Workplace Breastfeeding Legislation and Labor Market Outcomes

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Abstract

This paper studies the effects of legislation mandating workplace breastfeeding amenities on various labor market outcomes. Using the Panel Study of Income Dynamics, I implement both a traditional fixed-effects event study framework and the Interaction-Weighted event study technique proposed by Sun and Abraham (2020). I find that workplace breastfeeding legislation increases the likelihood of female labor force participation by 4.2 percentage points in the two years directly following implementation. Female labor force participation remains higher than before implementation in subsequent years, but the significant effect does not persist. Using the CDC’s Infant Feeding Practices Survey II, I then show that breastfeeding women who do not live in states with workplace breastfeeding supportive legislation are 3.9 percentage points less likely to work, supporting the PSID findings. The legislation mainly impacts white women. I find little effect on labor income or work intensity.

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Data Availability: As per the new conditions of use, data must be obtained through the Panel Study of Income Dynamics public use dataset, produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI. If necessary and possible, the author will create a Data Repository with the specific data extract posted for public use on OPENICPSR’s PSID Repository. The Infant Feeding Practices Survey II data is only available by request from the CDC. All code used for analysis is available from the author.

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1 Introduction

Since Becker’s seminal work in the 1960’s, economists and sociologists have been intensely studying the conflict between family and labor market obligations (Becker and Becker, 2009). In more recent years, researchers have turned their attention to the effectiveness of so-called “family-friendly” workplaces and legislation in reducing this conflict. Olivetti and Petrongolo (2017) suggest that policies making it easier to be a working mother may be more effective than policies extending parental leave rights in improving female labor market outcomes. While the United States lags far behind other developed nations in terms of parental leave and part-time work entitlement legislation (Blau and Kahn, 2013; Olivetti and Petrongolo, 2016), there are other ways in which current policy has potentially impacted the family-friendliness of workplaces. One such policy is state legislation requiring a workplace to provide breastfeeding amenities. This paper therefore mainly studies the effect of legislation requiring workplace breastfeeding amenities on female labor force participation - the requirement of such amenities represents an exogenous shock which increases the family-friendliness of a firm.

The acts of working and of breastfeeding are in direct competition for a mother’s time (Roe et al., 1999). For women who report breastfeeding in the CDC’s Infant Feeding Practices Survey II, the mean number of hours per week spent breastfeeding is 12.01 (standard deviation 8.42), and 16.45 (8.98) hours per week in the first 3 months after childbirth. There is therefore an opportunity cost of breastfeeding, measurable not only in terms of reduced income from work absences, but in reduction in the rate of acquiring human capital. The female worker’s decision process also impacts her employer, who loses productivity when she postpones returning to work in order to extend breastfeeding durations. The impact of maternal employment on breastfeeding initiation and duration has been examined at length in the medical literature (Kurinij et al., 1989; Fein and Roe, 1998; Abdulwadud and Snow, 2007). Women who return to full-time employment three months after the birth of a child breastfeed for a shorter durations (Lubold, 2016), and returning to work less than three months after the birth of a child is negatively associated with both breastfeeding duration and

\[1\] In this paper, I use “workplace breastfeeding” to mean any act of expressing breast milk while at the place of employment, whether that be physical breastfeeding of the child on the premises or pumping of milk for storage and later use.
initiation (Chatterji and Frick, 2005). The converse of these findings, i.e. the impact of breastfeeding on labor market outcomes for women, is less frequently studied.

Presumably, prior work has been undertaken with the policy goal of increasing breastfeeding rates and durations, not in order to understand how recent increases in breastfeeding rates might be impacting female employment decisions. This paper therefore attempts to fill this gap in the literature: as the “motherhood penalty” persists in the labor market (Adda et al., 2017), it is imperative for economists to understand whether workplace breastfeeding has a positive or negative impact on female labor force participation. Although modern policies which promote breastfeeding may significantly and negatively impact female labor supply, it is possible that employer-provided breastfeeding amenities may lessen the burden on working women, mitigating these effects. Mandal et al. (2014) suggest that “supportive workplaces” are related to higher breastfeeding intensity, for example. Del Bono and Pronzato (2012) find that availability of workplace breastfeeding facilities in the UK increases the probability of a mother returning to work by 4 and 6 months after birth, in addition to increasing the probability of breastfeeding in highly educated mothers. Hatsor and Shurtz (2019) exploit a safety recall of baby formula in Israel as a natural experiment to show that breastfeeding reduces the number of months in which a new mother works. This paper contributes to the above literature by examining how workplace breastfeeding policies impact female labor force participation in the United States.

Using the Panel Survey of Income Dynamics (PSID), the Childhood and Adoption Supplement to the PSID, and the Infant Feeding Practices Survey II, I estimate the impact of passage of workplace breastfeeding legislation on labor force participation. In both a traditional event study and an interaction-weighted framework, I find a significant and positive impact of the passing of the legislation on labor force participation of women. In the two year period after the law goes into effect, labor force participation for women in the PSID increases by 4.2 percentage points. The effect is even greater for women who have indicated that they breastfed at some point during the PSID sample period. Recalling that the treatment, implementation of a workplace breastfeeding law, is exogenous ex-ante to an individual woman’s decision to breastfeed (i.e., the state legislation being passed at a particular time is not driven by the individual’s breastfeeding decision), these results suggest that the law may causally
induce women to return to work and also to prolong breastfeeding durations. Because the PSID does not collect information on breastfeeding practices until 2017, I then study the Survey of Infant Feeding Practices II from the CDC, in which mothers report detailed information about their work and breastfeeding decisions in the year after giving birth. This allows me to explore the effects of the law on the group of women who do breastfeed. In this (non-causal) setting, I find that the workplace breastfeeding laws may reduce the negative impact of breastfeeding on returning to work. For women who eventually do return to work, the breastfeeding decision has no significant impact on the likelihood to work in any given three month period after giving birth – after the workplace law is taken into account. As family-friendly workplace policies may carry an implicit wage cost (Heywood et al., 2007), I also examine the effect of the workplace breastfeeding laws on female wages and labor income - and find no effect.

Because the decision to work and to breastfeed are endogenous to each other, most papers analyzing the effect of one on the other make use of simultaneous equation or bivariate probit models. This paper, however, is examining the impact of workplace policies (via state legislation), not the impact of the decision to breastfeed itself. The staggered timing of state-by-state implementation of the laws should alleviate endogeneity concerns conditional on controlling for possible confounding variables and other state policies\textsuperscript{2}. Event studies making use of such staggered treatment timing are commonly estimated using two-way fixed effect (FE) models which add leads and lags $l$ to the specifications. To address recently discovered shortcomings of traditional event study models (Goodman-Bacon, 2018), I implement the Interaction-Weighted (IW) technique of Sun and Abraham (2020) alongside two-way fixed effects analysis for all specifications. In both scenarios, there is a positive and significant, albeit temporary, effect of the legislation.

Conceptually, workplace breastfeeding legislation reduces the cost of breastfeeding. Mothers may be induced to breastfeed, thinking they will continue easily after returning to work - and only learning about the cost of breastfeeding when they actually do it (see, for example, Kuziemko et al. (2018)). This paper shows that the effect on female labor force participation is indeed positive but transitory. There is possible differential selection into breastfeeding based on the policy change. Importantly, this does

\textsuperscript{2}I describe other such state policies and report their timings in Section 2.
not mean that the findings of the paper do not demonstrate a combined causal impact of the policies on both breastfeeding and labor force participation, especially since the policy appears to impact the entire female labor force. The temporary effect of the policies may be due to incomplete alleviation of birth-related career interruptions. The realized personal costs of workplace breastfeeding may be greater than expected, or outweighed by other birth-related career impacts. Nonetheless, this paper is one of the first to examine the effects of breastfeeding legislation on labor market outcomes, and contributes to the literature on family-friendly policies, the child penalty, and the gender gap (Bertrand et al. 2010; Blau and Kahn 2017; Kleven et al. 2019).

2 Background

Before moving to the empirical analysis, here I briefly outline the evolution of breastfeeding practices and workplace policies in the United States. In section 2.2, I describe the legislation and other maternal workplace policies, and show that the presence of these other policies is not correlated with breastfeeding legislation or other state characteristics which may also impact labor force participation.

2.1 Breastfeeding in the United States

Breastfeeding rates have risen steadily in the United States during the past decades, following recommendation by the WHO that infants be exclusively breastfed until 6 months of age. Figure 1 shows that the rate of any breastfeeding at 6 months of age has risen steadily since 2000. In conjunction with this recommendation, the WHO and UNICEF launched the “Baby-Friendly Hospital Initiative” in 1991, which has been found to have a slight positive impact on maternal and infant health outcomes in the UK (Fallon et al. 2019). By 2012, Baby-Friendly hospitals accounted for 7% of births in the US (Pérez-Escamilla et al. 2016). It is unclear in the literature whether state workplace breastfeeding legislation arose as a result of WHO recommendations or Baby-Friendly hospital initiatives, and lack of data unfortunately precludes analysis of any effect of these initiatives on the state legislatures.

3 This initiative recommends hospitals provide breastfeeding training and support, and removes availability of free infant formula to new mothers.

