initiation and duration.

We next provide evidence that the hospital breastfeeding policies impacted maternal time allocation across child care and formal work. Changes in breastfeeding behavior may lead to changes in employment and time allocation because breast milk requires *maternal* time to be spent on infant feeding (either breastfeeding or pumping), thus imposing additional constraints on maternal time.[1] Moreover, the need to frequently pump breast milk when

[1] Throughout we use the term "breastfeeding" to refer to both breastfeeding directly, or to pumping breast milk and then bottle feeding it to the infant. Notably, our data do not allow us to distinguish between these two methods of infant feeding.

separated from the infant may decrease the relative benefit of external child care.[2] We may also observe changes in time allocation as breastfeeding is generally more time intensive than other methods of infant feeding (Smith and Forrester, 2013).

Using data from the ATUS we find that after a hospital breastfeeding regulation is implemented, mothers of infants significantly increase their time spent on child care, crowding out time spent on formal work. Our results suggest this increase in maternal child care is concentrated in basic/physical care activities (e.g. dressing, bathing, feeding), as opposed to educational/recreational care activities (e.g. reading, playing). We also show that the increased maternal time burden is driven by an overall increase at the household level in parental time spent on child care; there is no significant change in the amount of time fathers spend on child care following policy adoption.

We further examine the margins along which women adjust their employment using data from the CPS. These analyses provide suggestive evidence that, following implementation of a state hospital breastfeeding policy, women with infants substantially reduce both their labor force participation and current employment by approximately 1.3 and 1.8 percentage points, respectively, or by 2.3 to 3.4 percent relative to the respective sample means. Estimated effects are largest for mothers with infants between 0 and 3 months of age (i.e., the same group which experiences the largest increase in breastfeeding); falsification analyses show no evidence of changes for mothers without infants. Notably, existing evidence from other contexts suggests that short-run reductions in maternal employment during the postpartum period may cause substantially lower wages and earnings in the long run (Kuka and Shenhav, 2020).

Finally, we provide evidence on the mechanisms through which the policies impact breastfeeding using data from the Pregnancy Risk Assessment Monitoring System (PRAMS) and other supplemental sources. Our results show that the postpartum care regulations resulted in meaningful changes in the care women received during their hospital stay, including increases in the probability that breastfeeding mothers reported receiving breastfeeding information and help from hospital staff. We also find that states that required hospitals to

[2] Mothers are recommended to pump every 2 to 4 hours, for approximately 15 to 20 minutes each time (AAP, 2021; CDC, 2020).

have a lactation consultant on staff experienced substantial increases in the number of International Board Certified Lactation Consultants (IBCLCs) in the state, and this regulation component is independently important for sustained breastfeeding.[3]

This paper makes a number of contributions to the literature on the effects of breastfeeding policies and on the determinants of parental investment more broadly. Our analysis of state-level hospital regulations provides novel evidence on the effects of immediate postpartum interventions on initiation and duration of breastfeeding. While a set of papers in the medical and public health literature have examined the effects of similar, though more comprehensive, hospital-level policies, they either rely on cross-sectional comparisons or they are unable to address endogenous selection of a delivery hospital (Kuan et al., 1999; DiGirolamo et al., 2001; Taddei et al., 2000; Kramer et al., 2001; Coutinho et al., 2005; Philipp et al., 2001; Hawkins et al., 2015a).[4] Also closely related is work by Fitzsimons and Vera-Hernández (2021, 2016) that leverages variation in access to hospital lactation support induced by staff scheduling at hospitals in the UK. They show that lower-educated mothers that gave birth on the weekends were less likely to receive lactation support in the hospital and also less likely to breastfeed.

Beyond these papers, most of the existing literature on the determinants of breastfeeding has focused on policies or factors that impact the later postpartum period, such as paid family leave or maternal return to employment (e.g., Baker and Milligan, 2008; Pac et al., 2019; Hamad et al., 2019), laws that address breastfeeding in the workplace (Hawkins et al., 2013), or laws mandating insurance coverage of lactation support services and equipment (Kapinos et al., 2017; Gurley-Calvez et al., 2018). Given that breastfeeding is an extremely time sensitive parental investment,[5] analyzing policies that target the immediate postpartum

[3]Ideally, we would fully characterize each policy based on the specific set of components it contains, in order to identify which policy component is most important for affecting outcomes. Unfortunately, however, because states adopt these regulatory components in bundles, we are limited in our ability to separately identify the effects of individual components. As the requirement to provide a lactation consultant is relatively well-identified, this is the only component that we examine separately.

[4]The exception to this is Del Bono and Rabe (2012), which examines the effects of hospital-level policies and overcomes the endogenous selection issue by assigning treatment exposure based on the treatment status of the hospital closest to the mother's residence, as opposed to the treatment status at the hospital the mother delivers at.

[5]In particular, milk removal from the breasts soon after birth is associated with increased efficiency of milk production; if milk is not removed then biological mechanisms cause the cells to stop producing milk. Thus, the timing and frequency of breastfeeding in the first few days postpartum are critical for successful

period is crucial for understanding the determinants of breastfeeding.

We also contribute to the literature that examines the determinants of maternal labor force outcomes and household allocation of time across formal work and child care. While there is a rich literature examining the impacts of a variety of other factors on maternal employment,[6] evidence on the impact of breastfeeding is sparse (Mandal et al., 2014). Thus, our findings fill an existing gap in the literature by providing new evidence that maternal employment and time allocation are responsive to breastfeeding-promoting policies. In doing so, we also highlight an important indirect effect of breastfeeding-focused interventions. Given the emphasis in the United States on increasing breastfeeding rates, understanding the impacts of targeted breastfeeding policies on maternal employment and time allocation is important for quantifying the true costs and benefits of these policies.

The rest of the paper proceeds as follows: Section 2 gives some background information on the research regarding the benefits of breastfeeding, as well as some existing policies that are intended to support breastfeeding. In Sections 3 and 4, we describe our data sources and empirical strategy, respectively. Our results on breastfeeding are presented in Section 5; results on maternal time allocation are presented in Section 6. Finally, Sections 7 and 8 provide discussion and conclusions.

2 Background

There is a substantial body of research in the medical literature that shows breastfeeding is associated with a wide range of positive short- and long-run outcomes for both the mother and the child (Eidelman and Schanler, 2012; Ip et al., 2007; Chowdhury et al., 2015). Much of this literature, however, relies on cross-sectional variation in breastfeeding across women and therefore is unable to address important unobserved confounders that may drive both breastfeeding behavior and other positive outcomes. The causal evidence on the benefits of

breastfeeding (Neville and Morton, 2001; Hurst, 2007).

[6]For example, a number of articles examine the impact of paid family leave (Baker and Milligan, 2010; Han et al., 2009; Bailey et al., 2019; Trajkovski, 2019), expansions of the Earned Income Tax Credit (Kuka and Shenhav, 2020), unemployment (Gorsuch, 2016), poor early child development (Lafférs and Schmidpeter, 2020; Frijters et al., 2009), changes in child-care prices (Baker et al., 2008; Amuedo-Dorantes and Sevilla, 2014), or immigration enforcement (East and Velásquez, 2020).

breastfeeding is much sparser, and primarily comes from a large randomized breastfeeding support intervention conducted in Belarus in the 1990s. That study showed that increased breastfeeding significantly reduced gastrointestinal infections, eczema, and other skin rashes in the first year of life, with no consistent evidence of benefits across the broad array of other outcomes considered (Kramer et al., 2001, 2007a,b, 2008; Oken et al., 2013; Yang et al., 2018).[7] In higher-income countries, there is quasi-experimental evidence showing mixed effects on cognitive development and no significant improvements in child health (Baker and Milligan, 2008, 2015; Del Bono and Rabe, 2012; Fitzsimons and Vera-Hernández, 2021).

Although the causal evidence on the short- and long-run benefits to breastfeeding is limited, it is heavily promoted as the best method of infant feeding.[8] Both the World Health Organization (2011) and the American Academy of Pediatrics (2012) recommend that, unless medically contraindicated, babies should be exclusively breastfed for the first 6 months of life with continued breastfeeding recommended through at least 1 year of age.[9] Currently in the United States the vast majority of mothers initiate breastfeeding (84.1 percent in 2017), although the rates of women meeting the AAP and WHO recommended thresholds are much lower: only 25.6 percent of infants are exclusively breastfed at 6 months (with 58.3 percent breastfed at all at 6 months) and only 35.3 percent are breastfed at 12 months (CDC, 2019a). Moreover, there are persistent disparities in breastfeeding rates; in particular, non-Hispanic Black mothers are consistently much less likely to breastfeed than either white or Hispanic mothers.

As a result of these persistently low breastfeeding rates, improving breastfeeding outcomes has long been a public health priority in the United States. Notably, in 2011 the U.S. Surgeon General issued a call to action to support breastfeeding (US DHHS, 2011), and improvements in breastfeeding rates have been specific objectives of the Department of Health and Human Services "Healthy People" initiative for the past several decades (CDC,

[7] Notably, the direct effect of breastfeeding on infant health and development depends substantially on what the infant would be fed in place of breast milk (see, for example, Jayachandran and Kuziemko (2011)). In the Belarusian context the primary alternative to breastfeeding was water or juice (Brenœet al., 2020).

[8] For example, the Centers of Disease Control and Prevention (CDC) refers to breast milk as "the clinical gold standard" (`https://www.cdc.gov/breastfeeding/about-breastfeeding/why-it-matters.html`) and the American Academy of Pediatrics says it is "uniquely superior for infant feeding" (AAP, 1997).

[9] Medical contraindications to breastfeeding are rare, but can be due to conditions of either the infant (e.g. glactosemia) or the mother (e.g. human T-cell leukemia virus type I) (AAP, 2012).

2001; US DHHS, 2019, 2020). National- and state-level policies that have explicitly aimed to improve breastfeeding outcomes include the Affordable Care Act (ACA)[10] and laws regarding breastfeeding rights in the workplace or mothers' legal rights to breastfeed in a given location (Hawkins et al., 2013). Nearly all states currently allow breastfeeding in any public or private location; the majority also exempt breastfeeding mothers from public indecency laws.

Breastfeeding is also heavily promoted in the United States by a variety of non-profit organizations. Most relevant in our context is the international Baby-Friendly Hospital Initiative's (BFHI) "Ten Steps to Successful Breastfeeding" program, launched by the World Health Organization (WHO) and UNICEF in 1991 (UNICEF, 2005). This program outlines a set of ten hospital-level initiatives designed to increase breastfeeding, such as having a written breastfeeding policy and training healthcare staff to help women breastfeed (see Appendix Table A1 for all ten steps);[11] if a hospital implements all recommended policies, they are officially designated as Baby-Friendly®. Notably, these ten hospital-level initiatives closely overlap with the components of the state-level regulations we study, and during our sample period this program became increasingly widespread: between 2007 and 2019 the percent of births occurring in a Baby-Friendly facility increased from less than 3 percent to nearly 28 percent.[12]

In this paper 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, eleven of which adopt during our sample period. Although the specific regulations vary across states, the most frequent requirements include 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

[10]Several components of the Affordable Care Act explicitly pertain to breastfeeding, including the requirements that employers provide adequate break time and space for employees to express milk, and that all new insurance policies in the individual and group market, and new Medicaid coverage provided under the Medicaid expansion, cover lactation support and equipment rental with no cost sharing (Hawkins et al., 2015b).

[11]See `https://www.babyfriendlyusa.org/about/`

[12]We examine in Section 5.3 the association between adoption of our state policy of interest and the diffusion of Baby-Friendly hospitals, as well as potential interaction effects.

breastfeeding, and (5) patients must be permitted to have their baby stay with them 24 hours a day ("rooming in"). Notably, all states that adopt during our sample period adopt all of their law components as a single bundle; we do not observe incremental adoption of requirements over time. We provide more detail on the specific provisions of each of the state policies and the year of policy adoption in Appendix Figure A1.

The policies we examine are unique relative to other state-level breastfeeding interventions in that they focus on the immediate postpartum period and serve to set standards for the care that hospitals provide to new mothers. As previously mentioned, these state hospital policies do have meaningful overlap with the recommended initiatives that make up the BFHI's Ten Steps program.[13] However, the majority of the state-level hospital regulations require only a relatively small subset of the ten steps to be implemented, and many of them include the requirement that hospitals have a full time lactation consultant on staff, which is a provision not addressed by the BFHI.

By studying the effects of state-level hospital postpartum care regulations this paper makes several important contributions to the existing literature. Given that breastfeeding is an extremely time sensitive parental investment, analyzing policies that target the immediate postpartum period is crucial for understanding the determinants of breastfeeding. Additionally, since these policies are becoming increasingly widespread, providing evidence on the effectiveness of these regulations is independently important. Finally, changes in maternal time allocation and employment represent potentially important impacts of breastfeeding-promoting policies, and these impacts have not previously been examined.

3 Data Description

We use several data sets to estimate the effects of state-level hospital policies on breastfeeding and maternal time allocation.

Data on breastfeeding are from the National Immunization Survey–Child (NIS-Child),

[13]Indeed, three of the hospital breastfeeding policies (California, Illinois, and Florida) explicitly reference the BFHI. For example, California's regulation states that hospitals must have an infant-feeding policy that promotes breastfeeding, and this policy should "follow guidance provided by the Baby-Friendly Hospital Initiative or the State Department of Public Health Model Hospital Policy Recommendations" (Cal. Health & Safety Code 123366(c)).

2003-2017. The NIS-Child is an annual state-representative survey conducted by the CDC that targets children aged 19-35 months. Breastfeeding outcomes are self-reported, and include information on both initiation and duration of breastfeeding.[14] The measures of breastfeeding duration we examine capture breastfeeding along the *extensive* margin; an infant is considered breast fed until they have "completely stopped breastfeeding or being fed breast milk" (CDC, 2021).

Since the hospital breastfeeding policies apply to care received during the immediate postpartum period, we assign policy exposure based on year of birth, and, as we only observe state of residence at time of survey, we restrict our sample to the set of children still residing in their state of birth.[15] An additional limitation of the NIS-Child is that child age at time of survey is only provided in bins (19-23 months, 24-29 months, and 30-35 months) and month of survey is not included in the public-use files. Given this, we approximate child's year of birth as (year of survey-2) for infants that were 19-23 months at the time of survey, and as (year of survey-3) for infants that were 24-29 months or 30-35 months at the time of survey.[16]

Information on household and maternal time allocation are drawn from both the American Time Use Survey (ATUS), 2003-2018, and the IPUMS Current Population Survey (CPS), 2000-2018 (Flood et al., 2020). The ATUS is a nationally representative household survey conducted by the U.S. Census Bureau and the Bureau of Labor Statistics. Respondents are asked to record a detailed time diary of all activities over a given 24 hour period, including location of the activity and who else was present. The ATUS sample is drawn from the population of households that participate in the CPS, with surveys distributed approximately 2 to 5 months after CPS completion. Survey distribution is equally split across weekends and

[14]We note that the survey questions ask mothers if they "breastfed or fed breast milk" to their infant. Thus, we are not able to distinguish between breastfeeding and bottle feeding breast milk.

[15]This represents 90 percent of the full sample. In Appendix Table A6, column 7, we test if policy adoption is associated with changes in the probability of still residing in the infant's state of birth. Our estimated coefficient is very small in magnitude and not statistically different from zero.

[16]Based on the calendar months that the NIS-Child is fielded, we calculate that infants that were between 19-23 months of age when surveyed in year t, should have been born between February of year t-2 and May of year t-1; infants that were between 24-29 months of age when surveyed in year t, should have been born between July of year t-3 and December of year t-2; infants that were between 30-35 months of age when surveyed in year t, should have been born between February of year t-3 and June of year t-2. Measurement error in the birth year should bias our estimates towards zero.

weekdays.

For our main ATUS analyses we restrict our sample to women that report having an infant under one year of age,[17] and examine outcomes for mothers whose youngest child is between 2 and 18 years of age as a falsification test. To construct our outcome measures we assign all reported minutes of activities to one of four mutually exclusive categories: child care, formal work, unpaid domestic work, and leisure. For some analyses we further decompose child care into two sub-categories: time spent on basic/physical care and time spent on educational/recreational care. We note here that breastfeeding falls under the basic/physical child care category and unfortunately is unable to be disaggregated from other infant care activities, including giving child a bottle and feeding a child. We present in Appendix Table A2 more detail on the types of activities that are included in each of the time use categories.

We additionally use data from the IPUMS Current Population Survey (CPS) to examine the impact of the state hospital policies on time spent on formal work and maternal employment outcomes (Flood et al., 2020). The CPS is a monthly household survey of the non-institutionalized U.S. population, conducted by the U.S. Census Bureau and the Bureau of Labor Statistics. It is intended to measure employment of the civilian labor force, and thus contains detailed information on adult labor supply;[18] the primary outcomes we examine are current labor force participation, current employment,[19] positive work hours in the past week, and number of work hours in the past week. The CPS is structured as a rotating short panel survey in which each household is surveyed for 4 consecutive months, then rotated out of the sample for 8 months, and then once again surveyed for 4 consecutive months.

[17]To ensure that we observe the full set of infants for a given birth cohort, we drop the infants born in the year prior to the first survey year (2002), as we only observe relatively older infants of that cohort, and those born in the last survey year (2018), as we only observe relatively younger infants of that cohort. In our robustness checks we verify that our estimates are robust to including these partial cohorts.

[18]Since the CPS is intended to measure *civilian* employment, individuals in the armed forces are not asked many of the employment-related questions. Therefore we drop those individuals from our sample.

[19]We note that this measure of employment should capture women who are on temporary leave from their job. Specifically, respondents are asked if, during the preceding week, they had a full or part-time job, and are explicitly told to include any job from which they were "temporarily absent." Individuals reporting that they worked in the past week or that they had a job are recorded as employed (United States Census Bureau, 2021).

As with the ATUS, for our primary set of CPS analyses we limit our sample to women with an infant less than one year of age;[20] we also examine outcomes for mothers without infants as a falsification test. Although child age is only provided in the data in one year age bins, we leverage the panel design of the survey in order to assign more narrow age ranges (in months) for household infants. This allows us to examine how the impact of the hospital breastfeeding regulation changes as infants age. Specifically, for infants that are born or experience a first birthday during one of the 4 month panels, we are able to assign month and year of birth, and therefore determine infant age in months relatively precisely. By construction, these infants will only be observed when they are either between 0 and 3 months of age, or between 9 and 12 months of age. For infants born or experiencing birthdays while the household is rotated out of the sample, we are only able to assign a range of possible birth dates (spanning at least ten months).[21] Given descriptive evidence showing that there are stark dynamics in maternal work during the first year postpartum (particularly during the first 3 months),[22] in our preferred specification we limit the sample to mothers for which we are able to determine that the infant is either between 0 and 3 months of age or between 3 and 12 months of age. We verify that our results are robust to including all infants in the sample.

We also use data from the CDC's Pregnancy Risk Assessment Monitoring System (PRAMS), 2000-2018, to provide supplemental evidence on the effects and mechanisms of the state hos-

[20]As with the ATUS, in order to ensure that we observe the full set of infants for each birth cohort, we drop the infants born in 1999 (as we only observe relatively older infants of that cohort) and those born in 2018 (as we only observe relatively younger infants of that cohort). In our robustness checks we verify that our estimates are robust to including these partial cohorts.

[21]Specifically, if we observe a household without an infant in survey month n and then observe an infant (age 0) in survey month $n + 1$, we assume the infant was born in the interim. Similarly, if we observe an infant age 0 in survey month n, and then observe the infant to be age 1 in survey month $n + 1$, we assume they were born one year prior. In the cases of infants that are present in every wave of their household's panel but are never observed turning age 1 (due to, for example, the household not participating in all eight survey waves), we assume that the infant's birth date lies in the following range: (one year prior to the date of last survey wave observed at age 0, date of first survey wave observed at age 0).

[22]For example, Han et al. (2008) shows using data from the Early Childhood Longitudinal Study-Birth Cohort of 2001 that while less than 10 percent of mothers returned to work in the first month after birth, over 40 percent had returned by 3 months after birth. Beyond three months post-birth the rate of return is much more gradual, with approximately 60 percent of mothers back at work by 9 months post-birth. Laughlin (2011) presents similar patterns for first-time mothers who gave birth between 2000-2007 using data from the 2008 panel of the Survey of Income and Program Participation. Those data show that nearly 64 percent of first time mothers work during the first 12 months after birth, and by 3 months approximately 70 percent of first time mothers that are going to return to work in the first year have done so.

pital policies. The PRAMS surveys women who had a live birth in the past 2 to 4 months, drawn from a sample of state birth certificate records. For our analyses we use data on self-reported breastfeeding and information regarding the types of breastfeeding-related care the mother received during her immediate postpartum hospital stay; for falsification tests we also utilize information on prenatal care receipt and infant health at birth. This data set has two notable limitations, however. First, the set of states with available PRAMS data varies substantially across years, with between 19 and 36 states reporting in a given year.[23] Second, the survey items that pertain to breastfeeding-related care received at the hospital are part of an optional module for states, and thus the set of states and years during which these questions are asked is further restricted. These survey questions also have the substantial limitation of only being asked to mothers who initiated breastfeeding. We provide detailed information on the set of state-years the PRAMS data are available for, as well as how that coincides with state policy implementation, in Appendix Figure A2.

To provide further evidence on the mechanisms through which the state hospital policies impact breastfeeding outcomes, we use data on the number of International Board Certified Lactation Consultants (IBCLCs) in a given state, collected from the CDC's annual Breastfeeding Report Card and from archived versions of the International Board of Lactation Consultant Examiners website, for 2006-2016.[24] While IBCLC represents the only professional certification for lactation consultants, we note that there are other certifications available that would satisfy the regulatory requirements (such as Certified Lactation Specialists and Certified Lactation Counselors). We focus on IBCLCs both due to data availability and because this is the key measure that the CDC includes in their annual Breastfeeding Report Card. We also collected data from the CDC on the percent of live births in a given

[23]This variation is due both to states choosing not to participate in the survey in a given year, and because data are not 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 in order for the data to be publicly released has also changed over time, decreasing from 70 percent for 2000-2006, to 65 percent for 2007-2011, to 60 percent for 2012-2014, and to 55 percent from 2015 to present.

[24]Current and historic CDC Breastfeeding Report Cards are available here: `https://www.cdc.gov/breastfeeding/data/reportcard.htm`. State-level counts of IBCLCs are available here: `https://iblce.org/about-iblce/current-statistics-on-worldwide-ibclcs/`. Historic counts were retrieved from archived versions of the website, using web.archive.org

state and year that occurred at a Baby-Friendly designated facility, for the years 2007-2018,[25] and data on the number of Baby-Friendly designated hospitals, 2000-2018, from the Baby-Friendly USA website.[26] These data allow us to examine if hospitals are more (or less) likely to receive the Baby-Friendly designation following state policy adoption, as well as explore the extent to which the policy impact varies based on the prevalence of Baby-Friendly hospitals in the state at the time of implementation.

Information on the state adoption of postpartum care regulations was obtained from the LawAtlas Policy Surveillance Program database;[27] 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 Figure 1; in Appendix Figure A3 we show how that timing coincides with the available sample periods of our primary data sets. While there is generally substantial variation across space and time in the adoption of these regulations, 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. In order to address the potential concern that unobserved region-level shocks are driving both the adoption of the policies and the observed changes in outcomes, we estimate robustness checks that include region-by-year fixed effects.

As previously discussed, across states there is substantial heterogeneity in the specific components of the regulations. In order to capture this heterogeneity, we characterize the relative strength of each policy as the fraction of eleven possible components (each of the ten items corresponding to the WHO/UNICEF "Ten Steps for Successful Breastfeeding," plus the requirement for a lactation consultant) that a given policy mandates.[28] We follow the

[25]Retrieved from https://www.cdc.gov/nccdphp/dnpao/data-trends-maps/index.html on October 13, 2020.

[26]Retrieved from https://www.babyfriendlyusa.org/for-parents/baby-friendly-facilities-by-state/ on May 20, 2022.

[27]http://lawatlas.org/datasets/baby-friendly-hospital-1525279705

[28]The eleven categories are as follows: required to have a breastfeeding policy that is communicated to staff; required to train healthcare staff in breastfeeding support practices; required to inform patients about breastfeeding; required to make lactation consultant available; required to help initiate breastfeeding; required to provide mothers instruction on how to breastfeed and how to maintain lactation, even if they are separated from infant; requirements regarding provision of non-milk food or drink to infants; required to allow breastfeeding on demand; prohibition of provision of pacifiers/artificial nipples to breastfeeding infants; required to permit rooming-in; required to provide information on/refer mothers to breastfeeding resources and support groups.

regulatory component categorization provided by the LawAtlas database. Appendix Figure A1 details the specific components of each state's regulations, and the overall frequency of each component. Only one state (New York) adopts a policy that mandates all eleven possible categories; the median adopting state mandates two out of the eleven categories.

Ideally, we would also characterize the regulations based on the specific set of components that they contain, in order to identify which policy component is most important for affecting outcomes. Unfortunately, however, because states adopt these components in bundles we are limited in our ability to separately identify the effects of individual components. As the requirement to provide a lactation consultant is relatively well-identified (adopted by 9 separate states, 3 of which mandate only a lactation consultant and no other components), we do provide some suggestive evidence about the importance of this regulatory component.

4 Empirical Strategy

To identify the breastfeeding effects of state adoption of hospital postpartum care regulations we use NIS-Child data and estimate dynamic difference-in-differences models that rely on plausibly exogenous variation in the timing of policy adoption across states. Additionally, given the recent econometric literature demonstrating that in empirical settings such as ours (with staggered treatment adoption) difference-in-differences estimates may be biased in the presence of time-varying treatment effects or treatment effect heterogeneity (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020a), we also provide dynamic treatment effect estimates following de Chaisemartin and D'Haultfœuille (2020b).[29]

Specifically, for our baseline event study models we estimate:

$$Y_{ist} = \beta_0 + \sum_{k \in K} \beta_1^k HospitalPolicy_{st}^k + \beta_2 X_{ist} + Z_{st} + \tau_t + \delta_s + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the outcome of interest for mother i residing in state s who had an infant born

[29] We do not use the de Chaisemartin and D'Haultfœuille (2020b) as our preferred baseline estimator as it faces other empirical limitations. For example, the estimator only allows for one treatment at the group-time level. Thus, it does not allow for estimation of differential treatment effects across sub-groups, as in our analyses in Section 5.4 and 6.2. It is also the case in our context that for regressions with the CPS data (Section 6.1) the bootstrap procedure is unable to account for all control variables.

in year t. X_{ist} is a vector of the following individual characteristics, as measured at the time of survey: child's gender, child's race/ethnicity (Hispanic, white, Black, with other as the excluded category), child's age (19- 23 months, 24-29 months, with 30-35 months as the excluded category), an indicator variable for receiving WIC benefits, number of other children under 18 years old living in the home (only 1 child, 2 to 3 children, with 4 or more children as the excluded category), an indicator variable for whether the infant is the mother's first born child, maternal education (less than high school, high school, some college, with college or above as the excluded category), an indicator variable for whether the mother is over the age of 29,[30] and an indicator variable for whether the mother is married.[31]

$HospitalPolicy_{st}^{k}$ is a vector of indicator variables equal to one if state s in year t has had a hospital breastfeeding support regulation in effect for k years, $K = \{\leq -5, ..., -2, 0, ..., 3, \geq 4\}$, and is zero otherwise (year -1 is the omitted category), thus β_1^k represents our vector of coefficients of interest and captures the dynamic effects of the hospital postpartum care regulations. For robustness we also estimate specifications in which the binary variable that captures whether the state has any policy is replaced with a continuous variable (between zero and one) that captures the relative strength of the policy. By estimating event study specifications we are able to test for dynamic policy effects, as well as examine the extent to which outcomes were trending similarly in treatment and control states during the periods prior to policy adoption. As the key identifying assumption in this difference-in-differences model is that outcomes would have evolved similarly in states that did and did not adopt a hospital breastfeeding policy, evidence of parallel trends during the pre-treatment period provides significant support for this assumption.

