The Long-Run Effects of Childcare Subsidies on Maternal Labor Market Outcomes*†

Ashley Erceg‡

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Abstract

The current cost of childcare is pricing women out of the labor market. There has been some political focus on decreasing the cost of kindergarten and preschool to keep mothers employed, however, that may come too late. By the time a child is old enough to enroll in public schooling mothers have likely already decided to "stay at home" or return to work. Subsidizing childcare while children are younger may differentially impact women's labor market attachment. While there is a substantial literature documenting large returns to childcare subsidies, few studies examine their long-run effect or how the timing of subsidy may interact with women's labor market decisions. This paper investigates the long-run impact of childcare subsidies offered soon after first birth on a women's employment, earnings, and total hours worked. Using administrative data linking earnings records from the SSA/IRS to the SIPP 1984-2009 panels and the PSID, I implement a dynamic difference-in-difference model that exploits variation in the timing of birth and the 2003 expansion to the Child and Dependent Care Credit. I find that women exposed to the expansion when their children are young are more likely to work and have higher earnings. Early exposed women are 1.2 and 2.5 percentage points more likely to be employed zero to four years after giving birth, suggesting they return to work sooner. This effect grows in the long run. Early exposed women are 3.1 to 5.2 percentage points more likely to be employed five to ten years after giving birth, suggesting they are more likely to remain employed. This translates to large earnings returns for early exposed women. Five to ten years after giving birth early exposed women earn between $5,600-$6,300 more per year.

Keywords: Childcare Subsidies, Maternal Labor Supply, Work Incentives, Tax Credits, Childcare

JEL Codes: H20, H24, J10, J13

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‡University of Nebraska - Lincoln

1 Introduction

A recent survey found that over half of parents using childcare spend more than 20 percent of their household income on it.[1] Parents reported making significant work-related changes due to childcare costs, including reducing work hours and leaving the workforce entirely. Time away from work can lead to long-term differences in labor market outcomes, especially for women. Women with gaps in employment have trouble re-entering the labor market and have lower wages throughout their careers (Waldfogel, 1997; Ruhm, 1998; Lundberg and Rose, 2000). Past research has found that childcare subsidies increase women's labor supply in the short-run (Blau and Tekin, 2007; Davis et al., 2018), however, little research has focused on the long-run effects of childcare subsidies or how the *timing* of subsidy receipt may interact with women's labor market decisions.

This paper investigates the impact of childcare subsidies offered soon after first birth on women's employment, earnings, and total hours worked. Using administrative data, I'm able to estimate effects on employment and earnings up to ten years after a woman's first child is born. I leverage variation caused by timing of birth and the 2003 expansions to the Child and Dependent Care Credit (CDCC). The CDCC is a federal childcare subsidy that partially refunds the cost of childcare for working parents based on income. The credit experienced a substantial expansion as part of the Bush Tax Cuts in 2001. Effective starting in 2003, the expansion increased the value of the credit in two ways. First, it increased the maximum credit a family could claim, from \$720 to \$1,050. Second, the expansion flattened the phase-out region of the credit, allowing more families to collect a higher credit amount.

I use a dynamic difference-in-differences model to examine the effect of exposure to the 2003 CDCC expansion when a women's firstborn child is young. My data comes from two sources: administrative data that links women's complete earnings histories to the 1984-2008 Survey of Income and Program Participation (SIPP) panels, and data from the Panel Study of Income Dynamics (PSID). For my analysis, I define two cohorts of women based on the

[1] https://www.care.com/c/how-much-does-child-care-cost/

year they gave birth to their first child. Women who gave birth between 2001 and 2006 are my early exposed cohort. These women had access to a larger subsidy when their children were young (zero to two years old). I define women who gave birth between 1994 and 2000 as the late-exposed cohort; these women experienced an increase when their children were older (three to nine years old). To mitigate issues that may arise due to differences across birth cohorts, I introduce a third comparison group based on implicit eligibility requirements caused by the credit being non-refundable. Because the credit is non-refundable, low-income families often do not have enough tax liability to use the credit. Families with an adjusted gross income (AGI) below $15,000 were estimated to have received an average credit of $61 in 2017. In contrast, families making more than $15,000 had an average estimated credit of $547. For my empirical analysis, I define eligibility based on the income a woman makes in the year before giving birth to her first child. Women earning more than $15,000 are considered eligible, while women earning less than $15,000 are considered ineligible.

I find that women exposed to the expansion when their firstborn child is young are more likely to be employed and have higher earnings throughout their lives. Early exposed women are between 1.2 and 2.5 percentage points more likely to be employed in the short-run (zero to four years after giving birth), suggesting they returned to work sooner. This effect is larger in the long-run. Early exposed women are 3.1 to 5.2 percentage points more likely to be employed five to ten years after giving birth. This suggests that not only are early exposed women returning to work sooner but they are also more likely to remain employed. Early exposed women also have higher average earnings. In the short-run, early exposed women earn between $2,051 to $3,760 more per year. This effect increases substantially over time. Five to ten years after giving birth, early exposed women earn between $5,600-$6,300 more per year. Using synthetic data from the Synthetic SIPP Beta (SSB), I explore potential mechanisms through which the increase in earnings may have occurred. I find that ten years after giving birth, early exposed women have 0.4 additional years of employment, though I am unable to rule out zero effect using a placebo test. I find mixed evidence on the short-run effect on total annual hours worked. Dynamic difference-in-differences estimates suggest that early exposed women work an average of 3 hours less per week, though these results are insignificant in most years. triple-difference estimates suggest early exposed women work as much as seven hours more per week, suggesting that women may move from part- to full-time work.

This paper makes three contributions to the existing literature. First, while the CDCC is one of only two federal-level childcare programs offered by the United States, little research has been conducted on the policy. Research on the short-run impact of the CDCC expansion found positive and significant effects for married women. Pepin (2020) found that a 10 percent increase in CDCC benefits led to a 0.7 percent increase in employment, a 0.9 percent increase in hours worked, and a 5 percent increase in earnings among married mothers. They found no effect on single women, likely driven by the fact single women tend to have a lower average income and are less likely to be eligible for credit. I improve upon this paper by examining the long-run effects of this policy, and by focusing on how the timing of subsidy receipt interacts with women's labor market outcomes. My results support the findings in Peppin (2020). I find evidence that early exposed women are more likely to be employed and have higher earnings. While I find mixed results on hours worked, triple-difference estimates support that early exposed women work more hours annually.

My second contribution is providing evidence on the long-run effect of childcare subsidies. While there is a substantial literature on the short-run impact of childcare subsidies on women's labor supply (Berger and Black, 1992; Blau and Tekin, 2003; Tekin, 2005; Blau and Tekin, 2007; Davis et al., 2018), few studies focus on their long-run impact. The work examining long-run impact is mixed. Baker et al. (2019) examines the effect of a Canadian child tax benefit on labor force participation, finding no impact. In contrast, to Baker et al. (2019), Herbst (2017) finds large, long-run, returns to the Lanham Act of 1940, which heavily subsidized childcare in the United States. The program immediately increased full-time work among mothers and increased labor attachment later in life. The lack of studies on the long-run impacts of current programs stems largely from data constraints. Using administrative earnings data and the PSID, I'm able to construct year-by-year work histories for women over a long time frame. My results suggest that childcare subsidies have large long-run effects. Early exposed women are much more likely to remain employed and have significantly higher earnings in the long-run.

Work on the long-run effect of childcare programs has used preschool or kindergarten as a proxy for childcare. Past research has found that public schooling for children translates to increased labor force participation among mothers (Gelbach, 2002; Cascio, 2009). However, this impact has lessened over time. As more women have joined the labor force, school subsidization has had less of an impact on mothers' labor supply (Fitzpatrick, 2012). This may be because public schooling comes too late. When children are old enough to enroll in public school, mothers have likely already made their decision to "stay at home." Timing of subsidy receipt, in relation to first childbirth, may differentially affect mothers' labor market decisions. This leads to this paper's third contribution to the existing literature, I estimate the return to childcare subsidies given soon after a woman's first birth.

The CDCC is a work incentive for parents, working similarly to the Earned Income Tax Credit (EITC). Papers in EITC literature have examined the long-run effects of the EITC, finding positive returns for mothers' labor supply. Neumark and Shirley (2020) examines the long-run impact of the EITC on women's labor market outcomes using the PSID. They find evidence that exposure to a more generous EITC leads to long-run increases in earnings, likely due to human capital accumulation. Kuka and Shenhav (2020) examines the long-run impact of the EITC on mothers based on the timing of birth. Using Social Security Administration (SSA) data linked to the Current Population Survey (CPS) they find large returns to the program. Ten to nineteen years after their first birth, women who were eligible for the tax credit when their children were young had accrued more years of work experience and had higher earnings than mothers who were exposed when their children were older. This suggests that work incentives that occur soon after birth can lead to large gains in woman's labor supply. This paper examines the extent that the timing of childcare subsidies can increase the labor supply among mothers. The CDCC warrants additional examination because it targets a different population than most work incentives and childcare programs. In contrast to the EITC, the CDCC is primarily used by middle and high-income families. My long-run estimates are smaller in magnitude than the long-run estimates for the EITC, however, they suggest that work incentives still have a large effect on middle- and high-income women.

The rest of this paper is structured as follows: Section 2 outlines the institutional details of the CDCC, Section 3 discusses data, Section 4 presents my empirical strategy, Section 5 presents results, and Section 6 discusses and concludes.

2 Institutional Background

The Child and Dependent Care Credit is a federal, non-refundable, tax credit created to help working families pay for childcare expenses. It can be used for any dependent, under 13 years old who attends childcare. The credit can be claimed for most types of childcare, including care at a center, a family day care home or a church, vacation day camps, or care provided by a neighbor or a relative (except if provided by a spouse, a dependent, or a child of the tax filer under 19).[2] The credit is a work incentive, requiring that both parents (if married) must be working in order to be eligible. The credit may also be claimed by divorced or separated parents who have custody of their child, and by single parents. The credit amount is calculated as a percentage of childcare expenses, determined by the taxpayer's AGI (AGI). For married households, the credit is calculated based on the AGI of the spouse with lower earnings.

I use variation generated in the 2003 expansion to the CDCC, however, how the provision has changed since its inception warrants some discussion. Table (1) displays how the nominal and real maximum qualifying expenses and maximum credit per child, and maximum credit

[2] https://www.taxoutreach.org/tax-credits/care-credit/

rate have evolved over time. The credit was implemented in 1976 and expanded in 1981 and 2001 (effective 2003). The 1976 credit could be used for up to $2,000 dollars in childcare expenses at a max rate of 20 percent. A household could therefore claim a maximum of $400 per child. Starting in 1982, expenditures increased to a maximum of $2,400. This could be claimed with a maximum rate of 30 percent. The 1982 expansion introduced the rate schedule where the rate was determined based on AGI. This maximum credit could be claimed by households making $10,000 or less. The credit was reduced by 1 percentage point for each $2,000 of income between $10,000 and $28,000 until it hits 20 percent. Everyone making more than $28,000 was eligible for a 20 percent deduction.

This paper primarily uses the variation caused by the 2003 expansion. The expansion was introduced in 2001 as part of the Bush Tax Cuts. It changed the credit in two main ways. First, it increased the maximum credit a family could receive. It did this by increasing the maximum childcare costs you could claim from $2,400 to $3,000, and by increasing the maximum benefit rate from 30 percent to 35 percent. Families making a total of $15,000 or less can claim the 35 percent rate. Second, it flattened out the phase-out schedule so that more households were able to claim a larger credit amount. The credit is reduced to a minimum of 20 percent by one percentage point for each $2,000 of income between $15,000 and $43,000. The most important institutional detail is that the credit was not indexed for inflation, meaning the real value declined. When the credit was expanded in 1982, the real value[3] was approximately $1,740 per child per year, assuming a family was able to collect the maximum statutory amount.[4] By 2002, the real value had declined to approximately $940. After the 2003 expansion, you could collect approximately $1,345.

The U.S. Department of Treasury reports federal expenditure for a number of programs, including expenditure on the CDCC. The 2003 expansion increased the total benefit a family could claim, meaning that there should be an increase in expenditures in 2003 if the credit was effective. Figure (1) graphs estimated federal expenditure on the CDCC from 1995 to

[3] In 2014 dollars.

[4] I will discuss the differences between the statutory and effective credit amount later in this paper.

2019 in millions of dollars. Federal expenditures have varied greatly over time, but there is a general upward trend in spending from 2003 to 2019. The expansion did not broaden the number of families qualifying for the CDCC, only the amount for which they are eligible. This means that federal spending increased for eligible families receiving higher credit.

Figure (2) depicts the effective maximum federal CDCC benefits for families with one child before and after the 2003 expansion. The blue line graphs the maximum credit before the 2003 expansion and the red line graphs the maximum credit after the expansion. An important institutional detail is that the credit is not refundable, this creates a difference between the statutory credit and the effective credit. While the statutory maximum credit for one child is $1,050, the effective max is only $950. Prior to the 2003 expansion, the maximum effective credit a family could receive was approximately $600. After the expansion, the maximum effective credit a family could receive was approximately $950. Note that families with very low income either collect very little credit or are ineligible to collect the credit. This is because families with very low incomes do not have the necessary tax liability to claim the credit. An important institutional detail is that the CDCC is non-refundable.

Internal Revenue Service (IRS) tax information reports aggregate the CDCC with other tax credit programs, making it difficult to directly report the number of eligible families that utilize the credit in a given year. A report from the Office of Tax Analysis estimates that 6.3 million families received the CDCC in 2016 (Ackerman et al., 2016). Table (2) shows the estimated average benefit and number of claimants by AGI for the 2017 tax year. As demonstrated in Figure (2), tax data show significant heterogeneity in take-up. Families with very low income ($15,000 and under) are estimated to submit very few claims relative to other income brackets, making up only 0.06 percent of expected claimants in 2017. The average benefit amount is also lower with the average payout being only $61. Families making between $30,000 and $40,000 have the largest average benefit of $611. Notably, families with higher income still receive a relatively large benefit of over $500. I make use of this variation in credit amount by AGI in my empirical strategy. Individuals with an AGI of $15,000 or less are a “low-impact” group as they claim very little credit on average.

3 Data

My data comes from two sources.Data on women’s employment and earnings comes from administrative records from the SSA/IRS which has been linked to the 1984-2008 SIPP panels. I am able to access this data by working with the Synthetic SIPP Beta (SSB). The SSB is a synthetic data set that estimates the joint distribution of all variables in the data and takes random draws from the modeled distribution. The draws are then used to replace actual data to protect confidentiality. Users are permitted to have their code run against the true data. The main results in this paper are validated using the actual records. Data on hours worked comes from the Panel Study of Income Dynamics (PSID).

