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The relationship between financial security and mental wellbeing: The impacts of income support programs on mental health

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Relationship of financial security & mental wellbeing: the impact of income support programs on mental health

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Dedication and Acknowledgements

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Contributions

A version of chapter 1 was previously published in Health Affairs (see citations below) in collaboration with Kaitlyn Jackson and Rita Hamad. It is substantially the product of Akansha Batra's years of study at the University of California, San Francisco and was primarily conducted and written by her. The work she completed for this published manuscript is comparable to a standard dissertation chapter.

Batra A, Jackson K, Hamad R. Effects of the 2021 Expanded Child Tax Credit on Adults' Mental Health: A Quasi-Experimental Study. *Health Aff*. 2023;42(1):74-82.

Approved:

Rita Hamad, Dissertation Chair

Relationship of financial security & mental wellbeing:

The impact of income support programs on mental health

Akansha Batra

Abstract

Over 19 million adults in the U.S. experienced at least one major depressive episode in 2019, representing 7.8% of the population. One of the strongest perceived risk factors for depression is financial hardship. Studies have shown that financial security is positively associated with self-reported measures of physical health and mental health, while financial distress is negatively associated. However, there is little evidence that examines economic interventions as a way to reduce mental distress and related social disparities.

Since poverty and financial hardship are major risk factors for stress and mental health problems, it is imperative to identify population-level policies to improve mental wellbeing among at-risk groups. This dissertation examines the role of income support programs on mental wellbeing among at-risk groups. It evaluates whether population-level policies like the Earned Income Tax Credit (EITC) or the 2021 temporary expansion of the Child Tax Credit (CTC) can improve mental wellbeing among economically disadvantaged and racial/ethnic minoritized subgroups.

Chapter 1 examines the effects of the temporary expansion of the Child Tax Credit by the U.S. Congress, which provided economic assistance for families with children. A rigorous quasi-experimental difference-in-differences approach was used to examine the effects on mental health and related outcomes among low-income adults with children and racial/ethnic subgroups. Using the Census Household Pulse Survey results found fewer depressive and anxiety symptoms among low-income adults, with Black adults showing greater reductions in depressive symptoms compared to White adults, and adults of Black,

Hispanic and other racial/ethnic backgrounds showing greater reductions in anxiety symptoms. These findings are important for Congress and state legislators to weigh when considering making the CTC and other similar tax credits permanent.

Chapter 2 explores how state-level factors could modify the impact of federal policies through the example of the Child Tax Credit. Using a quasi-experimental triple differences approach and nationally representative data from the Census Household Pulse Survey, this study provides new evidence regarding how state policies affect geographical disparities in mental health.

Chapter 3 investigates the impact of two child-related tax benefit policies, the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC), on postpartum health outcomes. Using Centers for Disease Control and Prevention (CDC) Pregnancy Risk Assessment Monitoring System (PRAMS) data from 2004 to 2021 findings showed that breastfeeding decreased, postpartum depression increased among Hispanic women, and postpartum visits were reduced for other minority racial subgroups. Future work is needed to understand the specific impacts of child-related tax benefits on different subgroups of women.

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List of Abbreviations

API – Asian Pacific Islander

ARPA – American Rescue Plan Act

CDC – Centers for Disease Control and Prevention

CI – Confidence Interval

COVID – Coronavirus Disease

CTC – Child Tax Credit

DC – District of Columbia

DDD – Difference-in-Difference-in-Differences

DID – Difference In Differences

EITC – Earned Income Tax Credit

GAD – Generalized Anxiety Disorder

HPS – Household Pulse Survey

IRS – Internal Revenue System

MICE – Multiple Imputation using Chained Equations

OBRA – Omnibus Budget Reconciliation Act

PCA – Principal Component Analysis

PDS – Postpartum Depressive Symptoms

PHQ – Patient Health Questionnaire

PRAMS – Pregnancy Risk Assessment Monitoring System

RD – Regression Discontinuity

SD – Standard Deviation

SNAP – Supplemental Nutrition Assistance Program

TANF – Temporary Assistance for Needy Family

TRA – Tax Reform Act

US – United States

WIC – Special Supplemental Nutrition Program for Women, Infants, and Children

Chapter 1: Effects of the 2021 Expanded Child Tax Credit on Adults' Mental Health: A Quasi-Experimental Study

Akansha Batra, Kaitlyn Jackson, Rita Hamad

Abstract

The U.S. Congress temporarily expanded the Child Tax Credit (CTC) during the COVID-19 pandemic to provide economic assistance for families with children. While formerly the CTC provided $2,000 per child for mostly middle-income parents, in July-December 2021 it provided up to $3,600 per child. Eligibility criteria were also expanded to reach more economically disadvantaged families. There has been little research evaluating the effect of the policy expansion on mental health. Using the Census Household Pulse Survey (N = 812,314) and a quasi-experimental study design, we examined effects on mental health and related outcomes among low-income adults with children and racial/ethnic subgroups. We found fewer depressive and anxiety symptoms among low-income adults. Black adults demonstrated greater reductions in depressive symptoms compared with White adults, and adults of Black, Hispanic and other racial/ethnic backgrounds demonstrated greater reductions in anxiety symptoms. There were no changes in mental healthcare utilization. These findings are important for Congress and state legislators to weigh as they consider making the CTC and other similar tax credits permanent to support economically disadvantaged families.

Introduction

During the COVID-19 pandemic, there was a rapid rise in anxiety and depressive symptoms, disproportionately impacting economically disadvantaged families and people of color.(1) In June 2020, 37.8% of White adults reported adverse mental or behavioral health symptoms compared with 44.2% among Black adults and 52.1% among Hispanic adults.(2) Racial/ethnic minority groups were at increased risk of chronic stress in the pandemic as they were more likely to experience financial hardships and exacerbations of longstanding inequities in income, housing, and other social determinants of mental health.(3-7) Since poverty and financial hardship are major risk factors for stress and mental health problems, it is imperative to identify population-level polices to improve mental wellbeing among at-risk groups. Economic policies have the potential to affect mental health by addressing the social determinants of mental health like poverty, food insecurity, and healthcare access (see diagram, Appendix Figure 1).(8-10) These mechanisms and mental health itself can then affect physical health in the long run.(11)

In response to the financial hardship caused by the pandemic, in July 2021 the U.S. government expanded the Child Tax Credit (CTC), an economic support program for families with children, as part of the American Rescue Plan Act.(12, 13) The CTC was established in 1997 to provide financial relief for middle-income families. While formerly the CTC provided up to $2,000 per child, as part of the temporary 2021 expansion it provided a maximum of $3,600 per child; it was also made fully refundable to low-income and unemployed parents. Additionally, instead of being transferred in the form of an annual tax refund, in 2021 it was disbursed as monthly advance payments that were automatically transferred into the bank accounts of eligible families who had filed taxes in 2019 or 2020. Prior to the CTC expansion, the credit was not fully refundable, i.e., one third of American children did not receive the full value of the benefit because their families did not earn enough.(14) Children with single parents, those living in rural areas, Black and Hispanic children, and those in larger families were disproportionally ineligible.(14, 15) In contrast, about 90% of children were eligible for the expanded CTC, which was fully refundable, and benefits were larger for lower-income families.(16)

There has been limited work examining the effects of the expanded CTC, with studies suggesting it reduced child poverty by nearly half, and reduced material hardship and food insufficiency.(17-22) There are no studies to our knowledge on its mental health effects. There is, however, existing evidence on another major poverty alleviation program for low-income families with children, the Earned Income Tax Credit (EITC), showing the potential promise of poverty alleviation programs more generally. For example, the EITC has been shown to improve family income, housing, and access to health insurance, and to improve stress and mental health.(23-30) Studies suggest that the EITC's benefits have particularly benefited Black families.(8, 31) Yet the EITC is disbursed as an annual refund rather than monthly payments, and individuals must work to receive it, so EITC studies do not necessarily generalize to the potential impacts of the CTC, with its monthly payments and near-universality (including broader coverage of immigrant families).

This study addresses this critical gap by examining whether the 2021 CTC expansion improved mental health among adults with children, and specifically for low-income individuals and racial/ethnic minorities. Because of historical marginalization and structural racism, these groups have less wealth and lower income on average than higher-income and White individuals and therefore may have benefited more from this new source of income.

The expanded CTC expired at the end of 2021, and Congress continues to debate whether to make the expansion permanent, while state governments consider their own similar programs.(32) Evidence is therefore urgently needed to inform such conversations.

Methods

Sample

The sample was drawn from the U.S. Census Household Pulse Survey (HPS), a nationally representative repeated cross-sectional internet survey that began in April 2020 and continues weekly through the present.(33) The Census Bureau randomly selects HPS participants, who then complete an internet-based survey. We used data from waves 28-41 (April 14, 2021, to January 10, 2022) (N=944,189). Since the first monthly payment for the expanded CTC was made on July 15, 2021 (just before wave 34) and the last payment was made on January 15, 2022 (during wave 41), this provides 6 waves of pre-policy and 7 waves of post-policy data. Of note, a final larger lump-sum CTC payment was made during the spring of 2022 to those who filed taxes or claimed economic impact payments; our approach excluded observations during this period because of the ambiguity regarding defining the exposure period and potential recipients. Finally, we restricted the sample to respondents who provided responses on the mental health outcomes of interest (N = 812,314).

Exposure

CTC-eligible individuals with children under 18 whose interviews occurred during July-January 2022 were considered "exposed" to the expanded CTC. Furthermore, those with lower incomes were considered to have received the strongest exposure, since their benefits were larger than those with higher incomes.

In particular, the 2021 expansion increased CTC benefits from a maximum of $2,000 to a maximum of $3,600 per child for children under age 6 years, and up to $3,000 per child aged 6-17. Instead of being disbursed as part of an annual tax refund, the payment mode was changed to monthly advance payments. The full credit was available to single filers, heads of household, and married couples filing jointly with modified adjusted gross incomes under \$75,000, \$112,500, and \$150,000, respectively, for the 2021 tax year. This included those with zero earned income. The credit was phased out when income exceeded these thresholds. The first phase-out occurred when income exceeded these thresholds but was below \$400,000 (married filing jointly) or \$200,000 (all other filing statuses). The total credit per child was reduced by \$50 for each \$1,000 (or a fraction thereof). The credit would not be reduced below \$2,000 under this phase-out. The second phase-out applied to taxpayers with income more than \$400,000 (married filing jointly) or \$200,000 (other filing statuses). In this phase-out, the total credit per child was reduced \$50 for each \$1,000, and the credit could drop below \$2,000 until it reached \$0. Prior to the 2021 expansion, the CTC was not available to those with earnings below \$2,500, and those with lower incomes did not earn enough to qualify for the full amount (i.e., it was not fully refundable). Because of these changes to eligibility criteria, 88% of American families with children (39 million households) were eligible to receive payments beginning July 15, 2021.(34)

In this analysis, we assumed that eligible people received the credit (an approach similar to prior studies of the EITC and other safety net programs where administrative data on benefit receipt is unavailable).(8, 25, 26, 35) Notably, 65.4% of our sample who seemed eligible based on their self-reported demographic characteristics reported that they received the CTC, which indicates that our approach may involve some degree of measurement error. Notably, prior work has indicated that self-reported receipt of safety net benefits is unreliable; this may especially be the case if individuals were not aware of the automatic deposits into their bank accounts.(36)

Outcomes

We included several mental health outcomes measured in the HPS. First, depressive symptoms were captured using the two-item Patient Health Questionnaire (PHQ-2). In the PHQ-2, respondents are asked how often they have been bothered by 1) having little interest or pleasure in doing things and 2) feeling down, depressed, or hopeless. Answers range from "not at all" to "nearly every day." The two items are typically combined, and scores ≥3 indicate high risk of depression.(37)

Second, the two-item Generalized Anxiety Disorder (GAD-2) scale is a brief screening tool for generalized anxiety disorder. Individuals are asked if they are 1) feeling nervous, anxious, or on edge, and 2) not able to control or stop worrying in the past two weeks; and again how often they experience these symptoms.(38) A GAD-2 score ≥3 is considered high risk for anxiety.

