Essays on the Economics of Labor and Gender

by

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This dissertation contains three chapters analyzing the effect of labor policy and regulation on labor market outcomes, as well as cultural determinants of intimate partner violence. The first chapter, "What is the effect of Salary History Bans on the Labor Supply of Mothers?", examines the impact of state-level Salary History Bans (SHBs) on the employment status of mothers in the United States. SHBs, which prohibit employers from using salary history during the hiring process, aim to address gender pay disparities and promote fairer hiring practices. Existing literature has demonstrated the effectiveness of SHBs in reducing the gender pay gap by increasing wages for women. This paper investigates the effect of these increased wages on the employment rates of mothers. Utilizing a pseudo-panel approach, data from the Current Population Survey, and staggered implementation estimators developed by Callaway and Sant'Anna, I find that SHBs have little impact on overall employment rates among mothers. However, significant effects are observed within subgroups, particularly for mothers with at least one child under five, where SHBs increase employment rates by 2.12 percentage points. These findings suggest that SHBs not only contribute to narrowing the gender pay gap but also positively influence the employment outcomes of mothers with young children. The second chapter, "The Role of State-level Wage and Hour Protections under Weakened Federal Enforcement", investigates the impact of the Supreme Court decision in *EPIC Systems v. Lewis* on the incidence of wage theft, specifically focusing on overtime violations. The 2018 ruling mandated individual arbitration for wage and hour disputes, potentially undermining collective legal recourse at the federal level and thus deterring employees from reporting violations. Utilizing data from the Current Population Survey's Outgoing Rotation Group/Earner Study, we examine differential changes in over-

time work and apparent underpayment for overtime across states with varying strengths of administrative enforcement mechanisms. By imputing weekly earnings based on reported hours and wages, and identifying discrepancies indicative of underpayment, we identify instances of overtime violations. Our analysis, employing two-way fixed effects and event study methodologies, finds no significant differential impact of the *EPIC* decision between strong and weak enforcement states. Robustness checks excluding states without codified overtime statutes confirm these findings. This study contributes to the wage theft literature by developing a novel measure of overtime underpayment and leveraging unique state-level enforcement variation. Finally, the third chapter, “The Relationship between Female Deity Temple Exposure and Intimate Partner Violence” explores the historical roots of attitudes towards women by analyzing the relationship between intimate partner violence (IPV) and historical exposure to female deity temples in the South Indian state of Tamil Nadu. Using hand-collected data on historical temples, this study constructs a district-level measure of exposure to goddess temples. Employing an conditional on observables methodology and individual-level IPV data from the National Family and Health Survey, this paper investigates whether historical exposure to female deities correlates with current IPV incidence. The results suggest a counterintuitive association: higher exposure to female-deity temples is associated with increased IPV. This suggests a complex relationship between religious beliefs and gender norms, where cultural reverence for goddesses might not translate into respect for women, potentially exacerbating IPV.

Dedication

To my mother, my greatest source of inspiration.

Acknowledgements

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Chapter 1

What is the effect of Salary History Bans on the Labor Supply of Mothers?

1.1 Introduction

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 competitive 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 (Hansen and McNichols (2020); Sinha (2022); Bessen et al. (2021)). [3] As wages for women increase due to the implementation of SHBs,

[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 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.

we 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 diverge drastically in their career trajectories once children are born. This point in the life cycle initiates a departure from the labor force for some women and a transition from full-time to part-time work arrangements for others, while some women choose to remain in the labor force. This heterogeneity in labor supply is particularly pronounced among mothers when children are below school-age (under 5 years of age), when parents (usually mothers) face a tradeoff between earnings and high childcare costs. Blundell et al. (2016) find that women with children, both married mothers and single mothers, exhibit the highest labor supply elasticities among all women. Further, among mothers, as Apps et al. (2016) find in their structural analysis, labor supply of mothers with young children is particularly responsive to wages and cost of childcare relative to other groups of mothers. If this is the case, mothers with very young children are likely respond to the resulting wage growth from Salary History Bans 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 labor force participation of mothers?

Specifically, I examine the effect of state-level Salary History Bans on the labor force participation among mothers by examining three outcomes: labor force participation rates, mean hours worked, and employment rates, all 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 labor force participation 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 labor force participation rates, mean hours worked, and employment rates (outcome variables) 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 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 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 observations in the post-implementation period. 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 plot as well as an overall average treatment effect. I find little to no effect of Salary History Bans on the overall maternal labor force participation rate, average labor hours, or employment rate; using my preferred sample (outcome variables residualized at the individual level for education and

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

age), I find that SHBs increase the employment rate for mothers by 0.4 percentage points, though this is not a statistically significant result.

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 et al. (2016). These findings suggest that it is fruitful to understand whether 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 three categories of women: (1) mothers of any-age children, (2) mothers who have at least one child under 5, and (3) 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 labor force participation 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 et al. (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 labor force participation 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 are complementary 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.

1.2 Background on Salary History Bans

1.2.1 Conceptual Framework: How can Salary History Bans affect the Labor Supply of Mothers?

Salary History Bans were introduced to the public and political arena around 2015-2016 as corporate policies adopted by a few companies. It gained momentum in smaller administrative units, until the first state, Massachusetts, adopted one in 2016.

The primary objective of this policy tool is based on the hypothesis that, when employers have access to salary histories and make offers of pay based on salary histories, initial compensation can be perpetuated across time. If, at the beginning of their careers, women start with lower salaries than their male counterparts, this initial disparity in compensation can persist throughout the career trajectory and can contribute to the gender pay gap. Indeed, experimental evidence documented by Barach and Horton (2021) reveals that about half of all employers rely on Salary History during the screening process; in fact, they find that when employers' access to salary history is restricted, employers consider an applicant pool with less competitive compensation history and ultimately hire successful job candidates with less competitive compensation history.

As discussed previously, the empirical evidence on SHBs supports increased wages for women and gender pay gap mitigation, particularly for women 35 and older; since younger cohorts of women and men have similar starting salaries relative to the older cohorts, the previous findings are consistent with the theory of employers making offers of pay based on a rigorous screening process rather than salary history, which help correct for pay disparities at the point of career initiation. [5]

For younger cohorts of women and men, the main source of pay disparity is the "motherhood gap." Because childcare costs are high, once children are born, many mothers reduce their labor supply or exit the labor force, altogether; meanwhile, their male partners exhibit little variation in labor supply due to societal norms and higher earnings expectations compared to their spouses. Once all of their children are eligible (or almost-eligible) for public kindergarten, mothers who have exited the labor force or reduced their hours are often at a phase in their life cycle when they are ready to re-enter the labor force or increase their labor supply.

When SHBs are implemented, previous findings support that labor demand for women has increased. Employers make higher offers of pay to women. Mothers, on average, observe that wages have increased for women and are induced to increase their labor supply. In

[5] It has been theorized that Salary History Bans can have ambiguous effects on compensation. There is a large literature focused on statistical discrimination which documents that, when signals of productivity are withheld from employers, employers attach beliefs about job candidate based on observable characteristics. For example, Doleac and Hansen (2020) find that "Ban the Box" policies, which disallow employers from conducting criminal background checks on job candidates, induce employers to discriminate against black male candidates due to the statistical evidence that black males have criminal histories at higher rates than other demographics. SHBs, which whithhold pay history information from employers, can theoretically induce employers to assume that women have less competitive compensation history, prompting them to make lower offers of pay to job candidates. However, the empirical evidence does not support this theory.

addition, mothers who are considering leaving the labor market choose to remain in the labor force at greater rates, as their opportunity cost of exiting the labor force has increased.

Maternal labor supply responses to SHBs are potentially larger for women with younger children. As previously discussed, mothers who have at least one child under 5 are at a phase in their life cycle when they are ready to increase their labor supply (if they have reduced their labor supply in the first place). For this group, a change in wage can be highly salient; because they tend to be highly responsive to changes in wages, this group may have the highest labor supply response to SHBs. Conversely, mothers with children all older than 5 may have already returned to the labor force, since their childcare costs are relatively low. Summary statistics in Table 1 support this, as mothers whose children are 5 and above are employed at a higher rate than women who have some children under 5. As such, we can expect women with older children to have a smaller labor supply response to the increased wages resulting from Salary History Bans.

Indeed, my findings support the theory discussed above; mothers with younger children have a strong response to SHBs, while mothers with older children exhibit little change in labor supply.

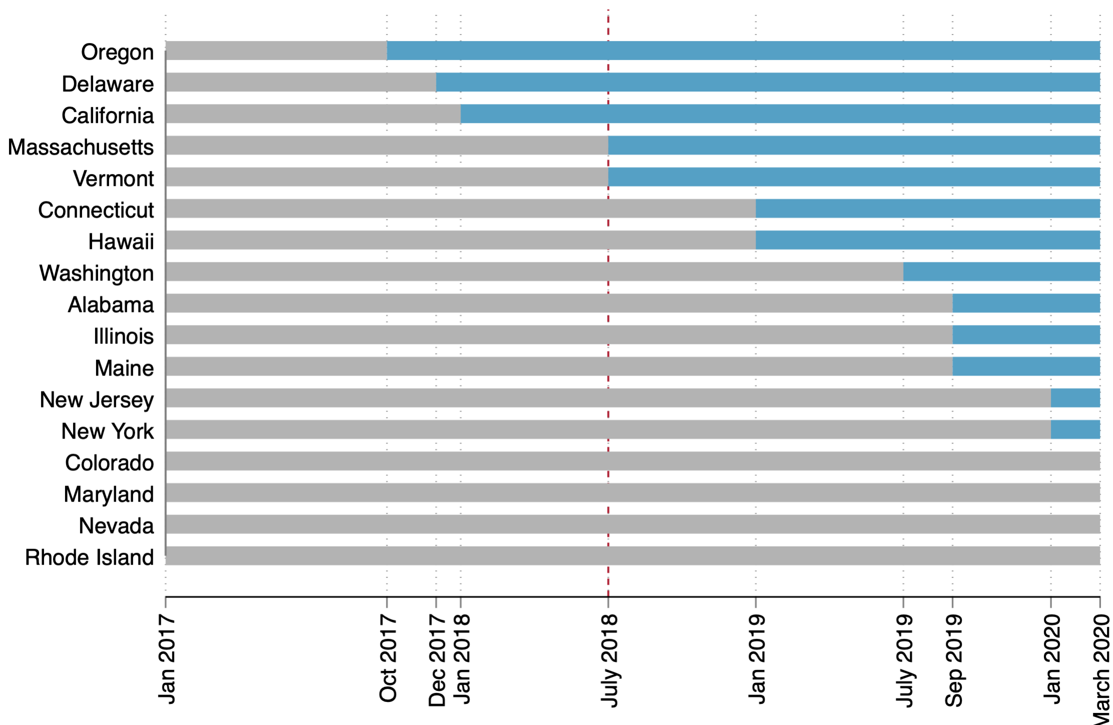
1.2.2 Policy Background and Rollout

There are two main categories of Salary History Bans passed at the state-level. The first is a Public Sector Salary History Ban, 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.[6] All-Employer Salary History Bans, on the other hand, are enacted through legislation and prohibit any employer in a state from asking for salary history information from job candidates. The different scopes of the two Salary History Bans make for completely different policy environments. My goal

[6] However, under any kind of Salary History Ban, employers still have access to a candidate's resume and job history.

is to study the SHB policy which has broader labor market implications for both public and private sectors, so the policy of focus in this paper is the All-Employer SHB. Moreover, potential ambiguity in the effects of Public SHBs on labor market outcomes across public and private sectors could complicate the analysis and interpretation of results. Therefore, I drop from the analysis those states which have only enacted a Public SHB. [7]

Figure 1.1: 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.

All-Employer Salary History Bans are a popular policy tool, adopted by 17 states as of September 2023. Implementation dates are given in Figure 1.1; the earliest implementer

[7] These states are Michigan, Pennsylvania, Virginia, and North Carolina.

is Oregon, with an effective date of October 2017. Due to large and abrupt changes in labor markets due to the COVID-19 pandemic, my analysis sample consists only of observations before March 2020. This is particularly relevant to my study, as maternal labor supply changed drastically due to COVID-19. Conducting a pre-COVID analysis ensures that my findings on labor supply of mothers are not specific to conditions of the pandemic, allowing for a better interpretation of the results.

Because my analysis is limited to the time period ending in February 2020, four of the seventeen All-Employer SHB implementers, whose implementation dates are after February 2020, always appear as "never treated" states in my analysis. I conduct a robustness analysis using these "eventually treated" (or "not-yet-treated") states as the only control states in the analysis.

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.

In the next section, I discuss the data and methodology employed in my analysis.

1.3 Data

For information on labor force participation and individual-level observables such as educational attainment and age, I use data from the Current Population Survey's Basic Monthly Files (CPS), maintained by the Integrated Public Use Microdata Series (Flood et al. (2023)) from the period of January 2010-March 2020. The 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 22-45 years of age who are also 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. This age restriction allows me to focus on mothers who are more likely to be in their prime working years and actively engaged in the labor market.

I consider three outcome variables in order to fully understand the labor supply of mothers in response to the SHB policy. The CPS collects information on whether or not individuals are participating in the labor force, how many hours they work in a usual week, and whether the respondent is employed, which I use as the outcome in my analysis. [8] 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 three 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, and, finally, (3) "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 treat these data as a cross-section. Furthermore, I aggregate the individual-level outcome variable to a state-year-month panel. In the

[8] For this analysis, I consider responses of "Unemployed," and "Not in Labor Force" as people who are not employed (employed = 0); 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. Similarly, I code all non-workers as having worked "0" hours during their usual week.

state-year-month panel, therefore, the outcome variable represents the maternal labor force participation rate (LFPR), mean weekly hours worked among mothers, and maternal employment rate for a state in a given year-month. In my preferred specification, I first regress the outcome variables on individual characteristics, then aggregate the residuals of the outcomes to the state-year-month level. This approach yields measures of maternal LFPR, mean weekly hours worked among mothers, and maternal employment rate that have been residualized for individual-level educational attainment and age. This approach helps to account for potential biases due to selection into survey response, maintaining the assumption that each state is an independent observation. Specifically, by accounting for individual-level characteristics such as educational attainment and age, this approach ensures that any observed differences in state-level outcomes (LFPR, mean weekly hours worked, and employment rate) are more likely to be attributable to the Salary History Ban rather than shifts in the demographic composition of the survey respondents over time.

Policy timing data, including data on type of SHB, SHB adoption dates, and SHB rollout dates, comes from a human resources publication online called HR Dive. [9] 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 direction.

1.4 Methodology

Salary History Bans (SHBs) are state-level policy tools with implementation dates that vary according to decisions made by each state's policymakers. Given the staggered rollout of SHBs across different states, I employ the method for staggered-implementation policy adoption as developed by Callaway and Sant'Anna in their 2021 paper.

[9] HRDive (2024) https://www.hrdive.com/news/salary-history-ban-states-list/516662/

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 states (which eventually implement All-Employer SHBs post-March 2020) and never-treated states (which never implement an SHB) as the comparison group for the treated states. In considering these two groups of states as a comparison group for SHB implementing states, I assume that, absent the All-Employer Salary History Bans, the trend of the maternal employment rate in the treated states and the trend of the maternal employment rate in comparison states would be "parallel," or follow the same trajectory.

The biggest threat to this assumption, with regard to the comparison group, is the potential endogeneity of SHB adoption. Factors that influence the decision of a state legislature to implement All-Employer Salary History Bans could also affect changes in maternal employment rates independent of the policy. For instance, states with more progressive policies may be more likely to implement SHBs and may also have different trends in maternal employment rates. In order to address this concern, as a robustness check, I use only the not-yet-treated states as the comparison group. Because these states intend to implement SHBs, I can better control for factors that drive policy adoption and better control for variations in maternal employment rate trends. However, my main results are reported using the comparison group of both untreated- and eventually-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"). [10] 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

[10] 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.

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.

