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EVIDENCE FROM CALIFORNIA

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ABSTRACT

This paper evaluates the effect of Paid Family Leave (PFL) on breastfeeding, which we identify using California's enactment of a 2004 PFL policy that ensured mothers up to six weeks of leave at a 55 percent wage replacement rate. We employ synthetic control models for a large, representative sample of over 270,000 children born between 2000 and 2012 drawn from the restricted-use versions of the 2003 – 2014 National Immunization Surveys. Our estimates indicate that PFL increases the overall duration of breastfeeding by nearly 18 days, and the likelihood of breastfeeding for at least six months by 5 percentage points. We find substantially larger effects of PFL on breastfeeding duration for some disadvantaged mothers.

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1. Introduction

Early childhood is a sensitive period for parental investments that promote health and human capital in adulthood (Heckman 2007; Almond, Currie, and Duque 2017). Building on Barker's fetal origins hypothesis (Barker 1997), a substantial economic literature has focused on understanding the hierarchy of investments and pinpointing the underlying technology of human capital production. Yet, increasing knowledge about the salience of individual investments and the mechanisms driving improved outcomes should not ignore the broad, substantive obstacles that might deter such investments. Structural features of the U.S.'s work-family policy framework might inhibit or promote parental investments in children, especially for infants who would otherwise start life at a marked disadvantage. Moreover, policies intended to promote parental investments have disparate or equitable effects. This study presents new evidence on the impact of Paid Family Leave (PFL) on breastfeeding, a parental investment important for healthy child development. To do so, we exploit the temporal and geographic variation in access to leave related to California's 2004 implementation of PFL. California is the most useful state for examining the effects of paid parental leave because the other existing state programs are either too recent to have sufficient post-program data (Rhode Island, New York) or too small to have sufficient sample size to study disparities in outcomes across population subgroups (New Jersey).

Breastfeeding has long been considered the most beneficial source of nutrition for infants (Centers for Disease Control and Prevention 2016). It has been linked to strengthened immunity, reduced likelihood of post-neonatal and SIDS mortality, and decreases in hospitalizations and deaths from infectious diseases, diarrhea, and respiratory infections (Pediatrics 2005; Grummer-Strawn and Rollins 2015; Pediatrics 2005; Chen and Rogan 2004; Victora et al. 2016). Breastfed children face lower risks of adverse, long-term health outcomes as well, including obesity, type II diabetes, and asthma (Victora et al. 2016), with limited evidence linking breastfeeding to higher IQ scores (Kramer et al. 2008). Breastfeeding is also associated with decreases in maternal breast and ovarian cancer, type II diabetes, and postpartum depression (Grummer-Strawn and Rollins 2015; Pediatrics 2005).[1]

[1] These associations may not always indicate causal relationships since there is nonrandom selection into breastfeeding.

In 2010, the Department of Health and Human Services put forth Healthy People 2020 targets to increase the prevalence and duration of breastfeeding among U.S. mothers. These efforts have achieved uneven success. Overall, U.S. mothers breastfeed their infants at higher rates today than at any point in documented history but low-income mothers, who were more likely to initiate breastfeeding through the 1960's, have become less likely to do so, while the reverse is true for middle- and high- income women (Dennis 2002; Dubois and Girard 2003; Callen and Pinelli 2004; Victora et al. 2016).

A leading reason for mothers stopping breastfeeding is the need to return to work (Thulier and Mercer 2009). The United States is the only developed country without a statutory national PFL entitlement, resulting in paid leave coverage being limited to that provided voluntarily by employers. In the absence of PFL entitlement, public health campaigns to increase breastfeeding may have limited impact. This is particularly true for lower income mothers who are less likely to receive employer-provided paid leave (Bartel et al. 2019).

While the relationship between Paid Family Leave and breastfeeding has been systematically studied outside the United States, the findings are not directly applicable to the U.S. due to the much longer periods of mandated leave in other countries. We provide what is to our knowledge the first U.S. evidence on the relationship between PFL and breastfeeding using a large, representative sample combined with appropriate econometric techniques. The sample contains over 270,000 mother-child pairs representing births between 2000 and 2012 and is drawn from the restricted-use 2003 – 2014 National Immunization Survey. We employ difference-in-difference models using synthetic control methods to compare the pre- versus post-law differences in outcomes of mother-child pairs in California to those outside the state. The synthetic control method is important because the parallel trends assumption is violated when employing a standard difference-in-difference methodology with all mothers from outside California as the control group. Our results suggest that PFL significantly increases overall breastfeeding duration by nearly 18 days (from a base of 221 days) and breastfeeding for at least six months by 4.9 percentage points (from a pre-PFL average of 53 percent) while having little effect on the probability of initiating breastfeeding. We find substantially larger effects for historically disadvantaged groups of women for both breastfeeding duration measures and these results are robust using a variety of alternative specifications and samples. Our evidence suggests that an extension of PFL to families in states without current mandates may have positive impacts on breastfeeding behaviors.

2. Policy Framework

Outside of the United States, maternity leave is typically employment-protected and paid at a high wage replacement rate for at least the initial period of time off work.[2] The United States does not offer paid maternity or parental leave as a statutory entitlement. Although up to 12 weeks of job-protected unpaid leave has been available since the enactment of the Family Medical Leave Act of 1993 (FMLA), unpaid leave is often most feasible for high-income or two-parent families, representing a marked disadvantage for the families for whom leave may be the most beneficial (Han, Ruhm, and Waldfogel 2009). Additionally, due to the eligibility requirements pertaining to employee's work history and firm size, less than 60 percent of employees are estimated to be eligible for unpaid leave under FMLA (Klerman, Daley, and Pozniak 2012).

California was the first U.S. state to enact a Paid Family Leave (PFL) entitlement in 2002 that went into effect July 1, 2004, providing eligible workers up to six weeks of paid leave in a twelve-month period at 55 percent of the worker's normal earnings up to a maximum benefit.[3] Five states and the District of Columbia have followed California, with PFL programs either currently in place or scheduled to go into effect in New Jersey (2009), New York (2018), Rhode Island (2014), Washington (2020), the District of Columbia (2020) and Massachusetts (2021). California's program is funded by payroll taxes and employees are eligible for PFL if they earned at least $300 during a 12-month base period within the 5 – 18 months before taking leave. PFL does not offer job protection but some employees may qualify under the FMLA or California Family Rights Act (CFRA). There is a 7-day unpaid waiting period in which claimants are ineligible to receive benefits and the first payment is remitted after an additional two weeks. In addition to the six weeks of leave provided under PFL, most California mothers may qualify for up to four weeks of paid pre-birth leave and six weeks of paid post-birth leave (eight weeks for children born by Cesarean section) under California's State Disability Insurance

[2] See Table PF2.1.A. https://www.oecd.org/els/soc/PF2_1_Parental_leave_systems.pdf
[3] In January, 2018, the wage replacement rate increased to 60-70 percent to a maximum of $1,216 http://www.edd.ca.gov/pdf_pub_ctr/de8714cf.pdf

program (SDI).[4] Prior research indicates that PFL increased leave-taking among new mothers and that the number of claims has steadily increased since the program took effect in 2004 (Rossin-Slater, Ruhm, and Waldfogel 2013; Bedard and Rossin-Slater 2016a; Baum and Ruhm 2016; Bartel et al. 2018).

3. Prior Literature

Ruhm (2000) was the first to document the relationship between parental leave and child health, finding that the duration of parental leave available through public policies in a given country/year reduced infant mortality. The relationship was estimated to be nonlinear, with diminishing returns detected after 25 weeks or so of leave. Using similar data and design, but with a longer panel and more countries, Tanaka (2005) and Shim (2013) found that parental leave generosity reduced the probability of low birth weight, increased immunizations, and decreased infant mortality. Though unpaid parental leave did not confer these advantages in these studies, two individual-level studies in the U.S. detected benefits of unpaid leave in terms of infant health (Rossin 2011) and paid leave in terms of mortality (Stearns 2015). Similar results were documented by Berger, Hill, and Waldfogel (2005), where early return to work by mothers – within 12 weeks of giving birth – was associated with reductions in breastfeeding, regular checkups, and the up-to-date status of immunizations. Similarly, Baker and Milligan (2008b) found that a 25-week expansion in paid maternal leave in Canada (from 25 weeks to 50 weeks) extended the duration of any and exclusive breastfeeding, while not having a statistically significant impact on breastfeeding initiation.[5] Lichtman-Sadot and Pillay Bell (2017) found that California's PFL program was associated with reductions in obesity, ADHD, and hearing related problems among elementary school children.

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[4] California is one of five states with a temporary disability insurance program. The other states are Hawaii, New Jersey, New York and Rhode Island.

[5] Results for longer-term child outcomes are less consistent. Baker and Milligan (2008a, 2011) failed to find an effect of the leave expansion on parenting quality, child behavior, or other cognitive and behavioral outcomes. Several studies of European leave expansions have found null effects on education and earnings (Rasmussen 2010; Dustmann and Schönberg 2012; Liu and Skans 2010). However, the 1977 expansion of leave in Norway resulted in a two percentage point decline in high school dropout rates and a five percent increase in wages at age 30 (Carneiro, Loken, and Salvanes 2015). More generally, it is unclear to what extent research on European paid leave expansions is informative about PFL effects in the US both because the entitlements are so much more generous and since there may be differences in the characteristics and behavioral responses of populations with historically generous safety nets (see discussion in review by Ruhm and Waldfogel, 2012).

To date, only two studies have examined the effect of California's PFL law on breastfeeding. Using a non-representative sample of healthy singleton births from the Infant Feeding Practices Survey, Huang and Yang (2015) found that PFL increased breastfeeding. As low-income families are particularly under-represented in this small-sample study, the average and distributional effects of PFL on breastfeeding remain unknown. Most relevant to the present investigation, Hamad, Modrek, and White (2018) examined the effect of California and New Jersey's Paid Family Leave entitlements using data from the public-use versions of the National Immunization Survey. Employing a difference-in-difference methodology, the authors find that PFL increased exclusive breastfeeding at six months, particularly among advantaged mothers. However, that study suffers from several limitations that our analysis rectifies. First, the public-use versions of the NIS exclude both county-level identifiers and the child's exact date-of-birth. Second, the breastfeeding incidence and duration variables are top-coded and re-weighted to prevent identification of the mother-child pairs in the public-use data, so that actual incidence and duration are unobservable. Third, all states other than California and New Jersey are used as control states but, as shown below, the parallel trends assumption is violated when doing so.[6] We address this problem by utilizing synthetic control methods.