We also included two binary outcomes capturing mental healthcare utilization, including mental health counseling or therapy within the last 4 weeks, or medication to help with emotions, behavior, or concentration.

Covariates

We adjusted models for variables representing potential confounders of the relationship between CTC receipt and the outcomes: gender, race/ethnicity, income, marital status, number of children, and education. We also included fixed effects for bi-weekly survey wave to account for secular trends in mental health that occurred during this period due to underlying factors affecting all individuals.

Statistical Analysis

Primary Analysis

We first calculated descriptive statistics stratified by (1) whether households included children and (2) whether the interview was conducted after the CTC expansion. We then estimated the effect of the expansion using a difference-in-difference-in-differences (i.e., triple-difference, or DDD) approach. DDD analysis builds on traditional difference-in-differences (DID) analysis, which is a quasi-experimental technique suited to examining the effects of policy changes while accounting for underlying trends.(39, 40) These methods compare pre-post changes in outcomes among a "treatment" group (i.e., adults with children), while "differencing out" underlying secular trends in outcomes in a "control" group (i.e., adults without children). The triple-difference approach enables further refinement of the treatment and control groups to estimate the effects on subgroups most affected by the policy. Specifically, we included an additional set of interaction terms between the primary exposure variable and a binary variable for whether an individual's income was below $35,000. This is because the lowest-income households were the primary beneficiaries of the expanded CTC, as they were more likely to be newly eligible and to receive the largest payments.

The triple interaction term in DDD models was therefore composed of three variables: (1) an indicator for whether the interview occurred after (versus before) the CTC expansion, (2) an indicator variable for adults with (versus without) children; and (3) an indicator variable for whether the individual belonged to a lower (versus higher) income group. The equation for the analysis and additional details about model assumptions are included in the Appendix, including Appendix Figure 1.2, Appendix Table 1.1, Appendix Table 1.2.

Secondary Analyses

Subgroup Analyses

We evaluated whether the CTC had a greater impact on mental health among racial/ethnic subgroups that may be more likely to benefit from the income boost. To do so, we conducted additional DDD analyses, including an interaction term between race/ethnicity and the primary exposure variable (i.e., the interaction between pre-post expansion and adults with versus without children).

Sensitivity Analyses

We conducted two sensitivity analyses. First, we assessed whether there were changes in the effects of the monthly CTC payments over time (for example, whether mental health improved initially but then returned to baseline). To do so, we modified the main analysis to include a categorical variable for bi-weekly survey wave instead of using a binary pre-post variable to represent time. Second, to account for missing values for key covariates, we conducted multiple imputation using chained equations (see Appendix).

Results

Sample Characteristics

The final sample included adults with children (112,862 observations before and 145,429 after the CTC expansion) and adults without children (237,901 observations before and 316,122 after the expansion) (Table 1.1). Adults with children were more likely to be younger, married, Hispanic, Black, and less educated compared to adults without children. Indicators of mental health were worse among adults with children. Importantly, DID analysis does not require that characteristics of the treatment and control group be similar, but rather that trends (i.e., slopes) in outcomes be parallel during the pre-revision period. Descriptions of the results of analyses to evaluate the validity of model assumptions are provided in the Appendix.

Effects of CTC Expansion

The CTC expansion was associated with decreased depressive (-1.7, 95% CI: -2.6, -0,7) and anxiety (-3.4, 95% CI: -4.5, -2.4) symptoms among low-income adults with children (Figure 1). We did not observe an association with utilization of mental health services or prescriptions.

Secondary Analyses

In subgroup analyses by race/ethnicity (Figure 1.2), there was a larger decrease in both depressive and anxiety symptoms among Black adults compared with White adults with children (interaction term coefficient for Black versus White -1.4 for depressive symptoms, 95% CI: -2.9, -0.00; -2.3 for anxiety symptoms, 95% CI: -3.9, -0.7). Adults of Hispanic and other racial/ethnic backgrounds also experienced greater reductions in anxiety compared with White adults (interaction term coefficient for Hispanic -2.3, 95% CI: -3.9, -0.7; interaction term coefficient for other racial/ethnic groups -3.3, 95% CI: -5.2, -1.4). There were no differences for Asian families compared with White families for and outcomes, and there were no significant differences by race and ethnicity in mental healthcare utilization (Appendix Figure 1.3).

In the secondary analysis in which we examined whether the mental health effects of monthly CTC payments changed over time, anxiety symptoms lessened, on average, soon after the payments started and remained relatively stable over time (Appendix Figure 1.4).

Depressive symptoms, which are arguably a more serious adverse mental health outcome, began decreasing after several payments had been disbursed. In the secondary analysis in which we imputed missing values for income, the results were similar to findings for the main analysis, suggesting that complete case analysis omitting those with missing income did not contribute to bias (Appendix Table 1.3-1.4).

Discussion

During the COVID-19 pandemic, the Child Tax Credit was temporarily expanded to millions of families for the first time, allowing 27 million additional children from the most economically disadvantaged families to receive the full benefit size.(41) This study examined the effects of this increased income on mental health among adults with children using a large serial cross-sectional national data set and rigorous quasi-experimental analyses. We found that the expanded CTC was associated with reduced anxiety symptoms among low-income adults with children, as well as greater mental health benefits among Black and Hispanic individuals. Previous studies have also shown a link between financial hardship and mental health.(42, 43) In the overall sample and among each subgroup, there was no change in mental healthcare visits or prescriptions, suggesting that healthcare utilization was not the primary pathway explaining the results.

The reduction in the prevalence of clinically meaningful anxiety symptoms (3.4% points) represents a 13.3% reduction from baseline anxiety levels (25.5%) among adults with children. While this may be a modest change in risk at the individual level, this represents a meaningful change in the distribution at the population level,(44) particularly considering the challenging pandemic-related circumstances during which it was implemented, and potential cumulative effects if the benefit were to be extended. The effect size is consistent with prior research finding that the other major U.S. anti-poverty program—the EITC—also improves long-run mental health among recipients.(23, 45) In fact, one prior paper examining the short-term impacts of the EITC found no effects on mental health;(46) it may be that the more regular payments of the expanded CTC were more effective in this respect. Additionally, while receipt of some public benefits may lead to feelings of stigma that reduce participation or worsen mental health,(47-49) the expanded CTC benefit was nearly universal with few administrative burdens among those who received automatic benefits, perhaps allowing it to be more impactful for mental health.(50)

We also noted that the mental health benefits of the CTC expansion were largest among adults of Black, Hispanic, and other racial/ethnic backgrounds. Of note, these groups stood the most to gain from the expanded CTC. During the COVID-19 pandemic, Black and Hispanic families reported higher rates of job loss, 44 percent and 38 percent in October 2021 respectively, compared to 23 percent for White families, with similar disparities during earlier periods.(51) Due to historical and current structural racism and marginalization, these groups also have less wealth and therefore less ability to withstand acute and chronic economic adversity.(8, 31, 52) Hispanic families are also more likely to be ineligible for other safety net policies because of immigration status, perhaps making the CTC a more salient program for them. For example, the federal EITC is only available to U.S. citizens and permanent residents, while the CTC was available to mixed immigration-status families as long as the child had a social security number. In contrast, we found that Asian individuals benefited similarly to White individuals. While Asians overall are likely to be of higher socioeconomic position than other communities of color, this may mask disparities within this heterogeneous group.

When examining one possible mechanism through which the increased income from the CTC may have improved mental health, we found no changes in mental healthcare utilization or prescriptions, suggesting that these were not the primary pathway explaining the reductions in depressive and anxiety symptoms, at least in the short term and in context of altered utilization patterns during the pandemic. However, recent studies using this data set and similar study designs have noted that the monthly CTC payments resulted in reductions in markers of financial hardship, with improved food sufficiency and more confidence in the ability to pay for housing.(18, 53) This is consistent with prior studies that have also shown that food sufficiency and reduced financial hardship are associated with improved mental health.(54-56)

This study has several strengths, including the use of a large serial cross-sectional diverse national data set, and a rigorous quasi-experimental study design. It provides timely evidence on a policy which is actively being debated by federal and state legislatures. The study also has limitations. One is that the HPS is a repeated cross-sectional survey, so we cannot observe changes in specific individuals' mental health after receiving CTC benefits as we could in a panel dataset. Additionally, HPS suffers from a high rate of non-response as with many other national surveys; results therefore may not generalize to those not included in this study. Another limitation is that covariates and outcomes were self-reported and may suffer from standard reporting biases. Finally, as with any DID analysis, there may be residual confounding based on contemporaneous policy changes or other exposures that differentially affected the treatment and control groups; we evaluated several model assumptions to lessen concerns about this issue.

The 2021 CTC expansion reduced child poverty by half, but its expiration caused millions of children to fall back into poverty.(19) Our study adds to a small but growing body of work that shows that the CTC not only increased food sufficiency but also improved mental health among adults with children, particularly the most marginalized groups. By reducing financial hardships, this policy has the potential to improve the environments in which vulnerable low-income children grow up. This study used a large serial cross-sectional diverse national data set and a rigorous quasi-experimental study design, providing timely evidence on a policy that is actively being debated by federal and state legislatures. These findings are important for Congress and state legislators to weigh as they consider making the CTC and other similar tax credits permanent to support economically disadvantaged families, particularly as the economic recovery from the pandemic drags on, and as already marginalized families continue to be left behind.

Table 1.1. Sample characteristics.

