The Health Effects of Paid Family Leave

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Abstract

This paper examines the effects of New Jersey's Paid Family Leave (NJ-PFL) program on maternal and infant health and whether these effects vary by race and access to job protection. Using restricted-use natality data from the National Vital Statistics System for 2006–2011, we estimate event-study and difference-in-differences designs comparing New Jersey with neighboring states before and after the program's implementation in 2009. NJ-PFL increased prenatal care utilization by 0.58 visits and reduced preterm delivery by 0.54 percentage points, or 4.2% relative to the pre-policy mean. Point estimates suggest larger improvements in prenatal care and preterm delivery among non-Hispanic Black mothers, as well as larger reductions in pregnancy-associated hypertension. Estimated benefits are also greater in areas with higher potential job-protection coverage, although the racial and geographic differences are imprecisely estimated under conservative inference. These findings highlight the heterogeneous effects of PFL on maternal and infant health and the potential importance of combining wage replacement with job protection to promote equitable access to leave.

1 Introduction

Stark racial and ethnic disparities in maternal and infant health in the United States have persisted for decades despite continued advances in medical care. Pregnancy-related mortality rates among American Indian and Alaska Native (AIAN) and Black women are more than three times higher than those of White women (63.4 and 55.9 vs. 18.1 per 100,000). Black, AIAN, and Native Hawaiian or Pacific Islander (NHPI) women also experience higher rates of preterm birth, low birthweight, and late or no prenatal care compared to White women (Hill et al., 2022).[1] These gaps at birth persist across the life course. As Currie (2011) demonstrates, early-life health disparities endure well into adulthood and can extend across generations, shaping patterns of inequality long after birth.

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[1] Appendix A describes racial disparities in birth risk factors in detail.

One reason for persistent racial disparities may be limited access to paid leave among Black mothers due to greater structural barriers. Survey evidence indicates that 26% of Black adults reported needing to take time off but were unable to do so (compared to 13% of White adults). "Could not afford to lose more wages or salary" (69%) and "risk of losing their job" (47%) are the two most commonly cited reasons for not taking leave (Pew Research Center, 2017). Moreover, the 2018 Family and Medical Leave Act survey shows that, when taking leave, Black mothers are far more likely to receive no pay at all (45%, compared to 27% among White mothers) (Women's Bureau, U.S. Department of Labor, 2023).

The introduction of Paid Family Leave (PFL) could serve as a potential lever to address persistent racial health disparities. In this paper, I examine whether PFL affects disparities in maternal health during pregnancy and infant health at birth. PFL is a state-level, employee payroll–funded program that provides workers with paid time off to bond with a new child or to care for a seriously ill family member, and most claims are filed by mothers after childbirth. Rossin-Slater et al. (2013) finds that after California's first-in-the-nation PFL program, average maternity leave increased from 1 to 7 weeks for Black mothers and from 4 to 7 weeks for White mothers. By increasing expected income and guaranteeing leave after childbirth, PFL may reduce financial stress during pregnancy—especially for Black mothers, who are more likely to be primary wage earners in their households (61% are single-mother breadwinners vs. 21% among White mothers) and face tighter budget constraints around childbirth (Anderson, 2016). Lower financial stress and better prenatal conditions might also lead to more engagement with prenatal care, fewer risky behaviors, and better fetal development, ultimately resulting in healthier birth outcomes. Prior research also shows that both paid and unpaid maternity leave improve maternal health and infant outcomes (Rossin, 2011; Stearns, 2015).

Despite a growing literature documenting the effects of U.S. PFL policies on parental leave-taking, women's labor market outcomes, and postnatal child health (Rossin-Slater et al., 2013; Das and Polachek, 2015; Baum and Ruhm, 2016; Byker, 2016; Bartel et al., 2018; Stanczyk, 2019; Bailey et al., 2019; Bana et al., 2020), much less is known about their implications for maternal health during pregnancy and birth outcomes, as PFL benefits do not take effect until after childbirth. Research focused on racial disparities is even more limited. This paper fills this gap by examining how New Jersey's affects maternal health during pregnancy, infant health outcomes at birth, and the differential racial effects that may either widen or reduce existing disparities. Using natality data from the National Vital Statistics System (NVSS) from 2006 to 2011, I analyze key infant health indicators—birthweight, gestational age, preterm delivery, and Apgar scores—along with maternal outcomes such as prenatal care utilization and pregnancy-associated hypertension. My primary focus is on racial and ethnic disparities, comparing outcomes between non-Hispanic White and non-Hispanic Black mothers, as these health gaps reflect broader underlying social and economic inequities. In addition, I use marital status as a supplementary proxy for socioeconomic background. To estimate the effects of NJ-PFL, I employ event-study and difference-in-differences (DD) models, comparing birth outcomes in New Jersey to those in neighboring states before and after the policy's implementation in the first quarter of 2009.

NJ-PFL is the first state program to combine PFL with additional job protection beyond the provisions of the FMLA. To understand how this added job protection shapes maternal and infant health outcomes, I exploit geographic heterogeneity in county-year job protection coverage within a triple-difference (DDD) framework. Specifically, I construct county-year measures of the proportion of people eligible for job protection using data from the Current Population Survey's Outgoing Rotation Group (CPS-ORG) and the Annual Social and Economic Supplement (CPS-ASEC).

My results indicate that NJ-PFL improved maternal care utilization and infant health by encouraging mothers to obtain more prenatal care and reducing preterm delivery. The policy increased prenatal care utilization by 0.58 visits, or 5.4% relative to the pre-policy mean, and reduced preterm births by 0.54 percentage points, or 4.2%. The prenatal-care estimate remains significant under wild cluster bootstrap inference ($p = 0.059$), while the reduction in preterm delivery remains significant at the 5% level ($p = 0.048$). The preterm-delivery result is consistent with previous research: Stearns (2015) find a 6.6% reduction in preterm births following the introduction of temporary disability insurance, while Rossin (2011) finds a 3% reduction following the implementation of the FMLA. Scaling the intention-to-treat estimate by the calibrated take-up rate of 45.2% implies a treatment-on-the-treated reduction of approximately 9.3% in preterm delivery among mothers who use NJ-PFL. This scaled estimate inherits the statistical uncertainty of the underlying intention-to-treat estimate. NJ-PFL is also associated with a 1.2-percentage-point reduction in pregnancy-associated hypertension, equivalent to approximately 33% of the pre-policy mean. Although economically meaningful and significant using conventional state-clustered standard errors, the estimate is not significant under wild cluster bootstrap inference ($p = 0.378$) and should therefore be viewed as suggestive. Its magnitude exceeds estimates reported in earlier studies of maternal health policies, although the statistical uncertainty precludes a definitive comparison.[2]

The racial subgroup estimates suggest larger improvements in prenatal care and preterm delivery among non-Hispanic Black mothers than among non-Hispanic White mothers. Prenatal visits increased by 0.771 visits (8.1%) among Black mothers, compared with 0.440 visits (4.0%) among White mothers. Preterm delivery declined by 1.13 percentage points (6.7%) among Black mothers, compared with 0.08 percentage points among White mothers. The estimates also suggest a larger reduction in pregnancy-associated hypertension among Black mothers: 2.10 percentage points (43.2%), compared with 0.68 percentage points among White mothers. However, these subgroup estimates do not remain statistically significant under wild cluster bootstrap inference, and the differences between Black and White mothers are not statistically conclusive.

The triple-difference estimates similarly suggest greater health improvements in areas with higher potential access to job-protected leave, particularly among Black mothers, but these estimates are imprecise under conservative inference. Taken together, the results provide evidence that NJ-PFL increased prenatal care utilization and robust evidence that it reduced preterm delivery in the full sample. The larger point estimates among Black mothers

[2]For example, Gardner (2024) find that targeted regulations of abortion providers, which restrict abortion access in the United States, increase hypertensive disorders of pregnancy by 11–16%.

and in areas with greater access to job protection are suggestive rather than conclusive, but they highlight the importance of examining heterogeneous policy effects and considering job protection alongside wage replacement when designing equitable leave policies.

This paper contributes to the prior literature in several ways. First, it extends prior research on parental leave policies and maternal and infant health outcomes. The analysis demonstrates that job-protected maternity leave provided under PFL can improve maternal health during pregnancy and infant health at birth by increasing income security and ensuring job protection for new mothers. These health gains are especially pronounced among Black mothers—particularly those who are unmarried. Prior research examining maternity leave policies under different institutional settings similarly suggests that impacts vary substantially by socioeconomic background. Under unpaid leave, as provided by the Family and Medical Leave Act (FMLA), Rossin (2011) finds that reductions in infant mortality are concentrated among college-educated and married mothers. By contrast, when leave is paid, disadvantaged groups appear to benefit most: Stearns (2015) shows that unmarried and Black mothers experience the largest reductions in abnormal deliveries.

Second, this paper broadens the scope of evidence on the health impacts of PFL by examining changes in outcomes that occur immediately before the benefits become available. Prior literature has focused primarily on postnatal infant health, when benefits begin to take effect. For example, Pihl and Basso (2019) find that PFL reduces infant hospitalizations for avoidable conditions, and Bullinger (2019) report improvements in child health and maternal mental health after birth. The study most closely related to this one is Oloomi (2016), who also use natality data to study California's PFL program and find improvements in infant health only among mothers above age 35, driven by reduced delays in first childbirth, with no detectable effects for younger mothers. However, these studies either do not examine heterogeneous effects across racial groups or do not find discernible differential impacts. This paper contributes by first examining disparities in access to PFL and then analyzing heterogeneous impacts across racial groups.

Third, studying NJ-PFL offers new insights into how differences in PFL program parameters—specifically, job protection—may contribute to differential changes in infant and maternal health outcomes. Nearly all prior research on PFL in the U.S. focuses on the first-in-the-nation program in California.[3] New Jersey is the second state to adopt PFL after California. The two programs are broadly similar in the generosity of financial support and the duration of leave, especially in their initial years. For example, California provides up to 55% of average weekly earnings (capped at $603) for 6 weeks, while New Jersey provides 6 weeks of leave with up to two-thirds of average weekly earnings (capped at $524). A key distinction is that New Jersey administers NJ-PFL together with the New Jersey Family Leave Act (NJ-FLA), which provides additional job protection during leave, beyond the federal FMLA, which primarily covers an employee's own medical condition (approximately 50%). By showing that health gains are concentrated in counties with high job protection

[3] Rossin-Slater et al. (2013); Huang and Yang (2015); Das and Polachek (2015); Baum and Ruhm (2016); Byker (2016); Lichtman-Sadot and Bell (2017); Bartel et al. (2018); Bullinger (2019); Pihl and Basso (2019); Stanczyk (2019); Bailey et al. (2019); Bana et al. (2020).

coverage, I provide suggestive evidence that job protection may be a critical factor affecting mothers' ability to take PFL.

Fourth, I apply the NJ-PFL take-up estimate developed in Chapter 1 to scale the intention-to-treat effects (ITT) from the difference-in-differences and event-study analyses. Under the assumption that the policy affects outcomes only through actual program participation, this adjustment converts the policy-level estimates into approximate average treatment effects on treated mothers (ATT). This approach provides a more direct measure of NJ-PFL's effects among program users.

Lastly, this paper contributes to the extensive literature on infant inequality and intergenerational mobility. Inequality at birth has lifetime implications (Aizer and Currie, 2014; Currie, 2011; Chetty et al., 2014). A growing body of research shows that inequalities in early life experiences contribute to the persistence of poor socioeconomic status and rising economic inequality in the United States. Studies show that PFL improves job continuity for women and benefits maternal and child health. By promoting equality among children from all socioeconomic backgrounds, PFL has the potential to mitigate the rise of inequality, foster long-term economic prosperity and stability, and consequently foster broader social and economic equity in the United States (Rossin-Slater and Stearns, 2020).

2 Backgrounds

2.1 Parental Leave Policies in the US

The U.S. is an outlier compared to other high-income countries across the globe as it does not guarantee paid maternal, parental, or family leave at a national level. Instead, the U.S. has a patchwork of federal, state, and local parental policies. Since 2004, thirteen states and the District of Columbia have PFL. While the extent of PFL benefits varies across nations, most developed countries provide their mothers at least nine months of paid leave. For instance, in Germany and Canada, mothers can receive up to a year of paid leave, with up to 67 percent and 55 percent of their weekly wages covered, respectively.

Before Paid Family Leave became available, mothers primarily relied on two existing parental leave policies to take time off around childbirth. At the federal level, the Family and Medical Leave Act of 1993 is the only nationwide parental leave policy in the United States. It provides eligible employees with twelve weeks of *unpaid* job-protected leave to care for a newborn, a recently adopted child, a sick family member, or for their own serious medical condition.[4] Due to stringent eligibility requirements—employees must have worked at least 1,250 hours for an employer with 50 or more employees in the preceding year—almost thirty years after its passage, only 56% of private-sector workers are eligible for FMLA. According to a 2018 survey, nearly half of FMLA claims are for an employee's own health condition, and only 27% are for maternity or paternity leave (Women's Bureau, U.S. Department of Labor, 2023).

[4]Employees may take FMLA leave all at once or, when medically necessary, in separate blocks of time or by reducing the number of hours worked each day or week.

At the state level, Temporary Disability Insurance (TDI) offers another avenue through which mothers can obtain leave during pregnancy and after childbirth. TDI provides workers with short-term partial wage replacement benefits when they are unable to work due to illness or injury. In 1978, TDI was expanded to include normal pregnancy as a qualifying condition. Currently, only five states in the United States have TDI programs—California, Hawaii, New Jersey, New York, and Rhode Island. Standard TDI leave for pregnancy typically begins four weeks before the due date and continues for six weeks postpartum. According to New Jersey claims reports, only around 25% of claims are for female employees bonding with a new child following childbirth.

In addition to the fragmented and limited access to maternity leave in the United States, Black mothers have less access to paid leave than White mothers. Figure 1 illustrates racial disparities in paid leave access. In the 2018 FMLA survey, respondents were asked whether they received full pay, partial pay, or no pay when taking leave. Nearly half (approximately 45%) of Black mothers reported receiving no pay at all, compared to 27% of White mothers. This is particularly concerning given that Black mothers are more likely to come from economically disadvantaged backgrounds and may have a greater need for financial support around childbirth. Anderson (2016) show that 61% of Black mothers are single-mother breadwinners, compared to 21% of White mothers. White mothers are also more likely to be married and non-breadwinners than Black mothers (50% vs. 19%).