The administrative data contains birth records for a person’s first- and last-born children, complete W-2 earnings histories (through 2014), and a number of demographic factors such as race, gender, age, the highest level of education, and marital status. Using this information I create a panel that includes women’s complete federal payroll tax (FICA) earnings histories as they would have been reported to the IRS. Importantly, I am only able to observe earnings that were reported to the IRS, meaning I may miss women who engage in unreported work. This data does not contain information on labor force participation, instead, I rely solely on earnings to determine if someone is employed. I categorize someone as employed if they have any positive earnings in a given year. All earnings information is converted into real 2014 dollars.

To examine how CDCC exposure affects hours of work, I use data from the PSID 1968-2019. The PSID began in 1968 with a nationally representative sample of 18,000 individuals belonging to 5,000 families. Since 1968 the PSID has followed these individuals and their descendants on an annual basis (biennial since 1997). They collect detailed economic and demographic information including employment, wages, earnings, total hours worked, education, marriage, and fertility. Respondents report their own labor market activities as well as those of their spouses. I use this information to create full year-by-year histories of women captured in the PSID. I assign parental history using the Child Birth and Adoption History File. The file contains detailed information on the history of childbirth and adoption for respondents in the PSID, covering 1985 to 2019.

I make a number of sample restrictions. I follow the same procedure in both the SSB and PSID. First, I include mothers who have their first child between 1994 and 2006. I only include individuals who have records for at least five years before giving birth and at least eight years after giving birth. My goal is to estimate the effect of the CDCC expansion on women who were employed prior to giving birth, so I limit my sample to only include women who were employed at least one year prior to giving birth. I consider mothers to be early exposed to the CDCC expansion if they gave birth to their first child between 2001 and 2006. I consider a mother late-exposed if they gave birth to their first child between 1994 and 2000.

I use the fact that the non-refundable credit creates implicit eligibility requirements in my empirical strategies. In my sample, I exclude states, where the CDCC is refundable because individuals with very low income, would be able to claim the credit in those states.[5] All samples described in this section only include individuals who live in non-refundable states.

4 Empirical Strategy

The goal of this paper is to estimate the effect of an increase in generosity in childcare subsidies given soon after first birth on mothers' labor market outcomes. Before examining the effect of childcare subsidies, I conduct a descriptive analysis of how childbirth changes women's employment, earnings, and hours worked. I estimate this effect using the following

[5]States where the CDCC is non-refundable include: Alabama, Alaska, Arizona, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Idaho, Illinois, Indiana, Kansas, Maryland, Massachusetts, Michigan, Mississippi, Missouri, Montana, Nevada, New Hampshire, New Jersey, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, West Virginia, Wisconsin, and Wyoming

two-way fixed-effect model:

$$y_{ib\tau} = \alpha_i + \sum_{k \neq -2} \beta_k D^k_{i\tau} + D_{i\tau} + \theta_t + \delta P_{s\tau} + \epsilon_{ib\tau} \tag{1}$$

In this analysis, I compare the labor market outcomes of mothers before and after giving birth. Here $y_{ib\tau}$ is the labor market outcome of interest for individual i, in the year of first birth, b, in the year (relative to first birth), τ. Following Borusyak et al. (2021), I drop two event years from my analysis because I do not have a group of never-treated individuals. I omit $\tau = -2$ and $\tau = -3$. $D_{i\tau}$ are event-time indicator variables. θ_t captures year fixed effects while α_i captures person fixed effects. P_{st} controls for state-level variables, including unemployment and the presence of universal preschool and/or kindergarten. Standard errors are clustered at the state-year level. My parameter of interest, β_k, captures the difference in labor market outcomes for women before and after giving birth.

Results suggest that women reduce their labor supply after giving birth. Panel (a) of Figure (3) presents the coefficients and confidence interval estimates from equation (1) on the changes in employment around first birth. My results suggest an immediate decrease in employment. In the year a woman gives birth, she is 9 percentage points more likely to be unemployed. This effect doubles a year after giving birth to 19 percentage points. The probability a woman is employed continues to decline in the short-run. Panel (b) of Figure (3) presents the coefficients and confidence interval estimates from equation (1) on the changes in earnings around first birth. These estimates include women with zero earnings. In the year a woman gives birth, she sees a reduction in earnings of $11,480. Women's earnings continue to decrease substantially as their children age. Five years after giving birth, a woman earns an average of $18,560 less. Panel (c) of Figure (3) presents the coefficients and confidence interval estimates from equation (1) on the changes in total annual hours worked around first birth. These estimates include women who work zero hours. Estimates suggest that women reduce their total annual hours worked after giving birth, though it is only statistically significant four to five years after giving birth.

Next, I estimate the effect of early exposure to the 2003 federal expansion of the CDCC. Following the estimation strategy outlined by Kuka and Shenhav (2020), I estimate a dynamic difference-in-differences equation. This model allows me to exploit variation across groups that receive treatment at different times. My first difference captures labor market outcomes before and after childbirth. My second difference captures differences in labor market outcomes between early and late-exposed mothers. In this design, treatment is defined as having a child and being exposed to the 2003 CDCC expansion within the first year after the first birth.

$$y_{ib\tau} = \alpha_i + \sum_{k \neq 2} \beta_k D_{i\tau}^k \cdot Early_b + \theta_t + D_{i\tau} + \delta P_{s\tau} + \epsilon_{ib\tau} \tag{2}$$

Here $y_{ib\tau}$ is the outcome of interest (employment, log earnings, and total annual hours worked) for individual i, year of first birth, b, in the year (relative to first birth), τ. $Early_b$ is an indicator for having a first birth between 2001 and 2006. As with equation (1), I omit $\tau = -2$ because I am unable to determine if a woman was pregnant in $\tau = -1$. θ_t captures year fixed effects while α_i captures person fixed effects. P_{st} controls for state-level variables, including unemployment and the presence of universal preschool and/or kindergarten. Standard errors are clustered at the state-year level. My parameter of interest, β_k, captures the difference in labor market outcomes for women before and after first birth relative to late-exposed mothers.

A standard difference-in-differences model is likely to be biased if the treatment and control groups do not share parallel trends prior to treatment. In this context, my identification assumption is that early and late-exposed mothers share common trends in labor market outcomes prior to "treatment"[6] such that absent the CDCC expansion they would have sim-

[6]Treatment here is defined as giving birth and being early exposed to the CDCC. Early exposed women

ilar outcomes. This is violated if early exposed women were changing their labor supply at a different rate than late-exposed mothers prior to first birth. The assumption of parallel trends is likely violated in this specification. Labor force participation among women has been steadily increasing over time, and mothers who are early exposed are likely younger than late-exposed mothers. This may cause me to conclude that early exposed mothers are more likely to work than late-exposed mothers due to the CDCC expansion when they are simply more likely to work due to the upward trend of women's labor force participation. Another important consideration is the timing of birth. My early exposed cohort experienced the Great Recession when their children were young, which may lead me to conclude that early exposed women are less likely to work. While I include year fixed effects in my model, the recession is likely to bias my results.

To mitigate this bias, I introduce a third comparison group based on implicit eligibility requirements created by the fact the CDCC is non-refundable. Mothers with an income less than \$15,000 are often ineligible to receive the tax credit because they do not have the necessary tax liability. In my triple-difference model, I define a woman as eligible if she has an income of over \$15,000 in the year prior to her first birth. While someone with an income this low may be eligible for a small amount of credit in certain circumstances, the credit amount is near zero.[7] Under this specification, parallel trends require that the gap between eligible and ineligible mothers would have evolved similarly for early and late-exposed mothers.

In this model, my first difference captures labor market outcomes before and after childbirth. My second difference captures differences in labor market outcomes between Early and late-exposed mothers. My third difference captures the difference in labor market outcomes between eligible and ineligible mothers.

$$y_{ib\tau} = \alpha_i + \sum_{k \neq 2} \beta_k D_{i\tau}^k \cdot Early_b \cdot Eligible_i + D_{i\tau} + \theta_t + \delta P_{s\tau} + \epsilon_{ib\tau} \tag{3}$$

gave birth to their first child between 2001 and 2003.

[7]The average credit amount for families with an AGI below \$15,000 is \$61.

Here $y_{ibs\tau}$ is the outcome of interest for individual i, in the year of first birth b, in the year (relative to first birth) τ. $Early_b$ is an indicator for having a first birth between 2001 and 2006. $Eligible_i$ is an indicator variable for being eligible for CDCC benefits.[8] I include all two-way interactions between $Early_b$, $D^k_{i\tau}$, and $Eligilbe_i$ as well as the main effects, they are omitted from the written model for simplicity. As with my dynamic difference-in-differences model, I omit $\tau = -2$. Coefficients are therefore interpreted relative to two years prior to first birth. θ_t captures year fixed effects, while α_i captures person fixed effects. $P_{s\tau}$ controls for state-level variables, including the state-level unemployment rate and an indicator for universal preschool and/or kindergarten availability. My coefficient of interest, β_k, captures the difference in outcomes between the gap in outcomes for Early and late-exposed eligible mothers and the gap in outcomes for early and late-exposed ineligible mothers. Standard errors are clustered at the state-year level.

Recent advances in two-way fixed effect methods have uncovered a number of potential issues associated with difference-in-differences methods. Roth et al. (2022) provides a nice summary of recent advances, which I will discuss here. In my preferred specification, treatment is defined by giving birth and by being exposed to the 2003 expansion to the CDCC when their child is zero to two years old. The primary concern in my setting arises from the fact that I am using staggered "adoption" in this setting because women in my sample give birth in different years. Under these circumstances, it is possible that Two Way Fixed Effect (TWFE) regression is using a "contaminated" comparison group where inappropriate units are compared to one another. In this paper, the potential "contaminated" comparison would arise if comparisons were made between units that have both been treated.[9] This may lead to TWFE coefficients having the incorrect sign due to negative weighting. Even if all weights are positive, they may not reflect the policy-relevant parameter, particularly if you are estimating the long-run effect, which I do in this paper. Several papers have come forth,

[8] I categorize mothers with earnings over $15,000 in the year prior to first birth as eligible.

[9] There are no never-treated or always-treated units in this paper. Every woman in my sample has a child eventually and no woman always has a child in my sample.

offering different diagnostic tests, as well as potential fixes for these issues.

The estimators created to solve this issue are more difficult to adapt in my circumstance. Newer estimation methods require that you identify a group of individuals who never receive treatment in any period. There is no such group in my circumstance, as every mother is theoretically eligible to receive the subsidy. A natural control group is using a group of women who have no children, however, this is problematic for several reasons. First, my fundamental question is not estimating the effect of having children on a woman's labor supply. My research question surrounds the effect of childcare subsidies on women's labor market decisions. In the ideal experiment, I would randomly assign mothers to different levels of subsidy generosity, not randomly assign birth. A second issue is that using a group of women without children significantly complicates the methodology where I estimate the effect of the federal-level expansion. Including a base group of never-treated women will cause equation (2) to become a triple-difference equation and equation (3) to become a quadruple-difference equation. Finally, the assumption of common trends is unlikely to hold when comparing women who choose to have children and women who choose not to have children. The decision to have children is likely endogenous to a woman's labor market choices.

5 Results

The goal of this paper is to estimate the effect of increased subsidy generosity given soon after a woman gives birth to her first child. In this section, I will discuss the results from the dynamic difference-in-differences and triple-difference models. In section 5.1, I will discuss the effect of early exposure to the 2003 expansion in the short-run, which I define as zero to four years after giving birth. Year zero should be treated as a child's infancy. In section 5.2, I will discuss the long-run effect of early exposure to the expansion in the long-run, which I define as five to ten years after giving birth.

5.1 Short-Run

Table (3) aggregates the results of my dynamic difference-in-differences and triple-difference models in the short-run. Coefficients are normalized to two years prior to giving birth and can be interpreted as the treatment effect of early exposure to the 2003 expansion relative to two years prior to giving birth.

I will begin by discussing the effect of early exposure on the probability of employment. Figure (4) graphs the coefficients and confidence intervals from equations (2) and (3). The light blue line depicts the estimates from my difference-in-differences model while the dark blue line graphs the estimates from my triple-difference model. There is some graphic evidence of differences in the probability of employment prior to giving birth between my treatment and control groups. Early exposed women are more likely to be employed prior to giving birth. This effect decreases as a woman approaches the year she gives birth. The solid dark blue line of Figure (4) presents triple-difference estimates of the effect of early exposure to the CDCC on women's employment. While there is evidence of differences in employment four years prior to giving birth, there is no statistically significant difference between my treatment and control groups in most years. My treated cohort includes women who gave birth in 2001 when the increase in subsidy was announced, so all treated women were aware that there was (or would be) an increase in the subsidy benefits. It's possible that the pre-trends are a treatment effect where women change their behavior prior to giving birth in anticipation of the increased subsidy amount.

Column (1) of Panel (a) presents the coefficients and standard errors from my dynamic difference-in-differences model on the effect of early exposure to the 2003 expansion on employment. I find that early exposed women are 1.2 percentage points more likely to be employed than late-exposed women over the first five years after giving birth. This is an increase of 1.2 percent at the mean. Column (1) of Panel (b) presents my triple-difference estimates and standard errors. My findings under this specification are more than double my findings under my difference-in-differences specification, though they are no longer statistically significant. They imply a 2.5 percentage point, or 2.6 percent increase in the probability of employment.

Next, I will discuss the effect of early exposure to the CDCC on log earnings. Figure (5) presents the event-study estimates from my dynamic difference-in-differences and triple-difference models. Due to disclosure issues, I cannot report the coefficients from $\tau = -1$, however, the estimate is calculated in the model. The light blue line graphs the estimates from my dynamic difference-in-differences model. There is some evidence of differences in earnings prior to giving birth. Estimates suggest that early exposed women earn more than late-exposed women prior to giving birth. The effect increases in size as a woman approaches the year she gives birth. My triple-difference specification passes the common trends assumption. The solid dark blue line depicts triple-difference estimates. There is no statistically significant evidence of differences in trends in my triple-difference model.

Column (2) of Panel (a) presents the coefficients and standard errors from my dynamic difference-in-differences strategy for the short-run effect of early exposure to the CDCC on earnings. I find that early exposed women earn an average of 7 percent more than late-exposed women. This would be equivalent to an average increase in earnings of $2,051 per year. Panel (b) of Column (2) presents the triple-difference estimates of the short-run effect of early exposure to the CDCC on earnings. triple-difference estimates suggest a larger, but statistically insignificant, effect on average earning. Early exposed women earn an average of 12.1 percent more per year over the first five years after giving birth. At the mean, this is equivalent to $3,757 per year at the mean.