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:

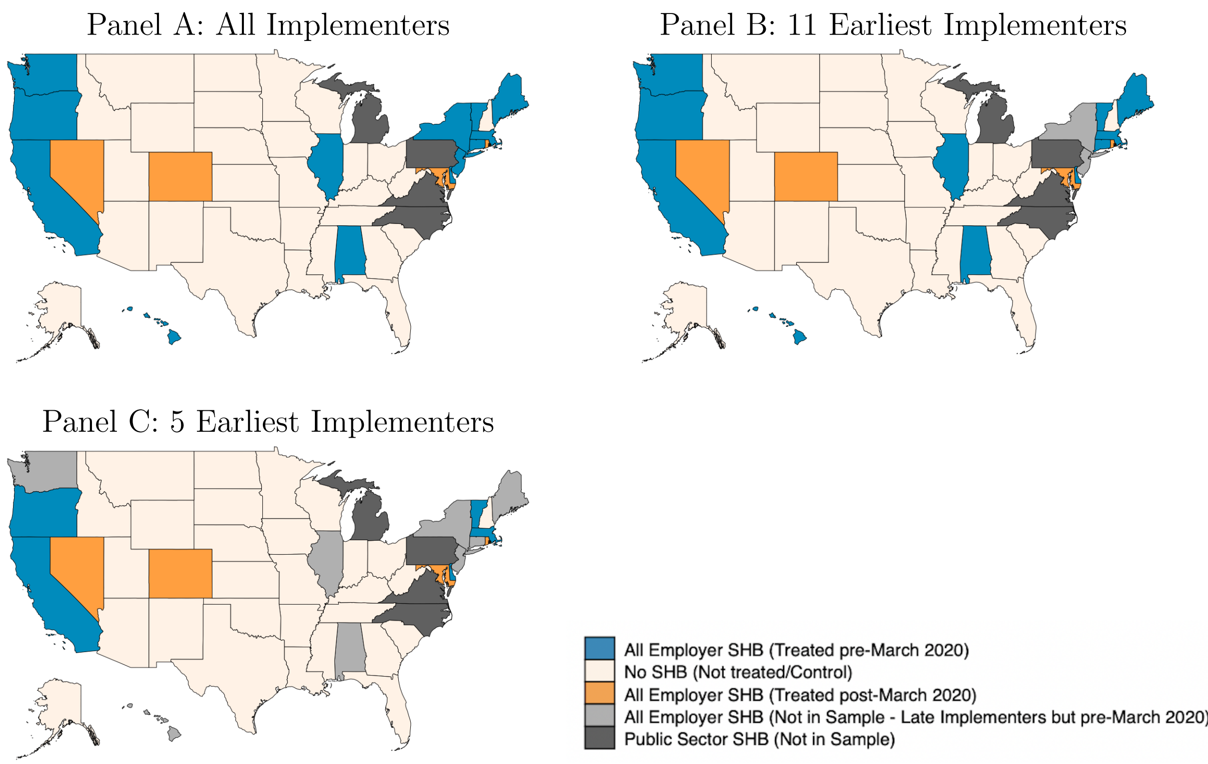
$$\theta_S^O(g) := \sum_{t=2}^{\tau} \theta_S(g) P(G = g)$$

Because I anticipate that the Salary History Ban's increase in wages will induce mothers to join the labor force, I expect $\theta_S^O(g)$ to be positive for all three outcomes, and I discuss estimates of this parameter in the following section.

Callaway and Sant'Anna recommend using a balanced panel for their method. This allows me to overcome biases related to policy timing: specifically, effects that are biased by rapidly changing composition of states in each period relative to the policy implementation period. I have balanced the panel in three ways, represented by Figure 1.2. 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.

Figure 1.2: All-Employer Salary History Ban Implementation, as of 2023



Source: HR Dive

(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, using the employment rate outcome in supplemental Section 1.8.

1.5 Results

Table 1.1: Summary Statistics of Mothers
Heterogeneity by Age of Children

	Any Age		**Some Under 5**		**5 and Above**	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Employed	0.69		0.61		0.73	
Education Bins						
HS, No Degree	0.10		0.10		0.10	
HS or Equal	0.24		0.23		0.25	
Some college	0.18		0.18		0.18	
Associate Degree	0.12		0.11		0.13	
Bachelors degree	0.33		0.35		0.33	
Advanced degree	0.03		0.03		0.03	
Age	38.66	8.53	32.39	6.26	42.09	7.61
Married	0.71		0.72		0.70	

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.

I present descriptive statistics using the underlying CPS data in Table 1.1. I report the mean and standard deviations of the outcome and control variables for the three categories of mothers based on the ages of their children.

Comparing employment rates across groups, the "Some Under 5" group of mothers has the lowest employment rate. This is consistent with the hypothesis that mothers with children under 5 may include those whose youngest are nearly ready for public Kindergarten, easing their childcare responsibilities and preparing them to re-enter the workforce. Thus, SHBs (and similar policies) may be particularly salient for this group.

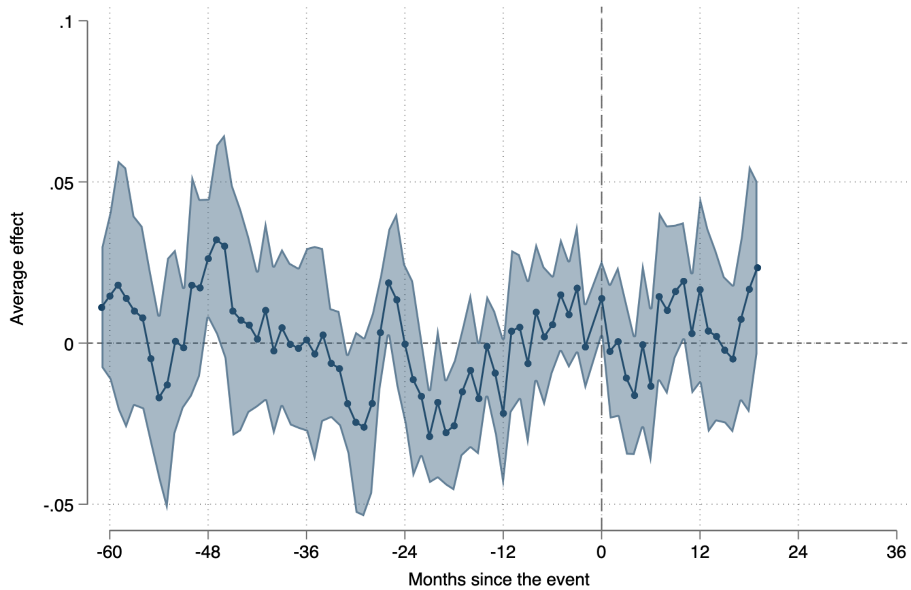
It is difficult to attribute any differences in overall ATTs to educational attainment, as the education distribution across the three categories is nearly identical. However, age and number of children are correlated; older mothers might be at different stages in their career and family life compared to younger mothers. When interpreting differences in the overall ATTs across groups of mothers with children of different ages, it is possible that a portion of the differences may be due to age-related factors, such as longer and more stable employment or greater work experience, in addition to differences in childcare responsibilities. Finally, marital status is also very similar across groups, making it unlikely that any differences in ATTs across the three groups can be attributed to this factor.

For the dynamic effects of SHBs on the labor force participation rate, Figure 1.3 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 differential 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.

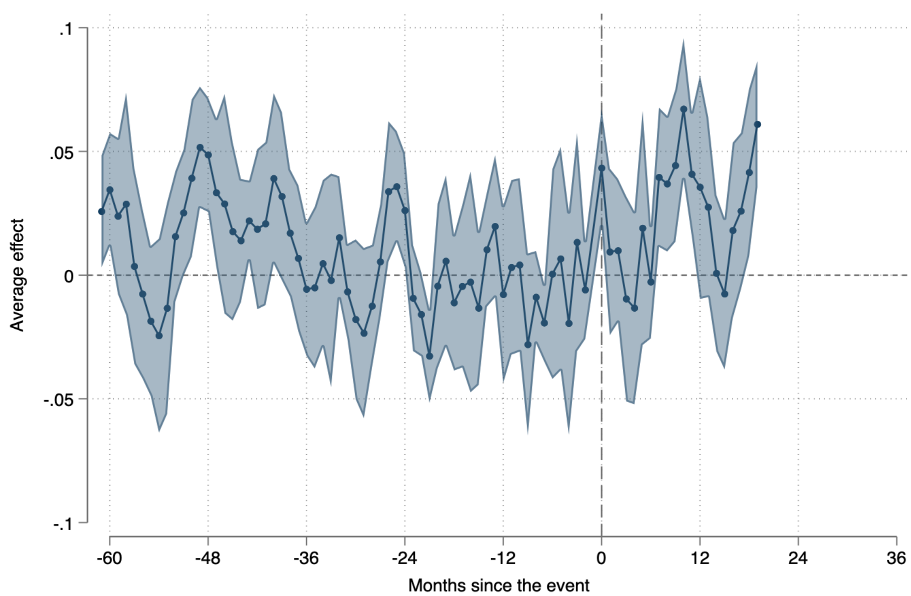
In Figure 1.4, I present event plots representing the effect of SHBs on average weekly labor hours. While the pre-trends in Panel A show the estimated average effect fluctuating around zero, in the months just before the event (approximately from month -20 to 0), there appears to be an upward trend in the change in average effect on weekly work hours. The estimated average effect is getting more positive each period prior to t=0, suggesting that any positive effects after SHB implementation may be due to the continuation of those trends rather than to the treatment. Though the increasing gap between the treatment and comparison states pose a potential challenge to the parallel counterfactual trends assumption, the confidence bands are quite wide and overlap zero, suggesting that parts of this trend are

Figure 1.3: Dynamic Effects of SHB on Labor Force Participation Rate Among Mothers
5 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



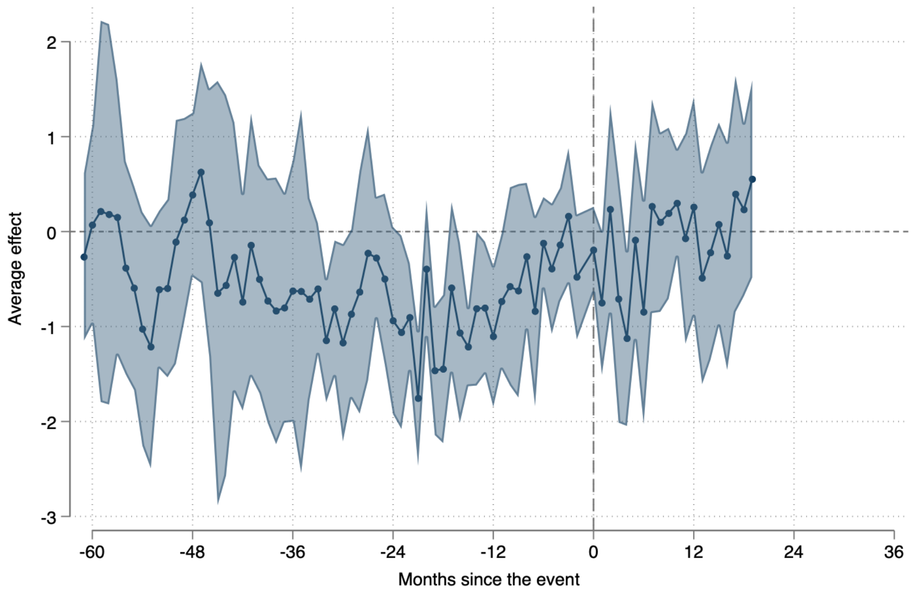
Panel B: Mothers with at least One Child Under 5



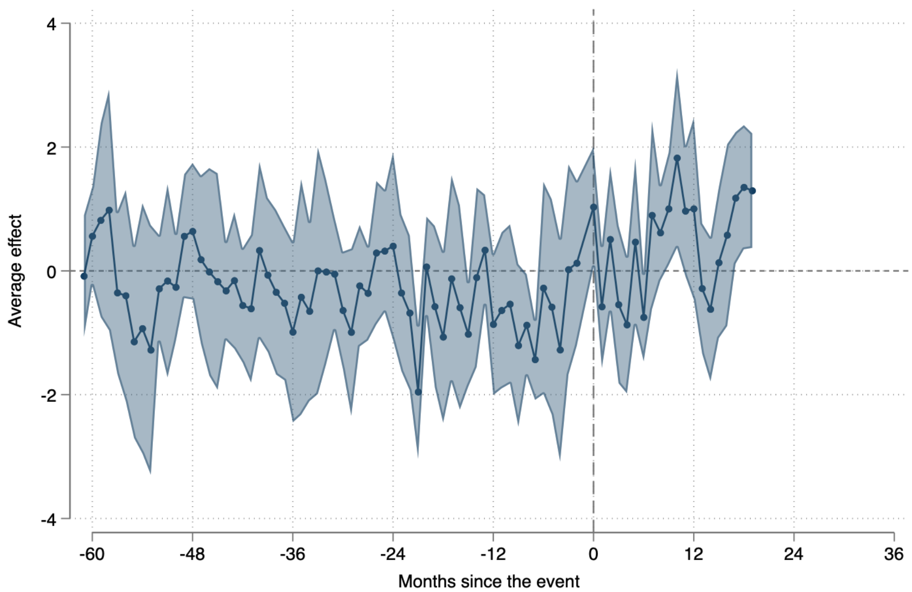
not statistically significant. In future drafts, I plan to investigate this potential violation of the parallel counterfactual trend.

Figure 1.4: Dynamic Effects of SHB on Average Weekly Labor Hours Among Mothers
5 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



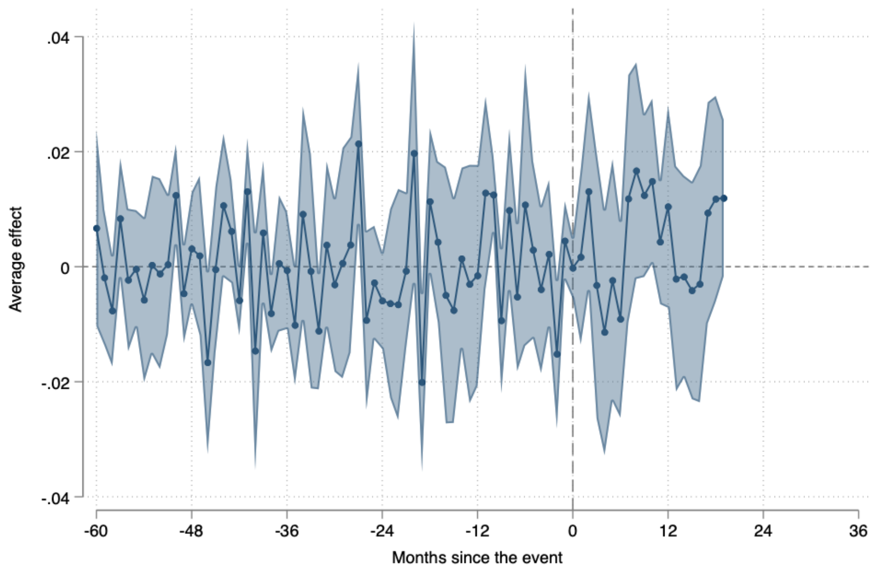
Panel B: Mothers with at least One Child Under 5



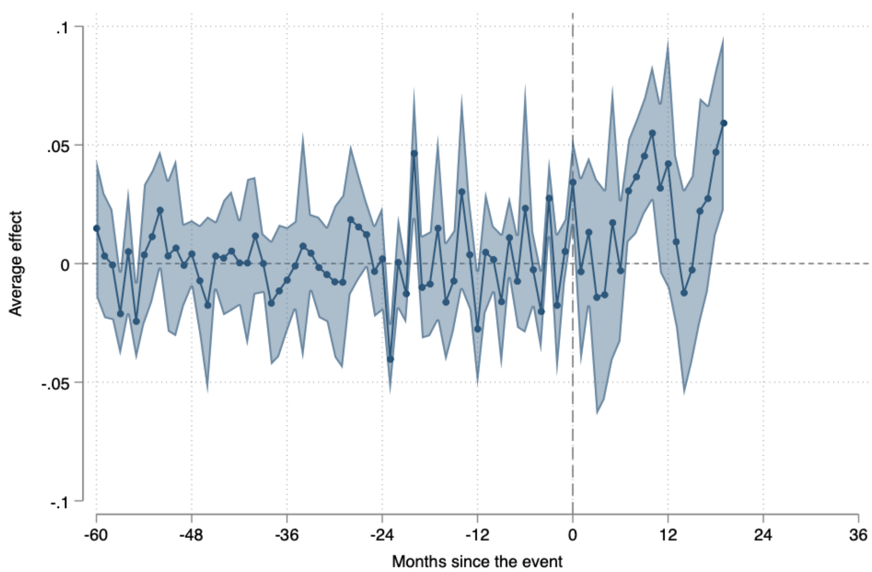
In Panel B of Figure 1.4, I present the results for Mothers with at least one child under 5. There appear to be no differential pre-trends in this analysis, and the policy effects are positive in many of the months following SHB implementation, though the estimates are not statistically significant at the 95 percent confidence level.