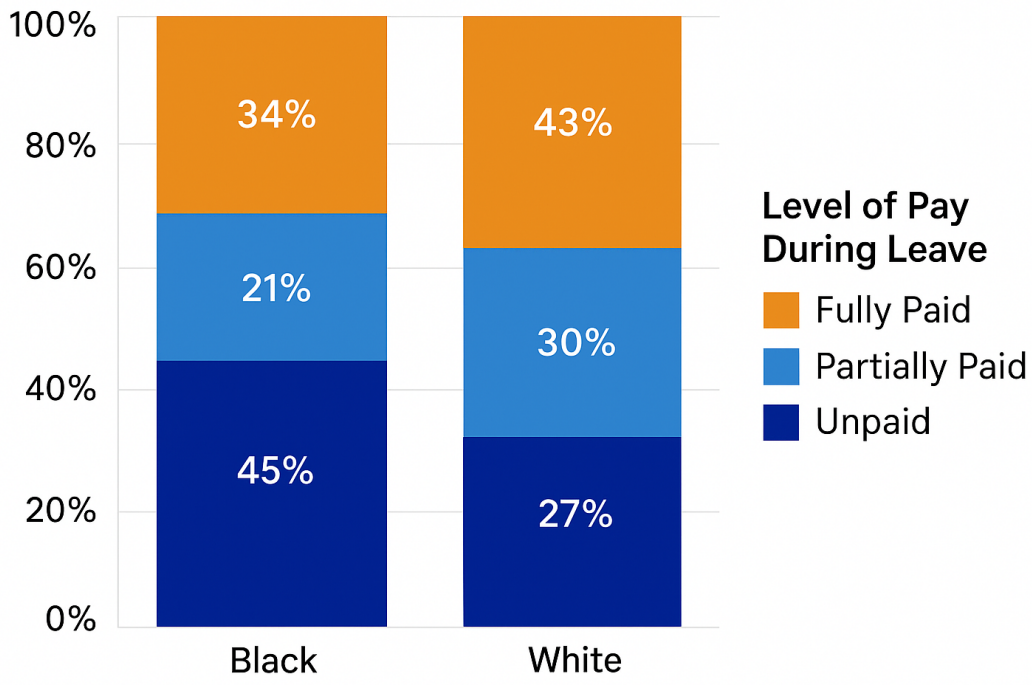


Figure 1: Percentage Taking Paid or Unpaid Family & Medical Leave

Notes: Partial pay includes those receiving partial pay or combining paid and unpaid time during leave. Data from the 2018 Family and Medical Leave Act Survey.

2.2 PFL in the US

When California introduced PFL in 2004, it became the first state to provide new parents with paid time off from work to spend time with their children (hereafter, "bonding") or to care for a seriously ill family member (hereafter, "caring"). In 2009, New Jersey followed suit and became the second state to implement a PFL program, providing up to six weeks of paid leave with benefits covering 67 percent of a worker's weekly earnings within a 12-month period. To qualify, an employee must either (1) have worked at least 20 weeks for a covered employer while earning no less than 20 times the minimum wage per week ($143 per week in 2009), or (2) have earned at least 1,000 times the minimum wage in total ($7,200 in 2009) during the one-year "Base Year" preceding the leave from a single employer.[5] The New Jersey PFL program is funded entirely through employee payroll deductions. Beginning January 1, 2009, employers were required to withhold 0.09% of an employee's wages and remit those funds to the New Jersey Department of Labor & Workforce Development to finance the program. The vast majority of claims in New Jersey are for bonding with a newborn (approximately 85%).[6]

Unlike California's PFL program, where leave-takers must rely on the more restrictive federal FMLA for job protection, New Jersey implements its own state-level *unpaid* job protection policy under the New Jersey Family Leave Act, providing additional support beyond the FMLA. In general, New Jersey employees are entitled to up to 12 weeks of job-protected leave within a 24-month period to bond with a new child if they work for a state or local government agency of any size, or for a private employer with at least 50 employees worldwide, and have been employed for at least one year with a minimum of 1,000 hours in the past 12 months. Another key difference is that the FMLA allows workers to take leave for their own serious medical condition, among other purposes, whereas the NJ-FLA, administered alongside NJ-PFL, applies only to leave taken for reasons other than one's own medical condition (e.g., bonding with a new child). Studying New Jersey's program not only provides new insights into whether PFL continues to maternal and infant health in a distinct political and economic context, but also clarifies how job protection interacts with PFL benefits.

As of 2022, thirteen states and the District of Columbia have PFL.[7] The length of the program typically ranges from six to twelve weeks, with up to 70 percent of the average weekly earnings provided. Figure 2 depicts the current status of PFL laws in the United States. Most of the state-run PFLs are funded entirely by employee payroll taxes. PFL can be thought of as a natural extension of TDI. First, most TDI states (i.e., CA, NY, NJ & RI) allow mothers to claim PFL immediately after claiming TDI, facilitating a smooth transition from TDI to PFL. Second, the cash benefits provided by TDI are very similar to that of PFL. Most mothers combine the PFL and TDI to maximize their duration of paid

[5]The "Base Year" refers to the 52 weeks immediately preceding the week in which family leave begins. For workers with multiple jobs, leave must be taken from only one employer.

[6]Data from the 2020 NJPFL report from the New Jersey Department of Labor & Workforce Development.

[7]CA (2004), NJ (2009), RI (2014), NY (2018), DC (2020), WA (2020), MA (2021), CT (2022), VA(2022), OR (2023), NH(2023), CO (2024), MD (2025), DE (2026). Figure 2 depicts the current status of state-PFL in the U.S.

maternity leave. Bana et al. (2020) show that conditional on taking PFL, 82% of mothers also take TDI.

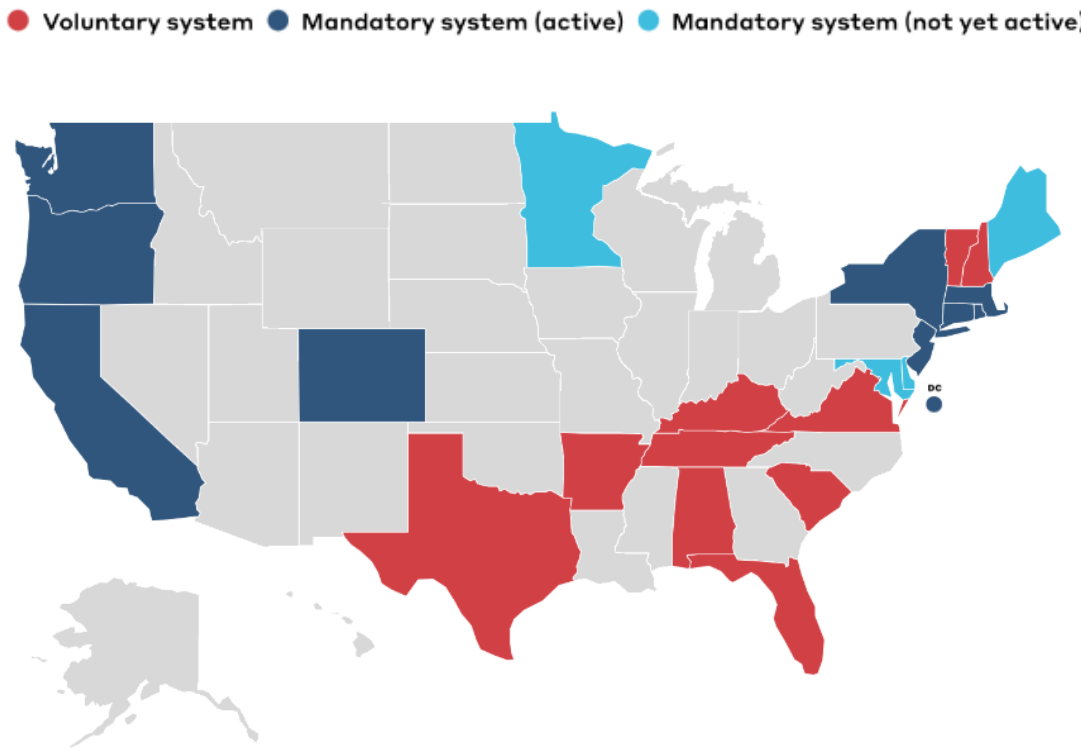


Figure 2: 2025 Status of PFL Laws
Source: Bipartisan Policy Center, 2025

2.3 Pathways Linking Paid Family Leave and Infant Health Disparity

2.3.1 Effects on Maternal and Infant health

Higher expected income paired with job-protected leave under NJ-PFL may alleviate financial and job-related stress during pregnancy, supporting healthier pregnancies and births. There is strong evidence consistent with this channel. Chatterji and Markowitz (2005) shows longer maternity leave reduces the frequency of depressive sympotom. Bullinger (2019) documents that California's PFL program improves maternal mental health outcomes. Bütikofer et al. (2021) shows the paid maternity leave in Norway improved a range of maternal health outcomes, including BMI, Blood pressure, pain and mental health. Additionally, Chung et al. (2016) finds that the anticipation of cash transfers can improve birth outcomes, leading to modest increases in birth weight and reductions in the incidence of low birth weight.

There are several additional pathways through which PFL may affect maternal health during pregnancy and infant health at birth. Reduced stress may encourage healthier be-

haviors during pregnancy. Bütikofer et al. (2021) show that the introduction of paid family leave in Norway increased exercise and reduced smoking, behaviors that can improve infant health outcomes (McGeary et al., 2020; Conway and Kutinova, 2006; Barreca and Page, 2015). Moreover, by reducing financial and job-related stress during pregnancy, PFL may directly improve infant health at birth. Existing research establishes that in-utero exposure to maternal stress adversely affects fetal development and birth outcomes. For instance, Currie et al. (2022) show that prenatal exposure to assaults significantly increases the risk of adverse birth outcomes.[8]

Prior studies on broader parental leave policies find that access to maternity leave—both paid and unpaid—improves maternal and infant health. Ruhm (2000) examines 16 European countries and finds that short, job-protected paid parental leave reduces perinatal and neonatal mortality compared to no paid leave. Rossin (2011) documents lower infant mortality rates and a reduced incidence of premature birth following the introduction of unpaid maternity leave under the FMLA. Similarly, Stearns (2015) finds reductions in low birth weight and preterm births associated with access to paid maternity leave through TDI.

2.3.3 Heterogeneous Effects by Race

The health impacts of PFL may vary across socioeconomic and racial or ethnic groups. Anderson (2016) show that 61% of Black mothers are single-mother breadwinners, compared to 21% of White mothers. they ofentime face tighter budget constraint, especially considering the high cost around childbirth. as a result, the fincial support provided by PFL might exert larger impact among balck mothers, resulting in a reduction in racial health diaprity between black and white mothers.

Lower barrier could mean greater relative gain. Fore example, after Rossin-Slater et al. (2013) finds that after California's first-in-the-nation PFL program, average maternity leave increased from 1 to 7 weeks for Black mothers and from 4 to 7 weeks for White mothers. Both black mothers take 7 weeks of leave after Califrnoia PFL, but the relative gain is a lot larger for black mothers we may also expect a higher health relative gain.

Prior research on broader on parenyal leave policy indicate financial support might be a criticle factor affecting its helath impact across racail groups, Rossin (2011) finds that when leave is uncompensated, as under the FMLA, reductions in infant mortality are concentrated among children of college-educated and married mothers, likely because these mothers are better able to afford unpaid time off. By contrast, Stearns (2015) shows that when leave is compensated, as under TDI, unmarried and Black mothers experience more pronounced reductions in preterm and low-birth-weight deliveries.

2.3.2 Barriers to take-up

[8] A large body of literature documents the relationship between in-utero conditions and infant health. Adverse outcomes may result from poor drinking water quality (Currie and Rossin-Slater, 2015) and extreme heat exposure (Kim et al., 2021). Extensive public health research also documents associations between prenatal maternal mental health and birth outcomes (Torche, 2011; Menclova and Stillman, 2020; Aizer and Currie, 2014).

Passing a paid leave law does not guarantee that workers are able to access or take leave, and barriers to access may be particularly acute for low-wage workers, workers of color, and other marginalized groups. On the intensive margin, mothers who take leave may benefit substantially; however, on the extensive margin, disproportionately fewer of them may be able to take time off in the first place.

First, the eligibility criteria may be disproportionately more challenging for mothers from racial minority groups. New Jersey's PFL program requires employees to work for the same employer for at least 20 weeks in the base year, which inherently creates barriers for mothers from low-SES backgrounds. These individuals are more likely to work in jobs characterized by unpredictable schedules, low wages, and high turnover. Indeed, studies consistently find that PFL claimants from the lowest-income levels are significantly underrepresented (Bana et al., 2018; Mason, 2019).

Second, the lack of job protection is another critical barrier that disproportionately affects workers from low-SES backgrounds. A survey conducted by the Heldrich Center for Workforce Development at Rutgers University's Bloustein School of Planning and Public Policy found that half of New Jersey workers were hesitant to take leave due to concerns about job security, seniority, or career advancement.[9] These concerns were more prevalent among workers earning less than $100,000 compared to those earning more—67% versus 40% feared job loss, while 65% versus 40% worried about losing seniority or advancement opportunities. Interestingly, Black mothers are more likely to be eligible for job protection under NJ-FLA, as they tend to work longer hours on average (74% eligible versus 69% for White mothers).

Third, financial barriers also pose a major impediment to leave-taking. Schneider and Harknett (2022) analyzed a national sample of workers and find that half of those who experienced a qualifying event—such as the birth of a child, a personal health need, or a caregiving responsibility—did not take any leave from their job. Among these workers, nearly three-quarters (71%) cited an inability to afford leave as the primary reason. Lastly, low awareness and lack of knowledge about PFL benefits are especially prevalent among low-wage workers (Appelbaum and Milkman, 2015; Goodman et al., 2020), further limiting the reach of PFL among low-income individuals.

2.3.4 Changes in Composition of Birth

The financial support from PFL may affect infant health disparities by altering fertility decisions and the demographic composition of births, as families respond differently to the cost of childbearing. Lower-income families are likely more responsive to PFL benefits due to tighter liquidity constraints and greater reliance on income replacement.

According to the quantity–quality trade-off model (Becker, 1960; Jones et al., 2008;

[9] Heldrich Center for Workforce Development. *New Jersey's Earned Sick Leave Law and Family Leave Insurance Program: Measuring the Awareness and Opinions of New Jersey Workers, October 2020 to October 2021*. Rutgers University, May 2022. https://heldrich.rutgers.edu/wp-content/uploads/2022/05/New_JerseyE28099s_Earned_Sick_Leave_Law_and_Family_Leave20Insurance_Program.pdf

De La Croix and Doepke, 2003), wealthier families typically prioritize investing in children's "quality"—such as education and health—over having additional children. While PFL reduces the direct financial burden of childbirth, high-wage women face greater opportunity costs of time away from work, making additional childbearing relatively more expensive. Moreover, higher-income families often face higher baseline expenses, such as housing and education costs, which reduce the marginal impact of universal benefits like PFL (Becker and Lewis, 1973).

Empirical evidence supports these differential responses. González and Trommlerová (2023) find that when Spain eliminated its universal child benefit, fertility declined most sharply among younger and lower-skilled couples, indicating that financial incentives play a greater role for resource-constrained families. Similarly, Byker (2016) shows that after the implementation of PFL, low-skill women are more likely to remain with their employers around childbirth rather than exit the labor force, underscoring their stronger financial dependence on paid leave benefits.

Consequently, the aggregate effects of PFL on infant health depend on how birth outcomes vary across socioeconomic groups. If PFL primarily enables low-income or low-skill mothers to maintain income stability and access prenatal care, overall infant health may improve as children born to disadvantaged mothers become healthier. However, if the policy induces a compositional shift toward births among more disadvantaged mothers—whose infants tend to have poorer health outcomes—the average health of all newborns could decline, widening health disparities across socioeconomic groups. Therefore, the net effect of PFL on infant health disparity remains ambiguous.