4. *Data*

We use restricted-use data from the 2003 – 2014 National Immunization Survey (NIS), a large and nationally-representative data set with detailed information on breastfeeding behaviors. The repeated cross-section design of the NIS results in a final sample of 271,309 child-mother pairs for births occurring between 2000 and 2012. The survey is administered by telephone to the parents and caregivers of children 19 – 35 months old, who report retrospectively about breastfeeding. Hence, the data from the 2003 survey includes information on children born in 2000 and 2001, while the latest wave we use, from 2014, includes information on children born in 2011 and 2012. The restricted-use version of the NIS data is necessary for our analysis since it provides geographic identifiers, the child's exact date of birth, and detailed information on breastfeeding duration.

[6] Hamad, Modrek, and White (2018) examine the parallel trends assumption using a graphical comparison of slopes, but do not use the more rigorous approach we employ, which reveals violations of the parallel trends assumption in nearly every outcome variable.

We examine breastfeeding initiation and duration using responses to questions indicating whether the child was ever breastfed, even for a short period of time, and the duration of all breastfeeding, including periods with supplementation with formula or water.

Our primary analytic sample contains all mother-child pairs who answered the breastfeeding questions and provided demographic information. Supplementary covariates include maternal education (< high school, high school, some college, and BA or higher), maternal age (< 19 years, 20-29 years, 30 years+), poverty level (< 50 %, 50 – 99%, 100 – 200 %, and ≥200 % of the Federal Poverty Level), race/ethnicity (white, black, Hispanic and other), and binary indicators for marital status, parity, and child gender.

The NIS data collection changed in two important ways over time. First, there was a substantial change in the breastfeeding questions in 2006.[7] From 2003 – 2005, the questions addressed breastfeeding behaviors apart from other sources of nutrition. From 2006 onward, questions were added that pinpointed the timing and duration of formula feeding relative to breastfeeding rendering those variables un-usable in our analysis. This change did not affect all breastfeeding behaviors as the timing and duration of other supplementation can co-occur in any combination with any breastfeeding, but it did mean that we were unable to examine exclusive breastfeeding because that outcome could not be defined consistently over time. Second, there is some evidence of state-level variability in breastfeeding estimates after the addition of a cell phone sample to the previous landline-only sampling method (in 2011 and 2012), although the differences were less than 5 percentage points for the majority of states (Scanlon et al. 2014).

In order to employ our primary synthetic control model estimation method, the unit of observation must be the same as the level of the policy change. Accordingly, we aggregate individuals into state-year cells based on the family's state of residence at the survey date and the year of the child's birth. Each state-year cell contains the relevant variable means, with weights used to make the sample population representative of the full state. Our preferred specifications make use of state-year level data but we also test the robustness of our results to the use of quarter-year and individual data.

[7] See https://www.cdc.gov/breastfeeding/data/nis_data/survey_methods.htm

We construct a set of variables measuring maternal labor market participation to approximate the unobserved likelihood of labor market participation for mothers in our sample. We use these variables as strata in our synthetic control analysis. Using data from the 2000 – 2014 Annual Demographic Supplement to the March Current Population Survey (CPS), we calculate four variables to approximate the potential employment of women in our sample. First, we calculate state-level estimates of the female-to-male population ratio based on the full sample of adult CPS respondents (ages 18-64). Second, we compute the full-time county-level employment rate of women to account for potential PFL eligibility. Third, we compute the employment rate of women with infants under one year to allow for state-level differences in fertility. Finally, we include a measure of predominance of female-dominated industries. In addition, we construct state-level covariates for our synthetic control analysis including the log of the population, per capita income, effective minimum wage, and the percent receiving welfare assistance from the University of Kentucky Center for Poverty Research (UKCPR) National Welfare Data files for 2000 – 2014).

5. Empirical Strategy

A basic difference-in-difference framework (DD) comparing the breastfeeding rates of California children born before and after the July 1, 2004 PFL implementation, to children born outside of California in the same period takes the form:

$$Y_{ijt} = \alpha + \gamma(CA \times POST)_{ijt} + \delta_1 X_{ijt} + \delta_2 POST_t + \delta_3 CA_j + \varepsilon_{ijt}, \qquad (1)$$

where Y_{ijt} is the outcome variable for child i in state j in year t, CA is a binary variable coded as one for children living in California and zero in the control states, $POST$ is a dichotomous indicator set to one for births that took place after the July 1, 2004 enactment of PFL, and zero otherwise. Demographic covariates and labor market controls are captured in X_{ijt}, including maternal race/ethnicity, marital status, completed education, age at child's birth, household poverty status, child parity, child gender, and child's year of birth. Finally, ε_{ijt}, is an error term, and $\hat{\gamma}$ captures the DD quasi-intent-to-treat (ITT) estimate. We include state-specific linear time trends to account for cross-state differences with respect to time as well.

The DD strategy relies on the assumption that treatment versus control group differences in outcomes would have remained the same in the absence of PFL implementation. If this parallel trends assumption is violated, the DD estimand, $\hat{\gamma}$, is likely to be biased by divergent pre-treatment trends. We investigate whether pre-treatment trends in the outcome variables are similar for California and elsewhere by estimating the following model, using observations for children i in state *j* and year *t* among children born before July 1, 2004:

$$Y_{ijt} = \alpha_0 + \alpha_1 X_{ijt} + \alpha_2 TR_t + \alpha_3 NONCA_{ijt} + \alpha_4 TR \times NONCA_{ijt} + \varepsilon_{ijt}. \qquad (2)$$

In (2), TR_{ijt} is a linear time trend for the period ending in 2004, and $NONCA_{ijt}$ is a dummy variable equal to zero for California and one for all other states. The coefficient of interest, $\hat{\alpha}_4$ indicates whether the pre-treatment time trend in the outcome variable differs between California and the other states.

Our estimates from equation (2) indicate that the parallel trends assumption is violated. Therefore, we rely upon a synthetic control estimates strategy (Abadie, Diamond, and Hainmueller 2010) for our preferred specifications. To apply the synthetic control method, states are weighted based on their statistical similarity to California in terms of pre-treatment trends. Specifically, we aggregate our data to the state-year level to calculate the weights using the pre-law trends in the outcome variables after including control variables from within the NIS data indicating the fraction of the state population: who are nonwhite, married, with income below the poverty threshold, maternal age below 20 years, and less than a high school education. As we discuss in depth in the robustness section, we vary the pre-law outcome variables and covariates, and estimate separate models with additional population-level labor force participation controls drawn from the March CPS and UKCPR National Welfare Data.

Following Abadie et al. (2010), we select a vector of weights W to minimize the distance between the characteristics of California, X_1, and the other control states (the donor pool), X_0W, such that:

$$\|X_1 - X_0W\|v = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)} \qquad (3)$$

Where V is a symmetric and positive semidefinite matrix of covariate weights chosen to minimize the mean squared prediction error of the outcome variables in the pre-law period.[8] The

[8] We omit New Jersey from our models since that state passed a PFL law in 2009.

final vector of state weights W sum to one, so that "synthetic California" is the weighted average of the states in the "donor pool". This method ensures that "synthetic California" has pre-trends in the outcome variables that are identical or nearly identical to California over the same period, after conditioning on the supplementary covariates.

We estimate equation (1), comparing the state-year level outcomes in California children to those in synthetic California, with the standard errors adjusted to deal with potential serial correlation due to the small number of clusters following the two-step approach proposed by Donald and Lang (2007).[9] We also compare synthetic California to California graphically, where the treatment effect is represented by the difference in outcomes between the two groups over time.

6. Main Results

Descriptive statistics for our analysis sample, summarized in Table 1, suggest that both the incidence and duration of breastfeeding increased over time both in California and in other states. California mothers appear to breastfeed more and longer, and California has a larger Hispanic population share and slightly more impoverished respondents than other states.

We test the assumption of pre-law trends in the breastfeeding variables in Table 2, with the sample limited to children born before 2004. Comparing the trends of non-California states (Not CA*Trend) to California, we find mixed but always statistically significant pre-trend differences in the incidence and duration of breastfeeding, indicating that the parallel trends assumption is violated. This justifies the use of synthetic control methods in our preferred specifications and indicates potential biases in prior research using standard DD models.

Table 3 shows the synthetic control results comparing the outcomes of children in California to those in synthetic California with a full set of controls from the NIS. The effect of California's PFL on breastfeeding incidence is marginally negative (column 1) though small, imprecisely estimated, and insignificant. Conversely, PFL is associated with an 18-day increase in breastfeeding duration that is significant at the 10 percent level (column 2). Compared to a pre-program mean in California of 221 days, this corresponds to growth of around 8 percent. The

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[9] The first-stage regression is estimated on the weighted, collapsed data that yields the regression-adjusted differences between California and synthetic California for each year. In the second step, these differences are regressed on the POST indicator. We present the coefficients from these estimations, with standard errors estimated using a student's-t distribution with 25 degrees of freedom.

coefficient in column 4 indicates that California's PFL increased the likelihood that a woman would breastfeed for at least six months by 5 percentage points, over a base of 53 percent. Interestingly, there is no effect on breastfeeding at least three months (column 3). The respective synthetic control weights are reported in Appendix Table A1.

Figures 1 through 4 show predicted impacts of PFL on breastfeeding incidence and duration estimated using synthetic control models, with separate effects allowed for each year before and after implementation. As anticipated, if the synthetic control methods are successful, the patterns for California and the synthetic controls are identical during the pre-program period. For the years after 2004, breastfeeding incidence is initially predicted to fall modestly (through 2008) and then to increase in 2010 and beyond. The patterns for duration, conditional on breastfeeding (Figure 2) are much clearer, with a modest initial effect and then a substantial increase in breastfeeding time emerging after 2007 (e.g. 47 days in 2010), mimicking the gradual take-up of leave use detected in California's administrative PFL data (Bedard and Rossin-Slater 2016). PFL appears to have no effect on whether a mother reports breastfeeding at least three months (figure 3), though the effect on six months closely tracks the overall duration pattern, with the largest effects in the later periods (figure 4). We report the annual treatment effect estimates from these models in Appendix Table A7.