Figure 1.5: Dynamic Effects of SHB on Employment Rate Among 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



The event plots showing the effect of SHBs on the employment rates among mothers (Figure 1.5) are very similar to the plots that represent the effect of SHBs on the labor force participation among mothers. In both panels, the estimates in the pre-implementation period fluctuate around zero suggesting that there aren't any pre-trends. In both panels, there is a positive effect of the policy on the employment rate, though in the case of all mothers, this effect is not significant in all time periods.

Table 1.2: 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) LFPR	(2) Mean Hours Worked per Week	(3) Employment Rate
No Controls	0.00728	-0.0176	0.00304
	(0.0101)	(0.409)	(0.00638)
Education Controls	0.00510	-0.0847	0.00350
	(0.00847)	(0.353)	(0.00529)
Education and	0.00485	-0.107	0.00406
Age Controls	(0.00880)	(0.375)	(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. Results presented in row 1 (No Controls) are estimates using the pseudo-panel obtained by aggregating employment and SHB policy variables to the state-year-month level. Results presented in row 2 (Education Controls) are estimates using the pseudo-panel obtained by first regressing employment on education bins and then aggregating the residuals to the state-year-month level. Results reported in row 3 (Education and Age Controls) are estimates using the pseudo-panel obtained by first regressing employment on education bins and age controls using individual-level observations; I then obtain the residuals from this regression and aggregate to the state-year-month level.

I report the overall ATTs for mothers with children of any age in Table 1.2. In the first row, No Controls, 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 the second row, Education Controls, I regress employment on education bins, then obtain the residual of employment before aggregating to the state-year-month level. In the third row, Education and Age Controls, I regress employment on education bins and age bins, obtain the residual of employment, and then aggregate to the state-year-month level. The outcome in each column is specified in the headings. The effects of the policy on both labor force participation rate and employment rate are positive, consistent with my hypothesis that the policy would increase labor supply among women. In my preferred specification, Education and Age Controls, the estimate of the effect of SHBs on labor force participation rate suggest that implementing a SHB increases the labor force participation rate by 0.485 percentage points. The estimate in column (3) suggests that implementing a SHB increases the employment rate among mothers by 0.406 percentage points, a 0.588 percent increase relative to the pre-period mean of 69 percent employment. However, based on the 95 percent confidence bands (standard errors are reported in the parentheses), we cannot reject the null hypothesis that the true effect of SHBs on labor supply, as measured by LFPR, mean hours worked each week, and employment rate, is different from zero.

The effect of Salary History Bans on mean hours worked per week are all negative. This, however, is consistent with the event plot presented in Panel A of Figure 1.4 (see the above discussion). Moreover, as discussed above, the confidence interval is large, suggesting statistical noise. For example, in my preferred specification, I report that an SHB is associated with a 0.107 decrease in the mean weekly hours worked. However, there is a 95 percent likelihood that the true effect is between the wide range of -0.842 hours per week and +0.628 hours per week. Thus, despite this negative estimate, I do not conclude that there is evidence that SHBs decrease labor supply among mothers.

In Table 1.3, I report the ATTs from the preferred specification for all three groups of mothers. The policy effect is strongest for the group of mothers with some children under 5. The effect of the policy for this group is positive, increasing labor force participation rate by 2.44 percentage points. This is a statistically significant effect, with a 95 percent confidence

Table 1.3: 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) LFPR	(2) Mean Hours Worked per Week	(3) Employment Rate
Any Age	0.00485	-0.107	0.00406
	(0.00539)	(0.375)	(0.00539)
Some Under 5	0.0244*	0.461	0.0212
	(0.0102)	(0.289)	(0.0138)
5 and Above	-0.0113	-0.673	-0.00440
	(0.00958)	(0.630)	(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.

interval of (0.441,4.439). The effects of the policy on Mean Hours worked per week and employment rate are positive, as well.

1.6 Robustness

In the results reported thus far, I have used both never-treated and not-yet-treated states as the control group. A concern arises that states eventually treated (Colorado, Nevada, Rhode Island, and Maryland), along with states treated before 2020, may differ from those that never intend to adopt an All-Employer SHB. In order to account for potential unobservable differences between the treated and never-treated groups, Callaway and Sant'Anna recommend using the eventually- (or not-yet-) treated units as the only members of the comparison group, instead. In this section, I will discuss the results obtained by using the four eventually-treated states as the comparison group, dropping the never-treated states, altogether.

Table 1.4: Robustness
Effect of SHB on Employment Status of Mothers
Did Estimator from Calloway and Sant'anna, 2021

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

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

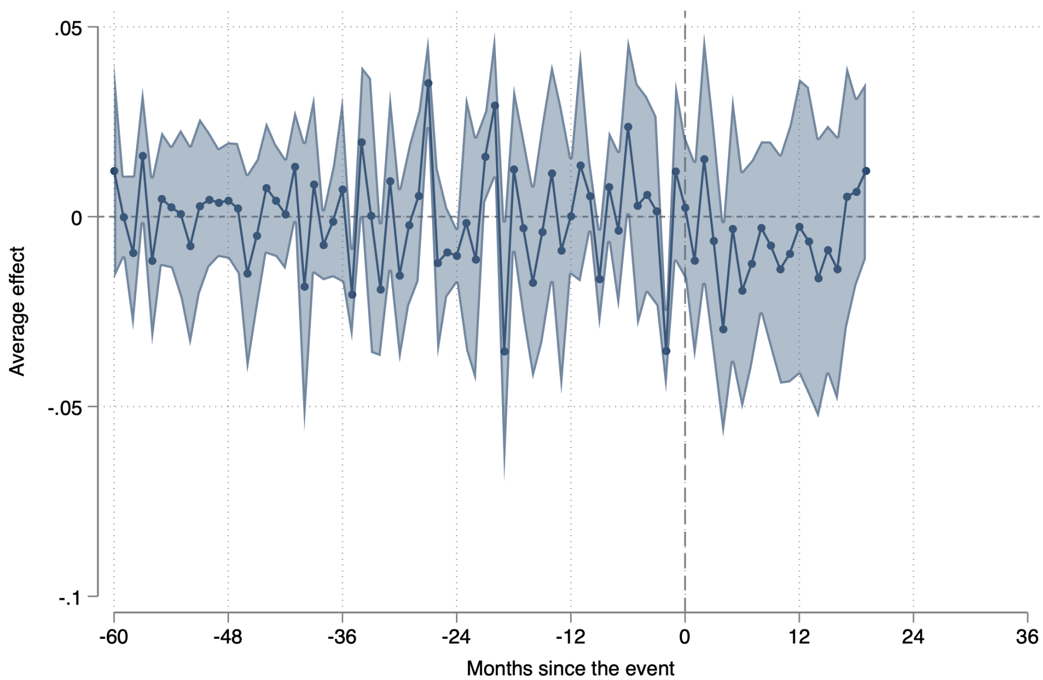
Source: CPS Basic Monthly Files, 2010-Mar 2020.
These are average treatment effects on the treated generated using the DiD method put forward by Calloway and Sant'anna, 2021. Each result is obtained by first regressing employment on education bins and age bins using individual-level observations, clustering 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.

In Figure 1.6, I present the results from including only the eventually-treated states as the sole constituents of the control group. I present the outcome for employment, alone. Both Panel A and B, the samples of all mothers and mothers with some children under

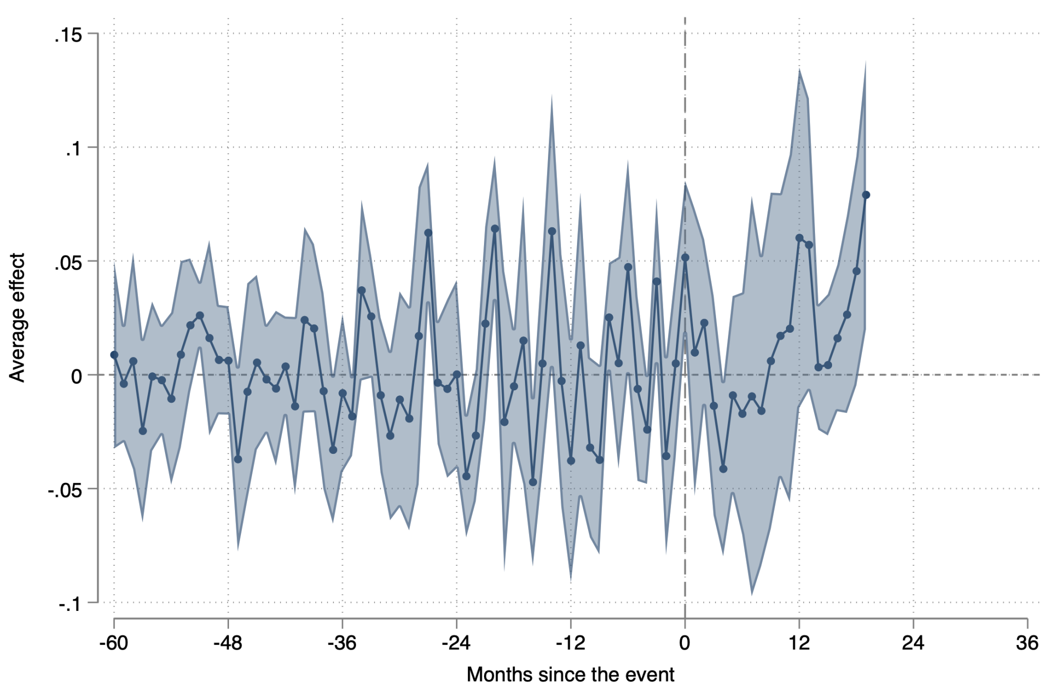
5, respectively, suggest that SHB has a positive effect on employment rates for mothers. Neither event plot displays any evidence of differential pre-trends. I report the overall ATTs for the robustness analysis in Table 1.4; neither estimate is statistically significant.

Figure 1.6: 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



1.7 Conclusion

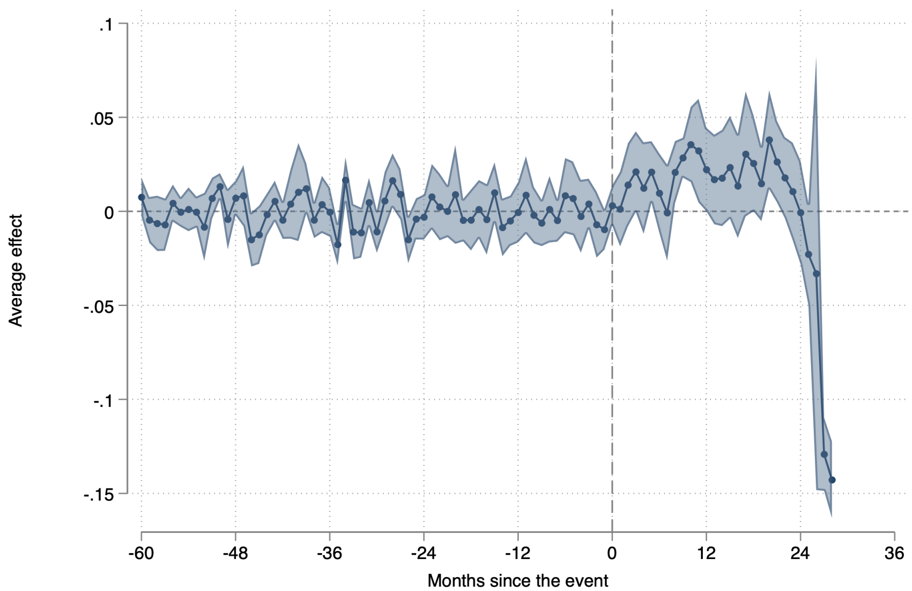
This paper underscores the broad implications of SHBs for labor supply. While previous studies have documented reductions in the gender pay gap following the implementation of All-Employer SHBs, this paper emphasizes the importance of understanding how these policy changes shape employment outcomes and labor supply decisions among women – specifically, women with children.

The findings shed light on the differential impacts of SHBs on mothers with varying caregiving responsibilities, based on the ages of their children.

1.8 Supplemental Tables and Figures

Figure 1.7: 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

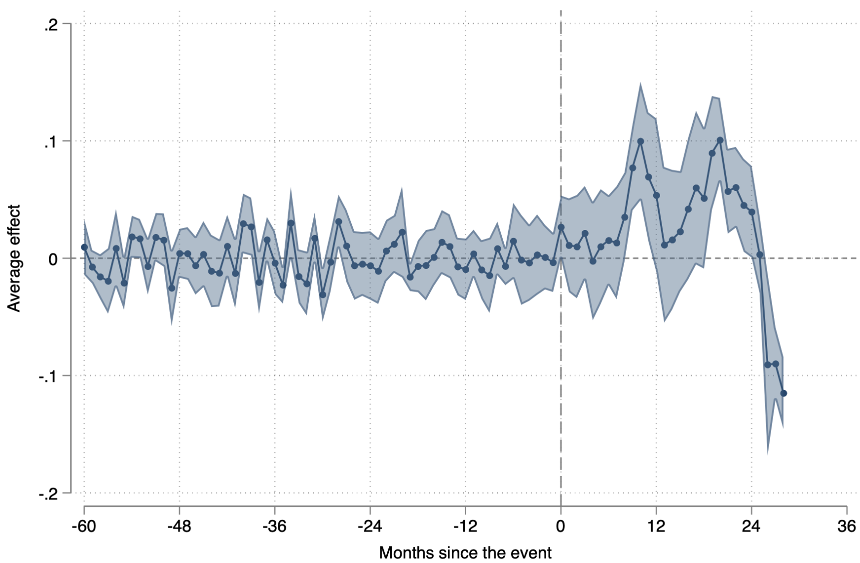
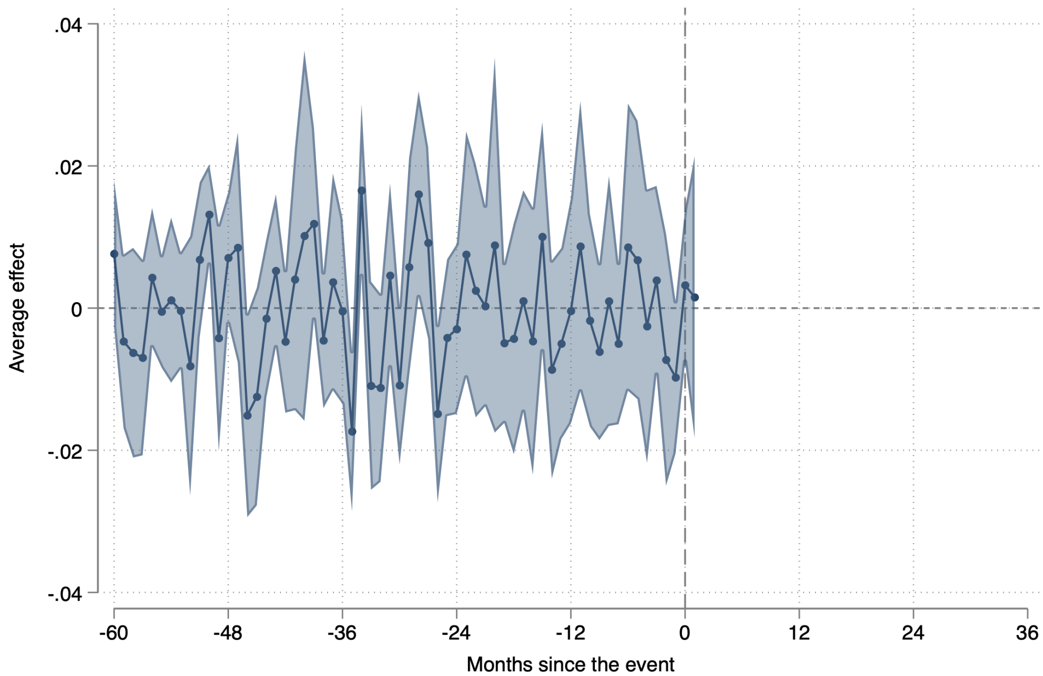