	Before July 15,2021		After July 15,2021	
	Adults without children	Adults with children	Adults without children	Adults with children
	Mean (SD) or Percent			
Age	57.3 (15.3)	44.8 (11.9)	56.1 (15.9)	44.0 (11.9)
Male	42.7	36.9	42.9	36.6
Married	53.9	70.7	52.7	69.9
Less than high school or high school	11.9	13.1	12.2	13.2
Income				
Less than $25,000	10.7	8.9	11.7	9.5
$25,000, $34,999	8.9	7.6	9.3	7.5
$35,000, $49,999	10.9	8.9	11.4	9.0
$50,000, $74,999	18.2	14.9	18.1	14.6
$75,000, $99,999	14.8	13.9	14.6	13.7
$100,000, $149,999	17.9	20.4	17.3	20.3
$150,000, $199,999	8.8	10.6	8.2	10.8
$200,000 and above	9.8	14.8	9.5	14.5
Race/Ethnicity				
Non-Hispanic White	79.3	68.9	78.9	68.6
Non-Hispanic Black	6.3	8.5	6.4	8.7
Asian	4.5	6.8	4.4	6.5
Hispanic	6.0	10.2	6.4	10.4
Other	3.9	5.7	3.9	5.8
Mental Health Outcomes				
Depressive symptoms (continuous)	1.5 (1.8)	1.6 (1.8)	1.3 (1.7)	1.4 (1.8)
Depressive symptoms (score ≥ 3)	16.4	18.4	17.2	19.9
Anxiety symptoms (continuous)	1.8 (1.9)	2.1 (2.0)	1.5 (1.9)	1.9 (2)
Anxiety symptoms (score ≥ 3)	20.1	25.5	21.6	29.3
Secondary Outcomes				
Utilization of mental health services	16.5	21.8	18.4	23.3
Mental health prescription	22.4	23.6	23.9	24.6
Confident in ability to pay mortgage/rent	78.3	72.3	75.8	70.3
Difficulty with household expenses	34.8	45.1	37.5	49.9
Food sufficiency	81.3	74.0	80.4	73.6
N	237,901	112,862	316,122	145,429

Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: N = 812,314. Data were drawn from the Household Pulse Survey, April 14, 2021-January 10, 2022, including individuals with non-missing information on the mental health outcomes of interest. Depressive symptoms were captured using the Patient Health Questionnaire-2 scale, and anxiety symptoms were captured using the Generalized Anxiety Disorder-2 scale; both were dichotomized at the standard cut-off of 3 or more to indicate high risk of mental health problems. Not married category includes single, divorced, widowed, separated. SD: standard deviation.

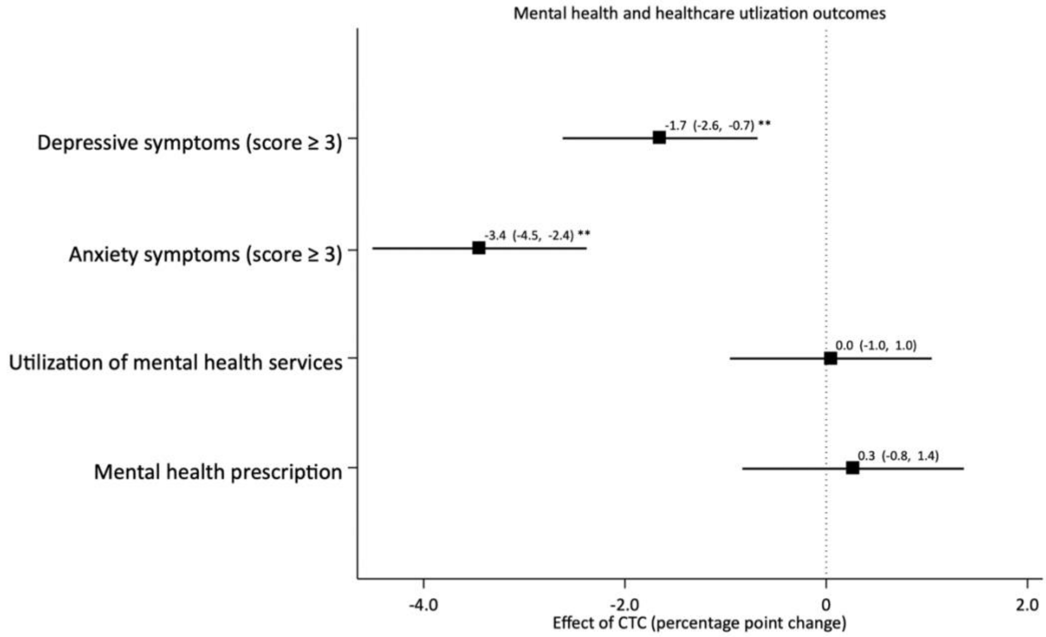


Figure 1.1. Effects of the 2021 Child Tax Credit expansion on mental health and healthcare utilization.
Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: **$p < 0.01$, *$p < 0.05$. Coefficients are plotted as point estimates (boxes) with 95% confidence intervals (whiskers). Coefficients are derived from models in which the primary exposure is a triple interaction term between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing adults with (versus without) children, and a binary variable for whether the interviewee belonged to a lower (versus higher) income group. All regressions adjusted for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves. Depressive symptoms were captured using the Patient Health Questionnaire-2 scale, and anxiety symptoms were captured using the Generalized Anxiety Disorder-2 scale; both were dichotomized at the standard cut-off of 3 or more to indicate high risk of mental health problems.

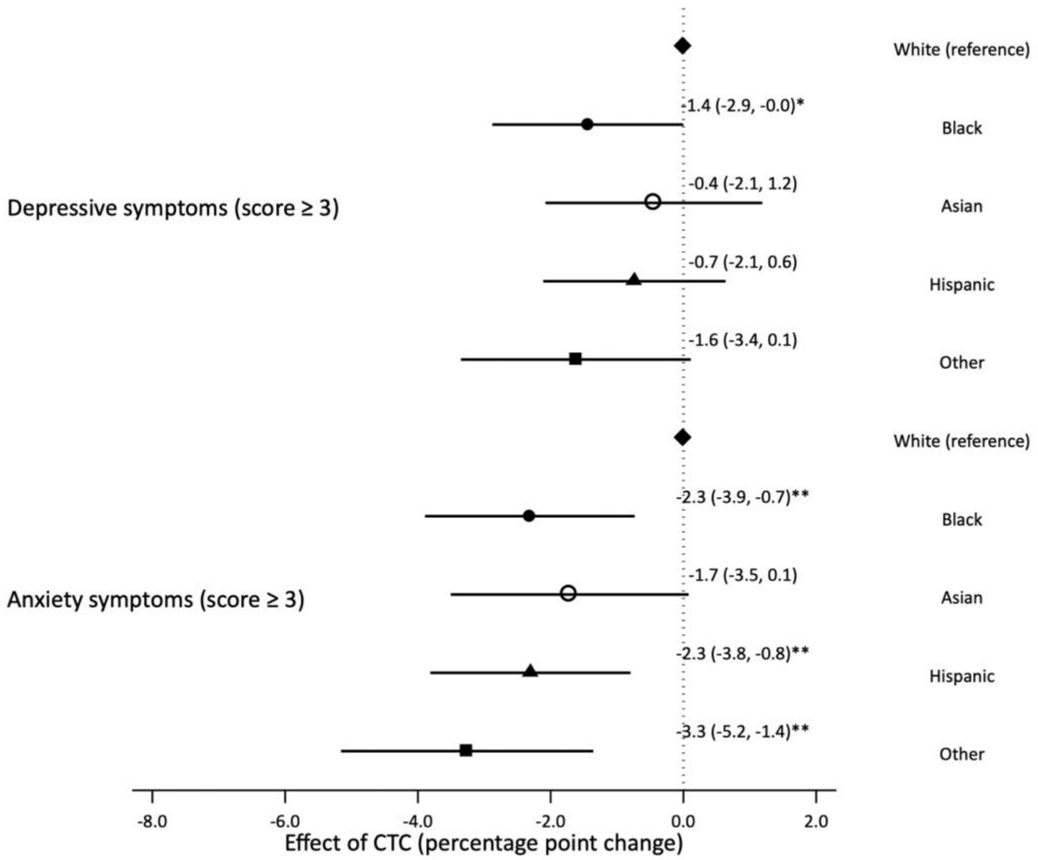


Figure 1.2. Racial differences in the effects of the 2021 Child Tax Credit expansion on mental health.
Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: **$p < 0.01$, *$p<0.05$. Coefficients are plotted as point estimates (boxes) with 95% confidence intervals (whiskers). Coefficients are derived from models in which the primary exposure is a triple interaction term between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing adults with (versus without) children, and a binary variable for whether the interviewee belonged to a given racial/ethnic group (reference category: White). All regressions adjust for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves. Depressive symptoms were captured using the Patient Health Questionnaire-2 scale, and anxiety symptoms were captured using the Generalized Anxiety Disorder-2 scale; both were dichotomized at the standard cut-off of 3 or more to indicate high risk of mental health problems.

APPENDIX

Supplemental Methods

Difference-in-difference-in-differences Analysis

The equation for the difference-in-difference-in-differences (DDD) model is:

$$
\begin{aligned}
Y_i = \alpha + \beta_1 Post_i x\ Children_i x\ Lowincome_i + \beta_2 Children_i x\ Lowincome_i \\
+ \beta_3 Children_i x\ Post_i + \beta_4 Lowincome_i x\ Post_i \\
+ \beta_5 Children_i + \beta_6 Post_i + \beta_7 Lowincome_i + \beta_8 Covars_i + \beta_9 Week_i + \varepsilon_i
\end{aligned}
$$

Y represents a mental health outcome of interest for each individual i. The variable $Children$ indicates whether households include children under 18. The variable $Post$ indicates whether the observation was recorded after Child Tax Credit (CTC) payments began in July 2021. $Lowincome$ in our analysis indicates whether the total household income is less than \$35,000. We included all two-way and three-way interactions between these three variables. $Covars$ represents individual-level covariates described in the main text, and $Week$ represents fixed effects for week of survey completion. The coefficient of interest is β_1, on the triple-interaction term, which represents the effect of the policy on low-income families with children. As is standard in difference-in-differences (DID) analyses, we used linear models for both continuous and binary outcomes, since interaction terms have different interpretations in non-linear models (57). For binary outcomes, analyses therefore represent linear probability models, and the coefficient can be interpreted as the percentage point change in risk.

Our analysis did not include survey weights, since the appropriateness of weights is diminished when adjusting for variables related to the sampling strategy and when the goal of modeling is causal inference rather than descriptive population characteristics (58).

Model Assumptions

DID models rely on several assumptions. The first is that, in the absence of treatment, no differences in the trends in outcomes would exist between the treated and control groups. For example, one possible violation of this assumption may stem from the fact that the reference period for the GAD-2 and PHQ-2 questions shifted from the "last 7 days" in phase 3.1 of the survey (weeks prior to July 5, 2021) to the "last 2 weeks" in phase 3.2 (weeks after July 21, 2021). This may lead to a change in the percent of people who answer affirmatively to these questions from the pre- to the post-period, although such changes may also be due to other outside societal factors, e.g., related to pandemic-related stressors. As long as this change is non-differential between the treatment and control groups (a key assumption of DID analyses), this should not lead to bias in our estimates, since a DID design is ideally suited to subtracting out secular trends in the outcomes using the control group as a reference. Also, the order of the questions also changed during phase 3.2, which might lead to different non-response patterns. Reassuringly, we found that average rates of missingness for all of the model covariates differed by <1% between phases 3.1 and 3.2, and they differed by < 0.1% for the outcomes in particular. Nevertheless, the findings should be interpreted with caution.

Also, while this counterfactual scenario fundamentally cannot be tested, we can examine whether the control group is an adequate comparator by examining whether trends in the outcomes during the pre-expansion period were similar (i.e., the "parallel trends" assumption). To do so, we first qualitatively assessed trends by plotting the trends for adults with versus without children during the pre-expansion period. The graphs illustrated parallel trends during these months for most outcomes. Mental health prescription medication was the only exception; regression results related to this outcome should therefore be interpreted cautiously. We also performed a quantitative evaluation of the validity of the parallel trends assumption by restricting the data to the pre-period and regressing each outcome on an interaction term between adults with versus without children and a continuous variable for time. In these tests, all coefficients were very small (Appendix Table 1.1). While the estimates for several secondary outcomes were statistically significantly different from zero, indicating possible violations of this assumption, this may be because of the large sample size, and the small coefficient sizes provide somewhat reassuring evidence that this assumption is met.