7. Heterogeneous Treatment Effects

Prior research suggests that disadvantaged mothers may be more responsive to PFL than their more advantaged peers (Baker and Milligan 2008b; Carneiro, Loken, and Salvanes 2015; Stearns 2015; Lichtman-Sadot and Pillay Bell 2017). For this reason, we next examine whether PFL has heterogeneous treatment effects by stratifying our synthetic control models by several markers of disadvantage, including: maternal age and education, poverty status, whether the family receives Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) or has experienced a disruption in phone service. Figures 5 through 8 show point estimates with 95 percent confidence intervals for the CA*post coefficient from a DD model using synthetic control groups estimated independently for each model. The markers are used to delineate historically disadvantaged groups from those who are relatively advantaged. Figure 5 presents the stratified models for breastfeeding incidence. The estimates are not uniformly significant or large in magnitude, but hint at the possibility that PFL may induce breastfeeding initiation among several disadvantaged groups. In particular, PFL was associated with a five percentage point increase in breastfeeding for families who experienced an interruption in phone service, with similar estimated effects for those with lower levels of education, and the most impoverished (< 50 % of the FPL).[10] Black/non-Hispanic respondents are nearly 7.5 percentage points more likely to initiate breastfeeding under increased PFL access. Conversely, estimated effects for the more advantaged groups are generally indistinguishable from zero (except for college-educated mothers, where breastfeeding initiation appears to decline). Negative but statistically insignificant predicted effects are also obtained for some other groups.

Figure 6 shows that PFL has more consistent positive effects on breastfeeding duration, and greater gains for some disadvantaged groups. For instance, respondents who experienced a phone interruption were estimated to increase the duration of breastfeeding by 65 days, with a corresponding 37 day increase for WIC recipients during the prior year, compared to little or no effect for their counterparts. Mothers with less than a high school education were predicted to breastfeed 63 additional days as the result of PFL, compared to around 35 days for high school graduates or those with some college and no change for college graduates. The estimates are also much larger for mothers at <50% or 50%-99% of the federal poverty line, compared to those with more financial resources, and for single versus married mothers. However, there are no clear racial differences.

Figures 7 and 8 show the same general patterns in breastfeeding at least three and six months, although the between group differences are rarely statistically significant. For instance, the largest PFL coefficients for breastfeeding at least three months (figure 7) occur among the most disadvantaged individuals in terms of education and poverty (13 and 8 percentage point increases, respectively). Similarly, PFL is associated with a 14 percentage point increase in the likelihood that respondents below 50 % of FPL breastfeed at least six months (figure 8) and with corresponding 8 percentage point increases for WIC recipients and the non-college educated.

8. Robustness Tests

[10] Stratifications by income, though endogenous, followed the same pattern as those by poverty rate. Results available upon request.

We performed a variety of robustness checks. First, we employed an alternative, residual-based method for incorporating the same sets of covariates. Here, we regress each respective set of covariates on each outcome, retaining the residuals to use in the weighting stage of the synthetic control estimation, rather than the raw variables themselves. The results of these estimations, shown in table A2, imply that our results are not being driven by our choice of covariates. Indeed, the overall direction and magnitude of coefficients is fairly consistent, though that on breastfeeding duration is less precisely estimated.

To ensure that our synthetic control estimates are not spuriously driven by the selection of weights used to construct synthetic California, we conducted a "leave-one-out" test (Abadie, Diamond, and Hainmueller 2015), which involved running the synthetic control models after systematically removing one state at a time from the donor pool (with replacement). We plot the treatment effect graphs on a single plot for comparison (California minus the synthetic California). The results of this test, shown in figures A1 – A4 (appendix), suggest that the set of states included in the donor pool do not generally affect the treatment effects, although the results for breastfeeding incidence (A1) and breastfeeding at three months (A3) are more sensitive than those for duration (A2) and breastfeeding at six months (A4).[11]

Next, following Borjas (2015) and Peri and Yasenov (2015), we employ a series of "placebo in space" PFL estimations, estimating separate models where each control state is erroneously classified as the treated state. This test indicates whether the effect detected for California is larger than the corresponding placebo effects. These results, shown in Appendix figures A5 – A8 are somewhat mixed but in ways that are consistent with the main pattern of our findings. Specifically, for breastfeeding incidence, where we find small or null predicted effects in our main specifications, there is little evidence of an especially large California effect (shown in black) compared to those in other states (show in gray). Comparing the root mean squared prediction error (RMSPE) – a measure of goodness of fit – across all placebo state effects during the validation period, the California estimate is smaller than for 40 percent of states, suggesting the absence of any strong true effect. Conversely, for breastfeeding duration, the effect for California is consistently near the top of the estimated effects, particularly after 2007. Indeed, 87

[11] There are only four alternative lines in each figure because just four states receive positive weights in the synthetic control models.

percent of states have a larger RMSPE in the same period, suggesting that the effect we detect is indeed more probable and non-random.[12] The same statistic for breastfeeding at least 3 and 6 months is 26 percent and 63 percent, respectively (figures A7 and A8). Overall, these results confirm that the strongest PFL effects are on overall durations and breastfeeding 6 months or more, with weaker effects for breastfeeding at least 3 months and little if any impact on breastfeeding initiation.

Since the PFL law went into effect in July of 2004, we conducted three additional sensitivity tests: 1) categorizing 2005 (rather than 2004) as the treatment year; 2) omitting 2004 from the analysis completely; 3) using fiscal rather than calendar year measures as the unit of observation. The results were largely robust to these alternatives as shown in Appendix tables A3 and A4. When we use 2005 as the treatment year, we find that the coefficients are similar in magnitude and direction, only differing in terms of significance on the duration of breastfeeding (table A3). Omitting 2004 altogether and re-defining the calendar year as the treatment year (table A4) follows the same pattern but with retained significance.

9. Maternal Employment

The National Immunization Survey lacks information on maternal employment, which prevents us from calculating the treatment-on-the treated (TOT) effect of CA-PFL. We employ two additional secondary analyses to partially overcome this limitation. First, we implement a series of difference-in-difference-in-difference (DDD) models, where the third difference is between mothers with high versus low expected rates of PFL eligibility. To do so, we exploit differences in county-level labor force participation (LFP) and employment rates of mothers with infants obtained from Current Population Survey (CPS) data. Specifically, we divide counties according to whether the median unemployment or labor force participation (LFP) rates of mothers with infants are above or below the median rates (51.9 % and 56.5 % respectively).[13] The assumption here is that fewer mothers would be PFL-eligible in low-employment and low-LFP counties, which should result in smaller estimated PFL effects in these counties.

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[12] Table of placebo RMSPEs available upon request.

[13] Though the CPS data omit counties in PUMAs under 100,000, they provide more detailed, year-specific maternal employment rates in the pre-law period than other data sources.

The synthetic control DDD results are shown in Table 4. In the models with state-specific linear time trends, we find that women in high-employment/LFP counties are no more likely to initiate breastfeeding in response to PFL than those in low-employment/LFP counties (column 1). Conversely, such mothers breastfeed 58 days longer in response to PFL when stratifying based on labor force participation and 26 days more when dividing the sample based on employment rates. Surprisingly, the DDD results suggest that that women in high-employment/LFP counties are more likely to breastfeed at least three months (column 3) in response to PFL, but are equally likely to do so at six months (column 4). One possibility is that mothers in high employment/LFP counties are not only more likely to be eligible for PFL but also more often return to work after leave, so that their breastfeeding duration will often be truncated at around three months as a result.

Our second strategy presumes that the treatment effect varies by a mother's propensity to breastfeed in the absence of treatment. We employ a repeated split sampling (RSS) approach to examine this possibility (Abadie, Chigos and West 2018). To do so, we first regress the breastfeeding outcome on baseline characteristics using a randomly selected half of the control group sample. Second, we use these coefficients to generate predicted breastfeeding outcomes for the remaining half of the control group, as well as the treatment group. Treatment and control observations are then categorized into tertile bins representing low, medium and high breastfeeding propensity and we then estimate the treatment effect within each bin. This random splitting and estimation process is repeated over 100 iterations and the average treatment effects are reported for each of three breastfeeding propensity bins, with standard errors estimated using 100 bootstrap repetitions. As more advantaged groups are both more likely to be employed and to breastfeed, the RSS approach captures the role of relative employment status, and helps reduce the potential bias from stratification by allowing treatment intensity to vary based on breastfeeding propensity. Our results shown in table 5 suggest a strong and consistent gradient in treatment intensity, with high-propensity mothers substantially more likely to initiate and prolong breastfeeding in response to PFL relative to the low-propensity mothers.

10. Discussion and Conclusion

Our results indicate that California's Paid Family Leave program raised the overall duration of breastfeeding by around 18 days, and the likelihood of breastfeeding for at least six months by 5 percentage points, corresponding to increases of 8 to 9 percent relative to pre-program means. The finding regarding duration of at least six months is important given the recommendation of the American Academy of Pediatrics (AAP) that mothers breastfeed for at least 6 months. There is little consistent evidence that PFL raised rates of breastfeeding initiation or durations of three months or longer. In addition to the high pre-program rates of breastfeeding in California (85 percent), the lack of an initiation effect may have occurred because California mothers already had access to paid leave in the first few weeks after birth through the state's temporary disability insurance program. Larger initiation effects might be obtained for PFL programs implemented in the 45 states and DC that do not offer TDI.

Beyond the average effects just described, we generally detect larger positive effects in breastfeeding duration among disadvantaged groups of women. For instance, among women experiencing a phone interruption in the prior year, PFL is estimated to increase the duration of breastfeeding by up to 65 days, while those without an interruption in service were largely unaffected. We also obtain evidence of relatively large duration effects for less educated and poor mothers, relative to their counterparts. In combination, these findings suggest that paid family leave may reduce disparities in breastfeeding by increasing durations among the most vulnerable groups. This finding makes sense, given that employer paid leave is tilted toward more advantaged workers and given that low-income mothers may be less able to afford to use the unpaid leave offered under the FMLA.