Figure 1.8: Dynamic Effects of SHB on Employment Status of Mothers
Balanced Panel: All Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

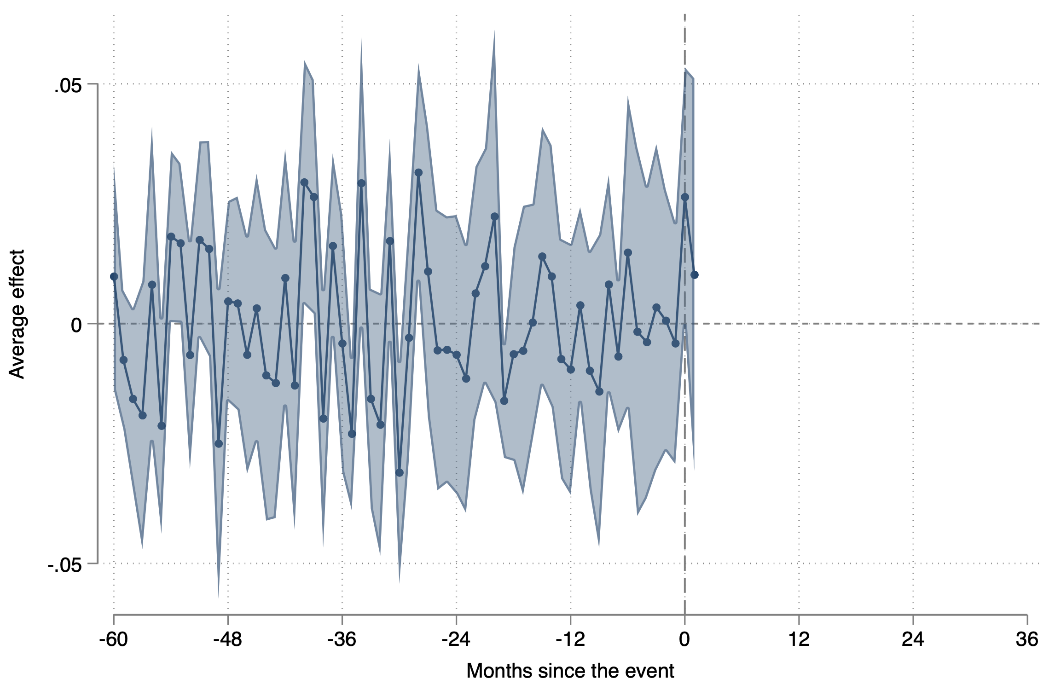
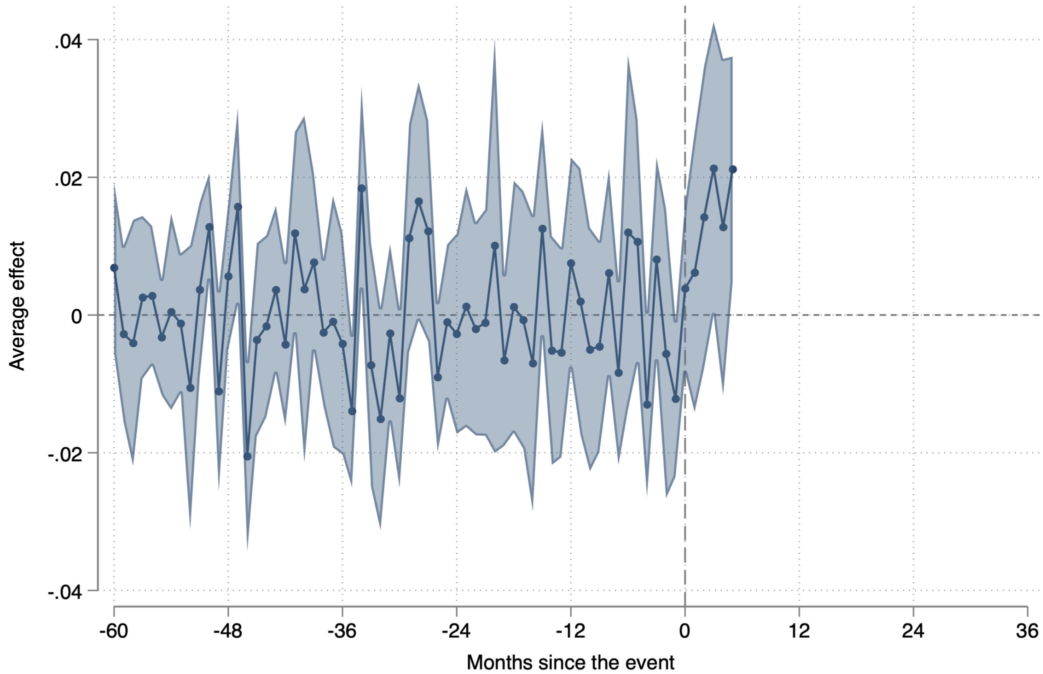


Figure 1.9: Dynamic Effects of SHB on Employment Status of Mothers
Balanced Panel: 11 Earliest Implementers
Event Study Estimates from Calloway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5

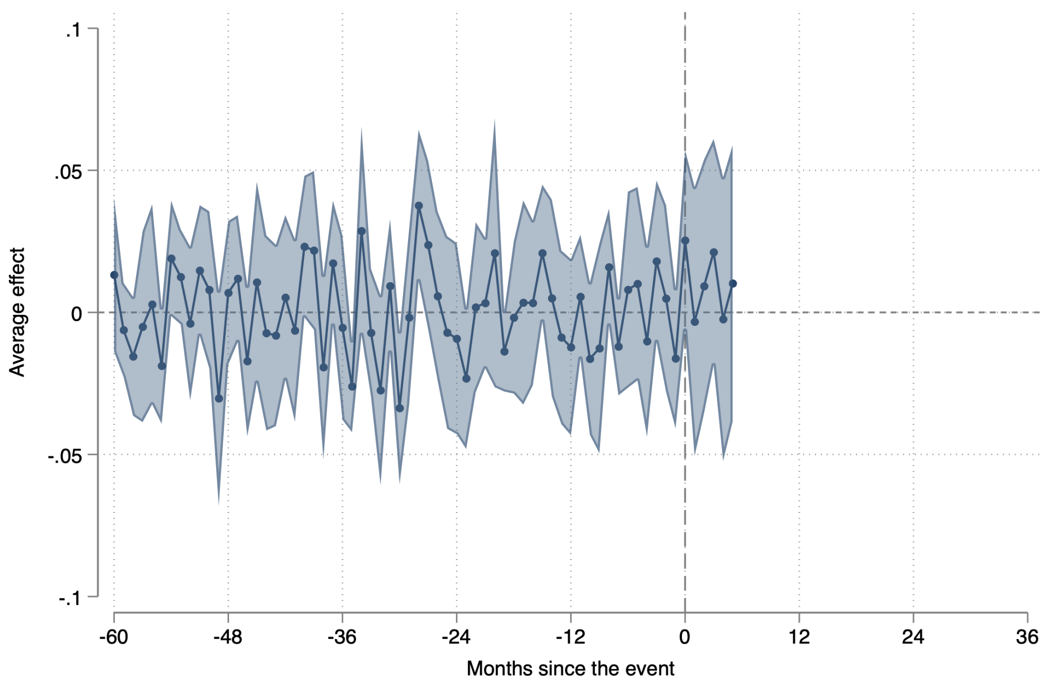


Table 1.5: Effect of SHB on Employment Status of Mothers
Unbalanced Panel, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1) Specification A	(2) Specification B	(3) 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 1.6: Effect of SHB on Employment Status of Mothers
Balanced: All Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1) Specification A	(2) Specification B	(3) 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 1.7: Effect of SHB on Employment Status of Mothers
Balanced: 11 Earliest Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1) Specification A	(2) Specification B	(3) 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 1.8: Effect of SHB on Employment Status of Mothers
Balanced: 5 Earliest Implementers, 2010-March 2020
DiD ATT Estimates from Calloway-Sant'anna, 2021

	(1) Specification A	(2) Specification B	(3) 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.

Chapter 2

The Role of State-level Wage and Hour Protections under Weakened Federal Enforcement

with Lauren Schechter

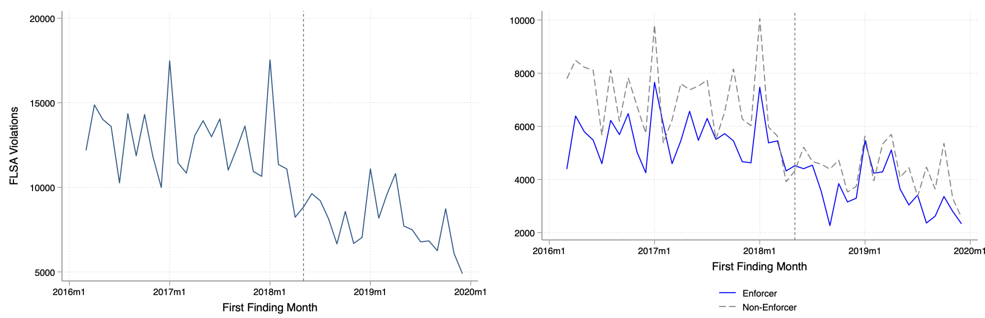
2.1 Introduction

Administrative enforcement of wage and hour laws is an important means for wage-earning employees to seek recourse for wage theft, including subminimum wage payment, underpayment for overtime work, or other forms of underpayment for wages earned. In *Epic Systems v. Lewis*, decided on May 21, 2018, the United States Supreme Court held that the Federal Arbitration Act requires courts to uphold and enforce agreements between employers and employees to engage in individual arbitration of wage and hour disputes. This holding prohibits class action proceedings for alleged Federal Labor Standards Act (FLSA) violations by employees subject to such agreements. One potential concern is that this holding may have a chilling effect on the reporting of wage and hour violations, rendering state and federal enforcement processes ineffective by eliminating employees' access to administrative enforcement. Indeed, we observe a large drop in reports of wage and hour law violations to the National Labor Relations Board Wage and Hour Division (see Figure 2.1). If such decreases in federal-level legal enforcement reduce employers' incentives to comply with the

Fair Labor Standards Act and other wage and hour regulations, the decision may lead to increases in the prevalence of subminimum wage payments, overtime violations, or other forms of wage theft.

Figure 2.1: Reported Wage and Hour Violations by Violation Start Month

(a) Overall (b) By Enforcement Status



Notes: Data source is NLRB Wage and Hour decision data from March 2016 to December 2019. Includes all FLSA violations believed to have started in a given month. Excludes mass violation investigations, defined as complaints in the top 1% of all observations, consisting of more than 230 violations in a single complaint.

In certain states, another avenue of resolution for wage and hour disputes exists in the form of state-level administrative agencies. While most states have an agency that administers and enforces wage and hour laws, there is considerable heterogeneity in the scope and functions across state-level agencies regarding dispute resolution. We have identified a key enforcement capability of state-level agencies; upon determining that an employee has been aggrieved in a wage theft dispute, some state-level agencies have the authority to order the employer to pay back wages (issue remedies).[1] [2] Issuing remedies is a strong enforcement capability because low wage earners may struggle to justify costly private right of action (including class action lawsuits). Filing a complaint with an administrative agency

[1] We use "order the employer to pay back wages," "order the employer to provide back pay," and "issue remedies" interchangeably.

[2] Using the Wage and Hour Laws: A State-by-State Survey, we identified five different administrative functions of state-level wage and hour law enforcement agencies in addressing wage theft – investigating firms for possible wage and hour law violations of their own initiative, handling complaints of wage and hour law violations (including conducting investigations, deposing witnesses, holding administrative hearings, and determining whether the employee has been aggrieved), issuing penalties, ordering employers to give back pay, and the power to litigate the case on behalf of the aggrieved employee. We then determined whether each state's agency had each of the five functions. See Supplemental Section on Treatment Status.

that can issue remedies is a low-cost and low-risk alternative, and, therefore, an inducement to seek resolution for a perceived incidence of wage theft. As such, the cost of violating wage and hour laws is higher for employers in these states. Following the "chilling effect" of the *EPIC* decision (demonstrated by Figure 2.1), then, we may expect a smaller increase in overtime violations in the strong enforcement states relative to the weak enforcement states.

In this paper, we present a descriptive analysis to explore whether the decision in *Epic Systems v. Lewis* caused differential changes in overtime work and apparent overtime violations between strong and weak enforcement states. We employ data from the Outgoing Rotation Group/Earner Study (ORG/ES) of the Current Population Survey for the period of May 2016-December 2019 and exploit the discrepancy between two measures of earnings, as well as number of hours worked, in the ORG/ES to capture instances of apparent underpayment for overtime work. Specifically, we impute net weekly earnings for each wage-earner by multiplying their hours worked in the previous week and their hourly wage; if an individual works more than 40 hours per week, we multiply their hourly wage by forty, and then multiply hours worked in excess of 40 by 1.5 times their hourly wage. Once we obtain this measure of weekly earnings, we compare it with the weekly earnings (from a typical week) reported by the respondent in the ORG/ES. If their reported weekly earnings is an "underpayment" of more than 10%, we classify this as an instance of apparent underpayment for overtime work, or "overtime violation". We define "treatment" as having legal recourse in the form of administrative enforcement. As such, in our analysis, treatment states are those under strong enforcement regimes, and comparison states are those under weak enforcement regimes. Using both two-way fixed effects and event study methodologies, we do not find a differential change in overtime work or overtime violations following the *EPIC* decision by potency of state-level administrative enforcement.

A few states do not have a codified overtime statute enshrined in their wage and hour law code, despite having an strong administrative enforcement regime. As a robustness check, we remove these states from the sample and repeat our empirical analyses. Still, we do not find a differential effect of the *EPIC* decision on either overtime work or overtime violations.

Much of the economics literature on wage theft in the United States is focused on sub-minimum wage payment (Neumark and Wascher (1992)). It is easy to measure sub-minimum wage payment in survey data. Moreover, the changes in minimum wage offer plausible policy variation. Our contribution in this paper is twofold; first, we devise a novel measure to measure underpayment for overtime work using Current Population Survey data; and second, we exploit variation in administrative enforcement capabilities across states using novel, hand-collected information on state-level wage and hour laws.

