I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17688

The Labor Market Effects of Pregnancy Accommodation Laws

Emily Battaglia
Jessica H. Brown

FEBRUARY 2025

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17688

The Labor Market Effects of Pregnancy Accommodation Laws

Emily Battaglia
University of Delaware

Jessica H. Brown
University of South Carolina and IZA

FEBRUARY 2025

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Labor Market Effects of Pregnancy Accommodation Laws*

Pregnancy accommodation laws require "reasonable accommodations" for pregnant workers, i.e., sitting down, lifting restrictions, and additional bathroom breaks. Although these laws may make it easier for women to remain employed during pregnancy, as a mandated benefit, they may also discourage employers from hiring employees who may become pregnant. We estimate the effect of pregnancy accommodation laws on labor market outcomes for women of childbearing age in order to determine whether these laws lead employers to discriminate against young women in hiring. Using a triple differences design comparing women's and men's labor market outcomes throughout the staggered roll-out of thirteen pregnancy accommodation laws from 2013 to 2016, we find no overall impact on female employment and wages. Under some specifications, we find women are more likely to choose occupations where physical abilities are important, suggesting possible increased accessibility. For subgroups more likely to be impacted - those with less education, in more physically-intense occupations, and married without children - we do find modest declines in earnings and employment. That the burden falls on both suggests women value the benefit but at less than it costs to provide.

JEL Classification: K31, J32, I18
Keywords: pregnancy accommodations, mandated benefits

Corresponding author:
Jessica H. Brown
University of South Carolina
Columbia, SC 29208
USA
E-mail: Jessica.Brown@moore.sc.edu

* The authors thank Adriana Corredor-Waldron, Hani Mansour, Mayra Pineda-Torres, Jonathan Roth, Lucy Xiaolu Wang, Katie Yewell and participants at the 2022 Southern Economic Association Annual Conference and 2024 Association for Public Policy Analysis & Management Annual Conference for helpful comments.

1 Introduction

Until the passage of the Pregnant Workers Fairness Act in 2023, federal law offered no protection for pregnant workers requiring workplace accommodations. Employers could legally fire pregnant workers who were unable to work without accommodations. Beginning in 2013, a number of states began adopting "pregnancy accommodation laws" to provide protections for employees who require job modifications in order to remain employed during pregnancy.[1] These laws have potentially far-reaching impacts, as nearly 2.5 million employed women give birth in the United States each year. Female labor force participation in the United States increased from 43% in 1970 to 60% in 2000 (U.S. Bureau of Labor Statistics, 2023), and as of 2015, two-thirds of American women pregnant with their first child worked during pregnancy (Gao and Livingston, 2015).

But the impact of these laws on female employment may reach beyond women who become pregnant. Because employers know that they may have to provide accommodations for a woman if she becomes pregnant, they may be less likely to hire her or pay lower wages when they do. These indirect effects of pregnancy accommodation laws have the potential to affect millions of women of childbearing age. Understanding these potential impacts is particularly important as pregnancy accommodation laws expand across the country, covering more female workers, and as the federal pregnancy accommodation law, the Pregnant Workers Fairness Act, went into effect in 2023.

In this paper, we provide the first evidence of the impact of pregnancy accommodation laws on the employment of women of childbearing age broadly. To estimate the impact of pregnancy accommodation laws on labor market outcomes for women, we use data from the Quarterly Workforce Indicators (QWI) and the American Community Survey (ACS) for years 2001 to 2019 and a triple differences research design that compares changes over time in the difference in the labor market outcomes between women of child-bearing age and men of the same age throughout the staggered law adoptions. Using the triple differences strategy allows us to non-parametrically control for changes in state labor markets. Labor market outcomes include labor force participation, employment, wages, and job sector.

Because our identification is based on staggered law adoption, with different states adopting laws in different years, we employ the estimator from Callaway and Sant'Anna (2021), which provides unbiased estimates of treatment effects in the presence of staggered treatment timing and potentially heterogeneous treatment effects. This estimator only directly accommodates a difference-in-differences design. In order to implement the triple differences design, we aggregate outcomes to the state level and subtract the male outcome from the female outcome and use this difference as the outcome. We also show robustness to the standard two-way fixed effects model and to the estimator from Borusyak et al. (2024), another estimator that provides unbiased estimates in the presence of staggered timing using a different approach. This design allows us to use thirteen state law changes from 2013 to 2016 as experiments to estimate a causal effect. We drop observations from the states with pregnancy accommodation laws implemented before 2000 since pre-period data is not available. We drop states treated after 2016 because there is not enough post-period data to create a balanced panel. The treated states are spread throughout the country, representing all but one Census division, giving a national picture of the effect of pregnancy accommodation laws.

Estimating the impact of the pregnancy accommodation laws on the employment and wages of women of childbearing age allows us to detect whether these laws cause discrimination against women who may become

[1] California and Hawaii both passed pregnancy accommodation laws before the year 2000, but no other states passed laws until 2013. Louisiana has an older law that requires reasonable transfer if available but not reasonable accommodations within the current position.

pregnant. Since the laws increase the cost of employing this group, we would expect to see reductions in their employment and/or wages. If no changes are observed, that suggests the laws do not impose additional costs on employers. This could mean that the accommodations were not very costly or employers were already offering accommodations before the laws. If labor market outcomes are affected, the relative incidence on employment versus wages tells us whether workers value the new benefit. If they do value the benefit, then under standard mandated benefit theory from Summers (1989), they will be willing to accept lower wages in exchange. But if workers do not value this benefit, we would expect to see a reduction in employment instead.

We find little evidence of an overall decline in employment or wages for women of child-bearing age as a result of pregnancy accommodation laws. At the 95% level, we can rule out declines in the employment-to-population ratio (EPOP) of more than 0.2 percentage points and declines in hourly wages of more than 1.0%. We also do not see any differential changes in hiring for women of child-bearing age as compared to men. Across two of the three specifications, we do find some evidence that, on average, women are employed in more physically-demanding occupations, as measured by the importance of physical skills and abilities by occupation from the O*NET database.

There are a few potential explanations for our findings of an overall lack of effect on women's employment and earnings. First, there could be a lack of awareness of the benefit. Prior research shows that workers are not always aware of their workplace benefits (Brown, 2022). If they are not aware of the benefit, they may not request it or take it into account when making employment decisions. And if workers are not using the benefit, then there is no cost to employers, so there would be no change in employment or wages. However, other research has found that pregnancy accommodation laws *did* increase employment during pregnancy (Ros Pilarz et al., 2023). Since these laws did have an overall effect on employment during pregnancy, it is likely that at least some pregnant workers were receiving accommodations, and thus a lack of knowledge is not the full story. A second possibility is that the accommodations did not end up being costly to the employer. But if the accommodations were not costly, that raises the question of why employers were not accommodating pregnant women prior to the law. One explanation could be imperfect information about the cost of accommodations. This would be consistent with prior research finding that the New York paid family leave program was not as costly for employers as some had feared (Bartel et al., 2023). Another possible explanation is that in the absence of the mandate, employers were concerned that other, non-pregnant, employees would request accommodations as well, and accommodating that larger group would be costly. It could also be the case that the accommodations were costly, but they allowed employees to remain with the employer, consistent with prior research on paid family leave in California (Bana et al., 2020), and employers were able to save in hiring costs. Again, perhaps the employer did not previously have complete information about how accommodations would affect retention.

Finally, another explanation for the lack of an average effect on women of childbearing age could be that accommodations are concentrated among or only costly for a subset of women, and therefore there is not enough power to detect effects for women as a whole. We test this explanation by focusing on groups that are more likely to be impacted by the laws, either because they are more likely to be in occupations where accommodations are necessary or because employers may perceive them as more likely to become pregnant. First, we restrict the sample to respondents whose highest level of education is at most a high school diploma and re-estimate the models on only this group. Lower-education workers are more likely to hold physically-demanding jobs, such as cashier or warehouse worker. Here, we still find no effect on employment at the extensive margin, labor force participation, or hourly wage. However, we do estimate a 0.4-week decrease in the number of weeks worked last year and a 3.3% decline in annual earnings. Second, we consider effects for workers based on the importance of physical abilities to their occupation (as measured by O*NET).[2] For workers whose job is one standard deviation or more above the mean in terms of importance of physical abilities, pregnancy accommodation laws are associated with a 0.6-week reduction in the number of weeks worked per year. That the effects are on the employment margin (as opposed to wages) suggests workers in these occupations do not fully value this new benefit. Third, we consider effects by family structure. We hypothesize that employers will see married women, particularly those without children, as more likely to become pregnant. Reassuringly, we find no effects (and small point estimates) for single women without children. We do find significant labor market effects for married women without children, who experience a 1.2 percentage point decline in labor force participation and a 3.1% decline in hourly wages as a result of pregnancy accommodation laws. Since much of the incidence for this group falls on wages, that suggests that this group does value the pregnancy accommodation benefits, though not at their full cost. Thus, when focusing on demographic groups and occupations that are more likely to be affected by the pregnancy accommodation laws (either through more costly accommodations or a higher probability of needing accommodations), we do estimate modest effects on labor market outcomes consistent with a modestly costly mandated benefit.

Our paper contributes to a broader literature on the effect of mandated benefits on employment and wages, particularly when the mandated benefit affects only one identifiable subgroup of potential employees. Previous work has found that group-specific mandated benefits may affect wages and/or employment of the targeted group, such as women of childbearing age (and their husbands) in the case of mandated maternity benefits in health insurance (Gruber, 1994) and individuals with disabilities in the case of required reasonable accommodations through the Americans with Disabilities Act or similar state laws (DeLeire 2000; Acemoglu and Angrist 2001; Beegle and Stock 2003; Hotchkiss 2004). With these policies, whether an applicant is a member of a covered group is usually observable at the time of hire. However, in our setting, although employers know that members of the targeted group (pregnant workers) will be a subset of a broader group (women of child-bearing age), the employer does not know at the time of the job application which women will become pregnant while employed, and thus become covered. Therefore, our work is most closely related to other research studying the effect of mandated benefits specifically for pregnant workers and new mothers. Consistent with our findings, Waldfogel (1999) and Baum (2003) find that the Family and Medical Leave Act, which provides up to twelve weeks of job-protected leave for employees at covered employers and is commonly used for maternity leave, had no detectable effect on wages or employment for young women, despite an increase in leave-taking. But other work shows that more costly benefits do have an impact on employment and wages, as in Lai and Masters (2005), who find that a set of new maternity and pregnancy benefits, including reasonable accommodations during pregnancy as in our setting but also mandated paid leave post-birth, decreased employment and wages for young women.

We also contribute to a nascent literature estimating the effects of pregnancy accommodation laws. Using the American Community Survey and considering women who gave birth within the past year, Shinall (2020) finds that pregnancy accommodation laws increase the probability of employment and weeks worked per year in the time around the birth of a new baby. Using the Survey of Income and Program Participation, Ros Pilarz et al. (2023) are able to specifically study labor market outcomes during pregnancy and find that these laws increased the labor force participation, employment, and earnings of pregnant women by 9% to 11%. Consistent with our results, they find that the magnitude of the effect is larger among women

[2] The occupation considered is either their current occupation if they are employed or their most recent occupation if they are not.

whose highest level of education is at most a high school degree. The larger employment effect in this subset of pregnant workers suggests that lower-educated women are more likely to be employed in occupations that require workplace accommodations. This heterogeneity is relevant in our context because it implies that the expected cost to employers of providing pregnancy accommodations is higher among this lower-educated subset as well, which could translate into larger labor market effects from the mandated benefit. Also consistent with our results, Pac et al. (2023) find that the laws did not have an overall effect on birth outcomes; however, after the law, babies born to mothers whose highest level of education is a high school degree or less were less likely to be small for gestational age relative to babies born to mothers with higher levels of education. They also find that effects are larger in counties with more employment in physically-intense industries. Thus far, this literature has focused on the impact of pregnancy accommodation laws on women who become pregnant. That this literature shows an effect of these laws on outcomes for pregnant women suggests that accommodations are being provided for at least some women. Thus, employers may take these potential future accommodations into account when considering whether and at what wage to hire a woman of childbearing age. Our analysis therefore expands the question to consider whether there are labor market effects on *all* women of childbearing age, regardless of whether they are pregnant. Although the literature shows positive impacts on employment and earnings around pregnancy, our work shows that for some subgroups of women, there is an overall negative impact on labor market outcomes given that the pregnancy accommodation laws function as a mandated benefit.

More broadly, our work contributes to the literature on the gender wage gap and child penalty. While pregnancy accommodation laws have the potential to reduce mothers' child penalty (the decline in wages and employment associated with the birth of a child) by increasing employment continuity, we find that for lower-education women, the laws lead to a modest increase in the gender wage gap. As such, we contribute to a large literature documenting and exploring possible causes of the gender wage gap and child penalties, such as Kleven et al. (2019), Rosenbaum (2021), Kleven et al. (2021), Andresen and Nix (2022), Andresen and Nix (2023), Kleven et al. (2023) and Kleven (2023) on child penalties and Antonczyk et al. (2010), Fields and Wolff (1995), Kunze (2005), and Goldin (2014) on the gender wage gap.

The remainder of the paper proceeds as follows. Section 2 provides additional background on pregnancy accommodation laws and their theoretical effects on employment and wages. Section 3 describes the data used for our analysis, and Section 4 presents the empirical strategy. Section 5 presents the results, and Section 6 provides a discussion of the results and concludes.

2 Pregnancy Accommodation Laws

This section provides background on pregnancy accommodation laws in order to better understand the setting for this paper. We discuss protections for pregnant workers in the absence of pregnancy accommodation laws, the additional requirements imposed by pregnancy accommodation laws, and a timeline of implementation of these laws by state. We then discuss the mechanisms through which pregnancy accommodation laws may affect the employment of women. Because these laws impose a mandated benefit for pregnant workers, they could affect employment of workers who may become pregnant even if they are not currently pregnant.

2.1 Background on Pregnancy Accommodation Laws

In the absence of specific pregnancy accommodation laws, pregnant workers are protected by the Pregnancy Discrimination Act of 1978 (PDA) and potentially the Americans with Disabilities Act of 1990 (ADA), depending on their circumstances. Both are somewhat limited in scope. The ADA requires employers to provide "reasonable accommodations" for workers with a disability. Pregnant women qualify for protections under the ADA only if pregnancy causes a disability, defined as "a physical or mental impairment that substantially limits one or more major life activity." Pregnancy does not qualify as a disability in and of itself, so being pregnant would not be enough for a woman to request lifting restrictions under the ADA, for example.

The PDA was an amendment to the Civil Rights Act of 1964 clarifying that discrimination based on sex includes discrimination due to pregnancy. It requires employers to treat their pregnant employees the same as "other persons not so affected but similar in their ability or inability to work." Thus, it only requires a reasonable accommodation if the employer is accommodating other employees. The law is ambiguous about whether accommodation is required if *any* other employees are accommodated or only if *all* other employees are accommodated. This distinction was the subject of the Young v. United Parcel Service (2015) Supreme Court case. In a 6-3 ruling, the court found that if any other employees are accommodated, then pregnant workers must also be accommodated if otherwise the employer's policy imposes a "significant burden" on pregnant workers and the employer's reason for imposing that burden is not "sufficiently strong." Importantly, the case law was ambiguous until the 2015 ruling by the Supreme Court. In fact, lower courts sided with UPS, and it was only once the case reached the Supreme Court that a court ruled in favor of the complainant, Peggy Young. And even after this case, employers have won court cases in which they did not accommodate pregnant employees even while accommodating other workers by arguing that they have a "sufficiently strong" reason to do so. For example, in Equal Employment Opportunity Commission v. Wal-Mart Stores East LP (2022), the circuit court ruled unanimously in favor of Walmart, stating that they had legitimate reasons for accommodating some workers (those injured on the job who otherwise would have filed for workers' compensation) but not pregnant workers. Thus, even after the *Young v. UPS* case, federal law and case law have provided far from universal access to pregnancy accommodations.

Pregnancy accommodation laws providing additional protections to pregnant workers beyond those required by the PDA and ADA were rare prior to 2013, but state- and local-level laws became increasingly common beginning in 2013. Prior to 2013, just two states (Hawaii and California) had laws requiring employers to make reasonable accommodations for pregnant employees, a requirement that goes beyond that required by the federal PDA.[3] But since then, from 2013 to 2019, twenty-one states and several local jurisdictions passed such laws. A list of states with the year of law passage is available in Table 1, and a map of treated states can be found in Figure 1.[4] The timing of the law implementation comes from the A Better Balance (2023) database, which we confirmed with a search of state statutes. In Maryland, lawmakers became aware of the lack of protections when the case brought by Peggy Young, who was from Maryland, made its way through the lower courts there (McDaniels, 2013). The nearby states of Delaware, New Jersey, and West Virginia were also early adopters. In addition to the state laws, most recently, the *federal* Pregnant Workers Fairness Act (PWFA) was signed into law in December 2022 and went into effect in June 2023, providing protections for all pregnant workers across the country at firms with 15 or more employees. Our work provides important insight into the potential effects of this federal law.