Finally, I will discuss the effect of early exposure to the CDCC on women's total annual hours worked. Figure (6) graphs the dynamic difference-in-differences and triple-difference estimates. The light blue line plots coefficients from the dynamic difference-in-differences model. There is no evidence of differences in total annual hours worked prior to giving birth estimates are small and include zero. The solid dark blue line plots the estimates from my triple-difference model. There is no statistically significant evidence of differences in trend, though there is some evidence that total annual hours worked is decreasing prior to giving birth.

Column (3) of Panel (a) presents the dynamic difference-in-differences estimates of the effect of early exposure to the expansion on total hours worked. These results are conditioned on remaining employed and therefore exclude individuals who work zero hours. I find that early exposed women work fewer hours than late-exposed women. Early exposed women work approximately 45 fewer hours per year over the first six years after giving birth (0-5 years). This would translate to an average decrease of approximately one hour per week. This result is statistically significant but is small in magnitude. Panel (b) of Column (3) presents triple-difference coefficients and standard errors. The results under this specification suggest that early exposed women increase their total annual hours of work by 275 per year. This would translate to an average increase of 5.3 hours per week. This may suggest that women may be moving from part- to full-time work in the years after giving birth. While I graphically pass the common trends assumption in both models, triple-difference estimates are less likely to be biased by differences across cohorts. Given the other findings, it's likely there are some significant differences in labor supply between early and late-exposed mothers.

5.2 Long-Run

In this section, I will discuss the long-run effect of early exposure to the 2003 CDCC on women's employment and earnings. Due to data constraints, I do not present results for the effect on total hours worked. Table (4) aggregates the results of my event-study model from equation (2). As with the short-run results, coefficients are normalized to two years prior to giving birth. They can be interpreted as the treatment effect of early exposure to the 2003 expansion five to ten years after giving birth, relative to two years prior to giving birth.

Column (1) of Panel (a) presents the coefficients and standard error estimates for the effect of early exposure to the expansion on long-run employment. Estimates suggest that early exposed women are 3.1 percentage points more likely to be employed five to ten years after giving birth. This is equivalent to an increase of 3.2 percent. This effect is nearly three times the size of the short-run effect, implying that women are more likely to remain employed after giving birth. Column (1) of Panel (b) presents triple-difference coefficients and standard error estimates for the effect of early exposure to the expansion on long-run employment. Estimates under this specification are larger in magnitude than the difference-in-difference model. Though they are statistically insignificant, they support that early exposed women are more likely to be employed. Five to ten years after giving birth, early exposed women are 5.2 percentage points more likely to be employed, a 5.4 percent increase.

Column (2) of Panel (a) presents the dynamic difference-in-differences coefficients and standard errors on the long-run effect of early exposure to the expansion on earnings. Estimates suggest a significant increase in earnings by 20 percent. At the mean, this would imply an average increase of nearly $6,300 per year. This long-run effect implies a fairly substantial cumulative effect for early exposed women. Column (2) of Panel (b) presents the triple-difference estimates on the long-run effect of early exposure to the 2003 expansion to the CDCC on earnings. Estimates are similar in size to the difference-in-differences estimates. Early exposed women see an 18 percent increase in average earnings five to ten years after giving birth. This is equivalent to an additional $5,625 per year.

6 Robustness Checks

In this section, I consider different model specifications. Estimates on employment and earnings use data from the SSB. The results presented here have not been validated against the actual administrative data. While the results differ in magnitude, the overall conclusion using synthetic data is similar to those found using administrative data. In one model I omit person fixed effects and instead use state-year fixed effects with a vector of person-level controls. These controls include controls for a woman's highest level of education[10], race, and

[10]This includes high school dropout, high school graduate, some college, and college graduation

marital status[11]. In the second model, I omit year fixed effects in favor of year-of-childbirth-fixed effects.

Figure B2 presents the results from my state-year fixed effect model on employment. The dashed blue line graphs the results from using person-year fixed effects (my main specification) while the solid red line graphs the results using state-year fixed effects and a vector of covariates. The results using state-year fixed effects are consistent and nearly identical to those using person-year fixed effects. Panel (a) presents my dynamic difference-in-differences specification. Results under this specification indicate that over the ten-year period after giving birth to their first child, women are 2.7 percentage points, or 2.9 percent more likely to work. While this result is larger than the results found using person-year fixed effects, all estimates fall within the confidence interval of the results under of the main specification. None of the results are statistically significant. Similar to results using person-year fixed effects, the largest result is 9 to 10 years after the birth of their first child, with an average increase of 3.2 percentage points or 3.5 percent. Panel (b) presents the coefficients associated with the triple-difference specification. As with the dynamic difference-in-differences equation under this specification, the results are well within the confidence intervals using person-year fixed effects. The estimates suggest that over the ten years after giving birth, women are an average of 2 percentage points or 2.2 percent more likely to be employed. These results are not statistically significant in any year.

Figure B2 presents the results from the state-year fixed effect model on log earnings. Panel (a) presents the coefficients from the dynamic difference-in-differences model. Results using state-year fixed effects are similar in magnitude to those using person-year fixed effects, though the results are statistically significant in most years. The estimates suggest that early exposed women earn as much as 14 percent more than their late-exposed counterparts. At the mean, this would be roughly equivalent to an average increase of approximately $3,500 per year. The effect is largest and is significant during the first five years after giving birth.

[11] Women are defined as either always single or married.

Over the five years after giving birth, a woman's earnings increases by an average of 15 percent an average increase of $3,765. While results under this specification are statistically significant, they are within the confidence intervals of the results using person-year fixed effects. Panel (b) presents the coefficients from the triple-difference model. As with the results from using person-year fixed effects, triple-difference estimates suggest that early exposed mothers have a decrease in earnings. Estimates are larger than the results from the main specification, with an average decrease in earnings of nearly $900. While larger in magnitude, these estimates fall within the confidence interval using person-year fixed effects and they are statistically insignificant in each event year.

Figure B3 presents the results from the state-year fixed effect model on total annual hours worked. Panel (a) presents the coefficients from the difference-in-differences specification. As with results using person-year fixed effects, estimates suggest that early exposed women work less than late-exposed women. Estimates under this specification are larger in magnitude using person-year fixed effects but they are within the confidence intervals of the estimates found using person-year fixed effects. This effect begins in the year prior to giving birth. Being early exposed is associated with an average decrease of 231 hours annually, this would imply an average decrease of roughly four and a half hours per week. This effect remains relatively stable until two years after giving birth. It decreases to an average decrease of 165 hours per week and becomes statistically insignificant. Panel (b) graphs the triple-difference estimates using state-year fixed effects. The blue lines are estimates associated with using person-year fixed effects and the red lines are associated with the output with the person and year-of-childbirth fixed effects. The results using this specification broadly match the estimates using person-year fixed effects and fall within the confidence intervals. triple-difference estimates suggest that early exposed women are more likely to work, though the results are statistically insignificant in most years. Women begin increasing their hours worked in the year prior to giving birth, by an average of 222 hours or approximately 4.3 hours per week. This effect increases until two years after giving birth where triple-difference estimates suggest an average increase of 655.8 hours annually or approximately 12 and a half more hours per week. This effect decreases in magnitude and becomes statistically insignificant.

In the second specification, I use a two-way fixed effect model with fixed effects by person and year of first birth as outlined in Bailey et al. (2019). Figure B4 presents the difference-in-differences and triple-differences estimates on the impact of early exposure to the CDCC on the probability of being employed. Panel (a) displays the coefficients and confidence intervals associated with the dynamic difference-in-differences strategy. The dashed blue line graphs estimates associated using person-year fixed effects and the solid orange line graphs estimates using person and year-of-childbirth fixed effects. Estimates under this specification suggest a negative but small effect. Early exposed women work an average of 0.7 percentage points less than late-exposed women over the first ten years after giving birth—a decrease of 0.8 percent. No results are statistically significant in any year. While the sign on this estimate is negative, it is small and within the confidence intervals using person-year fixed effects. Panel (b) presents the results from the triple-difference strategy. Estimates using the year of childbirth fixed effects are broadly the same as the estimates using person and year fixed effects, and the estimates fall within the confidence intervals. Triple-difference estimates suggest an average increase of 2.1 percentage points or 2.1 percent over the ten years after giving birth. None of these estimates are significant in any year.

Figure B5 presents the difference-in-differences and triple-differences estimates on the effect of early exposure to the CDCC on the log earnings. Panel (a) plots the dynamic difference-in-differences coefficients. Estimates under this specification broadly match the estimates from the main specification, though they suggest a small effect. All estimates are within the confidence interval using person-year fixed effects. Over the ten years after giving birth, early exposed women earn an average of 2.3 percent more or an average of $577 more each year. This effect is largest in the years surrounding their first birth. The effect is largest in the year prior to giving birth where early exposed women earn an average of 13 percent more than late-exposed women, an increase of nearly $3,300. This effect is statistically significant. This drops in the following years and becomes negative starting four years after giving birth, though it is not significant. The effect becomes positive again starting eight years after giving birth. Panel (b) plots triple-difference coefficients. Estimates under this specification are similar in sign, though they suggest a larger effect. Estimates suggest that early exposed women see an average decrease in earnings of approximately $869 per year over the first ten years after giving birth. No result is statistically significant in any year, and all estimates are within the confidence intervals of estimates using person-year fixed effects.

Figure B6 presents the difference-in-differences and triple-differences estimates on the effect of early exposure to the CDCC on total annual hours worked. Panel (a) plots the dynamic difference-in-difference estimates. Estimates using the year of childbirth fixed effects are roughly the same in size as the estimates using person-year fixed effects, and all are within the confidence interval of the main estimates. Early exposed women work an average of 81 hours less per year over the first ten years after they give birth to their first child. The effect is greatest the year after giving birth, early exposed women work an average of 144 hours less than late-exposed women, approximately three hours less per week. Panel (b) graphs triple-difference estimates. Estimates using the year of childbirth fixed effects are similar in magnitude to those using person-year fixed effects. Triple-difference estimates suggest that early exposed women increase their total hours worked. Over the five years after giving birth, mothers work an average of 215 hours more per year, an average of 4 additional hours per week. As with the main specification, these effects begin prior to giving birth and increase until two years after giving birth. Two years after giving birth, triple-difference estimates suggest an increase of nearly 12 hours per week. These effects decrease in subsequent years.

7 Discussion and Conclusion

Childcare is costly. Parents are making difficult choices between paying for childcare and working. Leaving the labor market can lead to long-run differences in women's lifetime earnings. Childcare subsidies reduce the cost of childcare, allowing working mothers to remain employed. While there is substantial work on the short-run effect of childcare subsidies, few studies examine their long-run effect or the interaction between timing of childcare subsidies and mothers' labor force attachment. This paper estimates the long-run effect of childcare subsidies offered soon after first birth on women's labor supply and labor market outcomes using a dynamic difference-in-differences model. I do so by exploiting variation caused by timing of birth and a federal-level expansion to the CDCC. The CDCC is a federal, non-refundable, tax credit that partially refunds the cost of child care for working families.

I find suggestive evidence that increases in generosity in the CDCC lead to an increase in the probability of employment and earnings. Using an event-study model, I find that women exposed to the 2003 expansion are between 1.2 and 2.1 percentage points more likely to be employed in the short-run. This effect grows to 3.1-5.2 percentage points in the long-run. These results suggest that early exposed women are more likely to go return to work when their children are young and remain employed in the long-run. Though triple-difference results are not statistically significant, they suggest a large effect and are similar to the difference-in-differences estimates in sign and magnitude. Estimates on employment are similar to those found in Peppin (2021). They find that a 10 percent increase in CDCC benefits is associated with a 0.5 to 0.7 percent increase in the probability of employment. The maximum credit increased from $720 to $1050 in 2003, an increase of approximately 46 percent. Using Peppin's estimates, and assuming constant treatment effects, this would imply an effect between 2.3 and 3.5 percent.

Employment estimates are more modest than those implied by EITC. Kuka and Shenhav (2020) find women exposed to the EITC expansion when their children are young are 6 percentage points more likely to be employed in the year following giving birth. It's possible that the differences are caused by the fact the CDCC expansion was smaller, the 1993 EITC expansion represented an increase of nearly \$1,000 (\$2,381 to \$3,300).

I find that early exposed women see large earnings returns. In the short-run, early exposed women earn between 7 and 12 percent more per year. At the mean, this is an increase between \$2,051 and \$3,760 more per year. This effect grows substantially in the long-run. triple-difference estimates suggest an increase between 18 and 20 percent. This would be equivalent to an increase between \$5,600 and \$6,300. Over the ten years after giving birth this would imply an average increase in income between \$41,000 and \$47,000 for the early exposed. My findings support the results found in Peppin (2021). Peppin (2021) finds that a 10 percent increase in childcare subsidies is associated with a 4-5 percent increase in annual earnings. Assuming constant treatment effects would imply an upper limit between 18 and 22.5 percent increase in income. My results suggest more substantial earnings returns to the CDCC than those implied by the EITC. This may be driven by the differences in demographics. The CDCC is mostly claimed by middle- and high-income women, who may have a larger potential earnings profile than those eligible for the EITC.

To explore the mechanism through which the increase in earnings may occur, I estimate the effect of early exposure on tenure ten years after the first birth using synthetic data. I find some statistically significant evidence that women early exposed to the credit have more years of employment compared to late-exposed women. Table (5) displays the coefficients. Early exposed women have 0.40 more years of labor force experience ten years after the first birth. This result is statistically significant and implies that early exposed women have around half a year more time employed than late-exposed women. While this is descriptive in nature, it suggests that the change in earnings may be driven by human capital accumulation. These results are not robust to my placebo test using eligible women.

I find mixed evidence on the effect of early exposure on total annual hours worked. Dynamic difference-in-differences estimates show that early exposed women reduce their labor supply at the intensive margin, early exposed women work an average of three hours less per week. These results are not significant. triple-difference estimates suggest that over the five years after giving birth, women increase their annual hours worked by an average of seven hours per week. This may indicate that women are moving from part- to full-time work. triple-difference estimates are less likely to be biased by differences across birth cohorts, lending more credibility to the triple-difference findings. Findings using the triple-difference model are in line with the findings in Pepin (2020). Assuming constant treatment effects, Pepin (2020) would imply an average increase of 4.5 percent in weekly hours worked. My estimates suggest larger returns, and average weekly hours worked increased by approximately 12.7 percent. Women with young children may be more responsive to the CDCC than the average married woman.

Taken together, my results suggest large labor market returns childcare subsidies. Short-run results are generally in line with the results implied by Pepin (2020), and long-run estimates suggest larger returns to early exposure to the CDCC. My results on employment are smaller than those found by Kuka and Shenhav (2020), where early exposure to the 1993 expansion to the EITC is associated with between 3 and 4 percentage point increase in employment five years after giving birth and between 4.3 and 5.5 percentage points higher employment 5 to 9 years after giving birth. My results support that exposure to a more generous childcare subsidy when a woman's children are young has a lasting effect on her labor supply.