Our results should be interpreted in light of several limitations. First, as we are unable to observe women's labor market participation and actual eligibility for PFL, we interpret our results as intent-to-treat estimates. Ideally, future research would incorporate PFL eligibility and estimate treatment-on-the-treated effects. We used several strategies to address this shortcoming but our sometimes mixed results imply that future research should more fully integrate pre-birth employment and eligibility to reduce any remaining residual bias. Second, although California's adoption of the PFL law was plausibly exogenous, it would be interesting to repeat this analysis using cohorts of mothers and children in other states, once their newer PFL laws have been in place for an adequate amount of time. Third, our results were generally but not completely robust to variations in specification and measurement, suggesting the need for caution when interpreting the findings.

Overall, our paper provides evidence that extending PFL to families in states without current mandates may increase breastfeeding durations, possibly leading to health improvements for children and mothers in the longer-term. Given that our positive findings are often concentrated among disadvantaged mothers, extensions of PFL may also reduce disparities in breastfeeding and associated outcomes. Another implication is that future research and legislation should acknowledge socioeconomic status as an important source of heterogeneity in the effects of PFL.

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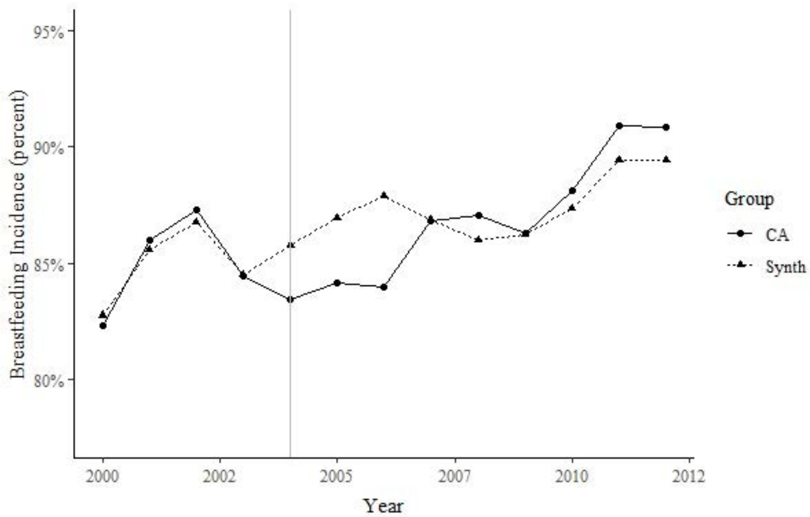
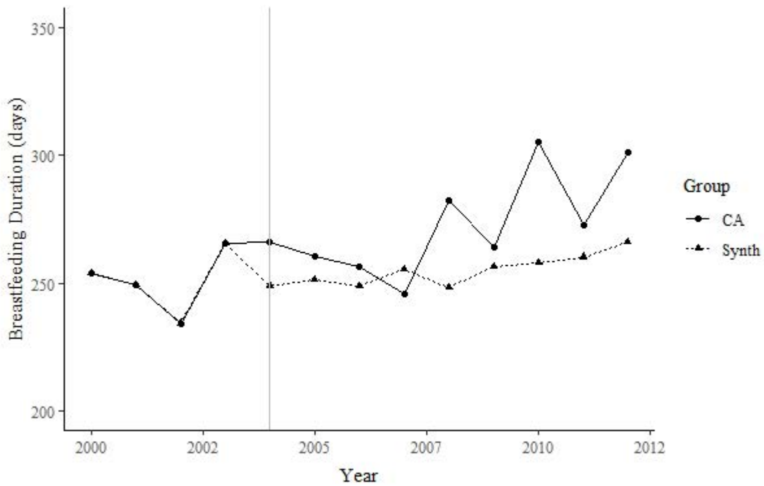
Figures

Figure 1: Breastfeeding Incidence, Synthetic Control Analysis (2004)



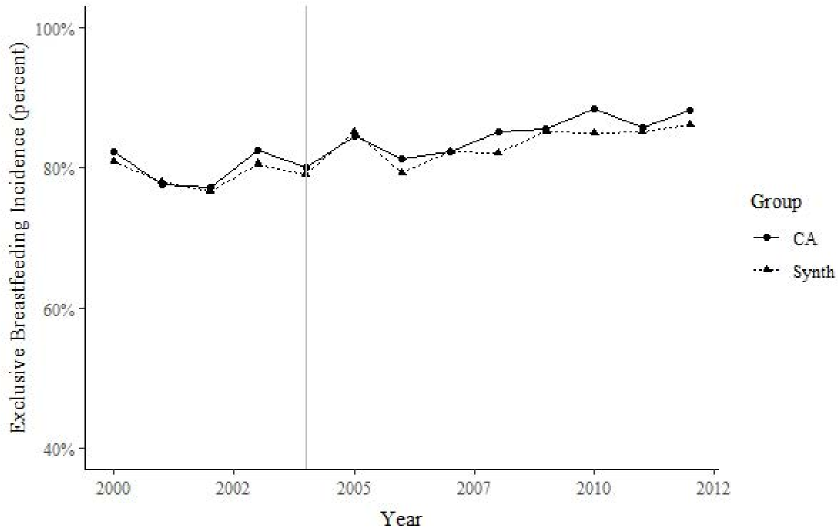
Note: Figure displays results from synthetic control analysis comparing the estimated effect of PFL on the incidence of breastfeeding in California (CA) compared to that of synthetic California (synth). Children drawn from the National Immunization Survey (2003 – 2014) are aggregated to the state-year level. Covariates include maternal race/ethnicity (the fraction of women in each state-year who are characterized by each race/ethnicity subcategory – white, black, Hispanic, and other), marital status (percentage of women married), completed education (percentage of women with each respective level of education - less than high school, high school, some college, and a college degree or higher), age at child's birth (percentage of women who are younger than 20, 20-29, or 30 +), household poverty status (percentage of women in each of four poverty categories - < 50 % of the Federal Poverty Level, FPL, 50-90% FPL, 100-200% FPL, and 200% FPL +), child parity, child gender, maternal immigration status, and child's year of birth.

Figure 2: Breastfeeding Duration, Synthetic Control Analysis (2004)



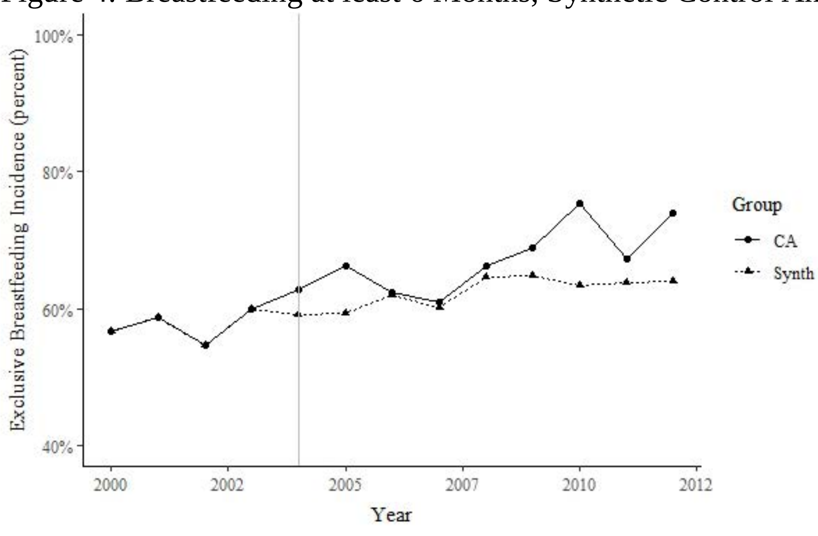
Note: See note on Figure 1. Figure displays results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth). Sample and covariates are the same as in figure 1.

Figure 3: Breastfeeding at least 3 Months, Synthetic Control Analysis (2004)



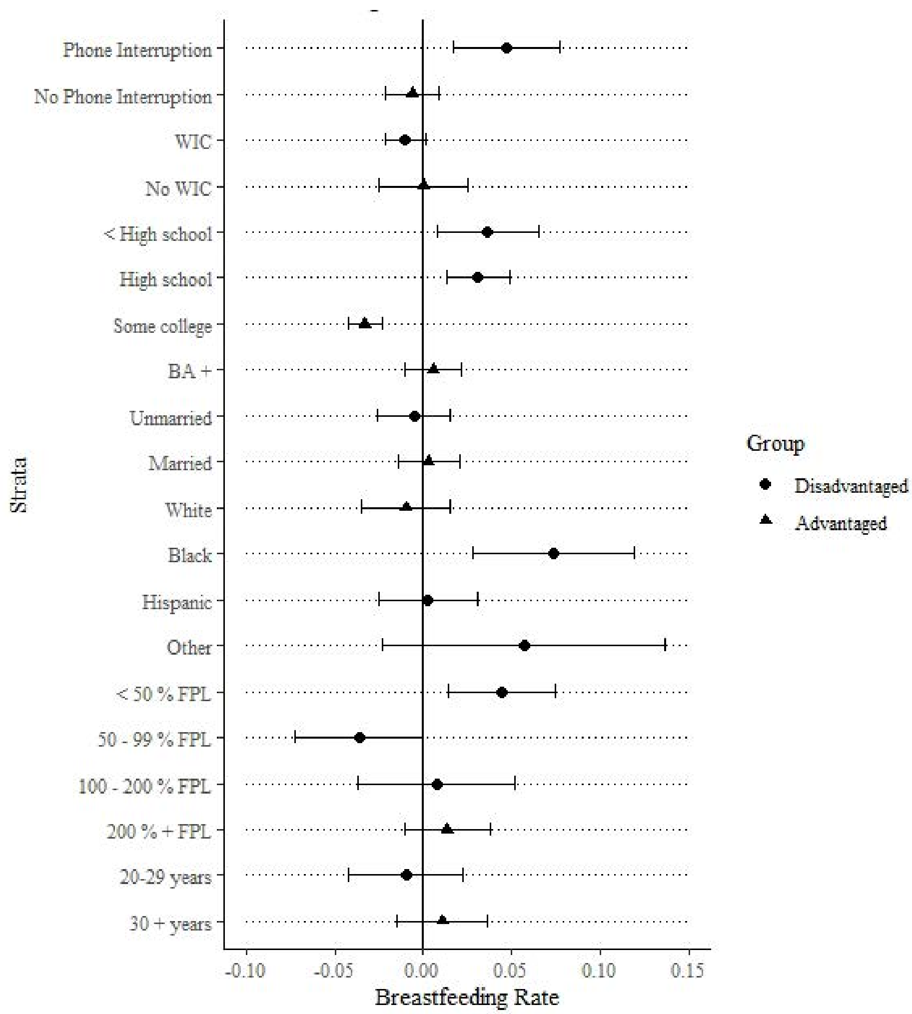
Note: See note on Figure 1. Figure displays results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding for at least 3 months (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth). Sample and covariates are the same as in figure 1.