3 Data

3.1 Birth Data

To examine changes in birth and pregnancy outcomes, I use the restricted-use version of the U.S. universal birth records from 2006 to 2011, obtained from the National Vital Statistics System (NVSS), covering three years before and after the implementation of PFL. It provides detailed individual-level demographic information of the mothers, including their age, race, marital status, state and county of residency, the degree of education. Additionally, the birth records include details of infant health information such as birth weight, gestational age, birth order, and the month when prenatal treatment started. I limit the study sample to mothers aged 18–44, the typical reproductive age range. I further restrict the sample to births with gestational ages between 28 and 40 weeks to focus on viable, term pregnancies. I exclude post-term births (beyond 40 weeks) because they are relatively rare and often reflect distinct medical or management factors. I also exclude observations with missing values for key outcomes—birth weight, number of prenatal visits, maternal weight gain during pregnancy, Apgar score, or pregnancy-associated hypertension—which account for 9.5% of total births (approximately 240,543 observations). The final analytic sample comprises 2,294,815 births.

Table 1 reports summary statistics for selected birth outcomes and demographic

characteristics from the NVSS data for New Jersey and its bordering states (New York, Pennsylvania, and Delaware) before and after the implementation of NJ-PFL. Asterisks denote statistically significant differences between the pre- and post-policy periods. I classify births as preterm when the gestational age falls below 37 weeks. Gestational age and the preterm delivery rate both decline over time in New Jersey and its neighboring states.

New Jersey mothers show a lower likelihood of being diagnosed with pregnancy-associated hypertension (hereafter, P-A hypertension). I group maternal race into three categories: non-Hispanic White, non-Hispanic Black, and non-Hispanic other races. Most mothers hold high school diplomas. Compared with mothers in bordering states, those in New Jersey are less likely to be non-Hispanic White and more likely to be married and have college degrees.

3.2 CPS Data and Job Protection Measure

To examine whether access to job-protected leave modifies the effects of NJ-PFL, I use the county-year measure of potential job-protection coverage developed in Chapter 1. The county-year job protection measure is constructed using the CPS ASEC–ORG, which provides information on workers' demographic characteristics, employment, and firm size. Under the NJFLA, workers are eligible for job-protected leave only if they are employed by firms with at least 50 employees.[10] I apply this statutory firm-size criterion to employed women aged 18–44 in every county-year of both New Jersey and the bordering control states, and estimate the share of workers who would satisfy the NJFLA firm-size requirement. Counties with above-median estimated coverage are classified as high job-protection counties, while those below the median are classified as low job-protection counties. Applying a common eligibility rule across treatment and control states provides a consistent measure of potential access to job-protected leave that can be incorporated into the triple-difference design. The analysis in this chapter uses this county-year measure to examine whether the effects of NJ-PFL differ across areas with relatively high and low levels of potential job protection.

The county-year measure should be interpreted as a proxy for potential access to job-protected leave rather than a direct measure of individual eligibility. Although the measure is based on the statutory NJFLA firm-size threshold, it does not imply that workers

[10]This firm-size threshold reflects the NJFLA eligibility rules in effect during the study period.

Table 1: Summary Statistics

	Full Sample			NJ			NY, PA & DE		
	Pre	Post		Pre	Post		Pre	Post	
Birthweight in grams	3267.029	3265.843	*	3257.826	3258.598		3269.585	3267.792	**
Gestation in weeks	38.636	38.690	***	38.597	38.667	***	38.647	38.696	***
Preterm delivery	0.122	0.115	***	0.127	0.117	***	0.121	0.114	***
P-A hypertension	0.034	0.035	*	0.031	0.030	**	0.035	0.036	***
Low birth weight (<2500 g)	0.084	0.083	***	0.086	0.084	**	0.083	0.083	**
5-min apgar score	8.892	8.894	**	8.924	8.921	**	8.883	8.887	***
Month prenatal care began	3.158	3.137	***	2.833	2.786	***	3.162	3.142	***
Mom <20 yrs old	0.076	0.067	***	0.063	0.055	***	0.080	0.070	***
Mom 20-24 yrs old	0.204	0.198	***	0.170	0.166	***	0.214	0.206	***
Mom 25-29 yrs old	0.268	0.271	***	0.257	0.257		0.271	0.275	***
Mom 30-34 yrs old	0.263	0.279	***	0.294	0.309	***	0.255	0.270	***
Mom 35-39 yrs old	0.151	0.147	***	0.174	0.169	***	0.145	0.141	***
Mom 40-44 yrs old	0.035	0.036	***	0.038	0.040	***	0.033	0.035	***
Mom 45-49 yrs old	0.002	0.003	***	0.003	0.003	***	0.002	0.003	***
Mom <HS education	0.183	0.182	***	0.133	0.030	***	0.190	0.184	***
Mom has HS education	0.817	0.818	***	0.867	0.970	***	0.810	0.816	***
Mom has some college	0.561	0.583	***	0.609	0.880	***	0.555	0.579	***
Mom has > college degree	0.396	0.409	***	0.721	0.762	***	0.393	0.404	***
Mom is non-Hispanic white	0.499	0.535	***	0.465	0.443	***	0.508	0.560	***
Mom is non-Hispanic black	0.124	0.155	***	0.152	0.149	***	0.116	0.156	***
Mom is Hispanic	0.199	0.201	***	0.258	0.263	***	0.183	0.184	**
Mom is married	0.608	0.597	***	0.658	0.645	***	0.594	0.584	***
Observations	3,107,102			666,929			2,440,173		

Notes: This table reports means of key variables from the Natality data. The analysis sample includes births to mothers residing in New Jersey and its bordering states between Q1 2006 and Q4 2011. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

in neighboring states are eligible for the NJFLA. Instead, it measures the share of workers who would satisfy the NJFLA firm-size criterion if the law were applied uniformly across counties. The measure also does not capture other determinants of individual eligibility, such as tenure or hours worked. Moreover, because geographic variation in firm-size distributions may reflect broader differences in local labor markets, counties with higher estimated job-protection coverage may also differ in industry composition, employer practices, or workforce characteristics. Consequently, the measure should be interpreted as capturing relative differences in potential access to job-protected leave rather than actual individual coverage.

4 Empirical Strategy

Previous research has documented substantial racial disparities in maternal and infant health outcomes, as well as heterogeneous effects of public policies across racial groups (Hill et al., 2022; Kuziemko et al., 2018). To account for these pre-existing differences, I estimate the models separately for non-Hispanic White and non-Hispanic Black mothers, the two largest racial groups in the sample. This approach allows the effects of NJ-PFL to differ across groups with historically distinct baseline health outcomes and responses to policy interventions.

To measure the effects of access to PFL on birth outcomes, I leverage the state-year variation in PFL event-study and difference-in-difference models. The estimating equation of the difference-in-difference design takes the following form:

$$Y_{isct} = \alpha_0 + \alpha_1 PFL_{st} + \delta' X_{ct} + \xi' W_{ct} + \theta_s + \rho_t + \epsilon_{isct} \tag{1}$$

for individual i residing in state s, county c in calendar year-quarter t. PFL_{st} is an indicator set to 1 for state-quarters in which PFL exists, and 0 otherwise. In this case, PFL_{st} equals 1 for New Jersey after the Q1, 2009. Control states include the bordering states of New Jersey, including New York, Pennsylvania, and Delaware.

NJPFL was enacted on May 2, 2008, with benefits becoming available on July 1, 2009. I define the post-policy period to begin with births in the first quarter (Q1) of 2009, the earliest birth cohort for which mothers who conceived around the policy announcement would have been exposed to the policy throughout nearly their entire pregnancy. Consequently, any changes in birth outcomes attributable to the policy are expected to emerge beginning with births in Q1 2009. All births occurring during the study period are included in the analysis regardless of whether the mother was eligible for or utilized NJ-PFL, as neither eligibility nor program participation is observed in the natality data. Accordingly, the estimated coefficient, α_1, should be interpreted as an intent-to-treat effect. By comparing all births before and after policy implementation, the ITT framework avoids bias arising from selective eligibility or program take-up and captures the differential change in birth outcomes in New Jersey relative to bordering states following the introduction of NJ-PFL.

The vectors X_{ct} and W_{ct} include time-varying county-level demographic and economic characteristics. Demographic controls include the share of mothers aged 25 and older

with a college degree and the share of mothers who are non-Hispanic White. Economic controls are obtained from the Bureau of Economic Analysis (BEA) Regional Economic Information System (REIS) and include per capita personal income, government transfer payments, and unemployment insurance compensation.[11] Additional controls include the county–month unemployment rate from the Local Area Unemployment Statistics (LAUS), to capture local labor market conditions affecting mothers' return-to-work decisions, and county–quarter maternal demographics (e.g., education and race) from natality data to capture local compositional changes over time. I also include calendar year–quarter fixed effects (θ_t) to account for aggregate time trends and state fixed effects (ρ_s) for time-invariant state differences. Standard errors are clustered at the state level to allow for within-state correlation. [12]

I also estimate a corresponding event-study model:

$$Y_{isct} = \beta_0 + \sum_{k=-6, k\neq -1}^{k=11} \pi_k \mathbb{1}[t - PFL_{st} = k] + \gamma' X_{ct} + \Lambda' W_{ct} + \theta_t + \rho_s + \epsilon_{isct} \tag{2}$$

for individual i residing in state s, county c in calendar year-quarter t. Y_{isct} is the outcome of interests, such as gestational age, preterm delivery rates, and pregnancy-associated hypertension (Later, Hyper.). The event-time indicators, $\mathbb{1}[t - PFL_{st} = k]$, reflect the quarter relative to NJ-PFL adoption and are set to 0 in all years for states without PFL throughout the study period (i.e, 2006-2011). All the other variables are the same as in equation (1).

A key identifying assumption underlying both the DD and event-study analyses is that, absent the implementation of NJ-PFL, maternal and infant health outcomes in New Jersey would have evolved similarly to those in neighboring states. In other words, the parallel trends assumption requires that no unobserved factors differentially affected outcomes in New Jersey at the same time that PFL was introduced. Violations of this assumption would lead the DD estimates to capture not only the effects of PFL but also other contemporaneous changes.

The placebo tests reported in Section 6 and the event-study estimates from Equation (2) suggest that the parallel trends assumption may not hold equally well across all outcomes and demographic groups, even after controlling for observable maternal characteristics. Moreover, the DD specification identifies the average effect of policy availability rather than actual exposure to the program. As documented in Chapter 1, although eligibility for NJ-PFL is widespread, only a fraction of eligible mothers ultimately utilize the

[11] Government transfer payments include income maintenance programs (e.g., Supplemental Security Income (SSI), the Earned Income Tax Credit (EITC), and the Supplemental Nutrition Assistance Program (SNAP)), medical benefits (e.g., Medicare and military health care), and retirement and disability payments. All dollar amounts are expressed in 2020 dollars, deflated using the CPI.

[12] Results are similar when restricting the comparison to New Jersey and Pennsylvania (see Figure C.1 and Tables A1–A3).

program. Consequently, the DD estimates may understate policy effects if many mothers remain effectively untreated despite residing in a treated state.

To address these concerns and examine whether access to job-protected leave modifies the effects of NJ-PFL, I estimate a triple-difference specification that exploits geographic variation in potential access to job-protected leave. I use the county-year measure of potential job-protection coverage developed in Chapter 1, which is constructed uniformly for county-year cells in both New Jersey and the neighboring control states by applying the NJFLA firm-size eligibility criterion. This provides a common proxy for potential access to job-protected leave across treatment and control areas.

The underlying premise is that mothers with greater access to job-protected leave are more likely to utilize paid family leave because they can take leave without facing the risk of job loss or employment disruption. If job protection facilitates paid leave utilization, the effects of NJ-PFL should be stronger in counties with greater potential access to job-protected leave. The DDD specification therefore tests whether the effects of NJ-PFL differ systematically between counties with relatively high and low levels of potential job-protection coverage while differencing out common shocks across states, time, and county types.

Specifically, I estimate the following DDD model.

$$\begin{aligned} Y_{iscy} = \alpha_0 &+ \alpha_1 Post_y + \alpha_2 Treatment_s + \alpha_3 HighProtection_{cy} \\ &+ \alpha_4 (Post_y \times Treatment_s) \\ &+ \alpha_5 (Post_y \times HighProtection_{cy}) \\ &+ \alpha_6 (Treatment_s \times HighProtection_{cy}) \\ &+ \alpha_7 (Post_y \times Treatment_s \times HighProtection_{cy}) \\ &+ \delta' X_{icy} + \xi' W_{cy} + \theta_s + \rho_y + \epsilon_{iscy}. \end{aligned} \tag{3}$$

In this specification, $Post_y$ and $Treatment_s$ denote the post-policy period and New Jersey, respectively. $HighProtection_{cy}$ is an indicator equal to one if individual i resides in a county-year in which the estimated share of employed women aged 18–44 working for firms meeting the NJFLA firm-size requirement is above the median, and zero otherwise.

The measure of $HighProtection_{cy}$ is constructed using CPS ASEC–ORG data for all county-year cells in New Jersey and the neighboring control states over the study period. For counties outside New Jersey, the indicator does not represent actual eligibility under the NJFLA; rather, it measures the share of workers who would satisfy the NJFLA firm-size criterion if the law applied. Applying the same statutory criterion to both treatment and control states isolates heterogeneity arising from differences in local firm-size distributions rather than differences in state-specific job-protection laws.

The coefficient on the triple interaction, α_7, measures whether the effect of NJ-PFL differs between counties with relatively high and low potential job-protection coverage. Specifically, it estimates the difference between the NJ-PFL treatment effect in high-

protection counties and the corresponding treatment effect in low-protection counties. A positive (negative) estimate of α_7 therefore indicates that the effect of NJ-PFL is larger (smaller) in counties with greater potential access to job-protected leave.

Because both high- and low-protection counties are exposed to NJ-PFL, the lower-order interaction, α_4, captures the estimated treatment effect of NJ-PFL in low-protection counties, while the sum of the lower-order interaction and the triple interaction, $\alpha_4 + \alpha_7$, captures the corresponding treatment effect in high-protection counties. Accordingly, the triple-interaction coefficient should be interpreted as the incremental effect associated with greater potential access to job-protected leave, rather than as the total treatment effect in high-protection counties.

Since PFL policies are implemented at the state level, all standard errors are clustered at the state level throughout the analysis. However, inference based on conventional cluster-robust standard errors may be unreliable when the number of clusters is limited. As a robustness check, I therefore conduct wild cluster bootstrap inference following the procedure proposed by Cameron et al. (2008). Specifically, I compute wild cluster bootstrap p-values using 9,999 bootstrap replications with Webb weights while maintaining clustering at the state level. The bootstrap procedure affects only statistical inference—the estimated coefficients remain unchanged—and provides more reliable p-values in settings with a relatively small number of clusters. Throughout the paper, the reported coefficient estimates are identical to those from the baseline regressions, while the wild cluster bootstrap results are used as an additional robustness check for statistical significance.

5 Results

5.1 Effects on Maternal Pregnancy Outcomes

Table 2 reports the estimated effects of NJ-PFL on maternal pregnancy outcomes based on Equation (1). Columns (1) and (2) include only state and year–quarter fixed effects; columns (3) and (4) add time-varying county-level economic controls; and columns (5) and (6) further include county-level demographic controls. These sequential specifications assess the sensitivity of the estimates to local economic conditions and changes in population composition. Conventional state-clustered standard errors are reported in brackets, together with wild cluster bootstrap p-values to account for the small number of state clusters.