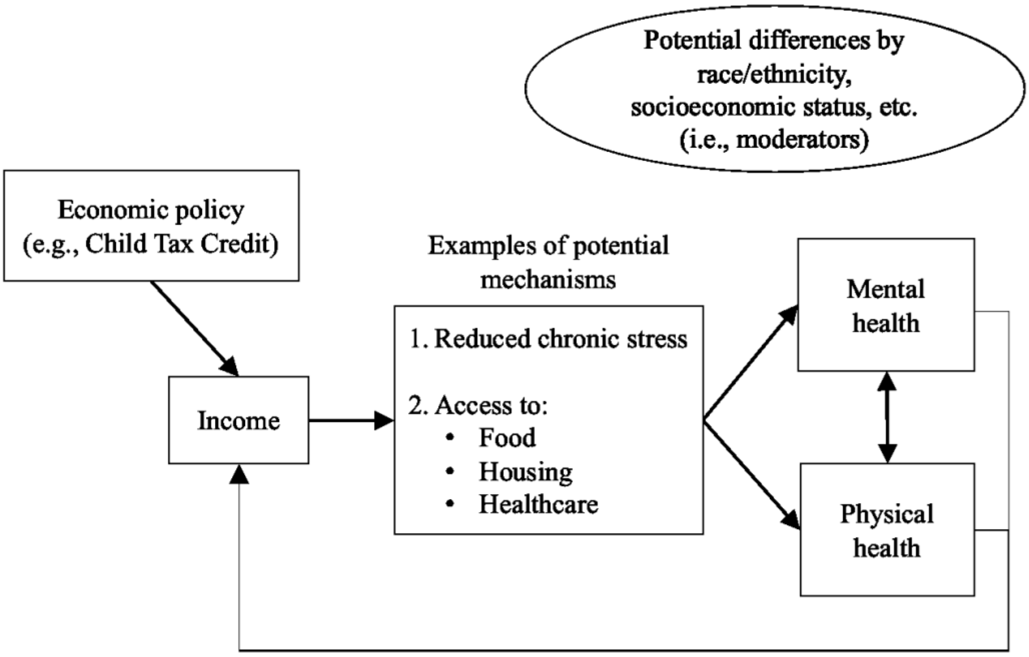
Another assumption is that there are no differential compositional changes in the treatment and control groups. For example, despite its random sampling procedure, HPS may have inadvertently selected respondents with different characteristics in different survey waves. Alternately, by shifting the order of the GAD-2 and PHQ-2 questions in the redesign of the phase 3.2 questionnaire, this may have affected the characteristics of the sample receiving these questions due to increasing drop-out from the survey as the questionnaire progresses. To evaluate this assumption, we conducted a balance test, which is a similar analysis as the primary analysis above, but in which each sociodemographic characteristic was the dependent variable on the left-hand side of the model. A null result for these regressions would suggest that there were no differential pre-post changes in composition among the adults with versus without children. There were statistically significant differences in a handful of sociodemographic characteristics (e.g., gender, marital status) (Appendix Table 1.2). This may mean that HPS unintentionally interviewed participants of different sociodemographic backgrounds across different waves, although again, these coefficients were very small and may be statistically significant due to the large sample size. To further evaluate the validity of this assumption, we also conducted an assessment of standardized differences, comparing whether the pre-post difference in the treatment group differed from that in the control group; differences were reassuringly less than 0.25 for each covariate. We controlled for all these variables in our analyses to account for potential confounding, but cannot rule out differences in unmeasured confounders, a limitation of any DID analysis.

Missingness

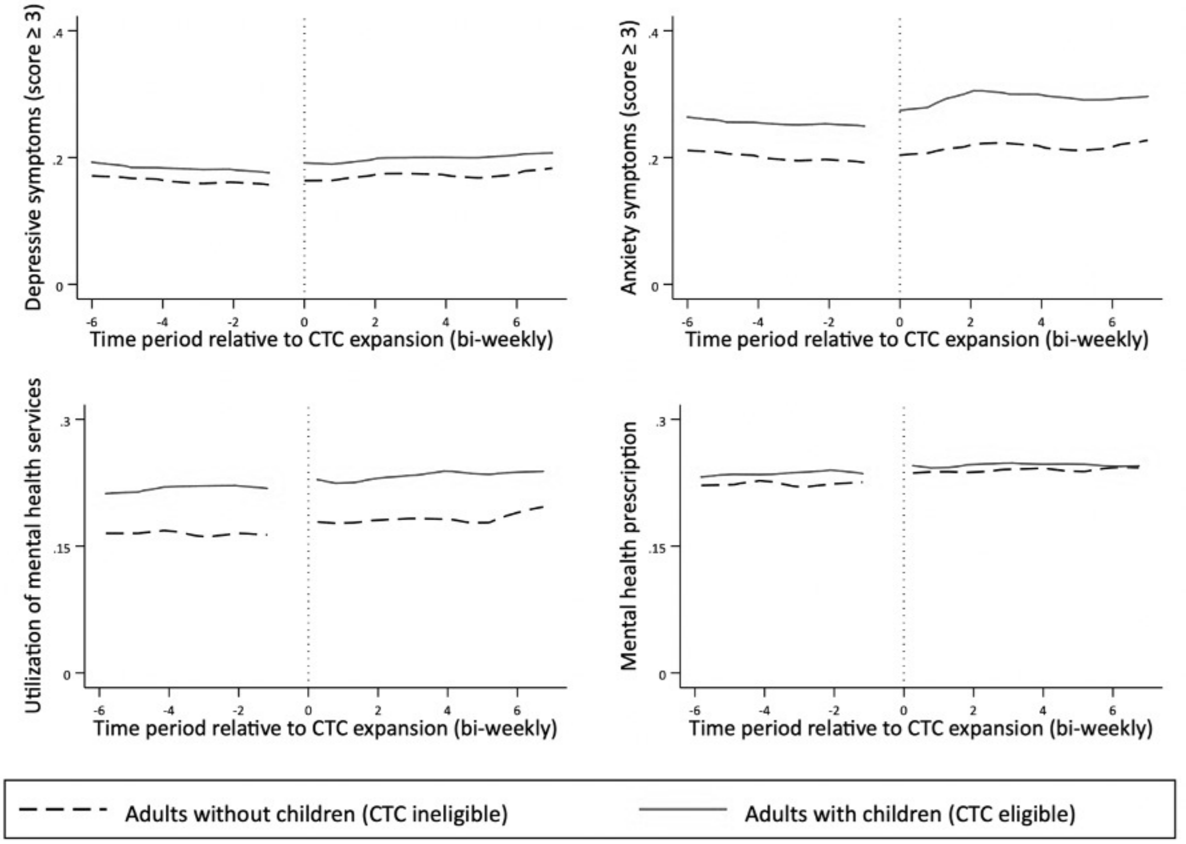
In our sample restricted to those with responses on the mental health outcomes of interest, missingness for each variable was less than 1%, with the exception of income, which was missing 10.9% of values. We therefore conducted a sensitivity analysis employing multiple imputation using chained equations (MICE) using the *mi* package in Stata to impute missing covariates. The MICE method does not require that variables be normally distributed, allowing us to include a variety of different variable types (e.g., categorical, binary). We assumed that data were missing at random rather that missing completely at random (59). All variables from the main models (including the outcomes) were included in the imputation models, in order to improve the prediction of income. We did not use imputed values of the outcome variables in our analyses, however, as this is likely to add noise to subsequent estimates (60). We produced 30 imputed data sets, which is a sufficient number to reduce sampling variability from the imputation process (61).

Sample Definition

Notably, HPS asks participants whether there are individuals under 18 in their households, but not whether these are their own children or dependents. Regardless, these children's caregivers or parents are likely to also be members of the household, such that the respondent may have benefited from increased household income, even if the children were not theirs. Since we cannot confirm that respondents are themselves parents, throughout this manuscript we therefore refer to them as "adults with children."



Appendix Figure 1.1. Potential pathways linking economic policy, poverty, and mental health.



Appendix Figure 1.2. Qualitative evaluation of parallel trends assumption.
Source: Author's analysis of data from U.S. Census Household Pulse Survey, bi-weekly waves from April 14, 2021 – January 10, 2022.
Note: The vertical dotted line represents the first payment of the expanded Child Tax Credit (July 15, 2021). Abbreviations: child tax credit (CTC)

Appendix Table 1.1. Quantitative evaluation of parallel trend assumption.

	Mental health and healthcare utilization outcomes			
	Depressive symptoms	**Anxiety symptoms**	**Utilization of mental health services**	**Mental health prescription**
Coefficient	0.001	-0.000	-0.001	-0.000
[95% CI]	[-0.001, 0.003]	[-0.002, 0.001]	[-0.003, 0.000]	[-0.002, 0.001]
(p-value)	(0.22)	(0.70)	(0.09)	(0.62)
Observations	309,010	309,124	308,810	309,199

Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: ***p < 0.01, **p<0.05. For the purposes of this analysis, the data set was restricted to the pre-expansion period. Coefficients are derived from models in which the primary exposure is an interaction term between a binary variable representing adults with (versus without) children and a continuous variable for time.

