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What is the effect of Salary History Bans on the Employment Status of Mothers?

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1 Introduction: Motherhood & the Effect of Salary History on Employment Outcomes

Salary History Ban (SHB) is a recent policy tool which prohibits employers from acquiring and using salary history during any stage of the hiring process. Seventeen states across the United States have implemented SHBs. [1] Salary history, known to be used by employers as a signal of productivity, can exacerbate the gender pay gap; since compensation history for women is, on average, less than that of men [2], employer reliance on compensation history for setting offers of pay can perpetuate lower pay for women. One of the main policy objectives of SHBs, therefore, is to reduce the gender pay gap by addressing this particular source of pay inequity.

Indeed, Salary History Bans have been found to narrow the gender pay gap, and this is largely due to increased earnings for women (Bessen, Meng, and Denk, 2021; Hansen and McNichols, 2020; Sinha, 2022). [3] As wages for women increase due to the implementation of SHBs, we

[1] As of September 2023. In addition, 5 states have adopted public sector Salary History Bans, affecting only employers in state or local government agencies within that state. I do not consider these states in my analysis due to the different policy environment, and, instead, focus on the effects of "All Employer" Salary History Bans. Moving forward, I use "Salary History Bans" or "SHBs" as shorthand for All-employer Salary History Bans.

[2] This is true for various reasons, including statistical and taste-based discrimination, negotiation gaps, unequal opportunity for advancement, and motherhood penalty.

[3] In addition, Mask (2023) finds that increase earnings for people who have scarred wages – a result of

can expect women to respond by increasing their labor supply. In their employment analysis of the California Salary History Ban, however, Hansen and McNichols (2020) find that there is almost no effect of the California SHB on the employment status of women.

Still, the literature documents that women exhibit striking changes in labor market participation at various phases of their life cycle. Women and men exhibit very similar labor supply behavior before parenthood, but this changes drastically once children arrive. This point in the life cycle initiates a departure from the labor force for some women or part-time work arrangements, while other women choose to remain in the labor force. This heterogeneity is particularly pronounced among mothers when children are below school-age, when parents (usually mothers) face tradeoffs between earnings and high childcare costs. In fact, in their structural analysis, Apps, Kabátek, Rees, and van Soest (2016) find that labor supply of mothers with young children is particularly responsive to wages and cost of childcare. If this is the case, this group will likely respond to Salary History Bans' effect on women's wages by increasing their labor supply at greater rates than women on average. In this paper, I investigate whether this is the case and study the following question: What is the effect of Salary History Bans on the employment status of mothers?

In particular, I examine the effect of state-level Salary History Bans on the employment rate among mothers. Using the estimator for staggered implementation developed by Callaway and Sant'Anna (2021), I exploit the variation in SHB adoption and variation in SHB policy timing among adopting states. I use employment status data from the Current Population Survey's (CPS) Basic Monthly Files for the period of January 2010-March 2020, along with data on policy timing from an online human resources publication, to construct a pseudo-, state-year-month panel of employment rates (outcome variable) and SHB adoption dates (policy variable). In my preferred sample, I regress employment on individual-level education attainment and age before aggregating to the state-year-month level in order to

starting their careers during a recession. Overall, the evidence shows that SHB, as a policy, induces employers to offer higher pay to workers with less competitive salary histories.

residualize the employment rate for individual selection into employment based on education or age.

These data consist of observations from the period of January 2010 through February of 2020. I do not include any observations from the period of March 2020 through the present, as the effects of the COVID-19 pandemic on labor market conditions are profound and longstanding. It is difficult to treat pre-pandemic and post-pandemic labor market behaviors, especially those of mothers, as though they are the same. For this reason, I study the pre-pandemic effect of Salary History Bans, alone.

Due to the variation in SHB policy timing from state to state and the sample cutoff date of March 2020 (see Figure 1) in the pseudo-panel, few treated states are observed for more than a handful of months in the post-implementation period. In order to overcome any policy-timing related biases associated with this unbalanced panel, I balance the panel; each balanced panel contains only those treated states with a given number of post-implementation period observations. In my preferred balanced panel, I include only the five earliest SHB implementing states, which allows me to observe each treated state in 19 post-implementation periods. [4]

Once I obtain estimates from the method developed by Callaway and Sant'Anna, I aggregate the group-time average treatment effects to an event study-style plot as well as an overall average treatment effect. I find little to no effect of Salary History Bans on the overall maternal employment rate; using my preferred sample (employment rate as the outcome, residualized at the individual level for education and age), I find that SHBs increase the employment rate for mothers by 0.4 percentage points.

Among all mothers, those with young children are known to be particularly sensitive to wages (and the cost of childcare), as demonstrated in a structural analysis by Apps, Kabátek, Rees, and van Soest (2016). These findings suggest that it is fruitful to understand whether

[4] In the appendix, I include the results for all balanced panels as well as the unbalanced panel.

there are differential impacts of Salary History Bans on mothers based on the ages of their children. I estimate the effect of SHBs on the maternal employment rate for the following four categories of women: (1) mothers of any-age children, (2) mothers who have at least one child under 5, (3) mothers whose children are all under 5, and (4) mothers whose children are between 5 and 18 years old. I find that Salary History Bans have the largest effect on mothers with at least one child under 5; for this group, SHBs increase the employment rate by 2.12 percentage points. For other groups of mothers, meanwhile, I find no effect of SHBs on employment rates.

Increased wages for women raises their opportunity cost of not being employed. As I discuss above, the labor supply of mothers should increase in response to higher wages. Moreover, higher wages in the market increase the opportunity cost of leaving the labor force. Therefore, mothers who are contemplating leaving the labor force are now inclined to remain, so as to not forgo these higher earnings. My findings of increased employment for mothers of young children is consistent with both of these mechanisms.

There is a growing literature documenting the effects of Salary History Bans on the gender pay gap. In their paper, Agan, Cowgill, and Gee (2021) find experimental evidence that employer compliance with SHBs depends heavily on voluntary disclosure behavior on part of job candidates. Moreover, they find that the voluntary disclosure behavior is highly correlated to gender of the job candidate. Still, there is empirical evidence that SHBs mitigate gender pay gap. Difference-in-differences studies find that the earnings gap is reduced, overall, across the United States (Sinha, 2022; Bessen, Meng, and Denk, 2021). Hansen and McNichols (2020) employ a synthetic control study to understand whether California's SHB mitigates the gender pay gap; they find that California's gender pay gap is narrowed after the SHB and that this largely due to rising wages for women. In extending the outcome to employment effects of Salary History Bans, this paper builds on the previous findings. While gender pay gap analyses largely capture the effect of SHBs on employer behavior, my paper analyzes the effects of this narrowing gender pay gap on employment, focusing on the workers' response.

In a heterogeneity analysis of their paper, Hansen and McNichols (2020) find that the shrinking gender pay gap is largely driven by wages for women whose children are all older than 5. My findings offer complementarity to these findings; the earnings effects, Hanson and McNichols find, are largest for mothers at a later phase in their life cycle, when their labor supply is less responsive to changes in the wage.

The remainder of the paper is arranged as follows: in Section 2, I provide a background on Salary History Ban policy; in Section 3, I discuss the data and methodology used in my analyses; and in Section 4, I discuss results.