4 See Murtagh and Moulton (2011)
discuss the legislation and other possible confounding effects related to the timing of its implementation by state.

Figure 1

![Labor Participation and Breastfeeding Rates, 2000-2017](image)

Data on female labor force participation taken from the BLS. Data on breastfeeding rates are from the CDC National Immunization Survey, which reports breastfeeding by year of infant birth. Any Breastfeeding indicates whether a child ever was fed breast milk. Breastfeeding at 6 months indicates whether a child was fed breast milk by the age of 6 months, both exclusively and in combination with other food sources.

### 2.2 Workplace Breastfeeding Legislation and Other Maternal Policies

Individual states began introducing legal statutes requiring or regulating various workplace breastfeeding amenities in 1998 (Table 1). As of this writing, there are 23 states with such statutes. The requirements range from requiring provision of break time for breastfeeding and pumping and requiring access to facilities and locations for such purposes, to requiring employees to be allowed to use their break times to express milk, to protecting female employees from job termination for engaging in lactation activities during breaks. Qualitative research has found that the majority of breastfeeding working mothers do not engage in physical breastfeeding during work hours,

\[^5\]I provide a list of statutes used in this paper in the Appendix.
instead primarily or always pumping milk at work [Felice et al., 2017] - however, the legislation often does not distinguish between the two practices. In some states, break time is explicitly provided only for pumping.

Figure 2 geographically depicts the presence of breastfeeding legislation by US state. It is clear these laws are not clustered in one area, and they are present in different political and cultural climates (California and Arkansas being arguably politically dissimilar, for example).

Figure 2: Breastfeeding Legislation States

There may be some concern that workplace breastfeeding legislation was enacted in tandem with other policies which could influence a mother’s labor force participation decisions. However, as shown in Table 1, there are only three states which provide paid maternity leave, 15 states with job protection for mothers taking leave after childbirth (whether paid or unpaid), and the timings of these policy implementations never coincide.\(^6\) Table 2 reports correlations between the overall presence of these policies, maternal labor force participation, and average childcare costs in 2013. Importantly, there is no evidence of a relationship between maternal labor force participation and the presence of workplace breastfeeding policies. Although there is mild correlation between the presence of the breastfeeding legislation and job protection following family leave, Table 1 indicates that the timing of these laws is unrelated. There is also no discernible pattern: some states enacted job protection before breastfeeding legislation, and some afterwards. Therefore, from an identification perspective, the timing

\(^6\)Information on other policies taken from Gault et al. (2014).
of paid leave or job protection laws should not interfere with any short-term impacts of the breastfeeding legislation.

3 Data

For the main analysis, I use the Panel Survey of Income Dynamics from 1997-2017. The survey has been conducted biennially since 1997. I first wish to examine the effect of breastfeeding legislation at the household level, collapsing individual observations into household units with separated variables by gender. I remove any households who have relocated states or who have experienced a change in family composition that results in the predominant female household member (head or spouse) leaving or being replaced. Removing households who have relocated addresses endogeneity concerns - if women are relocating states in order to gain access to workplace breastfeeding amenities after passage of the law, any results may be uninterpretable. Therefore, removing these women should address this potential issue. I exclude households without adult women present, as well as households who have not had a child younger than age 2 present during some point during the entire sample period. All monetary variables are reported in nominal terms and therefore I adjust them using the CPI for 1982, obtained from the Bureau of Labor Statistics. As the variable for labor force participation is coded in an extremely ambiguous manner\(^7\) I create a dummy variable for either gender indicating labor force participation if wages or labor income are reported to be \(>0\) in any two-year wave. After ensuring I have a panel mostly balanced in calendar time (see section 3), the final sample contains 1,511 households and 16,413 observations\(^8\).

I supplement the main dataset with the Childhood and Adoption History Supplement from the 2017 PSID (CAH). This cross-sectional supplement indicates, for each mother and child pair, whether breastfeeding was initiated. I merge these indicators with the main PSID sample by individual in order to analyze the effect of the workplace laws only on women who have indicated that they breastfed a child at some point within the sample period. This allows for interpretation of effect of the law on

\(^7\) The same number is used to indicate whether someone is currently working or whether they have never worked

\(^8\) NOTE: results are robust to relaxing the restriction of being balanced in calendar time. With the resulting larger sample, the effect is still significant and the magnitude even greater (See Appendix Table A4).
Table 1: Maternity Policies by State

| State                | Workplace Breastfeeding | Paid Leave | Job Protection | Maternal LFP | Childcare Costs |
|----------------------|-------------------------|------------|----------------|--------------|----------------|
| Alabama              | No                      | No         | No             | 70.1%        | $5,547         |
| Alaska               | No                      | No         | No             | 68.3%        | $10,280        |
| Arizona              | No                      | No         | No             | 59.8%        | $9,166         |
| Arkansas             | Yes 2009                | No         | No             | 62.9%        | $5,933         |
| California           | Yes 2001 Yes 2002       | No         | No             | 62.1%        | $11,628        |
| Colorado             | Yes 2008                | No         | No             | 65.1%        | $13,143        |
| Connecticut          | Yes 2001 No Yes 2013    | No         | No             | 72.2%        | $13,241        |
| Delaware             | No                      | No         | No             | 70.8%        | $9,058         |
| District of Columbia | Yes 2007 No Yes 2013    | No         | No             | 67.5%        | $21,948        |
| Florida              | No                      | No         | No             | 67.8%        | $8,376         |
| Georgia              | Yes 1999                | No         | No             | 68.5%        | $7,025         |
| Hawaii               | Yes 2013 Yes 1994       | No         | No             | 65.9%        | $11,748        |
| Idaho                | No                      | No         | No             | 58.4%        | $6,483         |
| Illinois             | Yes 2001                | No         | No             | 60.8%        | $12,569        |
| Indiana Weak         | 2008 No                 | No         | No             | 67.6%        | $8,281         |
| Iowa                 | No                      | No         | No             | 77.0%        | $9,185         |
| Kansas               | No                      | No         | No             | 69.9%        | $10,787        |
| Kentucky             | No                      | No         | No             | 66.1%        | $6,194         |
| Louisiana Weak       | 2013 No                 | No         | No             | 68.6%        | $5,655         |
| Maine                | Yes 2009 Yes 2012       | No         | No             | 75.3%        | $9,360         |
| Maryland             | No                      | No         | No             | 72.5%        | $13,897        |
| Massachusetts        | No                      | No         | Yes 2009      | 72.5%        | $16,549        |
| Michigan             | No                      | No         | No             | 68.4%        | $9,724         |
| Minnesota Yes 1998   | No                      | No         | Yes 2014      | 74.8%        | $13,993        |
| Mississippi          | No                      | No         | No             | 73.4%        | $5,496         |
| Missouri             | No                      | No         | No             | 70.4%        | $8,736         |
| Montana              | Yes 2007 No Yes 1995    | No         | No             | 69.8%        | $8,858         |
| Nebraska             | No                      | No         | No             | 72.9%        | $9,100         |
| Nevada               | No                      | No         | No             | 65.9%        | $10,095        |
| New Hampshire        | No                      | No         | No             | 71.1%        | $11,901        |
| New Jersey Yes 2018  | Yes 2009 Yes 1989       | Yes         | No             | 69.3%        | $11,534        |
| New Mexico Yes 2007  | No                      | No         | No             | 62.2%        | $7,523         |
| New York Yes 2007    | No                      | No         | No             | 67.5%        | $14,508        |
| North Carolina       | No                      | No         | No             | 70.8%        | $9,107         |
| North Dakota         | No                      | No         | No             | 73.3%        | $7,871         |
| Ohio                 | No                      | No         | No             | 70.6%        | $7,771         |
| Oklahoma Yes 2006    | No                      | No         | No             | 63.9%        | $7,741         |
| Oregon Yes 2007      | No                      | No         | Yes 2012      | 66.0%        | $11,078        |
| Pennsylvania No      | No                      | No         | No             | 70.5%        | $10,470        |
| Rhode Island         | Yes 2003 Yes 2013 Yes 1987 | No         | No             | 71.7%        | $12,662        |
| South Carolina       | No                      | No         | No             | 68.7%        | $6,372         |
| South Dakota         | No                      | No         | No             | 80.3%        | $5,571         |
| Tennessee            | Yes 1999 No Yes 2005    | No         | No             | 67.7%        | $5,857         |
| Texas                | No                      | No         | No             | 61.9%        | $8,619         |
| Utah                 | No                      | No         | No             | 52.8%        | $8,052         |
| Vermont Yes 2008     | No                      | No         | Yes 2013      | 69.4%        | $10,103        |
| Virginia Yes 2014    | No                      | No         | No             | 69.2%        | $10,028        |
| Washington No        | No                      | No         | Yes 2010      | 63.6%        | $12,332        |
| West Virginia        | No                      | No         | No             | 61.3%        | $7,800         |
| Wisconsin            | No                      | No         | Yes 2011      | 76.6%        | $11,342        |
| Wyoming              | No                      | No         | No             | 65.3%        | $9,233         |

a This table reports implementation of workplace breastfeeding legislation as well as paid maternal leave policy and leave job protection.

b Paid Leave refers to legislation mandating partial or fully paid leave following the birth of a child. Job Protection legislation mandates the ability to return to work after family or parental leave is taken, whether paid or unpaid. Data on legislation taken from Gault et al (2014).

c Maternal LFP is percentage of mothers in the labor force in 2013. Childcare Costs is the average cost of childcare in 2013. Both are taken from the IWPR’s analysis of the American Community Survey Public Use microdata.