Figure 4: Breastfeeding at least 6 Months, Synthetic Control Analysis (2004)



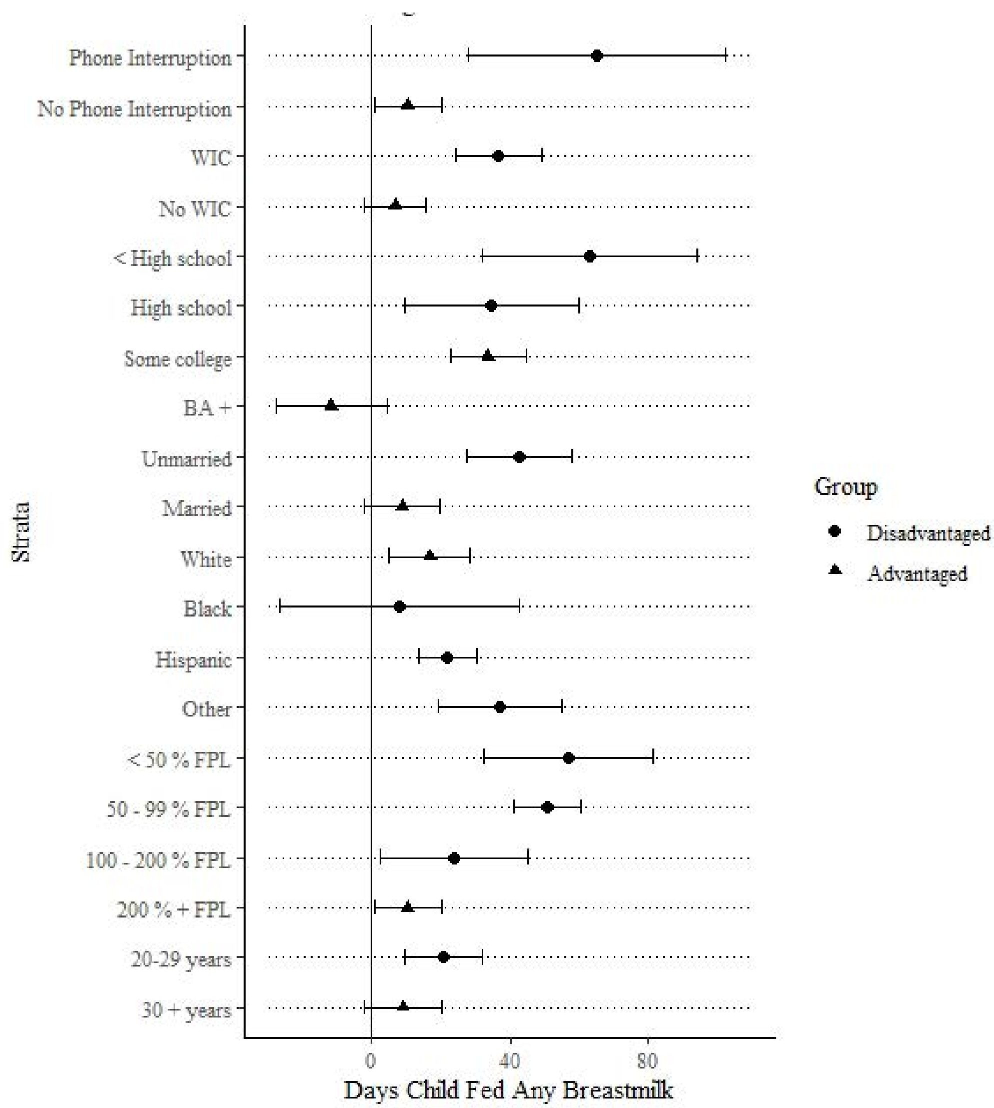
Note: See note on Figure 1. Figure displays results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding for at least 6 months (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth). Sample and covariates are the same as in figure 1.

Figure 5: Breastfeeding Incidence: Stratified Models



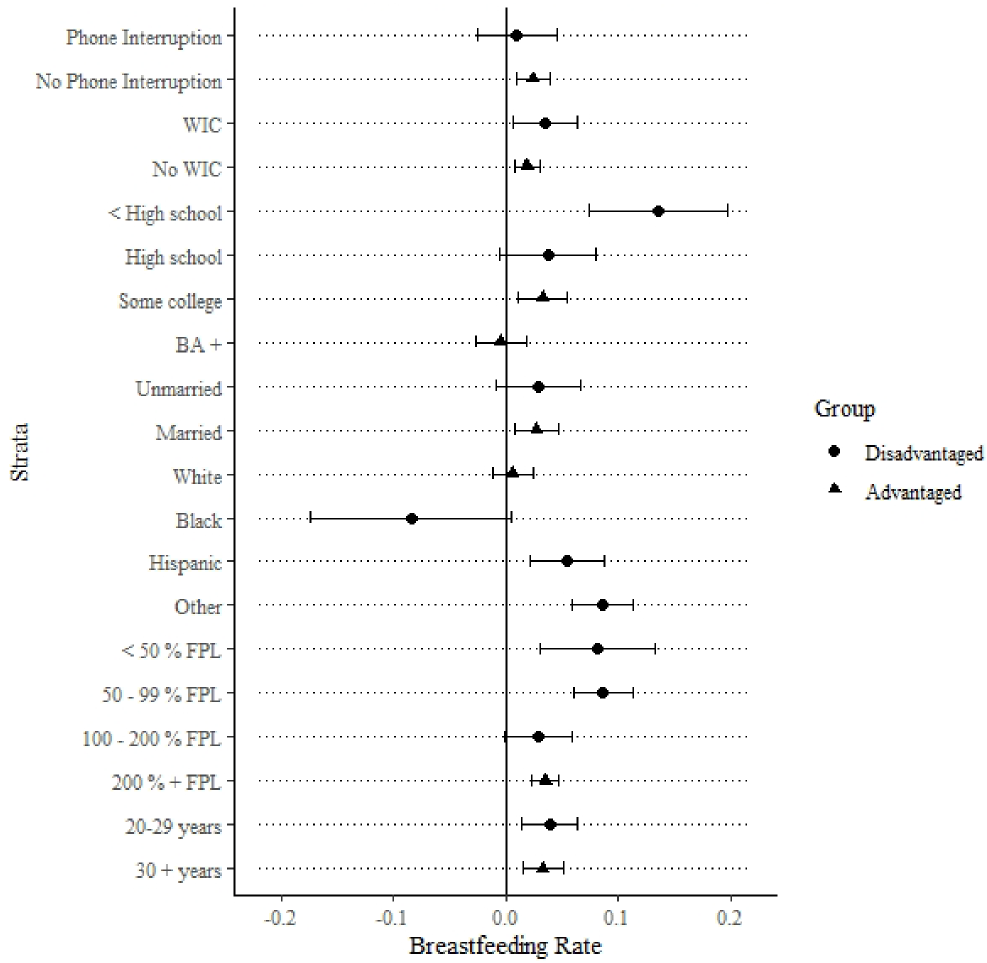
Note: See note on Figure 1. Graphical results from synthetic control analysis comparing the estimated effect of PFL on the incidence of breast feeding in California (CA) to that of synthetic California (synth) for subsamples stratified by demographic indicators. Coefficients are represented by points with error bars showing 95 percent confidence intervals. Sample and covariates are the same as in Figure 1.

Figure 6: Breastfeeding Duration: Stratified Models



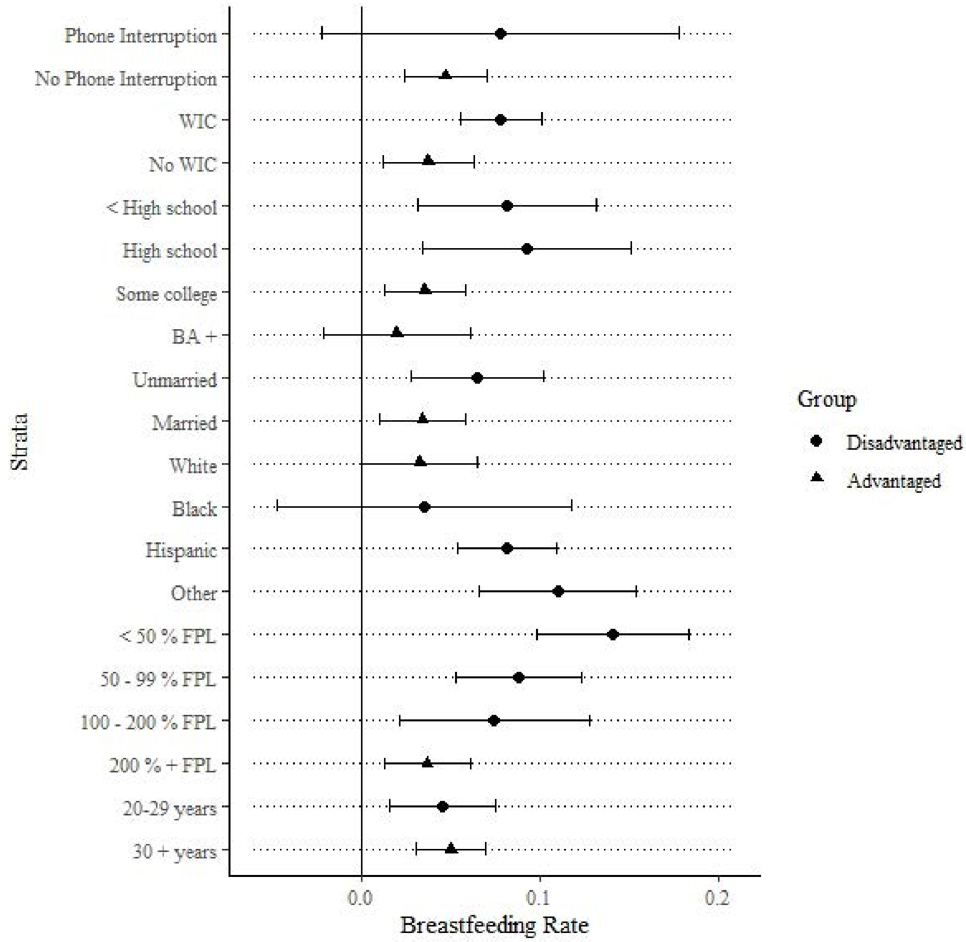
Note: Graphical results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth) stratified by demographic indicators. Coefficients are represented by points with error bars showing 95 percent confidence intervals. Sample and covariates are the same as in Figure 1.