Most pregnancy accommodation laws require employers to make "reasonable accommodations" for all

[3]Louisiana also has an older law on the books that requires employers to transfer pregnant employees to an appropriate position if one is available, but it does not require reasonable accommodations within the current position.

[4]In addition to the states listed in the table, Alaska and North Carolina passed reasonable accommodation laws that apply only to state agencies. Since these make up only a small subset of employers, we consider these states as untreated.

pregnant employees, similar to that required for individuals with disabilities by the Americans with Disabilities Act of 1990 (ADA) but without the requirement that the pregnancy cause a disability for the law to apply. Laws are similar in what types of accommodations are required. The 2014 New Jersey law provides a typical list of potential reasonable accommodations: "bathroom breaks, breaks for increased water intake, periodic rest, assistance with manual labor, job restructuring or modified work schedules, and temporary transfers to less strenuous or hazardous work" (N.J.S.A. 10:5s). Many white-collar workers would likely have access to these types of accommodations (or would not have need of them) even in the absence of pregnancy accommodation laws. Therefore, these laws are most likely to affect workers in other sectors like restaurants and retail. Laws vary in whether they provide exceptions if the accommodations would entail an "undue burden" on the employer and whether employers can require a doctor's note. In addition, they have different thresholds for the number of employees an employer can have before the laws are binding. Six states align with the ADA's threshold of 15 employees, while the remaining thirteen states have much lower thresholds, including seven states with laws that apply to all employers irrespective of size.

2.2 Theoretical Impact of Pregnancy Accommodation Laws on Female Employment

There are two separate channels through which pregnancy accommodation laws may affect female employment: one is directly by changing employment probability and/or industry of employment *during* pregnancy (with effects that may continue after pregnancy) and the other is indirectly by changing employment probability and/or industry of employment for women who are *not* currently pregnant but may become pregnant in the future. Assuming that employers comply with pregnancy accommodation laws, we would expect that the direct effect of the laws would be to increase the probability of employment during pregnancy and increase the probability of being employed in an industry that requires more strenuous activity. Workers who may otherwise have had to leave their job due to pregnancy may be able to remain employed with reasonable accommodations. Similarly, women who may have had to switch to a less strenuous line of work during pregnancy may be able to remain in their current position with accommodations such as lifting restrictions or the ability to sit down while working. Prior research confirms that the direct effect is operating: employment during pregnancy increases as a result of pregnancy accommodation laws (Ros Pilarz et al., 2023). In our context, this is important because it confirms that (at least some) employers are accommodating employees.

The indirect effect of pregnancy accommodation laws on female workers who are not pregnant may operate through a combination of a change in preferences of employers and potential employees. In the absence of pregnancy accommodation laws, forward-looking women who know they may become pregnant may sort into jobs that are better-suited for pregnancy. In the presence of pregnancy accommodation laws, they may be more willing to take more strenuous jobs knowing that they will be able to receive accommodations should they become pregnant. On the employer side, pregnancy accommodation laws are a mandated benefit that increases the cost of employing pregnant women, including any direct costs of the reasonable accommodations and the increased potential for lawsuits. Employers may therefore be less likely to hire women of childbearing age, who may become pregnant, and/or offer them lower wages when they do hire them in order to compensate for the potential for increased costs. Changes in employment due to these indirect effects are particularly important to understand as they have the potential to affect a large swath of the population - all women of childbearing age. These indirect effects are thus the focus of this paper, though the direct and indirect effects are not always possible to disentangle.[5]

Mandated benefits can affect employment and/or wages. Reservation wages for women of childbearing age would be expected to decline by an amount equal to their valuation of the benefit, assuming they are aware of it, as it is a form of nonmonetary compensation. If they value the benefit at less than it costs the employer to provide (or are unaware of the benefit), we would expect to see a decrease in employment of individuals in the potentially-targeted group, women of childbearing age (Summers 1989; Gruber 1994; Brown 2022). Prior literature has found mixed effects of mandated benefits. For example, like pregnancy accommodation laws, the ADA requires "reasonable accommodations" for a class of workers - in this case, disabled workers. Previous work shows the ADA may have reduced the employment of disabled workers (Acemoglu and Angrist, 2001), though more recent work estimates no employment effect (Hotchkiss, 2004). But using state-level implementation of disability employment laws that pre-dated the ADA, Beegle and Stock (2003) find that state-level disability discrimination laws were associated with lower earnings for disabled workers but not lower employment, which in the mandated benefits framework suggests that disabled workers valued these laws. Prior work also shows an effect on labor market outcomes from a different group-specific mandated benefit also targeting women of childbearing age: requiring health insurance to cover pregnancy and childbirth. Gruber (1994) finds that such mandates lower wages but not employment for married women of childbearing age, lower both wages and employment for unmarried women of childbearing age, and lower employment but not wages for married men of the same age (whose wives may be covered by the employer insurance). So the incidence of the mandated benefit required by the pregnancy accommodation laws depends on how much women who may become pregnant value the benefit it provides. The more they value it, the more of an effect would be seen in wages relative to employment.

3 Data

We obtain data on employment and wages from the Quarterly Workforce Indicators (QWI) and American Community Survey (ACS) for years 2001 to 2019. We supplement this data with information on the physical strenuousness of different occupations from O*NET. Below, we describe each data set in more detail.

3.1 Employment and Earnings Outcomes

The QWI provides state-level quarterly data on employment stocks and flows as well as average monthly earnings based on data reported to state Labor Market Information entities for the purposes of Unemployment Insurance eligibility. The data is merged with demographic information from the Decennial Census, Social Security Administration records, and individual tax returns.[6] The data allow for heterogeneity based on sex-by-age cells, which is useful for separating out employment and earnings for women of childbearing age to compare to men of the same age. Different states join the QWI at different times, with 80% of states available by 2000 and nearly all states available by 2005. The main advantage of the QWI is that it provides data on the near-universe of employment for participating states. The primary outcomes of interest from the QWI are measures of employment and employment transitions: (the log of) number of employees, number of new hires, and number of separations in sex-by-age bins at the state-quarter level.

We use ACS data from 2001 to 2019 in order to estimate the effect of pregnancy accommodation laws

[5] For an analysis of the effect of pregnancy accommodation laws on employment during pregnancy and early motherhood, see Ros Pilarz et al. (2023) and Shinall (2020).

[6] When demographic data is not available from one of these sources, it is imputed.

on additional employment outcomes and more detailed heterogeneity as well as to confirm the results from the analysis with the QWI. The ACS provides individual-level survey data on a 1% random sample of the population annually and includes questions on demographics and labor market status (Ruggles et al., 2022). While the QWI allows us to look at employment and monthly earnings, the ACS also allows for analysis of labor force participation and hourly wages. Furthermore, the ACS has data on occupation, and since it is individual-level data, we can also consider sex-by-age-by-education cells as well as family type in order to focus the analysis on individuals most likely to be impacted—women who are more likely to need accommodations, either because they are more likely to become pregnant or because they are more likely to be employed in occupations where accommodations are necessary.

Descriptive statistics for our ACS sample are available in Table 2. All statistics are calculated using person weights to reflect the overall population in the targeted age range. We restrict to individuals ages 18 to 44 years old in order to keep only respondents who are of childbearing age, for whom pregnancy accommodation laws lead to an increase in employers' expected cost of employment. We keep both female and male respondents in our sample in order to use the male respondents as an untreated control group of the same age within the state. The sample consists of 3.7 million respondents in treated states and 8.0 million respondents in untreated states, of whom approximately half are female. In both treated and untreated states, approximately 80% of the sample is in the labor force, and for both, the fraction in the labor force is 10 percentage points higher for men than for women (85% for men v. 75% for women in treated and 84% v. 74% in untreated). In treated states, the average hourly wage is $21.30 compared to $17.57 in untreated states. The gender wage gap is similar across the two groups - 35 log points in treated states and 37 in untreated states. Treated and untreated states look similar in terms of their racial composition, with treated states having slightly higher Asian populations. Respondents in untreated states have lower educational attainment on average, with 43% reporting at most a high school degree in untreated states as compared to 37% in treated states. Again, the gender gap (this time in the other direction) is similar, with men being eight to nine percentage points more likely to report at most a high school education. Note that while it is useful to understand the composition of the treated and untreated groups, identification does not require that the two groups look identical. The identifying assumption in this case is that labor market outcomes would have trended similarly across groups in the absence of the law changes. Since we focus on the triple differences specification with the difference between men's and women's outcomes as the third difference, it is reassuring, though not necessary, that this difference is similar across treated and untreated groups.

3.2 Occupation Characteristics

We turn to O*NET for information about the physical requirements of different occupations. O*NET is an online database that provides information on the skills, knowledge, and abilities required in different occupations (National Center for O*NET Development, 2022). We expect that pregnancy accommodation laws would be most relevant for jobs that are more physically demanding, so we focus on the extent to which each occupation requires stamina and physical strength. The physical strength category is measured across four types: dynamic, explosive, static, and trunk strength.[7] Each skill is given a score on a scale from 0 to 100 based on the importance of that skill for the occupation and the required intensity level of that skill for the job. For example, in the stamina category, a firefighter has a 66 for importance and a 55 for level, while an

[7] Dynamic strength measures the ability to repeatedly use muscle force; explosive strength measures shorter bursts using for running, jumping,and throwing; static strength measures the use of muscles to lift, push, pull, or carry large objects; and trunk strength measures the ability to use back and abdominal muscles to support the body.

economist has a 0 for importance and is coded as "Not relevant" for level. The O*NET database uses more detailed occupation codes than the ACS, so we collapse the O*NET data to the six-digit occupation level by taking the average scores for occupations in that category. Not all occupations in the ACS are described in the O*NET database. For these occupations, we match at the five-digit level when possible, followed by four, and so on, following the same aggregation method.

The primary outcome of interest that we construct from this dataset is a standardized measure of the importance of physical abilities to the job. We construct this measure as the standardized sum of the importance of stamina and the four physical strength categories:

$$Importance_j = \frac{\sum_i Importance_{ij} - \overline{\sum_i Importance_{ik}}}{\hat{\sigma}_{\sum_i Importance_{ik}}}$$

where $Importance_{ij}$ is the importance of skill i to occupation j. The measure is standardized to have a mean of zero and standard deviation of one across occupations in the O*NET database. This variable will therefore be measured in standard deviation units.

Table A1 in the Appendix provides a list of example occupations with their importance index values. The highest value is 4.20 standard deviations above the mean for athletes, and the lowest value is 1.12 standard deviations below the mean for accountants and auditors (economists are not far behind at 1.11 standard deviations below the mean). Food service managers are nearly exactly at the mean, registered nurses are 0.25 standard deviations above, while child care workers are 0.52 standard deviations above.

4 Model

Our empirical strategy follows a difference-in-differences design to estimate the impact of pregnancy accommodation laws on labor market outcomes. This design compares outcomes between individuals in states with pregnancy accommodation laws and without pregnancy accommodation laws before and after the laws went into effect, leveraging the staggered rollout of pregnancy accommodation laws across time. Our main specification is as follows:

$$Y_{st} = \beta Treat_{st} + \gamma_s + \gamma_t + \varepsilon_{st} \tag{1}$$

where s indexes states and t indexes year. In Equation 1, Y_{st} denotes the outcome variable of interest. In our primary specification, Y_{st} is actually the average outcome for women in state s minus the average outcome for men. This specification is analogous to a triple differences specification, where the difference in outcomes between women and men is the third difference and allows us to non-parametrically control for changes in state labor markets. Outcomes of interest include employment in any occupation, employment in physically-strenuous occupations, earnings, and weeks worked per year. The variable $Treat_{st}$ is an indicator variable that equals one if state s has passed a pregnancy accommodation law in or before year t. State fixed effects, γ_s, control for time-invariant differences in labor market outcomes across states. Time fixed effects, γ_t, control for any national changes in labor market outcomes. The error term is represented by ε_{st}.

Our coefficient of interest, β, represents our estimate of the effect of pregnancy accommodation laws on labor market outcomes. Under the identifying assumption that in the absence of the pregnancy accommodation laws, labor market outcomes (or the difference in labor market outcomes between women and men) in treated and untreated states would have trended similarly, β can be interpreted as the causal effect of pregnancy accommodation laws. We assess the validity of this assumption by estimating a dynamic version of Equation 1, which allows us to observe potential deviations in trends prior to the implementation of pregnancy accommodation laws. This regression takes the form:

$$Y_{st} = \sum_{\tau} \beta_{\tau}(Treat_s \times \alpha_{\tau}) + \gamma_s + \gamma_t + \varepsilon_{st} \quad (2)$$

where $Treat_s$ is an indicator for whether state s passes a pregnancy accommodation law and α_{τ} is an indicator for time relative to when a state passes a pregnancy accommodation law. As in Equation 1, Y_{st} is the outcome for state s in year t, γ_s and γ_t represent state and year fixed effects, and ε_{st} is the error term. The coefficients β_{τ} allow us to assess (1) whether treatment and control states were trending similarly prior to the passage of a pregnancy accommodation law and (2) whether the effect of pregnancy accommodation laws changes over time.

Following the recent literature on potential bias when using two-way fixed effects designs with staggered treatment, we estimate Equations 1 and 2 following Callaway and Sant'Anna (2021). In particular, we utilize both the not-yet treated and the never treated states as controls. We present dynamic results in figures for six years prior to and three years following pregnancy accommodation law passage. Note that with this estimator, post-event estimates compare the difference between the treatment and control groups in the current period to the period before treatment, while the pre-event estimates compare the current period to the previous period.[8] Estimates in all tables summarize the post-period ATT calculated following Callaway and Sant'Anna (2021). We additionally weight each regression by the working-age population in that state to help with precision. We check for robustness to the standard two-way fixed effects estimator and to the estimator from Borusyak et al. (2024), which is also robust to staggered treatment design with heterogeneous treatment effects but uses an imputation method to estimate the counterfactual instead of using the outcome at $\tau = -1$.

We first assess outcomes Y_{st} separately for males and females. We then consider the difference between male and female outcomes. In particular, we consider $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} and Y^m_{st} represent the average female and male outcomes in state s and year t. This specification is analogous to a triple difference estimator where we now consider differences between states that pass and do not pass a pregnancy accommodation law (first difference) across time (second difference), using males as an additional control group (third difference). The triple difference estimator is our baseline specification which we then consider for all heterogeneity analyses.

5 Results

Table 3 presents the main difference-in-differences and triple differences regression results using Equation 1 and the Callaway and Sant'Anna (2021) method to estimate the impact of pregnancy accommodation laws on labor market outcomes with the ACS data. Panel A considers the difference-in-differences results where the outcomes of interest are the average male outcomes in state s and year t (i.e., Y^m_{st}), Panel B considers the average female outcomes in state s and year t (i.e., Y^f_{st}), and Panel C considers the difference between the female and male outcomes in state s and year t (i.e., $Y_{st} = Y^f_{st} - Y^m_{st}$). As discussed in Section 4, Panel C is akin to a triple differences specification. Note that all coefficients in the table, with the exception of number of weeks worked per year, have been multiplied by 100 to improve readability.

[8] Therefore, unlike traditional two-way fixed effects event studies, this method generates an estimated coefficient and standard error at t=-1.

The point estimate in Column 1 suggests a 0.097 percentage point increase in unemployment after pregnancy accommodation laws for males and a 0.050 percentage point increase for females. The triple difference specification in Column 3 suggests a 0.043 percentage point decrease in unemployment for females relative to males after the passage of pregnancy accommodation laws. These point estimates are all small in magnitude relative to the baseline mean and statistically insignificant. Across all outcomes in the remaining columns (employment-to-population ratio in Column (2), labor force participation in Column (3), weeks worked in Column (4), log of hourly wage in Column (5), and log of earnings in Column (6)), we find that the effect of pregnancy accommodation laws on the difference between female and male outcomes is generally small in magnitude and consistently statistically insignificant. For example, for women of childbearing age, at the 95% confidence level, we can rule out declines in the employment-to-population ratio of more than 0.2 percentage points (Panel C, column (2)) and hourly wage declines of more than 1.0% relative to men of the same age (Panel C, column (5)). The results suggest that overall employers display minimal discrimination, if any, against women who may become pregnant.

Table 4 tests the robustness of the main triple differences ACS results to two other estimators: standard two-way fixed effects and the imputation estimator Borusyak et al. (2024). Panel A repeats the main Callaway and Sant'Anna (2021) results for ease of comparison. The two-way fixed effects estimates (Panel B) and Borusyak et al. (2024) estimates (Panel C) closely align with the main results, with generally small point estimates that lack statistical significance. The one exception is for the weeks worked outcome, where both alternative estimates suggest that pregnancy accommodation laws led to a 0.3-week reduction in weeks worked per year for women of childbearing age relative to men.

Figure 2 plots the corresponding coefficients from Equation 2 for the triple difference Callaway and Sant'Anna (2021) specification. The figure confirms the null result from Table 3. Overall, the point estimates are generally small in magnitude and largely statistically insignificant, suggesting no effect of pregnancy accommodation laws on labor market outcomes. The dynamic difference-in-differences estimates are available in Figure A1 for male outcomes and Figure A2 for female outcomes.