2.2 Background

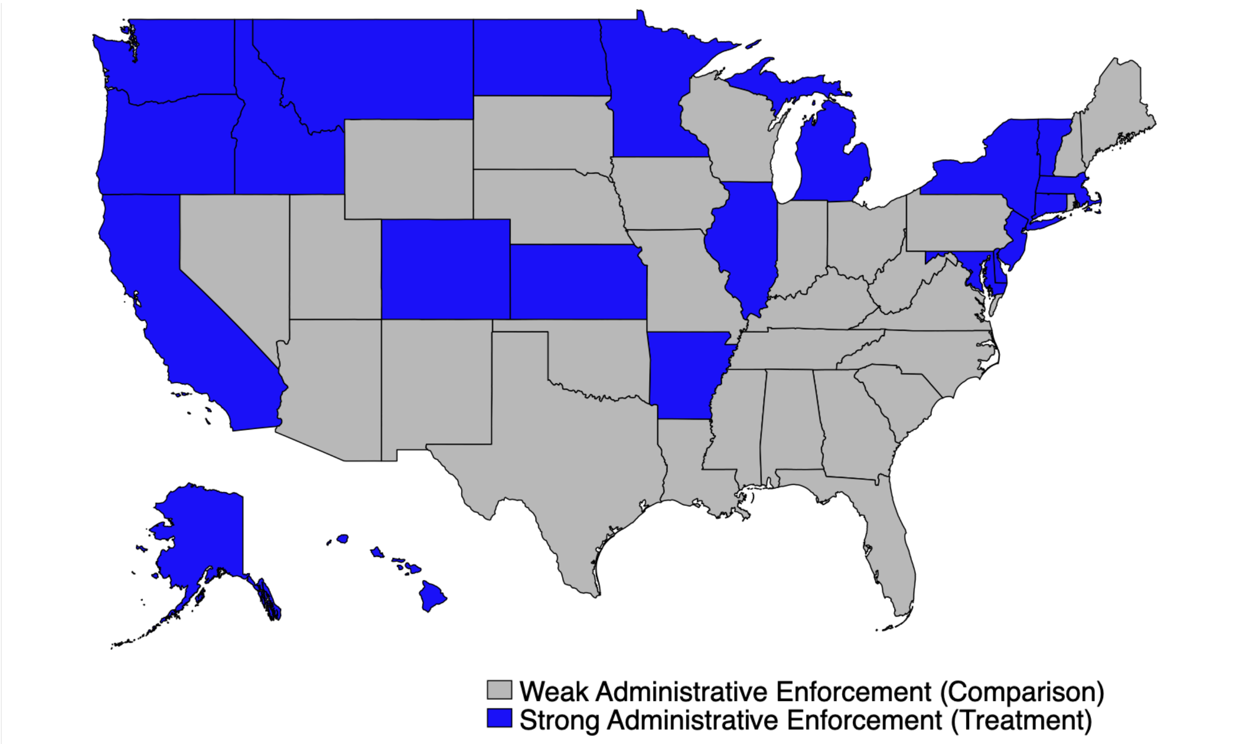
2.2.1 Administrative Enforcement of Wage and Hour Laws

Administrative enforcement of wage and hour laws varies greatly from one state to another. Four states (Alabama, Florida, Louisiana, and Mississippi) do not have a wage and hour agency that handles complaints against employers. Among the state-level agencies that handle complaints, the procedure is roughly as follows: the agencies are authorized and empowered to investigate the complaints.[3] These investigations broadly include actions such as entering the premises of a company (announced or otherwise, depending on the power of each specific agency), seizing documents and other items, issuing subpoenas, deposing witnesses, and holding administrative hearings. After investigating, agencies then determine whether a violation of wage and hour laws has occurred.

Upon adjudicating in favor of the employee, roughly half of state-level agencies have the power to issue a legally binding order to an employer to pay back wages. These "strong enforcers" constitute the treated group in our analysis, because we define treatment as hav-

[3] While some states investigate firms for instances of violations proactively, these are a subset of the states whose investigations are initiated by complaints of violations.

Figure 2.2: State Enforcement Status Map



Source: Wage and Hour Laws: A State-by-State Survey (2010)

ing administrative enforcement as an avenue of legal recourse. [4] [5] In the period before *EPIC*, employees in the "weak enforcer" states had access to administrative enforcement in the form of the National Labor Relations Board. Post-*EPIC*, these employees lost access to any administrative enforcement. Because these states go from receiving treatment (administrative enforcement) to not receiving treatment, "weak enforcers" constitute the comparison group in our analysis. In Figure 2.2 we include a map distinguishing the strong enforcers (treated group) from the weak enforcers (comparison group).[6] [7]

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[4] We require a state to not only be able to issue such an order, but to also be able to enforce this order in court.
Some states, in addition to ordering payment of back wages, may also order the employer to pay damages to the aggrieved employee. We do not require ordering payment of damages to be considered a strong enforcer in our analysis.

[5] We call states with such strong enforcement capabilities "strong enforcers" or "enforcers" moving forward.

[6] We also use the term "non-enforcers" to describe the weak enforcers (comparison group).

[7] In Section 2.8.1, we include a full description of the enforcement capabilities of each state-level wage and hour administrative enforcement agency.

2.2.2 Conceptual Framework

A state with a strong administrative enforcement regime, as we define it, offers employees a channel of remedy for an overtime violation that is low cost to pursue. In states with weak administrative enforcement, employees relied on National Labor Relations Board (NLRB) Wage and Hour Division, at the federal level, to handle administrative enforcement (or pursued collective action suits under the FLSA) for remedy. In the wake of the EPIC decision, not only was private right of action curtailed; there is evidence of a chilling effect causing NLRB complaints to fall, as well (see Figure 2.1). Without private right of action or the confidence to seek administrative enforcement at the federal level, employees in states with weak administrative enforcement find their options for remedy severely limited by the *EPIC* decision. In such states, employers' cost of engaging in overtime violations has decreased relative to states with strong enforcement.

Thus, while we expect overtime violations to increase overall as a result of the *EPIC* decision, we hypothesize that states with strong administrative enforcement regimes (treatment group) will see a smaller increase in overtime violations relative to states with weak administrative enforcement regimes (comparison group).

Moreover, it is possible that an employer may respond to a lower cost of engaging in overtime violations by asking their employees to work more overtime hours. In strong enforcement states, we expect pre-*EPIC* overtime hours to be lower than weak enforcement states, due to greater cost of potential overtime violations. We hypothesize that, following the decision, employees in states with weak administrative enforcement work more overtime compared to those in states with strong enforcement.

In the next section, we discuss the data used in our analysis, as well as our methodology for measuring overtime violations.

2.3 Data

We use Current Population Survey (CPS) data maintained by the Integrated Public Use Microdata Series (Flood et al. (2023)). Individual respondents in each household are surveyed over four consecutive calendar months per year for two consecutive years. Individuals are surveyed in the same calendar months in both years; that is, they are surveyed in months 1-4, not surveyed in months 5-12, then surveyed again in months 13-16.[8]

In each survey month, the CPS gathers data on individuals' demographics, labor market activity, and education, among other characteristics. To identify potential underpayment for overtime work, we use the Outgoing Rotation Group/Earner Study (ORG/ES), administered in months four and eight that an individual is surveyed. For those who are age 15+ and employed as a wage or salaried worker, this module includes questions on labor market aspects such as pay frequency, hourly wage, weekly earnings, and weekly hours worked. Using discrepancies between these reports of income, we construct a measure of underpayment for overtime work. Our method is outlined in the next section.

2.3.1 Measuring Underpayment for Overtime Work

In the ORG/ES module, each person who is employed and earns an hourly wage reports the following: (1) hours worked last week in their main job,[9] (2) their hourly wage rate, and (3) their usual weekly earnings. We compute the net pay each individual should have received in the previous week by multiplying the hours worked in the previous week by their hourly wage. If the individual worked overtime – that is, over 40 hours in the previous week – we multiply each hour in excess of 40 by time-and-a-half, or 1.5 times their hourly wage, and then add the overtime earnings to the product of 40 hours times their hourly wage. That is,

[8] For example, if an individual is surveyed in December 2013, January 2014, February 2014, and March 2014, they will be surveyed again in December 2014, January 2015, February 2015, and March 2015.

[9] ORG/ES includes information on secondary jobs, as well. However, since we have hourly wage rates for the first job, alone, we do not consider second jobs.

$ImputedWeekEarn_{ist} = (40 * HourWage_{ist}) + ((WorkHours_{ist} - 40) * HourWage_{ist} * 1.5).$

We then compare this imputed measure of weekly earnings that we construct against the reported usual weekly earnings measure reported by each individual in the CPS ORG/ES, as follows:

$$PercentDifference_{ist} = \frac{ImputedWeekEarn_{ist} - WeekEarn_{ist}}{WeekEarn_{ist}} \times 100$$

In the above equation, WeekEarn represents weekly earnings reported by an individual in the CPS. An individual is underpaid for their overtime work if they reported having earned at least ten percent less than the constructed weekly earnings measure; we define this apparent "underpayment" as an Overtime Violation. This is one of the main outcome variables in our analysis (along with Overtime Work).

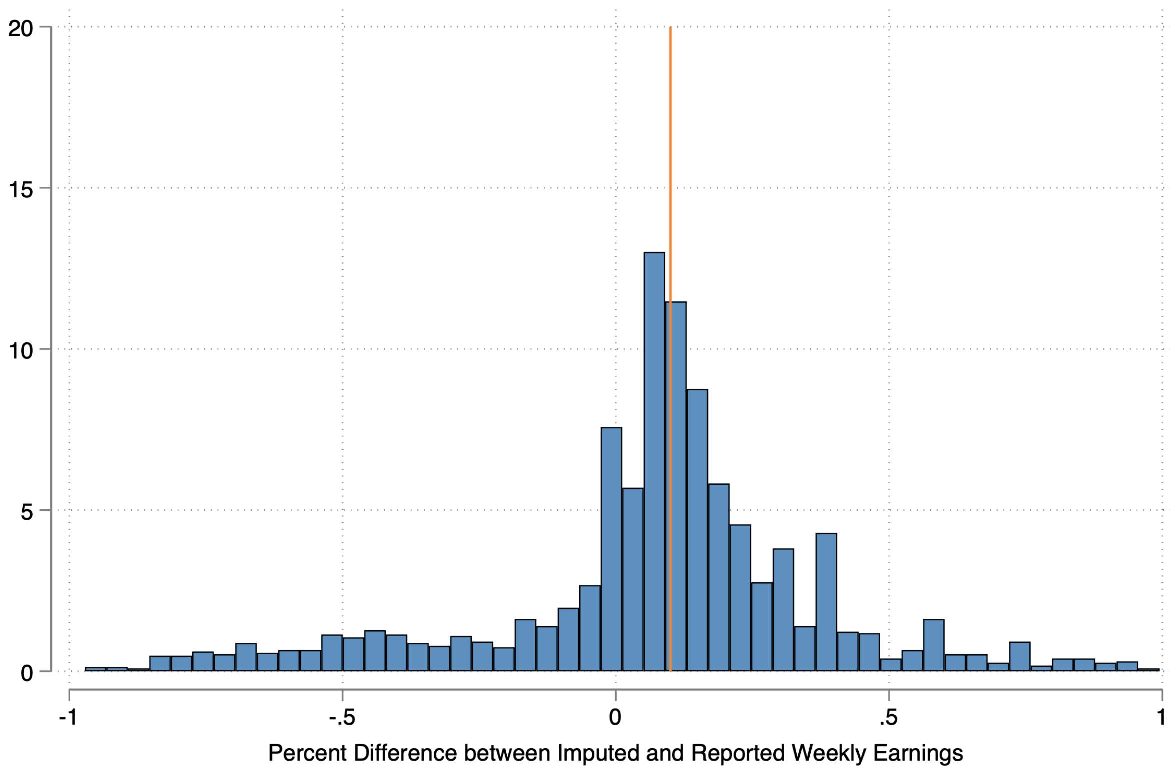
We include a histogram of the distribution of $PercentDifference_{ist}$ in Figure 2.3 for all instances of overtime violations.

2.3.2 Sample and Sample Restrictions

Our sample consists of prime-age workers, 15-55 years old. As noted above, our measure of underpayment for overtime work is only relevant for those paid by the hour, so we include only hourly wage-earning employees in the sample. Due to the COVID-19 pandemic's profound effect on the U.S. labor market, it is difficult to treat pre-pandemic and post-pandemic work, especially with respect to overtime work and pay, as though they are the same. For this reason, we drop all observations post-2020 and conduct a pre-pandemic analysis.

The Affordable Care Act's employer mandate, which requires employers to provide health insurance, took effect in 2016. We anticipate varying responses from employers in states with strong versus weak enforcement regarding employee work hours. Our analysis starts in May 2016, once employer reactions to the mandate have stabilized. Our analysis

Figure 2.3: Percent Difference between CPS WeekEarn and Imputed WeekEarn



Source: Current Population Survey, Outgoing Rotation Group/Earner Study 2016-2019
This histogram illustrates the distribution of the percent difference between imputed weekly earnings and reported weekly earnings in the CPS data. The construction of the percent difference is discussed in Section 2.3.1.
To obtain this distribution, we include only instances of Overtime Work – that is, when an individual reports working more than 40 hours in the previous week. We also truncate the distribution to only include percent differences between -100% and 100% (labeled as -1 and 1 in this graph), in order to better illustrate the shape of the distribution, as outliers beyond this range can obscure the main patterns.
The orange line, drawn where the percent difference is 10% (labeled 0.1), represents the threshold for the overtime violations variable: all instances where the percent difference between imputed weekly earnings and reported weekly earnings are 10% or more are classified as an instance of overtime violation in our analysis.

ends in December 2019, just before the COVID-19 pandemic, in order to conduct a pre-pandemic analysis.

In addition, we consider only workers whose occupation and industry of employment are covered by federal overtime laws. Certain occupations and industries are explicitly exempt from overtime compensation laws, and others are exempt due to the nature of the pay structure prevalent within that occupation. For example, many tipped workers such as restaurant servers and hotel cleaning staff, as well as many fire protection employees employed in the National Parks Service, whose schedules require unique flexibility are mostly exempt from overtime protections. We have included a full list of exempt occupations, along with details on why we have excluded them from our analysis, in Section 2.8.2.

2.4 Methodology

We hypothesize that, relative to employers in states with strong enforcement regimes, employers in states with weak administrative enforcement will respond to the Epic v. Lewis decision by (1) increasing overtime hours of current employees greater rates while (2) simultaneously underpaying them at greater rates. We make use of a basic two-way fixed effects methodology, modeling the outcome y_{ist} (overtime work and overtime violations) for person i, month-year t, and state s, as follows:

$$y_{ist} = \alpha_s + \lambda_t + \beta Post_t \times StrongEnforcer_s + \epsilon_{ist}$$

$Post_t$ is defined to indicate whether a month-year occurs after the EPIC v. Lewis decision date of May 2018. $StrongEnforcer_s$ indicates whether a state has a strong administrative enforcement agency. To control for time-invariant factors specific to each state, we include state fixed effects, α_s. To control for factors specific each month-year, we include a vector of year-month fixed effects, λ_t. Finally, we use $\epsilon_i st$ to represent the error term.

β represents the difference between the strong enforcers' and weak enforcers' change in the outcome variables (overtime work and overtime violations) following the EPIC decision. Because we expect both outcomes to to increase more in the control group states than in the treatment states, we expect this coefficient to be negative in both specifications. We discuss estimates of β in the next section.

We then expand the difference-in-differences specification to a richer event-study, where we nonparametrically model y_{ist} as a function of each event-period relative to the EPIC v. Lewis decision month-year. Specifically:

$$y_{ist} = \alpha_s + \lambda_t + \sum_{j=-21}^{18} \tau_j E_{st}^j + \epsilon_{ist}$$

E_{st}^j are a set of dummy variables indicating that a month-year is j periods away from the EPIC decision in May 2018, and the τ_j coefficients represent the change in overtime work or overtime violations in each time period. In the next section, we discuss our plot of the τ_j coefficients.

2.5 Results

Table 2.1: The Effect of EPIC v. Lewis on Overtime Work and Violations

	(1) Overtime Work	(2) Overtime Violations
STRONGENFORCER × POST	-0.00478	0.00216
	(0.00456)	(0.00549)
Observations	65039	65039
Pre-*EPIC* Mean (Outcome Variable)	.0625	.1190

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source: Current Population Survey, 2016-2019
In this table, we report estimates of β in the two-way fixed effects specification given in Section 2.4. The outcome used for each estimate is given in the column headings. “Overtime work” is a binary variable that equals 1 if a person works more than 40 hours. “Overtime Violations” is a binary variable that equals 1 if a person works overtime and their self-reported weekly earnings is more than 10 percent of an apparent underpayment relative to the imputed earnings using their reported hours worked and usual weekly hours worked. We provide a detailed explanation on this measure in Section 2.3.1.