* New York will enact paid family leave in 2021.
Table 2: Maternity Policy Correlations in 2013

|                        | Breastfeeding Law | Paid Leave | Job Protection | Maternal LFP | Childcare Costs |
|------------------------|-------------------|------------|----------------|--------------|-----------------|
| Breastfeeding Law      | 1.00              |            |                |              |                 |
| Paid leave             | 0.29*             | 1.00       |                |              |                 |
| Job Protection         | 0.43**            | 0.37**     | 1.00           |              |                 |
| Maternal LFP           | -0.05             | -0.03      | 0.18           | 1.00         |                 |
| Childcare Costs        | 0.24              | 0.17       | 0.51***        | 0.13         | 1.00            |

* p < 0.05, ** p < 0.01, *** p < 0.001

a This table reports correlations between the presence of different maternity policies and state characteristics in 2013.
b Information on maternal policies, labor force participation, and childcare costs taken from the IWPR.

an “intention to breastfeed” sample. Not every individual mother in the entire PSID is included in the CAH supplement, and the sample size is significantly reduced when restricting the analysis to this group of households. Nevertheless, results are robust to this restriction.

In addition to the PSID and CAH, I analyze the effect of workplace breastfeeding legislation on mothers who participated in the Infant Feeding Practices Survey II (IFPS), which was conducted by the Food and Drug Administration (FDA) and Centers for Disease Control and Prevention (CDC) in 2005–2007. The IFPS was a longitudinal study following mother-infant pairs from the third trimester of pregnancy through the first year of the child’s life. Questions on the survey include detailed information about duration and intensity of breastfeeding, work hours and employment status, as well as a number of questions regarding ease of workplace breastfeeding and pumping. The CDC clearly notes that the IFPS is not taken from a representative sample of the population, which I confirm by comparing characteristics of interest for the IFPS and PSID samples used in this paper. Another drawback of the IFPS data is the fact that individual labor income is not reported, either in aggregate or by period, and only categorical information about household-level income is available.

As in previous literature examining post-birth labor market activity, the IFPS II sample is restricted to women who worked before giving birth in order to condition on pre-birth characteristics (Mandal et al., 2014). As problems with breastfeeding are a significant negative determinant of both work initiation and intensity (Mandal et al., 2014), I include this response as a control variable in all specifications (it should be noted, however, that 88% of women in the survey report experiencing problems with breastfeeding during the first two weeks after giving birth). Because maternity leave
characteristics have been found to influence both work and breastfeeding decisions (Guendelman et al., 2009; Baker and Milligan, 2008), I also control for the mother’s length of maternity leave and for whether the leave is fully paid, partially paid, or non-paid.

3.1 Descriptive Statistics

Table 1 shows comparison of descriptive statistics for each dataset, using the PSID wave from 2005 merged with the 2017 CAH supplement to identify households in which a woman ever breastfed. Women in the IFPS II survey are much less likely to work than women in the PSID (52.34% versus 78.79%), are younger (29.14 versus 41.44), wealthier (average household income of $51,475 versus 36,228), more likely to be married (76.63% versus 66.02%), and are predominantly white (85.45% versus 55.87%). In the PSID sample, women who ever breastfed have higher rates of labor force participation (80.21%) and work more hours (28.27) than those who do not (78.52% and 27.08). In contrast, women who breastfeed in the IFPS II sample work fewer hours (10.25 versus 12.63) and have no differences in their rates of labor force participation. Women who report ever breastfeeding have fewer years of education than those who breastfed in the PSID, which contrasts with the fact that breastfeeding rates are increasing in education (Dubois and Girard, 2003). This is likely due to the increased age of women in the PSID sample.

Table A2 (Appendix) reports descriptive statistics by PSID wave. Female labor force participation drops over the course of the sample period as women age out of the workforce. Labor income and wages rise, as those who remain employed are likely to earn more over time. Importantly, there is no major difference in the treated group before law implementation and all other control households.

3.2 Workplace Legislation

In three states, the statues only apply to public employees, and in two states only to public school employees. Unlike Singh (2018) and Hauck et al. (2020), I do not include these states in my analysis, since such limited provisions do not impact the entire labor market. I also exclude states in which the law only “encourages” firms to
Table 3: Summary Statistics, by Breastfeeding Status

|                        | PSID |                   |                   | IFPS II |                   |                   |
|------------------------|------|-------------------|-------------------|---------|-------------------|-------------------|
|                        | Ever Breastfed | Never Breastfed | All               | Ever Breastfed | Never Breastfed | All               |
| Household Incomec($)   | 31,826 | 37,084           | 36,228            | 52,267  | 46,455            | 51,475            |
|                        | (30,225) | (57,831)        | (54,337)          | (27,149) | (28,100)          | (27,346)          |
| Labor Incomed($)       | 11,074  | 11,814           | 11,693            |          |                   |                   |
|                        | (13,066) | (13,528)        | (13,454)          |          |                   |                   |
| Contribution to HH Income |        | -                | -                 | 28.25%  | 28.33%            | 28.26%            |
|                        |        |                  |                   | (26.56%)| (26.19%)          | (26.50%)          |
| Labor Force Participation | 80.21%  | 78.52%           | 78.79%            |          |                   |                   |
|                        | (39.88%)| (41.07%)         | (40.88%)          |          |                   |                   |
| Work Hours, weekly\(e\) | 28.27  | 27.08            | 27.27             | 10.25   | 12.63             | 10.58             |
|                        | (18.57) | (18.53)          | (18.53)           | (15.15) | (16.88)           | (15.41)           |
| Age                    | 41.03   | 41.52            | 41.44             | 29.15   | 29.11             | 29.14             |
|                        | (10.48) | (11.04)          | -10.94            | (5.43)  | (5.45)            | (5.43)            |
| Married                | 63.25%  | 66.55%           | 66.02%            | 77.69%  | 69.97%            | 76.63%            |
|                        | (48.25%)| (47.19%)         | (47.24%)          | (41.65%)| (45.91%)          | (42.32%)          |
| White                  | 54.24%  | 56.19%           | 55.87%            | 84.81%  | 89.47%            | 85.45%            |
|                        | (49.86%)| (49.62%)         | (49.66%)          | (35.90%)| (30.74%)          | (35.27%)          |
| Black                  | 38.34%  | 37.84%           | 37.92%            | 4.05%   | 4.95%             | 4.17%             |
|                        | (48.66%)| (48.51%)         | (48.52)           | (19.72%)| (21.73%)          | (20.01%)          |
| Years of Education     | 12.54   | 12.98            | 12.9              |          |                   |                   |
|                        | (2.48)  | (2.47)           | (2.48)            |          |                   |                   |
| Some College           |        |                  |                   | 78.17%  | 59.44%            | 75.62%            |
|                        |        |                  |                   | (41.32%)| (49.18%)          | (42.95%)          |
| N                      | 566    | 2,915            | 3,481             | 2,048   | 323               | 2,371             |

\(a\) This table reports mean values for summary statistics and standard errors for the main datasets used in the paper. The 2017 Childhood and Adoption Supplement was merged with the PSID by person ID and collapsed to household units identified in both samples containing at least one adult woman. The resulting PSID sample wave in 2005 is then compared to the first wave of the IFPSII 2005 sample.

\(b\) "Ever Breastfed" are those who report breastfeeding in the 2017 CAH supplement to the PSID. For the IFPSII, they are women who ever breastfed during the 12-month post-partum period (in 2005).

\(c\) Household income is reported in non-uniform buckets in the IFPSII. Median values were imputed and used to calculate the statistics.

\(d\) Individual labor income is not reported in the IFPSII. Rather, female income is reported as a percentage of contribution to total household income. Likewise, number of years of education is not reported in the IFPSII.

\(e\) Work hours are reported on an annual basis in the PSID, this is divided by 52 weeks to obtain a weekly average.
provide lactation support. By restricting my analysis to states in which workplace breastfeeding legislation requires provision of amenities or break-time for all workers, results can be robustly interpreted. For analysis in this paper I therefore include 18 states which introduced their own legislation sometime between 1995 and 2013. As the PSID is conducted biennially, I count a wave as having been treated by the law if the law passed in the year of the survey or the year prior. Although this results in a mild reduction of specificity, it helps account for the fact that employers may take time to implement changes required by the law during the months after it is passed (Hauck et al., 2020).

### 4 Methodology

In this section I describe the various empirical models used in the paper, including the Interaction-Weighted estimation technique. The main results are estimated as in 3.1 and 3.2. Section 3.3 describes difference-in-differences specifications that are not in an event-study framework, which I use for the IFPS II sample and some restricted PSID samples (due to their small sizes).