The estimates change substantially when economic controls are introduced. In columns (1) and (2), the estimated effects on prenatal visits and pregnancy-associated hypertension are small and statistically insignificant. After controlling for county-level economic conditions, NJ-PFL is associated with 0.51 additional prenatal visits and a 1.06-percentage-point reduction in pregnancy-associated hypertension. Both estimates are significant at the 1% level using conventional state-clustered standard errors, although neither is significant under wild cluster bootstrap inference ($p = 0.159$ and $p = 0.263$, respectively). This sensitivity suggests that contemporaneous county-level economic conditions are important for isolating the estimated policy effects and cautions against interpreting the preferred

Table 2: Effects of PFL on Pregnancy Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre. Visits	Hyper.	Pre. Visits	Hyper.	Pre. Visits	Hyper.
Post×Treated (PFL)	-0.114	-0.0925	0.507***	-1.061***	0.575***	-1.194***
	[0.0633]	[0.262]	[0.0748]	[0.171]	[0.0914]	[0.127]
Log Per Capita Income, Dollars			-0.374***	-0.554	0.00508	-0.760
			[0.0531]	[0.262]	[0.105]	[0.331]
Log Per Capita Transfers, Dollars			-1.532	13.30**	-2.398	13.60**
			[1.661]	[3.208]	[1.716]	[3.964]
Log Per Capita Unemp.Compen.			0.828**	-1.009	0.706	-1.004
			[0.241]	[0.731]	[0.301]	[0.635]
Log SSI			-2.000	1.139	-2.289	1.793
			[1.278]	[0.977]	[1.208]	[1.056]
Log EITC, $1000			-3.056**	-0.651	-2.652*	-0.652
			[0.851]	[0.804]	[0.921]	[1.026]
Log SNAP, $1000			-1.714	-0.00126	-1.953	0.358
			[1.177]	[1.185]	[1.135]	[1.144]
Log Other Cash Trans.			-5.996*	1.346	-5.677*	2.164
			[1.920]	[2.478]	[2.183]	[2.877]
Log Medical			0.0857	-10.45**	1.011	-10.49**
			[1.065]	[2.046]	[1.462]	[2.661]
Log Retirement			0.299	-1.628**	-0.224	-1.354**
			[0.191]	[0.439]	[0.120]	[0.394]
Unemployment Rate			-0.116*	-0.102	-0.126**	-0.110
			[0.0405]	[0.0441]	[0.0327]	[0.0475]
Share Women aged 25+ with College degree					-0.411*	0.172
					[0.168]	[0.495]
Share non-Hispanic White					1.437***	-1.628*
					[0.222]	[0.556]
Wild bootstrap p-val.	0.466	0.686	0.159	0.263	0.059	0.378
Dept. var mean	10.61	3.581	10.61	3.581	10.61	3.581
Demo. Control					Yes	Yes
Econ. Control			Yes	Yes	Yes	Yes
YQ. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. obs.	2294815		2294815		2294815	

Notes: This table reports the α_1 coefficients from equation (1) for the number of prenatal visits and pregnancy-associated hypertension (×100), using the analysis sample from the NCHS birth certificate data. The sample includes births delivered between Q1 2006 and Q4 2011 to mothers aged 18–44. The unit of observation is a birth. Economic controls are measured at the county–year level from the BEA's REIS and include per capita income, transfers from governments and businesses, unemployment insurance compensation, income maintenance transfers (e.g., SSI, EITC, and SNAP), medical benefits (Medicare and military health care), and retirement and disability payments. Dollar amounts are expressed in 2020 dollars and deflated using the CPI. Additional controls include the county–year–month unemployment rate from the LAUS and county–quarter measures of maternal demographics from the natality data, including the share of mothers aged 25 or older with a college degree and the share of births to non-Hispanic White mothers. The key independent variable is PFL, which equals 1 for births in Q1 2009 or later. The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for the estimated effect of PFL. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.

specification without considering the full set of results.

Adding demographic controls in columns (5) and (6) produces comparatively modest changes, increasing the estimates to 0.58 additional visits and a 1.19-percentage-point reduction in hypertension. I prefer this specification because it accounts for both local economic conditions and changes in maternal composition. The estimated increase in prenatal visits represents a 5.4% increase relative to the sample mean of 10.61 and remains significant under wild cluster bootstrap inference ($p = 0.059$). The estimated reduction in hypertension corresponds to a 33.3% decline relative to its sample mean but is not significant under the wild bootstrap ($p = 0.378$). Thus, the results provide suggestive evidence that NJ-PFL increased prenatal care utilization, whereas the evidence of reduced pregnancy-associated hypertension is less conclusive. Estimates for both outcomes remain sensitive to the inclusion of covariates.

Table 3 reports estimates separately for non-Hispanic White and non-Hispanic Black mothers, hereafter referred to as White and Black mothers. The point estimates suggest larger improvements among Black mothers. NJ-PFL increased prenatal visits by 0.771 visits (8.1 percent) for Black mothers, compared with 0.440 visits (4.0 percent) for White mothers. Pregnancy-associated hypertension declined by 2.098 percentage points (43.2 percent) among Black mothers and by 0.680 percentage points among White mothers. Although all estimates are significant using conventional state-clustered standard errors, none remains significant under wild cluster bootstrap inference, with p -values ranging from 0.138 to 0.480. The subgroup results therefore suggest larger benefits among Black mothers but remain statistically inconclusive under conservative inference.

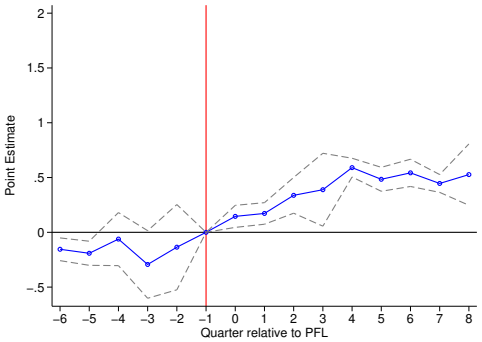
Figures 3–4 present event–study coefficients with 95% confidence intervals from equation (2) for maternal outcomes. The regressions include the full set of county-level economic and demographic controls from columns (5) and (6) of Table 2. Coefficients are normalized to the quarter immediately preceding NJ–PFL. Panel (a) shows results for the full sample, while panels (b) and (c) report subgroup estimates for White and Black mothers, respectively. For prenatal visits (Figure 3), pre-policy coefficients fluctuate around zero, indicating no clear pretrend. After PFL, estimates turn positive and rise steadily, consistent with the DD results. In the full sample, prenatal visits increase by roughly 0.3–0.6 visits within 2–6 quarters, with gains persisting thereafter. The pattern for White mothers is similar but smaller in magnitude, whereas for Black mothers the increase is substantially larger—about 0.7–1.0 additional visits by 4–7 quarters. Although the confidence intervals are wider in the Black subgroup due to smaller sample sizes and the additional disaggregation by quarter, the upward trend in prenatal visits is clear.

Figure 4 shows event–study estimates for pregnancy-associated hypertension. Post-policy, coefficients drift downward. The full sample and White mothers exhibit modest declines of about 0.5–1.5 percentage points over 3–7 quarters, while Black mothers experience much larger reductions of roughly 2–4 percentage points by 6–8 quarters. Although the confidence intervals for Black mothers are wider, they generally exclude zero after the early post-policy period, indicating improvements in this subgroup.

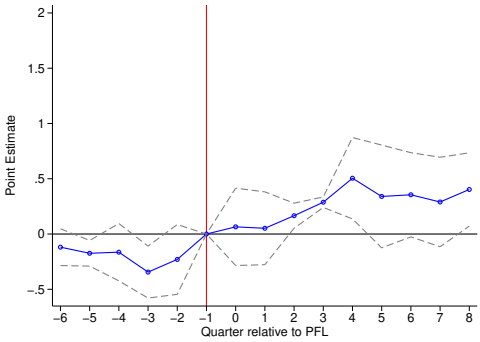
Table 3: DD Estimates of PFL Effects on Pregnancy Outcomes by Race

	No.Pre.Visits			P-A Hyper(×100)		
	(1) All	(2) White	(3) Black	(4) All	(5) White	(6) Black
Post×Treated (PFL)	0.575***	0.440**	0.771***	-1.194***	-0.680**	-2.098***
	[0.0914]	[0.189]	[0.180]	[0.127]	[0.270]	[0.365]
Wild bootstrap p-val.	0.059	0.377	0.138	0.378	0.480	0.356
Dept. var mean	10.61	11.03	9.555	3.581	3.73	4.858
Indiv. obs.	2294815	1180618	281611	2294815	1180618	281611
Econ. Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demo. Controls	Yes	Yes	Yes	Yes	Yes	Yes

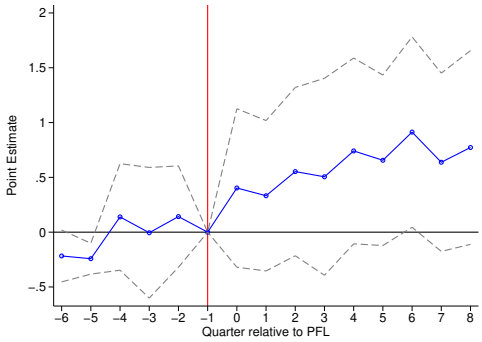
Notes: This table reports DD estimates for pregnancy outcomes using the full analysis sample (columns (1) and (4)) and separately for White mothers (columns (2) and (5)) and Black mothers (columns (3) and (6)). See the notes to Table 2 for details on the data, sample, and specification. Each regression includes the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. "No.Pre.Visits" denotes the number of prenatal visits, and "P-A Hyper" denotes pregnancy-associated hypertension (×100). The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for the estimated effect of PFL. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.



(a) All



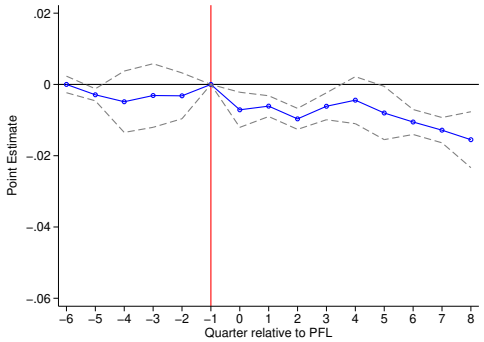
(b) non-Hispanic White



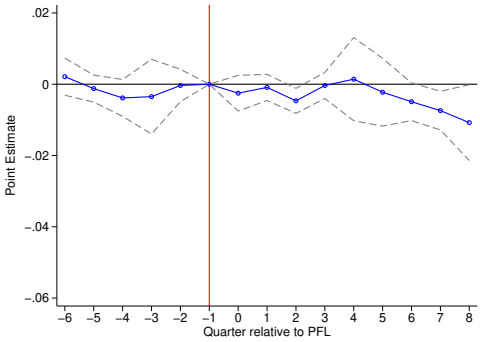
(c) non-Hispanic Black

Figure 3: Changes in No. Prenatal Visists

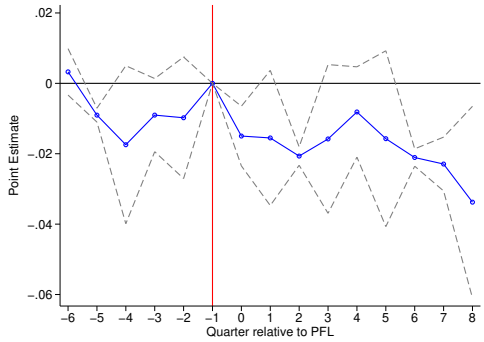
Notes: These figures plot event–study coefficients and 95% confidence intervals from estimating equation (2), using the full analysis sample (panel a) and subgroups of White (panel b) and Black (panel c) mothers, covering six quarters before and eight quarters after NJ-PFL implementation. All regressions include the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. The outcome is the number of prenatal visits. See notes under Table 2 for additional details on the data, sample, and specifications. Robust standard errors are clustered at the state level.



(a) All



(b) non-Hispanic White



(c) non-Hispanic Black

Figure 4: Changes in P-A Hypertension (×100)

Notes: These figures plot event–study coefficients and 95% confidence intervals from estimating equation (2), using the full analysis sample (panel a) and subgroups of White (panel b) and Black (panel c) mothers, covering six quarters before and eight quarters after NJ-PFL implementation. All regressions include the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. The outcome is the rate of pregnancy-associated hypertension (×100). See notes under Table 2 for additional details on the data, sample, and specifications. Robust standard errors are clustered at the state level.

Table 4: Effects of PFL on Infant Outcomes

	(1) Gest.	(2) Preterm	(3) Apgar	(4) Gest.	(5) Preterm	(6) Apgar	(7) Gest.	(8) Preterm	(9) Apgar
Post×Treated (PFL)	0.0146**	-0.231	-0.00206	0.00310	-0.349***	0.0578***	0.0202***	-0.536***	0.0497***
	[0.00584]	[0.158]	[0.0126]	[0.00658]	[0.0659]	[0.00647]	[0.00783]	[0.0719]	[0.00672]
Wild bootstrap p-val.	0.300	0.427	0.749	0.269	0.055	0.219	0.132	0.048	0.156
Dept. var mean	38.26	12.67	8.92	38.26	12.67	8.92	38.26	12.67	8.92
Demo. Control							Yes	Yes	Yes
Econ. Control				Yes	Yes	Yes	Yes	Yes	Yes
YQ. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. obs.		2294815			2294815			2294815	

Notes: This table reports DD estimates for infant health outcomes using the full analysis sample. See the notes to Table 2 for details on the data, sample, and specification. Each regression includes the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. "Gest." denotes gestational age, excluding late- and post-term births ($\geq$ 41 weeks) due to their higher perinatal risk. "Preterm" denotes the preterm delivery rate (×100) among births with a gestational age of less than 37 weeks. "Apgar" denotes the Apgar score, measured on a 0–10 scale. The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for the estimated effect of PFL. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.

5.2 Effects on Infant Health

Table 4 reports DD estimates for gestational age, preterm delivery, and Apgar scores. The specifications follow the same sequence as in the preceding analysis, with columns (7)–(9) including both county-level economic and demographic controls. Births delivered at or after 41 weeks are excluded from the gestational-age analysis because of their higher perinatal risk.

The estimated reduction in preterm delivery increases as controls are added, from 0.231 percentage points in the baseline specification to 0.349 percentage points with economic controls and 0.536 percentage points in the preferred specification. The preferred estimate represents a 4.2% decline relative to the sample mean and remains significant under wild cluster bootstrap inference ($p = 0.048$). By contrast, the preferred estimates indicate small increases of 0.0202 weeks in gestational age and 0.0497 points in Apgar scores, but neither remains significant under the wild bootstrap ($p = 0.132$ and $p = 0.156$, respectively). Thus, the strongest evidence indicates that NJ-PFL reduced preterm delivery, while improvements in the other infant outcomes remain suggestive.

Table 5 presents estimates separately for White and Black mothers. The estimated reduction in preterm delivery is considerably larger among Black mothers at 1.129 percentage points, or 6.7% relative to their sample mean, compared with 0.077 percentage points among White mothers. However, neither subgroup estimate is significant under wild cluster bootstrap inference ($p = 0.119$ for Black mothers and $p = 0.424$ for White mothers). The estimated effects on gestational age and Apgar scores are also generally positive for Black mothers but remain imprecise. Apgar scores increase by similar amounts for both groups, with the estimate for White mothers marginally significant under the wild bootstrap ($p = 0.063$). Overall, the subgroup estimates suggest potentially larger infant health gains

among Black mothers, particularly through reduced preterm delivery, but do not provide conclusive evidence of racial differences.