In Table 2.1, we present the results of our two-way fixed effects specifications with state and month fixed effects. Neither overtime work nor apparent overtime violations change significantly more between states with strong enforcement regimes and states with weak enforcement regimes. We find that the growth in the share of workers who worked overtime is 0.478 percentage points more negative in strong enforcement states following the *EPIC* decision, with a 95 percent confidence interval of (-0.014,0.004). By comparison to the pre-*EPIC* average rate of overtime work, 6.25 percent, our point estimate represents a modest 8 percent decrease. The lower bound of the confidence interval represents a 22.4 percent decrease relative to the pre-*EPIC* average, and the upper bound represents a 6.4 percent increase. The interval contains zero.

We also find that rates of apparent overtime violations have grown by an additional 0.216 percentage points in the strong enforcement states relative to weak enforcement states, which represents a 1.7 percent increase relative to the average overtime violation rate of 11.9 percent in the pre-*EPIC* period. The confidence interval suggests that, with 95 percent confidence, the true effect is no more negative than -0.009 percentage points (representing a 7.5 percent decrease compared to the pre-*EPIC* period overtime violation rate) and no more positive than 0.013 percentage points (a 10.9 percent increase).

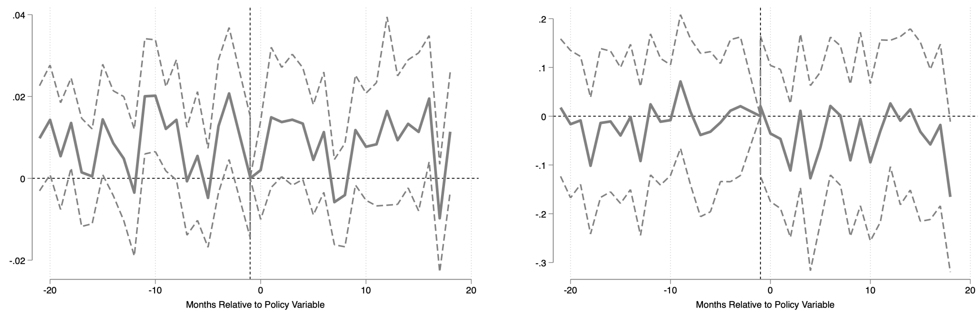
While we do not find statistically significant effects, the direction of the overtime work result is consistent with our hypothesis – incidences of overtime work appear to have increased in states with weak administrative enforcement relative to states with strong enforcement regimes. Perhaps this result can help explain the positive direction of the overtime violations result; if employers under weak administrative regimes are increasing their employees' overtime work hours, it is possible that the incidences of underpayment for overtime work have increased, while each underpayment amount has decreased. In this case, perhaps our measure of overtime violations does not capture the true increase in overtime violations.

Figure 2.4 shows event plots of the estimates using the event study specification. Reassuringly, there appear to be no differential pre-trends in either outcome between weak and strong enforcement states. Both plots show no effect of the EPIC case on the evolution of the difference in outcomes between strong and weak administrative enforcers.

Figure 2.4: Two Way Fixed Effects: Dynamic Effects Event Plot

(a) Overtime Work

(b) Overtime Violations



Source: Current Population Survey Earner Study/Outgoing Rotation Group, 2016-2019. In these graphs, we plot estimates of the coefficients τ_j in the event study specification given in Section 2.4.

2.6 Robustness

Four states (Delaware, Idaho, Kansas, and Minnesota) have strong enforcement regimes, but do not have overtime pay protections enshrined in their wage and hour law. Essentially, they do not have an overtime statute to strongly enforce. As a robustness check, we drop observations in those four states from our two way fixed effects and event study analyses.

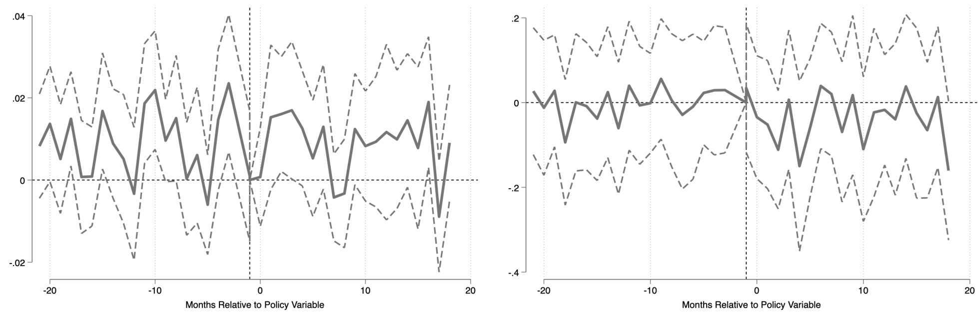
Table 2.2: The Effect of EPIC v. Lewis on Overtime Work and Violations

	(1) Overtime Work	(2) Overtime Violations
STRONGENFORCER × POST	-0.00562	0.00155
	(0.00450)	(0.00596)
Observations	65039	65039

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source: Current Population Survey, 2016-2019
In this table, we report estimates of β in the two-way fixed effects specification given in Section 2.4. The outcome used for each estimate is given in the column headings. "Overtime work" is a binary variable that equals 1 if a person works more than 40 hours. "Overtime Violations" is a binary variable that equals 1 if a person works overtime and their self-reported weekly earnings is more than 10 percent of an apparent underpayment relative to the imputed earnings using their reported hours worked and usual weekly hours worked. We provide a detailed explanation on this measure in Section 2.3.1.
In this robustness analysis, we drop the four states which have strong enforcement regimes but do not have an overtime statute.

Figure 2.5: Two Way Fixed Effects: Dynamic Effects Event Plot

(a) Overtime Work (b) Overtime Violations



Source: Current Population Survey Earner Study/Outgoing Rotation Group, 2016-2019. In these graphs, we plot estimates of the coefficients τ_j in the event study specification given in Section 2.4. In this robustness analysis, we drop four states from the sample. These states are strong enforcers, but do not have an overtime statute.

The results from the regression are presented in Table 2.2, and the results from the event study are presented in Figure 2.5. There is little change in the results from either analysis.

2.7 Conclusion

The policy environment of wage and hour laws, including enforcement, is complex. In our paper, we attempt to codify one aspect of administrative enforcement and understand the extent of its effect on employers' willingness to engage in overtime violations and other forms of wage theft. Perhaps other dimensions of administrative enforcement play a greater role in an employer's tendency to withhold overtime pay.

Moreover, employer response to fewer complaints may vary, as well. Employers may engage in other forms of wage theft, including misclassification of employees. This allows employers to evade paying benefits, overtime, and certain taxes, thereby reducing labor costs. As a result, workers may be denied legal protections and compensation they are entitled to under employment laws. It is possible that, following the decision in *EPIC*, employers engage in misclassification, and the underpayment is unobserved in our analysis.

Lastly, it is possible that state-level administrative enforcement mechanisms have been rendered ineffective following the *Epic v. Lewis* decision, particularly in the face of the sharp decline in reporting of violations after the decision (see Figure 2.1). If state enforcement, federal enforcement, and private lawsuits are complements rather than substitutes, state enforcement regimes may not be sufficient to fill any gaps left by reduced access to other remedies.

2.8 Supplemental

2.8.1 State-level Administrative Enforcement Capabilities

With the help of *Wage and Hour Laws: A State-by-State Survey* (McGillivary (2011)), we categorized the enforcement capabilities of state-level agencies into five major areas:

(1) **Investigations:** This signifies an agency's authority and power to initiate investigations into employers proactively, beyond merely responding to complaints.

(2) **Handling Complaints:** This is a broad category that encompasses an agency's power to receive wage and hour law-related complaints, conduct investigations into employers based on the complaints, depose witnesses, hold administrative hearings, and, finally, adjudicate the claim and determine whether the employee has been wronged. In all but four states (Alabama, Florida, Louisiana, and Mississippi), the wage and hour agency has this enforcement capability. Some states allow anonymous complaints while others do not, and some states explicitly describe their adjudication process as a "mediation"; however, as long as the above procedure roughly applies, we consider that agency to be one which handles complaints.

(3) **Issuing Penalties:** Upon finding that an employer has violated a wage and hour law, the agency can issue a penalty in the form of an administrative fine.

(4) **Issuing Remedies:** Upon finding that an employer has violated a wage and hour law, the agency can order the employer to pay back wages (and damages, if applicable in that state) to the aggrieved employees.

(5) **Litigation:** This is a broad category that encompasses an agency's power to bring the complaint before a court on behalf of the complainant (employee). In certain states, all other tools of the agency must be exhausted before bringing the case before a court. In other states, the agency can bring a lawsuit if an employer fails to pay back wages or fines. In all these cases, we categorize that agency as having the power to litigate.

Among those agencies that handle complaints, a majority have the power to issue remedies. Of those that can issue remedies, some states are able to enforce that order in a court. These state-level agencies are the treated group in our analysis. A full list of states and their enforcement capabilities is included in the table below.

2.8.2 Excluded Occupations, Industries, and Sectors

The following occupations are exempt from our analysis. We provide the exact industries and sectors for which we exclude the occupations, as well as a brief rationale.

(1) Buyers and Purchasing Agents, farm prod

- Industry: All
- Sector: All
- Exclusion Rationale: Livestock auction workers, Buyers of agricultural products, and Farm implement salespeople are exempt from overtime work FLSA

(2) Detectives and criminal investigators

- Industry: All

- Sector: Federal government employees
- Exclusion Rationale: Federal Criminal investigators are exempt from overtime work FLSA protections

(3) Waiters and waitresses

- Industry: All
- Sector: All
- Exclusion Rationale: This occupation is comprised of workers who are commissioned sales employees. See note below.

(4) Maids and Housekeeping cleaners

- Industry: All
- Sector: All
- Exclusion Rationale: This occupation is comprised of workers who are commissioned sales employees. See note below.

(5) Baggage porters, bellhops, and concierge

- Industry: All
- Sector: All
- Exclusion Rationale: This occupation is comprised of workers who are commissioned sales employees. See note below.

(6) Ushers, lobby attendants, and ticket takers

- Industry: Motion picture and video industries
- Sector: All

- Exclusion Rationale: Motion picture theater employees are exempt from overtime work FLSA protections

(7) Childcare workers

- Industry: Individual and family services, Private households
- Sector: All
- Exclusion Rationale: Babysitters on a casual basis and Domestic employees who live-in are exempt from overtime work FLSA protections

(8) Personal care aides

- Industry: Private households
- Sector: All
- Exclusion Rationale: Companions for the elderly and Domestic employees who live-in are exempt from overtime work FLSA protections

(9) Personal care and service workers, all

- Industry: Private households
- Sector: All
- Exclusion Rationale: Companions for the elderly and Domestic employees who live-in are exempt from overtime work FLSA protections

(10) Switchboard operators, including answer

- Industry: All
- Sector: All
- Exclusion Rationale: Switchboard operators are exempt from overtime work FLSA protections.

(11) Fishers and related fishing workers

- Industry: All
- Sector: All
- Exclusion Rationale: Fishing occupations are exempt from overtime work FLSA protections.

(12) Elevator installers and repairers

- Industry: All
- Sector: All
- Exclusion Rationale: Country elevator workers (rural) are exempt from overtime work FLSA protections. We take care to exclude only those workers who are based in rural locations.

(13) Aircraft pilots and flight engineers

- Industry: Air Transportation
- Sector: Private, for profit
- Exclusion Rationale: Airline Employees are exempt from overtime work FLSA protections.

(14) Flight attendants

- Industry: Air Transportation
- Sector: Private, for profit
- Exclusion Rationale: Airline Employees are exempt from overtime work FLSA protections.

(15) Taxi drivers and chauffeurs

- Industry: All
- Sector: All
- Exclusion Rationale: Taxicab drivers are exempt from overtime work FLSA protections.

(16) Locomotive engineers and operators

- Industry: Rail Transportation
- Sector: All
- Exclusion Rationale: Rainroad employees are exempt from overtime work FLSA protections.

(17) Railroad brake, signal, and switch oper

- Industry: Rail Transportation
- Sector: All
- Exclusion Rationale: Rainroad employees are exempt from overtime work FLSA protections.

(18) Railroad conductors and yardmasters

- Industry: Rail Transportation
- Sector: All
- Exclusion Rationale: Rainroad employees are exempt from overtime work FLSA protections.

(19) Subway, streetcar, and other rail trans

- Industry: Rail Transportation
- Sector: All

- Exclusion Rationale: Rainroad employees are exempt from overtime work FLSA protections.

(20) Sailors and marine oilers

- Industry: All
- Sector: All
- Exclusion Rationale: Seamen on American vessels and Seamen on other than American vessels are exempt from overtime work FLSA protections.

(21) Ship and boat captains and operators

- Industry: All
- Sector: All
- Exclusion Rationale: Seamen on American vessels and Seamen on other than American vessels are exempt from overtime work FLSA protections.

Table 2.3: State-by-State Administrative Enforcement Capabilities

State	**Investigations**	**Handling Complaints**	**Issuing Penalties**	**Issuing Remedies**	**Litigation**
Alabama					
Alaska	✓	✓	✓	✓	✓
Arizona	✓	✓	✓	✓	
Arkansas	✓	✓	✓	✓	✓
California	✓	✓	✓	✓	✓
Colorado	✓	✓	✓	✓	✓
Connecticut	✓	✓	✓	✓	✓
Delaware	✓	✓	✓	✓	✓
D.C.	✓	✓	✓	✓	✓
Florida					
Georgia	✓	✓			✓
Hawaii	✓	✓	✓	✓	✓
Idaho		✓	✓	✓	✓
Illinois	✓	✓	✓	✓	✓
Indiana	✓	✓	✓	✓	
Iowa		✓			✓
Kansas		✓	✓	✓	✓
Kentucky	✓	✓			✓
Louisiana					
Maine		✓			✓
Maryland	✓	✓	✓	✓	✓
Massachusetts	✓	✓	✓		✓
Michigan	✓	✓	✓	✓	✓
Minnesota	✓	✓	✓	✓	✓
Mississippi					
Missouri		✓			
Montana	✓	✓	✓	✓	✓
Nebraska		✓			
Nevada	✓	✓	✓	✓	
New Hampshire	✓	✓		✓	
New Jersey	✓	✓	✓	✓	✓
New Mexico		✓			✓
New York	✓	✓	✓	✓	✓
North Carolina		✓			✓
North Dakota	✓	✓	✓	✓	✓
Ohio	✓	✓		✓	
Oklahoma		✓	✓	✓	
Oregon	✓	✓	✓	✓	✓
Pennsylvania		✓			✓
Rhode Island		✓			✓
South Carolina		✓	✓		
South Dakota		✓		✓	
Tennessee		✓			✓
Texas		✓	✓	✓	
Utah		✓		✓	
Vermont	✓	✓	✓	✓	✓
Virginia		✓	✓	✓	
Washington	✓	✓	✓	✓	✓
West Virginia		✓			✓
Wisconsin		✓			
Wyoming		✓		✓	

Chapter 3

The Relationship between Female Deity Temple Exposure and Intimate Partner Violence

3.1 Introduction

Understanding the historical roots of attitudes towards women can provide valuable insights into addressing and mitigating harmful behaviors directed towards women. Intimate partner violence (IPV), one such behavior, is a public health issue around the world. According to the World Health Organization's handbook on IPV, of all women in various parts of the developing world who have ever been in a relationship, 13-61% reported ever having experienced physical violence by a partner, and 40-70% of female murder victims were killed by their husband or boyfriend.[1] IPV, therefore, is a public health crisis for many women in the developing world.