Z_{st} is a vector of other state policies, as well as state demographic and economic characteristics, which may potentially affect maternal behaviors and breastfeeding. Specifically, we control for the following state policies: laws that encourage or require employers to provide break time and/or private space for breastfeeding or expressing milk; laws prohibiting employer discrimination against breastfeeding employees; laws that allow breastfeeding in any

[30] This is the most detailed maternal age information that is consistently available across NIS-Child survey waves.

[31] Since in the NIS-Child all household level variables are measured at the time of survey (when the child is 19-35 months old), we show in Appendix Table A4 that our main results are not sensitive to removing the controls in the X vector.

public or private location; laws that exempt breastfeeding mothers from public indecency laws; laws that exempt breastfeeding mothers from jury duty; laws that require states to provide paid maternity leave;[32] and an indicator variable for whether or not a state has expanded Medicaid.[33] Information on workplace breastfeeding laws was obtained from Nguyen and Hawkins (2013), the National Council of State Legislators (2018),[34] and the United States Department of Labor Women's Bureau (2019).[35] Information on the implementation of Medicaid expansion is from the Kaiser Family Foundation (2015). Annual state-level demographic measures (fraction female; fraction Black, Hispanic, and other non-white races; fraction of individuals with high school degrees and college or more; fraction of individuals under 21 and between 21-64; and fraction of individuals below the federal poverty line) are constructed from IPUMS-Current Population Survey (Flood et al., 2020); we obtain annual state unemployment rates from the Bureau of Labor Statistics. In order to best capture the state characteristics that would have feasibly been relevant to the breastfeeding outcomes considered here, all variables contained in the Z_{st} vector are measured in the year in which the child was born.

All models additionally control for a full set of state of residence and child birth year fixed effects. We use sample weights as provided by NIS-Child,[36] and cluster standard

[32]We note that there is also state-level variation in the generosity of access to unpaid leave (beyond what is mandated by the federal Family and Medical Leave Act of 1993) and the requirements regarding availability of partially paid leave through Temporary Disability Insurance policies. These policies are not changing over our sample period, however, and so are controlled for through the inclusion of state fixed effects. See Appendix Table A3 for more detail on the overlap between state adoption of hospital breastfeeding support policies and other family leave and breastfeeding policies.

[33]While the ACA was a national-level policy, the effects may not be absorbed by year fixed effects since the requirement that all new insurance plans cover breastfeeding equipment and supplies, as well as lactation support and counseling without cost-sharing differentially affected households with private insurance. We do not control for this in our baseline specification because in the NIS-Child insurance status is not observed at time of birth (only at time of survey), and it is only observed for approximately 50 percent of our sample. As a robustness check we verify that all main results are not sensitive to controlling for whether the child is currently on Medicaid and including an interaction between post-ACA and Medicaid status. We also estimate equations where we include the interaction between post-ACA and either maternal education fixed effects or WIC receipt, as proxies for Medicaid status at time of birth. Our results are similarly robust to the inclusion of these controls and are available upon request.

[34]https://www.ncsl.org/research/health/breastfeeding-state-laws.aspx. Last accessed: July 16, 2020.

[35]https://www.dol.gov/wb/state-protections-pregnant-nursing-text.htm. Last accessed: July 16, 2020.

[36]In 2011 NIS-Child switched from single frame landline-only sampling to dual frame sampling that included landlines and cell phones, and in that year only both single and dual frame weights are provided. In all reported estimates we use dual frame weights starting in 2011. None of the main results are sensitive

errors at the state level (Bertrand et al., 2004). In the main tables we report standard difference-in-differences estimates for all models, in which the estimated effect of hospital breastfeeding policy adoption is summarized as the single coefficient on the indicator variable $HospitalPolicy_{st}$, which is equal to one if state s had adopted hospital postpartum care regulations by June of the infant's birth year t and is equal to zero otherwise.

5 Breastfeeding Results

5.1 Descriptive Statistics

Descriptive statistics for the NIS-Child sample are presented in Table 1. We provide variable means for the full sample (column 1) and separately for individuals who lived in a state that did versus did not adopt a hospital breastfeeding support policy during our sample period (columns 2 and 3, respectively). Across all states and years, 76 percent of mothers ever initiated breastfeeding, and 58 percent of mothers were still breastfeeding at 3 months. Beyond 3 months the rate of breastfeeding drops off rapidly, with only 44 percent breastfeeding at 6 months, and only 22 percent meeting the American Academy of Pediatrics (AAP) recommendation of breastfeeding at 1 year. The breastfeeding initiation and duration rates are consistently higher among mothers residing in states that adopted hospital breastfeeding laws, relative to non-adopting states. Across most observable characteristics, mothers and infants in adopting states look fairly similar to mothers and infants in non-adopting states. The notable exception is with regards to race/ethnicity: infants in adopting states are substantially more likely to be Hispanic or Black, and are less likely to be white, than infants in non-adopting states.

5.2 Effects on Breastfeeding

We first examine the effects of the hospital breastfeeding policies on initiation and duration of breastfeeding by using data from the NIS-Child and estimating the dynamic difference-in-differences model specified in equation (1). Specifically, we examine the effects of state policy

either to this decision or to the exclusion of weights.

adoption on initiation of breastfeeding and on any breastfeeding when the infant is 3 months, 6 months, and 1 year of age. We focus on these measures as they are key benchmarks in the literature.[37] The results from these analyses are presented in Figure 2. In this figure, each graph plots the coefficients from a separate regression in which the outcome is the variable given in the panel header and the treatment variable is specified as a binary indicator for adoption of a hospital breastfeeding policy.

Figure 2(a) shows that the implementation of a state hospital policy resulted in a significant and sustained increase in breastfeeding initiation. Furthermore, the estimated coefficients in the periods prior to adoption are consistently small in magnitude and not statistically different from zero. This finding is important for two reasons, as (1) it suggests that these policies were not endogenously adopted in response to state-level changes in breastfeeding initiation rates, and (2) it provides support for the identifying assumption that in the absence of policy adoption breastfeeding initiation rates would have evolved similarly between treatment and control states. For measures of breastfeeding duration we find similar patterns of effects (Figure 2 panels b-d), with significant and sustained increases following policy adoption, and no evidence of differential trends during the pre-period. These findings are robust to choice of treatment specification: estimates from models in which treatment is a continuous measure follow a similar pattern (see Appendix Figure A4).

For interpretation of effect magnitudes we focus on the single difference-in-difference estimates, which we present in Table 2. The estimates in column (1) show that the adoption of a state hospital policy increased the probability that a mother reported initiating breastfeeding by 3.8 percentage points (top panel), or by 5 percent relative to the sample mean. The coefficient in the bottom panel suggests that breastfeeding initiation rates would increase by 6 percentage points if a state adopted a policy that contained all 11 possible components; for the median policy strength (2 out of 11 components), this implies a 1.1 percentage point increase in breastfeeding initiation ($0.0605 \times 2/11$). Our estimates further show that adoption of hospital breastfeeding policies increased breastfeeding at 3 months, 6 months, and at

[37]These are also key benchmarks for policymakers as demonstrated, for example, by the fact that these are the measures used by the US Department of Health and Human Services in the Healthy People goals. We also note that while NIS-Child includes information about exclusive breastfeeding, we do not examine these outcomes due to a significant survey question redesign in 2006 and variable coding inconsistencies in later survey waves.

1 year by 4.1, 2.8, and 1.2 percentage points, respectively (top panel, columns 2, 3 and 4), although the estimated effect on breastfeeding at 1 year is only significant in specifications where the treatment variable captures the strength of the policy. Scaled by the relevant sample mean, the estimated effect of policy adoption on breastfeeding duration is quite stable, ranging from 5 to 7 percent.

We next perform several additional analyses in order to test the robustness of these results. First, we examine the sensitivity of the estimates to a number of different specification choices, such as excluding individual and state time-varying controls, including region-by-year fixed effects, and dropping always treated states. These results are presented in Appendix Table A4 and demonstrate that our results are remarkably robust across specifications. Second, to examine the potential for our results to be biased due to time-varying or heterogeneous treatment effects, we provide Goodman-Bacon (2021) decompositions in Appendix Figure A5 and Table A5. These decompositions show that the overall two-way fixed effect (TWFE) estimate is primarily identified off of comparisons between never adopting states and states that adopt during our sample period, reducing concerns that bias from time-varying treatment effects is driving our results. The individual 2×2 difference-in-differences coefficients from these comparisons are also clustered around the average effect (red line), suggesting treatment effects were relatively similar across states. To further address concerns that these potential biases are driving our results, we also implement the estimator proposed by de Chaisemartin and D'Haultfœuille (2020b).[38] The dynamic and placebo estimates from these analyses are presented graphically in Appendix Figure A6 and support our primary finding that adoption of hospital breastfeeding support policies resulted in a robust and sustained increase in breastfeeding.

Finally, as falsification tests we estimate the extent to which state policy adoption was associated with changes in the demographic characteristics of mothers giving birth (NIS-Child), the probability of receiving appropriate prenatal care, several measures of infant health at birth, and delivery modality (PRAMS). Given that the state hospital policies we examine should only impact postpartum care, changes in these other outcomes could

[38]Specifically, we use the *did_multiplegt* command in Stata and specify the *robust_dynamic* option. Standard errors are clustered at the state level and computed using 200 block bootstrap replications.

suggest that adopting states were also experiencing some other unobserved shocks that may affect outcomes during the postpartum period.[39] Results for maternal characteristics are presented in Appendix Table A6 and show no evidence that state adoption of a hospital breastfeeding policy was associated with positive maternal selection.[40] Similarly, estimated effects on measures of prenatal care receipt and infant health at birth, presented in Appendix Table A8, are generally close to zero and not statistically significant; only the probability of an infant being low birth weight is statistically significant, and the magnitude is small (0.1 percentage points). Thus, these results provide further support for the idea that the changes in breastfeeding we observe are driven by the hospital postpartum care policies, and not by other unobserved factors which may impact selection into motherhood or health care received during pregnancy.

5.3 Mechanisms

In this section we conduct a number of additional analyses in order to explore potential mechanisms through which the policies may have affected breastfeeding outcomes. First, we use self-reported information from PRAMS regarding the type of breastfeeding-related care women received during their postpartum hospital stay. Second, we examine the effect of the regulations on the prevalence of International Board Certified Lactation Consultants (IBCLCs) and how this varied for states that required hospitals to have a lactation consultant on staff versus those that did not. Finally, we examine whether adoption of a state hospital policy was associated with an increase in the probability that hospitals in the state were designated as Baby-Friendly, and whether the impact of the policy varied with respect to the baseline prevalence of Baby-Friendly facilities in the state.

We present in Table 3 the estimated effects of state hospital policy adoption on the probabilities that women who initiated breastfeeding report receiving various types of breastfeeding-

[39]We note, however, that changes in hospital breastfeeding policy may have spillovers more broadly to provider practice patterns. In particular, we might expect reductions in C-section deliveries, as anecdotal and descriptive evidence indicates that mothers that deliver via C-section are less likely to breastfeed and more likely to report breastfeeding difficulties (Hobbs et al., 2016).

[40]None of the estimates are statistically significant with the exception of a marginally significant *reduction* in the probability of having a college degree. Given the strong education gradient in breastfeeding behavior, however, this compositional change should bias us away from finding increases in breastfeeding rates. We also replicate these results using the PRAMS data; see Appendix Table A7.

related care during their immediate postpartum hospital stay. Specifically, we estimate the effect of policy adoption on the probability of reporting each of the following: received breastfeeding information from hospital staff, hospital staff helped with breastfeeding, allowed to breastfeed infant on demand, roomed-in with the infant, given a gift pack with formula, or connected with a breastfeeding support group prior to discharge. These results show that after adoption of a state hospital breastfeeding policy there were significant changes along 2 of the 6 dimensions of care we consider: mothers that initiated breastfeeding are more likely to report both that they received breastfeeding information from the staff and that the staff helped with breastfeeding. These results are consistent with the idea that hospitals changed the care they provided to breastfeeding mothers during their immediate postpartum stay. However, we note that since these particular survey questions are only asked of women who initiated breastfeeding, if the marginal woman deciding to breastfeed received different postpartum hospital care even in the absence of the laws, then the changes we observe across these outcomes may be driven by selection rather than by policy adoption. For completeness, since these survey questions are asked to mothers in a limited set of states and years (see Appendix Figure A2), we verify that we are able to replicate our main breastfeeding results using this same, smaller sample. We report these estimates in Appendix Table A9 (columns 3 and 4).[41]

We next examine the extent to which the breastfeeding support policies resulted in a meaningful change in exposure of women in a state to lactation consultants. Table 4 presents the estimates from difference-in-differences models in which the outcome variable is the natural log of the number of International Board Certified Lactation Consultants in a given state and year.[42] The estimate in column 1 shows that policy implementation resulted in a statistically significant 24 percent increase in the number of certified consultants (sample mean: 247 consultants in a given state-year). Moreover, if we replace the single hospital breastfeeding policy indicator with two separate indicators that capture (1) whether a state

[41]We also replicate our primary breastfeeding findings using all states and years of the PRAMS data, 2000-2018. These estimates are reported in Appendix Table A9 (columns 1 and 2). and show that the adoption of a hospital breastfeeding policy significantly increases both the initiation and duration of breastfeeding.

[42]In addition to the state characteristics controlled for in our baseline specification, to capture state-level changes over time in the number of women of child-bearing age, in these regressions we also control for ln(female population in the state-year, age 15-44).

has a requirement for a lactation consultant and (2) whether a state requires any of the other ten potential policy components, we find that the significant increase in lactation consultants is observed only in the set of states that specifically mandate their provision (column 2).

To analyze the extent to which the lactation consultant requirement independently impacts outcomes, we re-estimate our baseline difference-in-differences model using NIS-Child data and specify the policy as above, with two indicator variables that separately capture the presence of a lactation consultant requirement and the presence of any other requirements. These results, presented in Appendix Table A10, suggest that providing a lactation consultant may be independently important for increasing the probability that mothers initiate and sustain breastfeeding during the first 6 months. As previously discussed, ideally we would individually consider the role of each component of the policies, however, since states adopt these policies in bundles, the other individual components are not separately well identified.

Finally, using data from the CDC on the percent of live births in a given state and year that occurred at a Baby-Friendly facility and data from the Baby-Friendly USA website on the number of designated facilities in each state and year, we perform several analyses to examine the extent to which the proliferation of Baby-Friendly facilities may explain our findings. We first show, in Table 4 columns 3 and 4, that adoption of a state hospital policy is not significantly related to the percent of that state's live births occurring in a Baby-Friendly facility or the number of Baby-Friendly certified facilities. These null results support the idea that our results are driven by the adoption of the state-level policies, as opposed to differential changes in the probability of hospitals achieving the Baby-Friendly designation. We also show that our primary breastfeeding results are robust to controlling for the percent of births occurring in a Baby-Friendly hospital (Appendix Table A11 panel B).[43]

Although the adoption of a state hospital policy is not significantly associated with changes in the prevalence of Baby-Friendly hospitals, we do expect that the impact of the policy will vary based on how widespread Baby-Friendly facilities are in the state at the time of policy adoption. Thus, we additionally include in our specification the interaction

[43]For completeness, since these data are only available starting in 2007 we replicate our main breastfeeding results for the shorter sample period. These results are presented in Appendix Table A11 panel A.

between the state hospital policy indicator variable and the percent of births that occurred in a Baby-Friendly facility in the year of policy adoption. These results, presented in Appendix Table A11 panel C, suggest that, as expected, the impact of state hospital policy adoption on initiation and duration of breastfeeding was smaller in states that had more of their births occurring in Baby-Friendly facilities at the time of policy adoption. Specifically, for the average adopting state, which had 12.7 percent of births occurring at a Baby-Friendly hospital in the year of policy adoption (range: 0 to 27 percent), these results imply that the prevalence of Baby-Friendly hospitals reduced the impact of state policy adoption on breastfeeding initiation rates by 3.3 percentage points (0.258×0.127).

5.4 Heterogeneous Effects

To test for heterogeneity in the effects of the state hospital breastfeeding policies across different subpopulations of mothers, we re-estimate our baseline difference-in-differences model and additionally include interactions between the main treatment variable ($HospitalPolicy_{st}$) and maternal characteristics. In these specifications we also include the interaction between the maternal characteristic of interest and an indicator variable that captures whether a state ever adopts a policy, in order to allow the effect of a given maternal characteristic to vary between ever treated and never treated states.[44]

The results from these analyses are presented in Table 5 and provide evidence of heterogeneity across different races/ethnicities (Panel A). Specifically, we find that non-Hispanic Black mothers have significantly larger changes in breastfeeding outcomes in response to the adoption of a hospital breastfeeding policy, relative to non-Hispanic white mothers (omitted group). As non-Hispanic Black mothers are also the least likely at baseline to initiate and sustain breastfeeding compared to the other groups, our findings suggest that hospital breastfeeding laws may serve to reduce disparities across race/ethnicity in breastfeeding rates. Interestingly, we find limited evidence of heterogeneous effects of the laws across the other maternal characteristics that we consider (educational attainment, marital status, age, infant first born status, and WIC recipient status), although there is some evidence of relatively smaller effects for lower-educated (Panel B) and non-married (Panel C) women.

[44] Our estimates are not sensitive to the exclusion of this interaction term. Results available upon request.

Using data from the PRAMS, we also examine potential heterogeneity in the effects of the policy based on characteristics of the birth.[45] The results from these analyses, presented in Appendix Table A12, show no evidence of heterogeneity across the dimensions we consider (C-section birth, multiple birth, preterm birth).

6 Time Use Results

6.1 Effects on Maternal Time Use and Employment

In our next set of analyses we examine how state adoption of hospital breastfeeding policies impacts maternal time use and employment. There are several key reasons to expect that these policies may alter maternal allocation of time. First, since breastfeeding is a uniquely gendered activity (as it requires the mother to either be the one feeding the infant or to spend time pumping), it imposes additional constraints on maternal time. Second, the need to pump breast milk when separated from the infant may increase the relative cost of external child care and reduce the benefit of working outside the home. Third, it may be the case that breastfeeding is a more time-intensive activity than formula-feeding.[46]

In order to investigate these effects, we estimate the following modified version of the dynamic difference-in-differences model presented in equation (1):

$$Y_{istmy} = \beta_0 + \sum_{k \in K} \beta_1^k HospitalPolicy_{st}^k + \beta_2 X_{isy} + Z_{sy} + \mu_m + \gamma_y + \delta_s + \varepsilon_{istmy} \qquad (2)$$

where Y_{istmy} is the outcome of interest for mother i residing in state s who had an infant born in year t and was surveyed in month m of year y. Given that the time use and employment outcomes are measured contemporaneously, in this model we include calendar month of survey (μ_m) and year of survey fixed effects (γ_y) to control for seasonality and common employment shocks across states.[47] We also control for the set of state characteristics as

[45] We thank an anonymous referee for this excellent suggestion.

[46] Anecdotally, breastfed infants tend to eat more frequently. Additionally, if mothers are pumping and then the infant is being bottle fed breast milk, the time spent pumping represents an additional time cost relative to formula feeding.

[47] In the appendix we verify that results are robust to the alternative inclusion of infant birth year fixed effects.

measured in the year of survey (as opposed to year of infant's birth), and, since we are examining employment outcomes, we omit the state unemployment rate from the Z_{sy} vector. All other variables are as defined in equation (1).

Using data from the ATUS, the first set of time use outcomes we examine are time (in minutes) spent on the mutually exclusive categories of child care, formal work, unpaid domestic work, and leisure for the sample of women with infants in their household. We also decompose child care into two sub-categories: time spent on basic/physical child care and time spent on educational/recreational care.[48] Appendix Table A13 presents the descriptive statistics for these variables, both for the full sample (column 1) and separately for mothers residing in states that did versus did not adopt a policy during the sample period. These statistics show that mothers of infants spend an average of 205 minutes on primary child care per day (approximately 3.4 hours) and 140.4 minutes on formal work (2.3 hours); for time spent on child care, the majority of time is spent on basic/physical care (150 minutes), as opposed to educational/recreational care (54 minutes). Notably, mothers in newly adopting states spend more time on child care and less on formal work, relative to mothers in other states.

The results from the estimation of equation (2) using ATUS data are presented graphically in Figure 3. Given the small sample sizes in the ATUS, in order to reduce noise in the estimates we report relative event time in two-year bins. Examination of the pre-adoption estimates shows no evidence of systematic differential trends for the majority of outcomes, with the exception being time spent on educational/recreational care.

The single difference-in-differences coefficients are presented in Table 6 and show that, after adoption of a hospital regulation, women with infants significantly increased the amount of time spent on primary child care and significantly reduced their time spent on formal work. When we disaggregate time spent on child care by type, we find a statistically significant increase only for time spent on basic/physical care of the child (e.g. activities such as feeding and bathing). There is no significant change in time spent on educational/recreational care (e.g. activities such as reading or playing with child), however, the event study plot

[48] We note that breastfeeding falls under the basic/physical child care category, and is unfortunately not able to be disaggregated from other infant care categories such as giving the child a bottle and feeding the child.

suggests this result should be interpreted with caution due to differential trends prior to policy adoption. Overall, the point estimates imply that, on average, adoption of the hospital breastfeeding support policies increased maternal time on primary child care by 33 minutes per day (approximately 16 percent relative to the sample mean) and decreased time spent on formal work by 34 minutes per day (24 percent relative to the sample mean).

Additional analyses show that these results are consistently robust across specification choices, although they are sensitive to the omission of sample weights (see Appendix Table A14). The Goodman-Bacon (2021) decompositions, presented in Appendix Figure A7 and Table A15, show that individual 2×2 difference-in-differences estimates obtained from comparing only the never adopting and newly adopting states are similar to the aggregated estimate; they also receive the vast majority of the weight in the TWFE estimation. For completeness, we also provide the average effects obtained from the de Chaisemartin and D'Haultfœuille (2020b) estimator (see Appendix Table A16).[49]

To further characterize the impact of the hospital regulations on allocation of time we perform several supplemental analyses. First, we re-estimate equation (2) for the ATUS sample of all adults with an infant (i.e. the pooled sample of mothers and fathers) and for the sample of fathers of infants (Appendix Table A17). Next, we conduct falsification analyses using the sample of mothers whose youngest child is between 2 and 18 years old (Appendix Table A18). Overall, the results from these supplemental analyses demonstrate that the increased maternal child care burden following policy adoption was due to an increase at the household level in the amount of time spent on child care, with no significant change in the amount of time fathers spent on child care. Estimated effects from the falsification analyses using the sample of mothers without infants are consistently small in magnitude, and only one of the six estimates (unpaid domestic work) is even marginally significant. Thus, these findings provide support for the idea that the observed changes in time use for households with infants are driven by state policy adoption, as opposed to general changes in the behavior of parents.

We next use data from the Current Population Survey (CPS) to examine the impact of

[49]We note that these results are very imprecise, and none of the estimates are statistically different from zero. However, across all outcomes the confidence intervals do contain the main difference-in-differences estimate and the estimate from the Bacon Decomposition that relies on only never vs. timing comparisons.

the hospital postpartum care regulations on additional margins of maternal employment. As previously discussed, given the stark dynamics of maternal employment during the first year after birth, and in particular the existing evidence showing that the largest changes occur during the first three months postpartum, in our preferred specification we restrict our sample to the set of mothers whose infants' ages are determined to be either between 0 and 3 months or 3 and 12 months at the time of survey.[50] We also verify, however, that our results are robust to including the full set of mothers of infants.

The primary outcomes we examine in the CPS are indicator variables for current labor force participation, current employment, any formal work in the past week, and a continuous measure of the number of work hours reported in the past week (including reports of zero hours). Appendix Table A19 presents the summary statistics for these variables. These descriptives show that, on average, mothers in newly adopting states (column 2) are relatively less likely to report being in the labor force, being currently employed, or having worked in the past week.

The results from analyses using the CPS are presented in Figure 4; the difference-in-differences coefficients are provided in Table 7. Across all outcomes examined, the event study point estimates show no evidence of systematic differential trends prior to policy adoption.[51] The confidence intervals on the pre-implementation estimates are large, however, suggesting we do not have sufficient power to rule out potentially meaningful pre-trends (Roth, forthcoming). As a result, we interpret the results from the CPS analyses with caution.

For interpretation of magnitudes we focus on the difference-in-differences estimates in Table 7. Panel A provides suggestive evidence that for the pooled sample of mothers with infants that are between either 0 and 3 months of age or 3 and 12 months of age, policy

[50]This means that, for example, an infant for which we are only able determine their age at time of survey as being between 0 and 10 months, or 0 and 12 months, etc. is excluded from our sample.

[51]Dynamic and placebo estimates obtained using the de Chaisemartin and D'Haultfœuille (2020b) estimator are similar and are presented in Appendix Figure A8. We again note that since this estimator does not allow for treatment to vary within a group-time unit, for these estimates the treatment variable is an average of the cross-cohort treatment value in a given survey year (i.e. we are unable to estimate models at the state-survey year level that feature treatment variation at the state-birth cohort level). The bootstrap procedure was also unable to account for the full set of control variables; thus, we omit the set of individual-level controls from these analyses.

adoption reduced maternal labor force participation by 1.3 percentage points and current employment by 1.8 percentage points. Compared to the relevant sample means, these represent reductions of between 2.3 and 3.4 percent. If we allow the impact of the polices to vary based on the age of the infant (Panel B), we find that for mothers with infants between 0 and 3 months of age, state adoption of postpartum care regulations significantly reduced maternal employment and the probability of working in the past week by 3.0 and 4.8 percentage points, respectively. These point estimates suggest that during the first 3 months after birth some mothers reduced work by leaving employment, while others remained employed but increased their leave taking. For mothers of 0 to 3 month old infants we also find a significant 1.7 hour reduction in number of hours worked per week. The estimated effects for mothers with 3 to 12 month old infants are consistently smaller in magnitude and are only statistically significant for labor force participation and employment.[52] This pattern of results is consistent with our findings that the largest changes in breastfeeding occur during the first three months, and also suggests that the impact of the laws on maternal employment outcomes is relatively short-lived.