2 Background on Salary History Bans

Salary History Ban was introduced to the public and political arena around 2015-2016 as a corporate policy adopted by a few companies. It gained momentum in smaller administrative units, until the first state, Massachusetts, adopted one in 2016. The main objective of this policy tool is based on the hypothesis that, if future salary depends on previous salary, and if women start with lower pay than men, then the gender pay gap will be perpetuated and exacerbated through career trajectories.

There are two main categories of state-level SHBs. One is a Public Sector SHB or Public SHB, enacted through executive order by the governor of a state. The Public SHB prohibits only state and local-government employers from inquiring about a candidate's salary history.[5] All-Employer SHBs, on the other hand, prohibit any employer in a state from asking for salary history information from job candidates. In this paper, my focus is on the All-Employer SHBs, and whether they impact maternal employment. As such, I will

[5] Note, however, with any kind of Salary History Ban, employers still have access to a candidate's resume and job history.

exclude from analysis the Public SHB states, as these states have enacted a different policy with potentially ambiguous effects on labor market outcomes in both public and private sectors.

All-Employer SHBs are a popular policy, adopted by 17 states thus far. Implementation dates are given in Figure 1; the earliest implementer is Oregon, in October 2017. Since I do not include any analysis beyond March 2020, the last implementers before this date are New York and New Jersey, whose effective dates are both January 2020. This means I never observe four states – Colorado, Maryland, Nevada, and Rhode Island – when they have implemented SHB in my analyses.

One state, Wisconsin, has a policy in effect that prohibits any entity within the state from enacting an SHB. Because this policy continues the status quo of allowing employers to ask for salary history, In my analyses, I consider this state as part of the control or not-treated group.

2.1 Conceptual Framework: How can Salary History Bans affect the employment status of mothers?

If employers no longer have access to a job candidate's salary history, there are potentially ambiguous effects when it comes to employment status of mothers.

First, following the experimental findings of Barach and Horton (2021), we can expect employers to consider a wider pool of applicants and conduct a more thorough interview process when their access to job candidates' salary history is restricted. As the authors find, employers without salary history information interview and, subsequently, hire candidates whose previous salaries are, on average, lower than candidates interviewed and hired by employers who have access to salary histories. If employers at large behave like the employers in this experiment in the wake of SHBs, this can improve chances of employment for mothers, who have lower previous salaries due to discrimination, career interruptions, and other reasons.

However, the statistical discrimination literature forces us to consider the case where employers assume that women have lower previous salaries and make lower offers to female job candidates. This can induce women to decline offers lower than their willingness to accept; if the menu of job offers is, on average, worse after SHB implementation, then mothers might face worse employment outcomes relative to when compensation history was available to employers.

I find evidence that mothers, who have lower salary histories than most job candidates, are more likely to be employed after SHB implementation. This is consistent with the framework set by the experimental findings of Barach and Horton (2021).

3 Data & Methodology

3.1 Data

For information on employment status and other individual-level observables, I use data from the Current Population Survey's Basic Monthly Files (CPS) from the period of January 2010-March 2020. CPS is an unbalanced panel of individual-level data. Each individual is surveyed for four consecutive months, then they are surveyed in the same four consecutive calendar months in the following year. Each individual, therefore, can be observed in the CPS for a maximum of 8 observations. However, I treat these data as a cross-section; furthermore, I use these data from the CPS to build a state-year-month panel, which I discuss later in this section.

I limit the CPS sample to individuals who are between 22-64 years of age and individuals who are mothers; I define mothers in this dataset based on whether the respondent identifies as female and whether they have indicated that they have any of their own children (18 years and under) living with them. The CPS also asks whether the respondent is employed, which I use as the outcome in my analysis. [6] Furthermore, CPS includes information on educational attainment and age, which I use as individual-level controls.

CPS also collects detailed information on the number and age of a respondent's own children in the household. Using this, I explore the SHB policy effect on four different samples of mothers: (1) "Any Age" refers to the sample of mothers with children of any age, (2) "Some Under 5" refers to the sample of mothers with at least one child under 5, (3) "All Under 5" refers to the sample of mothers whose children are all under 5, and, finally, (4) "5 and above" refers to the sample of mothers whose children are all between 5-18 years of age. I employ 5 as the main cutoff age of children for this analysis, since most 5-year-old children qualify to attend public Kindergarten, allowing the parent(s) to plan for employment . Thus, mothers whose last children are about 5 years old may be finishing their childcare duties and getting ready to return to work.

Although the CPS has a panel structure, I will be treating these data as a cross-section. Furthermore, I will aggregate the individual-level employment variable to a state-year-month panel. In the state-year-month panel, therefore, the outcome variable represents the employment rate for a state in a given year-month. For my main specification, prior to aggregating the data, I will regress employment on the individual characteristics and then aggregate residuals of the employment to the state-year-month level. This measure of the employment rate has been residualized for individual-level educational attainment and age.

Policy timing data, including data on type of SHB, SHB adoption dates, and SHB rollout dates, comes from a human resouces publication online called HR Dive. [7] In this way, I obtain a state-year-month panel with employment rate (or residualized employment rate) as the outcome and SHB policy implementation time for each state.

I will discuss the methodology used on these state-year-month panel data in the next direc-

[6] For this analysis, I consider responses of "Armed Forces," "Unemployed," and "Not in Labor Force" as people who are not employed; anyone who indicates that they were at work the previous week or that they have a job, but didn't work last week, is counted in my analysis as employed.

[7] include link

tion.

3.2 Methodology

Salary History Ban is a state-level policy tool with varied timing based on implementation dates chosen by each state's policy-making body. Since SHBs are rolled out across states on a staggered schedule, I use the method for staggered-implementation policy adoption developed by Callaway and Sant'anna in their 2021 paper.

In accordance with the parallel trends assumption that Callaway and Sant'anna (2021) provide in their paper, I use the group of not-yet-treated and never-treated states as the comparison group for the treated states.

Callaway and Sant'anna's method first computes, for each time period t, an average treatment effect on the treated for each cohort of states that implemented an SHB in time period g ("SHB cohort g"). [8] Based on the potential outcomes framework, then, each individual ATT can be represented in the following way:

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_g = 1]$$

In the above equation, $Y_t(g)$ represents the employment rate in time period t for a state in SHB cohort g. $Y_t(0)$ represents the employment rate in time period t for never- or not-yet-treated states. This difference is averaged for all such states in SHB cohort g. This is called the group-time average treatment effect, and this is the "building block" for all the aggregate effects presented in this paper.

Using the group-time average treatment effects, I present both an event plot to show the dynamic treatment effects and an overall ATT, which can be interpreted as the two-period, "before-and-after" treatment effect of the SHB policy.

[8] Moving forward, I will call states that first implement an SHB in time period g "SHB cohort g." For those states that are never- or not-yet-treated, g takes a value of 0.