In India, according to data from the National Family and Health Surveys, 30% of women who have ever been in a relationship have endured intimate partner violence at some time in their lives. For the South Indian state of Tamil Nadu (See Figure 3.1), this same statistic much higher, at 42%.[2] In many parts of India, there is high acceptability of intimate partner violence; IPV is vastly underreported, and while IPV is illegal, it is poorly

[1] Heise and Garcia-Moreno (2002). These numbers are from a study of 10 developing countries.
[2] Paswan et al. (ndia)

enforced.[3] Moreover, there is a dearth of services such as domestic violence shelters (ICRW, 2000). It is important to find a viable solution to this high incidence of IPV in such parts of India as Tamil Nadu.

In fact, efforts are made to appeal to historical, cultural goddess-worship as a way to decrease gender-based violence; an NGO sponsored billboard campaign in 2012 depicted goddesses bearing physical signs of intimate partner victimization, urging the public to reflect on the inherent contradiction of venerating female deities while tolerating widespread abuse against women.[4] In order for this approach to be effective, it is important to understand the relationship between the perception of female goddesses and intimate partner violence.

In this paper, I present a descriptive analysis of the relationship between the incidence of intimate partner violence and the historic exposure to female deity temples. I use hand-collected data on historical temples, constructed 1500-500 years ago by kings with idiosyncratic preferences for deities, along with information on which temples are devoted to female deities, in order to construct a measure of exposure to goddesses at the district level. I focus on the south Indian state of Tamil Nadu due to the significant resources required for the hand-collection of data.[5] Specifically, I find the share of total temples in each district which are goddess-temples. For IPV data, I use individual-level data from the National Family and Health Survey (maintained by the Demographic and Health Surveys) Section on Domestic Violence, which questions women who were ever married on whether they've been physically, sexually, or emotionally abused. If a woman reports being physically or sexually abused, for purposes of my analysis, I consider it to be an incidence of intimate partner violence. I employ an conditional on observables methodology to study the relationship between the treatment, exposure to goddesses, on the incidence of intimate partner violence, the outcome variable.

[3] Burton et al. (2000)
[4] Jha (2013)
[5] I discuss issues of external validity in a later section.

If female-deity exposure is associated with a lower incidence of IPV, it is consistent with the hypothesis that the exposure to goddesses engenders a culture of respect towards women and lower acceptability of IPV. If exposure is associated with higher incidence of IPV, it may suggest a more complicated interpretation of the goddesses. There is documented evidence that aspects of religion are sometimes weaponized against women[6] . If women are told to endure hardships as a virtue, as the goddesses are sometimes represented as enduring[7] , they may view abuse and self-sacrifice as one such hardship and forego seeking help, reducing the risk of consequences of IPV for abusive husbands.

Preliminary results show evidence that female-deity exposure is associated with a higher incidence of IPV.

The economics literature documents that historical events and interventions affect outcomes in the modern-day[8] ; we further understand the impact of historical events and family structure on outcomes for women, including labor force participation and intimate partner violence (Tur-Prats (2019); Alesina et al. (2013)). A strand of the intimate partner violence literature investigates the determinants of IPV; short term determinants of IPV associated with the bargaining power distribution in the household (Tauchen et al. (1991); Farmer and Tiefenthaler (2003); Aizer (2010)), while Tur-Prats (2019) investigates a historical determinant of IPV - historic roots of family types and how they affect intimate partner violence outcomes. My paper dovetails these two primary areas of literature: the impact of historical influences on contemporary behavior and the potential causal relationships between perceptions of women and women's outcomes. It delves into the intersection of these fields by examining how culture shapes current gender-related outcomes.

Specifically, I study the cultural determinants of IPV in this paper. Sabarwal et al. (2012) explore the relationship between a husband's unfulfilled son preference (comparing husband's desired family composition to actual family composition) and the risk for the

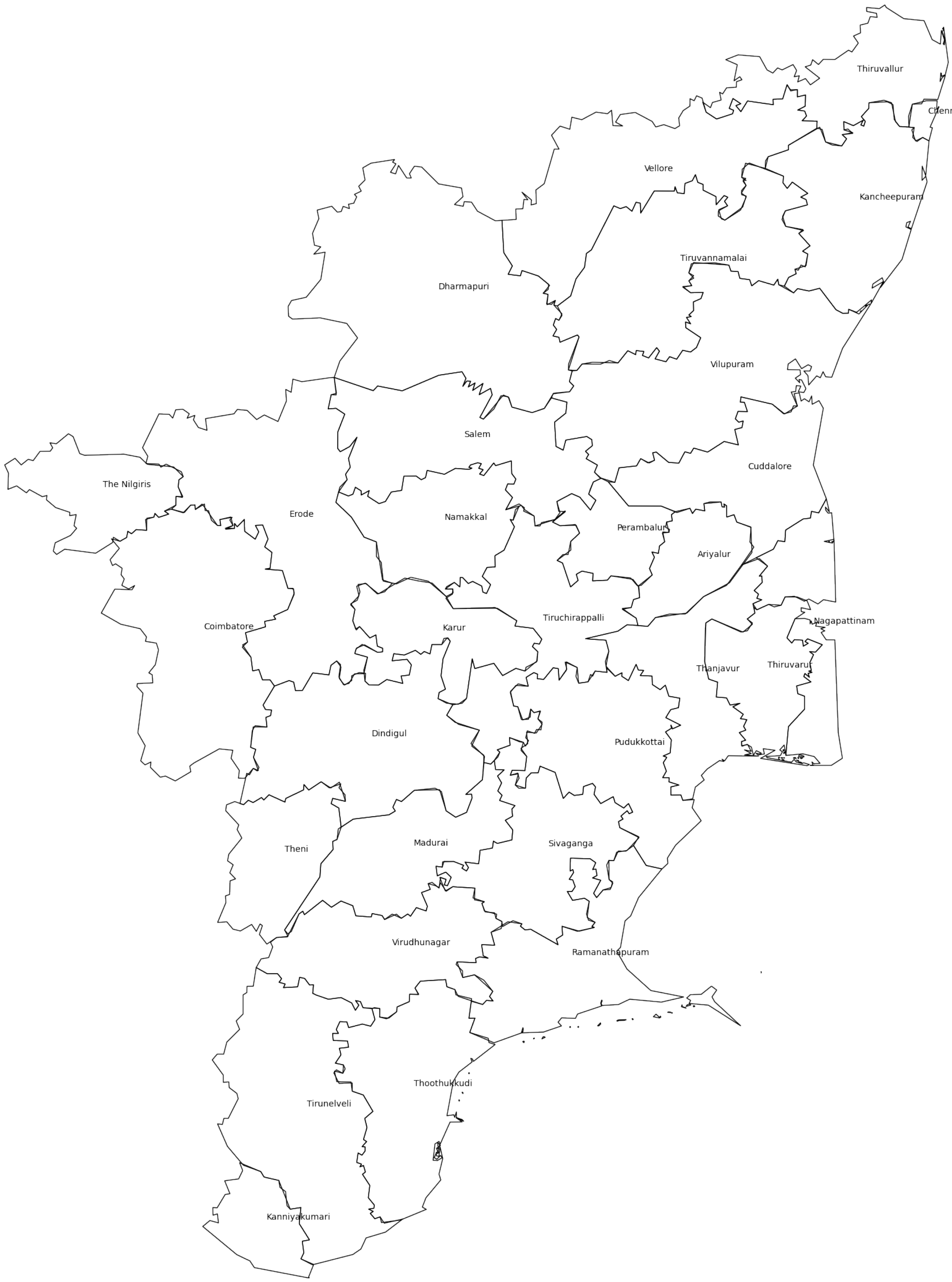
[6] Levitt and Ware (2006)
[7] Jayasundara et al. (2017)
[8] See Lowes et al. (2017); Dell and Olken (2020); Dell et al. (2018); Caicedo (2019)

wife's IPV victimization in India. While their study finds no significant association between unfulfilled son preference and IPV risk, it highlights the importance of understanding cultural determinants of IPV. My research complements this work by examining a different cultural aspect—historical exposure to female deity temples—and its impact on IPV, contributing a new perspective on how cultural and historical elements influence gender-based violence.

More broadly, this research question focuses on women's outcomes in a developing country, exploring the origins of behavior patterns concerning women. This can inform policy interventions to improve these outcomes in developing countries, thereby contributing to the extensive literature on reducing intimate partner violence, increasing women's literacy, improving women's health, and enhancing resource allocation toward women.

In the next section, I provide background on the historical temples as a treatment.

Figure 3.1: Tamil Nadu Map of Districts



3.2 Background and History

3.2.1 The Three Kingdoms: History and Temple Construction

The first historic temple complexes in Tamil Nadu appeared around the year 500. These were very expensive projects that only kings could afford to construct. For the next thousand years, three kingdoms in Tamil Nadu constructed almost all of the historic temples in Tamil Nadu. These three dynasties, Pandyas, Cholas, and Cheras, maintained relevance and power for a millennium. Even before the year 500, each of the dynasties maintained some regional stronghold in the southern-most part of the Indian peninsula, but they were also each a dominant power of the region at some point during the thousand-year period of 500-1500 AD.[9] During this time, as a matter of legacy, the reigning kings of each dynasty had a practice of constructing temples in honor of a deity that they favor. These favored deities could be quite idiosyncratic, as sons of kings who favored a particular deity were known to construct temples dedicated to different deities than their fathers, once they were king.[10] Between the change of dynasties and the construction of temples based on a king's preference, I propose that the "gender assignment" of deities across these historic temples is random.

3.2.2 Conceptual Framework

A district with a high level of exposure to female deities via goddess temples, may offer a chance for individuals in that district to engage more often and deeply with their level of regard for women. In turn, status of women in these districts – including incidence of intimate partner violence – may be improved relative to that of women in low-exposure

[9] Kulke and Rothermund (2004); Maps 4-8. These dynasties mainly gained power over the other two through being the main "uniting force" of tribes at various times rather than fighting wars. Other than these territorial disputes, the kingdoms coexisted for much of history.

[10] Rao (1961) p. 87.

districts. In this case, initiatives like the Abused Goddesses Campaign may successfully leverage religion to ameliorate IPV outcomes for women.

However, it may be possible that high exposure to female deities can engender a worse culture for the status of women. Research has documented that religion, in general, can act as an obstacle to seeking help for abuse (Beaulaurier et al. (2007)), and that certain religious elements may be used to oppress women (Levitt and Ware (2006)). If female deities are emphasized as symbols of sacrifice (Jayasundara et al. (2017)), women in high-exposure districts may be held to a high standard of self-sacrifice by society and themselves, making avenues of redress scarce and potentially increasing the incidence of intimate partner violence.

In the next section, I discuss the data used in this paper.

3.3 Data

3.3.1 Temples Data

I have hand-collected data on historical Hindu temples.[11] Overall, I obtain a list of 558 temples. I then categorize the temples as male or female-deity based on the main deity shrine in that temple. In Figure 3.2, I present a map of districts with the number of total temples in each district, and in Figure 3.3, I present a similar map with the number of female deity temples in each district.

I construct the exposure or treatment variable by dividing female deity temples by the number of total temples in each district. In Table 3.1, I report the exposure measure for each district. As documented in the table, there is a lot of variance in the exposure variable.

[11] I used the Google Maps API to find temples that are also "tourist attractions" in order to identify temples that were built by kings from the three kingdoms.

Figure 3.2: Tamil Nadu Map of Districts: Historic Temples Per District

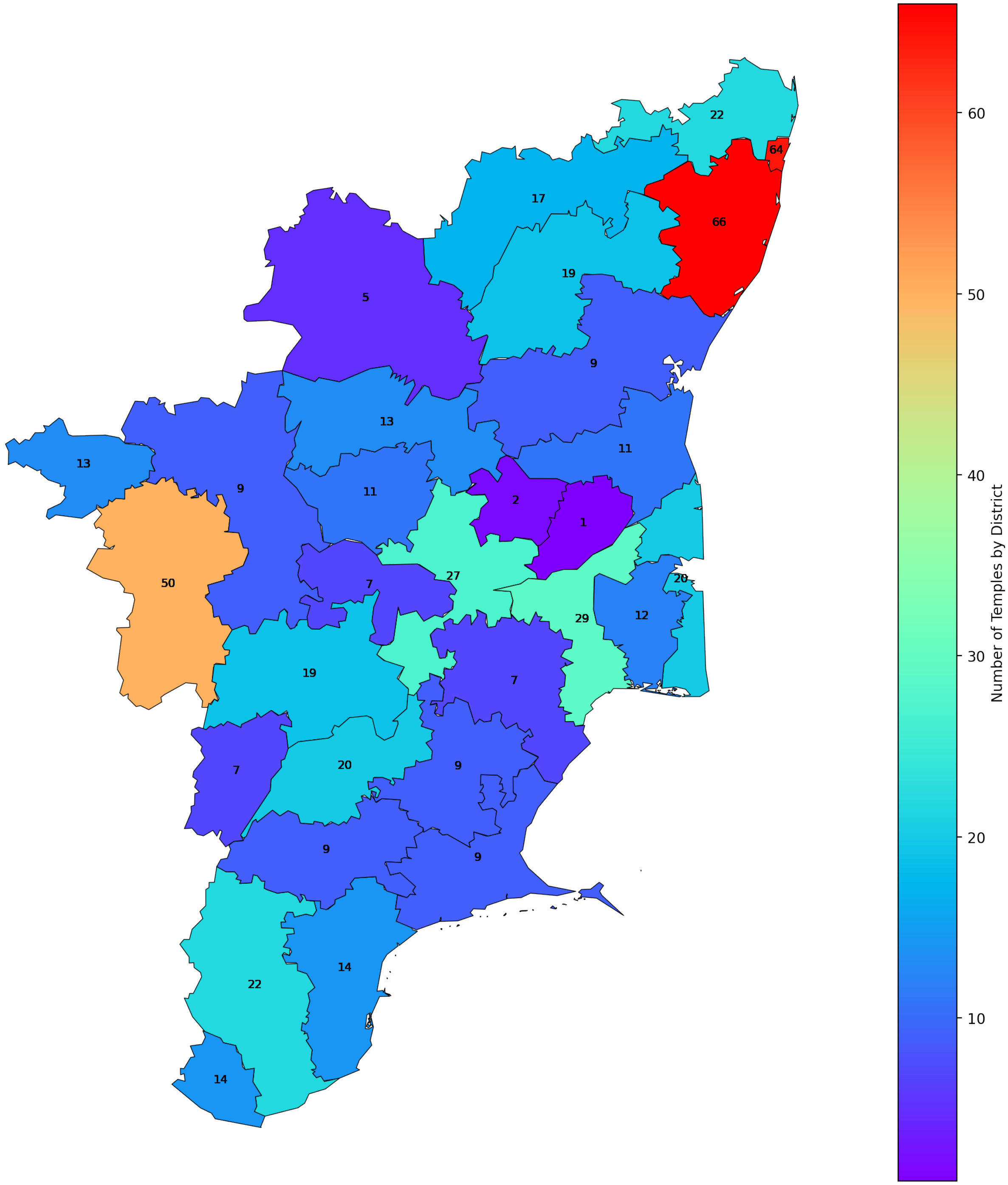


Figure 3.3: Tamil Nadu Map of Districts: Historic Female Deity Temples Per District