Figure 7: Breastfeeding for at least Three Months: Stratified Models



Note: Graphical results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth) stratified by demographic indicators. Coefficients are represented by points with error bars showing 95 percent confidence intervals. Sample and covariates are the same as in Figure 1.

Figure 8: Breastfeeding for at least Six Months: Stratified Models



Note: Graphical results from synthetic control analysis comparing the estimated effect of PFL on the duration of breastfeeding (conditional on any breastfeeding), in California (CA) to that of synthetic California (synth) stratified by demographic indicators. Coefficients are represented by points with error bars showing 95 percent confidence intervals. Sample and covariates are the same as in Figure 1.

Tables

Table 1: Descriptive Statistics

	Full Sample			CA			Not CA		
	Pre	Post		Pre	Post		Pre	Post	
Ever Breastfed	0.72	0.76	***	0.85	0.87	*	0.71	0.74	***
Breastfeeding Duration	221.07	234.36	***	254.82	271.07	*	215.45	228.05	***
>= 3 months	0.73	0.78	***	0.80	0.85	***	0.72	0.77	***
>= 6 months	0.53	0.59	***	0.59	0.67	***	0.52	0.57	***
Maternal Education									
Less than High School	0.18	0.18	***	0.26	0.27		0.17	0.17	
High School	0.33	0.28	***	0.30	0.24	***	0.34	0.29	***
Some College	0.18	0.21	***	0.17	0.19	***	0.19	0.22	***
College (BA) or higher	0.31	0.32	***	0.27	0.30	*	0.31	0.33	***
Maternal Age									
<= 19	0.03	0.02		0.02	0.02		0.03	0.02	*
20-29	0.43	0.40	***	0.39	0.36	*	0.44	0.41	***
30+	0.54	0.57	***	0.59	0.62		0.54	0.57	***
Married	0.71	0.66	***	0.72	0.67	**	0.70	0.65	***
Poverty									
< 50% FPL	0.11	0.16	***	0.14	0.19	***	0.11	0.15	***
50% - 99% FPL	0.16	0.17	***	0.20	0.21		0.15	0.17	***
100% - 200% FPL	0.24	0.21	***	0.23	0.19	***	0.24	0.21	***
200% + FPL	0.49	0.46	***	0.43	0.41		0.50	0.47	***
Race									
Hispanic	0.25	0.27	***	0.55	0.54		0.21	0.22	***
White non-Hispanic	0.54	0.49	***	0.28	0.26		0.58	0.53	***
Black non-Hispanic	0.12	0.13	***	0.05	0.04		0.13	0.14	***
Other non-Hispanic	0.09	0.11	***	0.13	0.16	**	0.08	0.11	***
Firstborn	0.42	0.43	***	0.40	0.41		0.42	0.43	***
Male	0.51	0.51		0.51	0.51		0.51	0.51	
Child's age (months)									
19-23	0.26	0.32	***	0.26	0.33	***	0.26	0.32	***
24-29	0.32	0.34	***	0.31	0.34	**	0.32	0.34	***
30-35	0.42	0.34	***	0.43	0.33	***	0.42	0.34	***
Foreign Born	0.01	0.01		0.02	0.01	***	0.01	0.01	**
Obsv	85,020	186,289		4,378	6,335		80,282	179,954	

Note: This table shows the descriptive statistics for children drawn from the 2003 – 2014 National Immunization Survey (born between 2000 and 2012). *** p<0.01, ** p<0.05, * p<0.1

Table 2: Analysis of Pre-law trends in the Outcome Variables

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
NotCA * Trend	0.01***	-3.55***	0.01***	-0.01***
	(0.00)	(0.63)	(0.00)	(0.00)
Trend	-0.01***	5.64***	0.00***	0.01***
	(0.00)	(0.15)	(0.00)	(0.00)
Not CA	-0.14***	-22.86***	-0.09***	-0.04***
	(0.01)	(3.62)	(0.01)	(0.01)

Note: Table shows result of OLS models estimated using children drawn from the National Immunization Survey born in the pre-PFL period (2000 – 2003). Covariates include maternal race/ethnicity (white, black, Hispanic, and other), marital status indicator, maternal completed education (less than high school, high school, some college, and a college degree or higher), maternal age at child's birth (younger than 20, 20-29, or 30 +), household poverty status (percentage of women in each of four poverty categories < 50 % of the Federal Poverty Level, FPL, 50-90% FPL, 100-200% FPL, and 200% FPL +), child parity, child gender, maternal immigration status, and child's year of birth. N = 95,411 for full sample, N=71,179 for breastfeeding sample. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Synthetic Control Estimated of the Effects of California Paid Family Leave Program on Breastfeeding

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
Treat*CA	-0.01	17.99*	0.00	0.05**
	(0.01)	(9.04)	(0.01)	(0.02)

Note: This table presents the estimates from differences-in-differences models using the synthetic control approach with 2004 as the first treated year. Children drawn from the National Immunization Survey (2003 – 2014) are aggregated to the state-year level. Covariates include maternal race/ethnicity (the fraction of women in each state-year who are characterized by each race/ethnicity subcategory – white, black, Hispanic, and other), marital status (percentage of women married), completed education (percentage of women with each respective level of education - less than high school, high school, some college, and a college degree or higher), age at child's birth (percentage of women who are younger than 20, 20-29, or 30 +), household poverty status (percentage of women in each of four poverty categories - < 50 % of the Federal Poverty Level, FPL, 50-90% FPL, 100-200% FPL, and 200% FPL +), child parity, child gender, maternal immigration status, and child's year of birth. State and year fixed effects are omitted, as these are accounted for when estimating the synthetic control weights. All models have 26 observations (13 for California, 13 for synthetic California over the entire period of inquiry). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: DDD Estimates of PFL on Breastfeeding

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
Treat*CA*high LFP	-0.03	57.79***	0.16***	0.04
	(0.02)	(17.17)	(0.04)	(0.04)
Treat*CA*high Emp	0.02	25.82*	0.08***	0.03
	(0.02)	(12.69)	(0.01)	(0.03)

Note: This table presents the estimates from difference-in-difference-in-difference models using the synthetic control approach. Children drawn from the National Immunization Survey (2003 – 2014) are observed at the state level. The third interaction term indicates whether the respondent's county is above or below the median county maternal labor force participation (LFP) and county maternal employment rates relative to the median pre-law rates. Covariates are the same as in table 3. Emp = employment rate, LFP = labor force participation. State and year fixed effects are omitted, as these are accounted for when estimating the synthetic control weights. All models have 26 observations (13 for California, 13 for synthetic California over the entire period of inquiry). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

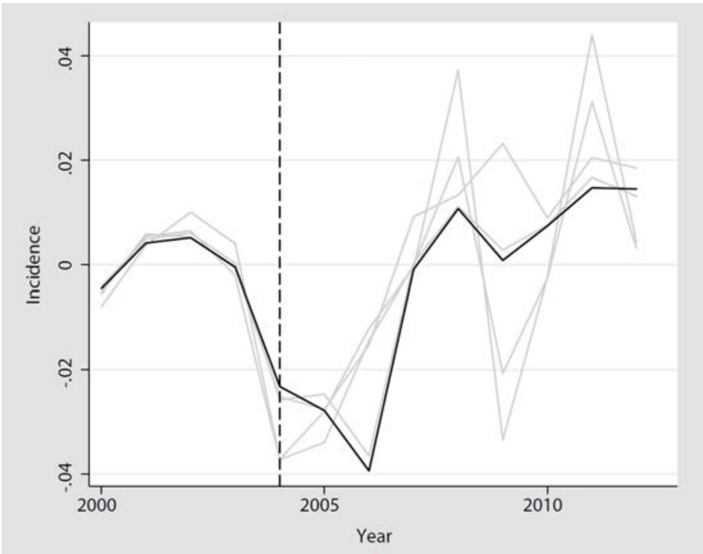
Table 5: Repeated Split Sampling (RSS) Estimates of PFL Effects on Breastfeeding

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
Low	0.01	0.00	0.01	0.00
	(0.02)	(6.42)	(0.01)	(0.02)
Medium	0.01	8.58	0.01	0.06***
	(0.02)	(10.18)	(0.01)	(0.02)
High	0.03***	15.58**	0.03***	0.04***
	(0.01)	(7.49)	(0.01)	(0.02)
Reps	100	100	100	100
Controls	x	x	x	x
State FE	x	x	x	x
Year FE	x	x	x	x

Note: This table presents the estimates from RSS models for models drawn from the same data source and containing the same covariates as in Table 3. Models are stratified by maternal propensity to breastfeed (low, medium, high). FE= fixed effects. All models have 52 observations. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