We examine the robustness of the maternal employment results to a number of specification choices, such as omitting individual and state control variables, including region-by-year fixed effects, using birth year and birth calendar month fixed effects as opposed to survey year and survey month fixed effects, and dropping always treated states. These results are presented in Appendix Tables A21-A22 and show that across specifications both the magnitude and significance of the estimated effects are quite robust. The Goodman-Bacon (2021) decompositions, presented in Appendix Figures A9-A10 and Tables A23-A24, show both that the TWFE estimates are identified primarily based on comparisons between never and newly treated states (as is preferable) and that the individual 2×2 estimates are generally clustered around the average effect.

We next estimate the effect of the policies on employment outcomes for fathers of infants. These results, presented in Appendix Table A25, suggest that state adoption of a hospital

[52]We present in Appendix Table A20 results using the full sample of mothers of infants (as opposed to limiting to the subset for which we can determine that the infants are between either 0 to 3 months or 3 to 12 months of age). These results are consistent with our main findings and continue to show significant reductions across the measures of employment, concentrated among the mothers of 0 to 3 month old infants.

breastfeeding support policy had little to no impact on paternal work. Across all outcomes considered, the estimates are consistently very small in magnitude and none are significantly different from zero. These null effects are consistent with our ATUS findings and suggest that mothers are bearing the vast majority of the additional time costs imposed by breastfeeding.

As a falsification test we estimate the effect of state hospital policy adoption on employment outcomes for the sample of mothers whose youngest child is between 2 and 18 years old. Results from these falsification tests are presented in Appendix Table A26 and provide no evidence of changes in the employment of mothers without infants: the estimates are small in magnitude and none are statistically different from zero.

To further probe the robustness of our findings, we use data from the Survey of Income and Program Participation (SIPP), 2000-2013, and a triple-difference identification strategy that additionally leverages within-mother changes in employment, before versus after giving birth. We discuss the data and empirical strategy in detail in Appendix B.1; the associated results are presented and discussed in Appendix B.2. Overall, these results are consistent with our main findings that hospital breastfeeding support policies reduce maternal employment outcomes in the months following birth.

6.2 Heterogeneous Effects

In our final set of analyses we examine heterogeneity in the impact of the policies on maternal employment outcomes, using data from the CPS. Given our finding that the main employment changes occurred among mothers with infants that are 0-3 months of age (described in section 6.1 above), to simplify exposition we focus our heterogeneity analyses on this same sub-sample of mothers. These results are presented in Table 8 and show that, consistent with the breastfeeding results, effects for non-Hispanic Black mothers are significantly larger relative to the effects for white mothers. We also similarly find limited consistent evidence of heterogeneity in the effect of the policy across the other maternal characteristics that are available in the CPS (educational attainment, marital status, age, and number of children under age 5 in the household).

We further explore heterogeneity in the effects of the breastfeeding policy by separately examining impacts among potential "high impact" sub-samples. For these analyses we focus on three groups of mothers of infants: Black mothers, mothers with no college education, and Black mothers with no college education. We consider these sub-groups to be high impact given: (1) our empirical evidence that the policy had the largest impacts on breastfeeding for Black mothers, relative to white mothers; and (2) the opportunity cost of staying home with the infant is likely lower for lower-educated women. As a result, we theoretically expect that the employment changes should be relatively larger among these sub-samples.

The results in Appendix Table A27 provide suggestive evidence that, as hypothesized, individuals in the high impact samples are consistently more likely to adjust their employment in response to policy adoption, relative to the baseline sample (Panel A).[53] The accompanying event studies similarly show sharp reductions across all measures of maternal employment coinciding with the timing of policy adoption (Appendix Figures A11 - A13), and these reductions are particularly stark and sustained for the highest impact sub-sample: Black mothers whose highest level of education is a high school degree or less. For all of the event studies the estimated pre-adoption coefficients are not statistically different from zero, although we again note that the confidence intervals are large and suggest we are under-powered to detect potentially meaningfully differential pre-trends (Roth, forthcoming).[54] Thus, we continue to interpret these findings with caution.

7 Discussion

The core finding from our analyses is that the adoption of state hospital breastfeeding policies significantly increased breastfeeding initiation by 3.8 percentage points and increased the probability of any breastfeeding at 3 and 6 months by 4.1 and 2.8 percentage points, respectively. These results suggest that the adoption of the hospital breastfeeding policies explains approximately 36 percent of the observed increase in breastfeeding initiation rates

[53]As with our main CPS results, these results are robust to specification choices (see Appendix Tables A28-A30), and we find no evidence of reductions in employment among the analogous sub-samples of fathers (Appendix Table A31) or for high impact mothers whose youngest child is between 2 and 18 years old (falsification test, see Appendix Table A32).

[54]Dynamic and placebo estimates obtained using the de Chaisemartin and D'Haultfœuille (2020b) estimator are similar and are presented in Appendix Figures A14, A15, and A16. For these smaller sub-samples the bootstrap procedure employed by the estimator is unable to accommodate the full set of state-level controls. Therefore, we omit them from these specifications.

observed in adopting states over our sample period, and between 17 and 25 percent of the observed increase in rates of breastfeeding at 3 and 6 months.[55] Our estimates further imply that in the last year of our sample period, approximately 53,590 additional infants were breastfed at birth (0.038 $\times$ 1.4 million births in treatment states), 56,809 additional were breastfed at 3 months, and 39,179 additional were breastfed at 6 months, as a result of policy adoption. Notably, the breastfeeding measures we examine capture only the *extensive* margin of breastfeeding at different points in time. If these hospital policies also impacted the *intensity* of breastfeeding, we will underestimate the number of women who altered their breastfeeding behavior in response to the policy.

We also find evidence suggesting that women significantly adjusted their employment along a number of margins in response to state adoption of hospital breastfeeding policies. Specifically, we estimate a 1.8 percentage point reduction in the probability of current employment among women with infants. Scaling this estimated effect by the same cohort of infants in newly adopting states as above, this translates to approximately 25,000 more women potentially withdrawing from employment as a result of the hospital breastfeeding policies.

Existing evidence suggests that reductions in post-birth maternal employment translate into meaningful reductions in wages and income in the long-run. For example, Kuka and Shenhav (2020) leverage a change in work incentives for single mothers at first birth due to an expansion of the Earned Income Tax Credit (EITC) and estimate that mothers who accumulated an additional 0.5 to 0.6 years of work experience due to returning to work sooner post-birth have on average 6 percent higher earnings in the long-run (10 to 19 years after birth). Overall, their calculations suggest that these higher earnings translate to a total of approximately $37,000 ($2016) of additional labor income over the two decades after birth. If we assume a similar impact in our context and scale this estimate by the 25,000 additional women leaving employment each year as a result of state policy adoption, this translates to nearly $925 million in forgone lifetime earnings for these women.

[55] During our sample period breastfeeding initiation rates in newly adopting states increased by approximately 10.5 percentage points (from 73.0 to 83.5 percent) and rates of breastfeeding at 3 and 6 months after birth each increased by approximately 16.5 percentage points (from 53.5 to 70.1 percent, and from 39 to 55.4 percent, respectively).

However, the increased breastfeeding and maternal time at home likely also generate substantial benefits in terms of infant health. In particular, our estimates suggest that as a result of the increased breastfeeding, there are approximately 6,312 fewer gastrointestinal infections and 4,619 fewer cases of atopic eczema expected per year (Kramer et al., 2001).[56] Combined with estimated costs of these diseases (including indirect costs for time spent visiting the doctor), this translates to nearly $27.6 million in averted costs (Bartick et al., 2017; Bickers et al., 2006).[57]

To estimate the health benefits of increased maternal time at home we draw on Rossin (2011), which examines the impact of increased access to unpaid parental leave due to the Family Medical Leave Act (FMLA) on infant mortality. For college-educated and married women (the group with the highest FMLA eligibility), Rossin (2011) estimates a treatment-on-the-treated effect of approximately 0.02 infant deaths averted annually per additional leave-taker. If we assume this estimate applies to our setting, this implies that approximately 500 infant lives were saved each year (25,000 new leave-takers $\times$ 0.02), for a total social savings of approximately $4.4 billion ($2016).[58] Altogether, these estimates suggest that the improvements in infant health from increased breastfeeding and maternal time at home likely exceed the long-run reductions in earnings.

8 Conclusion

In this paper we provide novel evidence on the effects of state hospital postpartum care regulations on breastfeeding and maternal time allocation. We first document that these policies were successful at achieving their intended goal: following adoption, mothers are significantly more likely to initiate and sustain breastfeeding. This finding is robust across

[56]For these calculations, we referenced the findings of the PROBIT randomized control trial, as reported in Kramer et al. (2001), and estimated the treatment effect of increased breastfeeding on gastrointestinal infection and eczema incidence by scaling the treatment-control difference in incidence by the treatment-control difference in breastfeeding rates at 3 months. This calculation implies that breastfeeding for 3 months reduces the probability of a gastrointestinal tract infection by 11 percentage points (4.1/36.9) and of atopic eczema by 8 percentage points (3/36.9).

[57]We note that there are other potential benefits from breastfeeding that we are not accounting for (e.g. reduction in expenditure on formula, utility for the infant or mother, improvements in cognitive development), but that may be important (Rees and Sabia, 2009; Fitzsimons and Vera-Hernández, 2021).

[58]We use the EPA's value-of-statistical life estimate of $8.8 million, in 2016 dollars (United States Environmental Protection Agency, 2022).

different specifications of the treatment variable and across different data sets, and we show that it is not driven by differential selection into motherhood. Moreover, we find that these hospital regulations may be effective at reducing disparities in breastfeeding across races and ethnicities: non-Hispanic Black mothers are least likely to breastfeed at baseline, and also the most responsive to the policy.

Our results also show that these policies substantially impacted maternal time allocation. After adoption of a state hospital regulation supporting breastfeeding, mothers spend more time on child care and less time on formal work. We find an overall increase in time spent on child care at the household level, with no evidence of a change in time spent on child care by fathers of infants. Thus, these results are consistent with the idea that breastfeeding imposes additional constraints on maternal time and alters the relative costs of external childcare. Notably, while women report returning to work as a significant obstacle to breastfeeding (CDC, 2019b), we are the first to provide evidence that policies aimed to increase breastfeeding may decrease maternal time spent on formal work.

Overall, our work makes important progress towards quantifying the true costs and benefits of breastfeeding-promoting policies. In the future, more work should be done to understand the persistence of the effects we identify, as well as the impact of these policies on other complementary maternal behaviors and parental investments in the child. Future research can also use the variation in adoption of these regulations to expand the existing research connecting breastfeeding to positive infant and maternal health outcomes in a causal framework.

This work has several notable limitations. First, our empirical strategy assumes that policy adoption is exogenous and does not coincide with the adoption of other policies that may potentially impact breastfeeding or maternal time allocation. Although we provide evidence that policy adoption did *not* occur in response to relative reductions in breastfeeding rates or at the same time as other related state policies we identify (Appendix Table A3), we cannot rule out other unobserved shocks. Second, we lack detailed information on hospital-level characteristics, precluding us from carefully examining hospital responses to policy adoption. Similarly, while we provide suggestive evidence on the relative importance of lactation consultants and the role of Baby Friendly Hospital prevalence at the time of policy adoption, we are unable to speak directly to the mechanisms driving the effects we estimate. Finally, we note that while the impact of breastfeeding support policies on maternal employment likely varies strongly by access to parental leave, our empirical setting does not have sufficient variation to allow us to credibly examine this interaction.

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Figures

Figure 1: **Timing of Adoption of State Hospital Breastfeeding Policies**

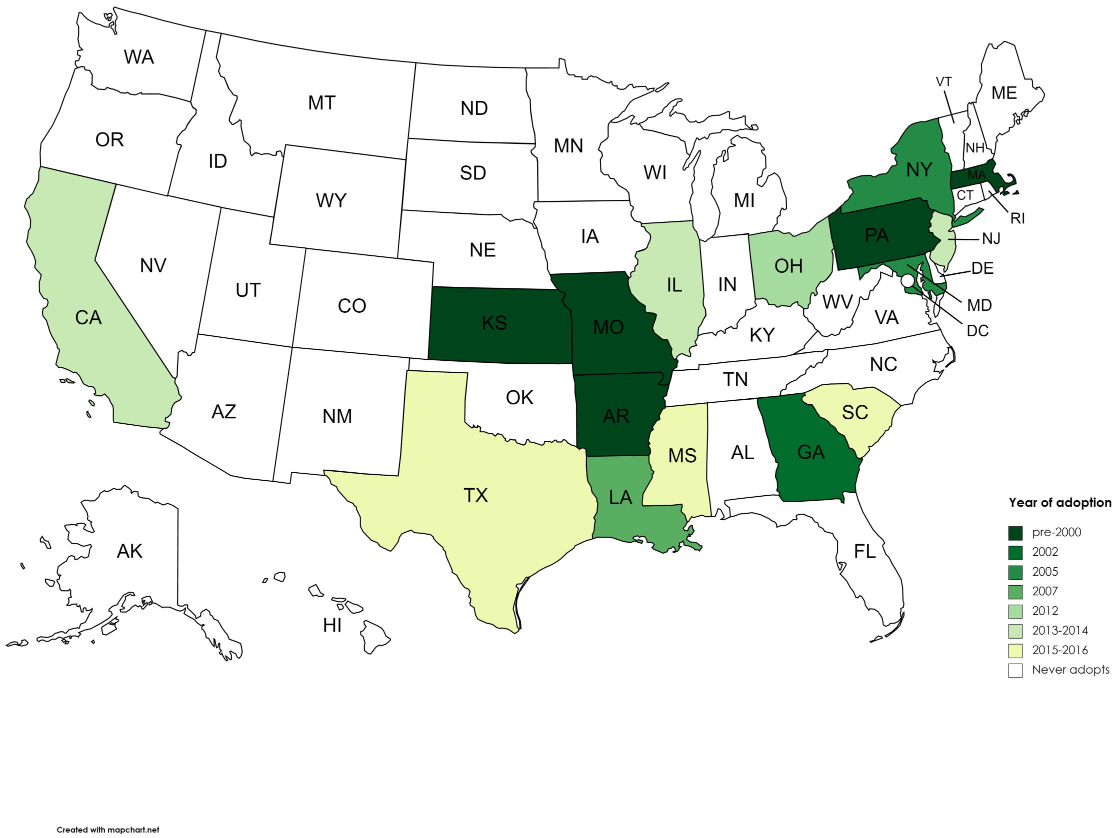
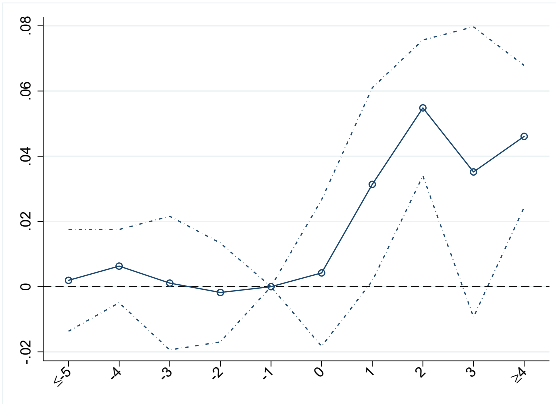
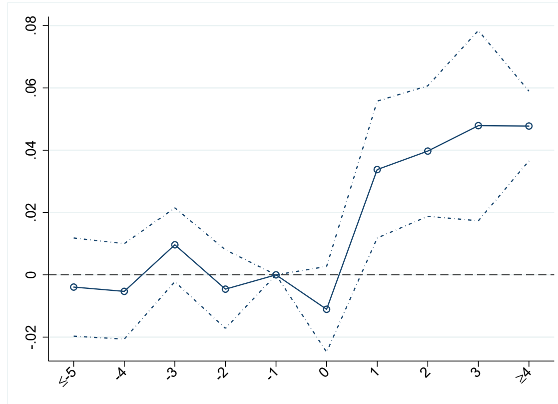


Figure 2: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes, NIS-Child (2003-2017)**

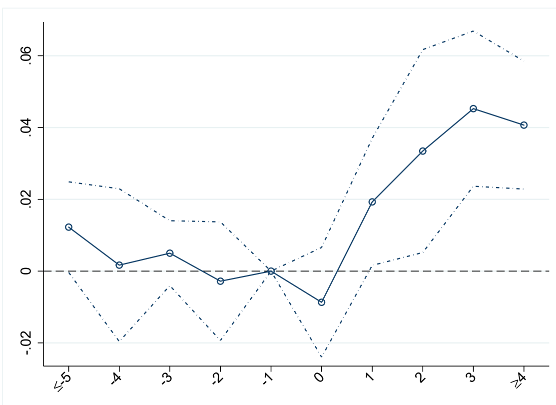
(a) Ever initiated breastfeeding



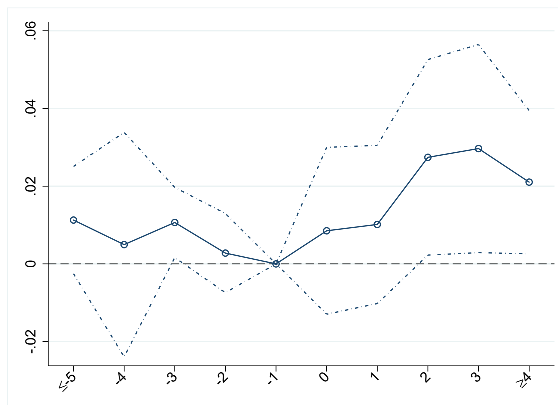
(b) Breastfeeding at 3 months



(c) Breastfeeding at 6 months



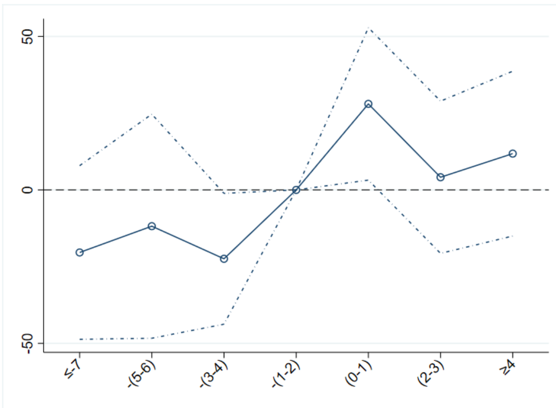
(d) Breastfeeding at 1 year



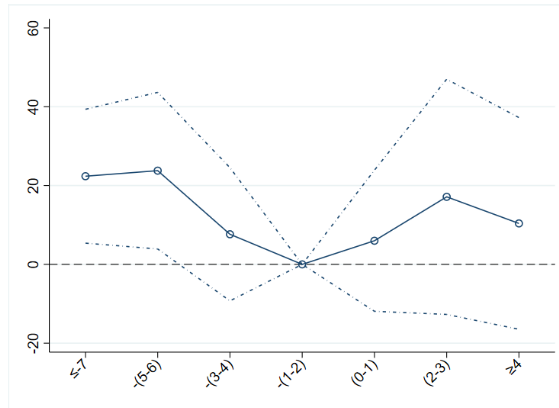
Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in the panel label and the treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. Regressions include birth year fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by NIS-Child sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Figure 3: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Time Use, ATUS (2003-2018)**

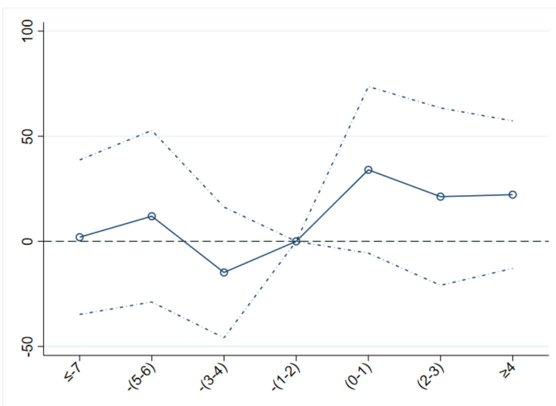
(a) Basic/Physical Care for Child



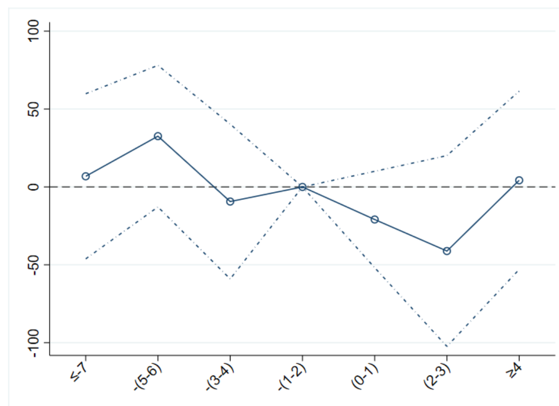
(b) Educational/Recreational Care for Child



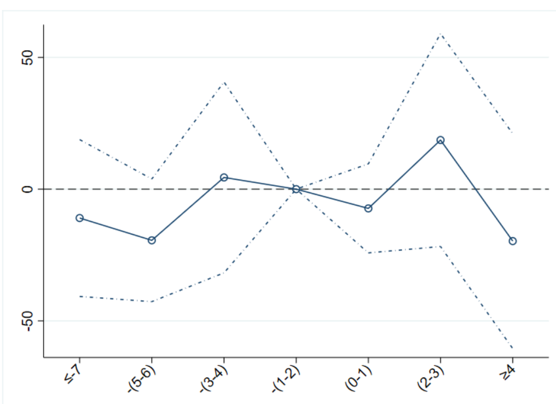
(c) Total Primary Child care



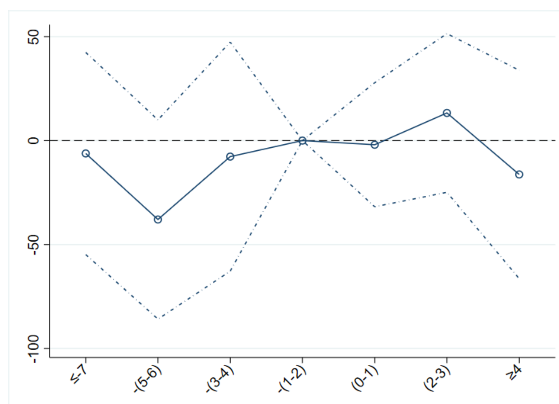
(d) Time on Formal Work



(e) Unpaid Domestic Work



(f) Leisure Time



Note: Each figure presents the estimates from a separate regression, in which the outcome variable is the number of minutes spent on the time use category specified in the panel label. Regressions are estimated using the sample of female adults with an infant under the age of 12 months in the household (see notes to Table 6 for details). The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the two years prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Figure 4: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Employment, CPS (2000-2018)**

Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in each panel label and the treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. The sample is the set of women with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year and month fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by CPS sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Tables

Table 1: Descriptive Statistics, NIS-Child (2003-2017)

	(1) Full sample	(2) Individuals in states that adopted a hospital regulation during sample	(3) Individuals in states that did not adopt a hospital regulation during sample
Breastfeeding outcomes			
Ever breastfed	0.757	0.775	0.747
Breastfed, 3 months	0.580	0.611	0.562
Breastfed, 6 months	0.436	0.462	0.420
Breastfed, 1 year	0.222	0.240	0.211
Child's characteristics			
Female	0.489	0.488	0.489
Firstborn	0.412	0.416	0.410
Ever received WIC	0.544	0.553	0.538
Non-Hispanic white	0.495	0.410	0.544
Hispanic	0.274	0.332	0.239
Non-Hispanic Black	0.131	0.144	0.124
Other ethnicity	0.101	0.113	0.093
Mother's characteristics			
Less than high school	0.188	0.203	0.180
High school	0.292	0.278	0.301
Some college	0.205	0.195	0.211
College degree or above	0.314	0.324	0.308
Married	0.656	0.653	0.658
Age: $<$29 yrs	0.423	0.392	0.441
NIS-Child Observations	354,642	78,545	276,097

Notes: All values are weighted means calculated by the authors from NIS-Child data, 2003-2017, using provided sample weights (landline only for 2003-2011, dual weights for 2012-2017). The states included in column 2 are California, Georgia, Illinois, Louisiana, Maryland, New Jersey, New York, and Ohio.

Table 2: Effects of Hospital Breastfeeding Support Policies on Breastfeeding Initiation and Duration, NIS-Child (2003-2017)

	(1) Breastfeeding Initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Sample mean	0.757	0.580	0.436	0.222
Hospital Policy	0.0383***	0.0406***	0.0280***	0.0121
	(0.00950)	(0.00731)	(0.00670)	(0.00812)
N	354,642	343,792	343,792	343,792
R-squared	0.121	0.134	0.128	0.0735
Hospital Policy Strength	0.0602***	0.0600***	0.0468***	0.0289***
	(0.00674)	(0.0103)	(0.0102)	(0.00605)
N	354,642	343,792	343,792	343,792
R-Squared	0.121	0.134	0.128	0.0735
Individual characteristics?	Y	Y	Y	Y
State/time varying Xs?	Y	Y	Y	Y
State and birth year fixed effects?	Y	Y	Y	Y

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

Notes: Results are from linear probability models and use NIS-Child sampling weights. The outcome variable is the indicator described in each column header. The treatment variable in the top panel is an indicator variable equal to one if the state had adopted a hospital breastfeeding policy by June of the infant's birth year; the treatment variable in the bottom panel is a continuous variable that ranges from zero to one, and captures the strength of the policy, as measured as the fraction of the 11 possible components that the policy mandates (median policy strength = 2/11). Infants are observed at ages 19-35 months, between 2003 and 2017. All models include controls for individual demographic characteristics (age at observation fixed effects, gender, race, number of children in the household, first born status, whether the child ever received WIC, and mother's age, education level, and marital status); state and birth year fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered at the state level.

Table 3: Effects of Hospital Breastfeeding Support Policies on Care During Postpartum Hospital Stay, PRAMS (2000-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
	Received breastfeeding info from staff	Staff helped with breastfeeding	Allowed to breastfeed on demand	Roomed-in with infant	Given gift with formula	Connected with breastfeeding support group
Sample mean	0.933	0.741	0.758	0.826	0.652	0.771
Hospital Policy	0.0128***	0.0295*	0.0151	0.0180	-0.0327	0.00247
	(0.00419)	(0.0169)	(0.0146)	(0.0175)	(0.0216)	(0.00975)
N	253,645	250,322	230,888	241,660	245,681	235,914
R-squared	0.0227	0.142	0.104	0.0461	0.232	0.0680

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable is the indicator described in each column header. The treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding support policy by June of the infant's birth year. Surveys are conducted when infants are approximately 2-6 months old, between 2000 and 2018, and the sample consists of the set of mothers reporting that they initiated breastfeeding, for the set of states and years provided in Appendix Figure A2. All models include controls for individual demographic characteristics (child gender, race/ethnicity fixed effects, fixed effects for number of previous live births, whether the mother received WIC during pregnancy, and fixed effects for mother's age group, education level, and marital status); state, birth year, and calendar month of birth fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction Black, Hispanic, and other non-white, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Regressions are weighted by PRAMS sample weights and standard errors are clustered at the state level.