Table 5: DD Estimates of PFL Effects on Infant Outcomes by Race

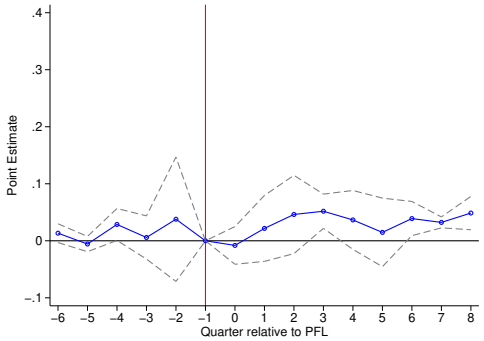
	Gest.			Preterm			APGAR		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	White	Black	All	White	Black	All	White	Black
Post×Treated (PFL)	0.0202***	-0.00609	0.0375	-0.536***	-0.077	-1.129***	0.0497***	0.0271***	0.0280
	[0.00783]	[0.0154]	[0.0261]	[0.0719]	[0.178]	[0.310]	[0.00672]	[0.00486]	[0.0181]
Wild bootstrap p-val.	0.132	0.652	0.302	0.048	0.424	0.119	0.156	0.063	0.151
Dept. var mean	38.26	38.36	37.97	12.67	11.32	16.79	8.925	8.926	8.852
Indiv. obs.	2294815	1180618	281611	2294815	1180618	281611	2294815	1180618	281611
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports DD estimates for infant health outcomes using the full analysis sample (columns (1) and (4)) and separately for White mothers (columns (2) and (5)) and Black mothers (columns (3) and (6)). See the notes to Table 2 for details on the data, sample, and specification. Each regression includes the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. "Gest." denotes gestational age, excluding late- and post-term births ($\geq$ 41 weeks) due to their higher perinatal risk. "Preterm" denotes the preterm delivery rate (×100) among births with a gestational age of less than 37 weeks. "Apgar" denotes the Apgar score, measured on a 0–10 scale. The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for the estimated effect of PFL. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.

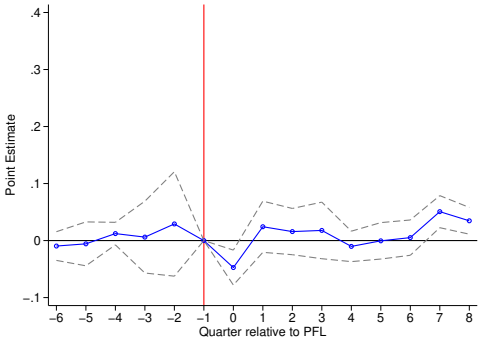
Figures 5 and 6 present the corresponding event-study estimates. For both outcomes, the pre-policy coefficients generally fluctuate around zero, providing no clear evidence of differential pretrends. Following implementation, gestational age tends to increase, while preterm delivery tends to decline. These changes are modest in the full sample and among White mothers but larger and less precisely estimated among Black mothers. In particular, the reduction in preterm delivery among Black mothers reaches approximately 3.5 percentage points four quarters after implementation, although the confidence intervals are wide. The event-study results are therefore consistent with the DD findings: NJ-PFL reduced preterm delivery in the full sample, with suggestive—but not conclusive—evidence of larger benefits among Black mothers.

5.3 Geographic Heterogeneity in Treatment Effects by Job Protection Levels

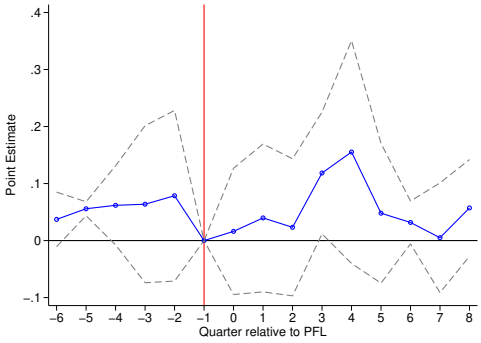
Figure 7 presents key results on infant health outcomes from my triple-difference analysis of how job protection interacts with PFL to generate geographic heterogeneity in program impacts. Specifically, this analysis focuses on four outcomes—birth weight, APGAR score, gestational age, and preterm delivery rate (×100)—with year-quarter coefficients estimated from the DDD model in Eq. (3). Each point represents the coefficient on the interaction between an indicator for a birth in a treatment state, an indicator for a birth in a likely eligible county-year-quarter under FLA job-protection provisions, and an indicator for the year-quarter on the x-axis. All controls and fixed effects listed in Eq. (3) are included in the regression used to estimate these coefficients. The results suggest that, after the introduction of NJPFL, there were positive effects on birth weight, APGAR scores, and gestational age, and a negative effect on the likelihood of preterm birth. Overall, these graphs provide



(a) All



(b) non-Hispanic White



(c) non-Hispanic Black

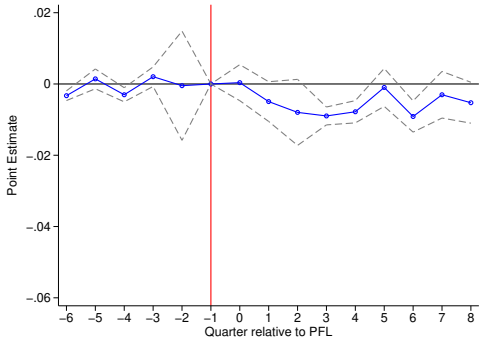
Figure 5: Changes in Gestational Weeks

Notes: These figures plot event–study coefficients and 95% confidence intervals from estimating equation (2), using the full analysis sample (panel a) and subgroups of White (panel b) and Black (panel c) mothers, covering six quarters before and eight quarters after NJ-PFL implementation. All regressions include the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. The outcome is gestational age in weeks, with births at or beyond 41 weeks excluded due to their higher perinatal risk. See notes under Table 2 for additional details on the data, sample, and specifications. Robust standard errors are clustered at the state level.

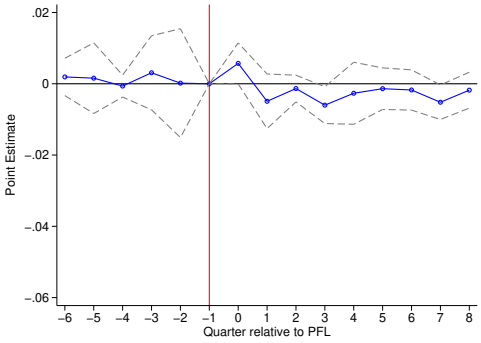
suggestive evidence that FLA provisions may have improved infant health for children of mothers who were likely affected by the policy.

Table 6 reports the DDD estimates of the effects of NJ-PFL on infant health outcomes for the full sample. As discussed in the previous section, the coefficient on $Post \times Treatment$ represents the estimated treatment effect in low-protection counties, while the corresponding effect in high-protection counties is obtained by adding the triple-interaction coefficient.

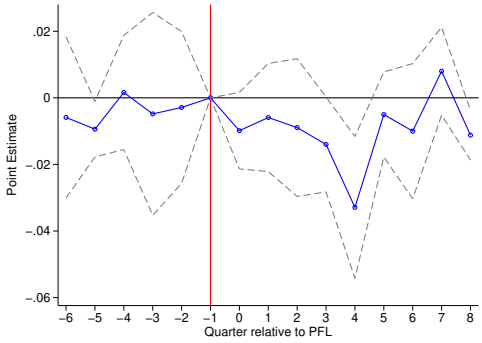
For gestational age (column 1), the estimated treatment effect is -0.065 weeks in low-protection counties and -0.013 weeks in high-protection counties, indicating that greater access to job-protected leave attenuates the decline in gestational age associated with NJ-PFL, although the implied treatment effect remains close to zero in high-protection counties. For preterm birth (column 2), the estimated treatment effect changes from a 0.404 percentage-point increase in low-protection counties to a 0.640 percentage-point reduction



(a) All



(b) non-Hispanic White



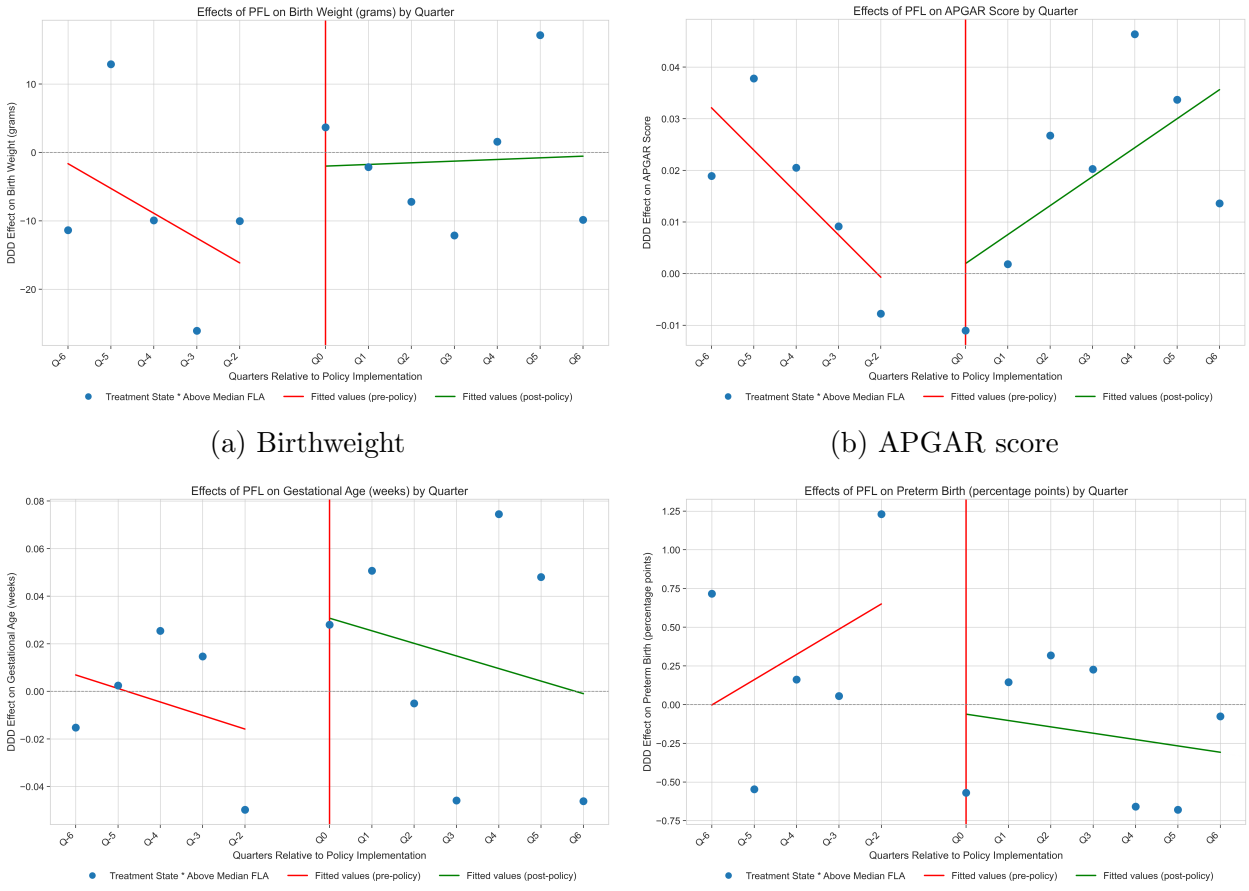
(c) non-Hispanic Black

Figure 6: Changes in Preterm Delivery Rate (×100)

Notes: These figures plot event–study coefficients and 95% confidence intervals from estimating equation (2), using the full analysis sample (panel a) and subgroups of White (panel b) and Black (panel c) mothers, covering six quarters before and eight quarters after NJ-PFL implementation. All regressions include the full set of county-level economic and demographic controls, as in columns (5) and (6) of Table 2. The outcome is the preterm delivery rate ($<$ 37 weeks) (×100). See notes under Table 2 for additional details on the data, sample, and specifications. Robust standard errors are clustered at the state level.

in high-protection counties, suggesting that the benefits of NJ-PFL are considerably stronger where potential access to job-protected leave is greater. In contrast, the estimated effects on APGAR scores (column 3) are modest in both county types (0.091 versus 0.073 points), providing little evidence that job-protection coverage modifies newborn condition at birth.

Overall, the DDD estimates indicate that heterogeneity by job-protection coverage is most pronounced for preterm birth. While the estimated effects on gestational age and APGAR scores differ little across county types, the reduction in preterm birth is concentrated in counties with greater potential access to job-protected leave, consistent with job protection enhancing the effectiveness of paid family leave.



(a) Birthweight

(b) APGAR score

(c) Gestational age

(d) Preterm birth rate

Figure 7: Triple-Difference Event-Study Estimates by Quarter

Notes: These figures plot the year-quarter DDD coefficients on the interaction between an indicator for a birth in New Jersey, an indicator for a birth after Q1 2009, and an indicator for a birth in a likely eligible county-year. The vertical line marks the policy implementation quarter. Pre- and post-policy fitted trends are shown for treated, high-eligibility counties. All controls and fixed effects listed in Eq. (1) are included.

Table 7 reports the DDD estimates separately for White and Black infants. For gestational age (columns 1–2), the point estimates suggest that greater potential access to job-protected leave attenuates the estimated decline for both groups. The treatment effect changes from −0.083 to −0.060 weeks for White infants and from −0.141 to 0.004 weeks for Black infants. However, neither triple-interaction estimate is significant under wild cluster bootstrap inference ($p = 0.135$ and $p = 0.315$, respectively). For preterm birth (columns 3–4), the point estimates indicate greater heterogeneity. The estimated treatment effect changes from a 0.397 percentage-point increase in low-protection counties to a 0.146 percentage-point reduction in high-protection counties among White infants. Among Black infants, it changes from a 1.330 percentage-point increase to a 0.678 percentage-point reduction. Although the triple interactions are significant using conventional standard errors, they are not significant under wild bootstrap inference ($p = 0.225$ for White infants and

Table 6: Triple-Difference Estimates of PFL Effects on Infant Outcomes (All Births)

	Gest. Age	Preterm	APGAR
	(1)	(2)	(3)
Post	0.164**	-1.494*	-0.032
	[0.076]	[0.896]	[0.034]
Treated	0.170***	-1.286**	0.228
	[0.015]	[0.613]	[0.163]
High Protection	0.005	-0.329**	-0.053
	[0.007]	[0.139]	[0.033]
Post×High Protection	-0.008	0.358***	0.050
	[0.005]	[0.113]	[0.035]
Post×Treated	-0.065***	0.404***	0.091***
	[0.010]	[0.146]	[0.035]
Treated×High Protection	-0.034***	0.691***	0.029
	[0.006]	[0.134]	[0.033]
Post×Treated×High Protection	0.052***	-1.044***	-0.018
	[0.006]	[0.135]	[0.035]
Wild bootstrap p-val.	0.130	0.155	0.682
Dept. var. mean	38.25	12.86	8.920
Observations	1,725,468	1,725,468	1,725,468
County Controls	Yes	Yes	Yes

Notes: This table reports triple-difference estimates of the effects of NJ-PFL on infant health separately for White and Black mothers. The specification follows equation (3). "Eligible" denotes $HighProtection_{cy}$, an indicator for counties with an above-median estimated share of employed women aged 18–44 working for firms that meet the NJFLA firm-size requirement. The coefficient on Post×Treated×Eligible is α_7, which measures the difference in the NJ-PFL effect between high- and low-protection counties. "Gest. Age" denotes gestational age, excluding late- and post-term births ($\geq$ 41 weeks). "Preterm" denotes the preterm delivery rate (×100) among births with a gestational age of less than 37 weeks. "APGAR" denotes the Apgar score, measured on a 0–10 scale. All regressions include county-level controls. The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for α_7. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.