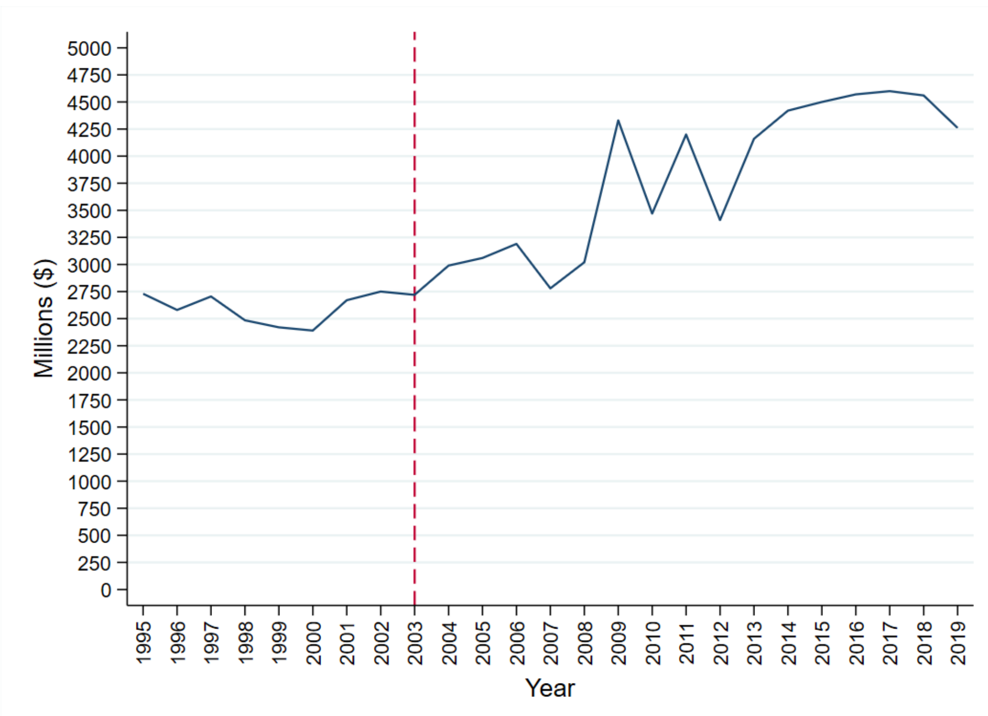
A Appendix

Table 1: CDCC Parameters Over Time

	1976	1982	2003
Max Qualifying Expenses	$2,000	$2,400	$3,000
Real Max Qualifying Expenses	$8,414	$5,953	$4,007
Max Benefit Rate	0.20	0.30	0.35
Max CDCC per Child	$400	$720	$1,050
Real Max CDCC per Child	$1,682	$1,786	$1,402

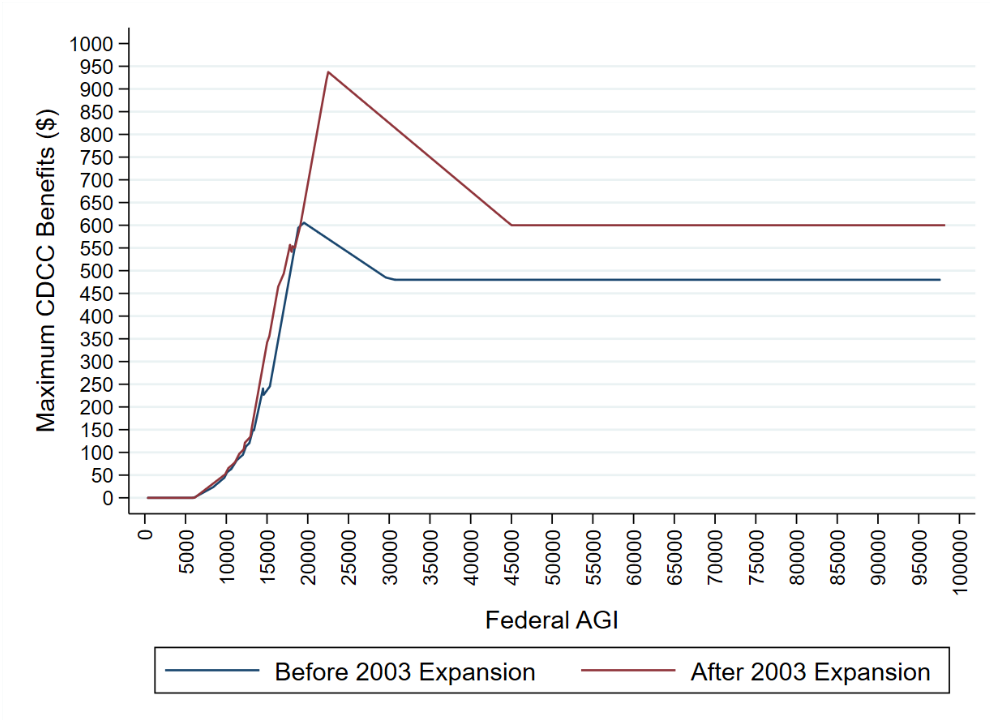
Federal CDCC parameters over time. Source: Information was retrieved from federal tax forms. Real dollars are calculated in 2014 dollars.

Figure 1: Estimate Federal Expenditure on CDCC



Estimated federal expenditure to the CDCC. Retrieved from U.S. Department of Treasury estimates of Tax Expenditures of the Federal Government from 1995-2019. The red dashed line indicates 2003 when the CDCC expansion was effective.

Figure 2: Estimated Benefit From CDCC Year 2017



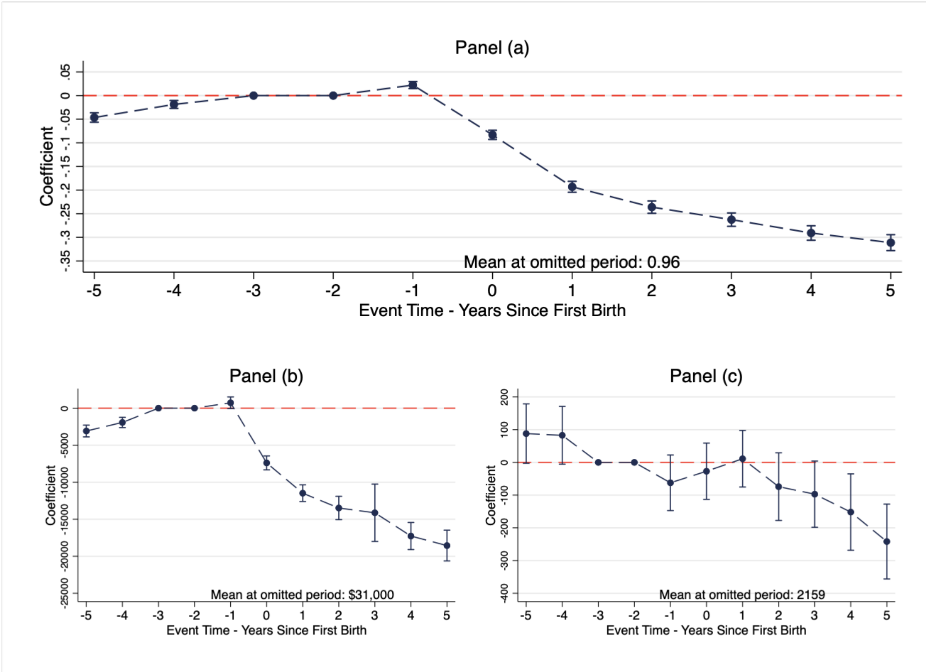
Maximum effective federal CDCC benefits for families with one child in nominal dollars. Estimates are created by using the Current Population Survey(Flood et al., 2022) and TAXSIM.

Table 2: Estimated Federal Benefit From CDCC Year 2017

Adjusted Gross Income	Number of Families	Average Benefit ($)
$0-$15,000	4,000	61
$15,000-$30,000	576,000	435
$30,000-$40,000	677,000	611
$40,000-$50,000	571,000	558
$50,000-$60,000	486,000	521
$60,000-$75,000	606,000	559
$75,000-$100,000	971,000	575
$100,000-$200,000	1,734,000	575
$200,000 and Over	626,000	544
Total	6,252,000	555

Source: Information was retrieved from the U.S. Department of Treasury. The table does not include state-level expansions to the CDCC. https://home.treasury.gov/system/files/131/WP-112.pdf

Figure 3: Changes in Employment around Childbirth



Pane(a) graphs the dynamic difference-in-difference estimates and confidence intervals the from equation (1) on the change in employment around the birth of a woman's first child. Panel (b) graphs the dynamic difference-in-difference estimates and confidence intervals from equation (1) on the change in earnings around the birth of a woman's first child. Data comes from the SSB with the sample restrictions outlined in Section III. Estimates include person and year fixed effects. Panel (c) graphs the dynamic difference-in-difference estimates and confidence intervals from equation (1) on the change in total annual hours worked around the birth of a woman's first child. Data comes from the PSID with the sample restrictions outlined in Section III. Estimates include person and year fixed effects. Standard errors are clustered at the state-year level.

Table 3: Short-Run Effect of Early Exposure to CDCC

	Employment	Log Earnings	Total Hours Worked
(a): DD			
PostBirth*EarlyExp	0.012***	0.07*	−44.59***
	(0.004)	(0.03)	(6.621)
(b): DDD			
PostBirth*EarlyExp	0.025	0.12	274.7***
	(0.031)	(0.111)	(64.76)
N	192,000	156,000	7,491

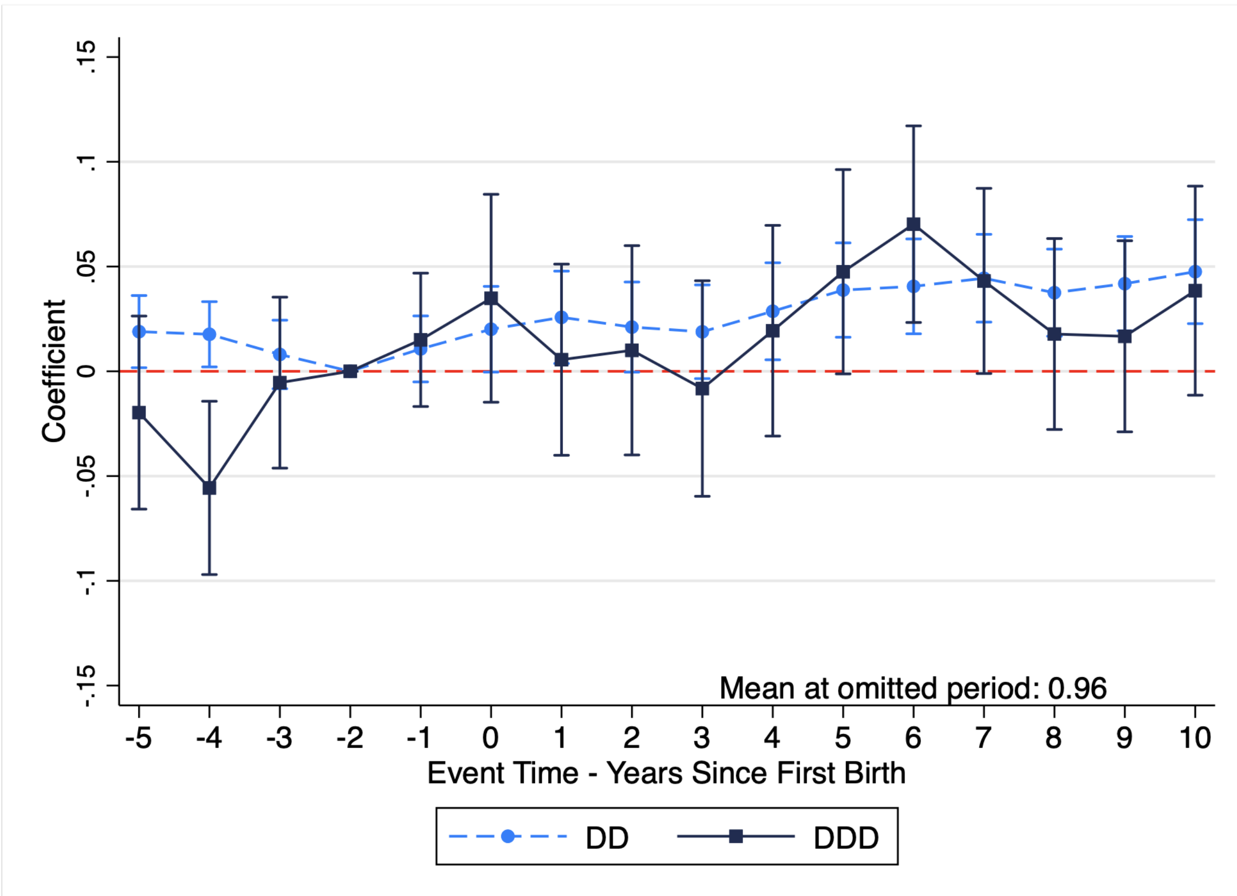
Panel (a) displays the difference-in-differences estimates on employment, earnings, and total annual hours, calculated using the coefficients from equation (1). Panel (b) displays the triple-difference estimates on employment, earnings, and total annual hours, calculated using the coefficients from equation (2). Column (1) displays the coefficients and standard errors for the effect of early exposure to the CDCC expansion on the probability a woman is employed zero to four years after giving birth to her first child. Column (2) displays the coefficients and standard errors for the effect of early exposure to the CDCC expansion on log earnings zero to four years after giving birth to her first child. Column (3) displays the coefficients and standard errors for the effect of early exposure to the CDCC expansion on total annual hours worked zero to four years after giving birth to her first child. Standard errors are clustered at the state-year level. Data on the effect on employment and log earnings comes from the SSB. Data on total annual hours worked comes from the PSID. I follow the sample restrictions as outlined in Section III in both samples. Per Census Bureau guidelines, sample counts have been rounded to the nearest 1000.