Table 5 presents the baseline difference-in-differences results using QWI data. Note that all coefficients have been multiplied by 100 to improve readability. Though the QWI data provides different outcomes and the data is collected differently, the results are consistent with the results seen in the ACS data. In particular, the point estimates on the triple difference estimates are small and statistically insignificant, except for Column (3), which shows a marginally significant increase in separations. At a 95% confidence level, we can rule out decreases in the number of employees of more than 0.3%, decreases in the number of new hires of more than 1.3%, and an increase in the number of separations by more than 3.4% for women of child-bearing age as a result of pregnancy accommodation laws. Table A2 in the Appendix tests the robustness of these results to the other two estimators. The Borusyak et al. (2024) estimate suggests a 0.7% *increase* in employment as a result of the laws. All other point estimates across the two other estimators are relatively small and not statistically significant.

Although we do not see significant overall changes in employment or wages of women of childbearing age, it still may be that their occupation choice is affected by pregnancy accommodation laws. The effect is theoretically ambiguous: pregnancy accommodation laws could allow women to choose or stay in more physically-intensive industries, but on the other hand, employers in these industries may be less willing to hire workers who may become pregnant and require accommodation. Therefore, we investigate whether pregnancy accommodation laws are associated with movements into (or out of) more manually intensive occupations. To do this, we estimate Equation 1 with measures of the index of the importance of physical abilities to the position (in standard deviation units) as the dependent variable.[9] Table 6 presents results where the dependent variable in Column 1 is the overall importance index, Column 2 is the share with an index above the mean, and Column 3 is the share with an index one standard deviation above the mean. Panel A presents results for the Callaway and Sant'Anna (2021) estimator, Panel B for standard two-way fixed effects, and Panel C for Borusyak et al. (2024).

Column 1 of Table 6 suggests women may shift into occupations with a slightly higher importance of physical abilities. Although the Callaway and Sant'Anna (2021) estimate suggesting that, on average, women of childbearing age are currently or most recently employed in occupations that are 0.006 standard deviations higher on the importance scale is not statistically significant, estimates from the other two estimates in Panels B and C are slightly larger and statistically significant at the 10% level. The increase is relatively small, representing only 2% of the gap between women's and men's average occupations, but still meaningful in that it shows that these jobs may have become more accessible for women, and that at least on average, the increased accessibility outweighs any new employer reluctance to hire women who may need to be accommodated down the road. In Column (2), the Callaway and Sant'Anna (2021) estimate of the change in the probability that a woman (relative to a man) is employed in an occupation with an above-mean importance index is small and not statistically significant. However, the two-way fixed effects and Borusyak et al. (2024) estimates both suggest a 0.8 percentage point increase in the probability of women being employed in these more intensely physical occupations. Across the estimators, the change in the probability of being in an occupation that is at least one standard deviation above the mean in the importance of physical abilities is very small and not statistically significant. Taken together, this table highlights that pregnancy accommodation laws may be associated with slight changes in the physical intensity of occupations in which women are employed, but the shifts are relatively small. Given the overall increase in the importance index is not accompanied by an increase in the probability of working in one of the most physically-intense occupations, the occupation shift is likely occurring into occupations that are somewhat physically demanding but not the most physically demanding. Dynamic estimates are presented in Figure 3. The dynamic estimates are consistent with the point estimates.

5.1 Heterogeneity

While our baseline results suggest little effect of pregnancy accommodation laws on employment or earnings outcomes on average, it is possible that effects are heterogeneous. While pregnancy accommodation laws are passed at the state level, not all individuals will be "exposed" to the law. For example, some occupations may not require changes to their traditional duties due to the nature of the work, while other occupations may require significant adjustments during pregnancy. In addition, some careers are likely to already provide reasonable accommodations if they are needed. In order to assess whether pregnancy accommodation laws differentially impact individuals who may be more "exposed" to the policy, we investigate heterogeneity by education, occupation, and family structure using the ACS data.[10]

[9] See Section 3.2 for a more detailed explanation of how this measure is constructed.

[10] We are unable to perform the heterogeneity analyses using the QWI data as the samples would not be directly comparable. For example, the sex-by-education data in the QWI is unable to be restricted to individuals of child-bearing age. In addition, QWI data has information on industry rather than occupation and does not contain any information on family structure.

5.1.1 Heterogeneity by Education

We first consider heterogeneity by education levels as women with lower levels of formal education are more likely to be employed in physically-demanding jobs that require accommodation. Table 7 presents the triple difference estimates separately for those with a high school diploma or less (Panel A) and more than a high school diploma (Panel B). Point estimates in Panel A all suggest a larger (negative) effect of pregnancy accommodation laws on women with less formal education as compared to the more-educated women in Panel B, suggesting heterogeneous effects based on education levels, though most outcomes are still not statistically different from zero. We do see marginally significant ($p<0.1$) declines in the number of weeks worked per year and annual earnings for women in the lower-education group. In particular, the point estimates in Panel A suggest annual earnings for women with low formal education relative to men in the same group fall by roughly 3.3% following the passage of pregnancy accommodation laws, though there is no detectable effect on hourly wage. In addition, women with lower formal education see a reduction of approximately 0.39 weeks worked per year.

Figure 4 plots the dynamic specification for individuals with low formal education. These figures confirm the results in the table. While there are no significant changes in unemployment, employment-to-population ratio, or labor force participation, there are decreases in income and weeks worked following the implementation of a pregnancy accommodation law. The corresponding plots for the triple differences dynamic specification for the group with higher formal education are available in Figure A4 in the Appendix.

5.1.2 Heterogeneity by Occupation

We next consider if the overall null result is masking heterogeneity by occupation type. In particular, workers in more physically strenuous occupations are more likely to require accommodations. We split individuals based on they physical intensity of their current (or most recent) occupation. In particular, we use the standardized importance measure developed in Section 3.2 to split the sample into occupations with an importance measure below mean, above mean, and one standard deviation above the mean.[11]

Table 8 shows the triple difference estimates based on the importance index of the occupation. The split in occupations at the mean does not reveal any clear differences in labor market outcomes between women in occupations where physical abilities are less important (Panel A) versus more important (Panel B). Estimates for both groups are generally small, and no estimates are statistically significant. For individuals with occupations one standard deviation above mean importance index, there is modest evidence of negative labor market effects. There is a statistically-significant decrease in weeks worked per year by 0.6 for individuals in one standard above mean occupations. Dynamic figures are presented for above mean occupations in Figure 5, while Figures A5 and A6 present dynamic figures for below mean and one standard deviation above mean, respectively.

Tables A5, A6, and A7 in the Appendix present estimates of the robustness of these estimates to the standard two-way fixed and Borusyak et al. (2024) estimators. The results are generally consistent across specifications except that for both below-mean and above-mean occupations, the other two estimators point to a reduction in the number of weeks worked per year by about 0.3 weeks. For occupations where the importance of physical abilities is more than one standard deviation above the mean, the other estimators suggest a 0.8- to 0.9-week decline in number of weeks worked per year, similar to but slightly larger than the 0.6 weeks from the Callaway and Sant'Anna (2021) estimate. Consistent with the larger estimated decline

[11] Figure A3 plots the distribution of the importance measure for males and females separately.

in weeks worked, both estimates suggest a decline in annual earnings of 3.8% for women of childbearing age in these occupations relative to men in the occupations.

5.1.3 Heterogeneity by Family Structure

As a final heterogeneity analysis, we consider whether pregnancy accommodation laws differentially affect individuals based on their family structure. Family structure may provide a signal to employers about the likelihood a woman will become pregnant (and therefore require accommodation). We split the sample based on marital status and the presence of children in the home, creating four groups: married with and without children, and single with and without children. We would expect single women without children to be the least affected since employers are likely to view them as the least likely to require accommodations in the near future. We speculate that married women without children are likely to be the most affected since they are probably the most likely to have children in the near future.

Table 9 presents these results for heterogeneity by family structure. Panel A shows the results for married with children, Panel B for married without children, Panel C for single with children, and Panel D for single without children. Importantly, we find no effect for single women of childbearing age who do not have children, the group of women least likely to need pregnancy accommodations, providing somewhat of a placebo check. This table highlights that much of the observed effects, both in terms of magnitude and significance, are concentrated among married women without children, a group where future pregnancies, and therefore accommodations, are likely. Somewhat surprisingly, for some outcomes, single women with children also experience negative labor market effects on a similar magnitude as married women without children, though they are mostly not statistically significant. For both groups, the results for log of hourly wage suggest a reduction in wages of 2 to 3 percent following a pregnancy accommodation law, contributing to a 3 to 4 percent reduction in earnings, though only the reduction in hourly wages for those married without kids is statistically significant. This amounts to a reduction in earnings of approximately $1,000 per year for married women without kids and $725 per year for single women with kids. In addition, we estimate one percentage point declines in labor force participation for both groups, though the decline in the employment-to-population ratio for single women with children is not statistically significant. Married women with children show, if anything, positive effects from the pregnancy accommodation laws, with a 0.7 percentage point decline in unemployment and 1.1 percentage point increase in the employment-to-population ratio. These changes could reflect an increase in employment accessibility and continuity due to the accommodation laws. The dynamic estimates are present in Appendix Figures A7 to A10.

Appendix Tables A8, A9, A10, and A11 present results for these specifications using two-way fixed effect and Borusyak et al. (2024) for married women with kids, married women without kids, single women with kids, and single women without kids, respectively. Results for married women with kids consistently show a positive effect of pregnancy accommodation laws on labor market outcomes for this group, suggesting that the laws allow them to stay employed while having children. Results for married women without kids are qualitatively similar to the main Callaway and Sant'Anna (2021) results, showing worsening labor market outcomes for women without children. However, for the two-way fixed effects specification, only the decline in hourly wage is statistically significant. The Borusyak et al. (2024) in Panel C of Table A9 show statistically-significant declines of 0.2 weeks worked per year, 3.5% in hourly wages, and 4.4% in annual earnings. For single women with children, two-way fixed effects estimate in Panel B of Table A10 suggest no detectable changes in labor market outcomes. Borusyak et al. (2024) estimates in Panel C suggest declines in the employment-to-population ratio, hourly wage, and annual earnings. Finally, for single women without children, results are largely consistent with no changes in labor market outcomes except that both other estimators find a 0.4-week reduction in the number of weeks worked per year. Overall, results show that estimates from the other estimators tell a qualitatively similar story by family structure.

6 Conclusion

This paper estimates the causal effect of pregnancy accommodation laws on labor market outcomes for women in their childbearing years. From an employer perspective, these laws impose a group-specific mandated benefit that increases the expected value of the cost of employing women of child-bearing age who could become pregnant. Thus, they could affect the employment and/or wages of millions of women, whether or not they ever become pregnant. Although the additional cost imposed by accommodations could lead to a decrease in wages for women of child-bearing age (if women value the benefit) or a decline in employment (if they do not), we find no measurable impacts on female employment, earnings, or wages overall. We do find some evidence that the laws led to changes in occupation choice: under two of the three specifications, female workers are more likely to be employed in occupations where physical stamina and strength are important. Pregnancy accommodation laws may give women the freedom to enter more physically-demanding occupations without worrying that they will need to quit should they become pregnant. The laws may also allow women who would have switched out of these occupations due to pregnancy to remain in those positions.

Although we do not find overall impacts on employment or wages, heterogeneity analysis reveals that there are labor market impacts for subgroups who are more likely to require pregnancy accommodations. Women with lower levels of formal education experience modest declines in earnings and weeks worked per year. Women whose current or most recent occupation is more physically-demanding experience declines in the number of weeks worked per year. Finally, there are generally no discernible labor market impacts on single women without children, who are less likely than married women to become pregnant and therefore require accommodation. But married women without children experience declines in hourly wages and are more likely to be out of the labor force. By contrast, married women with children are actually more likely to be employed, suggesting that the pregnancy accommodation laws may have increased employment continuity for this group. Declines in wages for married women without kids suggests that they at least partially value the new benefit.

Our results are important for understanding potential effects of future pregnancy accommodation laws, including the recently-implemented federal Pregnant Workers Fairness Act. Our work shows that on average, this benefit may not be especially costly to employers and could allow female workers more freedom to choose labor-intensive occupations. However, women whose highest level of education is at most a high school degree and women in highly labor-intensive occupations do experience negative consequences in terms of employment and wages, contributing to a widening of the gender wage gap. One caveat is that if the federal law received more publicity, then perhaps more women would take advantage of it and employers would be aware of it when making hiring decisions, leading to larger effects on employment and/or earnings of women of childbearing age. This is an important area for future research. Considering policy solutions that could lessen the cost for employers, such as grants or tax credits to partially or fully compensate for pregnancy accommodations, is another important area for future research.

7 Declarations

The authors have no relevant financial or non-financial interests to disclose. They did not receive any funding for this project.

The data sets used in this paper are publicly available and a replication package will be posted upon article acceptance.

References

A Better Balance (2023). State Pregnant Workers Fairness Laws. https://www.abetterbalance.org/resources/pregnant-worker-fairness-legislative-successes/. Accessed 2023-11-30.

Acemoglu, D. and Angrist, J. D. (2001). Consequences of employment protection? The case of the Americans with Disabilities Act. *Journal of Political Economy*, 109(5):915–957.

Andresen, M. E. and Nix, E. (2022). What causes the child penalty? evidence from adopting and same-sex couples. *Journal of labor economics*, 40(4):971–1004.

Andresen, M. E. and Nix, E. (2023). You Can't Force Me Into Caregiving: Paternity Leave and the Child Penalty. *Working Paper*.

Antonczyk, D., Fitzenberger, B., and Sommerfeld, K. (2010). Rising wage inequality, the decline of collective bargaining, and the gender wage gap. *Labour economics*, 17(5):835–847.

Bana, S. H., Bedard, K., and Rossin-Slater, M. (2020). The impacts of paid family leave benefits: regression kink evidence from california administrative data. *Journal of Policy Analysis and Management*, 39(4):888–929.

Bartel, A., Rossin-Slater, M., Ruhm, C., Slopen, M., and Waldfogel, J. (2023). The impact of paid family leave on employers: Evidence from new york. *Community, Work & Family*, pages 1–19.

Baum, C. L. (2003). The effect of state maternity leave legislation and the 1993 Family and Medical Leave Act on employment and wages. *Labour Economics*, 10(5):573–596.

Beegle, K. and Stock, W. A. (2003). The labor market effects of disability discrimination laws. *Journal of Human Resources*, 38(4):806–859.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event study designs: Robust and efficient estimation. *Review of Economic Studies*, page rdae007.

Brown, J. H. (2022). The tradeoff between knowledge of mandated benefits and moral hazard. *Southern Economic Journal*, 88(3):1037–1064.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

DeLeire, T. (2000). The unintended consequences of the americans with disabilities act. *Regulation*, 23:21.

Equal Employment Opportunity Commission v. Wal-Mart Stores East LP (2022). 7th U.S. Circuit Court of Appeals, No. 21-1690. https://law.justia.com/cases/federal/appellate-courts/ca7/21-1690/21-1690-2022-08-16.html.

Fields, J. and Wolff, E. N. (1995). Interindustry wage differentials and the gender wage gap. *ILR Review*, 49(1):105–120.

Gao, G. and Livingston, G. (2015). Working while pregnant is much more common than it used to be. *Pew Research Center*.

Goldin, C. (2014). A grand gender convergence: Its last chapter. *American economic review*, 104(4):1091–1119.

Gruber, J. (1994). The incidence of mandated maternity benefits. *American Economic Review*, pages 622–641.

Hotchkiss, J. L. (2004). A closer look at the employment impact of the Americans with Disabilities Act. *Journal of Human Resources*, 39(4):887–911.

Kleven, H. (2023). The geography of child penalties and gender norms: A pseudo-event study approach. NBER Working Paper No. 30176. Cambridge, MA: National Bureau of Economic Research.

Kleven, H., Landais, C., and Leite-Mariante, G. (2023). The child penalty atlas. NBER Working Paper No. 31649. Cambridge, MA: National Bureau of Economic Research.

Kleven, H., Landais, C., Posch, J., Steinhauer, A., and Zweimüller, J. (2019). Child penalties across countries: Evidence and explanations. In *AEA Papers and Proceedings*, volume 109, pages 122–126. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Kleven, H., Landais, C., and Søgaard, J. E. (2021). Does biology drive child penalties? evidence from biological and adoptive families. *American Economic Review: Insights*, 3(2):183–198.

Kunze, A. (2005). The evolution of the gender wage gap. *Labour Economics*, 12(1):73–97.

Lai, Y.-C. and Masters, S. (2005). The effects of mandatory maternity and pregnancy benefits on women's wages and employment in taiwan, 1984–1996. *ILR Review*, 58(2):274–281.

McDaniels, A. K. (2013). Bill would offer more protection to pregnant workers. *The Baltimore Sun*, 2013-03-22.