$p = 0.384$ for Black infants). For Apgar scores (columns 5–6), the estimated treatment effects differ little across county types, and neither triple interaction is significant under wild bootstrap inference ($p = 0.433$ and $p = 0.392$). Overall, the point estimates suggest that greater potential job-protection coverage may strengthen the effects of NJ-PFL on gestational age and preterm birth, particularly among Black infants. However, the wild bootstrap results provide no statistically conclusive evidence of heterogeneity by potential job-protection coverage.

Table 7: Triple-Difference Estimates of PFL Effects on Infant Health by Race

	Gest. Age		Preterm		APGAR	
	(1)	(2)	(3)	(4)	(5)	(6)
	White	Black	White	Black	White	Black
Post	0.306***	0.191	-2.484**	-2.875	-0.00737	0.00539
	[0.0440]	[0.210]	[1.107]	[1.896]	[0.0509]	[0.120]
Treated	0.146**	0.131*	-0.0816	2.837**	0.122	-0.102
	[0.0268]	[0.0763]	[0.571]	[0.808]	[0.174]	[0.0963]
Eligible	0.00999	0.0475**	-0.243	-0.791*	-0.0636*	-0.0657*
	[0.0136]	[0.0126]	[0.284]	[0.284]	[0.0241]	[0.0226]
Post×Eligible	0.00620	-0.0813**	0.00507	1.391*	0.0623	0.0479*
	[0.00826]	[0.0143]	[0.186]	[0.459]	[0.0300]	[0.0153]
Post×Treated	-0.0825***	-0.141***	0.397	1.330***	0.103*	0.0752*
	[0.0104]	[0.0102]	[0.292]	[0.204]	[0.0328]	[0.0426]
Treated×Eligible	-0.00969	-0.103***	0.188	1.252**	0.0530	0.0572*
	[0.0118]	[0.0110]	[0.263]	[0.233]	[0.0237]	[0.0314]
Post×Treated×Eligible	0.0229*	0.145***	-0.543***	-2.008**	-0.0469	-0.0223
	[0.00792]	[0.0188]	[0.205]	[0.413]	[0.0332]	[0.0170]
Wild bootstrap p-val.	0.135	0.315	0.225	0.384	0.433	0.392
Dept. var mean	38.38	37.98	11.15	16.78	8.919	8.859
Indiv. obs.	776092	242208	776092	242208	776092	242208
County Control	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports triple-difference estimates of the effects of NJ-PFL on infant health from separate regressions for White and Black mothers. The specification follows equation (3). "Eligible" denotes $HighProtection_{cy}$, an indicator for counties with an above-median estimated share of employed women aged 18–44 working for firms that meet the NJFLA firm-size requirement. The coefficient on Post×Treated×Eligible is α_7, which measures the difference in the NJ-PFL effect between high- and low-protection counties within each racial subgroup. "Gest. Age" denotes gestational age, excluding late- and post-term births ($\geq$ 41 weeks). "Preterm" denotes the preterm delivery rate (×100) among births with a gestational age of less than 37 weeks. "APGAR" denotes the Apgar score, measured on a 0–10 scale. All regressions include county-level controls. The conventional state-clustered standard errors reported in brackets are less conservative. The row labeled "Wild bootstrap p-val." reports the more conservative wild cluster bootstrap p-values for α_7. Significance stars are based on the conventional state-clustered standard errors: * $p < .1$, ** $p < .05$, and *** $p < .01$.

6 Robustness Checks

6.1 Testing for Differential Time Trends in DD Analysis

A causal interpretation of the DD estimate relies on the assumption that there would have been no difference in outcomes between the treatment and control groups had the PFL policy not been implemented. If the assumption of parallel trends is not met, differences in pre-treatment trends could introduce biases into the DD estimate. In my analysis, the event-study permits visual inspection of pre-trends before the legislation period, and it provides indications that the parallel trends assumption might not hold for specific variables. To further assess whether pre-treatment trends in the outcome variables are comparable between New Jersey and other regions, I estimate the following model using observations for children i in state s and year-quarter t among children born before Q1, 2009, the pre-period of PFL:

$$Y_{ist} = \alpha_0 + \alpha_1 X_{ist} + \alpha_2 TR_t + \alpha_3 NONNJ_{ist} + \alpha_4 TR \times NONNJ_{ist} + \epsilon_{ist} \tag{4}$$

TR_{ist} is a linear time trend for the period ending in Q1 2009, and $NONNJ_{ist}$ is a set of dummy variables equal to zero for New Jersey and one for all other states. The coefficient of interest, $\hat{\alpha}_4$ indicates whether the pre-treatment time trend in the outcome variables differs between New Jersey and other states.

Table 8: Analysis of Pre-Law Trends in the Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Gest. Age	Preterm	P-A Hyper	Gest. Age	Preterm	P-A Hyper
Panel a: All	-0.00203	0.0104	-0.0158*	-0.00243*	0.0199	-0.0140
	[0.00131]	[0.0166]	[0.00884]	[0.00131]	[0.0166]	[0.00884]
Panel b: White	0.00194	-0.00830	-0.0220*	0.00324*	-0.0137	-0.00138
	[0.00176]	[0.0229]	[0.0131]	[0.00176]	[0.0229]	[0.0131]
Panel c: Black	-0.00558	0.146***	0.0172	-0.00365	0.122**	0.0281
	[0.00427]	[0.0505]	[0.0271]	[0.00430]	[0.0508]	[0.0273]
County control	No	No	No	Yes	Yes	Yes

Notes: This table reports estimates of α_4 from separate regressions based on Equation 4. Standard errors are shown in brackets. Statistical significance is indicated as follows: ***p<0.01, **p<0.05, *p<0.1

Table 8 presents results from estimating equation 4. I find that the parallel trend assumption holds well in general, although there is some evidence of violations in certain variables. Columns (1) through (3) display outcomes without the inclusion of county controls. Columns (4) through (6) show my preferred specification, with all county controls included.

Panel (a) of the table presents the findings among mothers of all race groups, where the gestational age coefficient is statistically significant, but preterm delivery rate and Pregnancy-associated hypertension (P-A Hyper) coefficients are not significant. Panel

(b) illustrates the estimates for white mothers, demonstrating possible violations of the parallel trend assumption for gestational age. Panel (c) shows statistically significant difference in pre-trend of preterm delivery rate among black mothers.

6.2 Synthetic Control

In addressing the differentrial time trends between NJ and its bordering states, I also implement a synthetic control estimation strategy (Abadie et al., 2010), which constructs a weighted combination of untreated units that closely matches the treated unit's pre-treatment trajectory. To apply this method, I assign weights to states based on their similarity to New Jersey in pre-treatment trends, aggregating data at the state-quarter level. The "donor pool"—or potential comparison states—includes all U.S. states except California, where PFL was already in effect when NJ-PFL was enacted. I match on statewide percentages of low birth weight births, percentages of married women, and racial composition derived from natality data. Additionally, I incorporate economic indicators such as median household income, the share of the population in poverty, and the unemployment rate from the March CPS and LAUS, for all quarter-years preceding NJ-PFL's implementation in Q1 2009.

In accordance with Abadie et al.'s (2010) approach, I choose a set of weights, W, to minimize the distance between New Jersey's characteristics, X_1, and those of the donor pool, X_0W, such that:

$$\|X_1 - X_0W\| \, \nu = \sqrt{(X_1 - X_0W)'V(X_0 - X_0W)} \tag{5}$$

Where V is a positive semidefinite and symmetric covariance matrix obtained by selecting weights of covariates to minimize the mean squared prediction error of the outcome variables during the pre-law period. The final weights of the states, W, add up to one, resulting in a "synthetic New Jersey" that is the weighted average of donor pool states. This method guarantees that the "synthetic New Jersey" has pre-treatment trends in the outcome variables that closely resemble those of New Jeresy over the same period, after conditioning on the additional covariates. This formalized procedure reduces the "ad-hoc" nature of choosing the control group by making explicit the contribution of each comparison unit to the counterfactual of interest.

Appendix B presents the detailed results of the synthetic control analysis. The SC estimates indicate that the introduction of NJ-PFL is associated with improvements in several key pregnancy and infant health outcomes. In particular, I find evidence of longer gestational age, reductions in preterm deliveries, and a decline in pregnancy-associated hypertension, with these improvements most evident in the first nine quarters after implementation. The results are robust to leave-one-out analyses, which show that the findings are not driven by any single donor state, and to placebo tests, which confirm that the estimated effects for New Jersey are unusually large compared to placebo effects in other states. Taken together, the SC evidence reinforces the main conclusions from the DD and event-study analyses: NJ-PFL improved maternal and infant health outcomes overall. At the same time, the heterogene-

ity analysis suggests that these improvements were not evenly distributed across groups. Changes in pregnancy and infant health outcomes are primarily driven by non-Hispanic Black mothers.

6.3 Addressing Potential Bias from Commuting Flow

A potential concern is cross-state commuting: some NJ residents may work in neighboring states (especially New York), so treatment assignment based on residence can misclassify exposure and attenuate DD estimates. We address this in several ways. First, alongside our baseline comparison of New Jersey to all bordering states (NY, PA, DE), we re-estimate using Pennsylvania as the sole control state (see Appendix C for detailed results). This substitution helps reduce heterogeneity in the control group, since Pennsylvania is economically and demographically closer to New Jersey than New York and did not implement its own PFL during the study period. As shown in Appendix Figure C.1, key birth outcomes in NJ and PA exhibit broadly similar pre-trends. Moreover, excluding New York is valuable given the high volume of commuting from northern New Jersey into New York City, which raises the risk of contamination if many NJ residents are employed across the border. At the same time, this adjustment does not fully resolve the misclassification problem: NJ residents who live in the state but work in non-PFL states (such as New York or Pennsylvania) are still coded as "treated" in the data even though they are not directly exposed to NJPFL. Thus, while focusing on PA as the sole control likely reduces spillover bias and improves comparability, it cannot eliminate the underlying limitation. Overall, the main results are robust to changes in the set of control states.

7 Conclusion

This paper examines the effects of NJ-PFL on maternal care utilization and maternal and infant health using data from the National Vital Statistics System, administrative PFL claims, and the Current Population Survey. Employing difference-in-differences, event-study, and triple-difference designs, I find that NJ-PFL increased prenatal care utilization by 0.58 visits and reduced preterm delivery by 0.54 percentage points, or 4.2% relative to the pre-policy mean. Both estimates are statistically significant under the more conservative wild cluster bootstrap procedure, which accounts for the small number of state-level clusters ($p = 0.059$ and $p = 0.048$, respectively). The reduction in preterm delivery is consistent with previous research: Stearns (2015) finds a 6.6% reduction following the introduction of temporary disability insurance, while Rossin (2011) finds a 3% reduction following the implementation of the FMLA. NJ-PFL is also associated with reduced pregnancy-associated hypertension and improvements in gestational age and Apgar scores, although these findings do not remain significant under wild cluster bootstrap inference.

The racial subgroup estimates suggest larger improvements in prenatal care and preterm delivery among non-Hispanic Black mothers than among non-Hispanic White mothers. Prenatal visits increased by 8.1% among Black mothers, compared with 4.0% among White mothers, while preterm delivery declined by 6.7% among Black mothers but changed little among White mothers. The estimated reduction in pregnancy-associated hypertension

is also larger among Black mothers. However, these subgroup estimates are imprecise under wild cluster bootstrap inference and do not provide conclusive evidence of racial differences in policy effects.

Because NJ-PFL provides wage replacement but not job protection, the triple-difference analysis examines whether its health effects vary with potential job-protection coverage under the NJFLA. The point estimates suggest greater improvements in gestational age and preterm delivery in areas with higher potential NJFLA coverage, particularly among Black mothers, while differences in Apgar scores are small. However, the triple-interaction estimates are imprecise under conservative inference. The findings are therefore consistent with job protection strengthening the health benefits of paid leave but do not definitively establish this mechanism.

These distributional patterns align with previous research. Rossin (2011) finds that the health benefits of unpaid leave under the FMLA are concentrated among children of college-educated and married mothers, whereas Stearns (2015) finds larger improvements among unmarried and Black mothers when leave is compensated through temporary disability insurance. My findings add suggestive evidence that wage replacement may generate greater health benefits for historically disadvantaged populations.

Overall, this study provides evidence that NJ-PFL increased prenatal care utilization and robust evidence that it reduced preterm delivery, along with suggestive evidence of broader improvements in maternal and infant health. The larger point estimates among Black mothers and in areas with greater potential access to job protection highlight the importance of examining heterogeneous policy effects. As more states implement or expand PFL programs, pairing wage replacement with job protection may be important for ensuring that workers can use leave and share equitably in its potential health benefits.