Table 4: Long-Run Effect of Early Exposure to CDCC

	Employment	Earnings
(a): DD		
PostBirth*EarlyExp	0.031***	0.2**
	(0.004)	(0.035)
(b): DDD		
PostBirth*EarlyExp	0.052	0.18**
	(0.003)	(0.09)
N	192,000	156,000

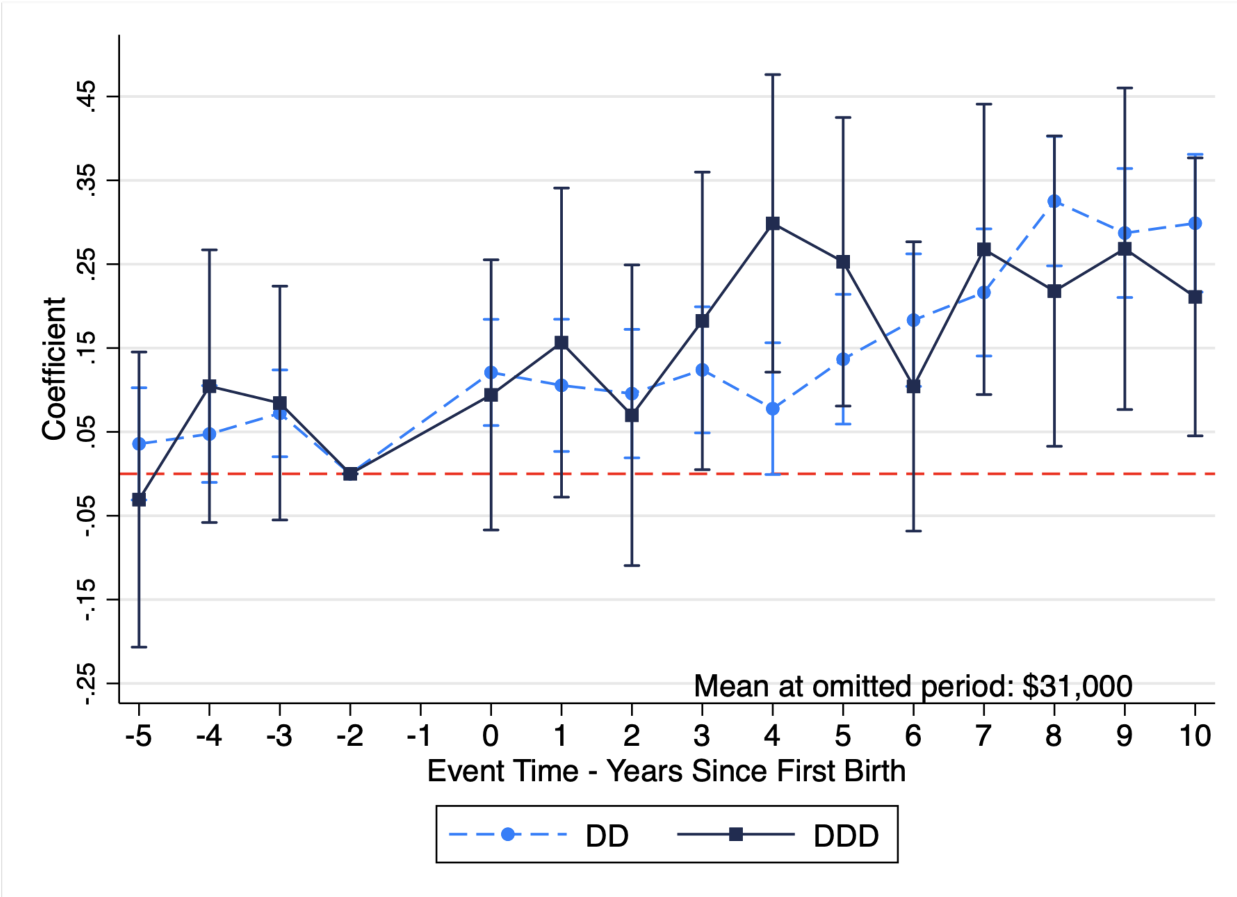
Panel (a) displays the difference-in-differences estimates on the effect of early exposure to the CDCC expansion on employment, earnings, and total annual hours, calculated using the coefficients from equation (1). Panel (b) displays the triple-difference estimates on employment, earnings, and total annual hours, calculated using the coefficients from equation (2). Column (1) displays the coefficients and standard errors for the effect of early exposure to the CDCC expansion on the probability a woman is employed five to ten years after giving birth to her first child. Column (2) displays the coefficients and standard errors for the effect of early exposure to the CDCC expansion on log earnings five to ten years after giving birth to her first child. Standard errors are clustered at the state-year level. Data comes from the SSB with the sample restrictions outlined in Section III. Per Census Bureau guidelines, sample counts have been rounded to the nearest 1000.

Figure 4: Effect of Early Exposure to CDCC Expansion on Employment



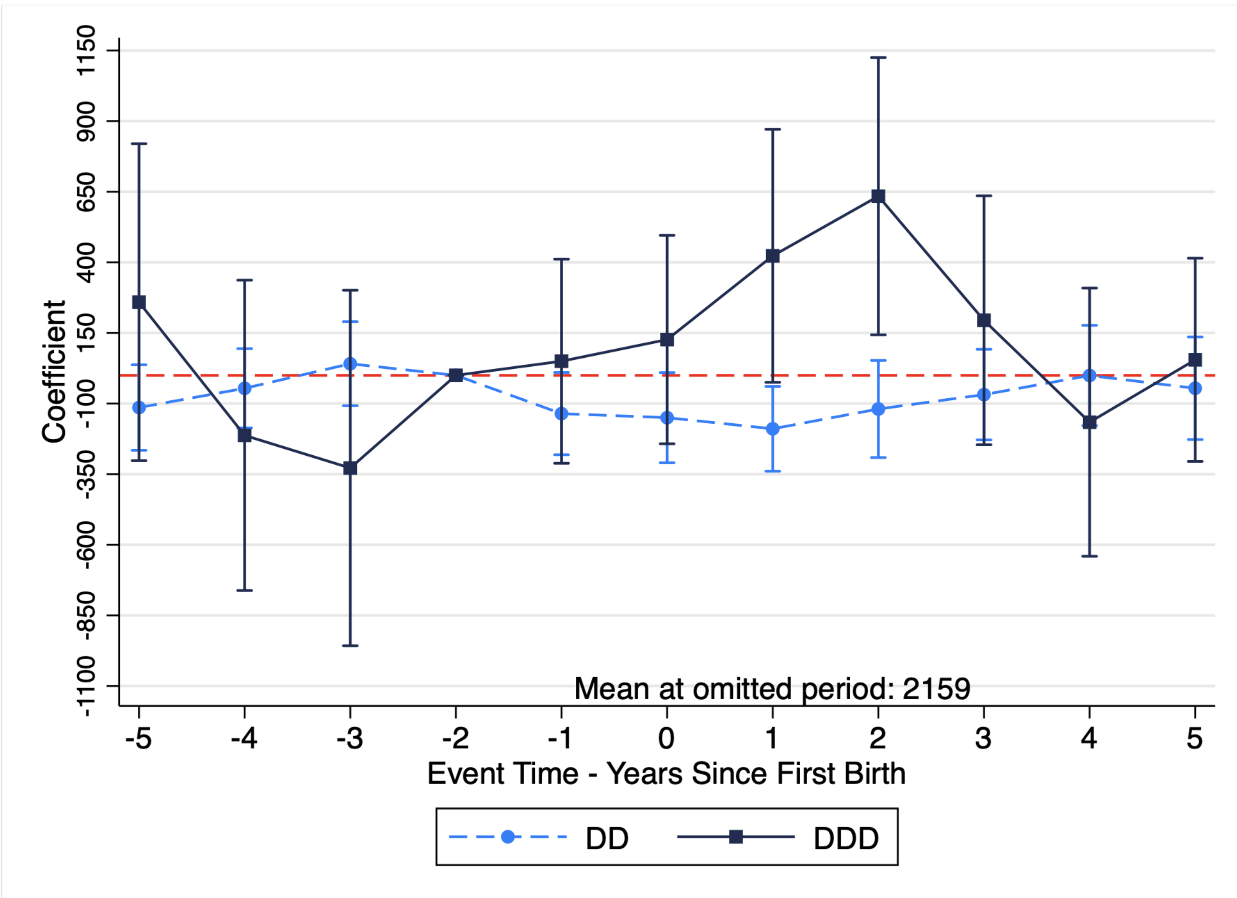
This figure plots the estimates on the effect of early exposure to the 2003 CDCC expansion on the probability of employment. The light blue line depicts the coefficients and confidence intervals from equation (2), while the dark blue line depicts the coefficients and confidence intervals from equation (3). Data comes from the SSB with the sample restrictions outlined in Section III. Standard errors are clustered at the state-year level.

Figure 5: Effect of Early Exposure to CDCC Expansion on Log Earnings



This figure plots the estimates on the effect of early exposure to the 2003 CDCC expansion on log-earnings. The light blue line depicts the coefficients and confidence intervals from equation (2), while the dark blue line depicts the coefficients and confidence intervals from equation (3). Data comes from the SSB with the sample restrictions outlined in Section III. Standard errors are clustered at the state-year level. Estimates at $\tau = -1$ were calculated, but could not be released due to disclosure risk.

Figure 6: Effect of Early Exposure to CDCC Expansion on Total Annual Hours Worked



This figure plots the estimates on the effect of early exposure to the 2003 CDCC expansion on total annual hours worked. The light blue line depicts the coefficients and confidence intervals from equation (2), while the dark blue line depicts the coefficients and confidence intervals from equation (3). Data comes from the PSID with the sample restrictions outlined in Section III. Standard errors are clustered at the state-year level.

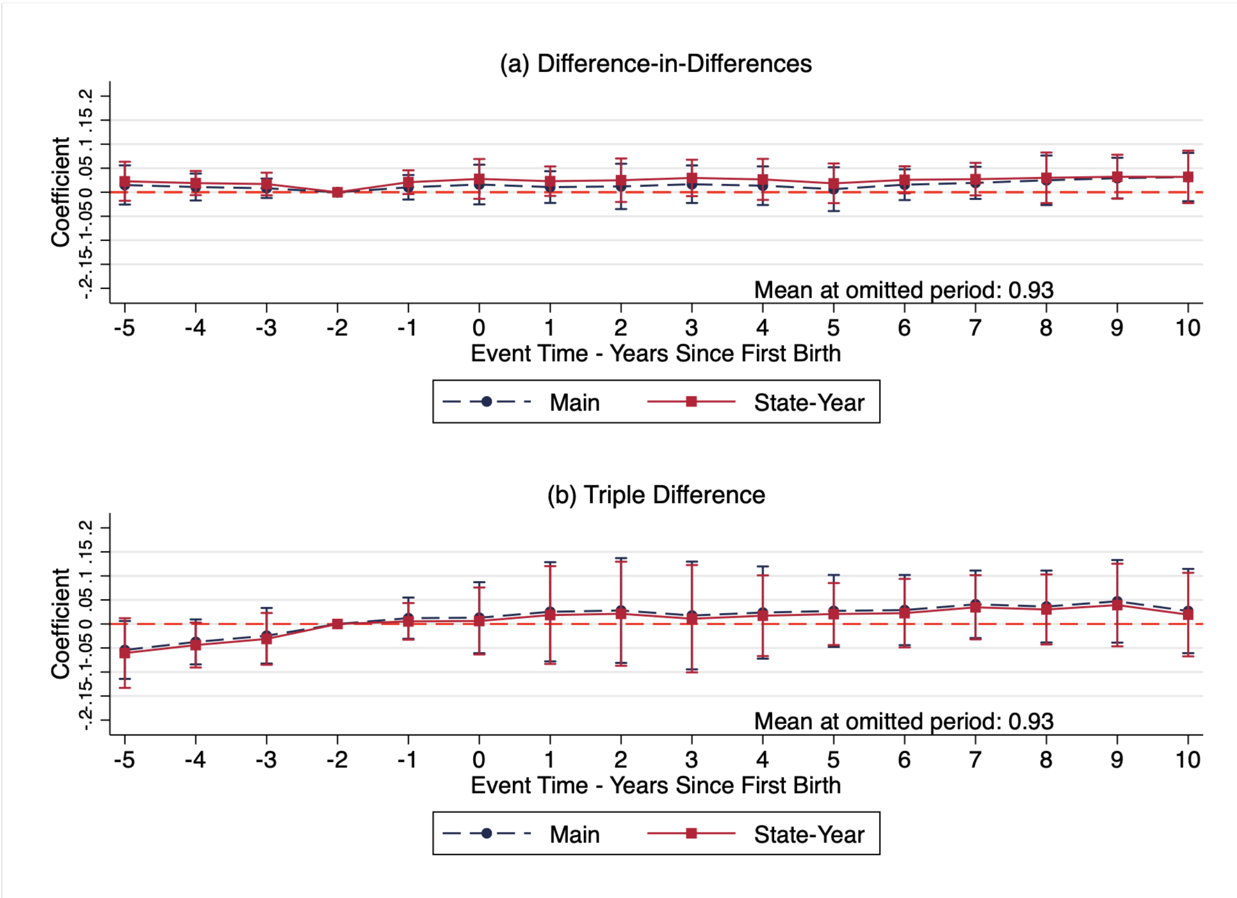
Table 5: Employment Tenure 10 Years after First Birth

	DD	DDD
PostBirth*EarlyExp	0.41^{***}	-0.059
	(0.069)	(0.118)
N	192,000	156,000

This table contains coefficient and standard error estimates from a difference-in-differences equation on the effect of early exposure to the 2003 expansion on total years employed 10 years after giving birth. Data comes from the SSB with the sample restrictions outlined in Section III. These estimates have not been validated using actual administrative records. Standard errors are clustered at the state-year level.