National Center for O*NET Development (2022). O*NET OnLine. https://www.onetonline.org/. Accessed 2022-11-02.

Pac, J., Pilarz, A. R., and Dudareva, Y. (2023). The effects of state pregnancy accommodation laws on infant health. *Working Paper*.

Ros Pilarz, A., Pac, J., and Walther, A. (2023). The effects of state pregnancy accommodation laws on maternal employment. *Working Paper*.

Rosenbaum, P. (2021). Pregnancy or motherhood cost? a comparison of the child penalty for adopting and biological parents. *Applied economics*, 53(29):3408–3422.

Ruggles, S., Flood, S., Goeken, R., Schouweiler, M., and Sobek, M. (2022). IPUMS USA: Version 12.0 [dataset]. Minneapolis, MN: IPUMS. https://doi.org/10.18128/D010.V12.0.

Shinall, J. B. (2020). Protecting pregnancy. *Cornell Law Review*, 106:987.

Summers, L. H. (1989). Some simple economics of mandated benefits. *The American Economic Review*, 79(2):177–183.

U.S. Bureau of Labor Statistics (2023). Labor Force Participation Rate - Women [LNS11300002], retrieved from FRED, Federal Reserve Bank of St. Louis. https://fred.stlouisfed.org/series/LNS11300002. Accessed November 9, 2023.

Waldfogel, J. (1999). The impact of the family and medical leave act. *Journal of Policy Analysis and Management*, 18(2):281–302.

Young v. United Parcel Service (2015). 575 U.S. 206. https://supreme.justia.com/cases/federal/us/575/12-1226/.

8 Tables

Table 1: State-Level Pregnancy Accommodation Laws

Year	States with new laws
Before 2000	California Hawaii
2013	Maryland
2014	Delaware Minnesota New Jersey West Virginia
2015	Illinois Nebraska North Dakota Rhode Island Washington, D.C.
2016	Colorado New York Utah
2017	Connecticut Nevada Washington
2018	Massachusetts South Carolina Vermont
2019	Kentucky Maine

Notes: States listed in the table passed laws requiring reasonable accommodations for pregnant workers in the given years. Within a year, states are listed in alphabetical order. States not listed in the table did not pass a state-level pregnancy accommodation law before 2020, with the following exceptions: Alaska and North Carolina passed accommodation laws that apply only to the state government and Louisiana has an older law only requiring transfer to a new position if available. Information about pregnancy accommodation laws is from the A Better Balance (2023) database.

Table 2: Summary Statistics (ACS)

	Treated States			Untreated States		
	Overall	Men	Women	Overall	Men	Women
Unemployed	0.08	0.08	0.07	0.08	0.08	0.08
In Labor Force	0.80	0.85	0.75	0.79	0.84	0.74
EPOP	0.74	0.78	0.70	0.73	0.77	0.68
Income, (2010 dollars)	31,785.31	38,605.30	24,950.62	25,795.63	31,524.43	20,021.16
Hourly Wage, 2010 dollars	21.30	22.92	19.52	17.57	18.91	16.06
Log(wage), 2010 dollars	10.02	10.19	9.84	9.87	10.04	9.67
Weeks Worked Last Year	44.16	44.80	43.45	43.93	44.67	43.10
Black	0.14	0.13	0.15	0.15	0.14	0.16
Asian	0.07	0.06	0.07	0.03	0.03	0.03
White	0.70	0.70	0.69	0.75	0.75	0.74
Other Non-White	0.10	0.10	0.09	0.07	0.08	0.07
HS Degree or Less	0.37	0.41	0.33	0.43	0.47	0.38
Age	31.11	31.02	31.20	31.00	30.89	31.11
Immigrant	0.21	0.21	0.21	0.14	0.15	0.13
Female	0.50	0.00	1.00	0.50	0.00	1.00
Observations	3,731,500	1,847,059	1,884,441	7,956,552	3,963,764	3,992,788

Notes: Table presents summary statistics for our analytic sample from the 2001 to 2019 rounds of the American Community Survey. Sample includes respondents of childbearing age, ages 18 to 44 years. Summary statistics are presented by treatment status, where treated states are states that ever implement a pregnancy accommodation law during this time period and untreated states do not. Statistics are further split by sex, where we consider women as treated by the laws and men as the untreated comparison group. All statistics are calculated using ACS-provided person weights. This table presents descriptive statistics for the micro-data used in the analysis, but note that in the regression analysis, we aggregate observations to the state level using person weights.

Table 3: Effect of Pregnancy Accommodation Laws on Labor Market Outcomes (ACS)

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Male outcomes						
Treated x Post	0.097	-0.096	-0.052	0.117*	0.926	1.577
	(0.335)	(0.304)	(0.239)	(0.063)	(0.761)	(1.056)
	[8.311]	[76.524]	[83.429]	[44.746]	[276.378]	[1,009.336]
Observations	720	720	720	720	720	720
Panel B: Female Outcomes						
Treated x Post	0.050	0.192	0.184	-0.035	1.036	0.620
	(0.235)	(0.234)	(0.212)	(0.103)	(0.662)	(1.081)
	[7.848]	[69.830]	[75.774]	[43.786]	[263.966]	[979.669]
Observations	720	720	720	720	720	720
Panel C: DDD Estimate						
Treated x Post	-0.043	0.290	0.241	-0.150	0.113	-0.915
	(0.261)	(0.258)	(0.307)	(0.135)	(0.562)	(1.293)
	[-0.503]	[-6.615]	[-7.605]	[-0.955]	[-12.543]	[-29.766]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Each entry represents a separate regression with the dependent variable specified in the column. Panels A, B, and C present results on the male sample, female sample, and the difference between female and male outcomes. The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. Note that in Panel C, this dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1.

Table 4: Robustness to Alternative Estimators and Control Groups

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	-0.043	0.290	0.241	-0.150	0.113	-0.915
	(0.261)	(0.258)	(0.307)	(0.135)	(0.562)	(1.293)
	[-0.503]	[-6.615]	[-7.605]	[-0.955]	[-12.543]	[-29.766]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	-0.001	0.124	0.071	-0.251***	0.383	-0.117
	(0.144)	(0.311)	(0.304)	(0.071)	(0.597)	(0.948)
	[-0.503]	[-6.615]	[-7.605]	[-0.955]	[-12.543]	[-29.766]
Observations	760	760	760	760	760	760
Panel C: Borusyak et al.						
Treated x Post	0.001	0.209	0.187	-0.277***	-0.018	-0.712
	(0.145)	(0.265)	(0.227)	(0.065)	(0.470)	(0.475)
	[-0.503]	[-6.615]	[-7.605]	[-0.955]	[-12.543]	[-29.766]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All specifications use males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table 5: Effect of Pregnancy Accommodation Laws on Labor Market Outcomes (QWI)

	(1) Log(Employment)	(2) Log(New Hires)	(3) Log(Separations)
Panel A: Male outcomes			
Treated x Post	-0.912	-2.682*	-5.831***
	(0.600)	(1.444)	(1.824)
	[1,369.581]	[1,194.044]	[1,202.310]
Observations	2,650	2,627	2,654
Panel B: Female Outcomes			
Treated x Post	-0.556	-2.691**	-4.250***
	(0.612)	(1.102)	(1.315)
	[1,366.225]	[1,182.270]	[1,191.507]
Observations	2,650	2,627	2,654
Panel C: DDD Estimate			
Treated x Post	0.358	0.052	1.667*
	(0.334)	(0.672)	(0.900)
	[-4.212]	[-12.181]	[-11.274]
Observations	2,650	2,627	2,654
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using the method from Callaway and Sant'Anna (2021). Data is from the Quarterly Workforce Indicators, and includes individuals ages 19 to 44 and all quarters in years 2001 to 2019. Each table entry represents a separate regression with the dependent variable specified in the column. Panels A, B, and C present results on the male sample, female sample, and the difference between female and male outcomes. The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. Note that in Panel C, this dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. All coefficients have been multiplied by 100 to improve readability. Standard errors clustered at the state level are reported in parentheses. Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1.

Table 6: Effect of Pregnancy Accommodation Laws on Occupation's Importance of Physical Abilities

	(1) Overall Index	(2) Index Above Mean	(3) Index 1SD Above Mean
Panel A: Callaway and Sant'Anna Estimate			
Treated x Post	0.006	0.002	-0.001
	(0.005)	(0.003)	(0.004)
	[-0.435]	[-0.148]	[-0.142]
Observations	720	720	720
Panel B: Two-Way Fixed Effects			
Treated x Post	0.011*	0.008***	-0.002
	(0.006)	(0.003)	(0.003)
	[-0.435]	[-0.148]	[-0.142]
Observations	760	760	760
Panel C: Borusyak Et al.			
Treated x Post	0.012*	0.008***	-0.002
	(0.006)	(0.002)	(0.003)
	[-0.435]	[-0.148]	[-0.142]
Observations	720	720	720
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications and control groups. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1.

Table 7: Effect of Pregnancy Accommodation Laws by Education Level (ACS)

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Low Education						
Treated x Post	-0.263	0.517	0.327	-0.387*	-0.681	-3.346*
	(0.355)	(0.456)	(0.614)	(0.226)	(0.904)	(1.914)
	[1.472]	[-11.759]	[-12.300]	[-1.584]	[-17.623]	[-41.225]
Observations	720	720	720	720	720	720
Panel B: High Education						
Treated x Post	0.029	0.258	0.279	-0.016	0.857	0.627
	(0.245)	(0.351)	(0.275)	(0.119)	(0.649)	(1.224)
	[-0.210]	[-6.425]	[-6.995]	[-1.114]	[-17.409]	[-34.934]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using the method from Callaway and Sant'Anna (2021) on respondents whose highest level of education is a high school degree or lower (Panel A) and respondents with more than a high school degree (Panel B). Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. The treatment group's mean of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1.

Table 8: Effect of Pregnancy Accommodation Laws by Importance of Physical Abilities

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Below Mean Index						
Treated x Post	0.259	-0.110	0.132	0.071	-0.135	0.739
	(0.240)	(0.366)	(0.333)	(0.178)	(1.186)	(1.727)
	[0.385]	[-4.332]	[-4.152]	[-1.289]	[-22.871]	[-39.578]
Observations	720	720	720	720	720	720
Panel B: Above Mean Index						
Treated x Post	-0.118	-0.056	-0.187	-0.281	0.529	-1.080
	(0.309)	(0.403)	(0.283)	(0.232)	(0.818)	(2.050)
	[-0.209]	[-4.101]	[-4.680]	[-1.633]	[-13.683]	[-39.439]
Observations	720	720	720	720	720	720
Panel C: 1 SD Above Mean Index						
Treated x Post	0.544	-0.356	0.041	-0.566**	0.292	-1.345
	(0.457)	(0.638)	(0.689)	(0.264)	(1.276)	(2.936)
	[-0.839]	[-3.825]	[-4.974]	[-1.889]	[-26.529]	[-61.078]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 estimated using the method from Callaway and Sant'Anna (2021). Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regressions, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table 9: Effect of Pregnancy Accommodation Laws by Family Structure

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Married With Kids						
Treated x Post	-0.744***	1.066*	0.477	0.024	0.539	0.562
	(0.195)	(0.596)	(0.538)	(0.110)	(0.928)	(1.248)
	[2.367]	[-26.043]	[-25.106]	[-3.366]	[-23.940]	[-60.548]
Observations	720	720	720	720	720	720
Panel B: Married Without Kids						
Treated x Post	-0.089	-1.075**	-1.228**	0.009	-3.082**	-3.105
	(0.611)	(0.525)	(0.500)	(0.179)	(1.364)	(2.015)
	[0.921]	[-6.878]	[-6.422]	[-1.192]	[-10.606]	[-25.608]
Observations	720	720	720	720	720	720
Panel C: Single With Kids						
Treated x Post	0.364	-1.172	-0.910*	-0.614*	-2.406	-3.602
	(0.762)	(0.906)	(0.509)	(0.359)	(1.845)	(3.124)
	[1.270]	[-9.991]	[-9.973]	[-0.968]	[-13.718]	[-33.830]
Observations	720	720	720	720	720	720
Panel D: Single Without Kids						
Treated x Post	0.103	0.348	0.461	-0.210	1.202	-0.704
	(0.393)	(0.416)	(0.587)	(0.182)	(0.996)	(1.428)
	[-3.137]	[2.604]	[0.263]	[-0.011]	[-6.850]	[-17.271]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 on individuals married with kids (Panel A), married without kids (Panel B), single with kids (Panel C), and single without kids (Panel D) estimated using the method from Callaway and Sant'Anna (2021). Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. Note that this is the mean of the difference between women and men. All coefficients, with the exception of those from the weeks worked regressions, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

9 Figures

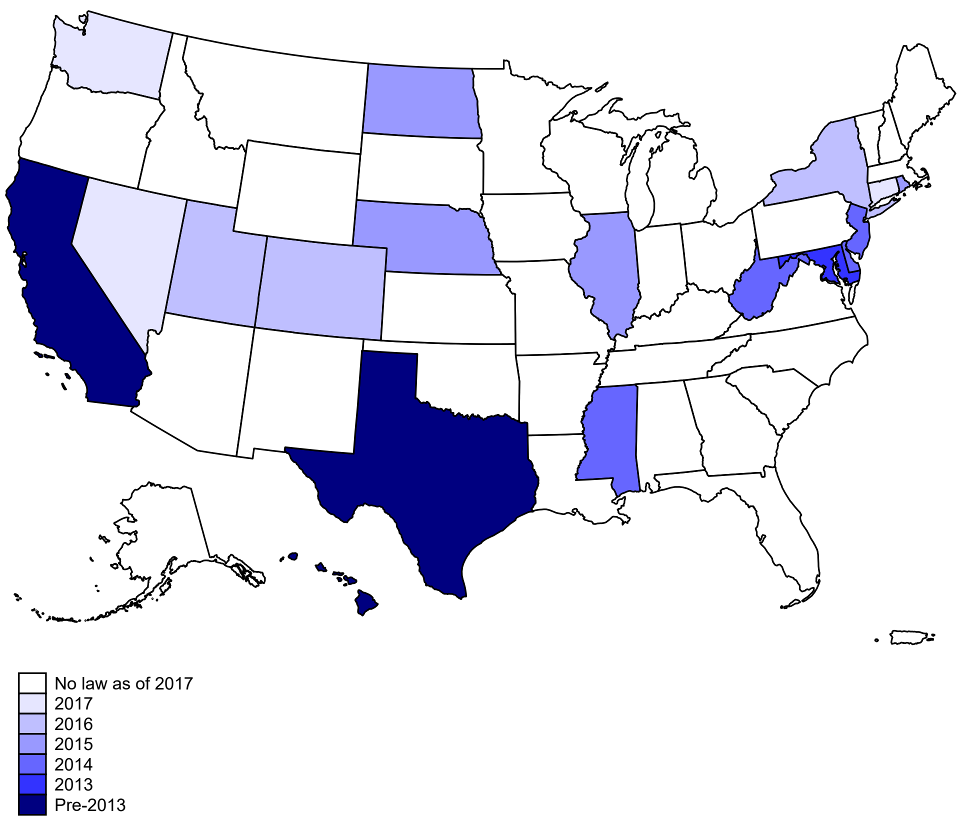
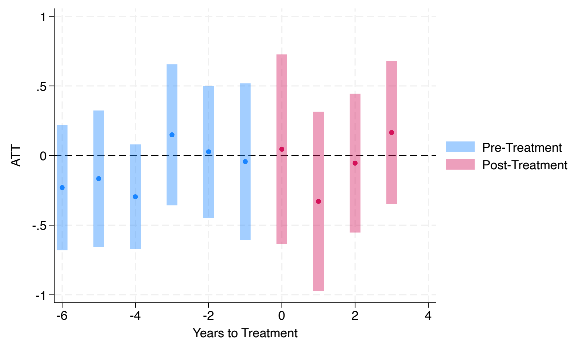
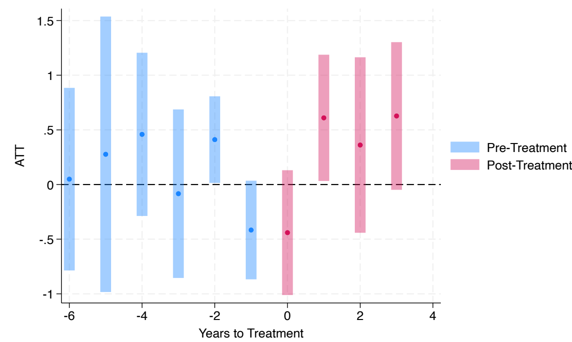


Figure 1: State Pregnancy Accommodation Law Timing

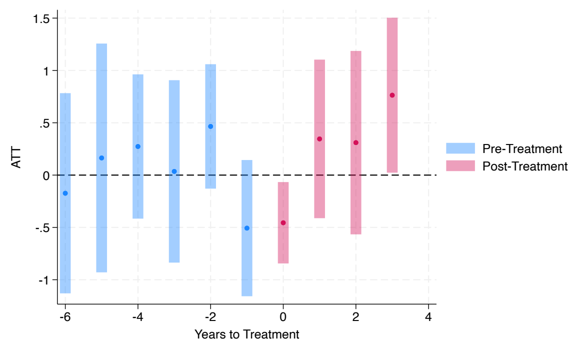
Notes: Figure displays states that passed state-level pregnancy accommodation laws that went into effect between 2013 and 2017.