In an event study framework, regression coefficients are interpreted as some average treatment effect for all cohorts that have experienced a period of treatment. There is a growing literature examining the validity of these difference-in-difference estimates (of which the event study framework is a type) (Goodman-Bacon, 2018; Athey and Imbens, 2018; De Chaisemartin and d’Haultfoeuille, 2020; Callaway et al., 2018; Imai and Kim, 2020). Sun and Abraham (2020), among others, show that these weighted averages are in actuality not causally interpretable, since they do not necessarily identify convex averages of cohort specific average treatment effects. This is due to estimate weights which do not coincide with actual sample frequencies. Long-run treatment effects are potentially downweighted for cohorts with early treatment onset.

There are many new proposed methods to address this weighting issue (Goodman-Bacon, 2018). In this paper, I use the correction proposed by Sun and Abraham (2020) importantly, this method is designed specifically with time-varying treatment effects.

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9 New Jersey enacted legislation in 2018, and I exclude this state due to the PSID sample ending the year prior.

10 Borusyk and Jaravel (2017) further discuss the consequences of weighting problems in the estimation of average treatment effects in event studies.
in mind, for a staggered treatment timing setting. In contrast, the De Chaisemartin and d’Haultfoeuille (2020) method is more generally applied to two-way fixed effects settings without being specific to staggered treatment timing and the Callaway et al. (2018) method does not allow for time-varying controls, which are arguably important in this setting. My findings (and findings from a traditional two-way FE model) indicate that effects evolve over time and that the Sun and Abraham (2020) approach is most appropriate.

When a control group is present in an event study model, unit fixed effects do not cause an identification problem (via collinearity) because they “pin down” the year fixed effects (Borusyak and Jaravel, 2017). Therefore, in all specifications, I keep observations in never-treated states as a control group. As in Sun and Abraham (2020), I remove the relative period $l = -1$ from the specification to avoid multicollinearity among the relative period indicators $D_{i,t}^l$. Rather than trim the sample to contain observations within the estimation window, I instead require the sample to be balanced in calendar time for at least ten waves. Trimming the sample would change the composition of the treated cohorts (Sun and Abraham, 2020), which may be particularly undesirable in this setting as there are a large number of control observations.

### 4.1 Two-way Fixed Effects Estimation

I first estimate a traditional two-way fixed effects “event study” regression

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{g \in G} \mu_g \mathbb{1}\{t - E_i \in g\} + \epsilon_{i,t}$$ (1)

on a panel of $i = 1, ..., N$ units for calendar time periods $t = 0, 1, ..., T$. $E_i$ is the initial time of treatment for unit $i$, $Y_{i,t}$ is the outcome of interest for unit $i$ at time $t$, and $\alpha_i$ and $\lambda_t$ are the unit and time fixed effects.

### 4.2 Interaction-Weighted Estimation

The interaction-weighted estimation process is described below. For further details and proofs, I refer the reader to Sun and Abraham (2020). Step one of the interaction-
weighted estimation process is to use a two-way (time and individual) fixed effects estimation which interacts cohort indicators \(1\{E_i = e\}\) with relative period indicators \(D_{i,\ell}\):

\[
Y_{i,t} = \alpha_i + \lambda_t + \sum_e \sum_{\ell \neq 1} \delta_{e,\ell}(1\{E_i = e\} \cdot D_{i,\ell}) + \epsilon_{i,t}
\] (2)

Because there are never-treated cohorts, this estimation is performed on all individuals. Resulting estimates are the \(CATT_{e,\ell}\), cohort average treatment effects for each cohort in relative time period \(\ell\) to treatment.

The second step is to estimate weights by sample shares of each cohort in each period \(\ell\), i.e. estimating \(\Pr\{E_i = e \mid E_i \in (\ell, T - \ell)\}\).

Lastly, the estimates from the first and second steps are used to form the IW estimator:

\[
\hat{\nu}_g = \frac{1}{|g|} \sum_{\ell \in g} \sum_e \hat{\delta}_{e,\ell} \Pr\{E_i = e \mid E_i \in (\ell, T - \ell)\}
\] (3)

In other words, the estimator is formed by taking a weighted average of the \(CATT_{e,\ell}\) from step 1, \(\hat{\delta}_{e,\ell}\), using the weights estimated in step two, the shares of each cohort in the relevant period.

4.3 Difference-in-Differences Regressions

For analysis of the Infant Feeding Practices Survey II data, I estimate a difference-in-difference regression as in Mandal et al. (2014). Cross-section OLS is performed on subsets of the data at months 3 and 6 after birth. Mixed regression (individual random effects and time fixed effects) of the following form is estimated:

\[
Y_{i,t} = \beta_1 (WorkplaceLaw \ast Breastfeeds_{i,t}) + \beta_2 WorkplaceLaw + \\
\beta_3 \ast Breastfeeds_{i,t} + X_i + Z_{i,t} + \lambda_t + \epsilon_{i,t}
\] (4)

Where \(WorkplaceLaw\) is a dummy indicating residing in a state with a workplace breastfeeding requirement law, \(Breastfeeds\) is a dummy indicator for individual \(i\) breastfeeding her baby in time \(t\), \(X_i\) is a vector of time-invariant controls, \(Z_{i,t}\) is a
vector of time-varying controls, and $\lambda_t$ is time fixed-effects. Time fixed-effects are used in order to control for feeding requirements changes a baby experiences over time. Unit fixed effects cannot be used since by definition, the treatment effect remains constant for an individual over time in this sample.

Importantly, the IFPSII is not a representative sample of the population (see Section 2), and because of potential endogeneity between the treatments, the decision to breastfeed and the presence of the law, the estimates from Equation (4) likely cannot be interpreted causally. Rather, I include this analysis to explore the mechanisms behind the event study specifications in 3.1 and 3.2.

5 Results

5.1 Breastfeeding Legislation and Labor Force Participation

The main results are reported in Table 3, which compares two-way fixed effects (FE) estimates (Equation 1) to the interaction-weighted (IW) estimates (Equations 2-3). Column $\hat{\mu}$ contains FE results, column $\hat{\nu}$ contains IW results, and columns $\hat{\delta}_{i,L}$ report CATT results for each cohort $i$. In both specifications, there is a significant increase in the likelihood of female labor force participation. The robust IW estimates show that the probability of participation increases by 4.2% and remains slightly elevated for 5-6 years (until $\ell = 2$) compared to before implementation. The effect is not persistent, however, and participation decreases in years following the law.
Figure 3: Effect of Legislation on Female Labor Force Participation

(a) Panel A

(b) Panel B

This figure depicts results from Table 4 with 90% confidence intervals. **Panel A** shows two-way fixed effects estimates and **Panel B** the interaction-weighted event study estimates. Time $t = -1$ is normalized to zero to account for multicollinearity between relative and calendar time.

Table 4: Effect of Legislation on Female Labor Force Participation

| Wave $l$ relative to law | FE Estimates | IW Estimates | CATT $e,l$ Estimates$^b$ |
|--------------------------|--------------|--------------|--------------------------|
|                          | $\hat{\mu}$  | $\hat{\nu}$  | $\hat{\delta}_{1,L}$ | $\hat{\delta}_{2,L}$ | $\hat{\delta}_{3,L}$ | $\hat{\delta}_{4,L}$ | $\hat{\delta}_{5,L}$ |
| -3                       | -0.022       | -0.008       | -                       | -                       | 0.034                  | -0.013                  | -0.001                  |
|                          | (0.029)      | (0.030)      | (0.014)                 | (0.037)                 | (0.053)                | (0.053)                 | (0.053)                 |
| -2                       | -0.0588**c   | -0.061**     | -0.075                  | 0.016                   | -0.002                 | -0.103                 |
|                          | (0.026)      | (0.026)      | (0.035)                 | (0.012)                 | (0.049)                | (0.064)                |
| -1c                      | 0            | 0            | 0                       | 0                       | 0                       | 0                       | 0                       |
| 0                        | 0.0422**     | 0.0415**     | 0.060                   | 0.019                   | -0.884                 | 0.089                   | 0.039                   |
|                          | (0.021)      | (0.021)      | (0.030)                 | (0.019)                 | (0.055)                | (0.060)                | (0.060)                |
| 1                        | 0.029        | 0.031        | -0.011                  | 0.021                   | 0.113                  | 0.113                   | 0.001                   |
|                          | (0.024)      | (0.024)      | (0.032)                 | (0.019)                 | (0.066)                | (0.078)                | (0.078)                |
| 2                        | 0.005        | 0.019        | -0.066                  | -0.032                  | 0.105                  | 0.193                   | 0.064                   |
|                          | (0.024)      | (0.024)      | (0.045)                 | (0.019)                 | (0.065)                | (0.055)                | (0.055)                |
| 3                        | -0.010       | -0.004       | -0.018                  | -0.040                  | 0.086                  | 0.101                   | -0.015                  |
|                          | (0.024)      | (0.024)      | (0.033)                 | (0.020)                 | (0.058)                | (0.084)                |

$^a$ This table reports the results of the Two-Way Fixed Effects (FE) and Interaction-Weighted (IW) Event Study models (Equations 1-3). Data is a balanced sample of the PSID from 1997-2017 where all individuals are present for at least 9 continuous waves. Treatment is the state workplace breastfeeding law going into effect. 18 states are treated, the rest are controls. Controls include age, education, childcare costs, age of the youngest child. SEs are clustered by household. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

$^b$ Cohort Average Treatment effects are reported for each individual cohort. A cohort is defined as all states in which the law goes into effect during the same two-year period.