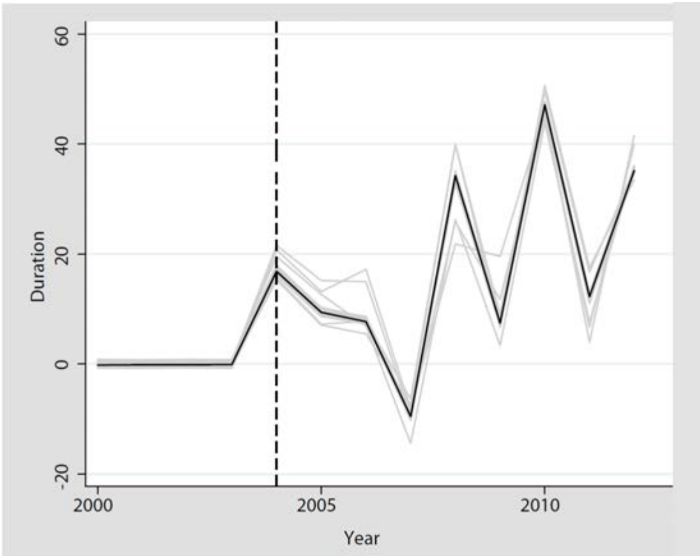
Appendix

Figure A1: Breastfeeding Incidence, Leave-one-out test



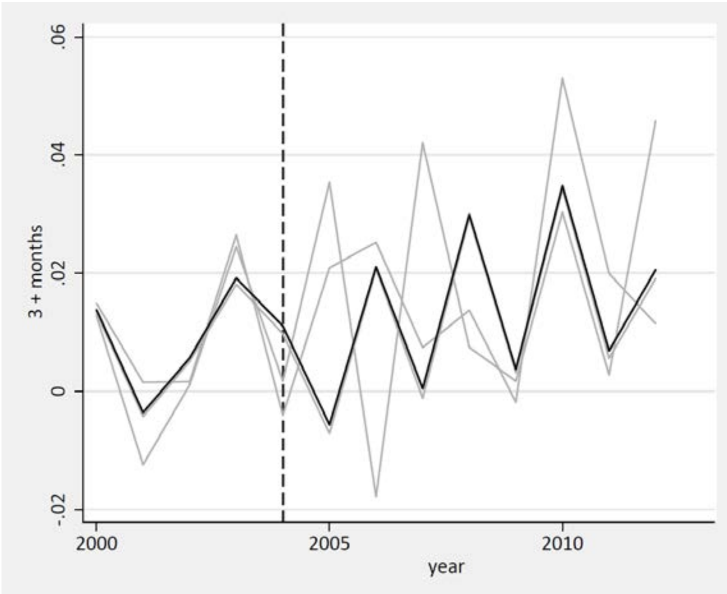
Note: See note on figure 1. Each line represents the treatment effect on breastfeeding incidence minus the synthetic treatment effect for our primary model with one state removed from each model. Though California remains the treated group, the control group is always n-1 states, with replacement. The black line represents our preferred specification with all states included in the donor pool, while the grey lines are the same effect estimated with n-1 states rotating out of the donor pool. The data source and covariates are the same as in figure 1. *** p<0.01, ** p<0.05, * p<0.1.

Figure A2: Breastfeeding Duration, Leave-one-out test



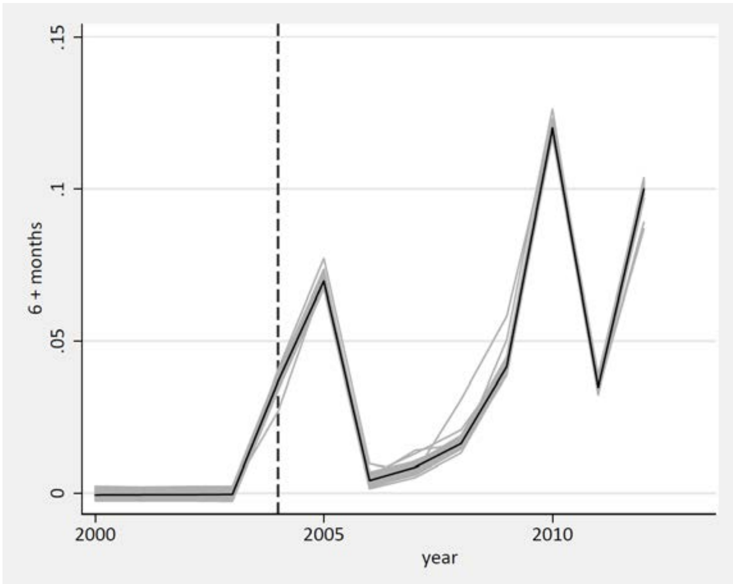
Note: See note on figures 1, 2 and A2. Each line represents the treatment effect on breastfeeding duration minus the synthetic treatment effect for our primary model with one state removed from each model. Though California remains the treated group, the control group is always n-1 states, with replacement. The black line represents our preferred specification with all states included in the donor pool, while the grey lines are the same effect estimated with n-1 states rotating out of the donor pool. The data source and covariates are the same as in figures 1 and 2. *** p<0.01, ** p<0.05, * p<0.1.

Figure A3: Breastfeeding 3+ months, Leave-one-out test



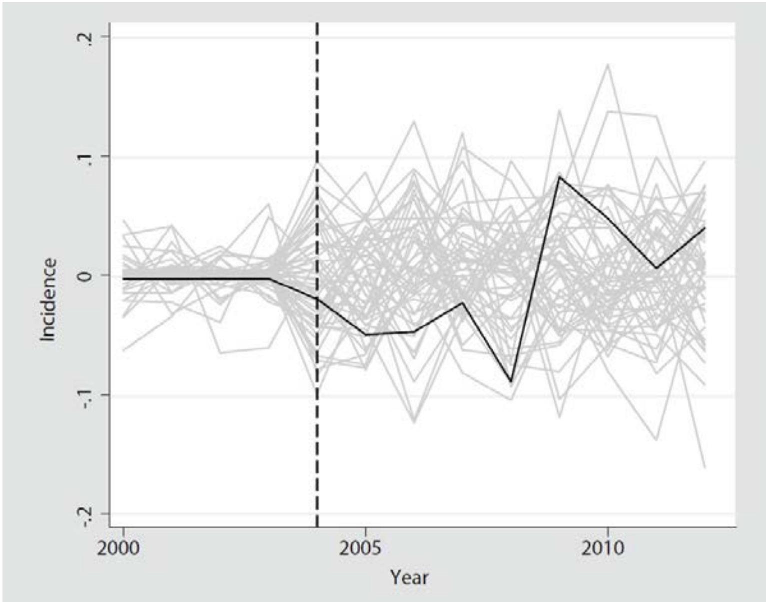
Note: See note on figures 1, 2 and A2. Each line represents the treatment effect on breastfeeding duration minus the synthetic treatment effect for our primary model with one state removed from each model. Though California remains the treated group, the control group is always n-1 states, with replacement. The black line represents our preferred specification with all states included in the donor pool, while the grey lines are the same effect estimated with n-1 states rotating out of the donor pool. The data source and covariates are the same as in figures 1 and 2. *** p<0.01, ** p<0.05, * p<0.1.

Figure A4: Breastfeeding 6+ months, Leave-one-out test



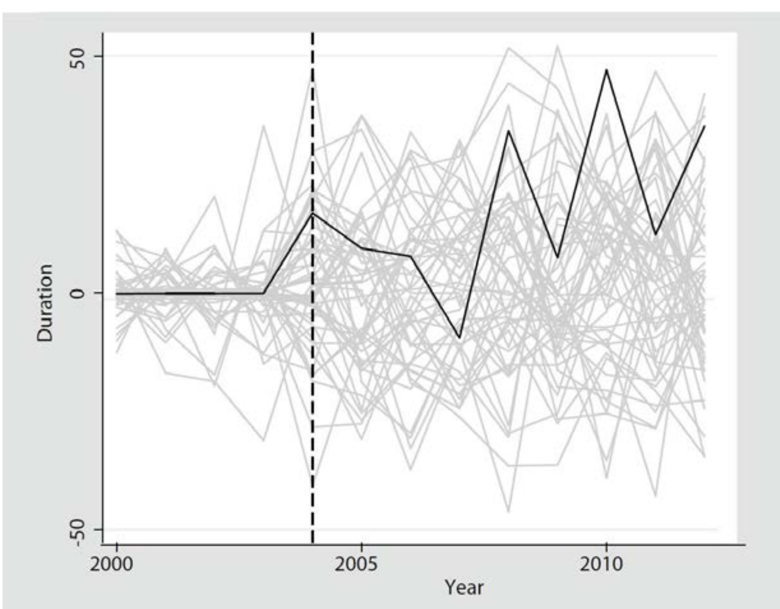
Note: See note on figures 1, 2 and A2. Each line represents the treatment effect on breastfeeding duration minus the synthetic treatment effect for our primary model with one state removed from each model. Though California remains the treated group, the control group is always n-1 states, with replacement. The black line represents our preferred specification with all states included in the donor pool, while the grey lines are the same effect estimated with n-1 states rotating out of the donor pool. The data source and covariates are the same as in figures 1 and 2. *** p<0.01, ** p<0.05, * p<0.1.

Figure A5: BF Incidence, PFL Placebo in-space tests



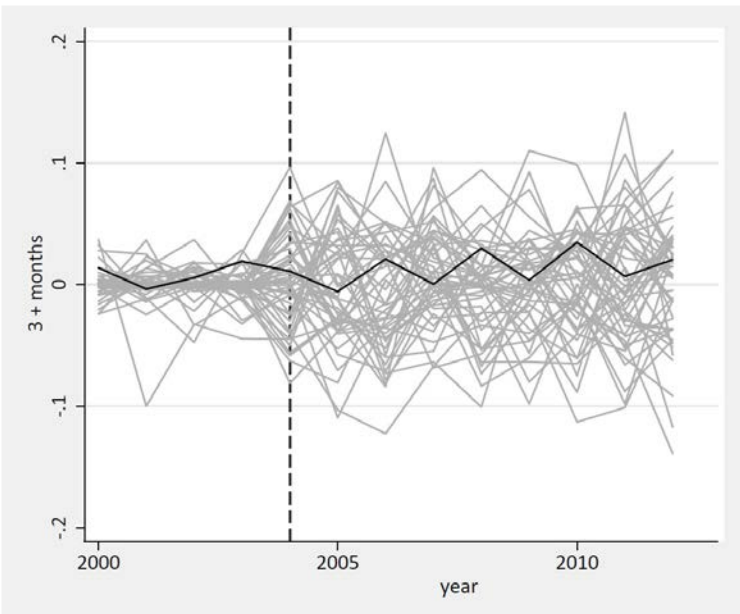
Note: See note on Figure 1. Each line represents the treatment effect for each state. The solid black line represents California, while the grey lines represent the same effect treating all other states than California as the treated state. Data source and covariates are the same as in Figure 1. *** p<0.01, ** p<0.05, * p<0.1.