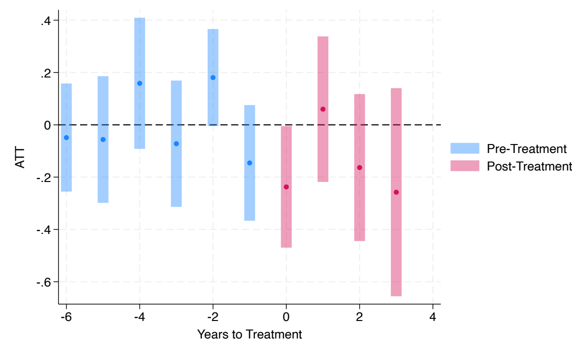
(a) Unemployment



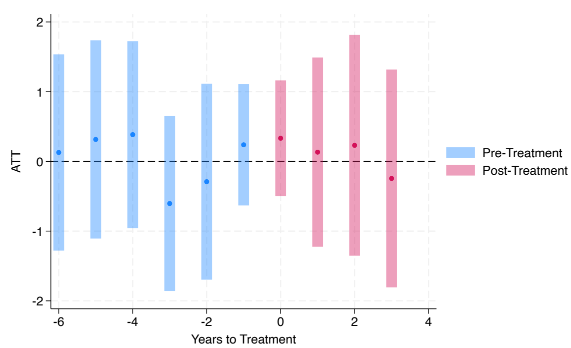
(b) Employment-to-Population Ratio



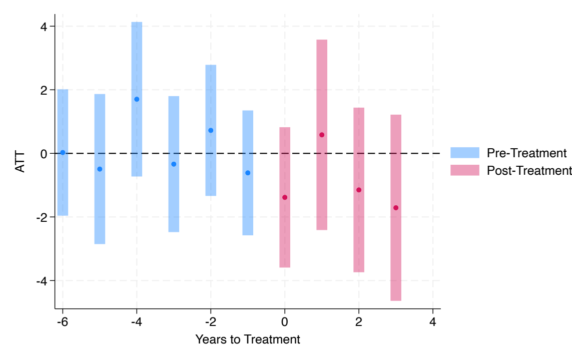
(c) Labor Force Participation



(d) Weeks Worked



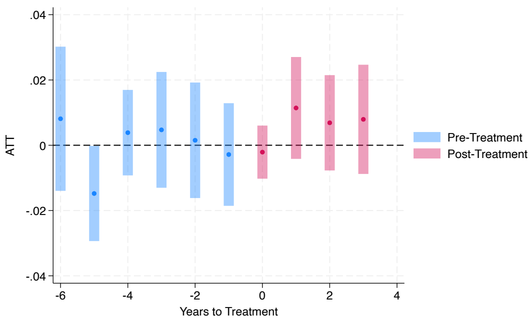
(e) Log of Hourly Wage



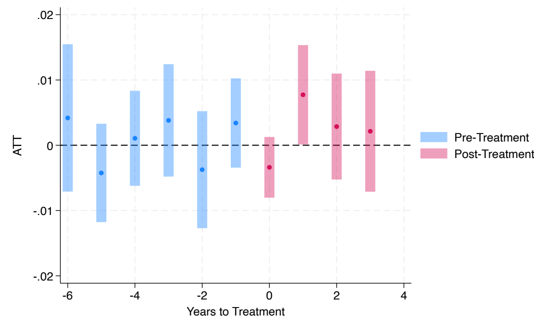
(f) Log of Earnings

Figure 2: Baseline DDD Estimates

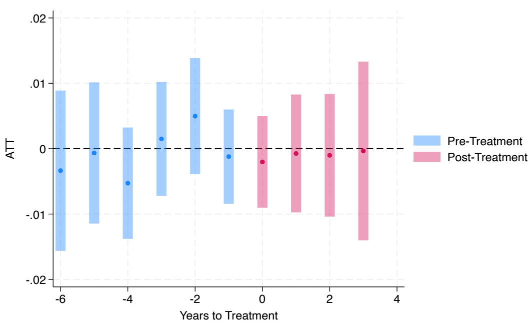
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



(a) DDD Overall Index



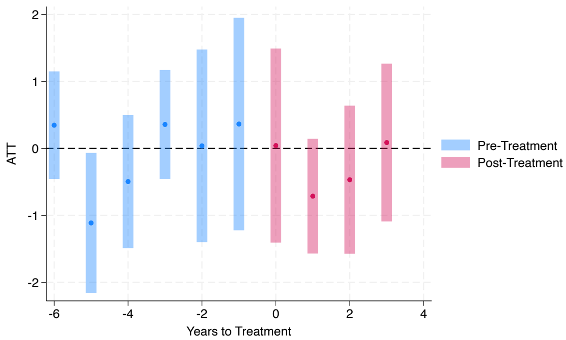
(b) DDD Above Mean Index



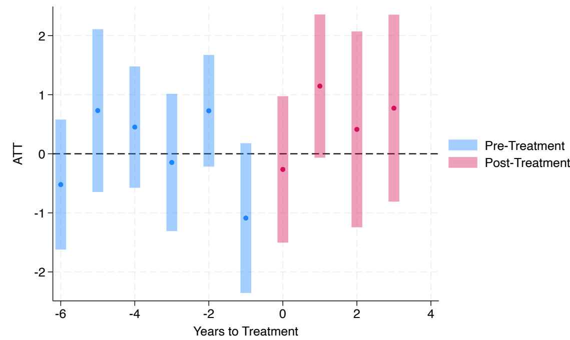
(c) DDD One SD Above Mean Index

Figure 3: Occupation Intensity Results

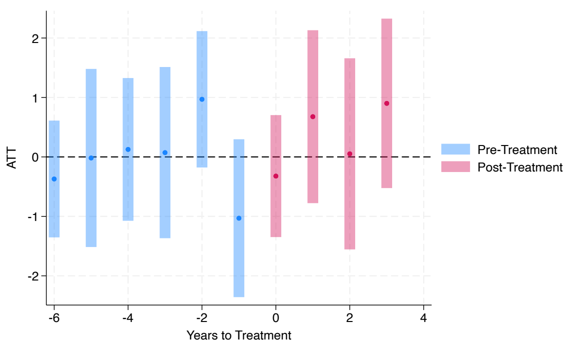
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. The dependent variable is based on an index representing the importance of physical abilities to the occupation, and the index is standardized to have a mean of zero and a standard deviation of one. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



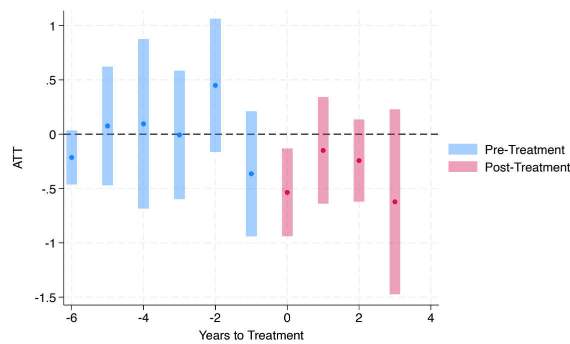
(a) Unemployment



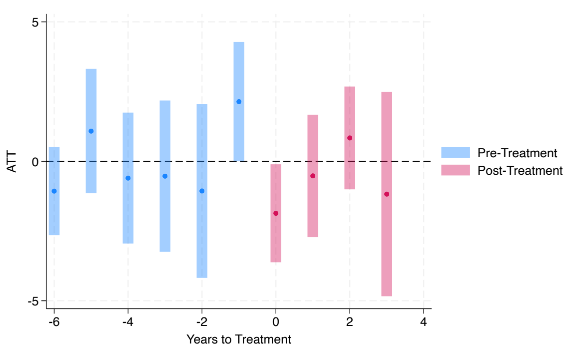
(b) Employment-to-Population Ratio



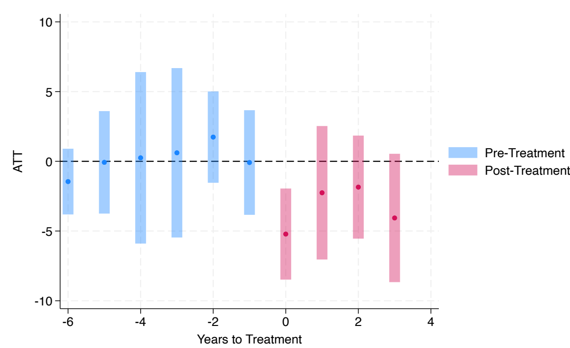
(c) Labor Force Participation



(d) Weeks Worked



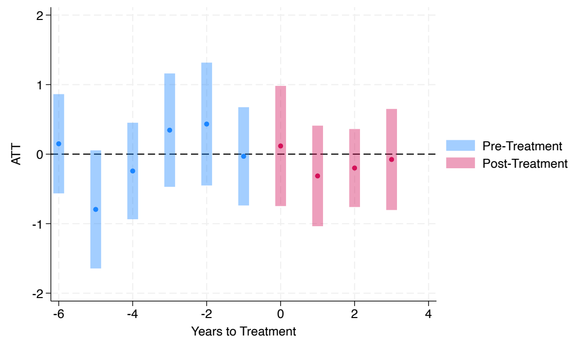
(e) Log of Hourly Wage



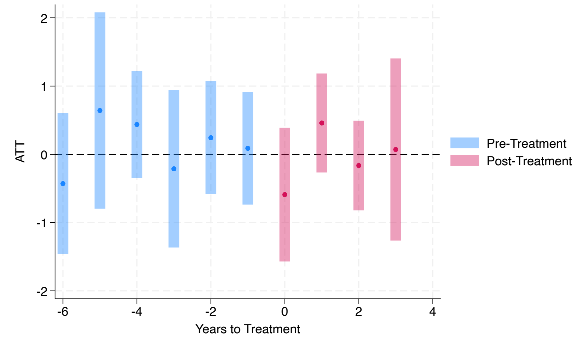
(f) Log of Earnings

Figure 4: Heterogeneity by Education: DDD Estimates for Low Education

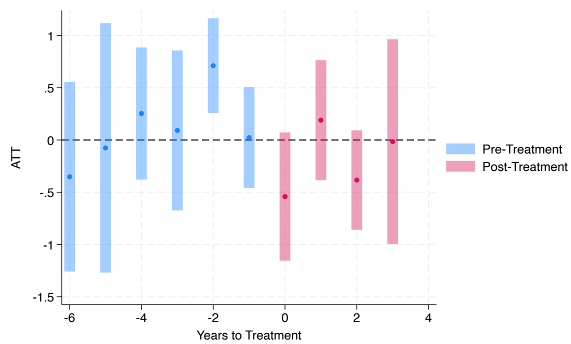
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 whose highest level of education is at most a high school diploma in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



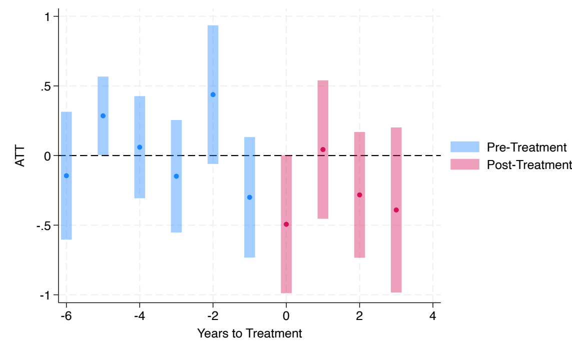
(a) Unemployment



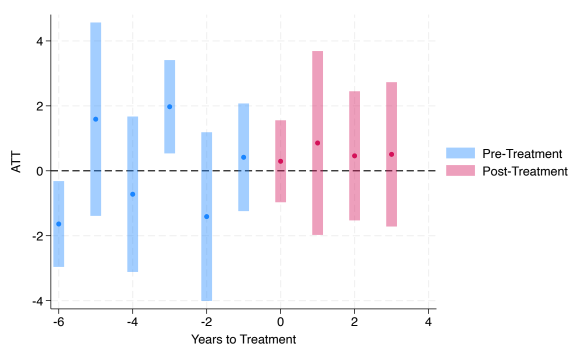
(b) Employment-to-Population Ratio



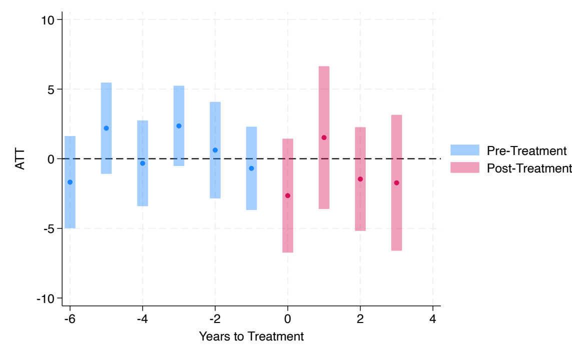
(c) Labor Force Participation



(d) Weeks Worked



(e) Log of Hourly Wage

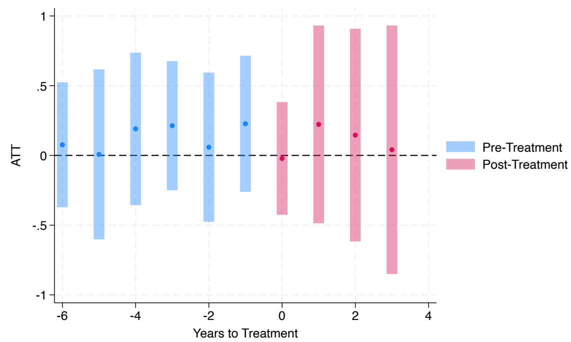


(f) Log of Earnings

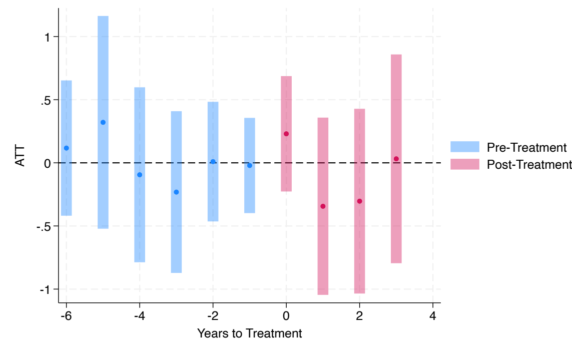
Figure 5: Heterogeneity by Above Mean Occupation: DDD Estimates

Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if the importance of physical abilities to their occupation is above the mean across occupations. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.