$^c$ Wave $t = -1$ is normalized to zero due to collinearity.

These results also demonstrate the usefulness of the IW framework: by applying the technique of [Sun and Abraham (2020)], I show a difference in coefficients between a correctly weighted estimator and the traditional event study (FE) result. Although
the effect of the legislation on labor force participation only remains significant in the two years directly after implementation, the IW estimates show a larger and more prolonged positive effect than the FE estimates. This is a clear example of the long-term FE estimates potentially being downweighted. Without making use of the correctly weighted procedure, the effect would appear to be more short-lived.

It should be noted that in wave $\ell = -2$ relative to the law, coefficients are negative and significantly different from zero. This indicates that the parallel trends assumption necessary for identification may be violated. Recent work on the validity of pre-trends testing encourages researchers to carefully apply economic knowledge when evaluating these assumptions in addition to statistical tests [Roth 2019]. Pre-trends may pose an issue if they indicate “anticipation” of treatment, or that the timing of treatment and outcomes are related. Negative coefficients for pre-treatment periods in my setting logically rule out treatment anticipation, since, if there were any anticipation, we would see a positive coefficient of women beginning to increase labor force participation in advance of the law implementation. Perhaps more concerning is the possible endogeneity of the law timing - however, Section 2 demonstrated that there was no relationship between labor force participation and the presence of the laws. Further examination reveals that this “pre-trend” effect is driven solely by two states in which workplace breastfeeding laws were passed in 2001. There is no economic reason that the parallel trends assumption is violated if only two states are causing this drop in coefficients.\footnote{It is possible that, since for these two states (which include California) period $\ell = -2$ was during a rapid period of growth of the dot-com bubble, the relative labor force participation as compared to year $\ell = -1$ was significantly lower. Estimates in $\ell = 0$ are robust to removing these states, and I report the alternate coefficients which do not exhibit significant pre-trends in the Appendix (Table A3)}

Removing the two states with pre-trends from the analysis weakens the IW estimate magnitudes, although the effect of the law is more prolonged. Taken altogether, the negative direction, limited presence (to two states) and lack of correlation with other state outcomes provide ample evidence that the negative pre-treatment coefficient is not a concern for causal identification.

Repeating the analysis after restricting the sample to women who are non-white, I find even stronger results (Table 4). In the period in which the law is passed, female labor force participation increases by over 10%. However, these results for non-white women are not robust to the exclusion of the states which exhibit significant pre-trends.
This suggests that only minority women in California and Illinois were positively impacted by the passing of the laws (if one assumes that the negative pre-trends in these states are not cause for concern). These findings imply that the workplace breastfeeding legislation has differential impacts by race, and mainly impacts white women. Since breastfeeding initiation and duration are well known to differ by race (Kurinij et al., 1989), this is not surprising. White women in the PSID sample are also more likely to be employed in any given period, and report higher mean labor incomes. The fact that California is one of the few states offering paid maternity leave and other “female-friendly” workplace policies indicates that the breastfeeding legislation may only impact minority female labor force participation in the presence of other supportive and/or progressive legislation.

I now report results for the small sample of women who are known to have ever breastfed (as reported in the CAH supplement to the PSID) in Table 5. Due to the small sample size, I examine only the two-year period in which the laws went into effect. Results are consistent with the main estimates. During the period in which the laws were passed, women who ever breastfed were 5.6% more likely to work. The effect is not long lasting, as demonstrated in column (4), which reports the combined effect of all years after implementation of the law (see prior section for specifications). These results, taken together with the main sample results, indicate that there were indeed significant, albeit temporary, impacts of the workplace breastfeeding legislation on female labor force participation.

5.1.1 IFPS II Sample: Labor Force Participation

Because there is no information indicating whether mothers in the PSID sample were breastfeeding during the years of law implementation, I now extend the analysis using the IFPS survey data. Results are reported in Table 6. The IFPS II was conducted from 2005-2006, so I include a state as having been treated by the law if the law went into effect any year prior to 2005. The outcome variable WorkplaceLaw * Breastfeeding_{i,t} represents a dummy for women i who reported breastfeeding during a 3-month wave t interacted with the law treatment dummy. There is no significant effect on labor force participation in the year after giving birth. However, accounting for the law treatment (column 2) reduces the magnitude of the effect of breastfeeding
Table 5: Effect of Legislation: Non-white Women

| Wave relative to law | FE Estimates (1)\(^a\) | IW Estimates (2) | FE Estimates (3) | IW Estimates (4) |
|----------------------|------------------------|-----------------|-----------------|-----------------|
| -3                   | 0.010 (0.052)          | 0.039 (0.056)   | 0.005 (0.056)   | -0.006 (0.057)  |
| -2                   | -0.0971** (0.033)      | -0.070* (0.035) | -0.076 (0.064)  | -0.109* (0.065) |
| -1                   | 0 0                   | 0 0             | 0 0             | 0 0             |
| 0                    | 0.127**** (0.027)      | 0.111**** (0.027)| 0.040 (0.035)   | 0.031 (0.036)   |
| 1                    | 0.019 (0.029)          | 0.024 (0.030)   | -0.025 (0.043)  | -0.025 (0.043)  |
| 2                    | -0.003 (0.032)         | 0.013 (0.033)   | -0.022 (0.048)  | -0.007 (0.047)  |
| 3                    | -0.003 (0.032)         | -0.002 (0.036)  | -0.029 (0.048)  | -0.023 (0.050)  |

\(^a\) This table reports the results of the Two-Way Fixed Effects (FE) and Interaction-Weighted (IW) Event Study models (Equations 2-3). Treatment is the state workplace breastfeeding law going into effect. Columns 1 and 2 restrict the unbalanced sample to all non-white women in the main (unbalanced) PSID sample (N=8,258). Columns 3 and 4 further remove observations from California and Illinois. Controls include age, log childcare costs, marital status, age of the youngest child, and education. *** p<0.01, ** p<0.05, * p<0.1.

\(^b\) Wave \(\ell = -1\) is normalized to zero due to collinearity.
Table 6: Labor Force Participation: Women who ever breastfed

|                                | (1)        | (2)        | (3)        | (4)        |
|--------------------------------|------------|------------|------------|------------|
| Law Enacted^a                  | 0.056**    | 0.056**    | 0.064*     | -          |
|                                | (0.028)    | (0.028)    | (0.039)    | -          |
| Post-Law Enacted^b             | -0.010     |            |            |            |
|                                | (0.024)    |            |            |            |
| Married                        | -          | -0.0167    | -          | -          |
|                                | (0.020)    | (0.020)    |            |            |
| Male Labor Income              | -          | -0.029**** | -          | -          |
|                                |            | (0.012)    |            |            |
| ln(Childcare Expenses)         | 0.0223**** | 0.0223**** | 0.027****  | 0.023****  |
|                                | (0.002)    | (0.002)    | (0.003)    | (0.002)    |
| Constant                       | 0.676      | 0.689      | 0.783      | 0.709      |
|                                | (0.073)    | (0.073)    | (0.129)    | (0.069)    |
| R-squared                      | 0.105      | 0.106      | 0.122      | 0.102      |
| N (households)                 | 1,354      | 1,354      | 955        | 1,354      |

This table reports the simple pre/post difference-in-difference effect of the breastfeeding law being passed, in a reduced sample of women who reported ever breastfeeding in the CAH supplement to the PSID. The outcome variable is labor force participation. Controls are as in Tables 1-2.

^a Law Enacted is a dummy variable indicating the two-year period in which the breastfeeding law went into effect. Data is from 1997-2017 where all individuals are present for at least 9 continuous waves.

^b Post-Law Enacted is a dummy variable indicating all years after a law went into effect. 18 states are treated, the rest are controls.

^c *** p<0.01, ** p<0.05, * p<0.1.
on labor force participation by roughly 8%, and show that it is only in states where
the workplace law is not in effect that breastfeeding has an impact on the decision
to work. Columns 3 and 4 demonstrate an even greater reduction of the effect of
breastfeeding after controlling for the laws in a reduced but balanced sample, where
all women returned their survey responses in 3, 6, and 9 months after giving birth.

The most striking result is shown in columns 5 and 6, where the data are restricted
to women who eventually did return to work postpartum during some period in the
survey year (recall that the initial sample was restricted to mothers who both worked
prior to giving birth and who indicated in the pre-natal period that they were planning
to return to work after giving birth). For these women who did eventually return to
work, the effect of the workplace breastfeeding law is to completely negate the negative
effect of breastfeeding on labor force participation. Although there is evidence of some
feedback between the decision to breastfeed and the decision to work in prior literature,
this result indicates that workplace breastfeeding policies may help women separate
the two decisions.