Table 4: Effects of Hospital Breastfeeding Support Policies on Prevalence of Lactation Consultants and Baby Friendly Facilities

	(1) Ln(Count of IBCLCs)	(2) Ln(Count of IBCLCs)	(3) % Births at Baby Friendly Facilities	(4) Number of Baby Friendly Facilities
Hospital Policy	0.243***		-0.0170	0.403
	(0.0788)		(0.0436)	(0.282)
Lactation consultant requirement		0.298**		
		(0.146)		
Non-lactation consultant requirement		-0.0192		
		(0.164)		
N	561	561	612	950
R-squared	0.804	0.805	0.703	0.415

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

Note: IBCLC stands for International Board Certified Lactation Consultant. Each observation is at the state-year level. Based on data availability, the sample in columns 1 and 2 are for the years 2006-2016; the sample in column 3 is for the years 2007-2018; the sample in column 4 is for the years 2000-2018. In addition to the state characteristics controlled for in the baseline specification (see notes to Table 2 for detail), each regression additionally controls for ln(female population in the state-year, age 15-44). Column 4 is estimated using a negative binomial model, the reported R-squared is the McFadden's pseudo R-squared. All regressions are unweighted; standard errors are clustered at the state level.

Table 5: Heterogeneity in the Effects of Hospital Breastfeeding Support Policies, NIS-Child (2003-2017)

	(1) Breastfeeding Initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Panel A: By race/ethnicity				
Hospital Policy	0.0336***	0.0275**	0.0171	0.00958
	(0.0115)	(0.0107)	(0.0103)	(0.0122)
× Non-Hispanic Black	0.0189	0.0383*	0.0339*	0.0145
	(0.0238)	(0.0199)	(0.0185)	(0.0113)
× Hispanic	-0.00859	0.00647	0.00466	-0.0118
	(0.0163)	(0.0247)	(0.0273)	(0.0139)
× Other	0.0198	0.0323	0.0232	0.0153
	(0.0162)	(0.0298)	(0.0286)	(0.0196)
Panel B: By mother's education				
Hospital Policy	0.0456***	0.0449***	0.0316***	0.0138*
	(0.00722)	(0.00672)	(0.00677)	(0.00687)
× No HS degree	-0.0453*	-0.0264	-0.0215	-0.0101
	(0.0262)	(0.0244)	(0.0199)	(0.0157)
Panel C: By mother's marital status				
Hospital Policy	0.0218	0.0278**	0.0195**	-0.000806
	(0.0206)	(0.0134)	(0.00927)	(0.00784)
×Married	0.0261	0.0197	0.0128	0.0207**
	(0.0237)	(0.0178)	(0.0125)	(0.0102)
Panel D: By mother's age				
Hospital Policy	0.0413***	0.0416***	0.0261***	0.0129
	(0.00722)	(0.00703)	(0.00552)	(0.00905)
× $\leq$ 29 years old	-0.00756	-0.00232	0.00512	-0.00145
	(0.0111)	(0.0108)	(0.0113)	(0.00901)
Panel E: By parity				
Hospital Policy	0.0341***	0.0368***	0.0281***	0.0157*
	(0.0104)	(0.00802)	(0.00683)	(0.00842)
×Firstborn	0.00985	0.00887	-0.000000836	-0.00820*
	(0.00684)	(0.00753)	(0.00484)	(0.00473)

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

Table 5 continued next page

Table 5 *continued from previous page*

	(1) Breastfeeding Initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Panel F: By WIC status				
Hospital Policy	0.0547***	0.0524***	0.0379***	0.0190**
	(0.0116)	(0.0133)	(0.00982)	(0.00927)
× Ever WIC	-0.0304	-0.0221	-0.0186	-0.0128
	(0.0266)	(0.0252)	(0.0208)	(0.0138)

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

Note: Results are from linear probability models and use NIS-Child sampling weights. The outcome variable is the indicator described in each column header. Infants are observed at ages 19-35 months, between 2003 and 2017. All models include controls for individual demographic characteristics (age at observation fixed effects, gender, race, number of children in the household, first born status, whether the child ever received WIC, and mother's age, education level, and marital status); state and birth cohort fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). All regressions additionally include an interaction between the indicator variable for ever adopting a hospital breastfeeding policy and the given heterogeneity variable. Standard errors are clustered at the state level.

Table 6: Effects of Hospital Breastfeeding Support Policies on Maternal Time Use, ATUS (2003-2018)

	(1) Total Primary Child Care	(2) Basic/ Physical Care for Child	(3) Educational/ Recreational Care for child	(4) Time Spent Working	(5) Unpaid Domestic Work	(6) Leisure Time
Hospital Policy	32.75**	28.25***	4.502	-34.35**	0.514	12.58
	(16.01)	(10.50)	(8.238)	(16.19)	(9.942)	(11.22)
N	3,932	3,932	3,932	3,932	3,932	3,932
R-Squared	0.20	0.16	0.10	0.14	0.12	0.23
Mean of Dependent	202.81	148.21	54.59	140.94	168.55	864.22

* p <0.10, ** p <0.05, *** p <0.01

Note: Outcome variables are measures of the number of minutes during the survey day spent on the time use category given in the column header. The treatment variable is an indicator variable that is equal to one if a hospital policy was in effect by June of the estimated birth year. All columns are weighted by ATUS sample weights and have state, survey year, and survey month fixed effects. All models include controls for individual demographic characteristics (number of household members and number squared, number of children in the household, and mother's age, race/ethnicity, education level, and marital status); state policies (see text for details); and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level) and fixed effects for the day of the week and if the survey day was a holiday. Standard errors are clustered at the state level.

Table 7: Effects of Hospital Breastfeeding Support Policies on Maternal Work, CPS (2000-2018)

	(1) Labor Force Participation	(2) Employed	(3) Worked Last Week	(4) Hours Worked Last Week (unconditional)
Panel A: Overall effect for women with 0-3 or 3-12 month olds				
Hospital Policy	-0.0132**	-0.0181***	-0.00790	-0.301
	(0.00514)	(0.00498)	(0.00586)	(0.301)
N	109,187	109,187	109,187	109187
R-Squared	0.0976	0.106	0.122	0.120
Mean of Dependent	0.576	0.534	0.434	14.54
Panel B: Decomposed by age of infant				
Hospital Policy	-0.0170	-0.0299***	-0.0476***	-1.661***
x baby 0-3 mos	(0.0108)	(0.00992)	(0.0132)	(0.478)
Hospital Policy	-0.0117*	-0.0133**	0.00837	0.255
x baby 3-12 mos	(0.00640)	(0.00611)	(0.00656)	(0.287)
N	109,187	109,187	109,187	109,187
R-Squared	0.0976	0.106	0.123	0.120
Mean of Dependent	0.576	0.534	0.434	14.54

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample is the set of women observed in the CPS with an infant between 0 and 3 months or between 3 and 12 months of age. All models include controls for individual characteristics (infant age at observation fixed effects, number of children in the household, number of children under age 5 in the household, and mother's age, education level, race/ethnicity, and marital status); state, survey year, and survey month fixed effects; state policies (see text for details); and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Regressions in Panel B additionally include an interaction between an indicator variable for infant age group and an indicator variable that captures if the state ever adopted a hospital policy. All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table 8: Heterogeneity in the Effects of Hospital Breastfeeding Support Policies on Maternal Employment 0-3 Months Postpartum, CPS (2000-2018)

	(1) Labor Force Participation	(2) Employed	(3) Worked Last Week	(4) Hours Worked Last Week (unconditional)
Panel A: By race/ethnicity				
Hospital Policy	0.00608	-0.00431	0.00263	0.0753
	(0.0231)	(0.0255)	(0.0144)	(0.580)
x Non-Hispanic Black	-0.0392	-0.0265	-0.0829***	-3.222***
	(0.0274)	(0.0247)	(0.0209)	(0.646)
x Hispanic	-0.0408	-0.0533	-0.0317	-0.938
	(0.0292)	(0.0321)	(0.0223)	(0.850)
x Other	-0.140***	-0.152***	-0.0623**	-2.800***
	(0.0333)	(0.0333)	(0.0258)	(0.966)
Panel B: By mother's education				
Hospital Policy	-0.0201	-0.0321*	-0.0170	-0.700
	(0.0151)	(0.0166)	(0.0131)	(0.572)
× No HS degree	0.000134	-0.00282	-0.0220	-0.582
	(0.0335)	(0.0267)	(0.0275)	(0.815)
Panel C: By mother's marital status				
Hospital Policy	-0.0578***	-0.0715***	-0.0326	-1.357**
	(0.0153)	(0.0150)	(0.0206)	(0.648)
x Married	0.0523**	0.0540**	0.0172	0.797
	(0.0206)	(0.0245)	(0.0240)	(0.717)
Panel D: By mother's age				
Hospital Policy	-0.0106	-0.0212	-0.0198	-0.667
	(0.0184)	(0.0172)	(0.0166)	(0.595)
x $\leq$ 29 yo	-0.0198	-0.0240	-0.00285	-0.293
	(0.0298)	(0.0287)	(0.0250)	(0.673)
Panel E: By number of young children				
Hospital Policy	-0.0208	-0.0322*	-0.0113	-0.454
	(0.0165)	(0.0174)	(0.0127)	(0.533)
x Only 1 child under age 5	0.000134	-0.00159	-0.0165	-0.609
	(0.0150)	(0.0136)	(0.0179)	(0.685)

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample is the set of women observed in the CPS with an infant between 0 and 3 months of age. All models include controls for individual characteristics (number of children in the household, number of children under age 5 in the household, and mother's age, education level, race/ethnicity, and marital status); state, survey year, and survey month fixed effects; state policies (see text for details); and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). All regressions additionally include an interaction between an indicator variable for the given heterogeneity variable and an indicator variable that captures if the state ever adopted a hospital policy. All models are weighted by CPS sample weights and standard errors are clustered at the state level.

A Appendix

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Figure A1: **Components of State Breastfeeding Policies**

State	Year of Law Adoption	Lactation Consultant	Staff Training	Inform Patients	Written/ Communicated	Rooming In	Non Breastmilk	Group/ Resources Info	Initiate BF	How to BF	On Demand BF	No Pacifiers	Total Components (out of 11)
California	2014	X		X	X	X		X					5
Georgia	2002					X							1
Illinois	2013	X	X	X	X		X	X	X				7
Louisiana	2007	X											1
Maryland	2005	X	X										2
Mississippi	2016		X	X	X		X						4
New Jersey	2014	X	X	X	X	X	X	X	X	X			9
New York	2005	X	X	X	X	X	X	X	X	X	X	X	11
Ohio	2012	X		X									2
South Carolina	2015	X											1
Texas	2016	X											1
Total States (out of 11)		9	5	6	5	4	4	4	3	2	1	1	

Note: We use the policy component categorizations developed by the LawAtlas Policy Surveillance Program database, available at http://lawatlas.org/datasets/baby-friendly-hospital-1525279705 and detailed below. Lactation consultant: state policy requires that hospitals must make a breastfeeding consultant available to maternity patients. Staff training: state policy requires that healthcare staff be trained in the skills necessary to implement practices that support breastfeeding among maternity patients. Inform patients: state policy requires hospitals to inform patients about breastfeeding (whether it be general, about the benefits and/or disadvantages, about initiation, or management). Written/communicated: state policy require hospitals' breastfeeding policy be written and/or communicated (whether it be to staff, to patients, posted, or provided directly). Rooming in: state policy requires hospitals to permit rooming-in, where the baby's crib is kept by the side of the mother's bed. Non-breastmilk: state policy includes requirements about when infants may be given food or drink other than breast milk. Group/resources info: state policy requires hospitals to foster the establishment of breastfeeding groups and/or refer mothers to them. Initiate BF: state policy requires hospitals to help mothers initiate breastfeeding within one hour of birth. How to BF: state policy require hospitals to provide mothers with instruction on how to breastfeed, and how to maintain lactation. On demand BF: state policy requires that hospitals allow mothers to breastfeed on demand. No pacifiers: state policy prohibits hospitals from giving pacifiers or artificial nipples (e.g., bottle feeding) to breastfeeding infants.

Figure A2: **PRAMS data availability**

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

● indicates data available and survey includes BFH care questions; ○ indicates data available, survey does NOT include BFH care questions. A blank cell means no data are available for that state-year. If a state is not listed, they do not have data available for any of the listed state-years. Gray shaded cells represent state-years in which there is a state hospital breastfeeding support law in effect.

Figure A3: **Timing of State Policy Adoption and Sample Periods of Primary Data Sources**

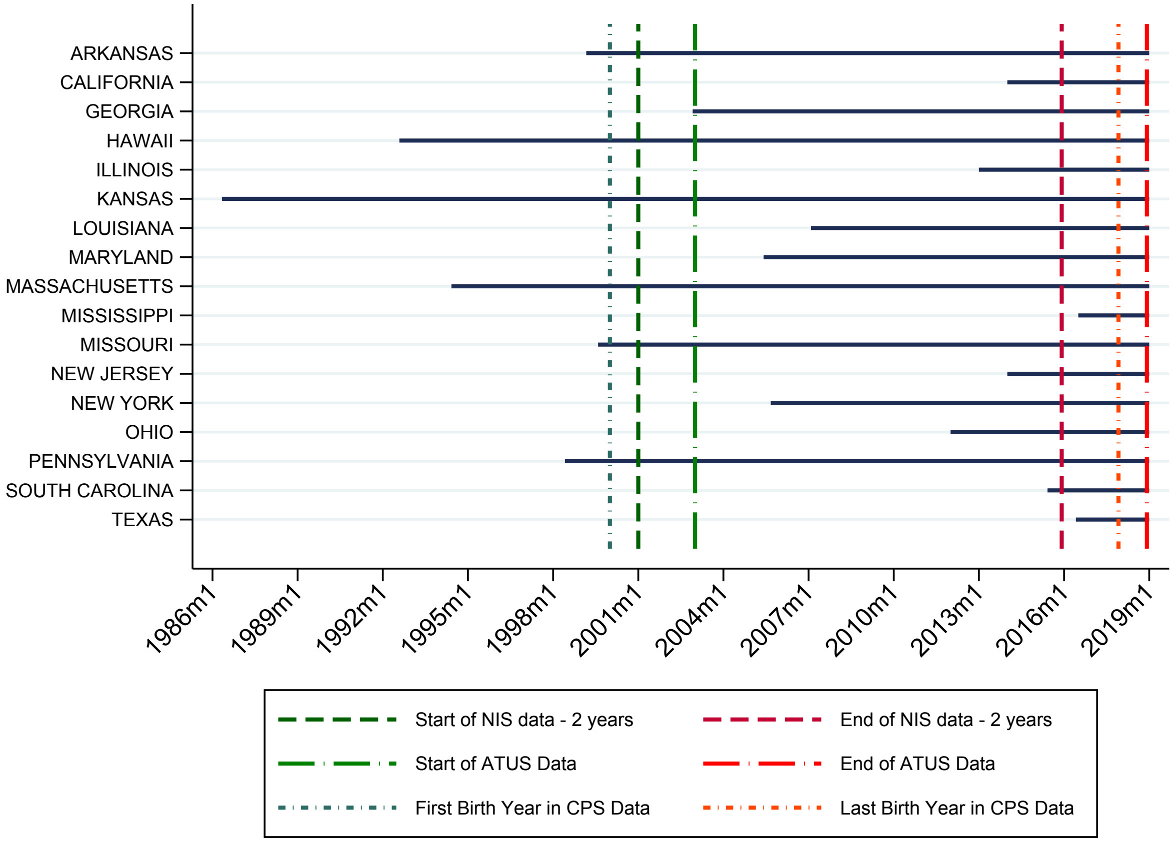
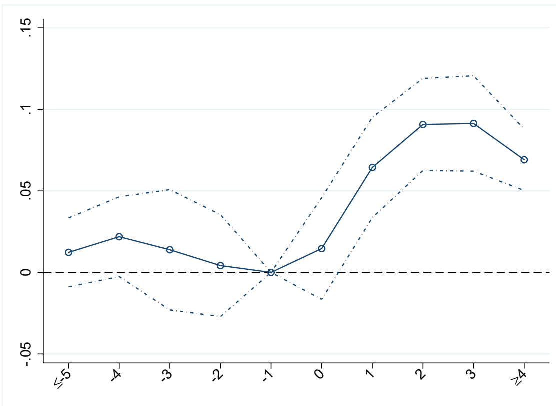
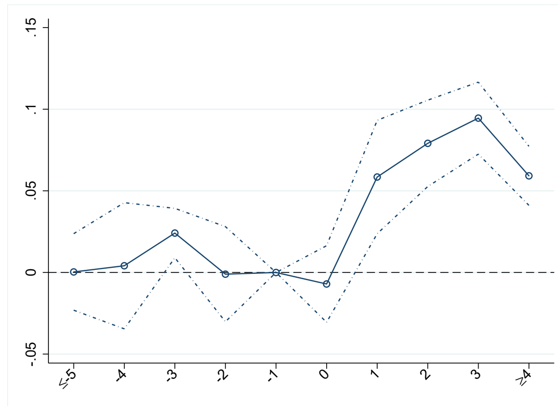


Figure A4: **Event Study Estimates of the Effect of the Strength of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes, NIS-Child (2003-2017)**

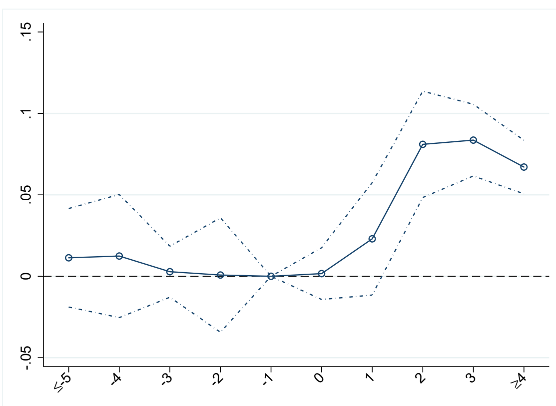
(a) Ever initiated breastfeeding



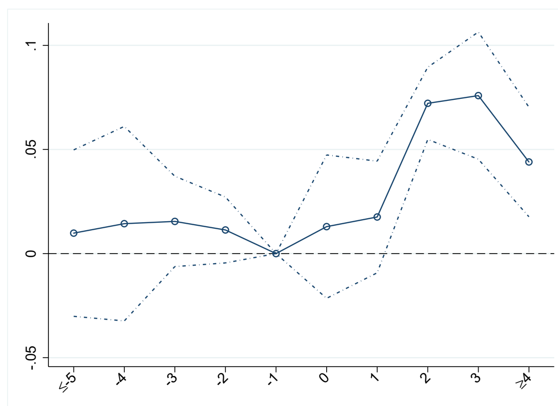
(b) Breastfeeding at 3 months



(c) Breastfeeding at 6 months



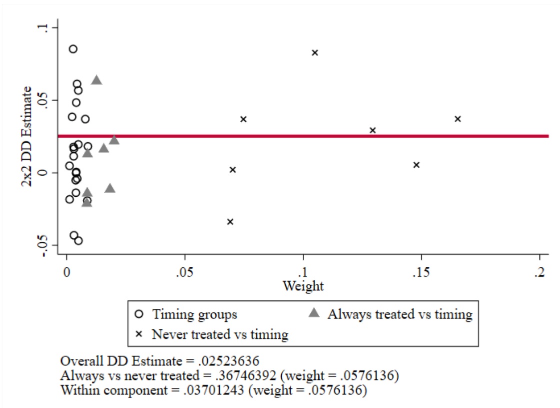
(d) Breastfeeding at 1 year



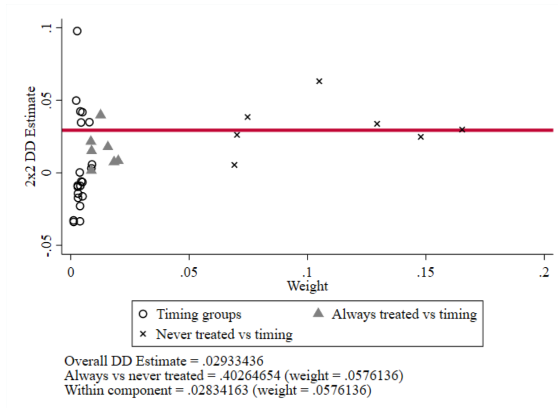
Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in the panel label and the treatment variable is a continuous measure of the strength of the state hospital breastfeeding support policy. Regressions include birth year fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by NIS-Child sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Figure A5: Goodman-Bacon (2021) **Decomposition, NIS-Child (2003-2017)**

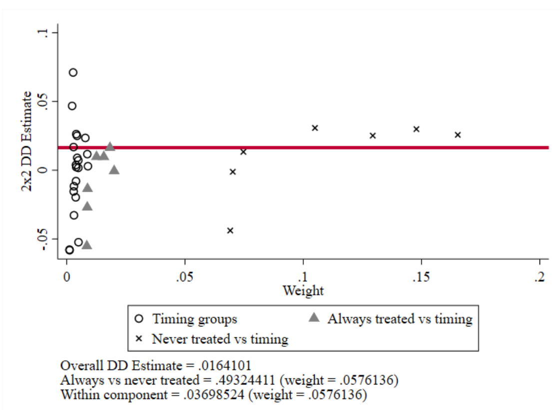
(a) Ever initiated breastfeeding



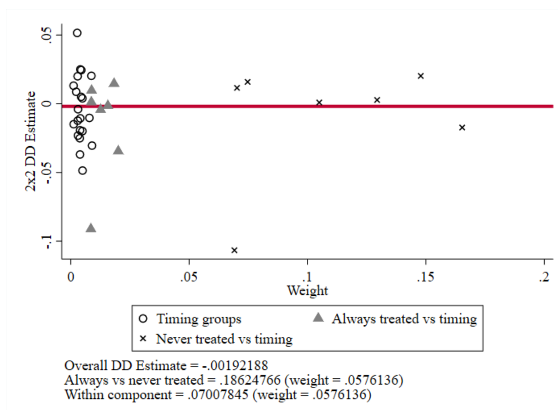
(b) Breastfeeding at 3 months



(c) Breastfeeding at 6 months



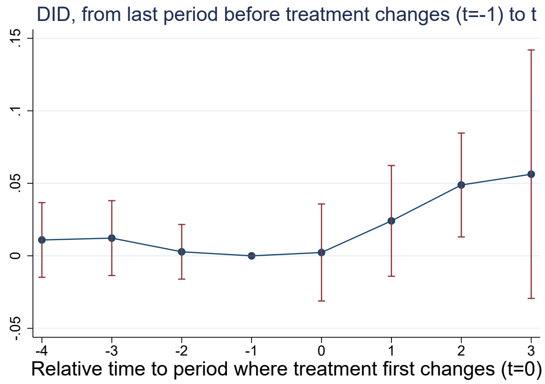
(d) Breastfeeding at 1 year



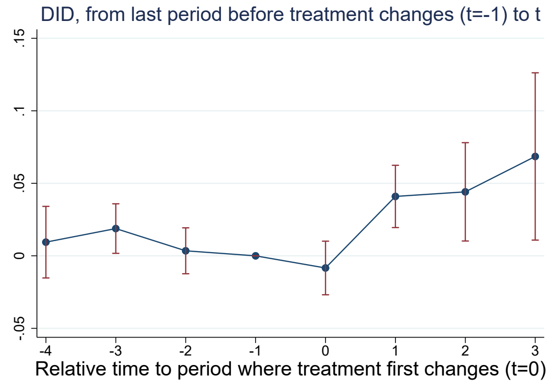
Notes: Each panel presents the Goodman-Bacon (2021) decomposition for the outcome variable listed as the panel header. The $\times$ symbols represent the estimate from a given 2×2 difference-in-differences model that compares never treated states versus states that adopt during our sample period. Hollow circles represent the estimates from models that make comparisons between early and late adopters; triangles represent the estimates from models that compare always treated states to states that adopt during the sample period. Table A5 summarizes the overall decomposition for each of the outcome variables.

Figure A6: **Dynamic Effects of Hospital Breastfeeding Support Policies on Breastfeeding Outcomes, de Chaisemartin and D'Haultfœuille (2020b) estimator, NIS-Child (2003-2017)**

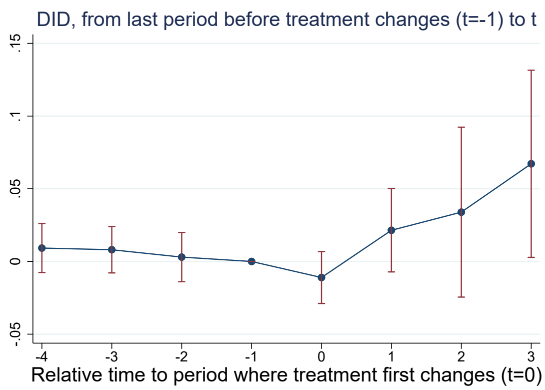
(a) Ever initiated breastfeeding



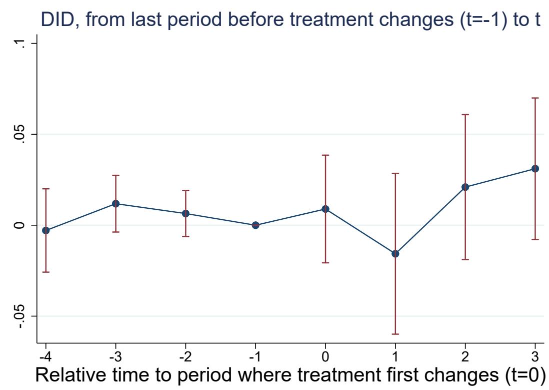
(b) Breastfeeding at 3 months



(c) Breastfeeding at 6 months



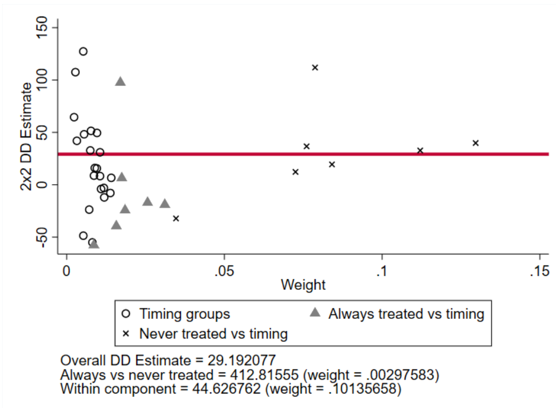
(d) Breastfeeding at 1 year



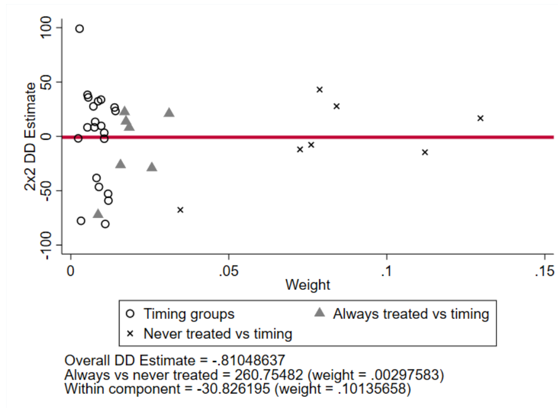
Note: Each figure presents the estimates for a separate outcome variable, as specified in the panel label. The treatment variable is a binary indicator capturing state adoption of a hospital breastfeeding support policy. Estimates are obtained in Stata using the *did_multiplegt* command with the *robust_dynamic* option specified. Standard errors are clustered at the state level and computed using 200 block bootstrap replications; vertical bars represent the 95% confidence intervals. Regressions include birth year fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by NIS-Child sample weights. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption.