For the event plot, the individual ATTs are aggregated as follows:

$$\theta_D(e) := \sum_{t=2}^{\tau} 1\{g + e \leq \tau\} ATT(g, g + e) P(G = g | G + e \leq \tau)$$

For the overall ATT, first, each group's average effect of participating in the treatment is given by:

$$\theta_S(g) = \frac{1}{\tau - g + 1} \sum_{t=2}^{\tau} 1\{g \leq \tau\} ATT(g, t)$$

This is further aggregated to an overall treatment effect on the treated, as follows:

$$\theta_S^O(g) := \sum_{t=2}^{\tau} \theta_S(g) P(G = g)$$

Callaway and Sant'anna recommend using a balanced panel for their method. I have balanced the panel in three ways, represented by Figure 2. The balanced panel definitions are as follows:

1. All Implementers: When I balance the panel to include all 13 treated states available to me in the sample, I have 1 post-period observation per treated state.

2. 11 Earliest Implementers: When I balance the panel to include only the 11 earliest implementers, this allows me to observe each state 5 times in the post-implementation period. In this panel, the last two implementers (New Jersey and New York) are dropped from the sample.

3. 5 Earliest Implementers: When I balance the panel to include only the 5 earliest implementers, this allows me to observe each state 19 times in the post implementation-period. The last 8 implementers are dropped from the sample.

In each of the above panels, the comparison group consists of those states which are never-treated and those states which are eventually treated (Colorado, Nevada, Rhode Island, and Maryland). For my main analysis, I report estimates, graphs, and other findings using the "preferred panel" of 5 Earliest Implementers. However, I will report all findings for all the above panels, including an unbalanced version using all implementers, in the Appendix.

4 Results

I begin the analysis by showing descriptive statistics using the underlying CPS data. In Table 1, I report the mean and standard deviations of the outcome and control variables for the four categories of mothers based on the ages of their children. Comparing employment rates across groups, "Some Under 5" group of mothers has the lowest employment rate, still lower than mothers whose children are "All Under 5." This is consistent with the hypothesis that mothers with some children under 5 includes mothers whose last children are just about old enough to start entering public Kindergarten, relieving these mothers of their childcare duties and readying them to re-enter the workforce. SHBs and other policies that could make re-entry easier may, therefore, be most salient for this group of mothers.

The educational attainment distribution is fairly similar across all groups – however, mothers whose children are "All Under 5" have the highest rates of Bachelors and Advanced degree attainment. It is difficult to attribute any differences in policy effects to educational attainment, but I will include education bins at the individual level regression in the preferred specification to obtain the residualized employment before aggregating to the state-year-month level. Age and number of children are correlated; I will use age bins at the individual level regression in the preferred specification to obtain the residualized employment before aggregating the state-year-month level. Marital status is also very similar across groups; again, it is unlikely that any differences in policy effects across groups can be attributed to marital status.

Figure 3 gives event plots based on the Callaway & Sant'anna estimators for the samples of mothers with children of any age as well as mothers with at least one child under 5. For the both samples, there appear to be no pre-trends. The policy effects are suggestive in Panel A, the sample of mothers with children of any age, but the policy effects are positive and statistically significant in months 8-12 for the sample of mothers with at least one child under 5.

I report the ATTs for mothers with children of any age in Table 2. In specification 1, I aggregate employment to the state-year-month level, such that the outcome is the employment rate for a state in a given year-month. In specification 2, I regress employment on education bins, then obtain the residual of employment before aggregating to the state-year-month level. In specification 3, I regress employment on education bins and age bins, obtain the residual of employment, and then aggregate to the state-year-month level. According to the results from specification 3, an All-Employer Salary History Ban results in a 0.406 percentage point increase in employment rate. While the effects are all positive, the standard errors are large and render the effects statistically insignificant.

In Table 3, I report the ATTs from specification 3, the preferred specification, for all four groups of mothers. The policy effect is strongest for the group of mothers with some children under 5. This ATT is significant at the 15% level. This result is consistent with the theory proposed earlier; this group consists of mothers whose last children are just under 5 and ready to enter the public school system. These mothers are at a point in their life-cycle when they are ready to work. For other groups, SHB does not appear to have a statistically significant effect on maternal employment rates.

[[Placeholder: talk about heterogeneity. Currently determining the size of each cell before aggregation to potentially justify using 2WFE for just the heterogeneous treatment effects tables]]

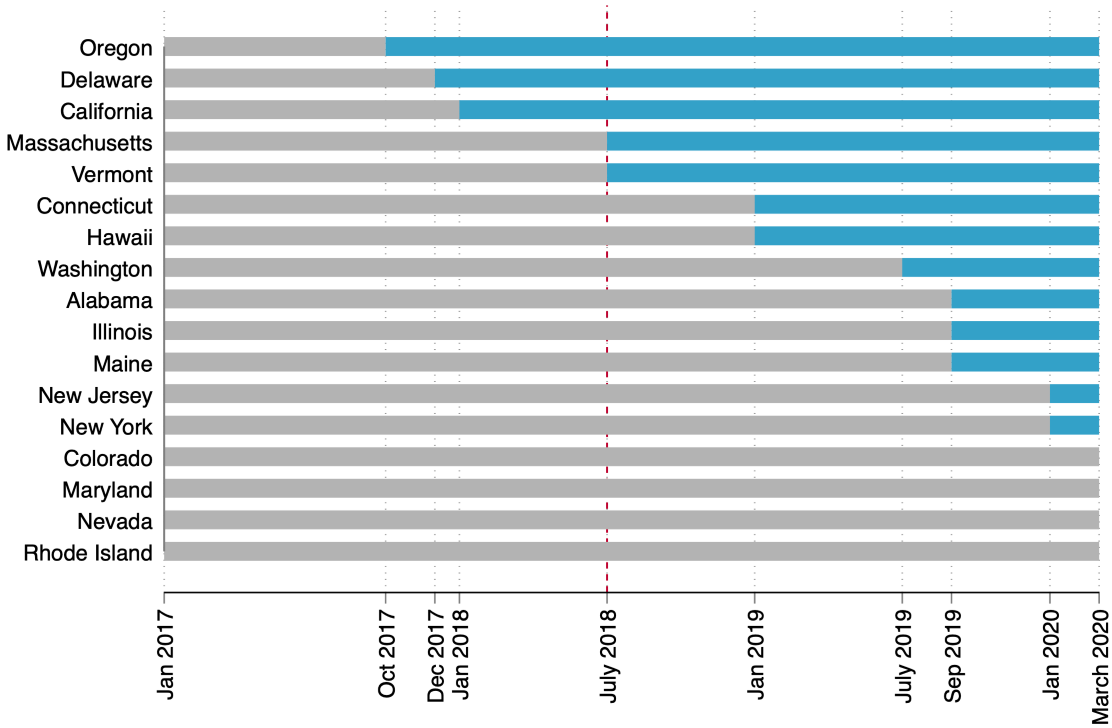
4.1 Robustness

In all the results I have reported, I have used both never-treated and not-yet-treated states as the control group. There is a potential concern that those states which are eventually treated (Colorado, Nevada, Rhode Island, and Maryland) are perhaps different from the states that never intend to adopt an All-Employer SHB at all. In their paper, Callaway and Sant'anna offer an alternate pre-trend condition in the case that eventually-treated and never-treated groups have these unobservable differences; they advise using the eventually-, or not-yet-treated units as a control. In this section, I will discuss the results from using the four eventually-treated states as the only control states in the analysis.