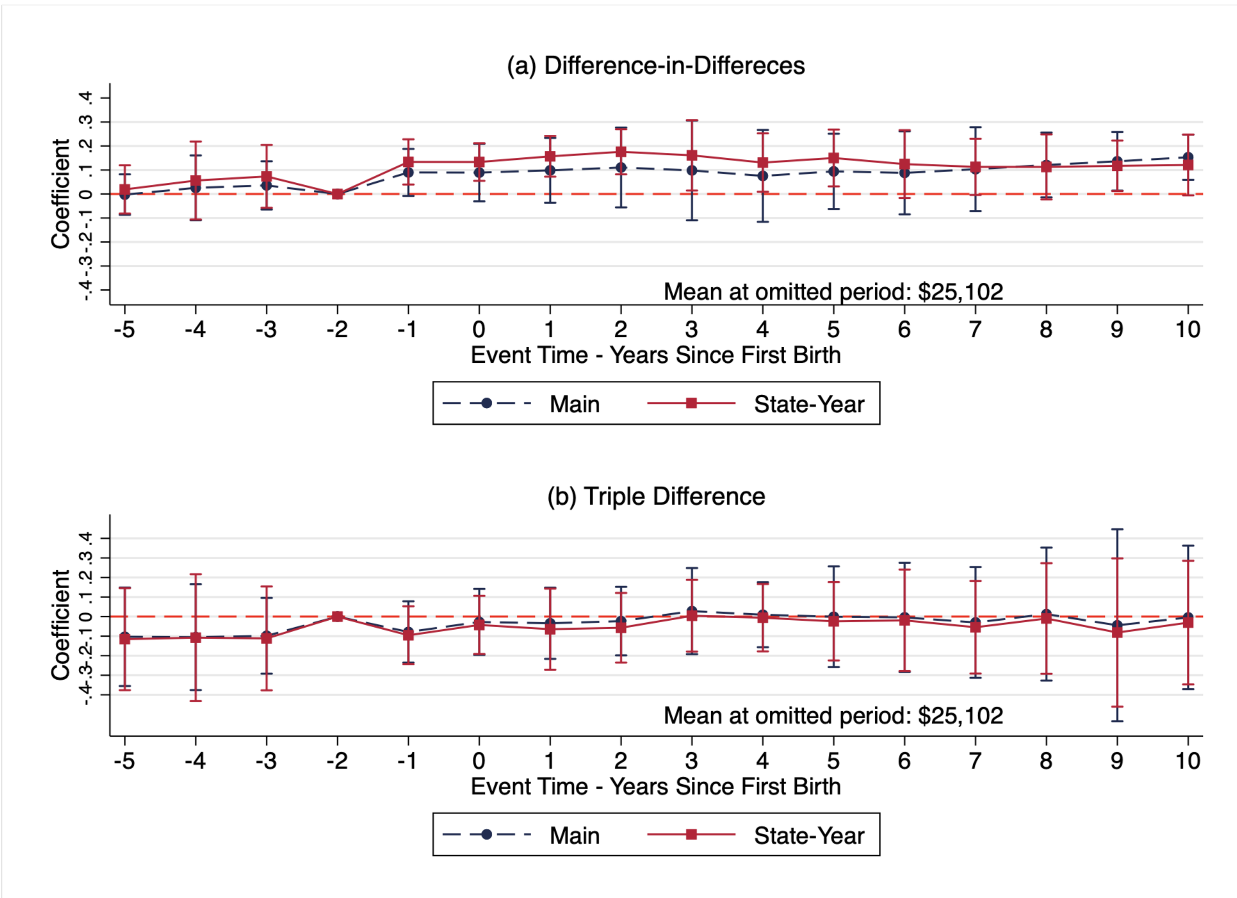
B Robustness Checks

Figure B1: Effect of Early Exposure to CDCC Expansion on Employment



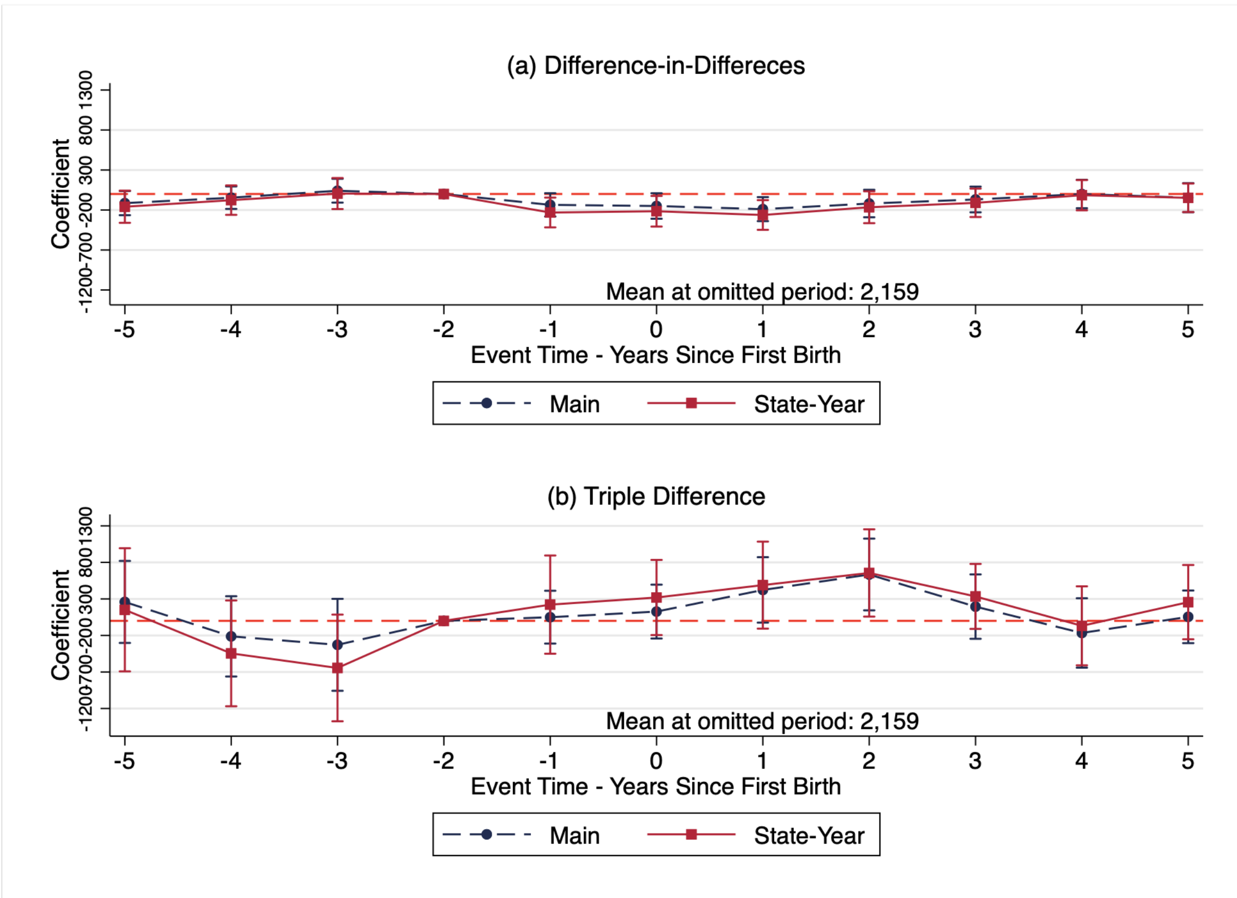
Panel (a) presents the dynamic difference-in-differences estimates and confidence intervals from equation (2) on changes in employment. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in employment. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid red line graphs estimates using state-year fixed effects and a vector of covariates. The vector of covariates includes a woman's highest level of education, race, and marital status. Data comes from the SSB with the sample restrictions outlined in Section III. Includes state and year fixed effects. Standard errors are clustered at the state-year level.

Figure B2: Effect of Early Exposure to CDCC Expansion on Log Earnings



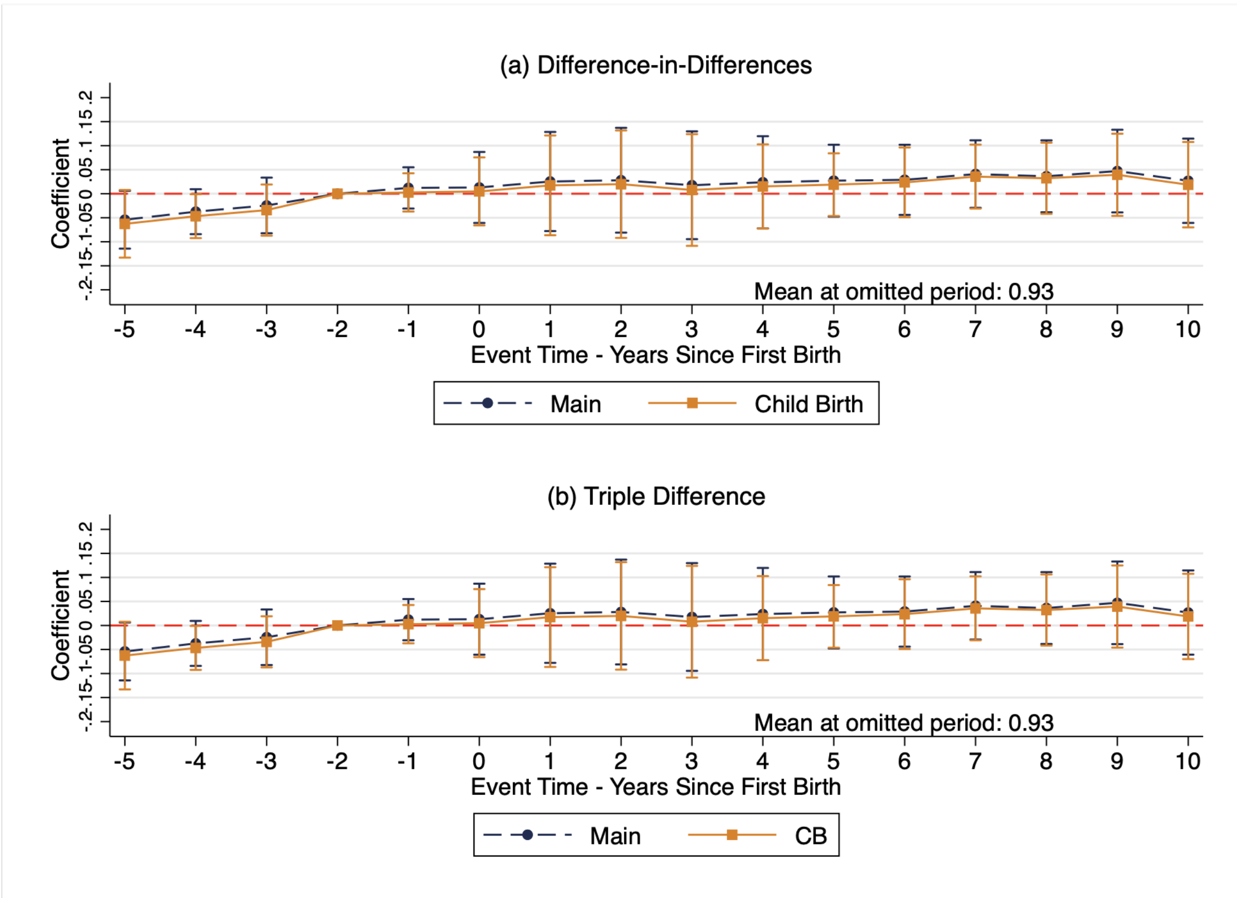
Panel (a) presents the dynamic difference-in-differences estimates and confidence intervals from equation (2) on changes in log earnings. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in log earnings. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid red line graphs estimates using state-year fixed effects and a vector of covariates. The vector of covariates includes a woman's highest level of education, race, and marital status. Data comes from the SSB with the sample restrictions outlined in Section III. Includes state and year fixed effects. Standard errors are clustered at the state-year level.

Figure B3: Effect of Early Exposure to CDCC Expansion on Total Annual Hours



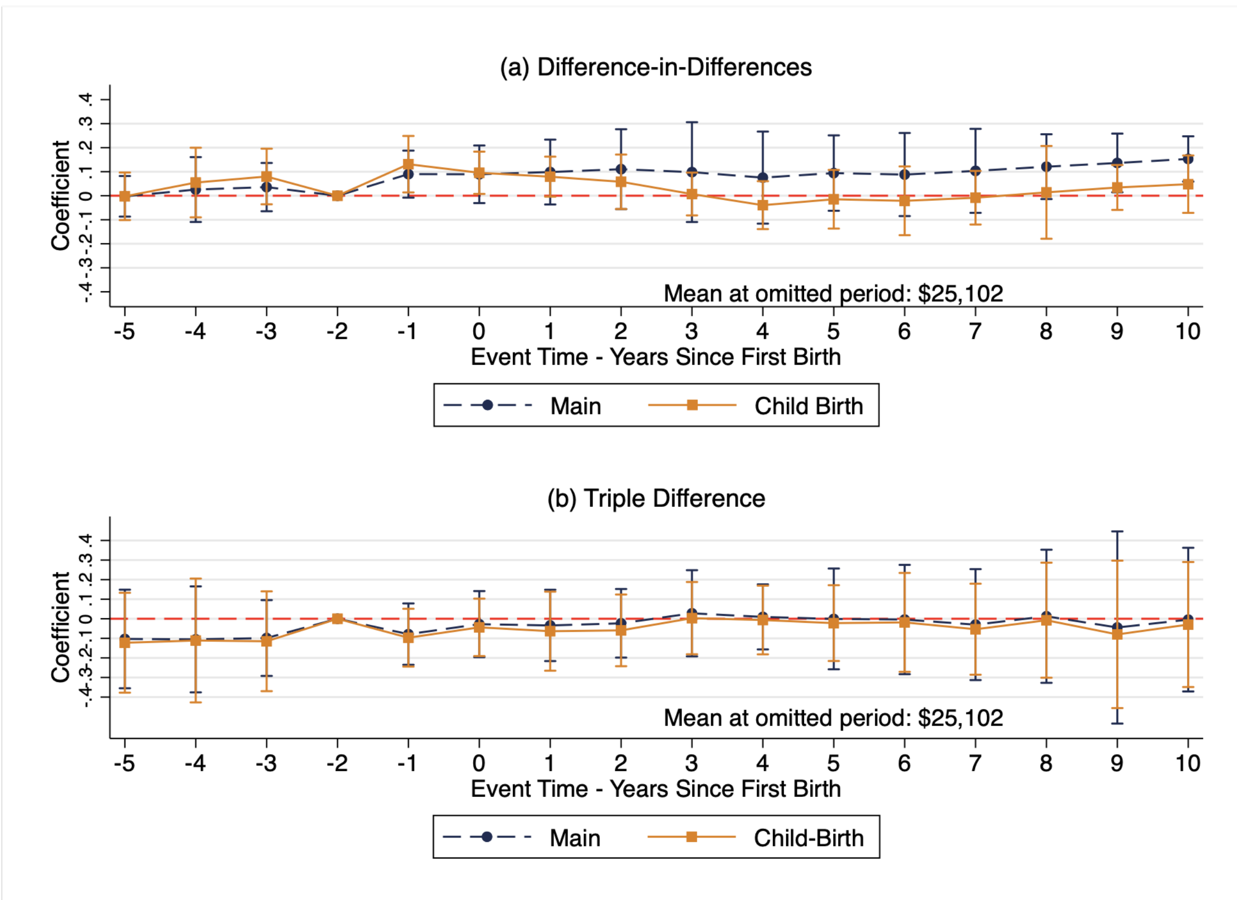
Panel (a) presents the dynamic difference-in-differences estimates and confidence intervals from equation (2) on changes in total annual hours worked. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in total annual hours worked. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid red line graphs estimates using state-year fixed effects and a vector of covariates. The vector of covariates includes a woman's highest level of education, race, and marital status. Data comes from the SSB with the sample restrictions outlined in Section III. Includes person and year fixed effects. Standard errors are clustered at the state-year level.