Figure A7: **Goodman-Bacon (2021) Decomposition, ATUS (2003-2018), sample of females with infants**

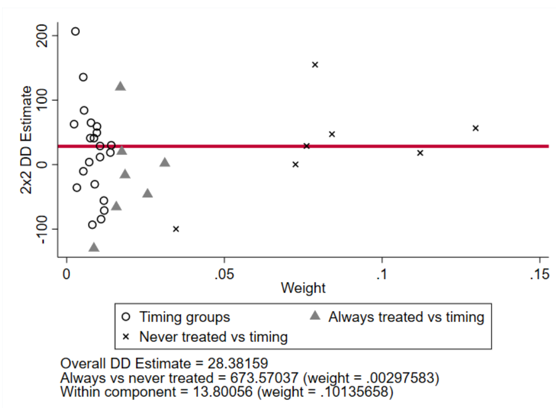
(a) Basic/Physical Care for Child



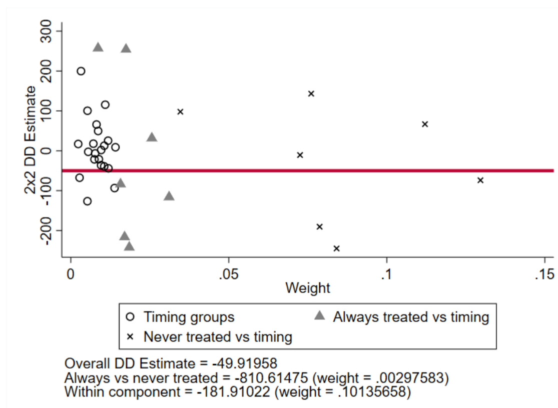
(b) Educational/Recreational Care for Child



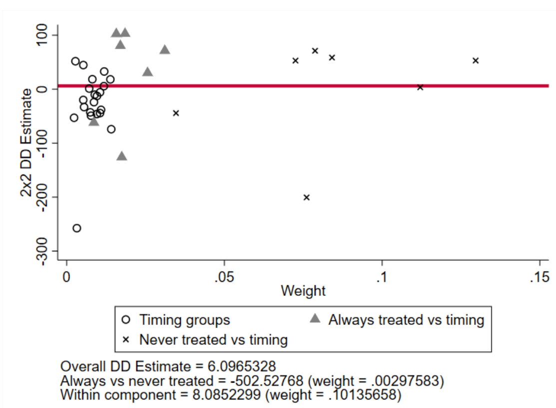
(c) Total Primary Child care



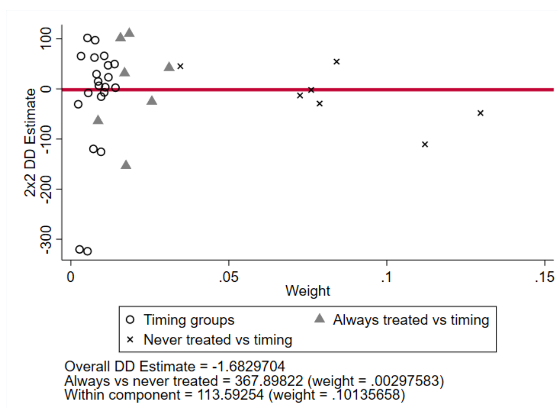
(d) Time on Formal Work



(e) Unpaid Domestic Work



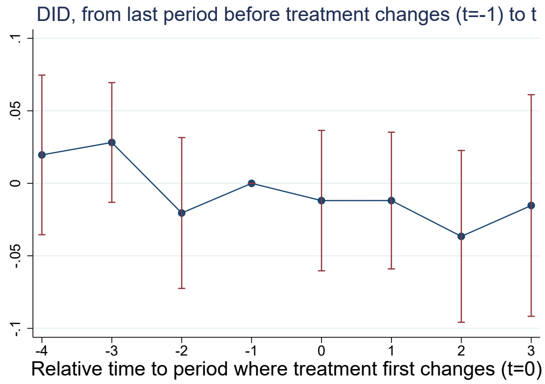
(f) Leisure Time



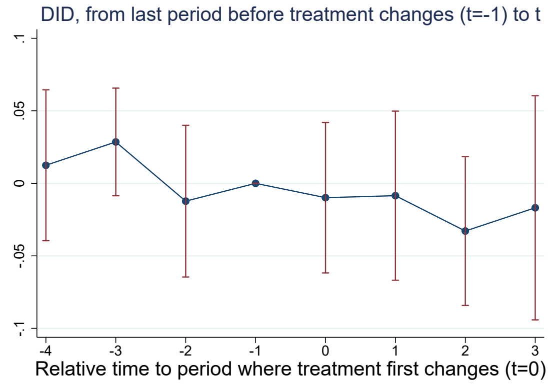
Note: Each panel presents the Goodman-Bacon (2021) decomposition for the outcome variable listed as the panel header. As the decomposition requires a balanced panel, only 30 states were included in this estimation (list available upon request). The × symbols represent the estimate from a given 2×2 difference-in-differences model that compares never treated states versus states that adopt during our sample period. Hollow circles represent the estimates from models that make comparisons between early and late adopters; triangles represent the estimates from models that compare always treated states to states that adopt during the sample period. Table A15 summarizes the overall decomposition for each of the outcome variables.

Figure A8: **Dynamic Effects of Hospital Breastfeeding Support Policies on Labor Market Outcomes, de Chaisemartin and D'Haultfœuille (2020b) estimator, CPS (2000-2018)**

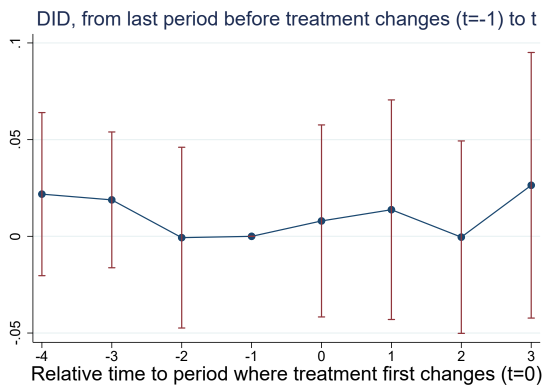
(a) Labor Force Participation



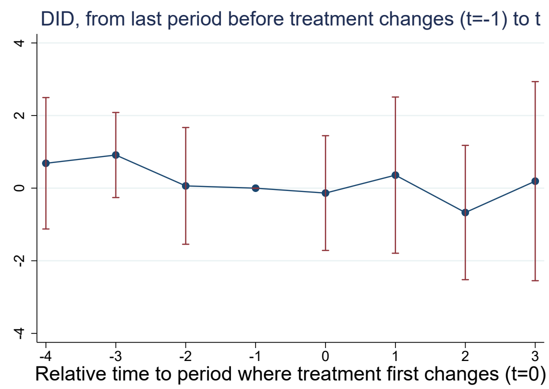
(b) Employed



(c) Worked Last Week



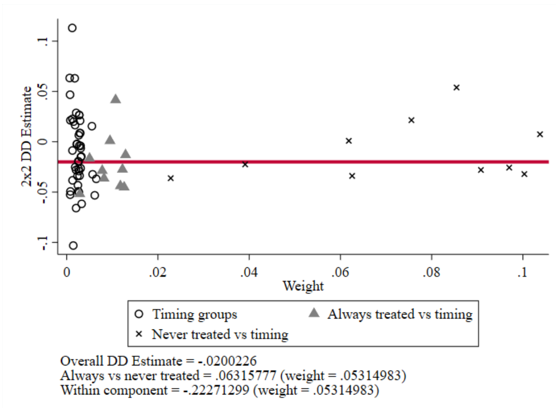
(d) Hours Worked Last Week (unconditional)



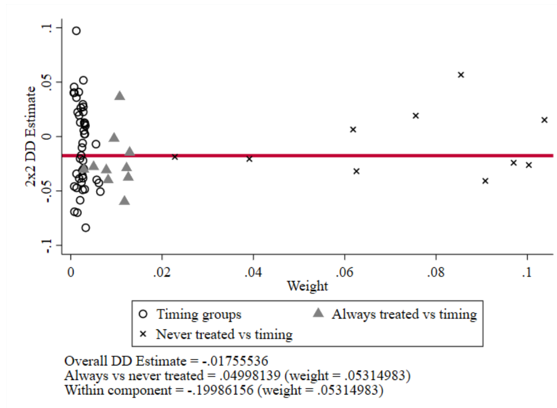
Note: Each figure presents the estimates for a separate outcome variable, as specified in the panel label. The treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. Estimates are obtained in Stata using the *did_multiplegt* command with the *robust_dynamic* option specified. Standard errors are clustered at the state level and computed using 200 block bootstrap replications; vertical bars represent the 95% confidence intervals. The sample is the set of women with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year and month fixed effects, state fixed effects, and the vector of state characteristics (see text). All regressions are weighted by CPS sample weights. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption.

Figure A9: **Goodman-Bacon (2021) Decomposition, CPS (2000-2018), main infant sample**

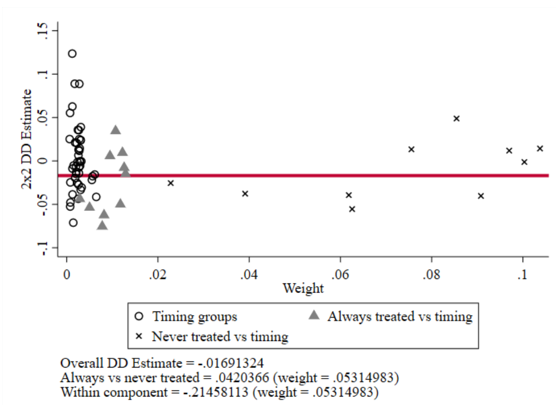
(a) Labor Force Participation



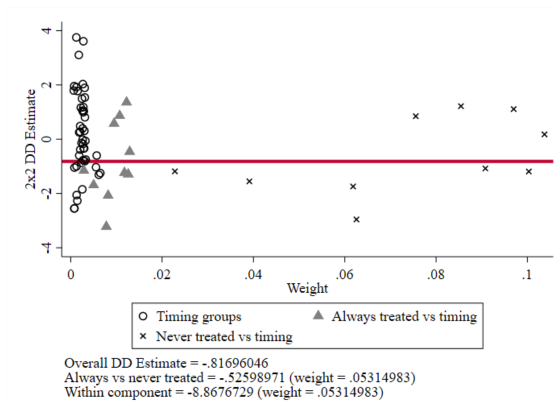
(b) Employed



(c) Worked Last Week



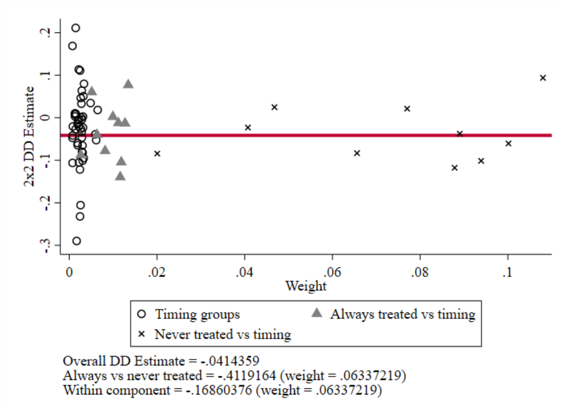
(d) Hours Worked (Unconditional)



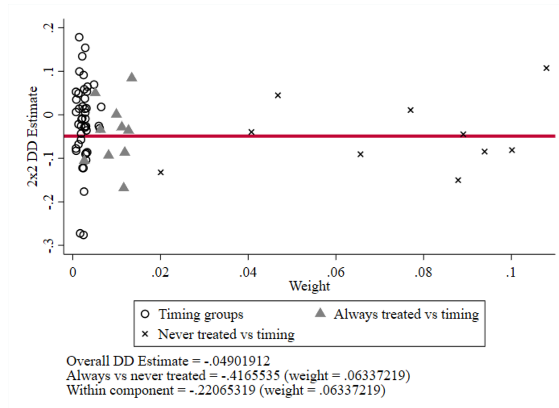
Note: Each panel presents the Goodman-Bacon (2021) decomposition for the outcome variable listed as the panel header. The $\times$ symbols represent the estimate from a given 2×2 difference-in-differences model that compares never treated states versus states that adopt during our sample period. Hollow circles represent the estimates from models that make comparisons between early and late adopters; triangles represent the estimates from models that compare always treated states to states that adopt during the sample period. Table A23 summarizes the overall decomposition for each of the outcome variables.

Figure A10: **Goodman-Bacon (2021) Decomposition, CPS (2000-2018), sample with 0-3 month old infants**

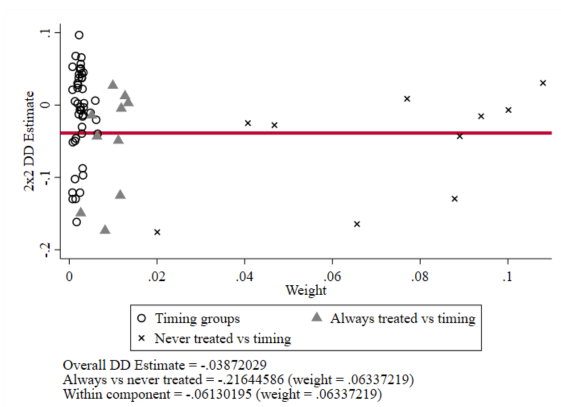
(a) Labor Force Participation



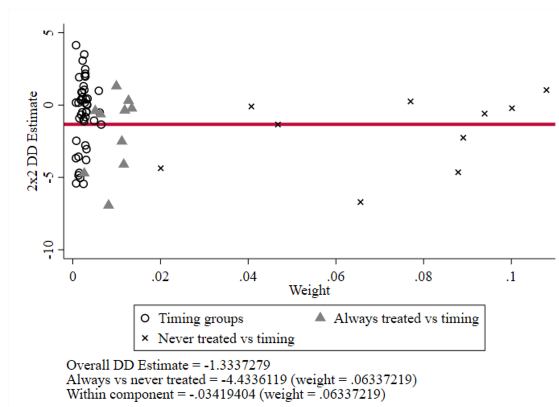
(b) Employed



(c) Worked Last Week



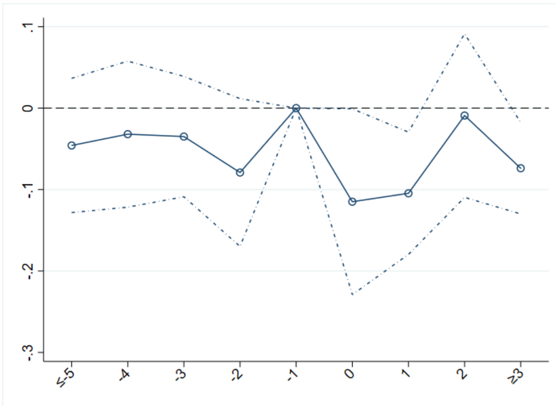
(d) Hours Worked (Unconditional)



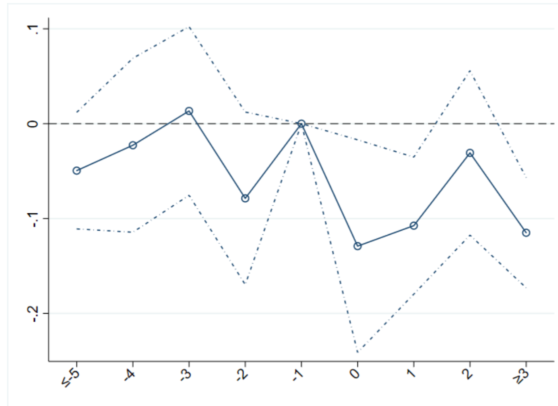
Note: Each panel presents the Goodman-Bacon (2021) decomposition for the outcome variable listed as the panel header. The × symbols represent the estimate from a given 2×2 difference-in-differences model that compares never treated states versus states that adopt during our sample period. Hollow circles represent the estimates from models that make comparisons between early and late adopters; triangles represent the estimates from models that compare always treated states to states that adopt during the sample period. Table A24 summarizes the overall decomposition for each of the outcome variables.

Figure A11: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Employment for Black Mothers, CPS (2000-2018)**

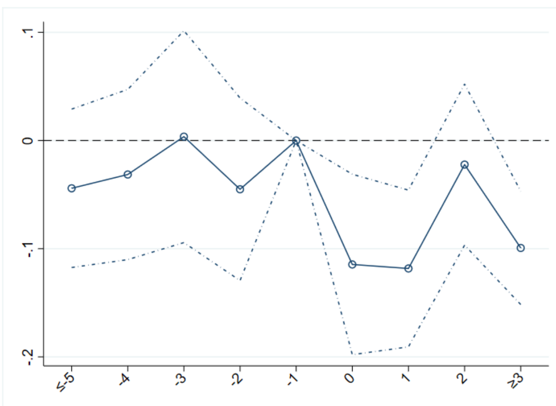
(a) Labor Force Participation



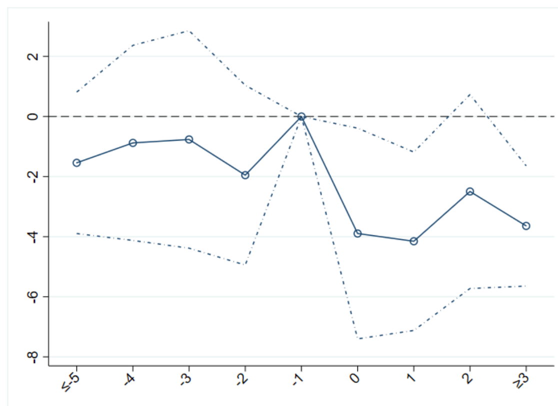
(b) Employed



(c) Worked Last Week



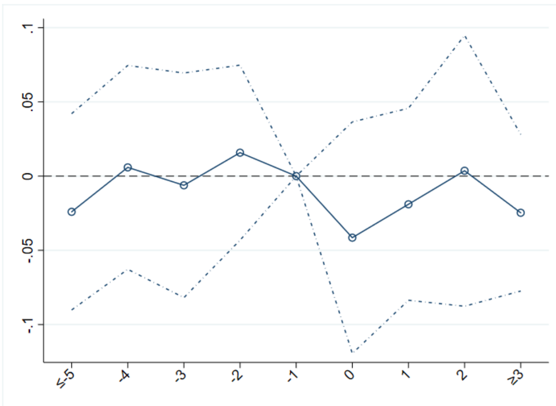
(d) Hours Worked Last Week (unconditional)



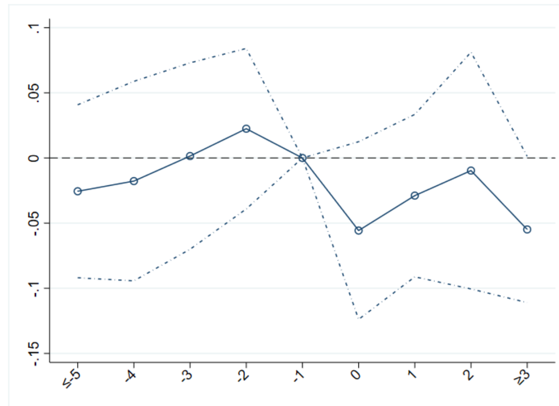
Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in each panel label and the treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. The sample is the set of non-Hispanic Black women with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year and month fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by CPS sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Figure A12: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Employment for Mothers with No College, CPS (2000-2018)**

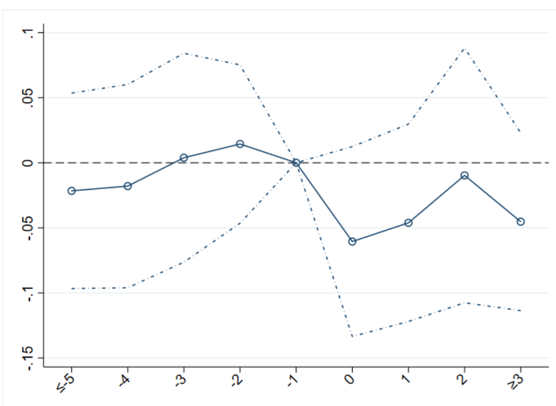
(a) Labor Force Participation



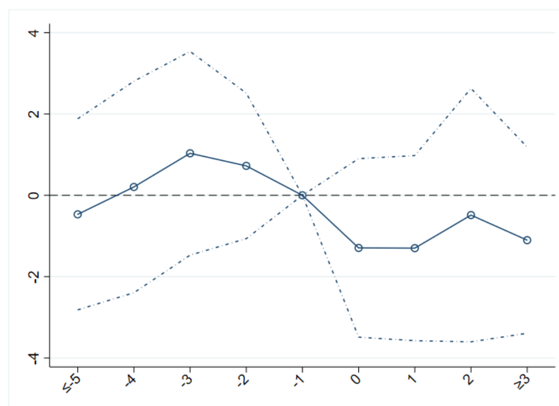
(b) Employed



(c) Worked Last Week

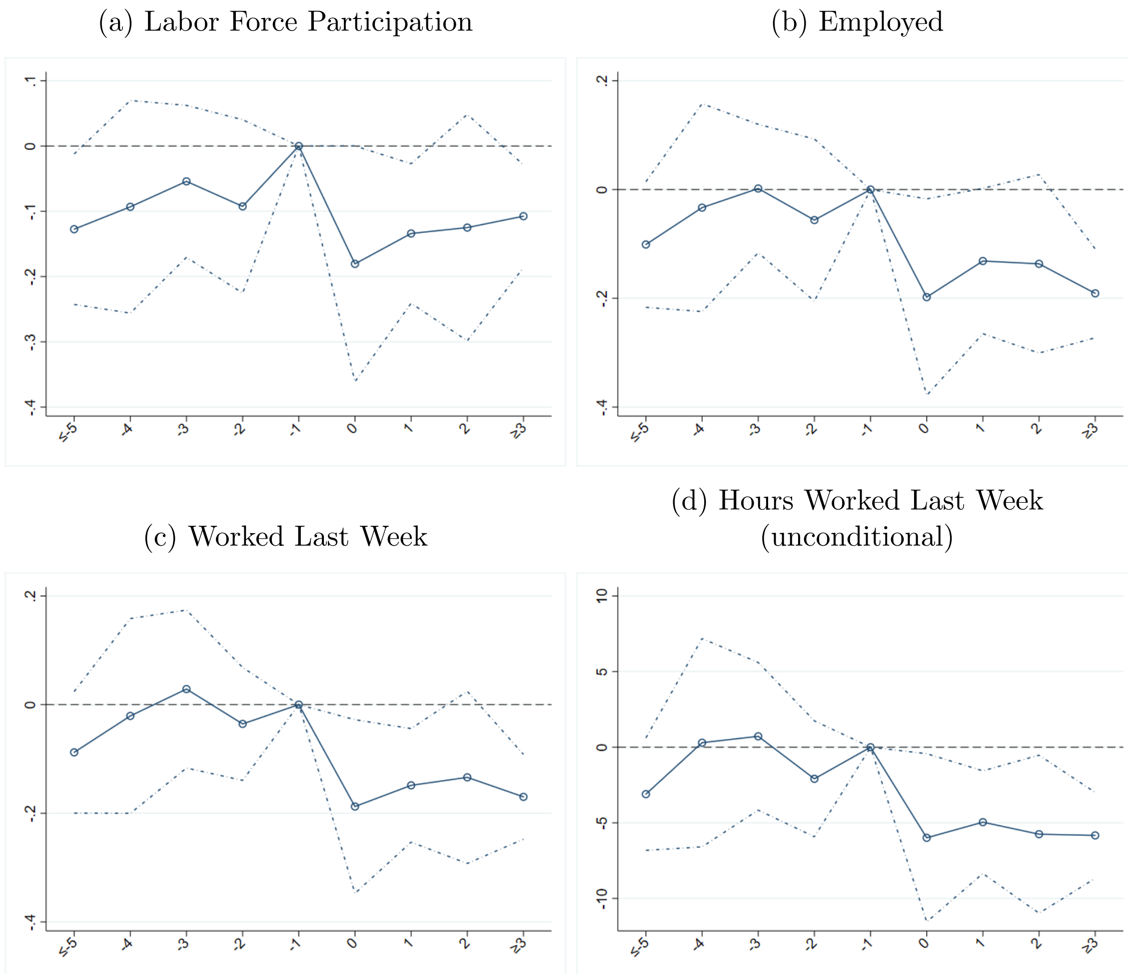


(d) Hours Worked Last Week (unconditional)



Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in each panel label and the treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. The sample is the set of women with no college education and with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year and month fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by CPS sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

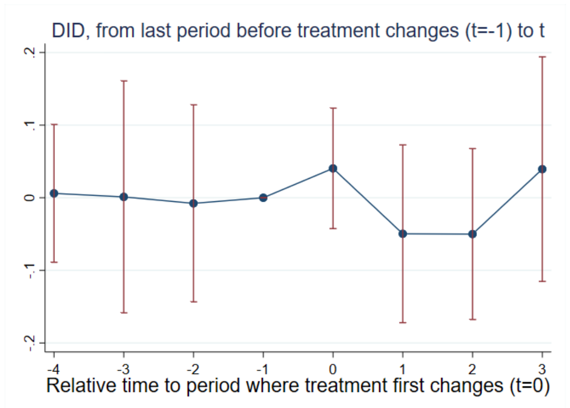
Figure A13: **Event Study Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Employment for Black Mothers with No College, CPS (2000-2018)**



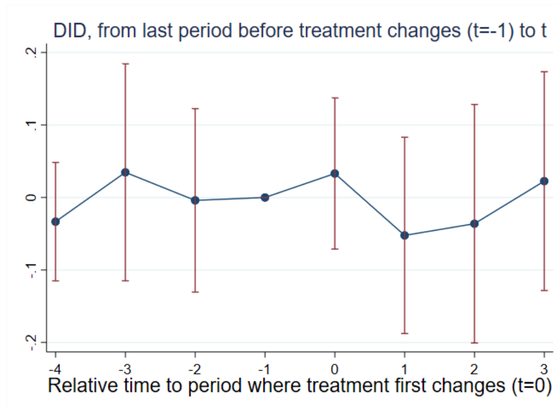
Note: Each figure presents the estimates from a separate regression, in which the outcome variable is as specified in each panel label and the treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. The sample is the set of non-Hispanic Black women with no college education and with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year and month fixed effects, state fixed effects, and the vector of individual and state characteristics (see text). All regressions are weighted by CPS sample weights; standard errors are clustered at the state level. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption. Dashed lines represent the 95% confidence intervals.