In Figure 4, I show the results from including only the eventually-treated group as the control group. Both Panel A and B, the samples of all mothers and mothers with some children under 5, respectively, show suggestive evidence that SHB having a positive effect on employment rates for mothers. Neither event plot displays any evidence of pre-trends. I report the ATTs for the robustness analysis in Table 6; neither estimate is statistically significant.

5 Figures and Tables

Figure 1: All-Employer Salary History Ban Policy Rollout



Source: HR Dive

The bars are gray when the policy is "off," or not yet implemented. The bars turn blue once the policy is effective in that state. The first effective date of any statewide SHB is October 2017 in Oregon. I will not include analyses beyond March 2020; therefore, Colorado, Maryland, Nevada, and Rhode Island, whose effective month-year is beyond this cutoff will only appear as control states in the analyses. The line through July 2018 defines the cutoff for the sample of the "5 Earliest Implementers" – that is, Oregon, Delaware, California, Massachusetts, and Vermont.

Figure 2: All-Employer Salary History Ban Implementation, as of 2023

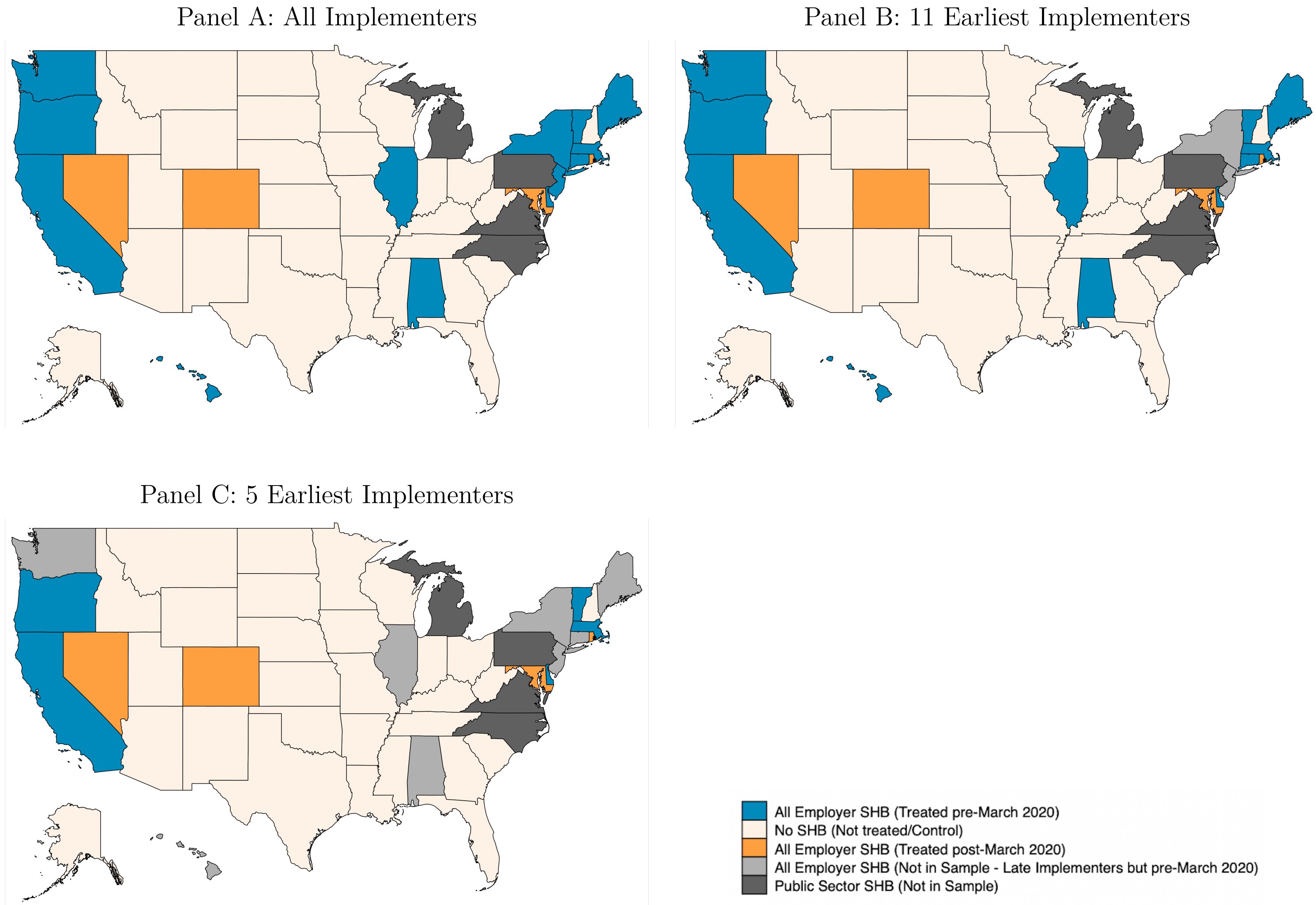


Table 1: Summary Statistics of Mothers: Heterogeneity by Age of Children

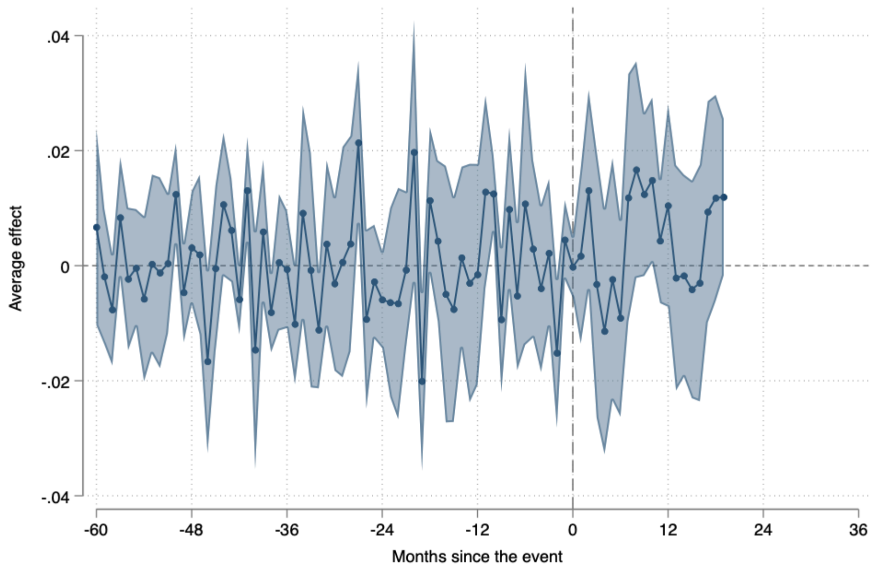
	Any Age		Some Under 5		All Under 5		5 and Above	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Employed	0.69	0.46	0.61	0.49	0.65	0.48	0.73	0.45
Education Bins								
HS, No Degree	0.10	0.29	0.10	0.29	0.06	0.24	0.10	0.29
HS or Equal	0.24	0.43	0.23	0.42	0.21	0.41	0.25	0.43
Some college	0.18	0.38	0.18	0.38	0.18	0.38	0.18	0.38
Associate Degree	0.12	0.33	0.11	0.32	0.11	0.31	0.13	0.33
Bachelors degree	0.33	0.47	0.35	0.48	0.40	0.49	0.33	0.47
Advanced degree	0.03	0.16	0.03	0.17	0.04	0.20	0.03	0.16
Age	38.66	8.53	32.39	6.26	30.70	6.14	42.09	7.61
Married	0.71	0.46	0.72	0.45	0.71	0.45	0.70	0.46

Source: CPS Basic Monthly Files, 2010-Mar 2020.