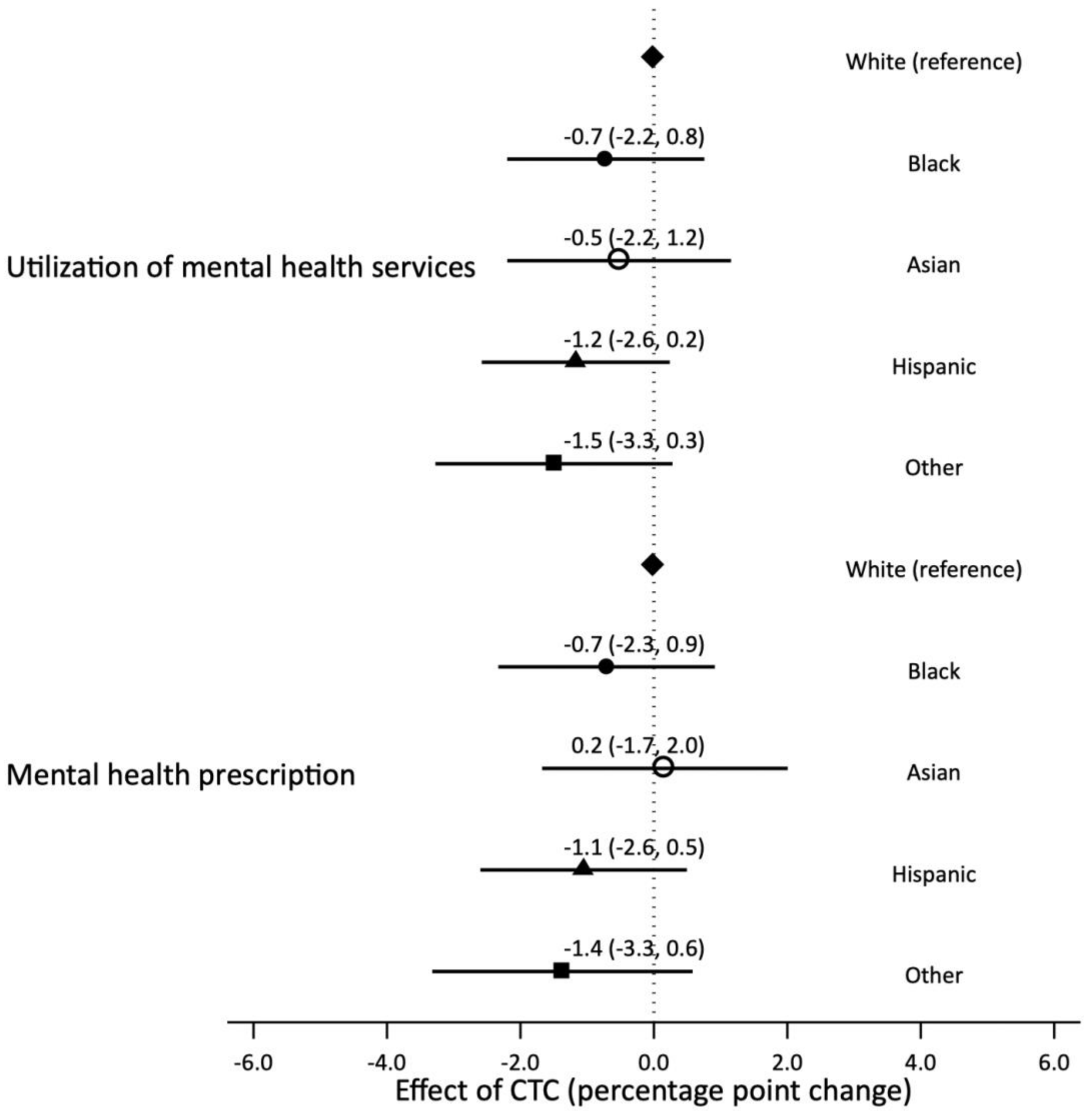
Appendix Table 1.2. Evaluation of differential compositional changes in treatment and control groups (continued on the next page).

Variables	Coefficient [95% CI] (p-value)
Age	0.285***
	[0.157, 0.412]
	(<0.001)
Male	-0.009***
	[-0.013, -0.005]
	(<0.001)
Marital Status	
Married	0.005**
	[0.001, 0.009]
	(0.02)
Separated	0.003
	[-0.001, 0.007]
	[0.105]
Never married	-0.008***
	[-0.012, -0.005]
	(<0.001)
Less than high school or high school	-0.001
	[-0.004, 0.002]
	(0.42)
Race/Ethnicity	
Non-Hispanic White	0.000
	[-0.004, 0.004]
	(0.92)
Non-Hispanic Black	0.002
	[-0.000, 0.005]
	(0.05)
Hispanic	-0.002
	[-0.004, 0.001]
	(0.15)
Asian	-0.003***
	[-0.005, -0.001]
	(0.003)
Other	0.002
	[-0.000, 0.003]
	(0.08)
Income	
Less than $25,000	-0.004***
	[-0.008, -0.001]
	(0.005)
$25,000 - $34,999	-0.006**
	[-0.008, -0.003]
	(<0.001)
$35,000 - $49,999	-0.004**
	[-0.007, -0.001]

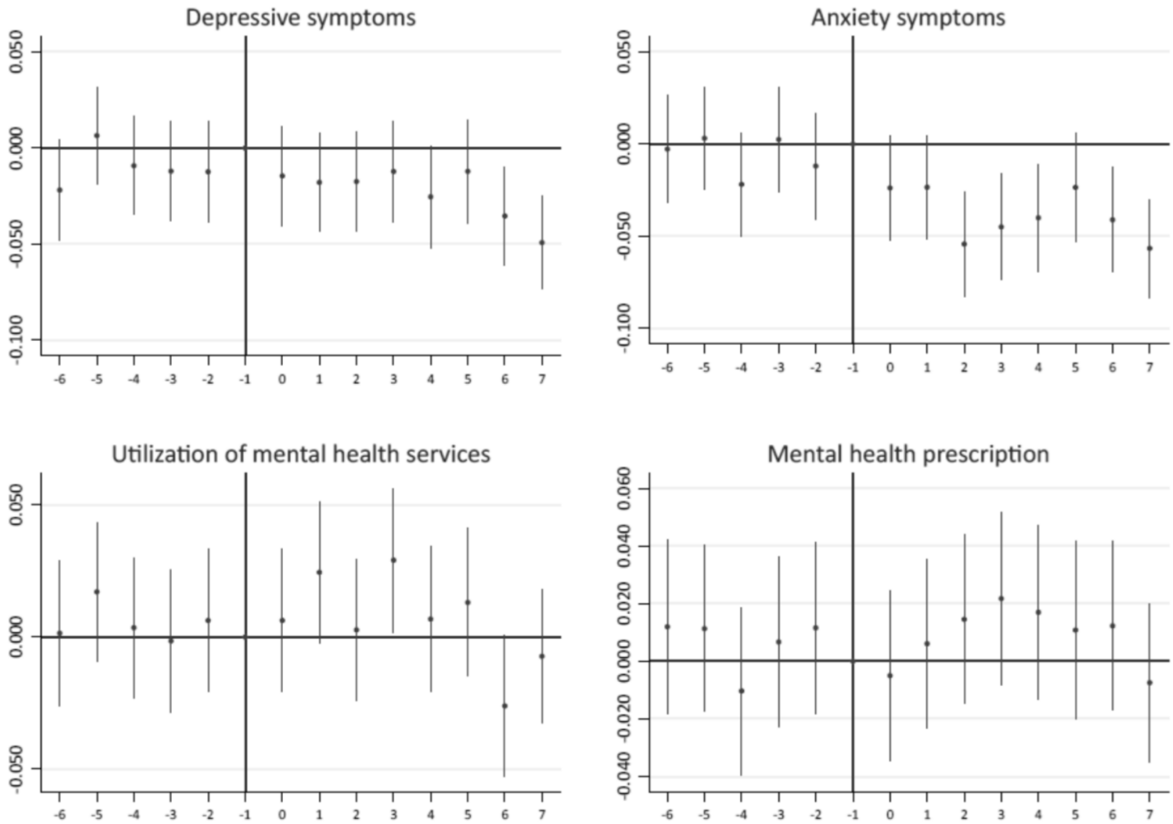
Appendix Table 2. Evaluation of differential compositional changes in treatment and control groups (continued from the previous page).

Variables	Coefficient [95% CI] (p-value)
$50,000 - $74,999	-0.001 [-0.004, 0.003] (0.76)
$75,000 - $99,999	0.001 [-0.003, 0.004] (0.63)
$100,000 - $149,999	0.006*** [0.002, 0.009] (0.005)
$150,000 - $199,999	0.008*** [0.005, 0.011] (<0.001)
$200,000 and above	0.000 [-0.003, 0.003] (0.87)

Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: ***p < 0.01, **p<0.05. Coefficients are derived from models in which the primary exposure is an interaction term between a binary variable for adults with (versus without) children and an indicator for whether the interview occurred after (versus before) the child tax credit expansion. The models examine whether differential compositional differences exist in the demographic characteristics of adults with and without children. A null result would indicate that there are no differential compositional changes in the treatment and control groups over time for a given covariate.



Appendix Figure 1.3. Racial differences in the effects of the 2021 Child Tax Credit expansion on mental healthcare utilization.
Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: ***p < 0.01, **p<0.05. Coefficients are plotted as point estimates (boxes) with 95% confidence intervals (whiskers). Coefficients are derived from difference-in-difference-in-differences models in which the primary exposure is a triple interaction term between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing adults with (versus without) children, and a binary variable for whether the interviewee belonged to a given racial/ethnic group (reference category: White). All regressions adjust for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves. Abbreviations: child tax credit (CTC)



Appendix Figure 1.4. Weekly effects of the 2021 Child Tax Credit expansion among low-income parents.
Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: Values along the X axis represent the number of waves relative to the CTC expansion. Coefficients are derived from difference-in-difference-in-differences models in which the primary exposure is an interaction term between a binary variable for adults with (versus without) children, an indicator for whether the adult belonged to a lower-income (versus high-income) group, and a categorical variables for which wave after (versus before) the CTC expansion the interview occurred. Abbreviations: child tax credit (CTC)

Appendix Table 1.3. Effects of the 2021 Child Tax Credit expansion on mental health and healthcare utilization among low-income parents, imputed data.

	Mental health and healthcare utilization outcomes			
	Depressive symptoms	**Anxiety symptoms**	**Utilization of mental health services**	**Mental health prescription**
Coefficient	-0.016**	-0.033***	0.001	0.003
[95% CI]	[-0.025, -0.006]	[-0.044, -0.023]	[-0.009, 0.011]	[-0.008, 0.014]
(p-value)	(0.001)	(<0.001)	(0.86)	(0.54)
Observations	747,070	747,405	746,852	747,361

Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: ***p < 0.01, **p<0.05. Missing income values were imputed using multiple imputation using chained equations. In this analysis lower income was defined as below $35,000 in annual household income. Coefficients are derived from difference-in-difference-in-differences models in which the primary exposure is a triple interaction term between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing adults with (versus without) children, and a binary variable for whether the interviewee belonged to a lower (versus higher) income group. All regressions adjusted for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves. Depressive symptoms were captured using the Patient Health Questionnaire-2 scale, and anxiety symptoms were captured using the Generalized Anxiety Disorder-2 scale. Abbreviations: child tax credit (CTC).

Appendix Table 1.4. Racial differences in the effects of Child Tax Credit expansion on mental health and healthcare utilization, imputed data.

Racial/ethnic subgroup (Reference: White) Coefficient [95% CI] (p-value)	Mental health and healthcare utilization outcomes			
	Depressive symptoms (binary)	**Anxiety symptoms (binary)**	**Utilization of mental health services**	**Mental health prescription**
Black	-0.017**	-0.028***	-0.01	-0.01
	[-0.031, -0.003]	[-0.043, -0.012]	[-0.024, 0.004]	[-0.026, 0.005]
	(0.02)	(<0.001)	(0.17)	(0.20)
Hispanic	-0.01	-0.025***	-0.013	-0.008
	[-0.023, 0.004]	[-0.040, -0.011]	[-0.026, 0.001]	[-0.023, 0.008]
	(0.16)	(0.001)	(0.07)	(0.33)
Asian	-0.004	-0.018**	-0.006	0.002
	[-0.02, 0.012]	[-0.035, -0.000]	[-0.023, 0.010]	[-0.016, 0.020]
	(0.62)	(0.05)	(0.45)	(0.83)
Other	-0.017**	-0.033***	-0.014	-0.015
	[-0.034, -0.000]	[-0.052, -0.015]	[-0.032, 0.003]	[-0.034, 0.004]
	(0.05)	(<0.001)	(0.10)	(0.12)
Observations	747,040	747,405	746,852	747,361

Source: Authors' analysis of data from U.S. Census Household Pulse Survey
Note: ***p<0.01, **p<0.05. 95% confidence interval in parentheses the second row. P-values in parentheses in the third row. Missing income values were imputed using multiple imputation using chained equations. Coefficients represent the triple interaction between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing parents with children (versus adults without children) and a binary variable for whether the interviewee belonged to a given racial/ethnic group (reference category: White). All regressions adjust for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves.