Figure A14: **Dynamic Effects of Hospital Breastfeeding Support Policies on Employment for Black Mothers, de Chaisemartin and D'Haultfœuille (2020b) estimator, CPS (2000-2018)**

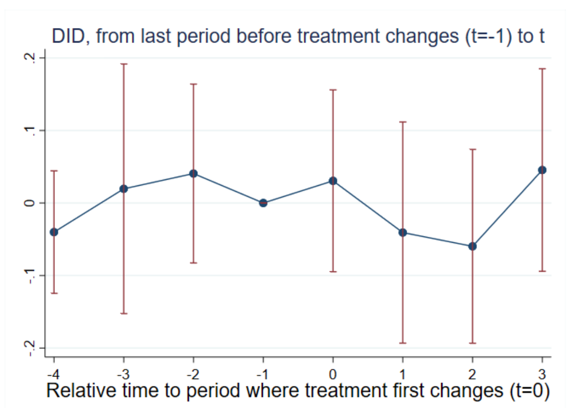
(a) Labor Force Participation



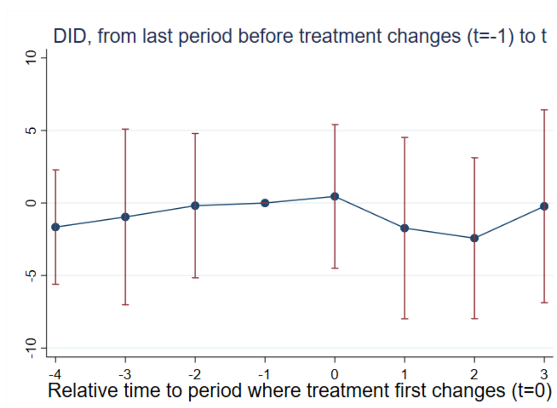
(b) Employed



(c) Worked Last Week



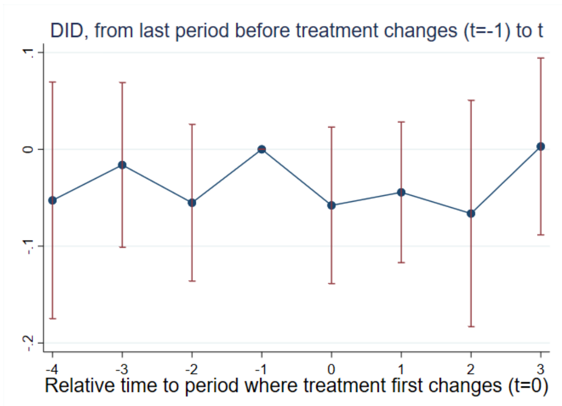
(d) Hours Worked Last Week (unconditional)



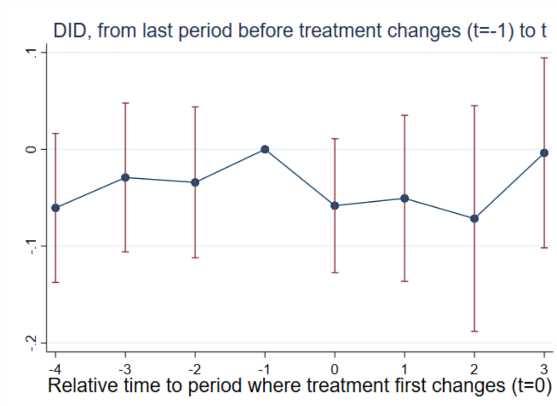
Note: Each figure presents the estimates for a separate outcome variable, as specified in the panel label. The treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. Estimates are obtained in Stata using the *did_multiplegt* command with the *robust_dynamic* option specified. Standard errors are clustered at the state level and computed using 200 block bootstrap replications; the vertical bars around each estimate represent the 95% confidence intervals. The sample is the set of non-Hispanic Black women with an own child age 0-3 months or 3-12 months at the time of survey. Regressions include survey year, survey month, and state fixed effects. The bootstrap procedure is unable to accommodate all state-level control variables; thus, they are omitted from these specifications. All regressions are weighted by CPS sample weights. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption.

Figure A15: **Dynamic Effects of Hospital Breastfeeding Support Policies on Employment for Mothers with No College, de Chaisemartin and D'Haultfœuille (2020b) estimator, CPS (2000-2018)**

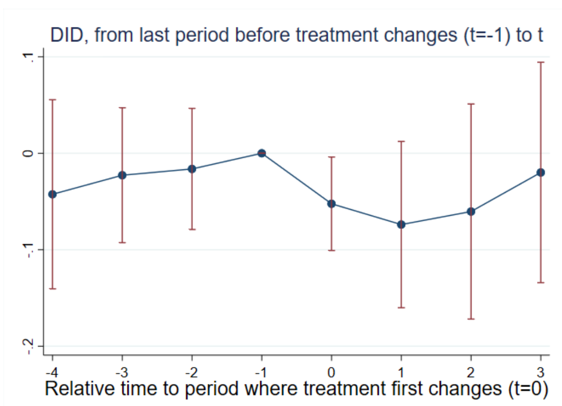
(a) Labor Force Participation



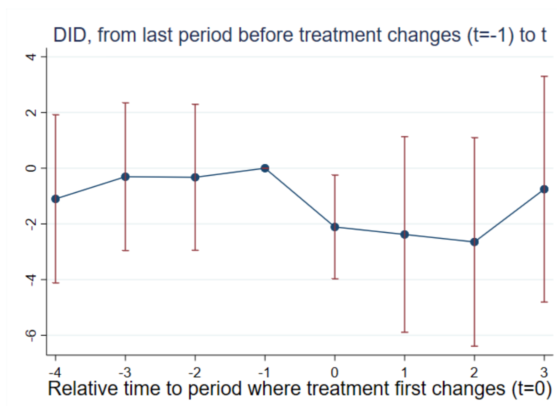
(b) Employed



(c) Worked Last Week



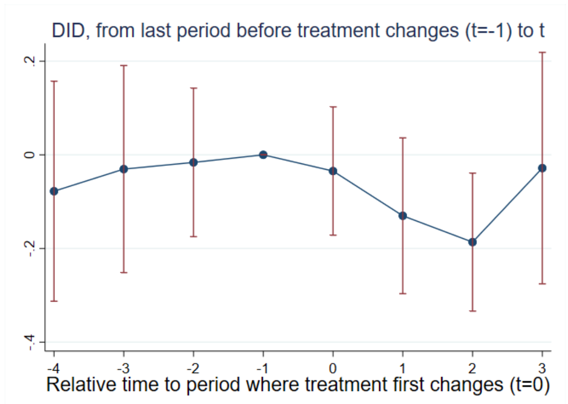
(d) Hours Worked Last Week (unconditional)



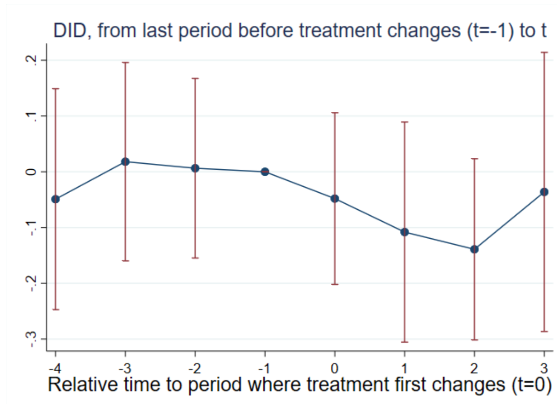
Note: Each figure presents the estimates for a separate outcome variable, as specified in the panel label. The treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. Estimates are obtained in Stata using the *did_multiplegt* command with the *robust_dynamic* option specified. Standard errors are clustered at the state level and computed using 200 block bootstrap replications; vertical bars around each estimate represent the 95% confidence intervals. The sample is the set of women with an own child age 0-3 months or 3-12 months at the time of survey whose highest level of education is high school or less. Regressions include survey year, survey month, and state fixed effects. The bootstrap procedure is unable to accommodate all state-level control variables; thus, they are omitted from these specifications. All regressions are weighted by CPS sample weights. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption.

Figure A16: **Dynamic Effects of Hospital Breastfeeding Support Policies on Employment for Black Mothers with No College, de Chaisemartin and D'Haultfœuille (2020b) estimator, CPS (2000-2018)**

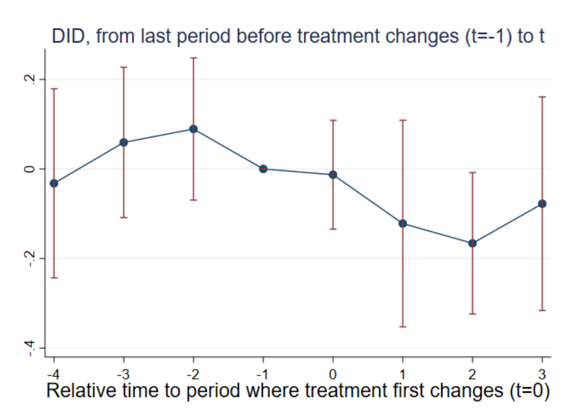
(a) Labor Force Participation



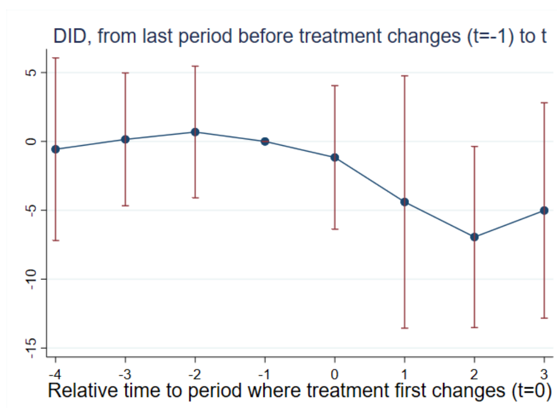
(b) Employed



(c) Worked Last Week



(d) Hours Worked Last Week (unconditional)



Note: Each figure presents the estimates for a separate outcome variable, as specified in the panel label. The treatment variable is a binary indicator capturing if the state ever adopted a state hospital breastfeeding support policy. Estimates are obtained in Stata using the *did_multiplegt* command with the *robust_dynamic* option specified. Standard errors are clustered at the state level and computed using 200 block bootstrap replications; vertical bars around each estimate represent the 95% confidence intervals. The sample is the set of non-Hispanic Black women with an own child age 0-3 months or 3-12 months at the time of survey and whose highest level of education is high school or less. Regressions include survey year, survey month, and state fixed effects. The bootstrap procedure is unable to accommodate all state-level control variables; thus, they are omitted from these specifications. All regressions are weighted by CPS sample weights. The x-axis measures event time relative to when a state adopts a hospital breastfeeding support policy; coefficients are relative to the excluded period of the year prior to policy adoption.

Appendix Tables

Table A1: WHO/UNICEF "Ten Steps to Successful Breastfeeding"

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

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

Table A2: Time Use Categories

Variable	**Included Categories of Activities and Examples**
Basic/ physical care for child	- Physical care for household children: dressing/bathing, feeding, putting to bed, etc. - Organizing and planning for household children - Looking after household children: supervising/watching - Attending household children's events - Waiting for/with household children - Picking up/dropping off household children - Providing or obtaining medical care for household children - Travel related to caring for and helping household children
Educational/ recreational care for child	- Reading to/with household children - Playing with household children (not sports) - Playing sports with household children - Arts and crafts with household children - Activities related to household children's education: homework, homeschooling, etc.
Time spent working	- Time spent working - Travel related to work
Home Production/ Unpaid Domestic Work **	- Household activities: cleaning, laundry, food and drink prep., home maintenance, household management, etc. - Consumer purchases: grocery shopping, purchase of other goods - Purchase of other services: childcare, financial, legal - Purchase and use of household services: interior cleaning, meal preparation, dry cleaning, lawn and garden services - Use of government services and participation in civic obligations - Non-social telephone calls: with educators, sales people, service providers
Residual Time (aka Leisure) **	- Personal care: sleeping, grooming, health related self-care - Use of personal care services: medical/health, grooming, etc. - Eating and drinking - Socializing, relaxing, and leisure: hanging out with friends or family, attending social events, relaxing, thinking, watching television or movies, playing games, hobbies, reading, attending performing arts or museums, etc. - Sports, exercise, and recreation: participating in various activities or attending events - Religious and spiritual activities: attending church, etc. - Volunteer activities: organizing, fundraising, providing various services, attending meetings, etc.

**Note: Category definitions following Trajkovski (2019)

Table A3: Timing of Adoption of Parental Leave and Breastfeeding Policies for Treated States

State	Hospital policy	Paid family leave+	TDI	More generous unpaid leave (beyond FMLA)‡	Provision of break time and private space by employers§	Employers prohibited from discriminating against breastfeeding employees	Breastfeeding permitted in any public/private location	Breastfeeding exempt from public indecency laws	Breastfeeding mothers exempt from jury duty
CALIFORNIA	2014	2004	1978	Pre-2001	2002	2013	1997		2000
GEORGIA	2002				1999*		1999		
ILLINOIS	2013				2001		2004	1995	2005
LOUISIANA	2007						2001	2001	
MARYLAND	2005						2003		
MISSISSIPPI	2016					2006	2006	2006	2006
NEW JERSEY	2014	2009	1978	Pre-2001		2018	1997		
NEW YORK	2005	2018	1978	Pre-2001	2007	2007	1994	1994	
OHIO	2012						2005		
SOUTH CAROLINA	2015						2008	2008	
TEXAS	2016								1995

+ Only 1 other state required PFL during our sample period: Rhode Island (2014); Two states take effect in 2020 (D.C. and Washington)

Only five states provide or require temporary disability insurance (TDI), which provides partial pay replacement to workers who take short-term (usually 6 weeks) leave from work due to injury, illness, pregnancy or childbirth. These programs existed prior to 1978, but were extended to cover pregnancy and childbirth in 1978 under the Pregnancy Discrimination Act. The 5 states are California, Hawaii, New Jersey, New York, and Rhode Island.

‡ More generous than federal minimum during our sample period (15 states): California, Connecticut, DC, Hawaii, Maine, Massachusetts, Montana, Minnesota, New York, New Jersey, Oregon, Rhode Island, Tennessee, Vermont, Wisconsin

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

*GA law simply encourages employer provision

Table A4: Robustness of Breastfeeding Effects to Specification Choices, NIS-Child (2003-2017)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome variable:								
Breastfeeding initiation	0.0208***	0.0231***	0.0333***	0.0383***	0.0244***	0.0257***	0.0393***	0.0397***
	(0.00768)	(0.00764)	(0.0101)	(0.00950)	(0.00705)	(0.00675)	(0.00932)	(0.00946)
Breastfeeding, 3 months	0.0242***	0.0273***	0.0341***	0.0406***	0.0241***	0.0234***	0.0415***	0.0409***
	(0.00513)	(0.00537)	(0.00837)	(0.00731)	(0.00488)	(0.00654)	(0.00725)	(0.00719)
Breastfeeding, 6 months	0.0111*	0.0143***	0.0214**	0.0280***	0.0177***	0.0142*	0.0290***	0.0287***
	(0.00656)	(0.00442)	(0.00817)	(0.00670)	(0.00588)	(0.00721)	(0.00704)	(0.00685)
Breastfeeding, 1 year	-0.00453	-0.00253	0.00874	0.0121	0.0119*	0.00248	0.0131	0.0123
	(0.00836)	(0.00738)	(0.00986)	(0.00812)	(0.00668)	(0.00653)	(0.00837)	(0.00826)
State and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes
State policy controls?			Yes	Yes	Yes	Yes	Yes	Yes
Other state/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes
Region×year fixed effects?					Yes			
NIS-Child sample weights?	Yes	Yes	Yes	Yes	Yes			Yes
Alternate NIS-Child weights?							Yes	
Dropping always treated states?								Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each estimate is from a separate regression in which the outcome variable is the indicator given in the first column of each row, and represents the coefficient on the binary Hospital Policy treatment variable. The estimates in column (4) are the main treatment estimates reported in Panel A of Table 2. Standard errors are clustered at the state level. For the specification in column (7), we use single-frame weights for the 2003-2011 sample years, and dual-frame weights for 2012-2017 sample years. Our main specification uses single-frame weights for 2003-2010 and dual-frame weights for 2011-2017.

Table A5: Goodman-Bacon (2021) Decomposition, NIS-Child (2003-2017)

	Ever Breastfed		Breastfeeding, 3 months		Breastfeeding, 6 months		Breastfeeding, 1 year	
	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value
Hospital Policy	0.0252	0.0200	0.0293	0.0000	0.0164	0.0210	-0.0019	0.8180
Decomposition								
	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight
Timing groups	0.0127	0.0866	0.0074	0.0866	0.0036	0.0866	-0.0059	0.0866
Always vs. timing	0.0118	0.0926	0.0153	0.0926	-0.0028	0.0926	-0.0128	0.0926
Never vs. timing	0.0264	0.7609	0.0325	0.7609	0.0173	0.7609	-0.0061	0.7609
Always vs. never	0.3675	0.0022	0.4026	0.0022	0.4932	0.0022	0.1862	0.0022
Within	0.0370	0.0576	0.0283	0.0576	0.0370	0.0576	0.0701	0.0576

Table A6: Maternal Characteristics Following the Implementation of Hospital Breastfeeding Support Policies, NIS-Child (2003-2017)

	(1) Maternal Education: College Degree	(2) Non-Hispanic White	(3) Hispanic	(4) Non-Hispanic Black	(5) Married	(6) Maternal Age: ≤ 29 years	(7) Moved from Child Birth State
Sample mean	0.314	0.495	0.274	0.130	0.657	0.422	0.0874
Hospital Policy	-0.0145*	-0.00399	0.00485	-0.00142	-0.0116	0.00650	0.00482
	(0.00758)	(0.00852)	(0.00683)	(0.00434)	(0.0149)	(0.00961)	(0.00508)
N	355,727	355,727	355,727	355,727	355,727	355,727	394,693
R-Squared	0.0232	0.117	0.167	0.0786	0.0223	0.0236	0.0169

* significant at 10%; ** significant at 5%; *** significant at 1%. Note: Results are from linear probability models and use NIS-Child sampling weights. The outcome variable is the indicator described in each column header, and the treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding policy by June of the infant's birth year. All models include controls for state policies; state unemployment rates; and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). The sample in column 7 additionally includes all infants that moved from their birth state of residence. Standard errors are clustered at the state level.

Table A7: Maternal Characteristics Following the Implementation of Hospital Breastfeeding Support Policies, PRAMS (2000-2018)

	(1) Maternal Education: College Degree	(2) Non-Hispanic White	(3) Hispanic	(4) Non-Hispanic Black	(5) Married	(6) Maternal Age: $\leq$ 29 years
Sample mean	0.313	0.603	0.170	0.144	0.630	0.591
Hospital Policy	-0.00422	-0.0128	0.00421	0.00180	-0.00541	0.00512
	(0.00415)	(0.0157)	(0.0111)	(0.00596)	(0.00740)	(0.00480)
N	691,827	691,827	691,827	691,827	690,924	691,793
R-Squared	0.0221	0.0837	0.0907	0.0635	0.0172	0.0266

* significant at 10%; ** significant at 5%; *** significant at 1%. Note: Results are from linear probability models and use PRAMS sampling weights. The outcome variable is the indicator described in each column header, and the treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding policy by June of the infant's birth year. All models include state of birth, calendar month of birth, and year of birth fixed effects, as well as controls for state policies; state unemployment rates; and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered at the state level.

Table A8: Prenatal Care (PNC), Infant Health, and Delivery Modality at Birth Following the Implementation of Hospital Breastfeeding Support Policies, PRAMS (2000-2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Late PNC	PNC Started 1st Trimester	Adequate PNC, Kotelchuck Index	Low Birth Weight, ≤2500 grams	Macrosomia, ≥4500 grams	Preterm Birth	C-Section
Sample mean	0.0373	0.823	0.764	0.0680	0.0124	0.0852	0.294
Hospital Policy	-0.00247	0.00310	0.000904	0.00124**	-0.000957	0.000121	-0.000361
	(0.00298)	(0.00600)	(0.00876)	(0.000589)	(0.000752)	(0.00155)	(0.00555)
N	639,180	662,301	675,882	675,882	674,688	675,882	674,756
R-Squared	0.0252	0.107	0.0524	0.0118	0.00491	0.00604	0.0331

* significant at 10%; ** significant at 5%; *** significant at 1%. Notes: Results are from linear probability models and use PRAMS sampling weights. The outcome variable is the indicator described in each column header, and the treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding policy by June of the infant's birth year. All models include state of birth, calendar month of birth, and year of birth fixed effects, as well as controls for state policies; state unemployment rates; and state demographic characteristics (fraction black, Hispanic, and other races, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered at the state level.

Table A9: Effects of State Hospital Breastfeeding Support Policies on Breastfeeding Initiation and Duration, PRAMS (2000-2018)

	Full Sample, 2000-2018		Table 3 sample	
	(1)	(2)	(3)	(4)
	Breastfeeding initiation	Breastfeeding, 8 weeks	Breastfeeding initiation	Breastfeeding, 8 weeks
Sample mean	0.803	0.581	0.814	0.593
Hospital Policy	0.0194***	0.0138*	0.0160**	0.0221***
	(0.00681)	(0.00757)	(0.00606)	(0.00729)
N	683,084	675,882	287,516	284,495
R-squared	0.134	0.161	0.138	0.153

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

Note: Results are from linear probability models and use PRAMS sampling weights. The outcome variable is the indicator described in each column header. The treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding support policy by June of the infant's birth year. Surveys are conducted when infants are approximately 2-6 months old, between 2000 and 2018. All models include controls for individual demographic characteristics (child gender, race/ethnicity fixed effects, fixed effects for number of previous live births, whether the mother received WIC during pregnancy, and fixed effects for mother's age group, education level, and marital status); state, birth year, and calendar month of birth fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction Black, Hispanic, and other non-white, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered at the state level.

Table A10: Effects of Lactation Consultant Policy Component on Breastfeeding Initiation and Duration, NIS-Child (2003-2017)

	(1) Breastfeeding initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Sample mean	0.757	0.580	0.436	0.222
Lactation consultant requirement	0.0393***	0.0244**	0.0362***	0.0000336
	(0.0129)	(0.0104)	(0.0118)	(0.00776)
Non-lactation consultant requirement	0.00554	0.0231**	-0.00361	0.0152
	(0.0136)	(0.0111)	(0.0117)	(0.00911)
N	354,642	343,792	343,792	343,792
R-Squared	0.121	0.134	0.128	0.0735

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

Note: Results are from linear probability models and use NIS-Child sampling weights. The outcome variable is the indicator described in each column header. Infants are observed at ages 19-35 months, between 2003 and 2017. All models include controls for individual demographic characteristics (age at observation fixed effects, gender, race, number of children in the household, first born status, whether the child ever received WIC, and mother's age, education level, and marital status); state and birth year fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction Black, Hispanic, and other non-white, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered at the state level.

Table A11: Role of Baby-Friendly Hospitals (BFH), NIS-Child (2007-2017)

	(1) Breastfeeding initiation	(2) Breastfeeding, 3 months	(3) Breastfeeding, 6 months	(4) Breastfeeding, 1 year
Panel A: Main Specification, sample restricted to infants born 2007 or later				
Hospital Policy	0.0411***	0.0464***	0.0222**	-0.0193
	(0.0129)	(0.00838)	(0.00989)	(0.0175)
N	151,542	144,898	144,898	144,898
R-Squared	0.122	0.139	0.145	0.0861
Mean of Dependent	0.790	0.622	0.474	0.249
Panel B: Controlling for Percent of Births in Baby-Friendly Hospitals in State-Year of Birth				
Hospital Policy	0.0413***	0.0464***	0.0222**	-0.0191
	(0.0130)	(0.00848)	(0.0101)	(0.0172)
% of Births in Baby-Friendly Hospital	-0.0234	0.00511	0.0111	-0.0464
	(0.0600)	(0.0569)	(0.0779)	(0.0341)
N	151,542	144,898	144,898	144,898
R-Squared	0.122	0.139	0.145	0.0861
Mean of Dependent	0.790	0.622	0.474	0.249
Panel C: Allowing impact of policy to vary based on percent of births in BFH at time of policy adoption				
Hospital Policy	0.0622***	0.0468***	0.0237**	0.0102
	(0.0173)	(0.00793)	(0.0110)	(0.00921)
Hospital Policy x % of Births in Baby-Friendly Hospital at Adoption Year	-0.258***	-0.00521	-0.0196	-0.378***
	(0.0927)	(0.0350)	(0.0671)	(0.0368)
% of Births in Baby-Friendly Hospital	-0.0185	0.00521	0.0115	-0.0391
	(0.0592)	(0.0568)	(0.0780)	(0.0391)
N	151542	144898	144898	144898
R-Squared	0.122	0.139	0.145	0.0862
Mean of Dependent	0.790	0.622	0.474	0.249

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

Note: Results are from linear probability models and use NIS-Child sampling weights. The outcome variable is the indicator described in each column header, and the treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding policy by June of the infant's birth year. The sample in all panels is limited to births that occurred in 2007 or later, as this is the first year data on the percent of live births in a state that occurred in a Baby Friendly Hospital are available. See notes to 2 for details on the specification and control variables. Standard errors are clustered at the state level.

Table A12: Effects of Hospital Breastfeeding Support Polices By Type of Birth, PRAMS (2000-2018)

	(1) Breastfeeding Initiation	(2) Breastfeeding, 8 weeks
Panel A: By delivery modality		
Hospital Policy	0.0197***	0.0155*
	(0.00716)	(0.00874)
× C-Section Birth	-0.0000925	-0.00473
	(0.00453)	(0.00654)
Panel B: Singleton vs. multiples		
Hospital Policy	0.0196***	0.0133*
	(0.00696)	(0.00771)
× Multiple Birth	-0.00697	0.0212
	(0.0186)	(0.0251)
Panel C: By preterm birth status		
Hospital Policy	0.0190**	0.0135
	(0.00713)	(0.00805)
× Preterm Birth	0.00457	0.00383
	(0.00792)	(0.00911)

* p <0.10, ** p <0.05, *** p <0.01

Note: Results are from linear probability models and use PRAMS sampling weights. The outcome variable is the indicator described in each column header. The treatment variable is an indicator variable equal to one if the state had adopted a hospital breastfeeding support policy by June of the infant's birth year. Surveys are conducted when infants are approximately 2-6 months old, between 2000 and 2018. All models include controls for individual demographic characteristics (child gender, race/ethnicity fixed effects, fixed effects for number of previous live births, whether the mother received WIC during pregnancy, and fixed effects for mother's age group, education level, and marital status); state, birth year, and calendar month of birth fixed effects; state policies (see text for details); state unemployment rates; and state demographic characteristics (fraction Black, Hispanic, and other non-white, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). All regressions additionally include an interaction between the indicator variable for ever adopting a hospital breastfeeding policy and the given heterogeneity variable. Standard errors are clustered at the state level.