A Racial Disparities in Birth Risk Factors

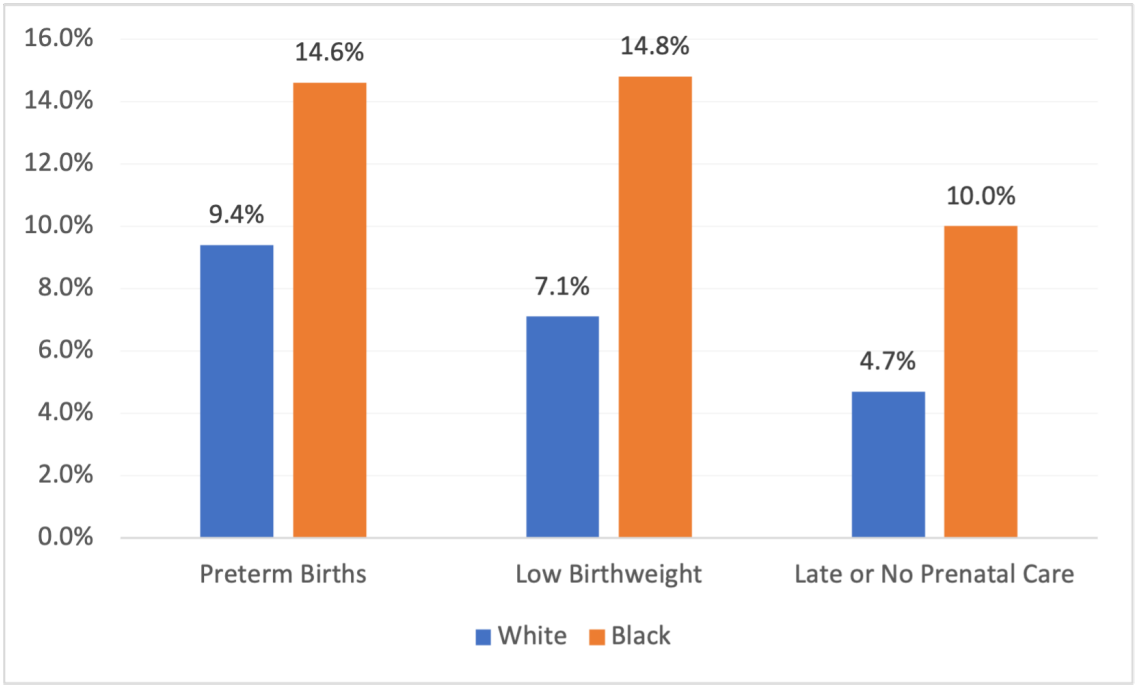
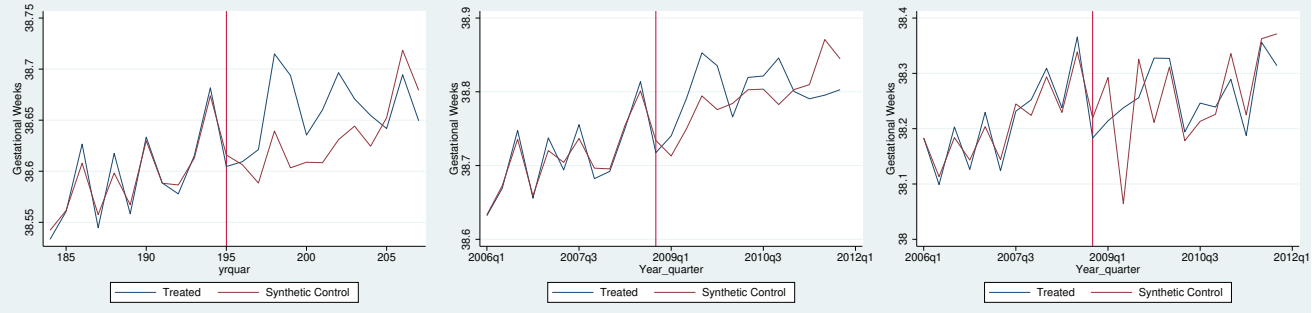


Figure A.1: Birth Risk Factors by Race, 2022
[1]Data from the 2022 National Vital Statistics Reports. Differences are statistically significant at the 5% level

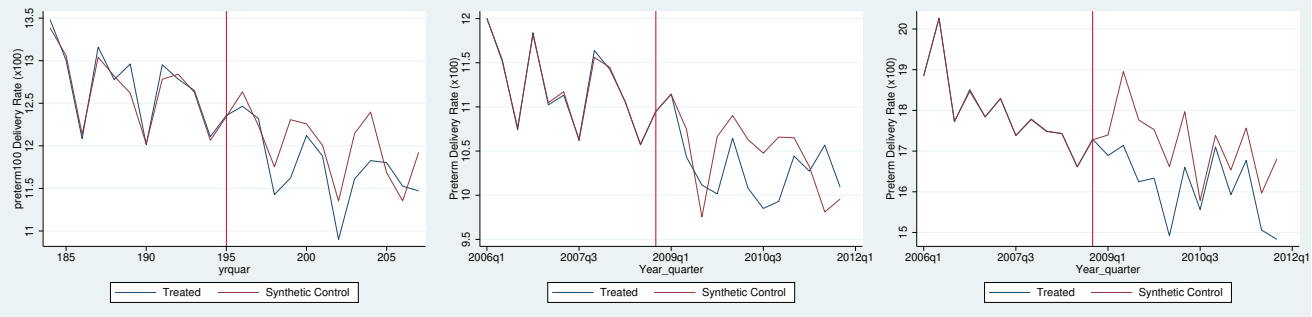
B Synthetic Control

B.1 SC Graphs



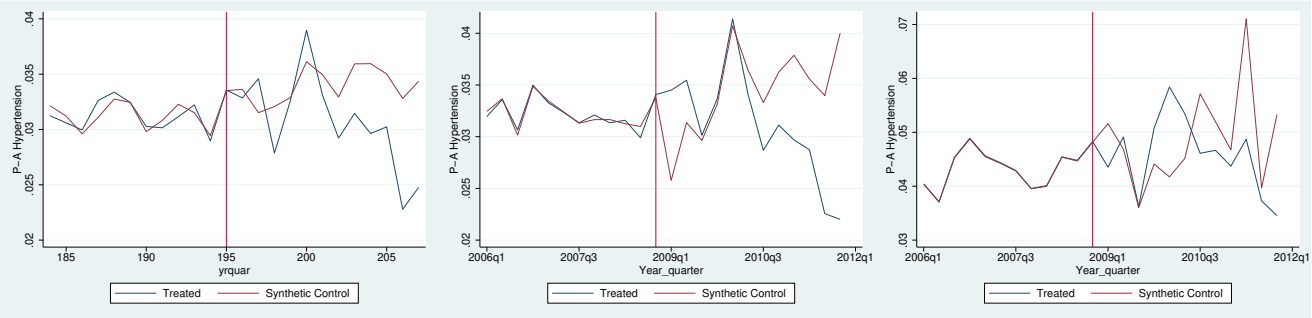
(a) All (b) non-Hispanic White (c) non-Hispanic Black

Figure B.1: NJ Synthetic Control, Changes in gestational weeks



(a) All (b) non-Hispanic White (c) non-Hispanic Black

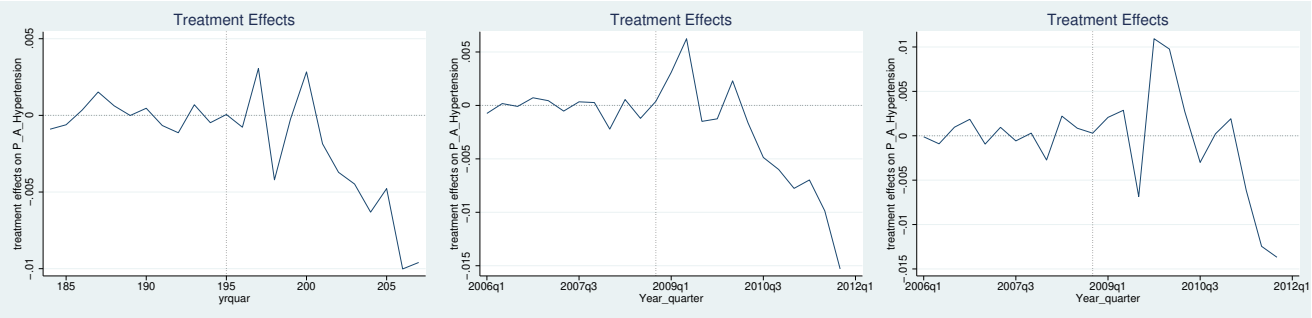
Figure B.2: NJ Synthetic Control, Changes in preterm delivery rate



(a) All (b) non-Hispanic White (c) non-Hispanic Black

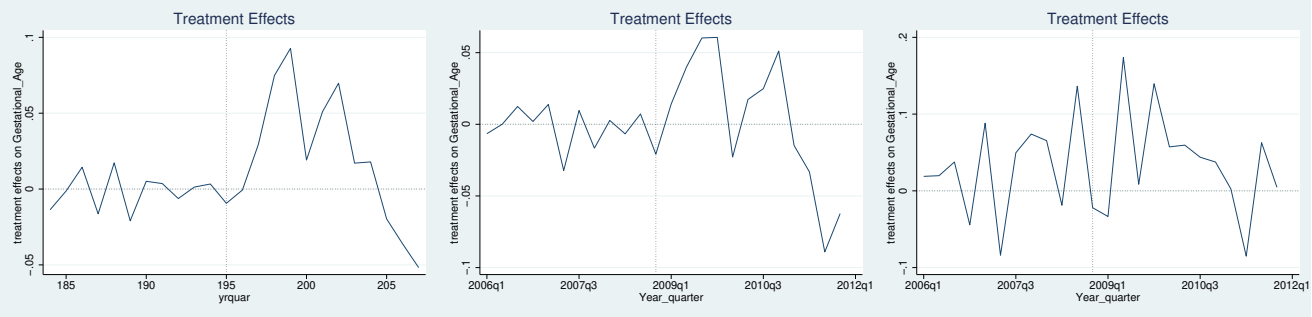
Figure B.3: NJ Synthetic Control, Changes in P-A Hypertension

B.2 SC Treatment Effect



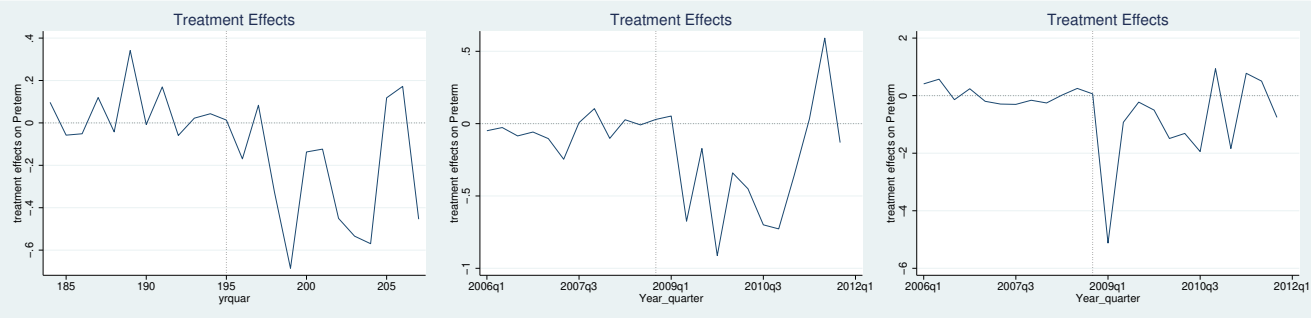
(a) All (b) non-Hispanic White (c) non-Hispanic Black

Figure B.4: Treatment effect: gestational week



(a) All (b) non-Hispanic White (c) non-Hispanic Black

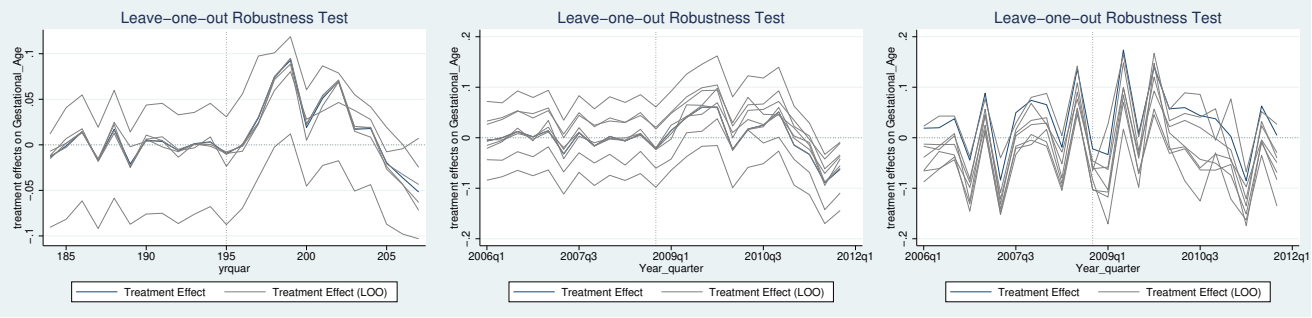
Figure B.5: Treatment effect: preterm delivery rate



(a) All (b) non-Hispanic White (c) non-Hispanic Black

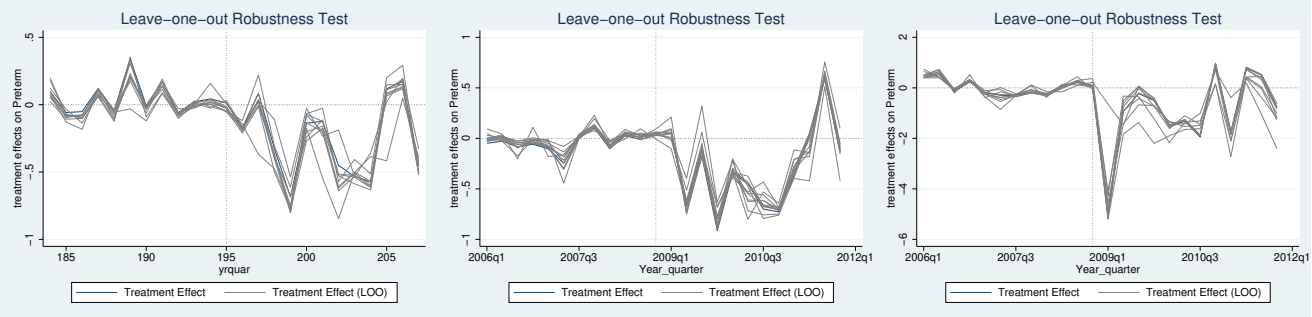
Figure B.6: Treatment effect: P-A Hypertension

B.3 SC Leave-one-out Test



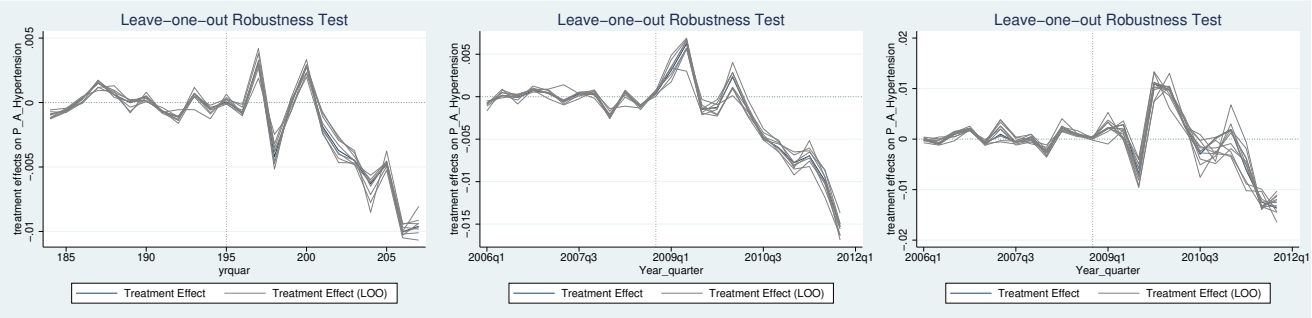
(a) All (b) non-Hispanic White (c) non-Hispanic Black

Figure B.7: NJ SC: Leave-one-out treatment effect



(a) All (b) non-Hispanic White (c) non-Hispanic Black

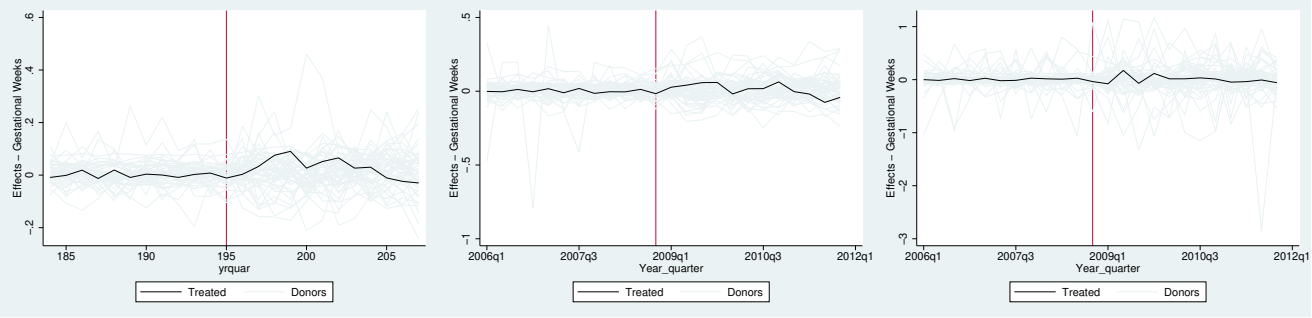
Figure B.8: NJ SC: Leave-one-out treatment effect