Appendix Table 1.5 Effects of the 2021 Child Tax Credit expansion on mental health and healthcare utilization among low-income parents, full results.

	Mental health and healthcare utilization outcomes			
Coefficient [95% CI] (p-value)	**Depressive symptoms (binary)**	**Anxiety symptoms (binary)**	**Utilization of mental health services**	**Mental health prescription**
After CTC expansion	-0.014*** [-0.019, -0.009] (<0.001)	-0.012*** [-0.017, -0.007] (<0.001)	0.008*** [0.003, 0.013] (0.001)	0.014*** [0.009, 0.019] (<0.001)
After CTC expansion*Parents with Children	0.014*** [0.010, 0.018] (<0.001)	0.032*** [0.028, 0.036] (<0.001)	0.001 [-0.004, 0.005] (0.77)	-0.004 [-0.008, 0.001] (0.13)
Income less than $35k	0.231*** [0.226, 0.236] (<0.001)	0.239*** [0.234, 0.245] (<0.001)	0.122*** [0.117, 0.127] (<0.001)	0.163*** [0.157, 0.168] (<0.001)
After CTC expansion*Income less than $35k	0.009*** [0.004, 0.014] (0.001)	0.005 [-0.000, 0.011] (0.06)	0.005** [0.000, 0.011] (0.05)	-0.010*** [-0.016, -0.005] (<0.001)
Parents with Children*Income less than $35k	0.017*** [0.009, 0.024] (<0.001)	0.028*** [0.020, 0.036] (<0.001)	-0.023*** [-0.031, -0.015] (<0.001)	-0.037*** [-0.046, -0.029] (<0.001)
After CTC expansion*Parents with Children*Income less than $35k	-0.017*** [-0.026, -0.007] (0.001)	-0.034*** [-0.045, -0.024] (<0.001)	0.000 [-0.010, 0.010] (0.93)	0.003 [-0.008, 0.014] (0.635)
Observations	721,026	721,391	720,838	721,347

Note: ***p < 0.01, **p<0.05. This figure provides the full results for the analysis illustrated in Figure 1. In this analysis lower income was defined as below $35,000 in annual household income. Coefficients are derived from difference-in-difference-in-differences models in which the primary exposure is a triple interaction term between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing adults with (versus without) children, and a binary variable for whether the interviewee belonged to a lower (versus higher) income group. All regressions adjusted for gender, race/ethnicity, income, marital status, number of children, and level of education as well as fixed effects for bi-weekly waves. Depressive symptoms were captured using the Patient Health Questionnaire-2 scale, and anxiety symptoms were captured using the Generalized Anxiety Disorder-2 scale.

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Chapter 2: How State Characteristics Moderated the Impacts of the 2021 U.S. Child Tax Credit Expansion on Mental Health and Mental Health Treatments

Abstract

Background: State-based disparities in mental health have been further exacerbated in the United States during the recent COVID-19 pandemic due to greater experience of pre-existing and COVID-related stressors. This study examined how the state policy environment contributes to the heterogeneous effects of federal policies relating to the Child Tax Credit (CTC) on mental health. In this study, we fill this gap in the literature by providing some of the first evidence of how the effects of a single policy (in this case, the federal CTC expansion) were moderated by the local state policy environment. Public health evidence is limited on how state-related factors are associated with mental health or mental health treatment outcomes.

Methods: We used individual level data from the U.S. Census Household Pulse Survey (HPS) for 14 waves from April 14, 2021, to January 10, 2022 (N = 944,189) to capture the period before and after the CTC expansion on July 15, 2021. The data provided state level identification and we used state level indicators to capture the state policy environment from official government sources. Our study examined whether state-level factors moderated (by state social safety net generosity and racial equity index) the effectiveness of the 2021 CTC expansion using a difference-in-difference-in-differences (DDD, or triple-difference) approach among individuals with children after CTC expansion compared to those adults without children and without CTC expansion.

Results: We were unable to reject the null hypothesis of no effect modification by state safety net generosity or by state racial equity on mental health outcomes. We found CTC effects on secondary outcomes were greater in states with a higher composite generosity index. With a higher social generosity index, confidence in ability to pay rent increased by 0.011 among individuals with children after CTC expansion compared to those adults without children and without CTC expansion (1.1, 95%CI 0.3, 2.0; p-value 0.009). There was no effect modification by state racial equity on secondary outcomes of interest.

Conclusion: Results showed that the association between the CTC and both mental health and material hardship was larger magnitude in states with larger safety net caseloads. This association was with no effect modification by state racial equity. Future research should investigate how safety nets and other social policies interact to affect population health and disparities.

Introduction

Due to increased financial pressures, food insufficiency, and altered health behaviors, the COVID-19 pandemic exacerbated stress and anxiety among U.S. adults.(1, 2) There is some evidence that state-level variation in mental health increased during the pandemic, possibly owing to policies/features of the social environment. (3, 4) For instance, the range of state-based disparities in adults with mental illness reporting not receiving the treatment when most needed increased to 16.9 % in the pandemic phase to 14.3 % from highest ranked Alabama to lowest ranked Utah at 31.2% in 2021.(5) In 2018, in the pre-pandemic period it was 10.5 % with lowest rate in Hawaii(15.8 %) and maximum in the District of Colombia (to 26.3 %).(6) By 2022, disparities in mental illness treatment between states also had worsened to 22.2%, with a lowest 14.9% in Hawaii and highest 37.1% in DC. (7) These disparities are likely influenced by state-level variation in access to healthcare services, funding for mental health services, and the presence of policies to address the social determinants of mental health (e.g., financial insecurity).(8)

States have longstanding differences in social and policy environments that predate the pandemic. For instance, there is a considerable variation in social safety net benefits coverage by state due to the policy environment including outreach, barriers to access, policy implementation among others.(9-11) Indeed, state governments play a prominent role in policymaking and overseeing local implementation of federal initiatives in the United States. This creates local variation in social and economic environments through differential investments in infrastructure, education, healthcare, and other areas.(12, 13) This in turn contributes to differences across states in individuals' financial and housing security, access to healthcare, and other social factors that are powerful determinants of mental health. State characteristics thus likely contribute to geographic disparities in anxiety, depression, and psychological distress (see Figure 1, conceptual framework). States have been at the forefront of policymaking during the pandemic without unified federal guidance.(14) Previous studies have assessed state-level COVID-19 policies' effect on population-level behaviors and found heterogeneous effects by state.(15, 16)

Numerous studies have evaluated the impacts of individual policies on mental health during COVID-19 pandemic.(2, 17-19) For instance, studies found that suicidal ideation increased substantially during the COVID-19 pandemic and that low-income households are especially vulnerable to mental distress during the COVID-19 outbreak.(2, 20, 21) According to another study, measures to halt evictions varied greatly across states, and helped to improve mental health by providing relief to families and individuals struggling to pay housing expenses.(17)

The effects of a single policy or political event on health are often isolated through sudden changes in policy. What is less clear is how the state policy environment contributes to heterogeneous effects of *federal* policies that are supposed to be uniformly implemented nationwide. For example, one study showed that the 2021 temporary expansion of the Child Tax Credit (CTC) by the federal government had a positive impact on the mental health of adults with children particularly in lower-income households.(18) Under the 2021 expanded CTC, the benefit was increased from $2,000 to $3,600 per child for children under 6, and from $2,000 to $3,000 per child, ages 6-17 during July to December 2021. More than 88% of American families with children (39 million households) were eligible for payments.(22) It is not clear how other aspects of the state policy environment may have interacted with the CTC to contribute to local variation in mental health impacts.

In this study, we study how state-level factors might have modified the impact of a federal policy like the CTC on mental health. In other words, we examine how the state policy environment contributes to the heterogeneous effects of the federal Child Tax Credit

(CTC). Few studies have explored how the state policy environment interacts with federal policies and investments to affect health-related outcomes. For example, a recent study found that the association between household income shocks and mental health is weaker for individuals who live in states with supportive social policies—primarily Medicaid and unemployment insurance—during the COVID-19 pandemic.(23) Another study found that states having better access to health care and a smaller proportion of uninsured individuals were associated with fewer cumulative infections and lower total COVID-19 deaths.(19)

In this study, we fill this gap in the literature by examining whether state-level factors (including safety net policies and racial equity) moderated the relationship between the 2021 CTC expansion and mental health outcomes among adults. Using a quasi-experimental study design and a large national sample, this study provides new evidence on how state policies interact to contribute to geographic disparities in mental health and mental health treatment.

Methods

Overview

The goal of this analysis is to examine how state-level factors moderated the impact of the 2021 CTC expansion on mental health and mental health treatment. The U.S. government expanded the Child Tax Credit (CTC) in July 2021 as part of the American Rescue Plan Act. In 1997, the CTC was created to provide financial relief to middle-income families. The payment mode was changed from annual tax refunds, with half of the benefit disbursed instead as monthly advance payments disbursed. Additionally, the expanded CTC was fully refundable (i.e., available even to families with lower incomes who did not previously qualify), and in fact benefits were larger for lower-income families. We carried out quasi-experimental difference-in-differences analysis, described in more detail below.

Data

We used data from the nationally representative Household Pulse Survey (HPS) conducted online by the U.S. Census Bureau.(20) HPS is a serial cross-sectional survey designed to capture real-time information on the COVID-19 pandemic's social and economic impacts. We included data for 14 waves from April 14, 2021, to January 10, 2022 (N = 944,189) to capture the period before and after the CTC expansion on July 15, 2021. The expanded CTC provided half the benefit to recipients as monthly payments starting on July 15, 2021 (just prior to wave 34) and ended on December 15, 2022 (just after wave 41). For our purpose, we studied adults with and without children under 18 in the household who were interviewed between July and December 2021. We restricted the sample to those with household earnings below $35,000 (N = 139,743), as these individuals are most likely to be new recipients of the CTC and to receive the largest benefits from the program (Figure 2.2) (14). Our sample was also restricted to participants who responded to at least one of the mental health questions and who had non-missing values for the covariates (depressive symptoms (PHQ-2) n= 138,330; anxiety symptoms (GAD-2) n= 138,407; utilization of mental health services n= 138,390; confident in ability to pay mortgage n= 97,496; food sufficiency n= 138,569) (Figure 2.2). Note that a final lump-sum payment with the remaining half of the CTC benefit was made in the spring of 2022 to those who filed taxes or claimed economic impact payments; because of ambiguity regarding the definition of the exposure period and potential recipients, our approach excluded observations during this period.