Table A13: Descriptive Statistics, ATUS (2003-2018)

	(1) Full Sample	(2) Mothers in states that adopted a hospital regulation during sample	(3) Mothers in states that did not adopt a hospital regulation during sample
Time Use Outcomes			
Total Primary Child Care	204.473	207.508	202.153
Basic/Physical Care for Child	150.140	152.074	148.662
Educational/Recreational Care for Child	54.333	55.434	53.491
Time Spent Working	140.440	127.232	150.537
Unpaid Domestic Work	169.708	174.187	166.284
Leisure Time	862.746	870.200	857.048
Survey conducted on Weekend	0.276	0.269	0.281
Survey conducted on Holiday	0.019	0.019	0.019
Mother's Characteristics			
Non-Hispanic White	0.554	0.430	0.649
Hispanic	0.247	0.374	0.150
Non-Hispanic Black	0.134	0.131	0.137
Other ethnicity	0.065	0.066	0.064
Number of people in household	4.657	4.869	4.495
Number of children <18	2.253	2.360	2.170
Less than high school	0.189	0.216	0.169
High school diploma	0.269	0.258	0.278
Some college	0.242	0.233	0.248
College degree or above	0.300	0.294	0.305
Married	0.652	0.628	0.671
Age: <29 yrs	0.520	0.505	0.532
Observations	4,296	1,729	2,567

Note: All values are weighted means calculated by the authors from ATUS 2003-2018 data, using provided sample weights. The sample consists of the set of women with a child under the age of one at the time of survey. The states included in column 2 are California, Illinois, Louisiana, Maryland, Mississippi, New Jersey, New York, Ohio, South Carolina, and Texas.

Table A14: Robustness of ATUS Outcomes to Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Total Primary Childcare									
Hospital Policy	39.34**	36.93***	28.24	32.75**	44.83***	30.14*	18.35	32.86**	38.37**
	(17.36)	(11.84)	(20.90)	(16.01)	(15.12)	(17.16)	(13.82)	(15.41)	(16.71)
Outcome: Basic/physical care for child									
Hospital Policy	24.01*	22.71**	25.11*	28.25***	40.82***	22.21*	8.207	27.86***	31.22***
	(12.36)	(10.26)	(13.67)	(10.50)	(9.345)	(11.36)	(7.826)	(9.862)	(10.73)
Outcome: Educational/recreational care for child									
Hospital Policy	15.32*	14.22**	3.128	4.502	4.010	7.928	10.15	4.996	7.154
	(8.214)	(7.075)	(8.864)	(8.238)	(9.611)	(8.086)	(8.598)	(8.163)	(8.539)
Outcome: Time spent working									
Hospital Policy	-31.03	-42.31**	-23.96	-34.35**	-34.67**	-27.31	-11.40	-35.06**	-37.39**
	(20.68)	(17.00)	(18.01)	(16.19)	(16.16)	(17.01)	(13.23)	(15.54)	(16.96)
Outcome: Unpaid Domestic Work									
Hospital Policy	11.77	13.96	-2.712	0.514	1.362	0.972	17.14*	4.558	-1.846
	(10.85)	(9.286)	(9.564)	(9.942)	(12.01)	(9.925)	(8.914)	(8.630)	(10.30)
Outcome: Leisure Time									
Hospital Policy	-19.21	-7.301	7.695	12.58	-4.671	6.378	-22.20**	10.43	11.09
	(14.83)	(9.388)	(17.48)	(11.22)	(14.14)	(11.67)	(10.84)	(9.926)	(11.81)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 2002 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents an estimate from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Table 6. Standard errors are clustered at the state level.

Table A15: Goodman-Bacon (2021) Decomposition, ATUS (2003-2018)

	Total Primary Child Care		Basic/ Physical Care for Child		Educational/ Recreational Care for Child		Time Spent Working		Unpaid Domestic Work		Leisure Time	
	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value
Hospital policy	28.3816	0.1490	29.1921	0.0130	-0.8105	0.938	-49.9196	0.2280	6.0965	0.8000	-1.6830	0.6000
Decomposition												
	Beta	Total Wt.	Beta	Total Wt.	Beta	Total Wt.	Beta	Total Wt.	Beta	Total Wt.	Beta	Total Wt.
Timing groups	9.459651	0.174148	13.29001	0.174148	-3.83036	0.174148	2.186456	0.174148	-19.8262	0.174148	-1.33171	0.174148
Always vs. timing	-8.71178	0.134087	-6.11793	0.134087	-2.59385	0.134087	-41.7851	0.134087	38.4391	0.134087	12.00886	0.134087
Never vs. timing	41.70545	0.587432	37.35969	0.587432	4.34576	0.587432	-40.5961	0.587432	8.632477	0.587432	-26.6745	0.587432
Always vs. never	673.5704	0.002976	412.8156	0.002976	260.7548	0.002976	-810.615	0.002976	-502.528	0.002976	367.8982	0.002976
Within	13.80056	0.101357	44.62676	0.101357	-30.8262	0.101357	-181.91	0.101357	8.08523	0.101357	113.5925	0.101357

Table A16: Average Effects from de Chaisemartin and D'Haultfœuille (2020b) estimator, ATUS (2003-2018)

	(1) Total Primary Child Care	(2) Basic/ Physical Care for Child	(3) Educational/ Recreational Care for Child	(4) Time Spent Working	(5) Unpaid Domestic Work	(6) Leisure Time
Average Effect	-21.904	-26.558	4.654	-42.382	2.498	47.412
	(32.683)	(31.307)	(21.688)	(84.872)	(39.299)	(61.376)
N	3,932	3,932	3,932	3,932	3,932	3,932
Mean of Dependent	202.81	148.21	54.59	140.94	168.55	864.22

Note: Standard errors are in parenthesis; they are clustered at the state level and computed using 200 block bootstrap replications. Each column represents the average effect of a separate estimation from the Stata command did_multiplegt with 3 dynamic effect periods after the state policy goes into effect. Regressions include survey year fixed effects, state fixed effects, and controls for related state-level laws (see text). All regressions are weighted by ATUS sample weights.

Table A17: Effects of Hospital Breastfeeding Support Policies on Parental Time Use, ATUS (2003-2018)

	(1) Total Primary Childcare	(2) Basic/ physical care for child	(3) Educational/ Recreational care for child	(4) Time spent working	(5) Unpaid Domestic Work	(6) Leisure Time
Panel A: Pooled Sample (Female and Male)						
Hospital Policy	15.37*	8.968*	6.400	-20.89	3.463	6.437
	(7.771)	(5.331)	(4.450)	(18.79)	(7.917)	(13.70)
N	6,689	6,689	6,689	6,689	6,689	6,689
R-Squared	0.23	0.21	0.08	0.27	0.15	0.24
Mean of Dependent	155.14	109.31	45.83	228.01	134.63	862.87
Panel B: Male Subsample						
Hospital Policy	5.947	7.251	-1.304	-0.451	-2.506	-1.866
	(6.557)	(5.516)	(2.404)	(14.26)	(6.851)	(15.17)
N	2,757	2,757	2,757	2,757	2,757	2,757
R-Squared	0.08	0.06	0.05	0.25	0.08	0.27
Mean of Dependent	93.35	58.88	34.47	340.88	90.67	861.11

* p <0.10, ** p <0.05, *** p <0.01

Note: Outcome variables are measures of the number of minutes during the survey day spent on the time use category given in the column header. All columns are weighted by ATUS sample weights and have state, survey year, and survey month fixed effects. Standard errors are clustered at the state level. See notes to Table 6 for additional specification details.

Table A18: Effects of Hospital Breastfeeding Support Policies on Time Use of Mothers *without* Infants, ATUS (2003-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Primary Childcare	Basic/ physical care for child	Educational/ Recreational care for child	Time spent working	Unpaid Domestic Work	Leisure Time
Hospital Policy	-0.901	-1.031	0.130	-6.851	-6.125*	-1.132
	(2.215)	(2.046)	(1.393)	(6.293)	(3.136)	(7.225)
N	43,646	43,646	43,646	43,646	43,646	43,646
R-Squared	0.15	0.14	0.06	0.17	0.14	0.24
Mean of Dependent	68.06	48.49	19.57	180.54	160.05	924.12

* p <0.10, ** p <0.05, *** p <0.01

Note: Outcome variables are measures of the number of minutes during the survey day spent on the time use category given in the column header. The sample is the set of women observed in the ATUS with children in the household and whose youngest child is between 2 and 18 years old. The treatment variable is an indicator variable that is equal to one if a hospital policy was in effect by June of the survey year. See notes to Table 6 for details on the specification and control variables. All models are weighted by ATUS sample weights and standard errors are clustered at the state level.

Table A19: Descriptive Statistics, CPS (2000-2018)

	(1) Full Sample	(2) Mothers in states that adopted a hospital regulation during sample	(3) Mothers in states that did not adopt a hospital regulation during sample
Employment Outcomes			
In labor force	0.576	0.556	0.593
Employed	0.534	0.513	0.552
Worked last week	0.434	0.416	0.449
Hours worked last week (unconditional)	14.538	14.168	14.854
Mother's characteristics			
Non-Hispanic white	0.614	0.510	0.702
Hispanic	0.193	0.269	0.128
Non-Hispanic Black	0.114	0.133	0.098
Other ethnicity	0.079	0.088	0.072
Only 1 child <5 yrs old in household	0.575	0.583	0.568
2 children in household	0.349	0.345	0.352
Only 1 child in household	0.382	0.376	0.386
Less than high school	0.123	0.138	0.110
High school diploma	0.251	0.251	0.251
Some college	0.278	0.268	0.287
College degree or above	0.348	0.343	0.352
Married	0.726	0.721	0.730
Age: <29 yrs	0.512	0.490	0.531
Observations	109,187	36,333	72,854

Note: All values are weighted means calculated by the authors from CPS 2000-2018 data, using provided sample weights. The sample consists of the set of mothers with an infant between 0 and 3 months or between 3 and 12 months of age at the time of survey. The states included in column 2 are California, Georgia, Illinois, Louisiana, Maryland, Mississippi, New Jersey, New York, Ohio, South Carolina, and Texas.

Table A20: Effects of Hospital Breastfeeding Support Policies on Maternal Work, CPS (2000-2018)

	(1) Labor force Participation	(2) Employed	(3) Worked last week	(4) Hours worked last week (unconditional)
Panel A: Overall effect for women with infants				
Hospital Policy	-0.00133	-0.00313	-0.00253	0.0384
	(0.00619)	(0.00602)	(0.00598)	(0.251)
N	257,734	257,734	257,734	257,734
R-Squared	0.0932	0.103	0.0820	0.0819
Mean of Dependent	0.575	0.528	0.437	14.61
Panel B: Decomposed by Age of Infant				
Hospital Policy	-0.00640	-0.0181**	-0.0460***	-1.580***
x baby 0-3 mos	(0.00934)	(0.00805)	(0.0145)	(0.476)
Hospital Policy	-0.000593	-0.000916	0.00368	0.269
x baby other age	(0.00755)	(0.00726)	(0.00556)	(0.241)
N	257,734	257,734	257,734	257,734
R-Squared	0.0932	0.103	0.0821	0.0821
Mean of Dependent	0.575	0.528	0.437	14.61

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample is the set of women observed in the CPS with an infant less than 12 months of age. See notes to Table 7 for details on the specification and control variables. All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table A21: Robustness of CPS Outcomes to Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Labor Force Status									
Hospital Policy	-0.00636	-0.00718	-0.0159***	-0.0132**	-0.0185***	-0.0132**	-0.0114*	-0.00812	-0.0113**
	(0.00682)	(0.00489)	(0.00578)	(0.00514)	(0.00646)	(0.00531)	(0.00586)	(0.00608)	(0.00534)
Outcome: Employed									
Hospital Policy	-0.00573	-0.00914*	-0.0185***	-0.0181***	-0.0264***	-0.0176***	-0.0114**	-0.0141**	-0.0170***
	(0.00700)	(0.00478)	(0.00528)	(0.00498)	(0.00618)	(0.00516)	(0.00545)	(0.00567)	(0.00491)
Outcome: Worked Last Week									
Hospital Policy	-0.0213***	-0.00377	-0.0347***	-0.00790	-0.0157**	-0.00525	-0.00568	-0.00723	-0.00534
	(0.00553)	(0.00509)	(0.00739)	(0.00586)	(0.00702)	(0.00571)	(0.00718)	(0.00597)	(0.00543)
Outcome: Hours Worked Last Week (unconditional)									
Hospital Policy	-0.823***	-0.200	-1.261***	-0.301	-0.699**	-0.211	-0.323	-0.194	-0.226
	(0.241)	(0.230)	(0.356)	(0.301)	(0.330)	(0.288)	(0.386)	(0.299)	(0.285)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 1999 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents an estimate from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Panel A of Table 7. Standard errors are clustered at the state level.

Table A22: Robustness of CPS Outcomes to Specification Choices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Labor Force Status									
Hospital Policy x baby 0-3 mos	-0.00557	-0.0107	-0.0136	-0.0170	-0.0223*	-0.0155	-0.0152	-0.0104	-0.0114
	(0.0141)	(0.0119)	(0.0121)	(0.0108)	(0.0122)	(0.00984)	(0.0128)	(0.0124)	(0.0118)
Hospital Policy x baby 3-12 mos	0.000305	-0.00581	-0.00760	-0.0117*	-0.0169**	-0.0124*	-0.00982	-0.00771	-0.0114**
	(0.00703)	(0.00532)	(0.00685)	(0.00640)	(0.00732)	(0.00693)	(0.00757)	(0.00665)	(0.00554)
Outcome: Employed									
Hospital policy x baby 0-3 mos	-0.0139	-0.0207**	-0.0255**	-0.0299***	-0.0384***	-0.0282***	-0.0242**	-0.0229*	-0.0258**
	(0.0125)	(0.0103)	(0.0109)	(0.00992)	(0.0104)	(0.0102)	(0.0112)	(0.0116)	(0.0110)
Hospital Policy x baby 3-12 mos	0.00278	-0.00438	-0.00880	-0.0133**	-0.0212***	-0.0133**	-0.00602	-0.0107*	-0.0133***
	(0.00775)	(0.00558)	(0.00646)	(0.00611)	(0.00753)	(0.00637)	(0.00705)	(0.00612)	(0.00473)
Outcome: Worked Last Week									
Hospital policy x baby 0-3 mos	-0.0387***	-0.0433***	-0.0449***	-0.0476***	-0.0554***	-0.0450***	-0.0458***	-0.0477***	-0.0341**
	(0.0128)	(0.0123)	(0.0131)	(0.0132)	(0.0134)	(0.0123)	(0.0130)	(0.0126)	(0.0127)
Hospital Policy x baby 3-12 mos	0.0172*	0.0123*	0.0111	0.00837	0.00104	0.0103	0.0107	0.00841	0.00638
	(0.00870)	(0.00647)	(0.00734)	(0.00656)	(0.00784)	(0.00666)	(0.00776)	(0.00659)	(0.00612)
Outcome: Hours Worked Last Week (unconditional)									
Hospital Policy x baby 0-3 mos	-1.364***	-1.557***	-1.539***	-1.661***	-2.048***	-1.578***	-1.736***	-1.638***	-1.332**
	(0.439)	(0.400)	(0.497)	(0.478)	(0.488)	(0.461)	(0.525)	(0.440)	(0.527)
Hospital Policy x baby 3-12 mos	0.525*	0.350	0.349	0.255	-0.127	0.322	0.253	0.360	0.227
	(0.311)	(0.242)	(0.307)	(0.287)	(0.322)	(0.278)	(0.386)	(0.289)	(0.263)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 1999 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents the estimates from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Panel B of Table 7. Standard errors are clustered at the state level.

Table A23: Goodman-Bacon (2021) Decomposition, CPS (2000-2018), main infant sample

	Labor Force Participation		Employed		Worked Last Week		Hours Worked (Unconditional)	
	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value
Hospital Policy	-0.0200	0.0150	-0.0176	0.0280	-0.0169	0.0890	-0.8170	0.0520
Decomposition								
	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight
Timing groups	-0.0144	0.1116	-0.0133	0.1116	-0.0005	0.1116	0.1127	0.1116
Always vs. timing	-0.0202	0.0935	-0.0228	0.0935	-0.0196	0.0935	-0.6315	0.0935
Never vs. timing	-0.0066	0.7388	-0.0047	0.7388	-0.0051	0.7388	-0.4028	0.7388
Always vs. never	0.0632	0.0030	0.0500	0.0030	0.0420	0.0030	-0.5260	0.0030
Within	-0.2227	0.0531	-0.1999	0.0531	-0.2146	0.0531	-8.8677	0.0531

Table A24: Goodman-Bacon (2021) Decomposition, CPS (2000-2018), 0-3 month old infants

	Labor Force Participation		Employed		Worked Last Week		Hours Worked (Unconditional)	
	Coef.	p-value	Coef.	p-value	Coef.	p-value	Coef.	p-value
Hospital Policy	-0.0414	0.0230	-0.0490	0.0210	-0.0387	0.0240	-1.3337	0.0550
Decomposition								
	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight	Beta	Total Weight
Timing groups	-0.0238	0.1121	-0.0149	0.1121	-0.0061	0.1121	-0.3438	0.1121
Always vs. timing	-0.0312	0.0930	-0.0387	0.0930	-0.0403	0.0930	-1.5196	0.0930
Never vs. timing	-0.0330	0.7287	-0.0393	0.7287	-0.0409	0.7287	-1.5635	0.7287
Always vs. never	-0.4119	0.0028	-0.4166	0.0028	-0.2164	0.0028	-4.4336	0.0028
Within	-0.1686	0.0634	-0.2207	0.0634	-0.0613	0.0634	-0.0342	0.0634

Table A25: Effects of Hospital Breastfeeding Support Policies on Paternal Work, CPS (2000-2018)

	(1) Labor force Participation	(2) Employed	(3) Worked last week	(4) Hours worked last week (unconditional)
Panel A: Overall effect for men with 0-3 or 3-12 month olds				
Hospital Policy	-0.00430	-0.0000840	0.000314	0.329
	(0.00326)	(0.00547)	(0.00646)	(0.363)
N	89,636	89,636	89,636	89,636
R-Squared	0.0385	0.0684	0.0503	0.0658
Mean of Dependent	0.952	0.909	0.878	37.71
Panel B: Decomposed by Age of Infant				
Hospital Policy	-0.000491	-0.00515	-0.0122	0.0479
x baby 0-3 mos	(0.00491)	(0.00668)	(0.00769)	(0.542)
Hospital Policy	-0.00592	0.00204	0.00562	0.442
x baby 3-12 mos	(0.00413)	(0.00621)	(0.00652)	(0.334)
N	89,636	89,636	89,636	89,636
R-Squared	0.0385	0.0685	0.0504	0.0659
Mean of Dependent	0.952	0.909	0.878	37.71

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample is the set of men observed in the CPS with an infant between 0 and 3 months or between 3 and 12 months of age. See notes to Table 7 for details on the specification and control variables. All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table A26: Effects of Hospital Breastfeeding Support Policies on Mothers without Infants, CPS (2000-2018)

	(1)	(2)	(3)	(4)
	Labor force Participation	Employed	Worked last week	Hours worked last week (unconditional)
Hospital Policy	0.00253	0.00190	0.00259	0.142
	(0.00278)	(0.00280)	(0.00262)	(0.133)
N	3,198,082	3,198,082	3,198,082	3,198,082
R-Squared	0.0678	0.0699	0.0616	0.0649
Mean of Dependent	0.738	0.697	0.667	24.25

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample is the set of women observed in the CPS with children in the household and whose youngest child is between 2 and 18 years old. The treatment variable is an indicator variable that is equal to one if a hospital policy was in effect by June of the survey year. See notes to Table 7 for details on the specification and control variables. All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table A27: Effects of Hospital Breastfeeding Support Policies on Maternal Work, CPS (2000-2018)

	(1) Labor Force Participation	(2) Employed	(3) Worked Last Week	(4) Hours Worked Last Week (unconditional)
Panel A: Baseline Sample				
Hospital Policy	-0.0132**	-0.0181***	-0.00790	-0.301
	(0.00514)	(0.00498)	(0.00586)	(0.301)
N	109,187	109,187	109,187	109,187
R-Squared	0.0976	0.106	0.122	0.120
Mean of Dependent	0.576	0.534	0.434	14.54
Panel B: Black mothers				
Hospital Policy	-0.00945	-0.0405**	-0.0432*	-1.488*
	(0.0179)	(0.0174)	(0.0217)	(0.850)
N	10,036	10,036	10,036	10,036
R-Squared	0.0920	0.111	0.131	0.145
Mean of Dependent	0.641	0.546	0.458	16.16
Panel C: Maternal education $\leq$ high school				
Hospital Policy	-0.00935	-0.0271*	-0.0312*	-1.038
	(0.0170)	(0.0157)	(0.0157)	(0.659)
N	39,235	39,235	39,235	39,235
R-Squared	0.0843	0.0748	0.0924	0.0905
Mean of Dependent	0.451	0.389	0.335	10.99
Panel D: Black mothers, education $\leq$ high school				
Hospital Policy	-0.0140	-0.0857**	-0.0889**	-2.903**
	(0.0304)	(0.0351)	(0.0354)	(1.433)
N	4,903	4,903	4,903	4,903
R-Squared	0.0916	0.0927	0.117	0.130
Mean of Dependent	0.555	0.439	0.375	12.59

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample differs for each panel, and is specified in the panel heading. All models include controls for individual characteristics, state, survey year, and survey month fixed effects, state policies, and state demographic characteristics (see text for details). All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table A28: Robustness of CPS Outcomes to Specification Choices, Black mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Labor Force Status									
Hospital Policy	-0.0306*	-0.0212	-0.0297	-0.00945	-0.0117	-0.0137	-0.0123	0.00164	-0.00431
	(0.0158)	(0.0147)	(0.0207)	(0.0179)	(0.0215)	(0.0187)	(0.0158)	(0.0179)	(0.0189)
Outcome: Employed									
Hospital Policy	-0.0436**	-0.0405**	-0.0533**	-0.0405**	-0.0589***	-0.0427**	-0.0284*	-0.0268*	-0.0361**
	(0.0181)	(0.0162)	(0.0202)	(0.0174)	(0.0216)	(0.0171)	(0.0151)	(0.0150)	(0.0177)
Outcome: Worked Last Week									
Hospital Policy	-0.0674***	-0.0510***	-0.0741***	-0.0432*	-0.0576**	-0.0443**	-0.0378**	-0.0310	-0.0385*
	(0.0152)	(0.0180)	(0.0193)	(0.0217)	(0.0229)	(0.0214)	(0.0183)	(0.0207)	(0.0223)
Outcome: Hours Worked Last Week (unconditional)									
Hospital Policy	-2.530***	-1.891**	-2.671***	-1.488*	-1.973**	-1.574*	-1.284*	-1.190	-1.225
	(0.579)	(0.737)	(0.751)	(0.850)	(0.889)	(0.838)	(0.730)	(0.801)	(0.867)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 1999 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents an estimate from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Panel B of Table A27. Standard errors are clustered at the state level.

Table A29: Robustness of CPS Outcomes to Specification Choices, Mothers with No College

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Labor Force Status									
Hospital Policy	-0.0232	-0.0134	-0.0252	-0.00935	-0.0125	-0.00948	-0.00671	-0.00269	-0.00914
	(0.0202)	(0.0187)	(0.0168)	(0.0170)	(0.0193)	(0.0167)	(0.0164)	(0.0159)	(0.0180)
Outcome: Employed									
Hospital Policy	-0.0296	-0.0231	-0.0398**	-0.0271*	-0.0361*	-0.0259	-0.0168	-0.0202	-0.0273
	(0.0221)	(0.0195)	(0.0158)	(0.0157)	(0.0193)	(0.0157)	(0.0161)	(0.0153)	(0.0163)
Outcome: Worked Last Week									
Hospital Policy	-0.0467**	-0.0285	-0.0592***	-0.0312*	-0.0382**	-0.0289*	-0.0214	-0.0228	-0.0313*
	(0.0201)	(0.0180)	(0.0168)	(0.0157)	(0.0186)	(0.0150)	(0.0151)	(0.0149)	(0.0160)
Outcome: Hours Worked Last Week (unconditional)									
Hospital Policy	-1.604**	-0.941	-2.042***	-1.038	-1.314*	-0.963	-0.764	-0.663	-1.014
	(0.741)	(0.655)	(0.696)	(0.659)	(0.778)	(0.644)	(0.612)	(0.628)	(0.675)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 1999 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents an estimate from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Panel C of Table A27. Standard errors are clustered at the state level.

Table A30: Robustness of CPS Outcomes to Specification Choices, Black Mothers with No College

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome: Labor Force Status									
Hospital Policy	-0.0532	-0.0200	-0.0583*	-0.0140	-0.0121	-0.0222	-0.0106	0.00317	-0.00981
	(0.0352)	(0.0350)	(0.0310)	(0.0304)	(0.0315)	(0.0275)	(0.0268)	(0.0305)	(0.0308)
Outcome: Employed									
Hospital Policy	-0.102***	-0.0770**	-0.118***	-0.0857**	-0.108***	-0.0908**	-0.0664**	-0.0649*	-0.0842**
	(0.0353)	(0.0349)	(0.0333)	(0.0351)	(0.0385)	(0.0355)	(0.0306)	(0.0334)	(0.0362)
Outcome: Worked Last Week									
Hospital Policy	-0.121***	-0.0836**	-0.138***	-0.0889**	-0.113***	-0.0946***	-0.0812**	-0.0697*	-0.0887**
	(0.0317)	(0.0322)	(0.0307)	(0.0354)	(0.0394)	(0.0346)	(0.0316)	(0.0357)	(0.0376)
Outcome: Hours Worked Last Week (unconditional)									
Hospital Policy	-4.272***	-2.826**	-4.754***	-2.903**	-3.870**	-3.127**	-2.683*	-2.539*	-2.680*
	(1.141)	(1.226)	(1.176)	(1.433)	(1.517)	(1.431)	(1.432)	(1.402)	(1.540)
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month and survey year FEs?	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Individual characteristics?		Yes		Yes	Yes	Yes	Yes	Yes	Yes
State/time varying Xs?			Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionXyear fixed effects?					Yes				
Birth year and birth month FEs?						Yes			
Weighted?	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Including 1999 and 2018 partial birth cohorts?								Yes	
Dropping always treated states?									Yes

* p <0.10, ** p <0.05, *** p <0.01

Note: Each column of each panel represents an estimate from a separate regression for the outcome variable listed in each panel header. The estimates in column (4) are the main treatment estimates reported in Panel C of Table A27. Standard errors are clustered at the state level.