"Any Age" describes the full sample of mothers – that is, women with children 18 and under living in the household. "Some Under 5" describes the sample of mothers with at least one child under 5. "All Under 5" is the sample of mothers whose children are all under 5 years of age. "5 and Above" is the sample of mothers whose children are between 5 and 18 years of age. "Some Under 5" and "5 and Above" are mutually exclusive and, when pooled, yeilds the sample of mothers ("Any Age"). "All Under 5" is a subset of "Some Under 5." The number of observations in each of the four categories, respectively, are as follows; 906,427; 292,741; 128,366; 613,686.

Figure 3: Dynamic Effects of SHB on Employment Status of Mothers
5 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

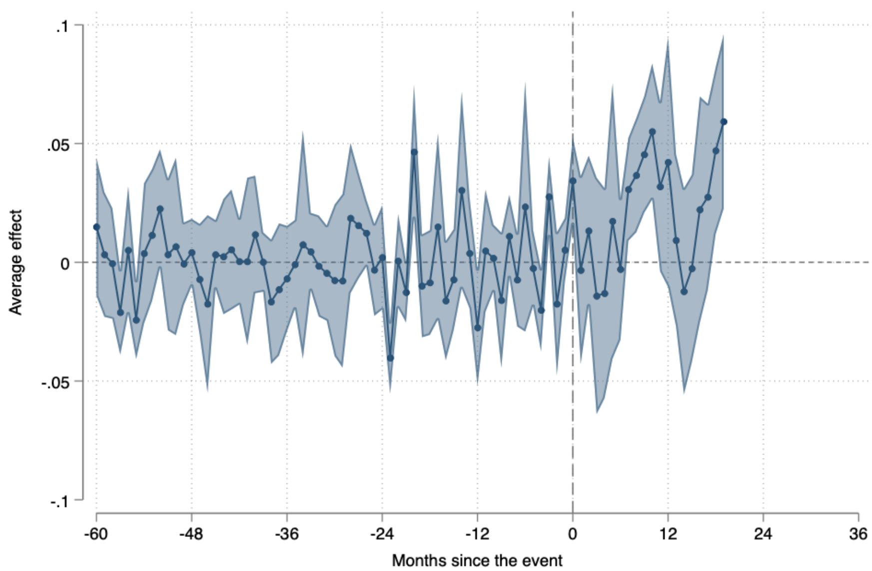


Table 2: Effect of SHB on Employment Status of Mothers
Mothers with Any Children in the Household, 2010-Mar 2020
5 Earliest Implementers
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1)	(2)	(3)
ATT	0.00304	0.00350	0.00406
	(0.00638)	(0.00529)	(0.00539)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Result in column (1) is an estimate obtained by aggregating employment and SHB policy variables to the state-year-month level and regressing employment on SHB. Result in column (2) is an estimate obtained by first regressing employment on education bins using individual level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable. Result in column (3) is obtained by first regressing employment on education bins and age bins using individual-level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable.

Table 3: Effect of SHB on Employment Status of Mothers
Heterogeneity by Age of Children, 2010-March 2020
Did Estimator from Calloway and Sant'anna, 2021

	(1)	(2)	(3)	(4)
	Any Age	Some Under 5	All Under 5	5 and Above
ATT	0.00406	0.0212	0.00977	-0.00440
	(0.00539)	(0.0138)	(0.0205)	(0.00670)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Each result is obtained by first regressing employment on education bins and age bins using individual-level observations, clustering standard errors at the state level; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then estimate the ATT using aggregated residuals, the policy variable, and the method put forward by Calloway and Sant'anna (2021). The headings indicate the sample used to obtain each result; samples are as described previously (see Table 1). The baseline employment rates for each of the four samples are as follows: (1) 0.70, (2) 0.63, (3) 0.68, and (4) 0.74.

Table 4: Effect of SHB on Employment Status of Mothers
Average Treatment Effects on the Treated by Education Level, 2010-March 2020
5 Earliest Implementers
Did Estimator from Calloway and Sant'anna, 2021

Panel A: Mothers with Children of Any Age						
	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS or Equal	Some College	Associate	Bachelors	Advanced
ATT	0.0249	0.0701*	-0.0430*	0.0918**	-0.00569	-0.0511
	(0.0270)	(0.0327)	(0.0189)	(0.0356)	(0.0256)	(0.0369)
Panel B: Mothers with at least One Child Under 5						
	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS or Equal	Some College	Associate	Bachelors	Advanced
ATT	-0.0475	0.219**	-0.00721	-0.0551	0.0443	-0.136**
	(0.0674)	(0.0820)	(0.0302)	(0.0382)	(0.0517)	(0.0495)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Each result is obtained by first regressing employment on age bins using individual-level observations, clustering standard errors at the state level; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then estimate the ATT using aggregated residuals, the policy variable, and the method put forward by Calloway and Sant'anna (2021). The headings indicate the sample used to obtain each result; (1) is those who have not completed high school, (2) is those who have completed high school or equivalent degree, (3) is those who have completed some college, but have not finished college, (4) is those who have an associate or 2-year degree, (5) is those who have completed a bachelors degree or equivalent, and (6) is those who have completed an advanced degree. In each of these samples, I include mothers with children of any age.

Table 5: Effect of SHB on Employment Status of Mothers
Heterogeneity by Spousal Status, 2010-March 2020
Did Estimator from Calloway and Sant'anna, 2021

Panel A: Mothers with Children of Any Age		
	(1)	(2)
	Not Married	Married
ATT	0.0209	0.0182
	(0.0158)	(0.0113)

Panel B: Mothers with at least One Child Under 5		
	(1)	(2)
	Not Married	Married
ATT	0.0704*	0.0282
	(0.0294)	(0.0286)