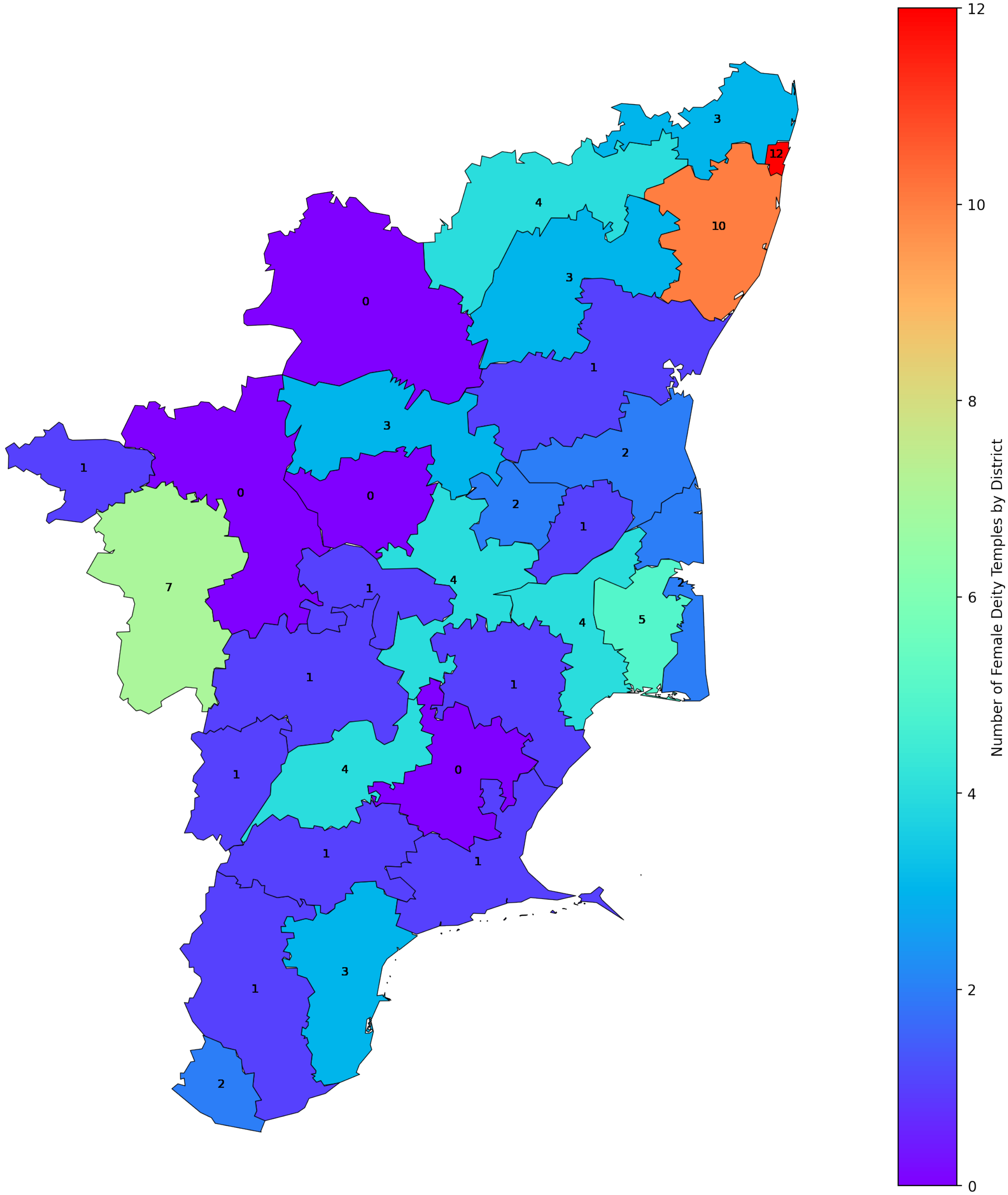


Table 3.1: Share of Temples Devoted to Female Deities by District in Tamil Nadu

Quartile of Share Female Deities	**District**	**Share Female Deities (Treatment Var)**
1	Dharmapuri	0.000
	Erode	0.000
	Krishnagiri	0.000
	Namakkal	0.000
	Sivaganga	0.000
	Dindigul	0.045
	Tirunelveli	0.053
	Tiruppur	0.067
2	The Nilgiris	0.077
	Nagapattinam	0.100
	Ramanathapuram	0.111
	Viluppuram	0.111
	Virudhunagar	0.111
	Thiruvallur	0.136
	Thanjavur	0.138
	Coimbatore	0.140
3	Kanniyakumari	0.143
	Karur	0.143
	Pudukkottai	0.143
	Theni	0.143
	Tiruchirappalli	0.148
	Kancheepuram	0.152
	Tiruvannamalai	0.158
	Cuddalore	0.182
4	Chennai	0.188
	Madurai	0.200
	Thoothukkudi	0.214
	Salem	0.231
	Vellore	0.235
	Thiruvarur	0.417
	Ariyalur	1.000
	Perambalur	1.000

Source: Hand-collected data from Google Maps API
Notes: Using hand-collected data, I obtained the number of total historical temple complexes in each district.
I then identified the temples that are devoted to female deities.
Share Female Deities (Treatment Variable), Female Deity Exposure, is the share of temples in each district that is devoted to female deities; this is presented in column 3 of the above table.
The quartiles in column 1 are based on the Treatment Variable, given in column 3.

3.3.2 National Family and Health Surveys and IPV Data

To measure the attitudes toward intimate-partner violence (IPV), I will use individual-level data from the Demographic and Health Surveys (DHS), a repository of demographic and health-related surveys from around the world. The DHS maintains the full collection of National Family Health Surveys (NFHS) of India, conducted by the International Institute for Population Sciences.[12] The NFHS was initiated in the early 1990s and has been conducted exactly four times in irregularly spaced waves. I use the fourth of these four

[12] ICF (2017)

cross-sectional surveys in my analysis, from 2015-16 (NFHS-4), the one wave of survey data that includes questions for women about their personal experience with IPV and their general attitudes towards IPV, which I henceforth refer to as the "domestic violence section." NFHS-4 importantly tabulates the domestic violence section at the district level, which is important for my analysis.

The NFHS is conducted in person by sending field workers to interview selected households at their homes. Interviews are conducted only if a member of the household answers the door. If a household is part of the sample, one eligible woman in the household aged 15-49 is asked the questions in the domestic violence section of the Women's Questionnaire.[13] The respondents in the Women's Questionnaire gives a reasonably representative sample of women in Tamil Nadu in the 2015-16 wave of the NFHS (sample size of 3,550 for ever-married women).

The domestic violence section's questions are focused on spousal violence and aim to capture the extent of physical, sexual, and emotional violence inflicted upon an ever-married woman by her current husband or previous husband(s). These questions are listed in 3.2. If a respondent answers "yes" to any of the questions about physical or sexual violence, they are counted as a victim of IPV in my analysis. In Table 3.2, I partition the districts into quartiles by exposure measure and report the share of individuals who answered "yes" to any question.

3.3.3 Covariates

The NFHS Section on Domestic Violence includes a wealth of information on women and their husbands including age, educational attainment, employment status, and religion. In addition, the NFHS includes a "wealth index." I use information from the 2011 Census of India on district-level population and share of female population.

[13] Paswan et al. (ndia). Only one woman per household is surveyed for the domestic violence section. This is in accordance with the World Health Organization's guidelines on ethics regarding collection of information on domestic violence (WHO 2013).

Table 3.2: Outcome Variable: Intimate Partner Violence Incidence

Quartile of Share Female Deity	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	IPV (outcome)
1	0.319	0.712	0.220	0.233	0.050	0.029	0.090	0.064	0.296	0.068	0.388
2	0.343	0.769	0.198	0.231	0.028	0.016	0.122	0.047	0.343	0.058	0.437
3	0.314	0.714	0.210	0.283	0.027	0.018	0.124	0.053	0.285	0.056	0.420
4	0.354	0.768	0.248	0.289	0.034	0.019	0.157	0.073	0.341	0.081	0.438
Total	0.335	0.740	0.219	0.258	0.035	0.021	0.122	0.059	0.315	0.066	0.420

Source: International Institute for Population Sciences (IIPS) and ICF. 2017. National Family Health Survey (NFHS-4), 2015-16: India. Mumbai: IIPS.
Notes: Quartile of Share Female Deity is obtained by arranging each District in ascending order by Exposure/Treatment Variable: Share Female Deity, then partitioning them by quartile based on Share Female Deity (see Table 1). The questions used to determine whether a respondent (woman) has been subjected to IPV are the ten questions in the NFHS Domestic Violence Section that pertain to Physical Violence and Sexual Violence. Each respondent is asked whether their current or former husband does the following:
(Question 1) pushes you, shakes you, or throws something at you (Physical Violence)
(Question 2) slaps you (Physical Violence)
(Question 3) twists your arm or pulls your hair (Physical Violence)
(Question 4) punches you with his fist or with something that could hurt you (Physical Violence)
(Question 5) kicks you, drags you, or beats you up (Physical Violence)
(Question 6) tries to choke you or burn you on purpose (Physical Violence)
(Question 7) threatens or attacks you with a knife, gun, or any other weapon (Physical Violence)
(Question 8) physically forces you to have sexual intercourse with him even when you did not want to (Sexual Violence)
(Question 9) physically forces you to perform any other sexual acts you did not want to (Sexual Violence)
(Question 10) forces you with threats or in any other way to perform sexual acts you did not want to (Sexual Violence)
The outcome variable in my analysis, IPV, indicates whether an individual has responded "yes" to any of the above questions.

3.4 Methodology

I hypothesize that there is a relationship between exposure to female deity (temples) and incidence of intimate partner violence. I make use of a basic conditional on observables effects methodology, modeling the outcome IPV_{id} (intimate partner violence) for person i in district d, as follows:

$$IPV_{id} = \beta_0 + \beta_1 FemDeity_d + \beta_2 X_i + \beta_3 W_d + \epsilon_{id}$$

IPV_{id} is defined to indicate whether an individual has been subjected to intimate partner violence. $FemDeity_d$ is the treatment variable, which measures each district's level of exposure to female deities on a continuous scale from 0 to 1. To control for individual-level characteristics, specifically age and wealth index, I include a vector of individual controls represented by X_i. To control for district-level characteristics, district population and proportion of women, I include a vector of these controls represented by W_d. Finally, I use ϵ_{id} to represent the error term.

Included in the vector X_i are age dummy variables and a set of five categorical controls for the wealth index, representing the categories of poorest, poorer, middle, richer, and richest. The district-level controls, given in the vector W_d are two continuous controls for population and proportion of females in each district.

β_1 is the main coefficient of interest, representing the effect of an increase in female deity exposure on intimate partner violence. Given the anticipated ambiguity of the effect, as previously discussed in Section 2.2, I empirically test the relationship between female deity exposure and intimate partner violence. I discuss estimates of β_1 in the next section.

3.5 Results

3.5.1 Main Results: Female Deity Exposure and Intimate Partner Violence

In Table 3.3, I present descriptive statistics by Quartile of the exposure variable, Share of Female Deity Temples, which help to inform the interpretation of my main results. The districts in each Quartile are, on average, very similar across the various characteristics in the table, suggesting they should serve as good comparisons for one another in my analysis.

My main results are summarized in Table 3.4. The baseline specification regresses intimate partner violence on the share of female deities in each district. The estimate of β_1,

Table 3.3: Balance Table

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Individual Level Variables				
Age	33.969	33.660	34.165	34.215
Wealth Index = Poorest	0.165	0.204	0.192	0.226
Wealth Index = Poorer	0.243	0.218	0.198	0.205
Wealth Index = Middle	0.230	0.216	0.193	0.181
Wealth Index = Richer	0.229	0.200	0.240	0.196
Wealth Index = Richest	0.132	0.162	0.177	0.193
Employed	0.328	0.301	0.328	0.313
Religion: Hindu	0.924	0.901	0.866	0.923
Religion: Muslim	0.035	0.054	0.038	0.039
Religion: Christian	0.041	0.044	0.096	0.037
Religion: Other	0.000	0.001	0.000	0.000
Education: No Education	0.193	0.168	0.182	0.195
Education: Incomplete Primary	0.054	0.042	0.039	0.048
Education: Complete Primary	0.101	0.085	0.082	0.097
Education: Incomplete Secondary	0.417	0.450	0.430	0.425
Education: Complete Secondary	0.106	0.104	0.123	0.110
Education: Higher	0.130	0.152	0.145	0.126
Husband's Education: No Education	0.168	0.142	0.136	0.175
Husband's Education: Incomplete Primary	0.068	0.048	0.045	0.041
Husband's Education: Complete Primary	0.099	0.094	0.077	0.092
Husband's Education: Incomplete Secondary	0.456	0.483	0.501	0.471
Husband's Education: Complete Secondary	0.078	0.082	0.074	0.083
Husband's Education: Higher	0.132	0.152	0.166	0.139
District Level Variables				
Population	1,268,887	1,373,340	1,211,011	1,312,637
Share Female Population	0.498	0.502	0.502	0.500

Source: International Institute for Population Sciences (IIPS) and ICF. 2017. National Family Health Survey (NFHS-4), 2015-16: India. Mumbai: IIPS.

0.0942, suggests that a 1 percentage point increase in the percent of female deity temples is associated with a 0.000942 percentage point increase in the likelihood of IPV incidence. This is a statistically significant result, with 95% confidence interval of (0.03600,0.1524), allowing us to rule out the probability that the true association is zero with 95% certainty. Adding age dummy variables attenuates the result slightly, but adding Wealth Index controls and district level controls (population and share of female population) diminish the point estimate's value while increasing the standard errors.

Table 3.4: The Relationship between Female Deity Exposure and Intimate Partner Violence

	(1) IPV	(2) IPV	(3) IPV	(4) IPV
Share Female Deities	0.0942**	0.0914**	0.0643*	0.0462
	(0.0297)	(0.0307)	(0.0251)	(0.0339)
Observations	3492	3492	3492	3408
Age Controls	no	yes	yes	yes
Wealth Index Controls	no	no	yes	yes
District Level Controls	no	no	no	yes

Standard errors in parentheses. All Standard errors are clustered at the district level.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

With the full set of baseline controls, the effect of interest is 0.0462, representing that a 1 percentage point increase in the percent of female deity temples is associated with a 0.000462 percentage point increase in the likelihood of IPV incidence. The 95 percent confidence interval of (-0.0202,0.1126) does not rule out the possibility that the true estimate might be zero.

The specification in Column (4), with the full set of baseline controls, is my preferred specification.

Thus far, these results suggest that districts with greater exposure to female deities may have higher incidence of IPV. This is consistent with the theory that Hindu goddesses may be associated with interpreting female deities as figures of self-sacrifice and rendering women unlikely to seek support for abuse. If women are less likely to seek support for abuse, husbands face lower risk of consequences for their abuse, and IPV becomes prevalent.

In the next section, I explore controls that may be potential mechanisms for the main effect.

Table 3.5: The Relationship between Female Deity Exposure and Intimate Partner Violence

	(1) IPV	(2) IPV	(3) IPV	(4) IPV	(5) IPV	(6) IPV
Share Female Deities	0.0393	0.0418	0.0448	0.0403	0.0461	0.0399
	(0.0350)	(0.0343)	(0.0338)	(0.0342)	(0.0338)	(0.0341)
Observations	3408	3408	3408	3408	3408	3408
Education Controls	yes	yes	no	yes	no	yes
Husband's Education Controls	no	yes	no	yes	no	yes
Employment Controls	no	no	yes	yes	no	yes
Religion Controls	no	no	no	no	yes	yes

Standard errors in parentheses. All standard errors are clustered at the district level.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.5.2 Main Results: Education, Husband's Education, Employment, and Religion

Because temples influence IPV by influencing overall gender attitudes, it may be inappropriate to control for other characteristics also shaped by these attitudes: namely, the controls for education, employment, and observed religion. Adding these controls may capture the mechanism through which temples influence IPV. Despite this concern about overcontrolling, I present specifications which include controls for Education, Husband's Education, Employment, and Religion in Table 3.5. In all of the specifications, the estimate – both magnitude and statistical significance – is unchanged.

3.6 Conclusion

Intimate partner violence, one aspect of women's well-being, is multifaceted. The way individuals engage with religion, including symbolism, belief systems, and its interaction with society, can vary from person to person. Initiatives like the Abused Goddesses Campaign attempt to leverage religion to mitigate abuses toward women. However, if people do not engage with this imagery in the intended manner, such a campaign may be rendered ineffective or worse.

In this paper, I attempt to establish a descriptive relationship between female deity exposure and the incidence of intimate partner violence using a cross-sectional, conditional on observables analysis. I find, consistently across all specifications, that more exposure to female deities – through exogenous placement of historical temples – is associated with more intimate partner violence.

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