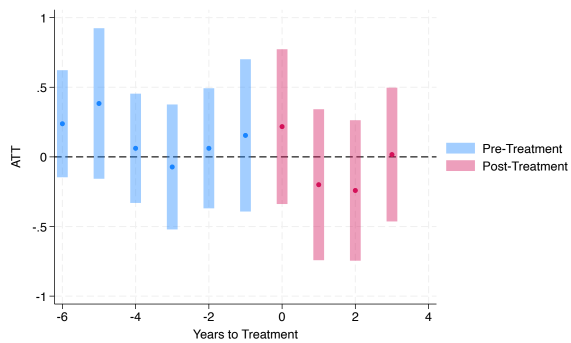
Appendix A Additional Results



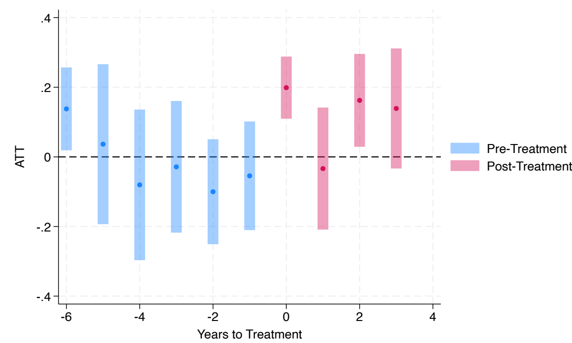
(a) Unemployment



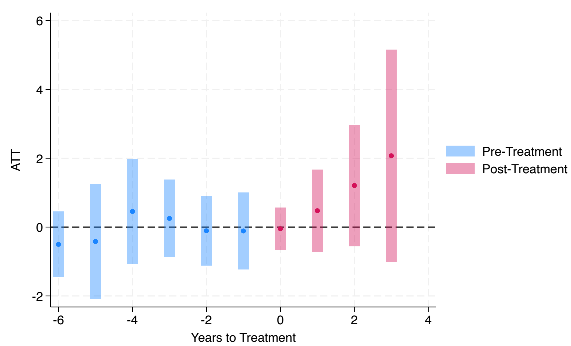
(b) Employment-to-Population Ratio



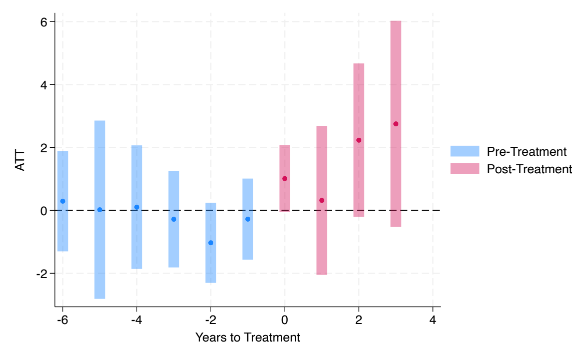
(c) Labor Force Participation



(d) Weeks Worked



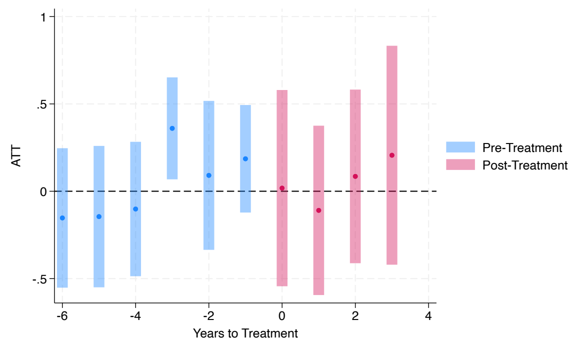
(e) Log of Hourly Wage



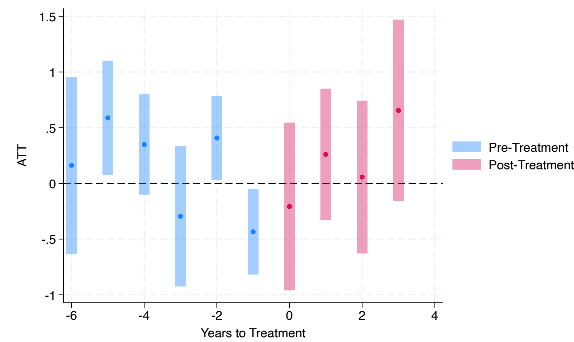
(f) Log of Income

Figure A1: Baseline Male Difference-in-Difference Outcomes

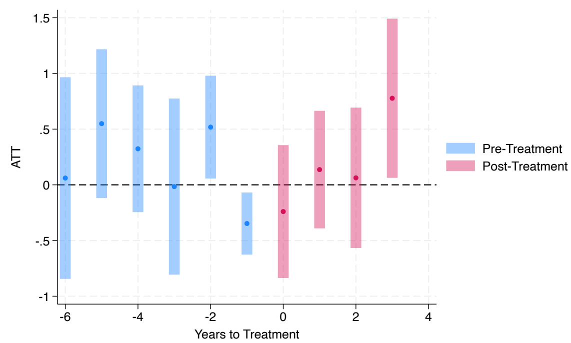
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from male ACS respondents ages 18 to 44 in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



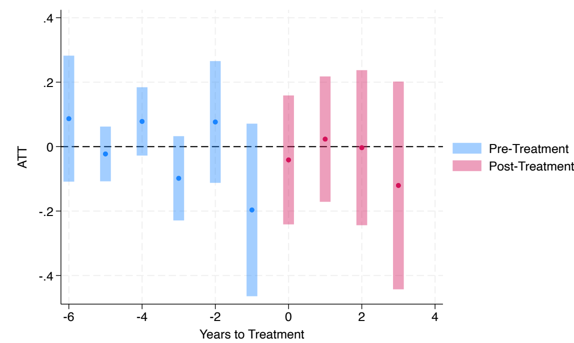
(a) Unemployment



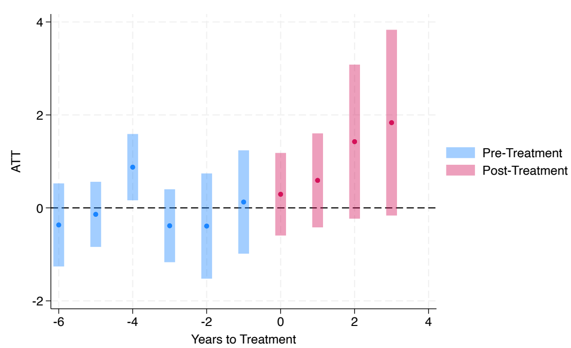
(b) Employment-to-Population Ratio



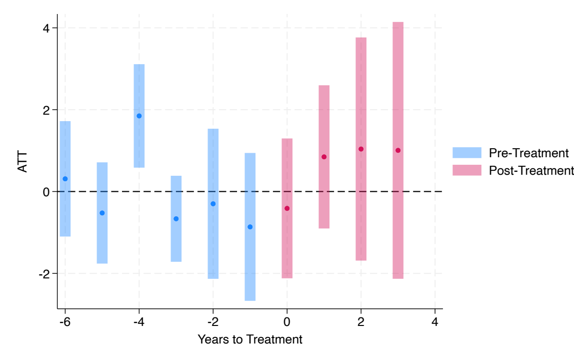
(c) Labor Force Participation



(d) Weeks Worked



(e) Log of Hourly Wage



(f) Log of Income

Figure A2: Baseline Female Difference-in-Difference Outcomes

Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from female ACS respondents ages 18 to 44 in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.

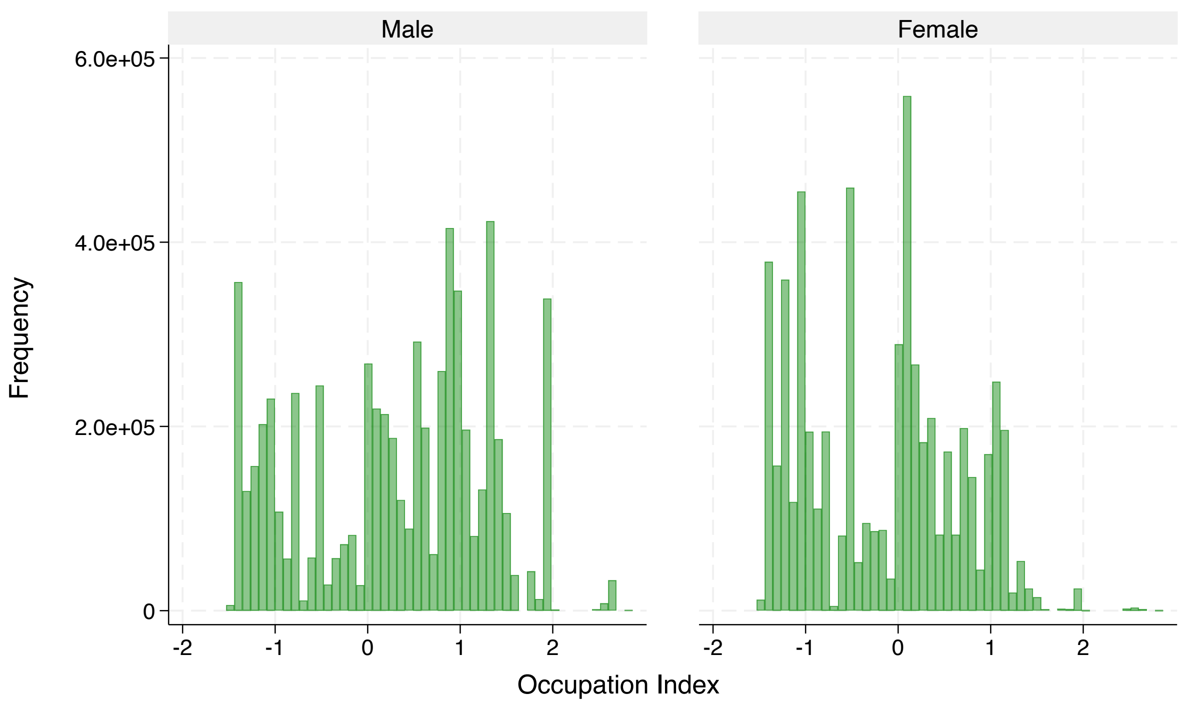
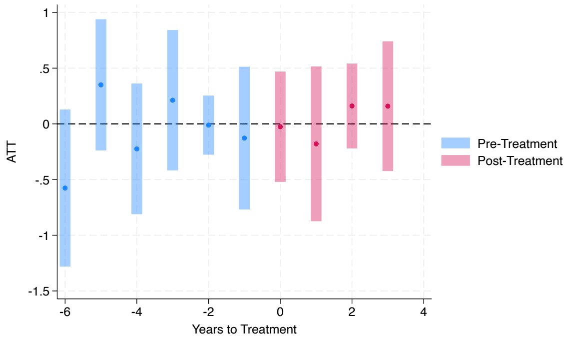
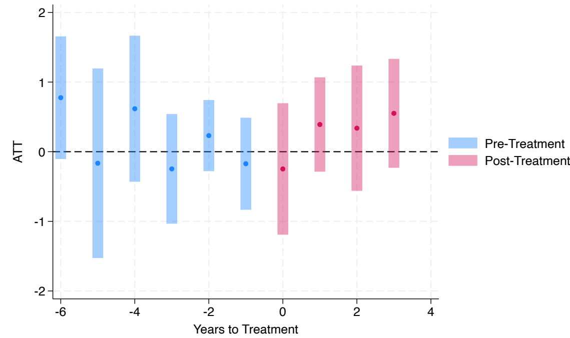


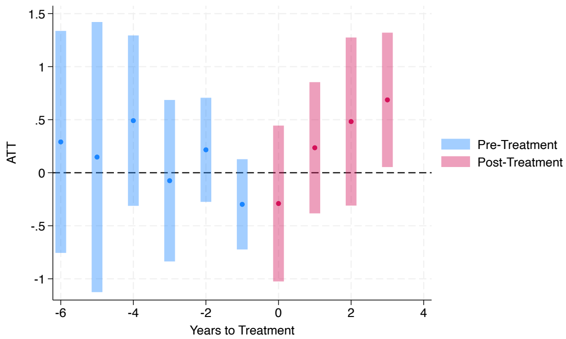
Figure A3: Occupation Physical Abilities Importance Index Distribution
Notes: Figures display the frequency distribution of the importance index for male and female respondents. The index represents the importance of physical abilities to the occupation (either current or most recent) and is standardized to have a mean of zero and a standard deviation of one.



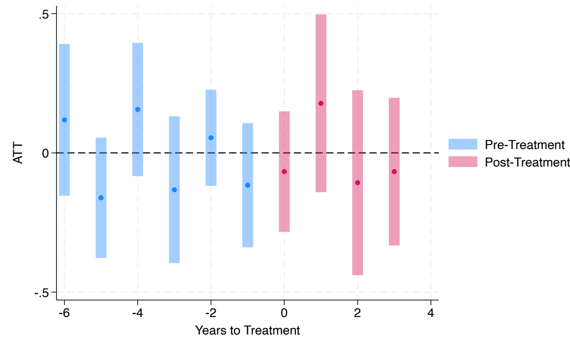
(a) Unemployment



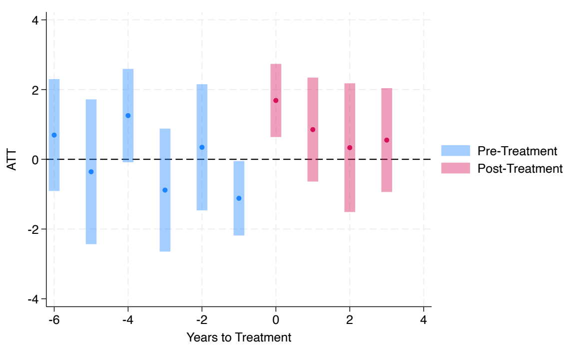
(b) Employment-to-Population Ratio



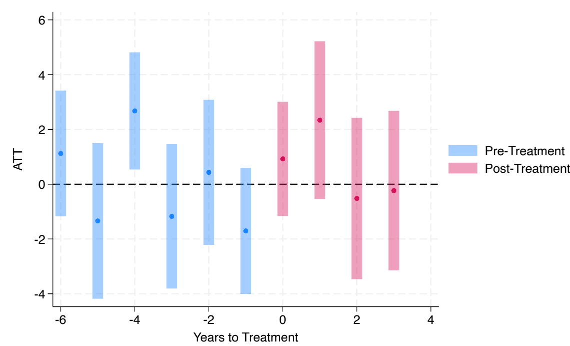
(c) Labor Force Participation



(d) Weeks Worked



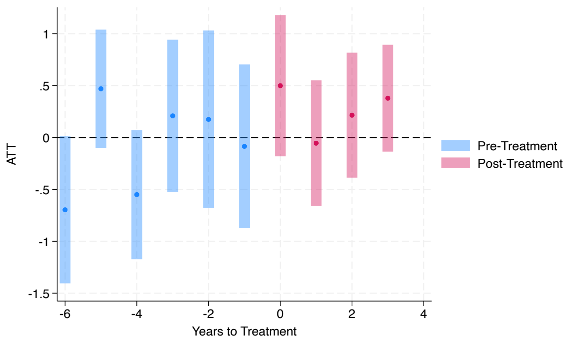
(e) Log of Hourly Wage



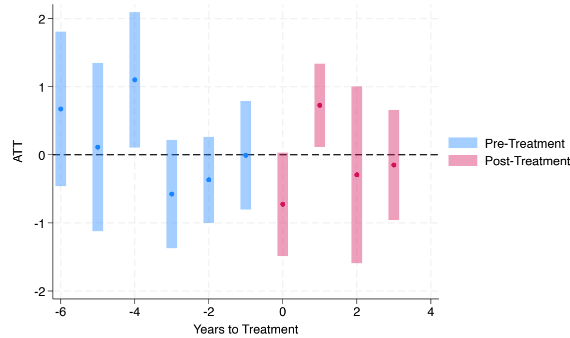
(f) Log of Income

Figure A4: Heterogeneity by Education: DDD Estimates for High Education

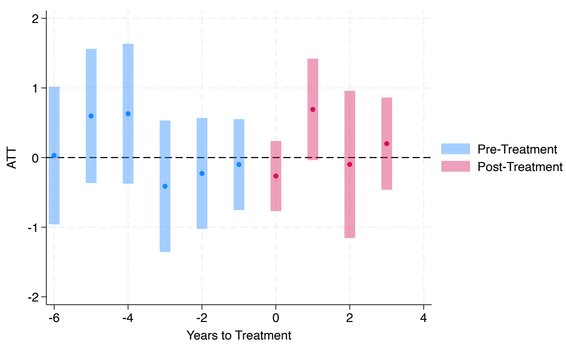
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 whose highest level of education is at least some college in survey years 2001 to 2019. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



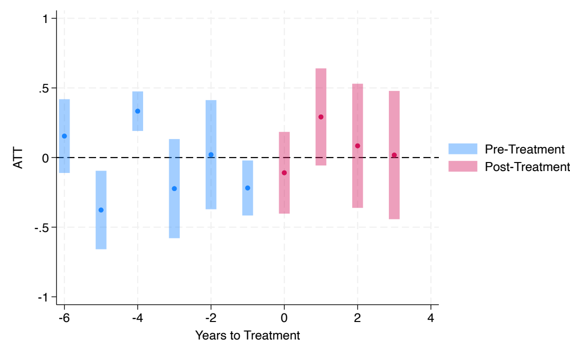
(a) Unemployment



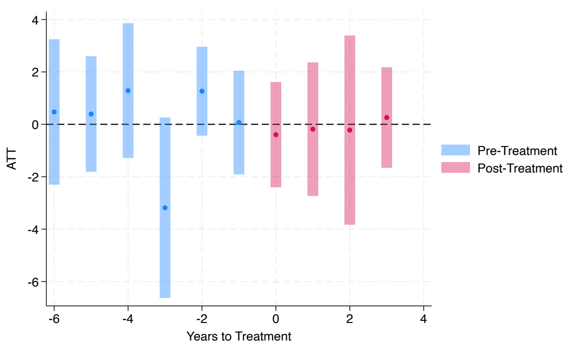
(b) Employment-to-Population Ratio



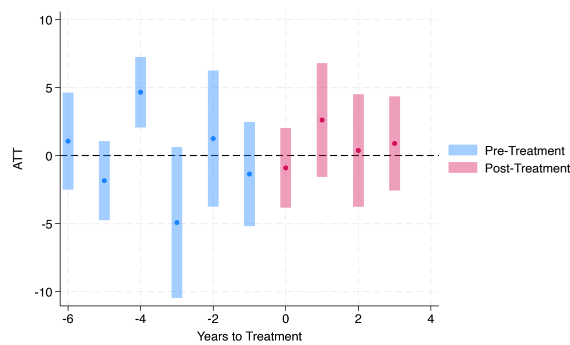
(c) Labor Force Participation



(d) Weeks Worked



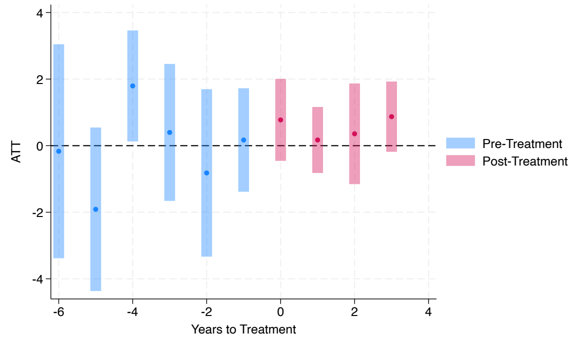
(e) Log of Hourly Wage



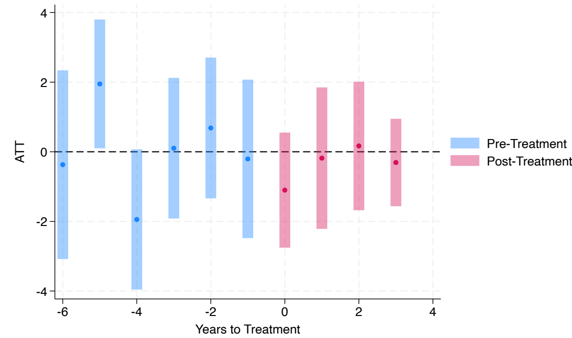
(f) Log of Income

Figure A5: Heterogeneity by Below Mean Occupation: DDD Estimates

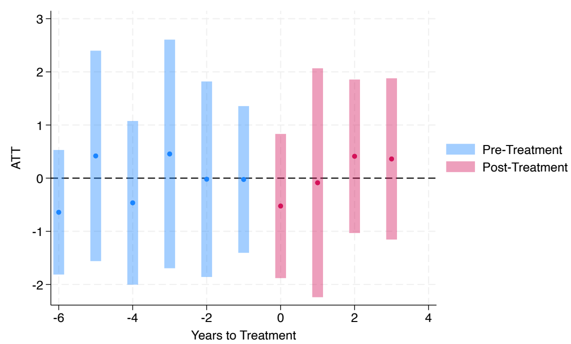
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if the importance of physical abilities to their occupation is below the mean across occupations. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