Figure A6: BF Duration, PFL placebo in-space tests



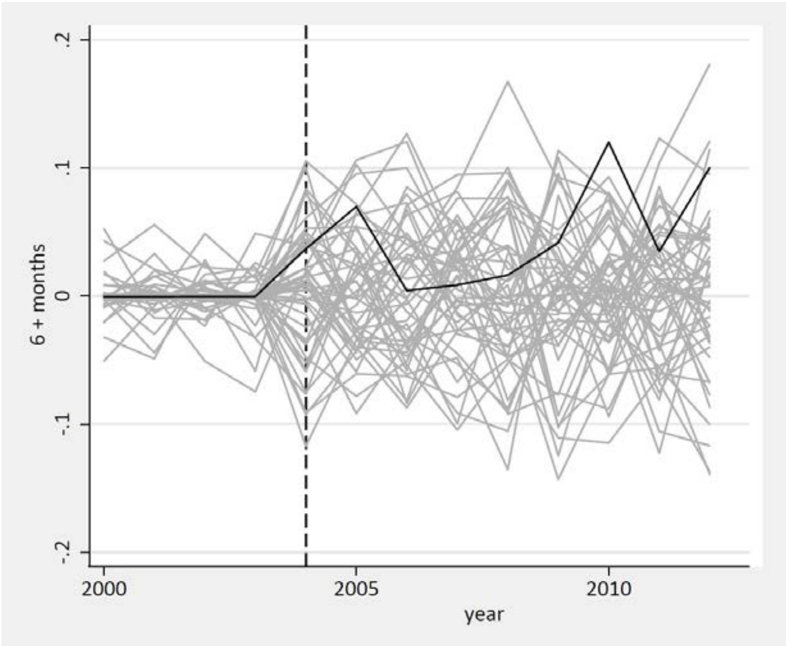
Note: See note on Figures 1 and 2. h line represents the treatment effect for each state. The solid black line represents California, while the grey lines represent the same effect treating all other states than California as the treated state. Data source and covariates are the same as in Figure 1. *** p<0.01, ** p<0.05, * p<0.1.

Figure A7: BF 3 + months, PFL placebo in-space tests



Note: See note on Figures 1 and 2. Each line represents the treatment effect for each state. The solid black line represents California, while the grey lines represent the same effect treating all other states than California as the treated state. Data source and covariates are the same as in Figure 1. *** p<0.01, ** p<0.05, * p<0.1.

Figure A8: BF 6 + months, PFL placebo in-space tests



Note: See note on Figures 1 and 2. Each line represents the treatment effect for each state. The solid black line represents California, while the grey lines represent the same effect treating all other states than California as the treated state. Data source and covariates are the same as in Figure 1. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Synthetic Control Weights (preferred specifications)

State FIPS	Ever Breastfed	Breastfeeding Duration	3 + months	6+ months
Alabama	0.000	0.005	0.001	0.000
Alaska	0.290	0.007	0.000	0.000
Arizona	0.000	0.006	0.003	0.000
Arkansas	0.000	0.003	0.001	0.000
Colorado	0.000	0.004	0.004	0.000
Connecticut	0.000	0.012	0.002	0.000
Delaware	0.000	0.004	0.002	0.000
DC	0.000	0.006	0.000	0.000
Florida	0.000	0.004	0.003	0.000
Georgia	0.000	0.004	0.001	0.000
Hawaii	0.000	0.218	0.003	0.000
Idaho	0.000	0.007	0.001	0.000
Illinois	0.000	0.006	0.012	0.000
Indiana	0.000	0.134	0.005	0.000
Iowa	0.000	0.003	0.004	0.000
Kansas	0.000	0.005	0.002	0.000
Kentucky	0.000	0.002	0.001	0.000
Louisiana	0.000	0.002	0.002	0.000
Maine	0.000	0.157	0.002	0.000
Maryland	0.000	0.004	0.002	0.000
Massachusetts	0.000	0.005	0.266	0.000
Michigan	0.000	0.003	0.022	0.000
Minnesota	0.000	0.010	0.002	0.000
Mississippi	0.000	0.002	0.001	0.000
Missouri	0.000	0.005	0.003	0.000
Montana	0.000	0.006	0.004	0.000
Nebraska	0.000	0.004	0.001	0.000
Nevada	0.000	0.010	0.003	0.000
New Hampshire	0.000	0.017	0.001	0.000
New Mexico	0.000	0.006	0.001	0.382
New York	0.000	0.005	0.004	0.000
North Carolina	0.000	0.003	0.005	0.000
North Dakota	0.000	0.004	0.002	0.000
Ohio	0.000	0.004	0.002	0.000
Oklahoma	0.048	0.002	0.001	0.000
Oregon	0.000	0.203	0.000	0.000

Pennsylvania	0.000	0.003	0.006	0.000
Rhode Island	0.000	0.005	0.003	0.000
South Carolina	0.000	0.008	0.001	0.180
South Dakota	0.000	0.009	0.003	0.000
Tennessee	0.000	0.002	0.002	0.000
Texas	0.000	0.004	0.001	0.000
Utah	0.253	0.013	0.441	0.263
Vermont	0.000	0.048	0.002	0.000
Virginia	0.000	0.006	0.005	0.000
Washington	0.409	0.008	0.000	0.000
West Virginia	0.000	0.005	0.164	0.175
Wisconsin	0.000	0.003	0.001	0.000
Wyoming	0.000	0.004	0.003	0.000

Table A2: Robustness check: alternative covariates from regression residuals

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
A: Primary Models				
Treat*CA	-0.01	15.96	0.01	0.05**
	(0.01)	(9.94)	(0.02)	(0.02)
B: Macro controls				
Treat*CA	-0.01	12.46	0.00	0.04*
	(0.01)	(9.95)	(0.01)	(0.02)
C: Employment controls				
Treat*CA	-0.01	14.91	0.00	0.04**
	(0.02)	(9.60)	(0.01)	(0.02)
D: All controls				
Treat*CA	-0.01	12.25	0.00	0.04*
	(0.01)	(9.82)	(0.01)	(0.02)

Note: See note on Tables 1 and 2. This table presents synthetic control estimates from DD models using the synthetic control approach with 2004 as the first treated year. Unlike the results in table A$, rather than include the covariates in the weight estimation stage, we first estimate OLS regressions with each set of covariates, using the residuals from said regressions in the weight estimation stage. Children are drawn from the National Immunization Survey (2003 – 2014) are aggregated to the state-year level. Covariates in panel A include maternal race/ethnicity (the fraction of women in each state-year who are characterized by each race/ethnicity subcategory – white, black, Hispanic, and other), marital status (percentage of women married), completed education (percentage of women with each respective level of education - less than high school, high school, some college, and a college degree or higher), age at child's birth (percentage of women who are younger than 20, 20-29, or 30 +), household poverty status (percentage of women in each of four poverty categories - < 50 % of the Federal Poverty Level, FPL, 50- 90% FPL, 100-200% FPL, and 200% FPL +), child parity, child gender, maternal immigration status, and child's year of birth. Panel B includes those in panel A, as well as the log of the state population, per capita income, minimum wage, and fraction of welfare recipients. Covariates in panel C those in panel A, as well as include employment rate of women with infants, ratio of women employed relative to men, fraction of full-time women employed, and the fraction of female-dominated industries. Covariates in all panels include. Panel D includes all controls. All models have 26 observations (13 for California, 13 for synthetic California over the entire period of inquiry). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: Synthetic Control Estimates Using 2005 as First Treatment Year

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
Treat*CA	0.00	15.03	0.01	0.04*
	-0.01	-9.97	-0.01	-0.02

Note: See note on Tables 1 and 2. This table presents the estimates from DD models using the synthetic control approach with 2005 as the first treatment year. Otherwise, data and covariates are the same as in Tables 1 and 2. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Synthetic Control Estimates with Alternative Year Measures

	(1)	(2)	(3)	(4)
	Ever Breastfed	Breastfeeding Duration	3 months +	6 months +
A: No 2004				
Treat*CA	0.00	18.12*	0.01	0.05**
	(0.01)	(9.66)	(0.01)	(0.02)
B: Fiscal year				
Treat*CA	0.01	14.55*	0.02	0.05*
	(0.02)	(7.85)	(0.01)	(0.03)

Note: See note on Tables 1 and 2. This table presents the estimates from DD models using the synthetic control approach employing a fiscal year definition (July – June) in columns 1-4, and omitting 2004 altogether in columns 5-8. Otherwise, data and covariates are the same as in Tables 1 and 2. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Synthetic Control treatment effect estimates

	Ever Breastfed			Breastfeeding Duration			3+ months			6+ months		
	CA	Synth CA	Trt. Effect	CA	Synth CA	Trt. Effect	CA	Synth CA	Trt. Effect	CA	Synth CA	Trt. Effect
2000	0.84	0.84	-0.01	255.95	253.37	2.58	0.82	0.81	0.01	0.57	0.57	0.00
2001	0.88	0.87	0.01	253.71	252.32	1.39	0.78	0.78	0.00	0.59	0.59	0.00
2002	0.91	0.89	0.01	233.30	237.06	-3.77	0.77	0.77	0.01	0.55	0.55	0.00
2003	0.85	0.86	-0.01	297.61	294.18	3.43	0.82	0.81	0.02	0.60	0.60	0.00
2004	0.91	0.75	0.16	284.36	248.64	35.72	0.80	0.79	0.01	0.63	0.59	0.04
2005	0.90	0.79	0.11	264.42	194.48	69.94	0.85	0.85	-0.01	0.66	0.59	0.07
2006	0.84	0.87	-0.04	300.15	232.55	67.60	0.81	0.79	0.02	0.62	0.62	0.00
2007	0.87	0.74	0.14	289.82	253.02	36.80	0.82	0.82	0.00	0.61	0.60	0.01
2008	0.89	0.87	0.01	300.53	275.51	25.02	0.85	0.82	0.03	0.66	0.65	0.02
2009	0.88	0.89	-0.01	257.98	263.68	-5.70	0.85	0.85	0.00	0.69	0.65	0.04
2010	0.92	0.88	0.04	311.75	257.62	54.13	0.88	0.85	0.03	0.75	0.63	0.12
2011	0.94	0.91	0.03	290.50	261.82	28.68	0.86	0.85	0.01	0.67	0.64	0.03
2012	0.96	0.90	0.07	335.05	257.02	78.03	0.88	0.86	0.02	0.74	0.64	0.10

Note: See note on tables 1 and 2. This table presents the synthetic control estimates for California and Synthetic California. The ‘treatment effect’ column is the difference between the California and Synthetic California estimates. Data and covariates are the same as in tables 1 and 2.