Table 7: IFPS II: Labor Force Participation

|                | (1)      | (2)       | (3)       | (4)       | (5)       | (6)       |
|----------------|----------|-----------|-----------|-----------|-----------|-----------|
| WorkplaceLaw*Breastfeeding | -0.026   | -0.041    | -0.043    | -0.023    |          |           |
|                | (0.026)  | (0.047)   | (0.023)   |           |           |           |
| Law            |          | -0.026    | -0.002    | -0.004    |          |           |
|                |          | (0.032)   | (0.034)   | (0.010)   |           |           |
| Breastfeeding  | -0.039***| -0.031*   | -0.053*** | -0.044**  | -0.020**  | -0.015    |
|                | (0.016)  | (0.017)   | (0.019)   | (0.213)   | (0.010)   | (0.011)   |
| Constant       | 0.530    | 0.524     | 0.715     | 0.706     | 0.696     | 0.692     |
|                | (0.129)  | (0.129)   | (0.137)   | (0.137)   | (0.053)   | (0.054)   |
| Observations   | 3,724    | 3,724     | 2,464     | 2,464     | 3,264     | 3,264     |
| N (individuals)| 931      | 931       | 616       | 616       | 816       | 816       |

*Results reported for the IFLS II sample as in Equation 4. The outcome variable is returning
to work during a three month period post-birth. Columns (1) and (2) include the entire sample,
columns (3) and (4) are a balanced sample in which all women completed the final waves of the
survey, and columns (5) and (6) restrict the data to women who returned to work postpartum
during the sample period.

bControls in all specifications include education level, age, household income, occupation,
race/ethnicity indicators, how many house spent breastfeedings per week, whether the respon-
dent indicated ever having problems breastfeedings, whether the respondent had paid maternity
leave, and whether the mother had ever breastfed previous children.

*** p<0.01, ** p<0.05, * p<0.1.
5.2 Other Labor and Household Effects

Table 7 reports the effects of the legislation on female labor income, annual hours of work, and weekly reported hours of housework. For this analysis, I restrict the sample to women who are employed during the entire sample period. Columns (1), (3) and (5) report the two-way fixed effects estimates and columns (2), (4) and (6) report the interaction-weighted estimates. As is clear from the table and Figure 4, there is little effect of the law on any of the outcome variables. There is a slight reduction in work hours (-48.78) during the period in which the law is passed, according to the IW estimates (column (4)). This is consistent with women leaving the labor force for a few days or weeks after giving birth. It may also represent the fact that women who do return to work after giving birth and choose to pump or breastfeed at work may work fewer hours while at work. The number of hours is only slightly more than a full-time work week.

Figure 4: Labor Income and Hours Worked

This figure depicts results from Table 8 with 90% confidence intervals. Panel A shows interaction-weighted estimates for labor income and Panel B the interaction-weighted event study estimates for hours worked. Time $t = -1$ is normalized to zero to account for multicollinearity between relative and calendar time.

5.3 Robustness Tests

In addition to repeating the results for a full unbalanced panel and removing California, I also address concerns about my strict legal classification procedure by re-estimating the main results including the seven states with “weak” legal protections for workplace

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Footnote: Further estimates (i.e. control coefficients) are reported in Appendix table A2.
Table 8: Breastfeeding Legislation: Other Outcome Variables

| Variables: | Labor Income (1) | Labor Income (2) | Work Hours (3) | Work Hours (4) | Housework Hours (5) | Housework Hours (6) |
|-----------|-----------------|-----------------|---------------|---------------|--------------------|--------------------|
| Time      |                 |                 |               |               |                    |                    |
| -3        | -0.025          | -0.019          | -95.776*      | -98.433*      | 0.097              | -0.025             |
|           | (0.064)         | (0.067)         | (49.798)      | (51.553)      | (1.166)            | (1.199)            |
| -2        | 0.012           | -0.004          | -40.767       | -32.148       | 0.207              | 0.228              |
|           | (0.058)         | (0.062)         | (39.197)      | (41.685)      | (0.855)            | (0.924)            |
| -1        | 0               | 0               | 0             | 0             | 0                  | 0                  |
| 0         | 0.024           | 0.038           | -44.768       | -48.783*      | -0.474             | -0.429             |
|           | (0.047)         | (0.046)         | (29.462)      | (29.545)      | (0.850)            | (0.868)            |
| 1         | 0.033           | 0.021           | -6.626        | -10.574       | 0.338              | 0.240              |
|           | (0.051)         | (0.052)         | (38.648)      | (39.563)      | (0.850)            | (0.884)            |
| 2         | -0.041          | -0.045          | 22.483        | 23.526        | 0.684              | 0.485              |
|           | (0.056)         | (0.056)         | (42.005)      | (41.263)      | (0.789)            | (0.786)            |
| 3         | 0.040           | 0.045           | -33.754       | -30.441       | 1.915**            | 2.023**            |
|           | (0.047)         | (0.047)         | (35.337)      | (36.427)      | (0.840)            | (0.848)            |
| Labor Income | -               |                 | 374.927***   | -0.813***     |                    |                    |
|           |                 |                 | (25.098)      | (0.290)       |                    |                    |
| Constant  | 7.914***        | -1.644.613***   | 31.826***     |               |                    |                    |
|           | (0.432)         | (495.367)       | (9.442)       |               |                    |                    |
| Observations | 6.876          | 6.876           | 6.876         | 6.876         | 6.876              | 6.876              |
| R-squared | 0.748           | 0.670           | 0.524         |               |                    |                    |

* Estimates using the full PSID household sample and modelled as in equations 1-3. Columns 1-2 are the labor income outcome variable, columns 3-4 are work hours, and columns 5-6 are housework hours. Controls include age, education, ln childcare costs, age of the youngest child, number of children present in the household, and religious preference of the household head.

a Work hours are reported as annually. Housework hours reported as weekly.

b Columns 1, 3 and 5 report two-way fixed effect estimates (person and year fixed effects). Columns 2, 4, and 6 report interaction-weighted estimates as in Abraham and Sun 2020.

c *** p<0.01, ** p<0.05, * p<0.1.
breastfeeding. These states have one of the following provisions: they either only require workplace amenities for public or school employees, only “suggest” that firms provide such amenities, or they have legal provisions for a firm to designate itself as “breastfeeding friendly” in some formal manner. Because these provisions do not apply to the entire labor force and in many cases would only apply to a small fraction of workers, ex ante we would assume that these laws have no impact on the total labor force participation of women. However, to avoid any confusion about the classification procedure, I repeat the analysis also including these states as treated in the event study framework.

Results are reported in Appendix Table A5. There is no longer a significant impact of the introduction of breastfeeding legislation on female labor force participation. This non-result would be expected if the original classification procedure was correct. There should be no effect in states where the legislation only applies to a small segment of the workforce, or is not legally enforceable in some manner.

Lastly, to alleviate concerns that there are confounding characteristics which may both induce a state’s legislature to pass breastfeeding laws and encourage mothers to work, I also repeat the analysis controlling for the number of women elected to a state’s legislature each year. Results are consistent with this specification, indicating that it is not cultural or gender norms within a state both influencing passage of the laws and the labor force participation decision.

6 Conclusion

This paper demonstrates a significant positive impact of workplace breastfeeding legislation on female labor force participation. The effect, however, is not persistent. If optimal amount of breastfeeding is chosen so that marginal benefits equal the marginal costs (in terms of labor income, for example), perhaps women who return to work after implementation of the laws experience realized personal costs of workplace pumping and breastfeeding that are greater than expected. Therefore, in subsequent years, the legislation has no effect on the optimization decisions of breastfeeding mothers, as true

\footnote{Note that inclusion of extra states increases the number of CATT cohorts from 5 to 7.}

\footnote{Results available from the author by request.}
marginal costs are not reduced\textsuperscript{17}. If the experienced benefits of workplace breastfeeding are not as significant as most women are led to believe, or if it inherently difficult to pump or breastfeed at work regardless of the provision of workplace breastfeeding amenities, it is understandable that the effects of workplace breastfeeding legislation would not be persistent ex-post. Indeed, Chen et al. (2006) find that despite ample provision of workplace breastfeeding amenities, Taiwanese mothers are not very likely to continue to breastfeed after returning to work. Felice et al. (2017) show that working mothers report feeling “isolated and embarrassed” for engaging in workplace pumping.

It should be noted, however, that women in the IFPS II sample are much more likely to report high job satisfaction if they reside in a state with the workplace breastfeeding policies in place. This demonstrates a potential spillover effect - even though working mothers may not make significantly more use of the workplace breastfeeding amenities, their very provision can change the worker’s perception of her firm, which may increase her likelihood to return to her job after giving birth.