State-level variables were downloaded from online databases, including the National Equity Atlas maintained by PolicyLink from the University of Southern California Equity Research Institute(21), and the University of Kentucky Poverty Research Center, and linked to the HPS based on respondents' state of residence. (22)

Outcomes

Outcomes included three measures of mental health. Depressive symptoms were assessed in HPS using the two-item Patient Health Questionnaire (PHQ-2). The PHQ-2 is a shortened form of the nine-item PHQ-9 and has been validated in numerous studies as a reliable tool for screening for depression.(23) The PHQ-2 asks respondents how often they feel down, depressed, or hopeless and how frequently they feel little interest or pleasure in doing things. Each of the two items is scored from 0 ("not at all") to 3 ("nearly every day"). We combined the two items and scores of ≥3 were used to indicate high depression risk.(24)

Second, anxiety symptoms were captured using the two-item Generalized Anxiety Disorder (GAD-2) scale. The GAD-2 is a brief screening tool for generalized anxiety disorder based on the first two questions of the seven-item GAD-7 scale(25). Individuals are asked to rate how often they feel nervous, anxious, or on edge, and how often they have been unable to control or stop worrying in past two weeks. Like PHQ-2, each item is scored from 0 to 3, with scores ≥3 indicating high anxiety risk.(25) The third outcome measured mental health treatment and asked was whether the participant had received counseling or therapy within the last four weeks.

In addition, we included two secondary outcomes capturing material hardship, including binary variables for household food sufficiency and confidence in the ability to pay rent/mortgage or rent next month. For food sufficiency, this was measured based on responses as to whether they had enough food to eat daily in the last 7 days. These were added as expected to explain the potential mechanism.

Exposure

Individuals with children under 18 in the household who were interviewed between July and December 2021 were included in the exposed group. Those without children or who were interviewed before July 2021 were considered unexposed. Notably, HPS does not indicate whether children in the household are those of the respondent, e.g., they may be grandchildren or non-relations; nevertheless, children in the home indicate that the household likely received benefits, possibly spilling over to the HPS respondent. Also, while HPS asks respondents about whether they received the CTC, self-reported receipt of safety net benefits is unreliable, particularly for benefits like the CTC which was primarily automatically deposited in recipients' bank accounts, so we therefore focused on eligible participants rather than those who self-reported receipt.(26, 27) Prior work suggests that individual and state-level factors are associated with CTC receipt among eligible individuals,(28) so this may result in measurement error but is akin to an intent-to-treat analysis and is common in other studies of US poverty alleviation programs where administrative data on benefit receipt is not available.(28-36)

Moderator

We hypothesized that the state policy and racial equity environment might modify the effect of the CTC on mental health, so we create two variables representing these state factors. First, we created a composite index capturing state social safety net generosity in the participant's state of residence. The composite measure for each state was created using principal component analysis (PCA) that combined information on five of the largest social safety net benefits for families with children: 1) Temporary Assistance for Needy Family (TANF); 2) SNAP; 3) Special Supplemental Nutrition Program for Women, Infants, and Children (WIC); 4) Medicaid; and 5) the earned income tax credit (EITC) (Table 2.7). Measures were from 2020 to ensure they represented a "baseline" level before CTC expansion. Measures for TANF and SNAP caseloads were calculated as the weighted population caseload of each program ([caseload/population] × 100). Since Medicaid has a relatively larger caseload, we calculated it as (caseload/population) × 1000. For state EITC, this was operationalized as the percent of the federal credit that the state provides. PCA analysis resulted in 5 principal components. For ease of interpretability, we selected the first component to serve as the composite index for this study. Table 2.6 and Figure 2.4 show variable loadings for this first component and the scree plot, respectively. The first component for this 5-variable explained 48% of the variance in the data (Table 2.6).

The second state factor we considered was a racial equity index, based on indicators of inclusion and prosperity, which is publicly available online from PolicyLink at the University of Southern California Equity Research Institute.(21) It measures the degree of racial equity in a region by combining multiple indicators and assessing whether progress is being made in racial equity and overall prosperity. Racial disparities are measured by the inclusion score, with a higher score indicating fewer gaps. This is particularly important for identifying and addressing systemic racism and disparities across a variety of aspects of society. They help ensure that policies and practices do not perpetuate racial inequalities and are aligned with efforts to achieve a more equitable society.

Covariates

Models were adjusted for individual-level characteristics that represented potential confounders of the relationship between CTC receipt and mental health, including sex, age, marital status, and education level. We also included self-reported race and ethnicity, categorized as Asian, Hispanic, non-Hispanic Black, non-Hispanic White, or other races/ethnicities. The latter group is heterogeneous, but we could not further disaggregate it due to small cell sizes and unstable estimates. We also included indicator variables for each wave (i.e., time fixed effects) to adjust for underlying temporal (i.e., secular) trends in the outcomes.

Missingness

Our sample was restricted to non-missing outcomes, as all outcomes had less than 1% missingness, except for one self-reported secondary outcome confidence in your ability to pay your mortgage/rent (30%). The missingness was due to the question being restricted to those with a mortgage or rent on their homes. Among participants for whom we had outcome data, we had missingness only for marital status and missingness was less than 1% (0.54 %). Figure 2.2 shows our sample restricted to non- missing outcomes and covariates. We therefore performed complete case analysis.

Statistical Analysis

We first tabulated individual-level sample characteristics by CTC eligibility (i.e., presence of children in the household) and whether the interview was conducted after the CTC expansion (i.e., July 15, 2021). We then examined whether the effects of the CTC expansion on mental health were moderated by state characteristics by using a difference-in-difference-in-differences (DDD, or triple-difference) approach. This method builds upon traditional difference-in-differences (DID) analysis, a quasi-experimental method that examines policy impacts while accounting for underlying trends.(37, 38) These methods allow for comparison of pre-post changes in outcomes among a "treatment" group (in this case, adults with children) while “differencing out” secular (i.e., underlying temporal) trends in outcomes in a "control" group (in this case, adults without children). The DDD approach allows for a test of effect modification by incorporating an additional triple multiplicative interaction term between the state-level variables and the primary exposure variable. In this study, the triple interaction term in DDD models was composed of three variables: (1) an indicator of whether the interview occurred after or before the CTC expansion, (2) an indicator variable of adults with (versus without) children, and (3) a variable representing continuous measure of the safety net benefit index of the state in which the individual resides.

An example of the equation for the DDD model is:

$$
\begin{aligned}
Y = \alpha + \beta_1 Post\ x\ Children\ x\ Generosity + \beta_2 Children\ x\ Generosity \\
+ \beta_3 Children\ x\ Post + \beta_4 Generosity\ x\ Post \\
+ \beta_5 Children + \beta_6 Post + \beta_7 Generosity + \beta_8 Covars + \beta_9 Week + \varepsilon
\end{aligned}
$$

Here, Y represents our outcomes of interest. $Children$ indicates whether adults had children in the household, which is our treatment indicator. $Post$ indicates whether the individual was interviewed after CTC payments began in July 2021. $Generosity$ is a continuous measure of the safety net benefit index. The equation also includes all two-way interactions between these three variables. The coefficient of interest is β_1, on the triple-interaction term, which represents the effect modification of state-level factors on the relationship between the CTC expansion and the outcomes of interest. The equation also includes weekly time-fixed effects represented by $Week$ and individual-level covariates $Covars$.

Using a similar equation, we investigated whether the effects of the CTC expansion on the mental health and secondary outcomes were moderated by state racial equity indices.

This involved replacing the generosity index with the racial equity index in the equation above.

DID Assumptions

DID analyses rely on several assumptions. The first assumption is that there are parallel trends in the outcomes in the treatment and control groups before the intervention period, which would suggest that they are comparable. We first did a qualitative assessment of this “parallel trends assumption” through visual inspection by plotting the trends for adults with children versus without children during the pre-expansion period. In addition, we assessed the validity of the parallel trends assumption quantitatively by regressing each outcome on an interaction term between adults with children versus without children and a continuous variable for time.

The other assumption is that there are no differential compositional changes across the study period in the treatment and control groups. To test this validity of this assumption, we implemented similar models to the primary analysis, in which each sociodemographic characteristic was the dependent variable in the model. A null result in these analyses would suggest no differential compositional changes in the two groups.

Secondary Analysis

In a secondary analysis, we investigated effect modification for each policy separately compared to earlier combined composite measure. To test for effect modification, we continued with above DDD analysis and involved replacing the generosity index with the binary state-level policy in the equation above. We dichotomized state-level policy at the median value rather than using them as continuous variables for easier interpretation.

Results

Sample Characteristics

Our final sample consisted of adults with children (112,862 observations before and 123,283 after the CTC expansion) and adults without children (237,901 observations before and 269,840 after the expansion). Adults with children were more likely to be younger, female, and Black or Asian (Table 2.1). State characteristics were roughly balanced among adults with and without children before and after the CTC expansion. Mental health and material hardship were worse among adults with children across the entire study period. Importantly, DID analysis does not require that characteristics of treatment and control groups be similar, but rather that trends (i.e., slopes) of outcomes be parallel during the period before the CTC expansion.

Evaluating DID Model Assumptions

We evaluated the validity of model assumptions as discussed above in the method section. The graphical inspection of the parallel trends, illustrated in figure 2.3, shows all five outcomes included in this study demonstrated parallel trends prior to Child Tax Credit expansion. In the quantitative tests, all coefficient estimates were not statistically significantly different from zero (Table 2.2). This provides additional evidence that this assumption is met.

We found few differential compositional changes in key covariates among adults with children and adults without children in two sociodemographic characteristics (adult's age, Asian racial subgroups) (Table 2.3). Our analyses controlled for all sociodemographic characteristics to accommodate potential confounding but cannot eliminate differences in unmeasured confounders.

Effect modification by composite indices of state safety net generosity and racial equity on mental health outcomes

We were unable to reject the null hypothesis of no effect modification by state safety net generosity or by state racial equity on the mental health outcomes (Table 2.4).

Effect modification by composite indices of state safety net generosity and racial equity on secondary outcomes

We did find positive effects of the CTC on secondary outcomes were greater with in states with a higher composite generosity index. With higher social generosity index, confidence in ability to pay rent increased by 0.011 among individuals with children after CTC expansion compared to those adults without children and without CTC expansion (1.1, 95%CI 0.3, 2.0; p-value 0.009) (Table 2.4). There was no effect modification for the mental health outcomes or material hardship. There was no effect modification by state racial equity for secondary outcomes of interest (Table 2.4).

Effect modification by single state policy generosity

Several single indicators of state safety net policy generosity moderated the effectiveness of the CTC expansion. For example, for individuals with children after CTC expansion in states with higher WIC caseloads, food sufficiency increased in greater magnitude by 0.027 compared to those adults without children and without CTC expansion (2.7, 95%CI 0.9, 4.5; p-value 0.003) (Table 2.5). Also, the reduction in depressive symptoms was greater in states with higher Medicaid caseloads (-7.5, 95% CI -14.7, -0.3; p-value 0.042) among individuals with children after CTC expansion compared to those adults without children and without CTC expansion. We did not find effect modification by TANF or SNAP caseloads or state EITC rates for any outcome (Table 2.5).

Discussion

This study complements the growing evidence on the effects of single economic policies on mental health and its social determinants, providing some of the first evidence of how the effects of a single policy (in this case, the federal CTC expansion) were moderated by the local state policy environment. The overall findings of our work showed null effect modification of state-level factors on the relationship between the CTC expansion and the outcomes of interest your main question of interest (i.e. about mental health outcomes). We found that improvements in material hardship outcomes was greater in states with larger caseloads of safety net programs, with no effect modification by state racial equity. Using single safety net programs analysis, we found that improvements mental health and material hardship were greater in states with larger caseloads of single safety net programs like WIC participation or Medicaid beneficiaries. This suggests that even federal policies may not impact recipients in all states equally, due to local factors that may contribute to widening geographic disparities in health.