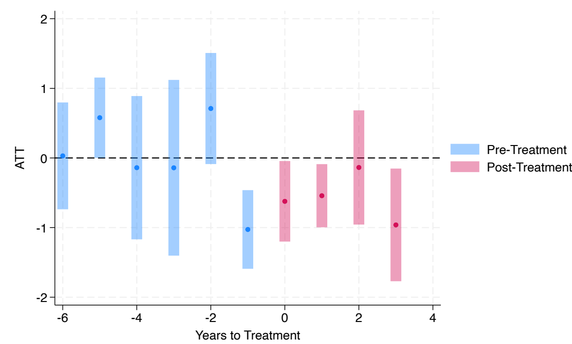
(a) Unemployment



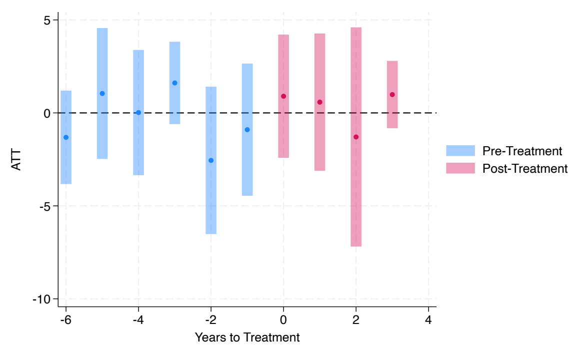
(b) Employment-to-Population Ratio



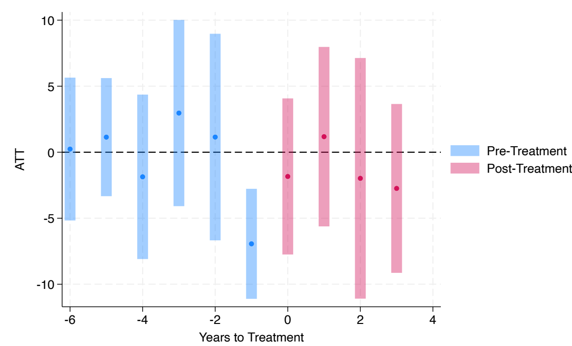
(c) Labor Force Participation



(d) Weeks Worked



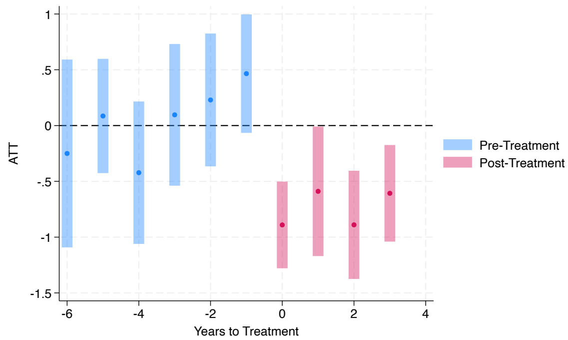
(e) Log of Hourly Wage



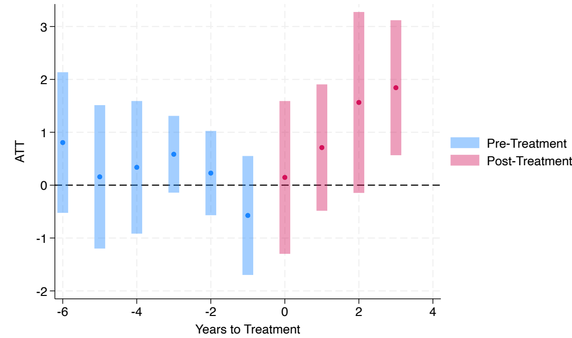
(f) Log of Income

Figure A6: Heterogeneity by 1SD Above Mean Occupation: DDD Estimates

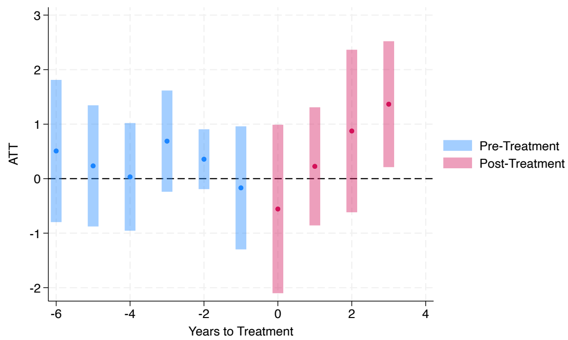
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if the importance of physical abilities to their occupation is at least one standard deviation above the mean across occupations. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



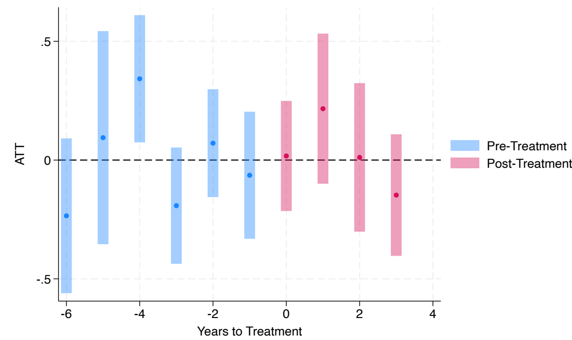
(a) Unemployment



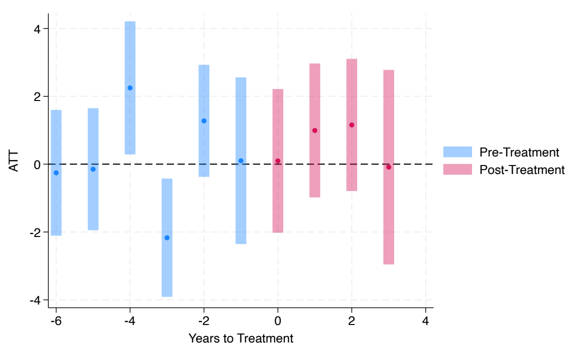
(b) Employment-to-Population Ratio



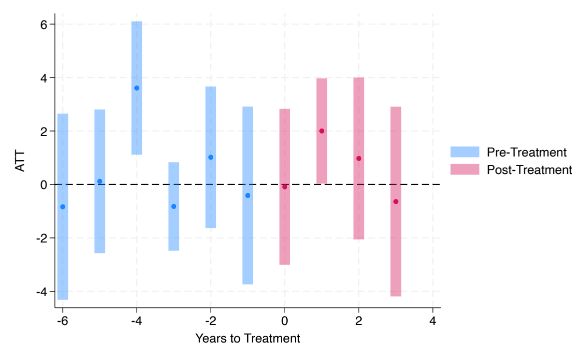
(c) Labor Force Participation



(d) Weeks Worked



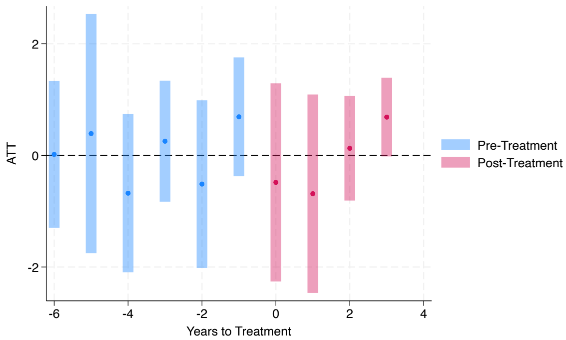
(e) Log of Hourly Wage



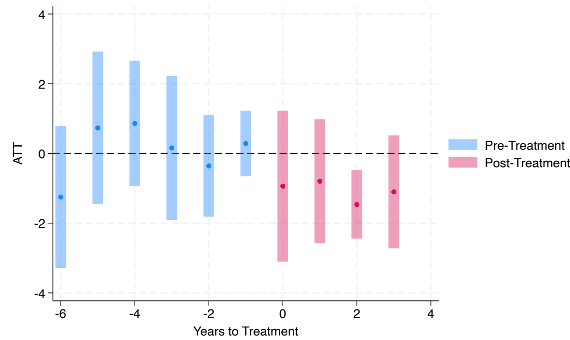
(f) Log of Income

Figure A7: Heterogeneity by Family Structure: DDD Estimates for Married With Kids

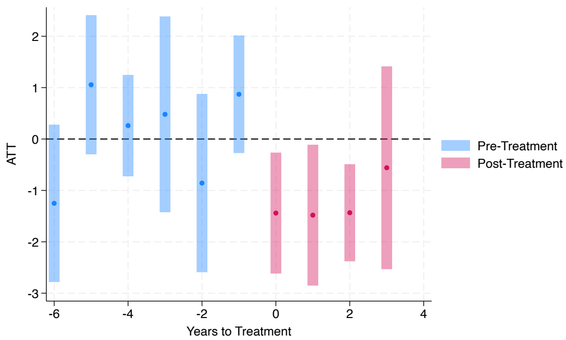
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if they are married and have at least one child. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



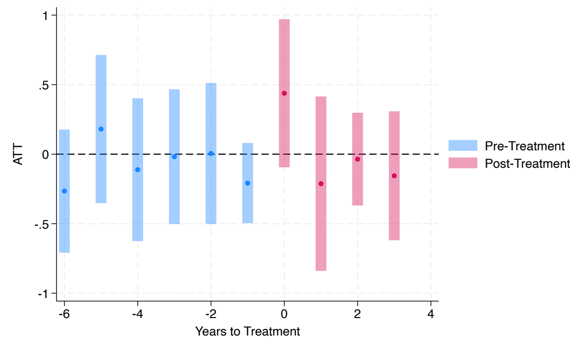
(a) Unemployment



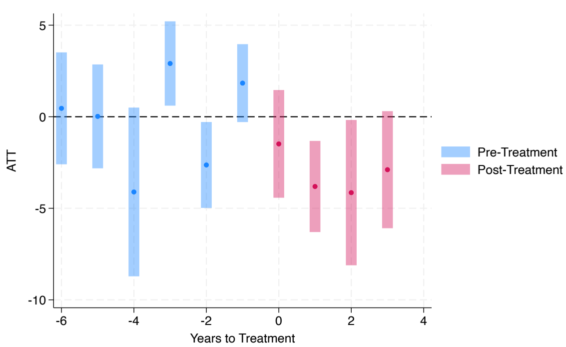
(b) Employment-to-Population Ratio



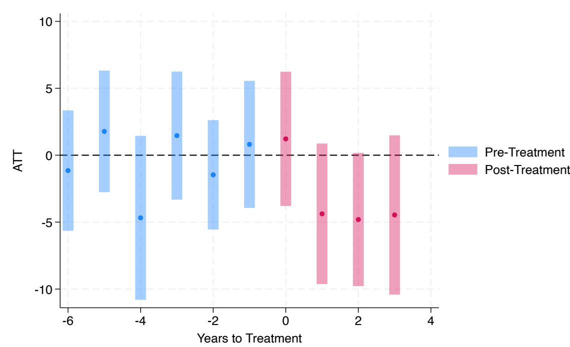
(c) Labor Force Participation



(d) Weeks Worked



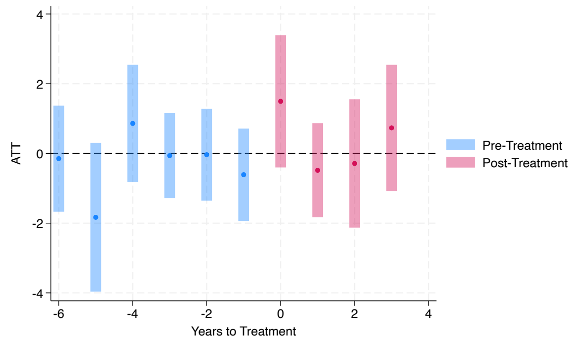
(e) Log of Hourly Wage



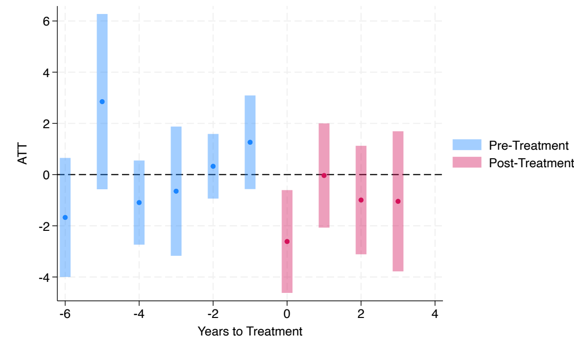
(f) Log of Income

Figure A8: Heterogeneity by Family Structure: DDD Estimates for Married Without Kids

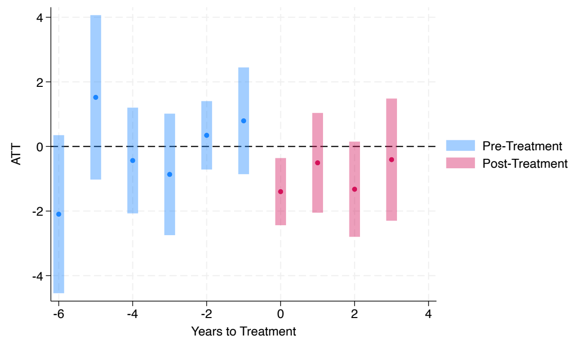
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if they are married and have zero children. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



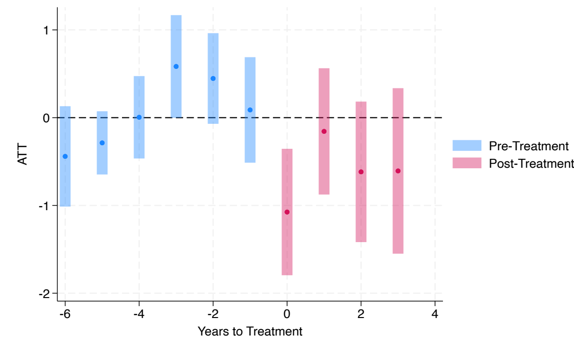
(a) Unemployment



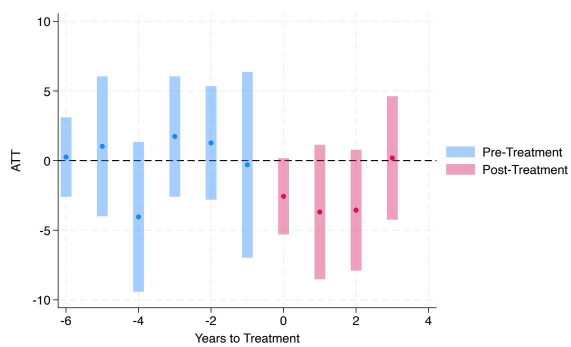
(b) Employment-to-Population Ratio



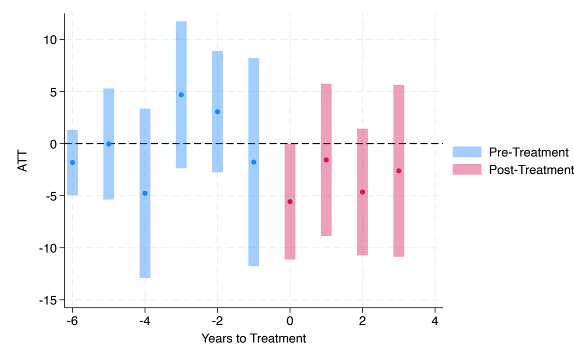
(c) Labor Force Participation



(d) Weeks Worked



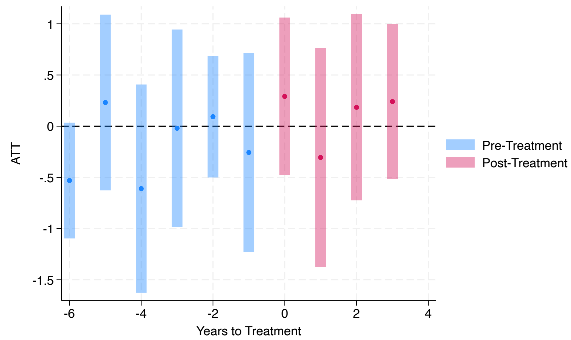
(e) Log of Hourly Wage



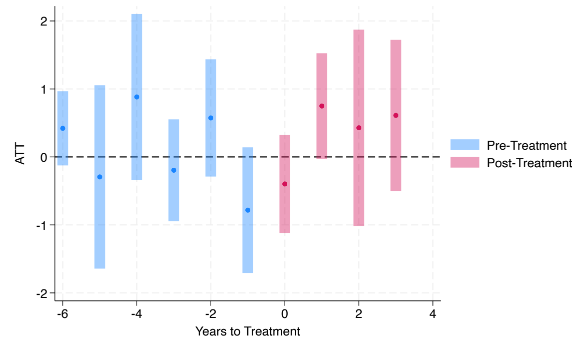
(f) Log of Income

Figure A9: Heterogeneity by Family Structure: DDD Estimates for Single With Kids

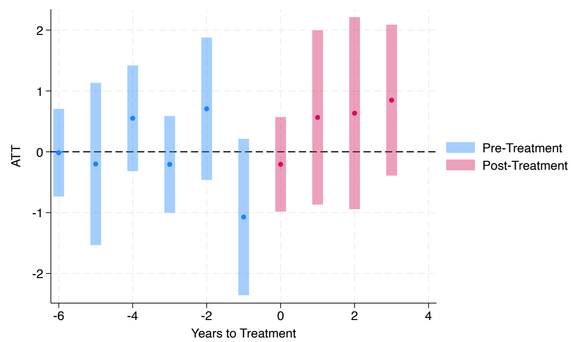
Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if they are unmarried and have at least one child. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.