The main finding, that labor force participation increases during the few years after implementation of workplace breastfeeding legislation, is transient. Ex ante, this was not anticipated and is a surprising result. We would expect that any benefits to returning to work after birth with these amenities in place would be persistent. This transience could be due to firms somehow making up for the increased costs of these provisions in ways which otherwise inconvenience working mothers, or it could be due to a miscalculation of the benefits of workplace breastfeeding and pumping by new mothers becoming self-corrected in subsequent years. Determining whether the supply or demand side of the labor market is responsible for the transient effect is beyond the scope of this paper without having detailed information about the rationale behind participation decisions of women. Survey evidence, however, indicates that work schedule flexibility is related to workplace breastfeeding continuation (Felice et al., 2017), which suggests that the presence of breastfeeding legislation alone, without other family-friendly policies in place, may not be enough to support the full-time labor force participation of mothers. Future research should also examine the

\textsuperscript{17}Although prior literature expounds the health benefits of breastfeeding without much exception, there is emerging evidence casting significant doubt onto the true magnitude of health effects, and on the validity and methodology of the most frequently cited studies. Studies making use of sibling comparisons find no positive cognitive effects of breastfeeding (Rothstein, 2013), for example. Oster (2020) provides a thorough and practical overview of the current breastfeeding health literature.
impacts of recent hospital shifts towards Baby-Friendly practices on female labor force participation (Nelson and Grossniklaus, 2019).

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7 APPENDIX
| State            | Statute                                      |
|------------------|----------------------------------------------|
| Arkansas         | Ark Code Ann §11–5-116 (2009)                |
| California       | Cal [Labor] Code §§1030–1033 (2001)          |
| Colorado         | Colo Rev State §§ 8–13.5–101-104 (2008)      |
| Connecticut      | Conn Gen Stat §31–40w (2001)                 |
| District of Columbia | DC Code §2–1401.05 (2007)                  |
| District of Columbia | DC Code §§2–1402.81–83 (2007)              |
| Georgia          | Ga Code Ann §34–1-6 (1999)                  |
| Hawaii           | Haw Rev Stat §§378–2, 10 (1999)             |
| Illinois         | 820 Ill Comp Stat 260/ (2001)               |
| Maine            | Me Rev Stat Ann tit 26, §604 (2009)         |
| Minnesota        | Minn Stat §181.939 (1998)                   |
| Montana          | Mont Code Ann §§39–2-215–217 (2007)         |
| New Mexico       | NM Stat §28–20-2 (2007)                     |
| New York         | NY [Labor] Law §206-c (2007)                |
| Oklahoma         | Okla Stat tit 40, §435 (2006)               |
| Oregon           | Or Rev Stat §§653.075, 0.077, 0.079, 0.253 (2007) |
| Rhode Island     | RI Gen Laws §23–13.2–1 (2003, amended 2008) |
| Tennessee        | Tenn Code Ann §50–1-305 (1999)              |
| Vermont          | Vt Stat Ann tit 21, §305 (2007)             |
Table A2: Summary Statistics: Untreated Outcomes

| Labor Force Participation | All Controls | Treated, Before Implementation |
|---------------------------|-------------|-------------------------------|
|                           | Mean        | Std Dev | N    | Mean        | Std Dev | N    |
| Wave 1                    | 0.792       | 0.406   | 1420 | (E_i ≥ 1)   | 0.698   | 301  |
| Wave 2                    | 0.804       | 0.397   | 1421 | (E_i ≥ 1)   | 0.740   | 334  |
| Wave 3                    | 0.829       | 0.377   | 1188 | (E_i ≥ 2)   | 0.837   | 160  |
| Wave 4                    | 0.821       | 0.384   | 1187 | (E_i ≥ 3)   | 0.830   | 159  |
| Wave 5                    | 0.799       | 0.401   | 1187 | (E_i ≥ 4)   | 0.792   | 159  |
| Wave 6                    | 0.791       | 0.407   | 1087 | (E_i ≥ 5)   | 0.814   | 59   |
| Wave 7                    | 0.759       | 0.428   | 1028 |              |         |      |
| Wave 8                    | 0.730       | 0.444   | 1028 |              |         |      |
| Wave 9                    | 0.711       | 0.453   | 1028 |              |         |      |
| Wave 10                   | 0.691       | 0.462   | 1028 |              |         |      |

| Female labor income       |             |        |      |             |        |      |
| Wave 1                    | 12,864      | 12,728 | 1124 | (E_i ≥ 1)   | 13,761 | 210  |
| Wave 2                    | 13,657      | 11,632 | 1142 | (E_i ≥ 1)   | 13,880 | 247  |
| Wave 3                    | 14,394      | 11,069 | 985  | (E_i ≥ 2)   | 15,300 | 134  |
| Wave 4                    | 15,068      | 13,198 | 974  | (E_i ≥ 3)   | 16,450 | 132  |
| Wave 5                    | 15,374      | 13,065 | 949  | (E_i ≥ 4)   | 15,300 | 126  |
| Wave 6                    | 14,990      | 11,548 | 860  | (E_i ≥ 5)   | 13,396 | 48   |
| Wave 7                    | 16,592      | 13,550 | 780  |              |         |      |
| Wave 8                    | 16,268      | 12,683 | 750  |              |         |      |
| Wave 9                    | 16,313      | 13,154 | 731  |              |         |      |
| Wave 10                   | 16,562      | 13,418 | 710  |              |         |      |

| Female wages              |             |        |      |             |        |      |
| Wave 1                    | 12,862      | 13,011 | 1074 | (E_i ≥ 1)   | 13,832 | 200  |
| Wave 2                    | 13,595      | 11,271 | 1090 | (E_i ≥ 1)   | 13,864 | 236  |
| Wave 3                    | 14,385      | 10,956 | 938  | (E_i ≥ 2)   | 15,196 | 128  |
| Wave 4                    | 15,117      | 13,435 | 913  | (E_i ≥ 3)   | 15,358 | 126  |
| Wave 5                    | 15,385      | 12,778 | 884  | (E_i ≥ 4)   | 15,496 | 119  |
| Wave 6                    | 15,210      | 11,777 | 807  | (E_i ≥ 5)   | 13,627 | 47   |
| Wave 7                    | 16,794      | 13,837 | 731  |              |         |      |
| Wave 8                    | 16,368      | 12,860 | 716  |              |         |      |
| Wave 9                    | 16,375      | 13,409 | 698  |              |         |      |
| Wave 10                   | 16,357      | 12,687 | 683  |              |         |      |

*a This table reports summary statistic by PSID wave for all control households in the main balanced sample, and by treated households before law implementation. E_i refers to the cohort treatment time group (see Section 3.2). There are five waves in which laws were enacted in the US (5 E_i cohorts beginning in wave 2), and each household in these states acts as a control unit for treated households prior to implementation.

*b Wave 1 corresponds to 1997 with each wave occurring two years apart.
Table A3: Other Outcome Variables: Full Specifications

| VARIABLES          | (1) ln(labor income) | (2) Work Hours | (3) Housework |
|--------------------|----------------------|----------------|---------------|
| 1 - 3              | -0.025               | -0.028         | 0.022         |
|                    | (0.064)              | (0.064)        | (0.084)       |
| 1 - 2              | 0.012                | -0.015         | 0.010         |
|                    | (0.058)              | (0.054)        | (0.072)       |
| 1 - 0              | 0.024                | 0.022          | 0.020         |
|                    | (0.047)              | (0.045)        | (0.059)       |
| 1 - 1              | 0.033                | 0.033          | -0.016        |
|                    | (0.051)              | (0.049)        | (0.069)       |
| 1 - 2              | -0.041               | -0.050         | -0.030        |
|                    | (0.056)              | (0.054)        | (0.067)       |
| 1 - 3              | 0.140                | 0.020          | 0.017         |
|                    | (0.047)              | (0.046)        | (0.058)       |
| 1 - 4              | -0.010               | -0.006         | 0.001         |
|                    | (0.018)              | (0.018)        | (0.025)       |
| ln(labor income)   | 0.022***             | 0.019***       | 0.029***      |
|                    | (0.054)              | (0.064)        | (0.086)       |
| ln(female lab income) | 374.927***          | 328.198***     | -0.813***     |
|                    | (25.098)             | (35.253)       | (0.290)       |
| Constant           | 7.914***             | 1.817***       | 6.641***      |
|                    | (0.432)              | (0.541)        | (0.953)       |
| Observations       | 6.876                | 6.872          | 4.495         |
| R-squared          | 0.748                | 0.789          | 0.762         |

Robust standard errors in parentheses
*** p < 0.01, ** p < 0.05, * p < 0.1

Notes:

- Work hours are reported as annually. Housework hours reported as weekly.
- All variables except ln(labor income) and ln(female lab income) are expressed in natural logs.
- ln(Childcare expense) is expressed in natural logs.