Standard errors in parentheses

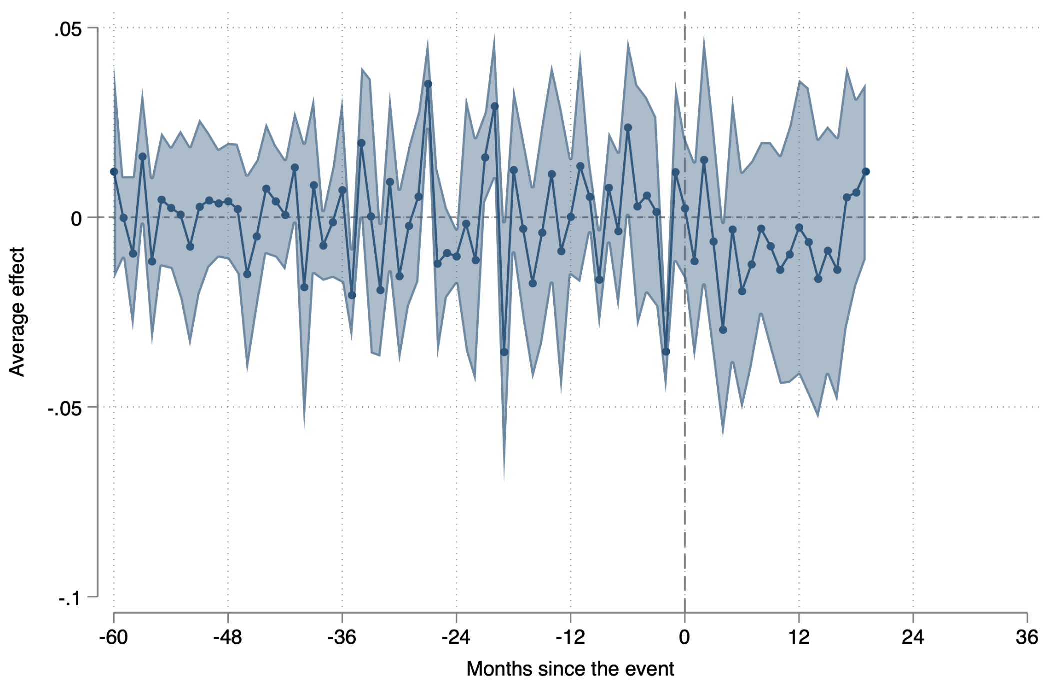
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Each result is obtained by first regressing employment on education bins and age bins using individual-level observations, clustering standard errors at the state level; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then estimate the ATT using aggregated residuals, the policy variable, and the method put forward by Calloway and Sant'anna (2021). The headings indicate the sample used to obtain each result; in each of these samples, I include mothers with children of any age.

Figure 4: Robustness
Dynamic Effects of SHB on Employment Status of Mothers
5 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

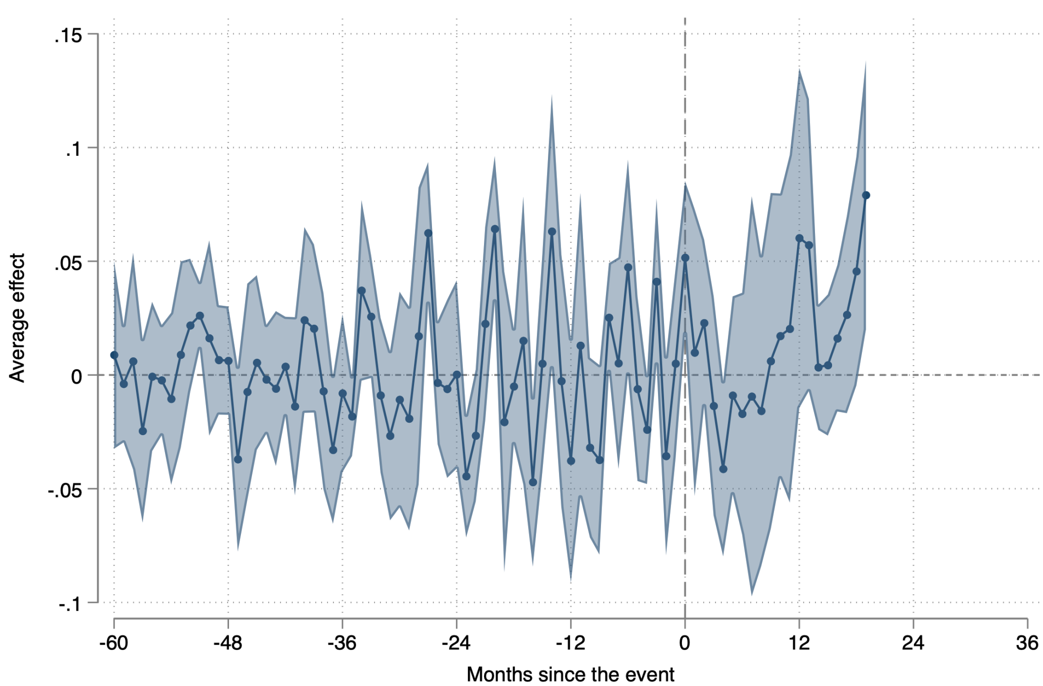


Table 6: Robustness
Effect of SHB on Employment Status of Mothers
Did Estimator from Calloway and Sant'anna, 2021

	(1)	(2)
	Any Age	Some Under 5
ATT	-0.00614	0.0157
	(0.0121)	(0.0201)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

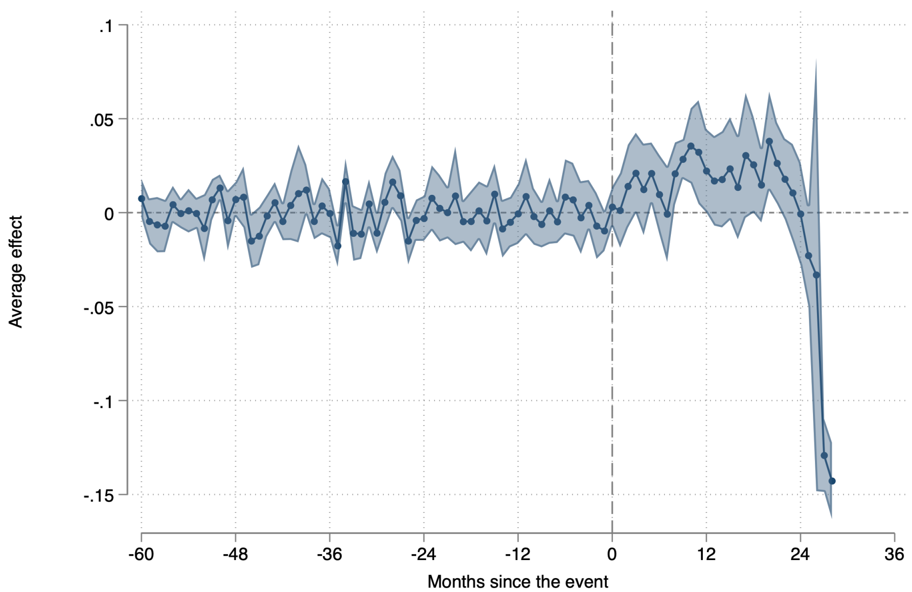
Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Each result is obtained by first regressing employment on education bins and age bins using individual-level observations, clustering standard errors at the state level; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then estimate the ATT using aggregated residuals, the policy variable, and the method put forward by Calloway and Sant'anna (2021). The headings indicate the sample used to obtain each result.