Figure B4: Effect of Early Exposure to CDCC Expansion on Employment



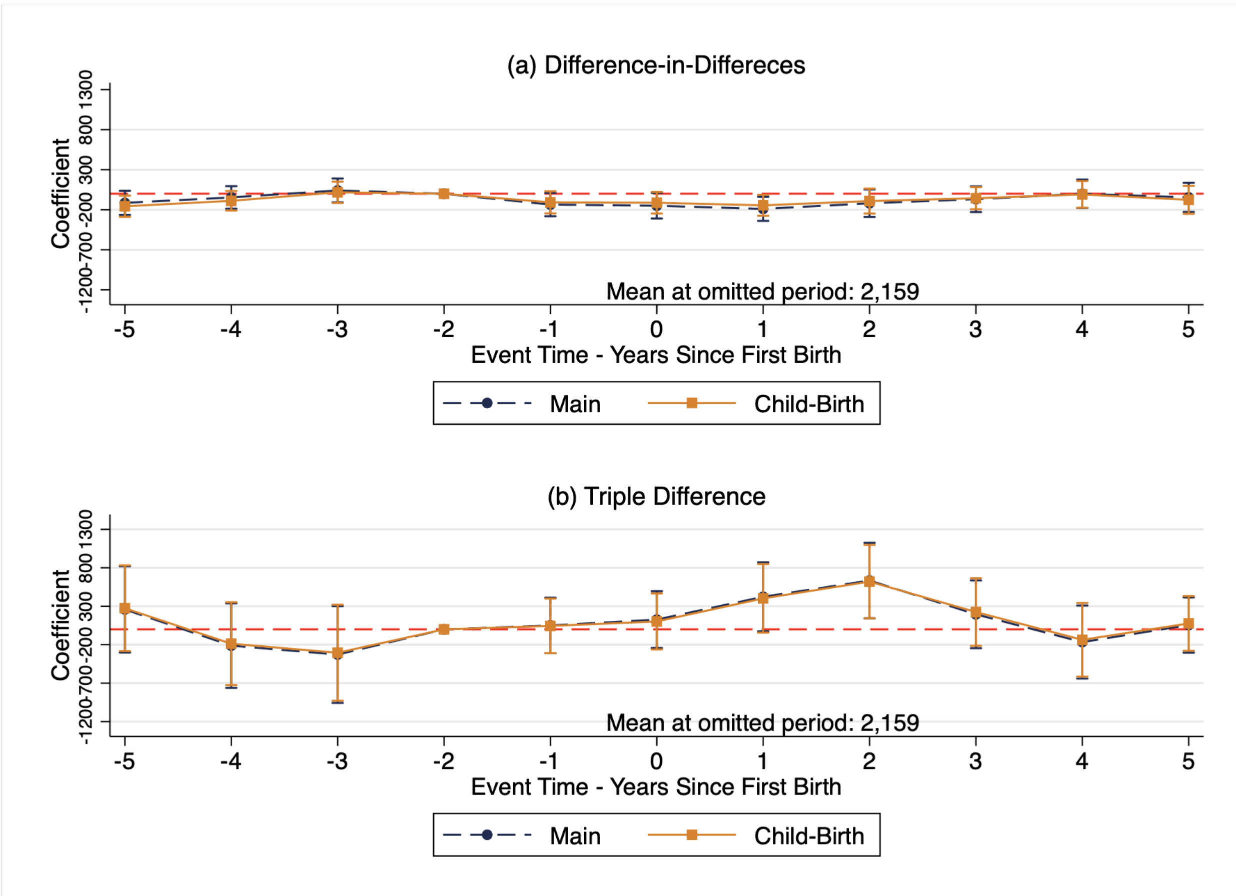
Panel (a) presents the dynamic difference-in-differences coefficients and confidence intervals from equation (2) on changes in employment. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in employment. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid orange line graphs estimates using year-of-childbirth fixed effects. Data comes from the SSB with the sample restrictions outlined in Section III. Includes person and year of first-birth fixed effects. Standard errors are clustered at the state-year level.

Figure B5: Effect of Early Exposure to CDCC Expansion on Log Earnings



Panel (a) presents the dynamic difference-in-differences estimates and confidence intervals from equation (2) on changes in log earnings. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in log earnings. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid orange line graphs estimates using year-of-childbirth fixed effects. Data comes from the SSB with the sample restrictions outlined in Section III. Includes person and year of first-birth fixed effects. Standard errors are clustered at the state-year level..

Figure B6: Effect of Early Exposure to CDCC Expansion on Total Annual Hours Worked



Panel (a) presents the dynamic difference-in-differences estimates and confidence intervals from equation (2) on changes in log earnings. Panel (b) presents the triple-difference estimates and confidence intervals from equation (3) on changes in log earnings. The dashed blue line graphs the estimates from the model using person-year fixed effects while the solid orange line graphs estimates using year-of-childbirth fixed effects. Data comes from the SSB with the sample restrictions outlined in Section III. Includes person and year of first-birth fixed effects. Standard errors are clustered at the state-year level.

C Appendix: Difference-in-Differences Model

A key empirical question is the marginal effect of an additional dollar of benefits on women's employment, earnings, and total hours worked. In this section, I leverage state-level variation in the CDCC. A total of 23 states and D.C. have passed state-level expansions to the CDCC, which can substantially increase the total benefit a family could receive. I estimate the effect of increased generosity given at different points after a woman's first birth using a dosage model. I construct a simulated instrument that estimates the average CDCC benefits a person would receive in a given state and year using TAXSIM. A key issue with using CDCC benefits is the endogeneity between the cost of childcare and the average credit amount, as states with high childcare costs may increase CDCC benefits. To overcome this issue, I construct a ratio of the simulated benefit to the average cost of childcare in a given state and year. The average cost of childcare is constructed using data from the Quarterly Workforce Indicators (QWI). As part of a heterogeneity analysis, I examine the effect on women eligible for the credit separately from women who would likely be ineligible for the credit using the same eligibility criteria as my previous strategy.

C.1 State-Level Credits

Since the credit's inception, a total of 23 states and D.C have passed legislation that increases the subsidy amount as a percentage of the federal subsidy amount. This means that federal expansions also increase the state-level generosity of the subsidy. There is a large amount of variation at the state-year level in subsidy amount, as well as the type of credit. Of these 13 states and D.C. are non-refundable and 10 are refundable.[12] States with a state-level expansion where the CDCC is non-refundable include: Arkansas, Colorado, Delaware, D.C., Georgia, Kansas, Maine, Maryland, New Jersey, Ohio, Oklahoma, Oregon, Rhode Island,

[12] Nebraska is only refundable for AGI under $29,000 or less, and Vermont is refundable for filers below $30,000 ($40,000 for married filers), I consider these states as refundable in my sample.

and South Carolina.[13]

Figure (3) graphs the state-level credits for my sample from 1994 to 2010. This graph is constructed using TAXSIM. The green line is the federal credit. There is a substantial amount of heterogeneity in credit generosity. In my sample, Delaware has the highest state credit. It increases the federal credit's generosity by 50 percent. Oklahoma has the smallest increase at 20 percent. Because state-level credits are calculated as a percentage of the federal expansion, all credits increase substantially in 2003.

C.2 Data

I use data from the SSB and the PSID for this analysis. In contrast to my main results, the results in this section have not been validated against the actual confidential data. The SSB links administrative records SSA/IRS to the 1984-2008 SIPP panels. The SSB is a synthetic data set that estimates the joint distribution of all variables in the data and takes random draws from the modeled distribution. The draws are then used to replace actual data to protect confidentiality. The data used for this analysis comes from four implicate files. The output presented in this paper averages estimates across these four implicates per Census Bureau recommendation. As with my main analysis, I categorize someone as employed if they have any positive earnings in a given year. All earnings information is converted into real 2014 dollars.

To examine how CDCC exposure affects hours of work, I use data from the PSID 1968-2019. The PSID began in 1968 with a nationally representative sample of 18,000 individuals belonging to 5,000 families. Since 1968 the PSID has followed these individuals and their descendants on an annual basis (biennial since 1997). They collect detailed economic and demographic information including employment, wages, earnings, total hours worked, education, marriage, and fertility. Respondents report their own labor market activities as well those of their spouses. I use this information to create full year-by-year histories of women

[13]Arkansas passed legislation to make the credit refundable in 2010. It was non-refundable during the period in my sample.

captured in the PSID. I assign parental history using the Child Birth and Adoption History File. The Child Birth and Adoption History File contains detailed information on the history of childbirth and adoption for respondents in the PSID, covering 1985-2019.

Due to data constraints, my sample includes individuals from 1994-2010 in both the sample for SSB and PSID samples. In my analysis, I estimate regressions separately based on eligibility for the CDCC. I categorize someone as eligible if they earned greater than $15,000 in the year prior to giving birth and ineligible if they earn $15,000 or less in the year prior to giving birth. In my sample, I exclude states, where the CDCC is refundable as individuals with very low income, would be able to claim the credit in those states.[14] All samples described in this section only include individuals who live in non-refundable states.

C.2.1 Childcare Cost Data and Synthetic Instrument

For my analysis, I construct a ratio of average CDCC benefits to the average cost of childcare benefits in a state and year. I use data from the QWI from 1994 to 2010 to calculate the average cost of childcare. The QWI is a Census data product that collects data on a number of economic indicators, such as quarterly earnings. This data can be broken down by NAICS industry groups. For this project, I draw data for individuals employed in childcare services. I then aggregate the data to the state-year level to measure the average annual earnings for childcare workers in each state and year.

Rather than relying on observed benefits, which are likely endogenous to both childcare utilization as well as labor supply, I construct a simulated instrument for CDCC benefits. Using Current Population Survey data accessed through IPUMS (Flood et al., 2022), I draw a nationally representative sample of respondents in 2002. I use 2002 because the 2003 expansion is the single largest expansion in my sample. Because state-level expansions are

[14]States, where the CDCC is not refundable, include: Alabama, Alaska, Arizona, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Idaho, Illinois, Indiana, Kansas, Maryland, Massachusetts, Michigan, Mississippi, Missouri, Montana, Nevada, New Hampshire, New Jersey, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, West Virginia, Wisconsin, and Wyoming

most often tied to federal credit and it caused a dramatic increase in benefits for many states. I link these respondents to the fourth wave of the SIPP 2001 panel, which includes information on childcare utilization and cost. I identify information about marital status, and if applicable, spousal wage. I then replicate this sample in each state and each year from 1994 to 2010. Childcare costs and wages are all converted into 2014 dollars. Using this replicated sample, I use TAXSIM to estimate the total CDCC benefits that each person in my sample would have received in each state and year. TAXSIM outputs information on both state and federal-level CDCC benefits, which I add together to construct the total benefits.

Table 6 presents survey-weighted descriptive statistics. I do not condition on employment prior to giving birth in this sample. Seventy-four percent of the sample is employed, with an average annual income of $34,379. The average annual cost of childcare is $6,806 while the average simulated subsidy amount is $451 over this time period.

C.3 Dosage Model

In my dosage model, I estimate the effect of different "doses" of credit around each event year using a difference-in-difference model. The model is estimated using a two-way fixed effect model, with state and year fixed effects. I estimate the effect of increases in the CDCC relative to the cost of childcare on employment, earnings, and total hours worked. CDCC benefits are conditional on employment, and should intuitively have a positive impact on labor market outcomes. Rather than focusing directly on CDCC benefits, I construct a ratio of synthetic CDCC benefits to estimated childcare as outlined in section 3.2. States that pass expansions to the CDCC may do so because of an increased cost of childcare, which may bias my results. For example, D.C. has a larger credit than Georgia, however, D.C. has. more expensive child care. While D.D. may have a much larger benefit, it may not have as much "bite" as the CDCC in Georgia because of differences in childcare costs. Taking the ratio allows me to analyze the "real" effect of CDCC benefits by controlling for the average cost of childcare. Following Callaway et al. (2021), I estimate the following difference-in-differences equation:

$$y_{igs\tau} = \alpha + \beta_1 ratio_{sg\tau} + X_i + \theta_t + \chi_s + \epsilon_{igs\tau} \tag{4}$$

Here $y_{ib\tau}$ is the outcome of interest (employment, log earnings, and total annual hours worked) for individual i, in the group g, in the year of first birth, b, in the year (relative to first birth), τ. I estimate this regression separately for eligible and ineligible individuals separately, where g indicates whether they are eligible or ineligible. I define eligibility using the same criteria as in my main analysis. Individuals who earn more than $15,000 in the year prior to giving birth are considered eligible, while those earning $15,000 or less in the year prior to giving birth are considered ineligible for the credit. $ratio_{st}$ represents the ratio of my simulated CDCC benefits in a given state and year to the estimated childcare cost in a given state and year multiplied by 100. CDCC benefits are calculated based on the benefits a woman would have received in that state and year. I include a group of women without children to whom I assign a benefit amount of zero. These women should not react to changes in the credit amount. I empirically test whether women without children respond to changes in the CDCC. X_i includes a vector of covariates including a respondent's education level, race, and marital status. θ_t captures year fixed effects and χ_s captures state fixed effects. I estimate this equation for all individuals, and then separately for eligible individuals and ineligible individuals. My coefficient of interest, β_1 captures the effect of a 1 percentage point increase in the ratio of CDCC benefits to childcare costs. I estimate this equation in each event year around birth.

While there is some evidence of pass-through, indicating that childcare workers may change their behavior in response to the CDCC, they should represent a relatively small portion of my sample. I empirically test this assumption by estimating the effect of an increase in the CDCC on women without children. Estimates support that women without children do not react to changes in the CDCC as my estimates are small in magnitude and are not statistically significant. I find that women without children are not more likely to work or have higher earnings in response to higher credit levels. These results are both statistically insignificant as well as small in magnitude.

Identifying the $ATT(d|d)$ requires several assumptions: (1) I need to include a group that never receives any treatment in any period. Here, I include a group of women without children. Women without children are never eligible to receive any portion of the subsidy. (2) No anticipatory effects. (3) Parallel trends, this requires that the path of outcomes that women with any dose, d, would have experienced if they had not participated in the treatment is the same as the path of outcomes that women in the untreated group (women without children) actually experience. This assumption states that, had they not had any children, mothers would have followed the same employment and earnings path.

Under these assumptions, β_1 captures the difference between the change in outcomes for mothers treated with a given dosage and women without children. In this paper, I am interested in comparing outcomes across different levels of the ratio of CDCC benefits to childcare cost, or the $ATE(d)$. In order to identify the $ATE(d)$ I must make a fourth, significantly stronger assumption, strong parallel trends. Strong parallel trends require that the average change in outcomes over time across all women if they had been assigned that amount of dose is the same as the average change in outcomes over time for all women that experienced that dose. This is stronger than assumption (3) because it requires assumptions about trends between different doses, d instead of just untreated potential outcomes. This allows for some selection in a particular dose but requires that on average across all does, there is no selection into a particular dose. This fourth assumption is violated if, for example, a parent systematically moves across state lines in order to have access to lower childcare and a higher amount of credit. This assumption is likely to hold in this context, as it is unlikely that parents would move across state lines to reap a relatively small change in benefits or childcare costs.