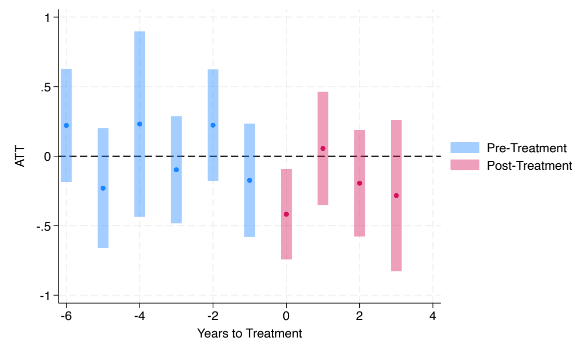
(a) Unemployment



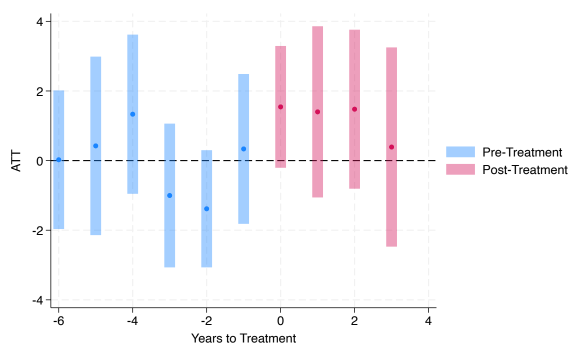
(b) Employment-to-Population Ratio



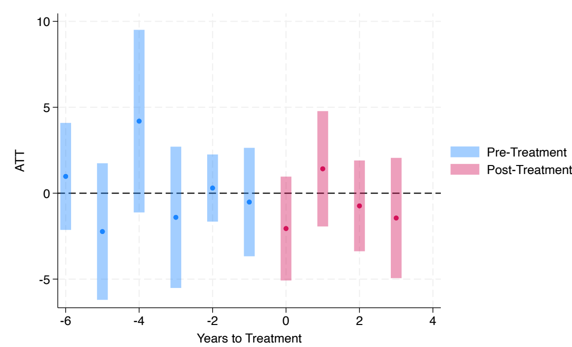
(c) Labor Force Participation



(d) Weeks Worked



(e) Log of Hourly Wage



(f) Log of Income

Figure A10: Heterogeneity by Family Structure: DDD Estimates for Single Without Kids

Notes: Figures display results from estimating Equation 2 using the method from Callaway and Sant'Anna (2021) and data at the state-year level from ACS respondents ages 18 to 44 in survey years 2001 to 2019. Individuals are included if they are unmarried and have zero children. Figures represent a triple differences specification, where the dependent variable is the outcome for women minus the outcome for men, that is: $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level, and error bars represent 95% confidence intervals.

Table A1: Importance Index: Example Occupations

Importance Index	Occupation SOC	Occupation Name
4.20 (most physical)	27-2021	Athletes
3.95	33-2011	Firefighters
3.62	39-9031	Personal trainers
2.26	31-9011	Massage therapists
1.16	29-1123	Physical therapists
0.52	39-9011	Childcare workers
0.25	29-1141	Registered nurses
0.00	11-9051	Food service managers
-0.18	41-2010	Cashiers
-0.50	21-1021	Child, Family, and School Social Workers
-1.11	19-3011	Economists
-1.12 (least physical)	13-2011	Accountants and Auditors

Notes: Table displays example occupations and values of the standardized importance index, including the highest and lowest values the index takes on. The importance index measures the importance of physical abilities to the job and is constructed as the standardized sum of the importance of stamina and the four physical strength categories from the O*NET database: $Importance_j = \frac{\sum_i Importance_{ij} - \overline{\sum_i Importance_{ik}}}{\hat{\sigma}_{\sum_i Importance_{ik}}}$, where $Importance_{ij}$ is the importance of skill i to occupation j. The measure is standardized to have a mean of zero and standard deviation of one.

Table A2: Robustness of Alternative Estimators in QWI Data

	(1) Log(Employment)	(2) Log(New Hires)	(3) Log(Separations)
Panel A: Callaway and Sant'Anna			
Treated x Post	0.358	0.052	1.667*
	(0.334)	(0.672)	(0.900)
	[-4.212]	[-12.181]	[-11.274]
Observations	2,650	2,627	2,654
Panel B: Two-Way Fixed Effects			
Treated x Post	0.477	0.916	0.631
	(0.578)	(0.720)	(0.769)
	[-4.212]	[-12.181]	[-11.274]
Observations	2,802	2,779	2,806
Panel C: Borusyak Et al.			
Treated x Post	0.745**	0.813	0.588
	(0.323)	(0.627)	(0.690)
	[-4.212]	[-12.181]	[-11.274]
Observations	2,650	2,627	2,654
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using alternative difference-in-difference specifications. Data are from the Quarterly Workforce Indicators, and includes all quarters in years 2001 to 2019. Each table entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. Panels A, B, and C present results using males age 19-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A3: Robustness: Effect of Pregnancy Accommodation Laws for Low Education

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	-0.263	0.517	0.327	-0.387*	-0.681	-3.346*
	(0.355)	(0.456)	(0.614)	(0.226)	(0.904)	(1.914)
	[1.472]	[-11.759]	[-12.300]	[-1.584]	[-17.623]	[-41.225]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.164	-0.222	-0.163	-0.483***	-0.038	-2.132*
	(0.254)	(0.466)	(0.538)	(0.138)	(0.600)	(1.223)
	[1.472]	[-11.759]	[-12.300]	[-1.584]	[-17.623]	[-41.225]
Observations	760	760	760	760	760	760
Panel C: Borusyak et al.						
Treated x Post	0.123	-0.033	0.057	-0.483***	-0.141	-2.269**
	(0.242)	(0.304)	(0.290)	(0.117)	(0.678)	(0.959)
	[1.472]	[-11.759]	[-12.300]	[-1.584]	[-17.623]	[-41.225]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the low education sample. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A4: Robustness: Effect of Pregnancy Accommodation Laws for High Education

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	0.029	0.258	0.279	-0.016	0.857	0.627
	(0.245)	(0.351)	(0.275)	(0.119)	(0.649)	(1.224)
	[-0.210]	[-6.425]	[-6.995]	[-1.114]	[-17.409]	[-34.934]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	-0.112	0.545**	0.458**	-0.187***	0.220	-0.204
	(0.128)	(0.277)	(0.234)	(0.070)	(0.469)	(0.780)
	[-0.210]	[-6.425]	[-6.995]	[-1.114]	[-17.409]	[-34.934]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	-0.069	0.526*	0.487**	-0.223***	0.035	-0.641
	(0.114)	(0.272)	(0.234)	(0.068)	(0.447)	(0.520)
	[-0.210]	[-6.425]	[-6.995]	[-1.114]	[-17.409]	[-34.934]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the high education sample. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A5: Robustness: Effect of Pregnancy Accommodation Laws for Below Mean Physical Intensity Index

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	0.259	-0.110	0.132	0.071	-0.135	0.739
	(0.240)	(0.366)	(0.333)	(0.178)	(1.186)	(1.727)
	[0.385]	[-4.332]	[-4.152]	[-1.289]	[-22.871]	[-39.578]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.097	0.232	0.323	-0.238***	0.515	-0.034
	(0.132)	(0.215)	(0.199)	(0.072)	(0.792)	(1.186)
	[0.385]	[-4.332]	[-4.152]	[-1.289]	[-22.871]	[-39.578]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	0.068	0.251	0.323*	-0.282***	0.048	-0.829
	(0.121)	(0.209)	(0.174)	(0.063)	(0.619)	(0.863)
	[0.385]	[-4.332]	[-4.152]	[-1.289]	[-22.871]	[-39.578]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample with occupations with a physical index indicator below the mean. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. All models use males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A6: Robustness: Effect of Pregnancy Accommodation Laws for Above Mean Physical Intensity Index

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	-0.118	-0.056	-0.187	-0.281	0.529	-1.080
	(0.309)	(0.403)	(0.283)	(0.232)	(0.818)	(2.050)
	[-0.209]	[-4.101]	[-4.680]	[-1.633]	[-13.683]	[-39.439]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.233	-0.112	0.039	-0.274**	1.071	1.180
	(0.267)	(0.297)	(0.164)	(0.118)	(0.668)	(1.264)
	[-0.209]	[-4.101]	[-4.680]	[-1.633]	[-13.683]	[-39.439]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	0.272	-0.163	0.023	-0.317***	0.868	0.679
	(0.229)	(0.287)	(0.174)	(0.100)	(0.652)	(0.805)
	[-0.209]	[-4.101]	[-4.680]	[-1.633]	[-13.683]	[-39.439]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample with occupations with a physical index indicator above the mean. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A7: Robustness: Effect of Pregnancy Accommodation Laws for 1 Standard Deviation Above Mean Physical Intensity Index

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	0.544	-0.356	0.041	-0.566**	0.292	-1.345
	(0.457)	(0.638)	(0.689)	(0.264)	(1.276)	(2.936)
	[-0.839]	[-3.825]	[-4.974]	[-1.889]	[-26.529]	[-61.078]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.440	-0.275	0.014	-0.802***	-0.423	-3.752**
	(0.527)	(0.523)	(0.357)	(0.210)	(1.150)	(1.874)
	[-0.839]	[-3.825]	[-4.974]	[-1.889]	[-26.529]	[-61.078]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	0.627*	-0.404	0.029	-0.884***	-0.866	-3.876***
	(0.353)	(0.382)	(0.312)	(0.142)	(0.852)	(1.087)
	[-0.839]	[-3.825]	[-4.974]	[-1.889]	[-26.529]	[-61.078]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample with occupations with a physical index indicator one standard deviation above the mean. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^{f} - Y_{st}^{m}$, where Y_{st}^{f} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^{m} is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A8: Robustness: Effect of Pregnancy Accommodation Laws for Married With Kids

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	-0.744***	1.066*	0.477	0.024	0.539	0.562
	(0.195)	(0.596)	(0.538)	(0.110)	(0.928)	(1.248)
	[2.367]	[-26.043]	[-25.106]	[-3.366]	[-23.940]	[-60.548]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	-0.084	1.678***	1.583***	0.050	1.551*	3.205**
	(0.158)	(0.396)	(0.383)	(0.068)	(0.888)	(1.269)
	[2.367]	[-26.043]	[-25.106]	[-3.366]	[-23.940]	[-60.548]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	-0.120	1.806***	1.741***	0.047	1.130	2.735**
	(0.123)	(0.315)	(0.331)	(0.075)	(0.826)	(1.229)
	[2.367]	[-26.043]	[-25.106]	[-3.366]	[-23.940]	[-60.548]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample of individuals married with children. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A9: Robustness: Effect of Pregnancy Accommodation Laws for Married Without Kids

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	-0.089	-1.075**	-1.228**	0.009	-3.082**	-3.105
	(0.611)	(0.525)	(0.500)	(0.179)	(1.364)	(2.015)
	[0.921]	[-6.878]	[-6.422]	[-1.192]	[-10.606]	[-25.608]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.106	-0.482	-0.480	-0.160	-2.506*	-2.832
	(0.247)	(0.454)	(0.418)	(0.105)	(1.313)	(1.807)
	[0.921]	[-6.878]	[-6.422]	[-1.192]	[-10.606]	[-25.608]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	0.034	-0.465	-0.500	-0.241***	-3.496***	-4.377***
	(0.249)	(0.406)	(0.334)	(0.084)	(1.107)	(1.361)
	[0.921]	[-6.878]	[-6.422]	[-1.192]	[-10.606]	[-25.608]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample of individuals married without children. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A10: Robustness: Effect of Pregnancy Accommodation Laws for Single With Kids

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	0.364	-1.172	-0.910*	-0.614*	-2.406	-3.602
	(0.762)	(0.906)	(0.509)	(0.359)	(1.845)	(3.124)
	[1.270]	[-9.991]	[-9.973]	[-0.968]	[-13.718]	[-33.830]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	0.392	-1.010	-0.747	-0.189	-1.211	-2.049
	(0.410)	(0.672)	(0.585)	(0.206)	(1.191)	(1.854)
	[1.270]	[-9.991]	[-9.973]	[-0.968]	[-13.718]	[-33.830]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	0.333	-0.829*	-0.583	-0.129	-1.972***	-2.713***
	(0.372)	(0.434)	(0.358)	(0.171)	(0.646)	(0.811)
	[1.270]	[-9.991]	[-9.973]	[-0.968]	[-13.718]	[-33.830]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample of individuals single with children. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y^f_{st} - Y^m_{st}$, where Y^f_{st} is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y^m_{st} is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A11: Robustness: Effect of Pregnancy Accommodation Laws for Single Without Kids

	(1) Unemployment	(2) EPOP	(3) LFP	(4) Wks. Worked	(5) Log(Hourly Wage)	(6) Log(Earnings)
Panel A: Callaway and Sant'Anna						
Treated x Post	0.103	0.348	0.461	-0.210	1.202	-0.704
	(0.393)	(0.416)	(0.587)	(0.182)	(0.996)	(1.428)
	[-3.137]	[2.604]	[0.263]	[-0.011]	[-6.850]	[-17.271]
Observations	720	720	720	720	720	720
Panel B: Two-Way Fixed Effects						
Treated x Post	-0.027	-0.207	-0.272	-0.375***	0.635	-0.818
	(0.187)	(0.262)	(0.219)	(0.117)	(0.440)	(0.973)
	[-3.137]	[2.604]	[0.263]	[-0.011]	[-6.850]	[-17.271]
Observations	760	760	760	760	760	760
Panel C: Borusyak Et al.						
Treated x Post	-0.007	-0.243	-0.289	-0.385***	0.648	-0.817
	(0.184)	(0.302)	(0.231)	(0.105)	(0.396)	(0.716)
	[-3.137]	[2.604]	[0.263]	[-0.011]	[-6.850]	[-17.271]
Observations	720	720	720	720	720	720
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from estimating Equation 1 using data at the state-year level from ACS survey years 2001 to 2019 using alternative difference-in-difference specifications on the sample of individuals single without children. Each entry represents a separate regression with the dependent variable specified in the column. All models use the triple differences specification, with a dependent variable of $Y_{st} = Y_{st}^f - Y_{st}^m$, where Y_{st}^f is the person-weighted mean of the outcome variable for female respondents living in state s at time t, while Y_{st}^m is the mean for male respondents. Panels A, B, and C present results using males age 18-44 as a control group. Panel A presents results using the method from Callaway and Sant'Anna (2021). Panel B presents results using a standard two-way fixed effects regression. Panel C presents results using the method from Borusyak et al. (2024). The treatment group's average of the dependent variable in event time $\tau = -1$ is displayed in brackets. All coefficients, with the exception of those from the weeks worked regression, have been multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.