A Supplemental Figures and Tables

Figure A1: Dynamic Effects of SHB on Employment Status of Mothers
Unabalanced Panel
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

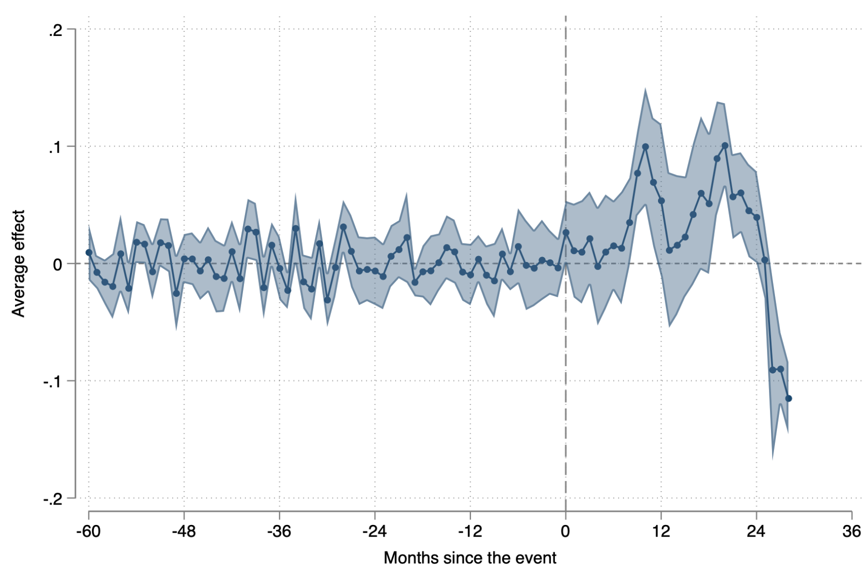
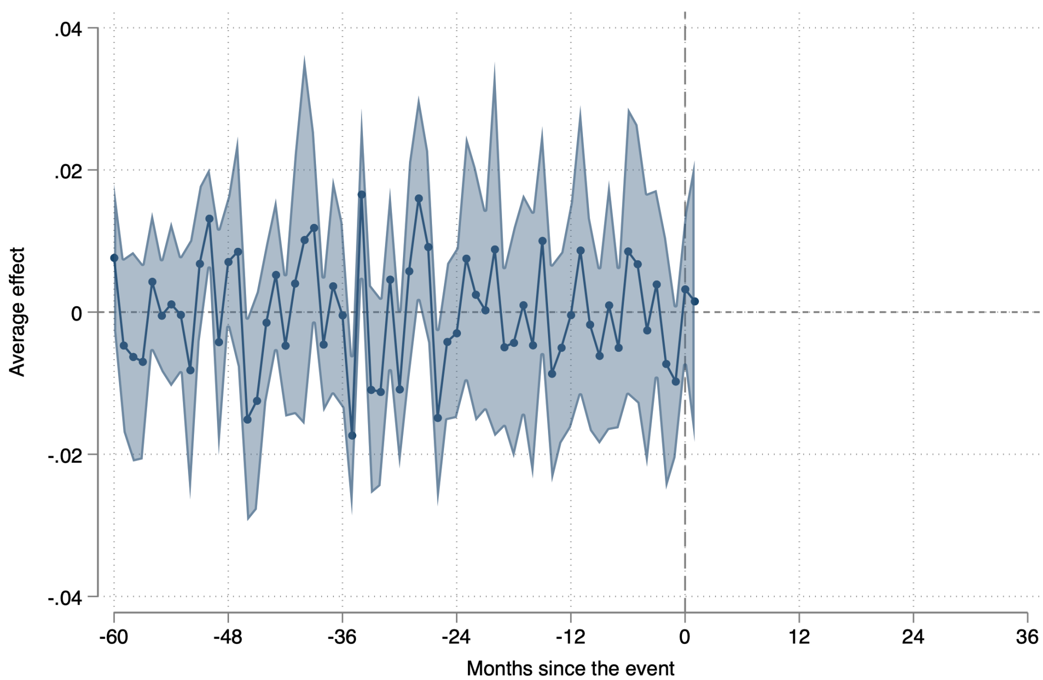


Figure A2: Dynamic Effects of SHB on Employment Status of Mothers
Balanced Panel: All Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

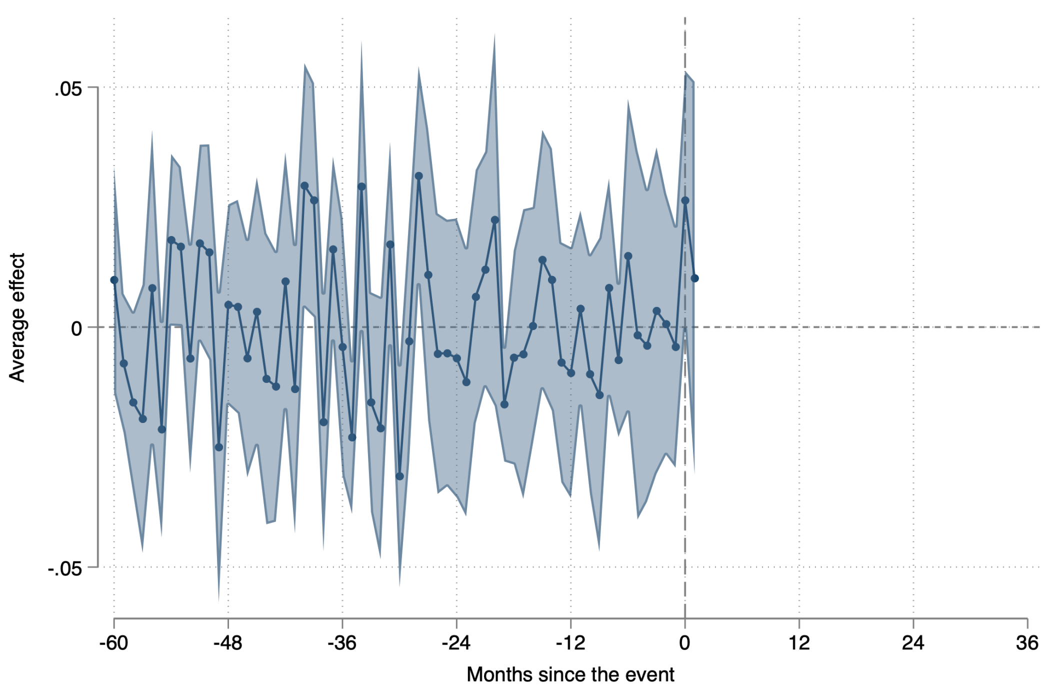
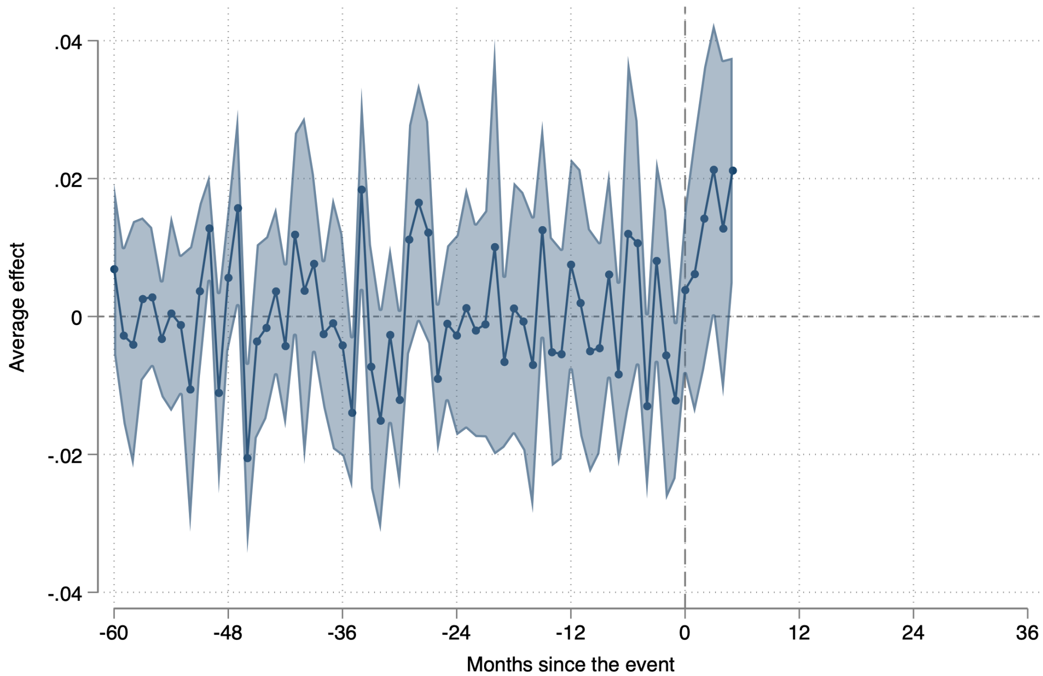