C.4 Results

Figure (11) graphs coefficients from equation (5) for individuals who would be eligible and individuals who would be ineligible for the subsidy. The dark green line plots coefficients for eligible individuals, the light green line plots for all individuals, and the green line plots for ineligible individuals. If the changes in employment are driven by differences in the CDCC benefits, we would expect that increases in employment would be driven by those who are eligible. Here, I define eligibility based on earnings in the year prior to giving birth. I categorize someone as eligible if they earn more than $15,000 in the year prior to giving birth and ineligible if they make $15,000 or less. Consistent with the effect being driven by differences in CDCC benefits, eligible individuals see the largest increase in the probability of employment. Estimates between eligible and ineligible individuals largely fall outside of each other's confidence intervals. Estimates suggest that women are forward-looking and increases in CDCC benefits lead to increases in the probability of employment prior to giving birth. Five years prior to giving birth, a one percentage point increase in the ratio of benefits to childcare cost is associated with 2 percentage points, or 2.7 percent more likely to be employed for eligible women. This result is not significantly different than zero for ineligible women. For both groups of women, the effect increases over time until the year prior to giving birth. Notably, the effect for both eligible and ineligible is nearly the same in the year prior to giving birth. In the year prior to giving birth, a 1 percentage point increase in the ratio of benefits to childcare costs is associated with a 3.4 percentage point increase in the probability of employment. At the mean, this would imply that holding the cost of childcare constant, a $100 increase in benefits is associated with a 1 percent increase in the probability of employment. This effect drops sharply for ineligible individuals to a 2.1 percentage point increase in the probability of employment. In both cases, the effect attenuates over time but remains statistically significant.

Figure (12) plots the effect of a 1 percentage point increase in the ratio of CDCC benefits to the cost of childcare cost on earnings in each year around their first birth. The dark green line plots coefficients for eligible individuals while the light green line plots the coefficients for individuals who are unlikely to be eligible for the subsidy. There is some evidence that a 1 percent increase in the ratio leads to decreases in earnings prior to giving birth, however, this effect is relatively small in magnitude and decreases leading up to the first birth. Five years prior to giving birth, a 1 percentage point increase in the ratio leads to a decrease in earnings by an average of $1,286. As a woman approaches her first birth, this effect decreases in magnitude and flips sign. A year prior to giving birth a 1 percentage point increase in the ratio is associated with an average increase of $800 in earnings, this effect is statistically significant at conventional levels. The effect is largest in the years following first birth. Two years after giving birth, a one percentage point increase in the ratio is associated with an average increase in earnings of $1,193. Results indicate large and significant effects for ineligible women. The effect increases in magnitude leading up to their first birth but then decreases after giving birth. While the actual effect is negative, it decreases in magnitude after giving birth, indicating that the policy may have a net positive effect.

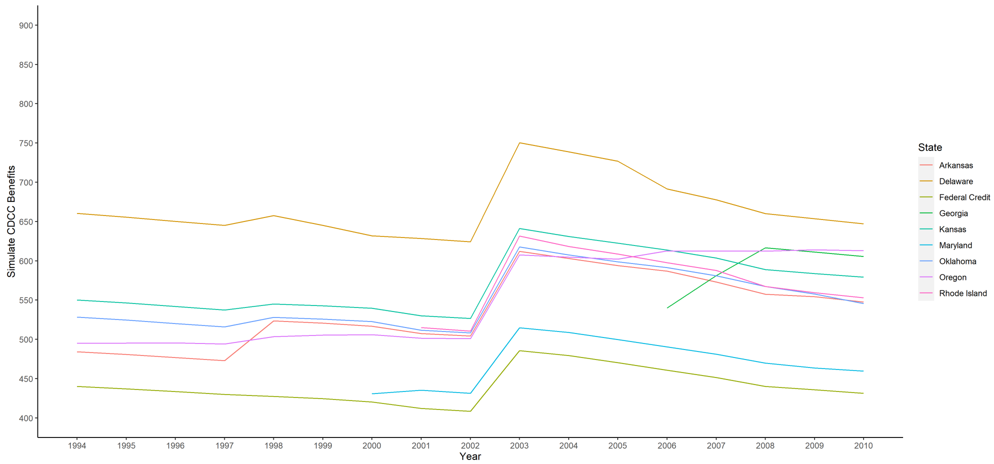
Figure (13) displays coefficients from a heterogeneity analysis where I estimate equation (5) for individuals who would be eligible and individuals who would be ineligible for the subsidy. The dark green line plots coefficients for individuals who would be eligible for the subsidy while the light green line plots coefficients for individuals who would be ineligible for the subsidy. Estimates suggest that increases in total annual hours worked prior to giving birth are driven by mothers who are likely eligible for the subsidy. A 1 percentage point increase in the ratio of benefits to childcare costs is associated with an average annual increase of 25 hours over the five years prior to giving birth. This is a modest increase of around half an hour per week. Estimates suggest that eligible mothers work less after giving birth, however, these estimates are not statistically different from zero and are small in magnitude. One to five years after giving birth, eligible women work an average of four hours less annually.

C.5 Conclusion

In this section, I consider the marginal effect of an additional $1 subsidy benefit on the probability of employment, earnings, and total annual hours worked. Using state-level expansions to the CDCC I construct a simulated instrument that captures changes in state-level CDCC benefits. Rather than directly estimating the effect of an additional $1 in benefit, I use a ratio of average CDCC benefits to the average cost of childcare in a given state and year. In effect, I am able to estimate the "real" effect of an additional $1 of subsidy benefit holding constant the cost of childcare.

My results suggest large labor market returns to CDCC benefits. These results are largely driven by increases in labor supply among eligible mothers, though there is some evidence that being ineligible also increases their labor supply. My results suggest that eligible women respond to the CDCC before giving birth, and this effect increases until the year before giving birth. In the year prior to giving birth, a 1 percentage point increase in the ratio of benefits to childcare costs is associated with a 3.4 percentage point increase in the probability of employment. At the mean, this would imply that holding the cost of childcare constant, a $100 increase in benefits is associated with a 1 percent increase in the probability of employment. This effect decreases as a woman's firstborn child ages but remains large. Eligible women also see an increase in earnings. In the year prior to giving birth, a 1 percentage point increase in the ratio is associated with an average increase of $804 in earnings. There is no statistically significant effect on hours worked after giving birth. These findings highlight the importance of childcare costs in women's labor market decisions, as they are larger in magnitude than the findings in the main results of this paper. Previous research on the CDCC has also found that most of the benefit amount passes through to providers. Rodgers (2018) finds that 75 cents of every dollar pass through to childcare providers in the form of higher costs and wages from childcare providers. If policymakers want to entice marginal women to remain employed, they must meaningfully increase the subsidy relative to the cost of childcare.

Figure C1: State-Level Expansions Simulated Instrument



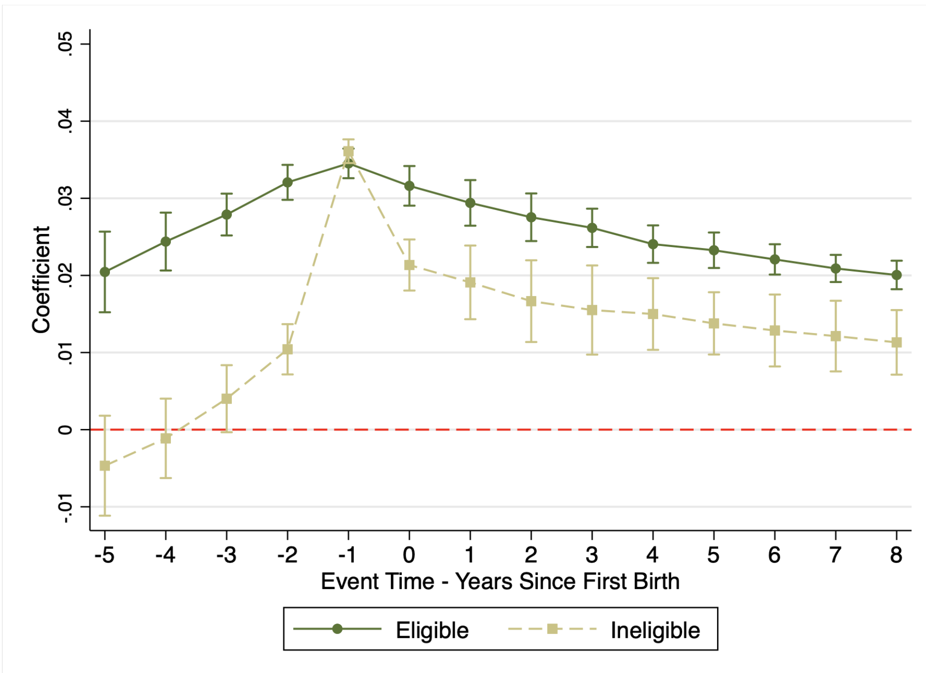
This graphic displays the simulated instrument of average CDCC benefits over time. Estimates are created using the method outlined in the data section.

Table C1: Characteristics of State CDCC Programs

Variable	
Employed	0.74
	(0.44)
Earnings	\$34,279
	(173051)
Childcare Cost	\$6,806
	(804.075)
CDCC	451
	(50)
N	312,171

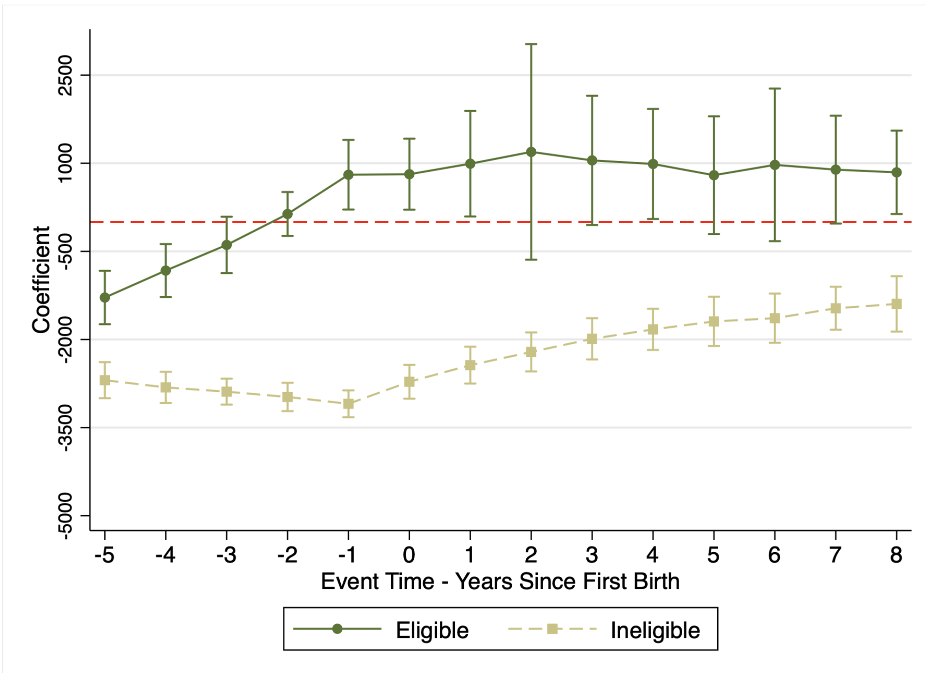
This table displays the survey-weighted descriptive statistics using the data as outlined in the data Section C2. Data comes from the SBB. Average CDCC and cost of childcare are calculated using methods outlined in C.2.1.

Figure C2: Effect of Ratio on Employment: Eligible vs Ineligible



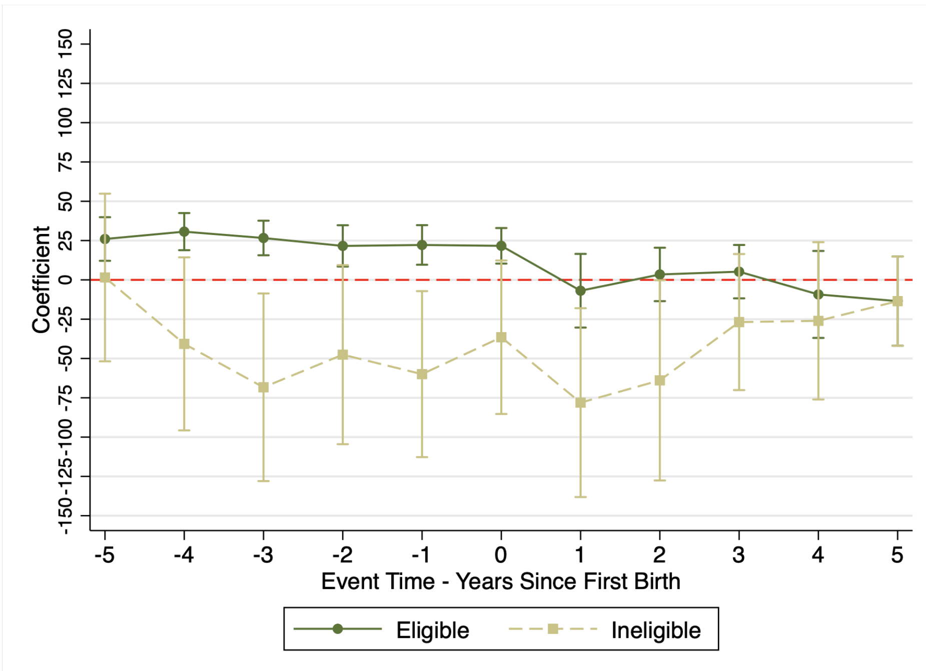
This figure present difference-in-differences estimates and confidence intervals from equation (5) on changes in employment at each event time. The solid dark green lines indicate the coefficients are for eligible individuals, the dashed light green lines indicate the coefficients are for ineligible individuals. Data comes from the SSB with the sample restrictions outlined in Section III. Includes state and year fixed effects. Standard errors are clustered at the state-year level.

Figure C3: Effect of Ratio on Earnings: Eligible vs Ineligible



This figure present difference-in-differences estimates and confidence intervals from equation (5) on changes in earnings at each event time. The solid dark green lines indicate the coefficients are for eligible individuals, the dashed light green lines indicate the coefficients are for ineligible individuals. Data comes from the SSB with the sample restrictions outlined in Section III. Includes state and year fixed effects. Standard errors are clustered at the state-year level.

Figure C4: Effect of Ratio on Total Annual Hours Worked: Eligible vs Ineligible



This figure present difference-in-differences estimates and confidence intervals from equation (5) on changes in earnings at each event time. The solid dark green lines indicate the coefficients are for eligible individuals, the dashed light green lines indicate the coefficients are for ineligible individuals. Data comes from the PSID with the sample restrictions outlined in Section III. Includes state and year fixed effects. Standard errors are clustered at the state-year level.

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