Table A31: Effects of Hospital Breastfeeding Support Policies on Paternal Work, CPS (2000-2018)

	(1) Labor Force Participation	(2) Employed	(3) Worked Last Week	(4) Hours Worked Last Week (unconditional)
Panel A: Baseline Sample				
Hospital Policy	-0.00430	-0.0000840	0.000314	0.329
	(0.00326)	(0.00547)	(0.00646)	(0.363)
N	89,636	89,636	89,636	89,636
R-Squared	0.0385	0.0684	0.0503	0.0658
Mean of Dependent	0.952	0.909	0.878	37.71
Panel B: Black fathers				
Hospital Policy	0.0107	-0.000619	-0.0187	-1.535
	(0.0245)	(0.0238)	(0.0274)	(1.205)
N	5,891	5,891	5,891	5,891
R-Squared	0.113	0.142	0.119	0.130
Mean of Dependent	0.902	0.815	0.785	32.44
Panel C: Paternal education $\leq$ high school				
Hospital Policy	-0.00288	0.000895	-0.0115	-0.158
	(0.00508)	(0.0110)	(0.0110)	(0.691)
N	33566	33,566	33,566	33,566
R-Squared	0.0511	0.0753	0.0660	0.0799
Mean of Dependent	0.931	0.861	0.837	34.71
Panel D: Black fathers, education $\leq$ high school				
Hospital Policy	0.0475	0.0301	-0.00622	-2.857
	(0.0397)	(0.0327)	(0.0410)	(1.805)
N	2,660	2,660	2,660	2,660
R-Squared	0.159	0.163	0.144	0.153
Mean of Dependent	0.861	0.733	0.708	28.10

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample differs for each panel, and is specified in the panel heading. All models include controls for individual characteristics, state, survey year, and survey month fixed effects, state policies, and state demographic characteristics (see text for details). All models are weighted by CPS sample weights and standard errors are clustered at the state level.

Table A32: Effects of Hospital Breastfeeding Support Policies on Mothers without Infants, CPS (2000-2018)

	(1)	(2)	(3)	(4)
	Labor Force Participation	Employed	Worked Last Week	Hours Worked Last Week (unconditional)
Panel A: Baseline Sample of Mothers without Infants				
Hospital Policy	0.00253	0.00190	0.00259	0.142
	(0.00278)	(0.00280)	(0.00262)	(0.133)
N	3,198,082	3,198,082	3,198,082	3,198,082
R-Squared	0.0678	0.0699	0.0616	0.0649
Mean of Dependent	0.738	0.697	0.667	24.25
Panel B: Black Mothers without Infants				
Hospital Policy	0.00349	0.00875*	0.00926*	0.328
	(0.00502)	(0.00500)	(0.00539)	(0.227)
N	349,736	349,736	349,736	349,736
R-Squared	0.0607	0.0800	0.0709	0.0832
Mean of Dependent	0.792	0.716	0.690	26.17
Panel C: Mothers without Infants, education ≤ high school				
Hospital Policy	0.00525	0.00477	0.00472	0.143
	(0.00408)	(0.00381)	(0.00350)	(0.146)
N	1,238,081	1,238,081	1,238,081	1,238,081
R-Squared	0.0686	0.0678	0.0623	0.0638
Mean of Dependent	0.667	0.611	0.590	21.18
Panel D: Black Mothers without Infants, education ≤ high school				
Hospital Policy	0.0155	0.0149	0.0129	0.245
	(0.0101)	(0.00923)	(0.00896)	(0.394)
N	157,463	157,463	157,463	157,463
R-Squared	0.0485	0.0612	0.0573	0.0662
Mean of Dependent	0.722	0.623	0.602	22.17

* p <0.10, ** p <0.05, *** p <0.01

Note: The outcome variable for each regression is described in the column header. The sample differs for each panel, and is specified in the panel heading. All models include controls for individual characteristics, state, survey year, and survey month fixed effects, state policies, and state demographic characteristics (see text for details). All models are weighted by CPS sample weights and standard errors are clustered at the state level.

B Supplemental Analysis using the SIPP

B.1 SIPP Data Description and Methods

We also use data from the Survey of Income and Program Participation (SIPP), 2000-2013, to further examine the impacts of breastfeeding support policies on maternal employment outcomes. The SIPP is a series of nationally representative panel surveys; each panel includes between 14,000 and 52,000 participating households. Households are part of the SIPP panel for approximately four years and are interviewed three times per year during that period, with each interview covering outcomes in the four preceding months. For our analyses, we use the 2001, 2004, and 2008 panels of the SIPP; thus, individuals in our sample were surveyed between late 2000 and the end of 2013. We end our sample in 2013 due to a major redesign implemented at the start of the 2014 panel which caused outcomes to no longer be comparable across panels.

The employment outcomes we examine in the SIPP are monthly-level measures of any labor force participation, defined as an indicator variable equal to one if the mother either worked or looked for work at any point during the month, and is zero otherwise; a continuous measure of total hours worked; and an indicator variable for working any positive hours during the month. We note that there are additional labor and employment variables in the SIPP, however, they either are reported as four month averages, thus limiting our ability to examine them precisely relative to birth timing, or they are not consistently available across our full sample period.

To construct our analytic data set, we focus on the sample of women who gave birth during their participation in the SIPP panel. The SIPP provides information on infant's month and year of birth, as well as mother's state of residence each month, allowing us to more precisely assign treatment exposure relative to our analyses using the CPS. Additionally, for each monthly-level employment observation, we are able to precisely assign the number of months the observation is relative to the timing of the focal birth. In order to cleanly focus on employment dynamics around a given birth, we drop all mother-birth observations for which the mother had another birth in the 12 months prior to or in the 12 months after the focal birth. Finally, to make sure that changes in the sample composition are not driving any observed employment dynamics around birth, we also restrict our sample to mothers that we observe for at least 6 months before and 6 months after birth. Our final data set is at the mother-birth-month level and includes 7,452 mother-birth pairs.

For our analyses using the SIPP we implement a triple-difference strategy, which allows us to leverage the fact that we observe monthly-level employment outcomes in the months before *and* after giving birth. For this model, we expand our baseline difference-in-differences specification by additionally comparing women's outcomes after birth to their *own* outcomes prior to birth. Specifically, we estimate the following event study equation:

$$
\begin{aligned}
Y_{ijsmy} = \beta_0 &+ \sum_{j \in J} \beta_1^j \left(\textit{Months Since Birth}_i^j \times \textit{HospitalPolicy}_i \right) \\
&+ \sum_{j \in J} \beta_2^j \left(\textit{Months Since Birth}_i^j \times \textit{Ever Hospital Policy}_i \right) \\
&+ \sum_{j \in J} \beta_3^j \left(\textit{Months Since Birth}_i^j \times \gamma_y \right) \\
&+ \beta_4 \left(\textit{Ever Hospital Policy}_i \times \gamma_y \right) \\
&+ \sum_{j \in J} \beta_5^j (\textit{Months Since Birth}_i^j) \\
&+ X_{imy} + Z_{sy} + \mu_m + \gamma_y + \delta_i + \varepsilon_{ijsmy}
\end{aligned} \tag{3}
$$

where Y_{ijsmy} is the outcome of interest in month m of year y for mother-birth pair i residing in state s, for which the birth occurred j months ago. Since employment outcomes are measured monthly in the SIPP, we include calendar month (μ_m) and year of survey (γ_y) fixed effects to control for seasonality and common employment shocks across states. δ_i represents a vector of mother-birth fixed effects, which flexibly control for time-invariant characteristics of a given mother-birth pair (e.g. infant birth order, maternal labor market experience at time of birth, state of residence at time of birth). $\textit{HospitalPolicy}_i$ is an indicator variable equal to one if a hospital breastfeeding support regulation was in effect in the state of residence at the time of birth for mother-infant i, and is zero otherwise.[59]

Unlike in the event study specification in equation 2, which traces out how the impacts

[59] In our main analyses using the CPS we consider a mother exposed to the policy if it was in effect by June of the infant's birth year, since we do not observe precise month of birth in those data. Estimates using SIPP data and this alternate treatment definition are very similar and are available upon request.

of the policy change over time relative to policy *adoption*, for these specifications we are interested in tracing out how the impacts of the policy change relative to the timing of *birth*. Therefore, we include in our specification the vector $MonthsSinceBirth_i^j$, which is a series of indicator variables equal to one for a mother-infant observation i in the binned j months from birth, $j \in J = \{\leq -13, -(10-12)..., -(4-6), (0-2), (3-5), ..., (9-11), \geq 12\}$, and is zero otherwise (the 1-3 months prior to birth are the omitted category), that flexibly controls for changes in maternal labor outcomes as infants age. The interaction between $MonthsSinceBirth_i^j$ and $HospitalPolicy_i$ allows for maternal employment outcomes to evolve differently for mother-births that are exposed to hospital breastfeeding support policies, relative to unexposed mothers. Thus, β_1^j represents our vector of coefficients of interest and captures the dynamic effects of the hospital postpartum care regulations. Notably, since our policy of interest consists of an intervention that occurs during the postpartum hospital stay, we should not expect policy adoption to impact maternal employment outcomes during the months *prior* to birth, and so we view the coefficients on β_1^j for $j = \{\leq -13, -(10-12)..., -(4-6)\}$ as falsification tests.

We also include in our specification the following two-way interactions: the interaction between $MonthsSinceBirth_i^j$ and an indicator variable for residing in a state that ever adopts a breastfeeding support policy ($EverHospitalPolicy_i$), to allow for baseline differences in employment dynamics for adopting and non-adopting states; between $MonthsSinceBirth_i^j$ and year fixed effects (γ_y) to allow for common national-level changes in employment dynamics over time; and between $EverHospitalPolicy_i$ and year fixed effects (γ_y) to allow for differential employment shocks that equally impact pre- and post-birth employment in adopting versus non-adopting states. X_{imy} is a vector of the following time-varying mother characteristics, as measured at the time of survey: age, age squared, education, and marital status. As before, Z_{sy} is a vector of other state policies in effect in the current state of residence, as well as state demographic and economic characteristics which may potentially affect maternal employment and breastfeeding. We use individual sample weights as provided by SIPP and cluster standard errors at the mother-birth level (Bertrand et al., 2004).

B.2 SIPP Analysis Results

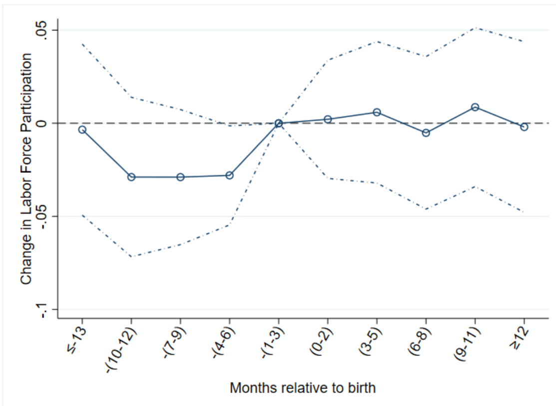
The triple-difference estimates capturing the dynamic effect of hospital breastfeeding support policies on maternal employment outcomes are graphically presented in Figure B1. We also estimate regressions in which the vector of indicator variables capturing months relative to birth is replaced with a single *PostBirth* indicator variable; we report the single triple difference coefficient on the interaction between *PostBirth* and *HospitalPolicy* in Table B1.[60] For this set of analyses, in which we use the full sample of mothers and examine employment outcomes over the 12 months prior to and following birth, the estimates are not inconsistent with the policies reducing total hours worked in a month and the probability of working any positive hours in a month; for our measure of labor force participation, there is some evidence of a differential trend in the months prior to birth (Figure B1, Panel A).

We next re-estimate our event study model for the high impact sub-samples of mothers. These results are graphically presented in Figures B2, B3, and B4, for Black mothers, mothers whose highest level of education is a high school degree or less, and for Black mothers with a high school degree or less, respectively. Consistent with our findings using the CPS, these results show that for Black mothers the adoption of hospital breastfeeding policies caused a sharp reduction in the total hours worked and the probability of working any positive hours during the months immediately following birth. We also observe reductions along these dimensions for the sub-group of Black mothers with no college education (Figure B4 and Table B1, Panel D). There is little evidence of significant changes for the lower education sub-group: the point estimates are actually *positive*, although none are statistically significant (Table B1, Panel C). Overall, however, we find that these results support our core finding: state breastfeeding support policies reduce maternal employment in the short-run, particularly for Black mothers.

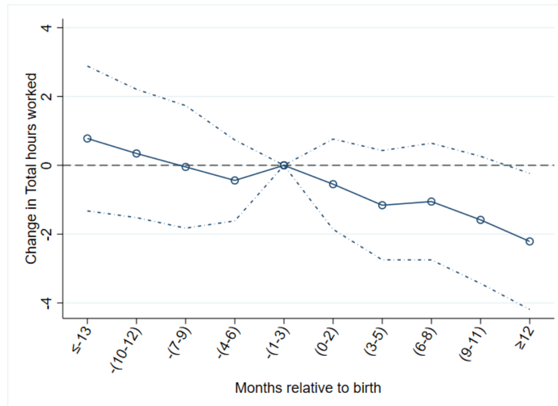
[60] We also estimate a version of the triple difference where we focus on employment in the first three months after birth, compared with pre-birth employment, in order to be comparable to our CPS sub-analyses examining dynamics for this sub-group of mothers. These results are similar and are presented in Appendix Table B2.

Figure B1: **Dynamic Triple Difference Estimates of the Effect of Hospital Breastfeeding Support Policies on Maternal Employment, SIPP (2000-2013)**

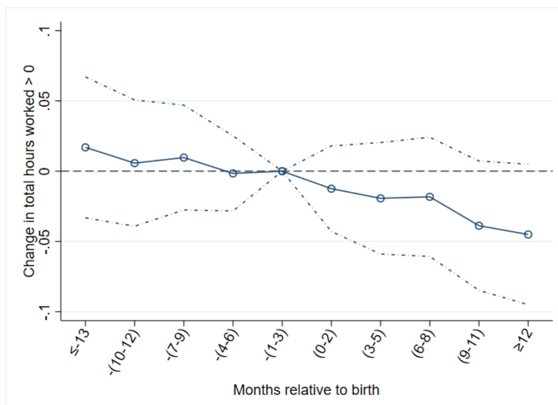
(a) Any Labor Force Participation



(b) Total Hours Worked



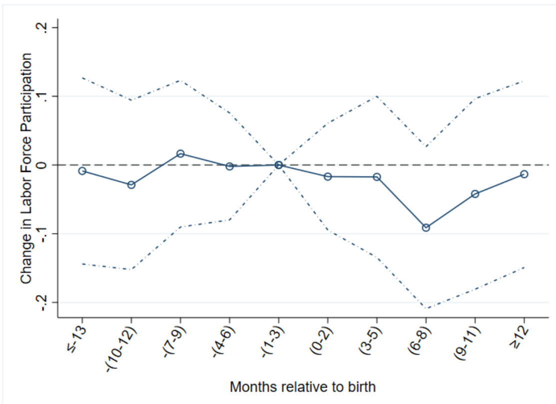
(c) Any Positive Hours Worked



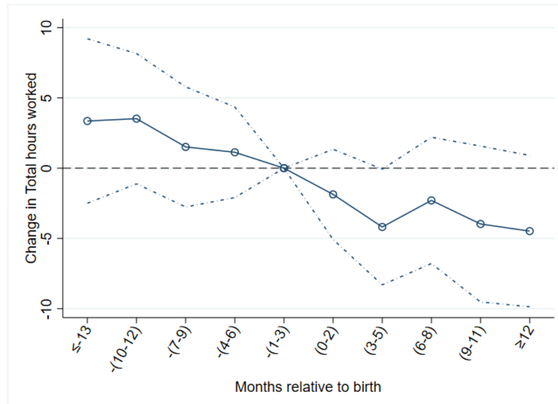
Note: Each figure presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each panel label. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy and the vector of indicator variables capturing months relative to birth. Regressions include calendar year and month fixed effects; fixed effects for months relative to birth; mother-birth pair fixed effects; all two-way interactions between calendar year, months relative to birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level. The x-axis measures months relative to birth; coefficients are relative to the excluded period of 1-3 months prior to birth. Dashed lines represent the 95% confidence intervals.

Figure B2: **Dynamic Triple Difference Estimates of the Effect of Hospital Breastfeeding Support Policies on Employment of Black Mothers, SIPP (2000-2013)**

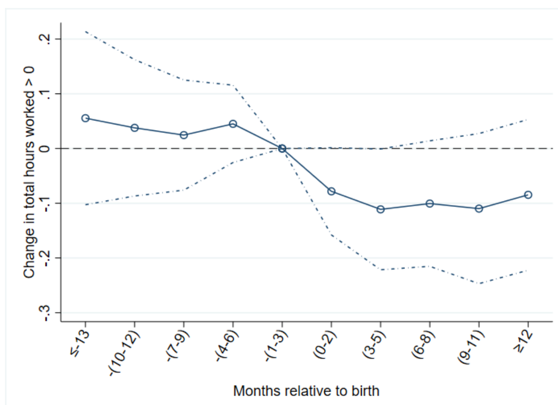
(a) Any Labor Force Participation



(b) Total Hours Worked



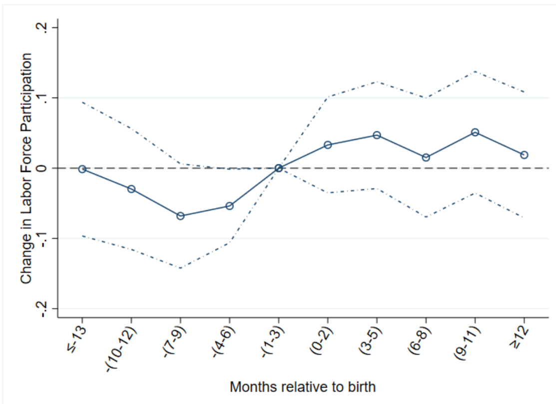
(c) Any Positive Hours Worked



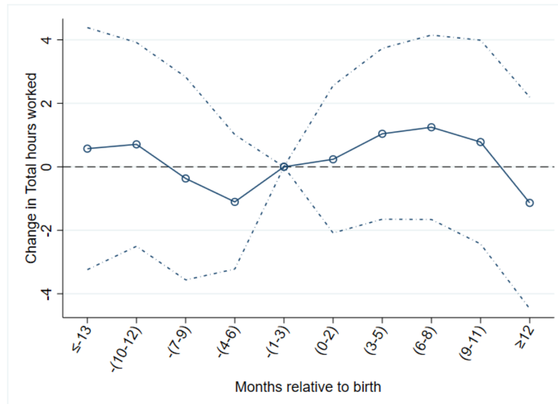
Note: Each figure presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each panel label. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy and the vector of indicator variables capturing months relative to birth. Regressions include calendar year and month fixed effects; fixed effects for months relative to birth; mother-birth pair fixed effects; all two-way interactions between calendar year, months relative to birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level. The x-axis measures months relative to birth; coefficients are relative to the excluded period of 1-3 months prior to birth. Dashed lines represent the 95% confidence intervals.

Figure B3: **Dynamic Triple Difference Estimates of the Effect of Hospital Breastfeeding Support Policies on Employment of Mothers with No College, SIPP (2000-2013)**

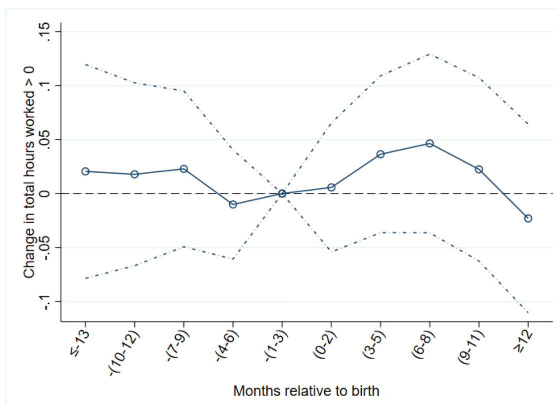
(a) Any Labor Force Participation



(b) Total Hours Worked



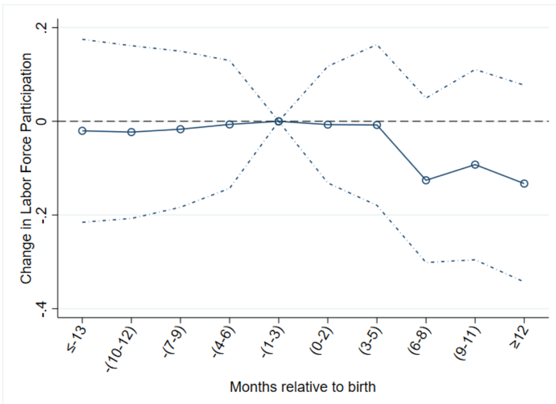
(c) Any Positive Hours Worked



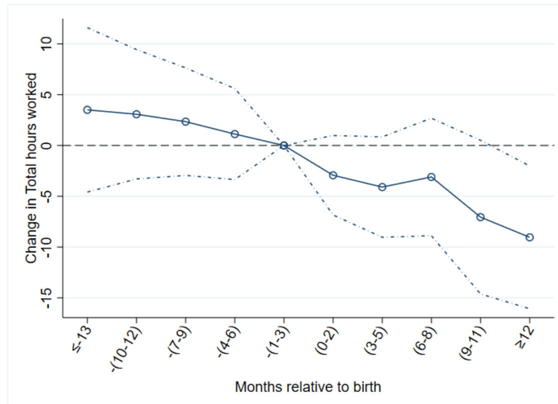
Note: Each figure presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each panel label. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy and the vector of indicator variables capturing months relative to birth. Regressions include calendar year and month fixed effects; fixed effects for months relative to birth; mother-birth pair fixed effects; all two-way interactions between calendar year, months relative to birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level. The x-axis measures months relative to birth; coefficients are relative to the excluded period of 1-3 months prior to birth. Dashed lines represent the 95% confidence intervals.

Figure B4: **Dynamic Triple Difference Estimates of the Effect of Hospital Breastfeeding Support Policies on Employment of Black Mothers with No College, SIPP (2000-2013)**

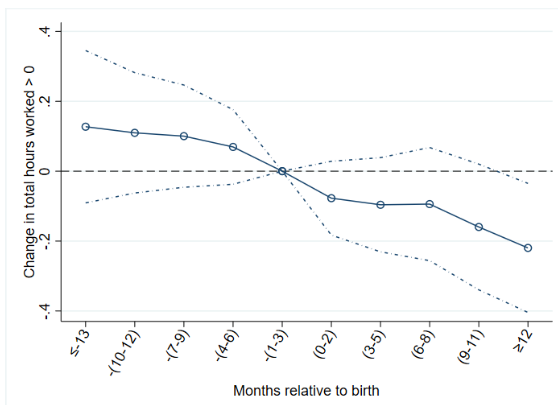
(a) Any Labor Force Participation



(b) Total Hours Worked



(c) Any Positive Hours Worked



Note: Each figure presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each panel label. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy and the vector of indicator variables capturing months relative to birth. Regressions include calendar year and month fixed effects; fixed effects for months relative to birth; mother-birth pair fixed effects; all two-way interactions between calendar year, months relative to birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level. The x-axis measures months relative to birth; coefficients are relative to the excluded period of 1-3 months prior to birth. Dashed lines represent the 95% confidence intervals.

Table B1: Triple Difference Estimates of Effects of Breastfeeding Support Laws for Mothers with 0-11 mo. Baby vs. Before Baby's Birth, SIPP (2000-2013)

	(1) Labor Force Participation	(2) Total hours worked	(3) Any Positive Hours Worked
Panel A: Baseline Sample			
Post Birth x Hospital Policy	0.0197	-1.018	-0.0237
	(0.0174)	(0.743)	(0.0180)
N	195,006	195,006	195,006
R-Squared	0.66	0.67	0.67
Mean of Dependent	0.65	20.76	0.56
Panel B: Black mothers			
Post Birth x Hospital Policy	-0.0406	-4.676**	-0.128**
	(0.0467)	(1.984)	(0.0498)
N	23,996	23,996	23,996
R-Squared	0.58	0.61	0.62
Mean of Dependent	0.65	20.66	0.56
Panel C: Maternal education ≤ to high school			
Post Birth x Hospital Policy	0.0544	0.904	0.0185
	(0.0348)	(1.298)	(0.0340)
N	71,585	71,585	71,585
R-Squared	0.59	0.62	0.63
Mean of Dependent	0.49	14.26	0.40
Panel D: Black mothers, education ≤ high school			
Post Birth x Hospital Policy	-0.0531	-6.348***	-0.174***
	(0.0630)	(2.035)	(0.0599)
N	12,111	12,111	12,111
R-Squared	0.52	0.60	0.59
Mean of Dependent	0.51	14.01	0.40

* p <0.10, ** p <0.05, *** p <0.01

Note: Each cell presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each column label. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy at the time of birth, and an indicator variable capturing if the observation occurs after birth. Regressions include calendar year and month fixed effects; fixed effects for post-birth; mother-birth pair fixed effects; all two-way interactions between calendar year, post-birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level.

Table B2: Triple Difference Estimates of Effects of Breastfeeding Support Laws for Mothers with 0-3 mo. baby vs. before baby's birth, SIPP (2000-2013)

	(1) Labor Force Participation	(2) Total hours worked	(3) Any Positive Hours Worked
Panel A: Baseline Sample			
Post Birth x Hospital Policy	0.0233	-0.628	-0.0202
	(0.0189)	(0.827)	(0.0192)
N	144,844	144,844	144,844
R-Squared	0.70	0.71	0.71
Mean of Dependent	0.66	21.68	0.58
Panel B: Black mothers			
Post Birth x Hospital Policy	-0.0231	-3.540*	-0.119**
	(0.0478)	(2.122)	(0.0517)
N	17,747	17,747	17,747
R-Squared	0.62	0.66	0.66
Mean of Dependent	0.66	21.17	0.58
Panel C: Maternal education $\leq$ to high school			
Post Birth x Hospital Policy	0.0470	-0.0440	-0.0070
	(0.0382)	(1.646)	(0.0389)
N	53,261	53,261	53,261
R-Squared	0.64	0.67	0.68
Mean of Dependent	0.50	14.79	0.42
Panel D: Black mothers, education $\leq$ high school			
Post Birth x Hospital Policy	-0.0383	-6.182***	-0.177***
	(0.0722)	(2.319)	(0.0647)
N	9,065	9,065	9,065
R-Squared	0.58	0.66	0.66
Mean of Dependent	0.52	14.29	0.42

* p <0.10, ** p <0.05, *** p <0.01

Note: Each cell presents the estimates from a separate triple difference regression, in which the outcome variable is measured at the monthly level and is as specified in each column label. The sample is limited to pre-birth observations and observations up to 3 months after birth. The reported estimates are the coefficients on the interactions between an indicator variable capturing if the focal birth took place in a state with an effective hospital breastfeeding support policy at the time of birth, and an indicator variable capturing if the observation occurs after birth. Regressions include calendar year and month fixed effects; fixed effects for post-birth; mother-birth pair fixed effects; all two-way interactions between calendar year, post-birth, and an indicator variable for being in an ever adopting state; and the vector of time varying individual and state characteristics (see text). All regressions are weighted by SIPP sample weights; standard errors are clustered at the mother-birth level.