Figure A3: Dynamic Effects of SHB on Employment Status of Mothers
Balanced Panel: 11 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

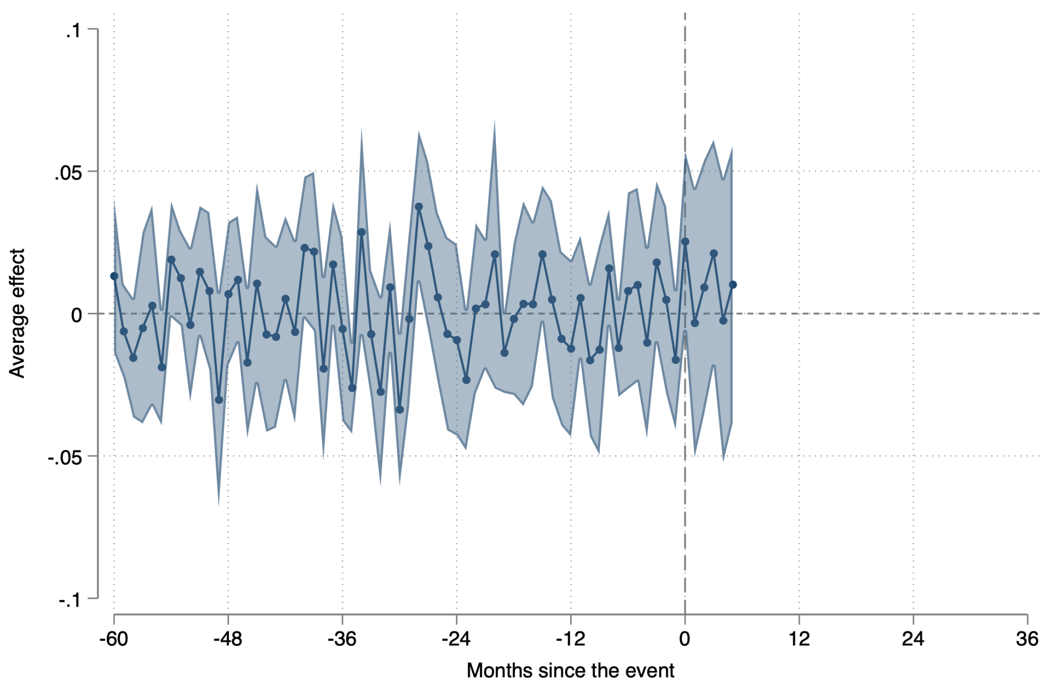


Table A1: Effect of SHB on Employment Status of Mothers
Unbalanced Panel, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1)	(2)	(3)
	Specification A	Specification B	Specification C
Sample: Any Age	0.0133	0.0148+	0.0148+
	(0.00850)	(0.00831)	(0.00831)
Sample: Some Under 5	0.0359	0.0340*	0.0340*
	(0.0220)	(0.0168)	(0.0168)
Sample: All Under 5	-0.000226	0.0156	0.0156
	(0.0257)	(0.0263)	(0.0263)
Sample: Between 5 and 18	0.00469	0.00629	0.00629
	(0.0105)	(0.00937)	(0.00937)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Result in column (1) is an estimate obtained by aggregating employment and SHB policy variables to the state-year-month level and regressing employment on SHB. Result in column (2) is an estimate obtained by first regressing employment on education bins using individual level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable. Result in column (3) is obtained by first regressing employment on education bins and age bins using individual-level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable.

Table A2: Effect of SHB on Employment Status of Mothers
Balanced: All Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1)	(2)	(3)
	Specification A	Specification B	Specification C
Sample: Any Age	0.00291	0.00241	0.00239
	(0.00666)	(0.00741)	(0.00701)
Sample: Some Under 5	0.0164	0.0187	0.0183
	(0.0160)	(0.0159)	(0.0159)
Sample: All Under 5	0.00928	0.00718	0.00689
	(0.0227)	(0.0212)	(0.0210)
Sample: Between 5 and 18	-0.00474	-0.00635	-0.00681
	(0.00825)	(0.00870)	(0.00888)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Result in column (1) is an estimate obtained by aggregating employment and SHB policy variables to the state-year-month level and regressing employment on SHB. Result in column (2) is an estimate obtained by first regressing employment on education bins using individual level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable. Result in column (3) is obtained by first regressing employment on education bins and age bins using individual-level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable.

Table A3: Effect of SHB on Employment Status of Mothers
Balanced: 11 Earliest Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1)	(2)	(3)
	Specification A	Specification B	Specification C
Sample: Any Age	0.0135+	0.0129+	0.0133+
	(0.00730)	(0.00738)	(0.00727)
Sample: Some Under 5	0.0108	0.0104	0.0101
	(0.0222)	(0.0180)	(0.0180)
Sample: All Under 5	0.0255	0.0271	0.0258
	(0.0286)	(0.0255)	(0.0257)
Sample: Between 5 and 18	0.0146*	0.0140*	0.0138*
	(0.00699)	(0.00646)	(0.00665)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Result in column (1) is an estimate obtained by aggregating employment and SHB policy variables to the state-year-month level and regressing employment on SHB. Result in column (2) is an estimate obtained by first regressing employment on education bins using individual level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable. Result in column (3) is obtained by first regressing employment on education bins and age bins using individual-level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable.

Table A4: Effect of SHB on Employment Status of Mothers
Balanced: 5 Earliest Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1)	(2)	(3)
	Specification A	Specification B	Specification C
Sample: Any Age	0.0177+	0.0189*	0.0199*
	(0.00908)	(0.00839)	(0.00849)
Sample: Some Under 5	0.0381	0.0382*	0.0385*
	(0.0258)	(0.0185)	(0.0184)
Sample: All Under 5	-0.00321	0.0211	0.0192
	(0.0327)	(0.0314)	(0.0316)
Sample: Between 5 and 18	0.00950	0.0117	0.0122
	(0.0119)	(0.0101)	(0.0102)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Result in column (1) is an estimate obtained by aggregating employment and SHB policy variables to the state-year-month level and regressing employment on SHB. Result in column (2) is an estimate obtained by first regressing employment on education bins using individual level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable. Result in column (3) is obtained by first regressing employment on education bins and age bins using individual-level observations; I then obtain the residuals from this regression, aggregate to the state-year-month level, and then regress the aggregated residuals on the SHB policy variable.

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