- Education (years):
  - Education:
    - 0 (0.01) = 0.011
    - 1 (0.016) = 11.369
    - 2 (0.021) = 15.529
    - 3 (0.026) = 20.69
    - 4 (0.031) = 30.95

- Age of Youngest Child:
  - 0 (0.026) = 0.015
  - 1 (0.031) = 0.020
  - 2 (0.036) = 0.025
  - 3 (0.041) = 0.030
  - 4 (0.046) = 0.035

- Number of Children:
  - 0 (0.01) = 0.005
  - 1 (0.015) = 0.009
  - 2 (0.021) = 0.013
  - 3 (0.026) = 0.018
  - 4 (0.031) = 0.023

- Black:
  - 0 (0.01) = -0.120
  - 1 (0.015) = -0.122
  - 2 (0.019) = -0.125
  - 3 (0.024) = -0.128
  - 4 (0.028) = -0.132

- Latin/o/a:
  - 0 (0.01) = -0.219
  - 1 (0.015) = -0.220
  - 2 (0.019) = -0.221
  - 3 (0.024) = -0.222
  - 4 (0.028) = -0.223

- Work Hours:
  - 0 (0.01) = 0.000
  - 1 (0.015) = 0.001
  - 2 (0.019) = 0.002
  - 3 (0.024) = 0.003
  - 4 (0.028) = 0.004

- ln(family income):
  - 0 (0.01) = 0.000
  - 1 (0.015) = 0.001
  - 2 (0.019) = 0.002
  - 3 (0.024) = 0.003
  - 4 (0.028) = 0.004

- ln(male labor income):
  - 0 (0.01) = 0.000
  - 1 (0.015) = 0.001
  - 2 (0.019) = 0.002
  - 3 (0.024) = 0.003
  - 4 (0.028) = 0.004

- ln(female lab income):
  - 0 (0.01) = 0.000
  - 1 (0.015) = 0.001
  - 2 (0.019) = 0.002
  - 3 (0.024) = 0.003
  - 4 (0.028) = 0.004

- Constant:
  - 0 (0.01) = 7.914
  - 1 (0.015) = 1.817
  - 2 (0.019) = 6.641
  - 3 (0.024) = 1.806
  - 4 (0.028) = 3.641

- Robust standard errors in parentheses

- *** p < 0.01, ** p < 0.05, * p < 0.1
Table A4: Main Results: No California or Illinois

| Wave $\ell$ relative to law | $\tilde{\mu}$ | $\tilde{\nu}$ | $\delta_{1,L}$ | $\delta_{2,L}$ | $\delta_{3,L}$ | $\delta_{4,L}$ | $\delta_{5,L}$ |
|-----------------------------|---------------|---------------|----------------|----------------|----------------|----------------|----------------|
| -3                          | -0.017        | -0.008        | 0.034          | -0.013         | -0.001         |                 |                 |
|                             | (0.030)       | (0.030)       | (0.014)        | (0.037)        | (0.053)        |                 |                 |
| -2                          | -0.049        | -0.063        | -0.079         | 0.013          | -0.002         | -0.103         |                 |
|                             | (0.037)       | (0.099)       | (0.165)        | (0.012)        | (0.050)        | (0.064)        |                 |
| -1                          | 0             | 0             | 0              | 0              | 0              | 0              | 0              |
| 0                           | 0.056***      | 0.038*        | 0.053          | 0.012          | -0.903         | 0.091          | 0.046          |
|                             | (0.028)       | (0.020)       | (0.040)        | (0.047)        | (0.019)        | (0.055)        | (0.060)        |
| 1                           | 0.034         | 0.050*        | -0.013         | 0.097          | 0.123          | -0.006         |                 |
|                             | (0.033)       | (0.031)       | (0.047)        | (0.051)        | (0.019)        | (0.066)        | (0.079)        |
| 2                           | 0.028         | -0.034        | -0.069         | -0.139         | 0.091          | 0.187          | 0.060          |
|                             | (0.034)       | (0.097)       | (0.048)        | (0.201)        | (0.019)        | (0.064)        | (0.056)        |
| 3                           | 0.026         | 0.056         | -0.017         | 0.077          | 0.104          | -0.015         |                 |
|                             | (0.035)       | (0.038)       | (0.053)        | (0.069)        | (0.02)         | (0.059)        | (0.084)        |

$^a$ N = 13,980. Sample excludes observations from California and Illinois, two states which exhibit significant pre-trends in the main analysis.

$^b$ Wave $\ell = -1$ is normalized to zero due to collinearity.

$^c$ Cohort Average Treatment effects are reported for each individual cohort. A cohort is defined as all states in which the law goes into effect during the same two-year period.

Table A5: Main Results: Unbalanced Panel

| Wave $\ell$ relative to law | $\tilde{\mu}$ | $\tilde{\nu}$ | $\delta_{1,L}$ | $\delta_{2,L}$ | $\delta_{3,L}$ | $\delta_{4,L}$ | $\delta_{5,L}$ |
|-----------------------------|---------------|---------------|----------------|----------------|----------------|----------------|----------------|
| -3                          | 0.044         | 0.049         | 0.154          | 0.066          | 0.017          |                 |                 |
|                             | (0.028)       | (0.028)       | (0.010)        | (0.036)        | (0.048)        |                 |                 |
| -2                          | -0.046**      | -0.037*       | -0.052         | 0.126          | -0.008         | -0.031         |                 |
|                             | (0.022)       | (0.022)       | (0.030)        | (0.010)        | (0.041)        | (0.058)        |                 |
| -1                          | 0             | 0             | 0              | 0              | 0              | 0              | 0              |
| 0                           | 0.072****     | 0.071****     | 0.048          | 0.115          | -0.765         | 0.009          | 0.068          |
|                             | (0.017)       | (0.017)       | (0.033)        | (0.027)        | (0.014)        | (0.043)        | (0.043)        |
| 1                           | 0.033*        | 0.037*        | -0.010         | 0.060          | 0.228          | 0.080          | -0.049         |
|                             | (0.019)       | (0.019)       | (0.038)        | (0.029)        | (0.015)        | (0.048)        | (0.050)        |
| 2                           | 0.008         | 0.012         | -0.071         | 0.033          | 0.205          | 0.058          | 0.005          |
|                             | (0.020)       | (0.020)       | (0.038)        | (0.030)        | (0.014)        | (0.054)        | (0.051)        |
| 3                           | -0.027        | -0.026        | -0.069         | -0.013         | 0.191          | -0.014         | -0.024         |
|                             | (0.020)       | (0.020)       | (0.038)        | (0.028)        | (0.015)        | (0.055)        | (0.062)        |

$^a$ N = 25,380. Sample is unbalanced in calendar time. Households are present for at least two waves between 1997-2017. 50% of the sample is present for a consecutive seven wave period or more.

$^b$ Wave $\ell = -1$ is normalized to zero due to collinearity.

$^c$ Cohort Average Treatment effects are reported for each individual cohort. A cohort is defined as all states in which the law goes into effect during the same two-year period.
### Table A6: Weak Legal Provisions

| Wave \( \ell \) relative to law | \( \hat{\mu} \)    | \( \hat{\nu} \)    | \( \hat{\delta}_{1,L} \) | \( \hat{\delta}_{2,L} \) | \( \hat{\delta}_{3,L} \) | \( \hat{\delta}_{4,L} \) | \( \hat{\delta}_{5,L} \) | \( \hat{\delta}_{6,L} \) | \( \hat{\delta}_{7,L} \) |
|-------------------------------|----------------|----------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|
| -3                            | -0.013         | -0.016         | (0.022)                   | (0.024)                   |                           |                           |                           |                           |                           |
|                               | (0.022)        | (0.024)        | (0.073)                   | (0.030)                   | (0.042)                   | (0.086)                   |                           |                           |                           |
| -2                            | -0.021         | -0.026         | (0.019)                   | (0.020)                   | (0.033)                   | (0.058)                   | (0.035)                   | (0.048)                   | (0.075)                   |
|                               | (0.019)        | (0.020)        | (0.033)                   | (0.058)                   | (0.035)                   | (0.048)                   | (0.075)                   |                           |                           |
| -1                            | 0              | 0              | 0                         | 0                         | 0                         | 0                         | 0                         | 0                         |                           |
| 0                             | 0.022          | 0.016          | 0.028                     | 0.064                     | 0.006                     | -0.053                    | 0.005                     | 0.024                     | 0.055                     |
|                               | (0.016)        | (0.016)        | (0.044)                   | (0.040)                   | (0.029)                   | (0.043)                   | (0.044)                   | (0.044)                   | (0.098)                   |
| 1                             | 0.021          | 0.021          | 0.052                     | -0.016                    | 0.004                     | -0.020                    | 0.049                     | 0.038                     | 0.026                     |
|                               | (0.017)        | (0.018)        | (0.044)                   | (0.047)                   | (0.032)                   | (0.066)                   | (0.045)                   | (0.054)                   | (0.110)                   |
| 2                             | -0.005         | 0.028          | -0.044                    | -0.069                    | -0.036                    | 0.223                     | 0.112                     | 0.048                     | 0.281                     |
|                               | (0.019)        | (0.020)        | (0.046)                   | (0.049)                   | (0.035)                   | (0.034)                   | (0.047)                   | (0.051)                   | (0.219)                   |
| 3                             | -0.017         | -0.004         | -0.022                    | -0.018                    | -0.048                    | 0.212                     | 0.050                     | -0.048                    |                           |
|                               | (0.019)        | (0.018)        | (0.048)                   | (0.053)                   | (0.032)                   | (0.036)                   | (0.043)                   | (0.061)                   |                           |

*a* N = 8,825. Treated states include those with weak legal provisions for workplace breastfeeding amenities. States with weak provisions include Virginia, Louisiana, Texas, Indiana, Mississippi, North Dakota, and Washington.

*b* Wave \( \ell = -1 \) is normalized to zero due to collinearity.

*c* Note that including extra states increases the number of cohorts \( \ell \) from 5 to 7.

*d* Cohort Average Treatment effects are reported for each individual cohort. A cohort is defined as all states in which the law goes into effect during the same two-year period.