(a) All (b) non-Hispanic White (c) non-Hispanic Black

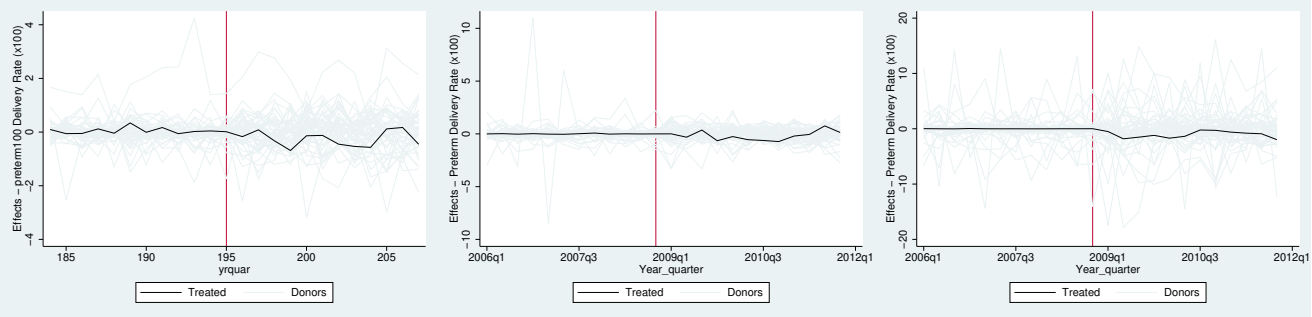
Figure B.9: NJ SC: Leave-one-out treatment effect

B.4 SC In-Place Placebo Test



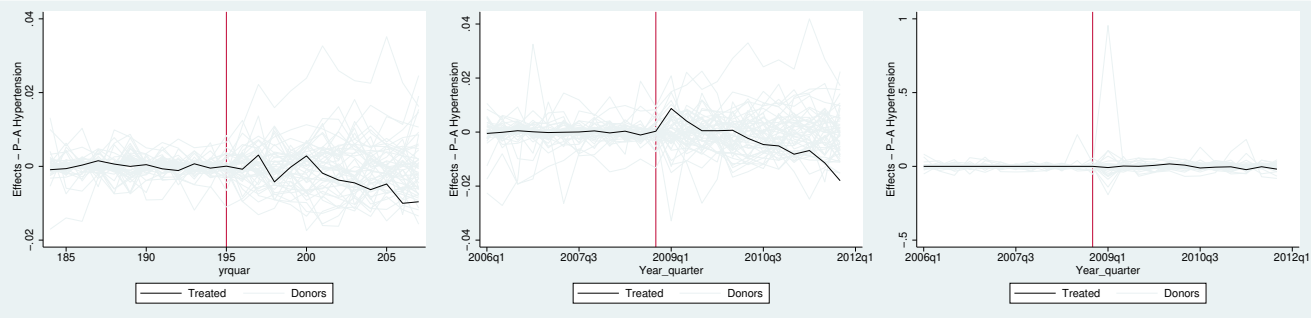
(a) All (b) non-Hispanic White (c) non-Hispanic Black

Figure B.10: In-place Placebo: Gestational weeks



(a) All (b) non-Hispanic White (c) non-Hispanic Black

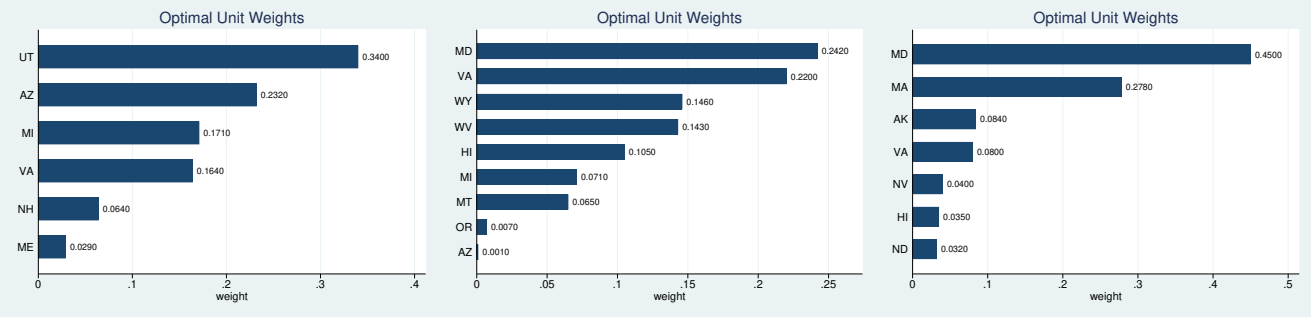
Figure B.11: In-place Placebo: Preterm delivery



(a) All (b) non-Hispanic White (c) non-Hispanic Black

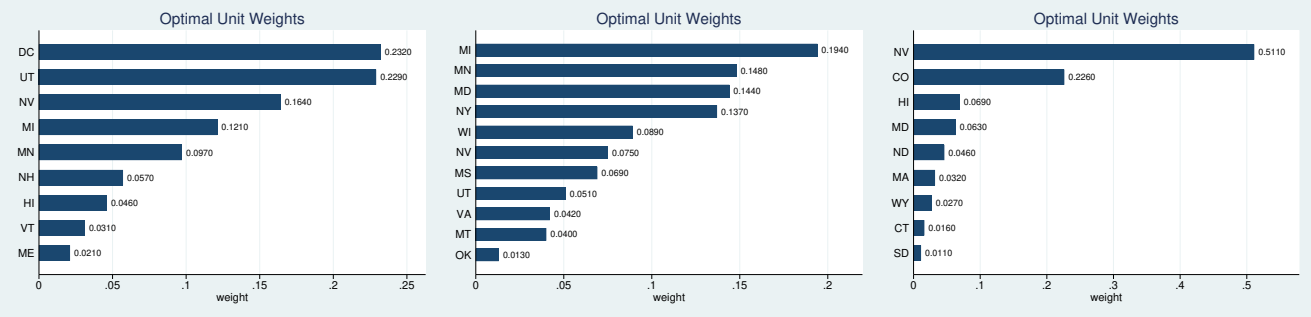
Figure B.12: In-place Placebo: P-A hypertension

B.5 SC Optimal Unit Weight



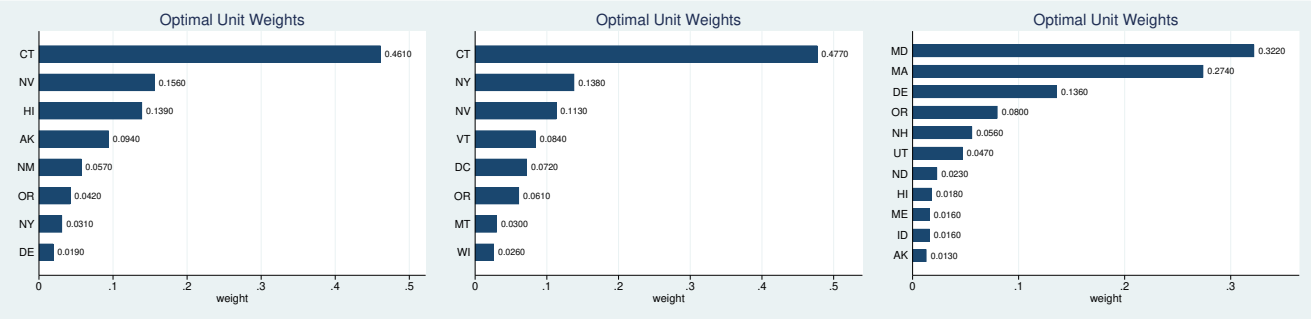
(a) All (b) non-Hispanic White (c) non-Hispanic Black

Figure B.13: Optimal Unit Weight: Gestational Weeks



(a) All (b) non-Hispanic White (c) non-Hispanic Black

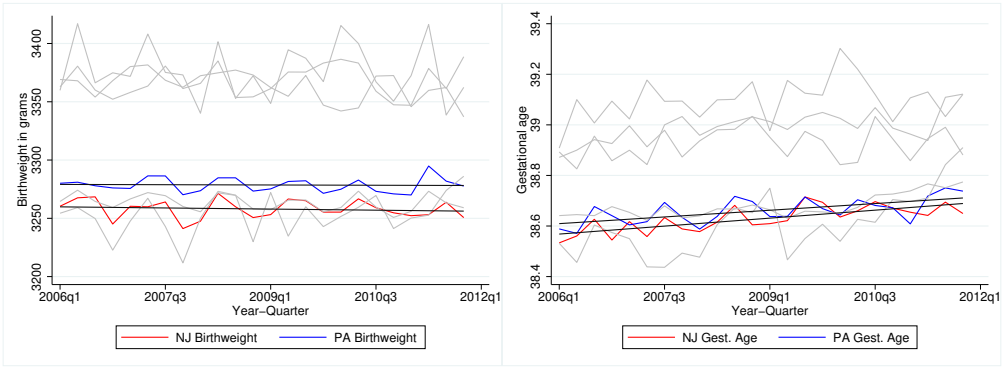
Figure B.14: Optimal Unit Weight: Preterm Delivery



(a) All (b) non-Hispanic White (c) non-Hispanic Black

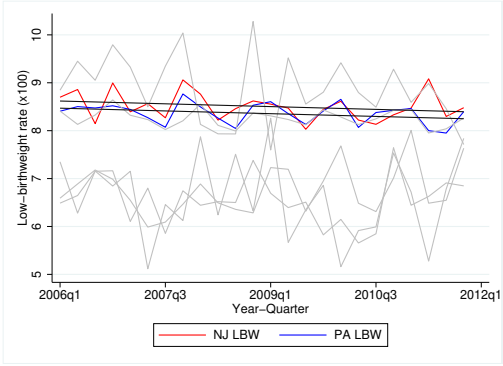
Figure B.15: Optimal Unit Weight: P-A hypertension

C NJ vs PA Analysis

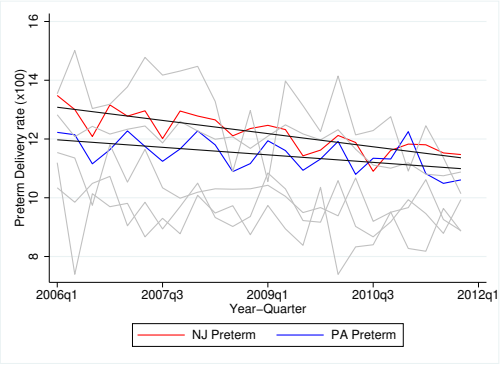


(a) Birthweight

(b) Gestational Week



(c) LBW



(d) Preterm

Figure C.1: North East Region Birth Outcomes: NJ vs PA

Table A1: Summary Statistics

	Obs(overall)	PA before	NJ before	Diff.(PA-NJ)	S.E.
Birthweight in grams	2,048,647	3281.027	3261.044	19.983***	(0.853)
Gestation in weeks	2,049,877	38.665	38.638	0.027***	(0.004)
Low birth weight (<2500 g)	2,054,841	0.083	0.084	-0.001***	(0.000)
5-min apgar score	2,046,817	8.804	8.923	-0.118***	(0.001)
P-A hypertension	2,054,841	0.043	0.030	0.012***	(0.000)
Month prenatal care began	1,119,079	3.212	2.824	0.387***	(0.009)
Mom <20 yrs old	2,054,841	0.086	0.058	0.028***	(0.000)
Mom 20-24 yrs old	2,054,841	0.224	0.167	0.056***	(0.001)
Mom 25-29 yrs old	2,054,841	0.283	0.257	0.027***	(0.001)
Mom 30-34 yrs old	2,054,841	0.255	0.303	-0.048***	(0.001)
Mom 35-39 yrs old	2,054,841	0.124	0.173	-0.049***	(0.000)
Mom 40-44 yrs old	2,054,841	0.026	0.040	-0.013***	(0.000)
Mom 45-49 yrs old	2,054,841	0.002	0.003	-0.001***	(0.000)
Mom >50 yrs old	2,054,841	0.000	0.000	-0.000***	(0.000)
Mom <HS education	1,115,960	0.155	0.129	0.025***	(0.001)
Mom has HS education	1,115,960	0.845	0.871	-0.025***	(0.001)
Mom has some college	1,115,960	0.585	0.621	-0.036***	(0.001)
Mom has college_degree_more	1,033,281	0.400	0.750	-0.350***	(0.003)
Mom is non-Hispanic white	2,054,841	0.693	0.458	0.235***	(0.001)
Mom is non-Hispanic black	2,054,841	0.139	0.150	-0.011***	(0.000)
Mom is Hispanic	2,054,841	0.093	0.259	-0.165***	(0.001)
Mom is married	2,054,841	0.598	0.655	-0.057***	(0.001)

Notes: Asterisks indicate significant differences in means between PA and NJ (***p<0.01, **p<0.05,*p<0.1).

Table A2: Effect of PFL on Birth Outcomes for White and Black Mothers

	Birthweight		Gest.age		LBW		Preterm	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	White	Black	White	Black	White	Black	White	Black
Births after PFL	8.521***	6.322	0.0421***	-0.00358	-0.179	-0.536*	-0.493***	-0.827**
	[2.855]	[6.351]	[0.0117]	[0.0305]	[0.126]	[0.326]	[0.151]	[0.358]
Dept. var mean	3333.4	3087.7	38.76	38.20	7.209	13.49	10.50	16.78
Indiv. obs.	911636	222774	912255	223078	914242	223854	914242	223854

Standard errors in brackets

* $p < .1$, ** $p < .05$, *** $p < .01$

	(1)	(2)	(3)	(4)	(5)	(6)
	LBW	Gest.Age	Preterm	LBW	Gest.Age	Preterm
Births after PFL	-0.734***	0.0816***	-1.025***	-0.290***	0.0408***	-0.618***
	[0.0988]	[0.00917]	[0.115]	[0.0998]	[0.00928]	[0.116]
Dept. var mean	8.420	38.65	11.80	8.420	38.65	11.80
Indiv. obs.	1548938	1545497	1548938	1548938	1545497	1548938
County control	Yes	Yes	Yes	Yes	Yes	Yes
Demographic control	No	No	No	Yes	Yes	Yes

Standard errors in brackets

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: LBW and preterm delivery rates are scaled by 100

Table A3: Effect of PFL on Adverse Birth Outcomes

	LBW		Preterm		LBW		Preterm	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	White	Black	White	Black	White	Black	White	Black
Births after PFL	-0.179	-0.536*	-0.493***	-0.827**	-0.0868	-0.740*	-0.211	-0.896**
	[0.126]	[0.326]	[0.151]	[0.358]	[0.300]	[0.391]	[0.350]	[0.427]
Dept. var mean	7.209	13.49	10.50	16.78	9.023	14.14	12.15	17.34
Indiv. obs.	914242	223854	914242	223854	240364	165944	240364	165944
Sample	All	All	All	All	Unmar.	Unmar.	Unmar.	Unmar.

Standard errors in brackets

* $p < .1$, ** $p < .05$, *** $p < .01$

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