The state's policy environment may contribute to the effectiveness of the expanded CTC through several mechanisms. First, it may be that higher caseloads for a given program indicate that some states are able to enroll higher numbers of individuals who are eligible for that program, thereby increasing take-up. For individuals who are eligible for multiple programs at the same time, receiving benefits in different forms (e.g., cash assistance, nutrition support, insurance coverage) may have synergistic benefits for families struggling in multiple domains.(39) Second, it may be that states with higher caseloads who actively enroll more participants also tend to provide more support of economically disadvantaged families in general, such that our measures of the state policy context are proxies for state generosity for low-income families in a more holistic sense.(40) One implication is the need to better coordinate and integrate policies and programs to ensure that individuals can access multiple benefits. This concept is often referred to as "adjunctive eligibility" or "stackable benefits," because it involves streamlining the enrollment process and aiding individuals with accessing multiple programs at the same time.(41, 42) There is a need to create a more comprehensive support system and reduce barriers for those who need assistance as part of this effort. Policymakers must assess how multiple programs can be consolidated or streamlined to make them more effective. In addition, social safety net policies interact in many ways, and their effectiveness can be influenced by multiple factors. It is imperative that policymakers consider these interactions and their consequences when designing, implementing, and evaluating safety net programs.

This study has several strengths, including the use of multiple waves of a nationwide diverse data set and the application of rigorous quasi-experimental methods. Nevertheless, the study has limitations. Survey data often suffers from reporting biases, especially on sensitive topics like income and mental health. In addition, HPS is a serial cross-sectional survey, so we cannot observe changes in an individual's mental health after receiving CTC benefits as we might in a panel dataset. Also, HPS was designed to provide rapid biweekly data during the COVID-19 pandemic and may lack generalizability to periods other than COVID-19. Lastly, using caseloads as a measure of state policy generosity is challenging to interpret it might mean that the state is effective at getting people enrolled, or it might just mean that there are more low-income people in that state, thereby conflating take-up with participation. This should be investigated more in future work using administrative data that more clearly distinguish between eligible and ineligible populations.

Conclusion

This study is among the first to examine the effects of a single federal policy within the context of a state policy environment, acknowledging that policies do not occur in isolation from the local policy landscape. Overall, our results show that the effectiveness of a recent (temporary) expansion to the child tax credit was not substantially moderated by the existing safety net landscape in different states. Future research should continue to investigate how safety net and other social policies interact to affect population health and disparities, e.g., how state characteristics influence the local effectiveness of other federal policies like SNAP or the EITC. This study also has important implications for policymaking to maximize safety net policy synergies and ensuring that eligible individuals receive support to enroll in the programs for which they are eligible.

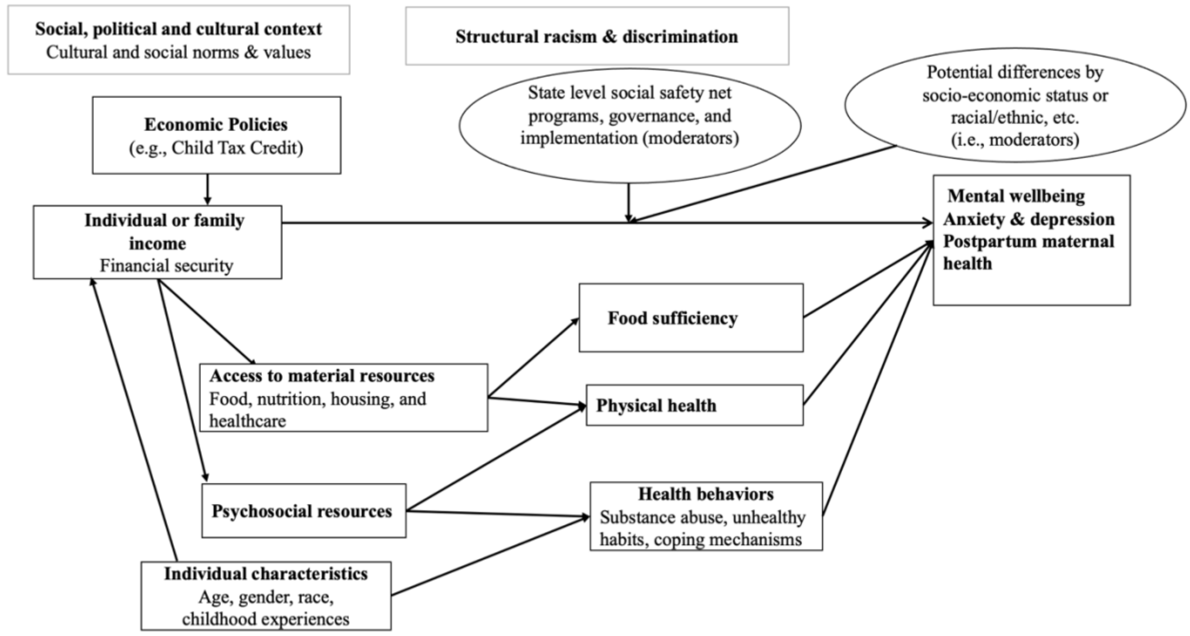

Figure 2.1. Potential pathways linking state characteristics and state policies with mental health.

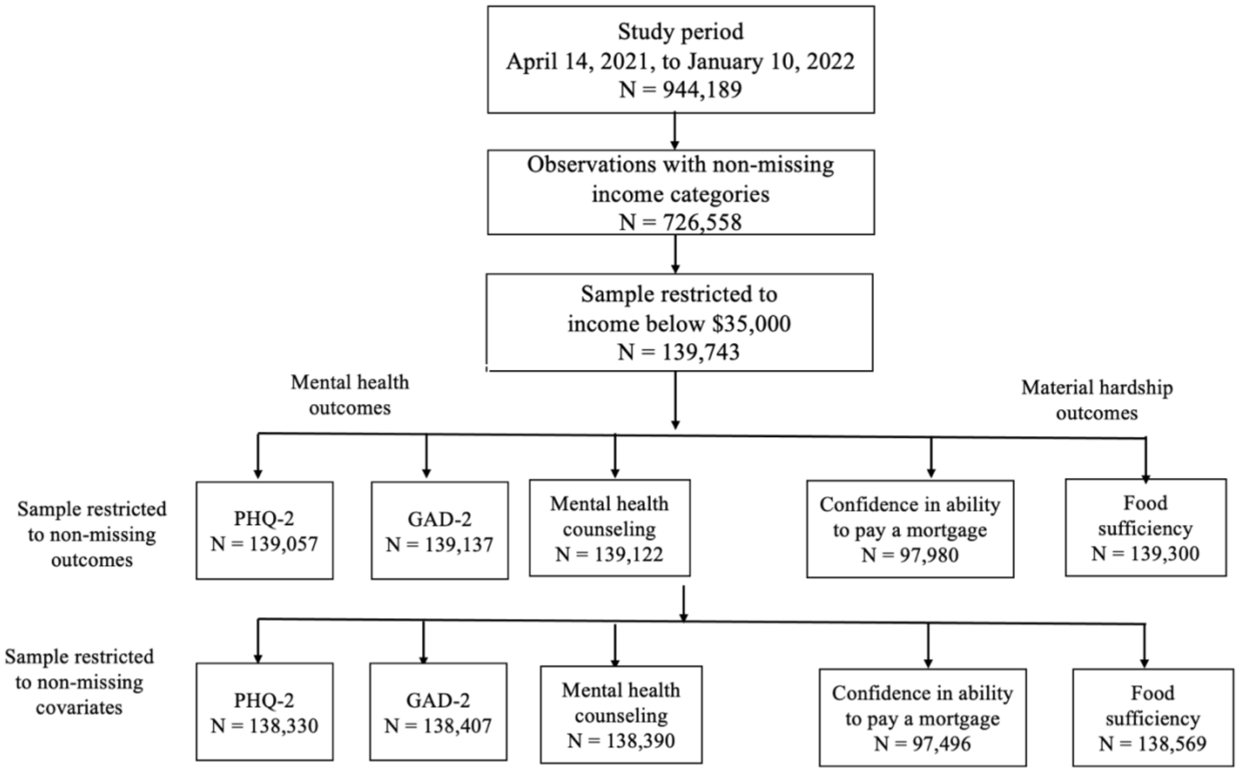

Figure 2.2. Sample flow chart.
Note: Data were drawn from U.S. Census Household Pulse Survey weeks 28-40. Abbreviations: PHQ-2: Patient Health Questionnaire-2; GAD-2: Generalized Anxiety Disorder-2

Table 2.1. Sample characteristics.

	Before July 15,2021		After July 15,2021	
	Adults without children Mean (SD) or % (N)	Parents with children Mean (SD) or % (N)	Adults without children Mean (SD) or % (N)	Parents with children Mean (SD) or % (N)
Individual characteristics				
Age (years)	56.37 (16.77)	44.60 (14.13)	55.27 (16.99)	44.23 (14.25)
Male	35.59 (14,824)	22.37 (3,642)	36.41 (21,849)	22.12 (4,824)
Marital status				
Married	22.69 (9,392)	32.73 (5,290)	22.82 (13,624)	32.94 (7,153)
Never Married	77.31 (32,000)	67.27 (10,874)	77.18 (46,089)	67.06 (14,567)
Less than college	23.88 (9,949)	34.16 (5,561)	24.21 (14,528)	34.04 (7,424)
Race/Ethnicity				
Asian	3.76 (1,566)	3.86 (629)	3.49 (2,094)	4.16 (907)
Hispanic	10.05 (4,188)	20.40 (3,321)	10.08 (6,047)	20.48 (4,466)
Non-Hispanic Black	9.04 (3,767)	17.23 (2,805)	9.38 (5,628)	17.58 (3,835)
Non-Hispanic White	73.04 (30,424)	52.67 (8,573)	72.79 (43,673)	51.89 (11,316)
Other	5.87 (2,444)	9.44 (1,537)	5.72 (3,434)	9.14 (1,993)
Mental health outcomes				
Depressive symptoms (PHQ-2)	1.91 (2.02)	2.15 (2.05)	2.01 (2.02)	2.22 (2.07)
Anxiety symptoms (GAD-2)	2.09 (2.14)	2.55 (2.16)	2.23 (2.13)	2.66 (2.18)
Material hardship outcomes				
Utilization of mental health services	24.7 (10,235)	28.2 (4,564)	26.8 (16,009)	30.09 (6,527)
Confident in ability to pay mortgage	78.3 (113,942)	72.3 (62,250)	75.9 (125,130)	70.8 (66,831)
Food sufficiency	56.9 (23,691)	42.2 (6,847)	54.3 (32,463)	42.4 (9,197)
State characteristics				
Composite measure of social policies	0.00 (1.56)	0.19 (1.54)	-0.01 (1.54)	0.00 (1.54)
TANF caseload	0.31 (0.26)	0.30 (0.26)	0.31 (0.26)	0.30 (0.26)
SNAP caseload	6.08 (1.86)	6.12 (1.82)	6.09 (1.86)	6.11 (1.82)
WIC participation	1.80 (0.35)	1.81 (0.35)	1.83 (0.35)	1.80 (0.35)
Medicaid beneficiaries	230.16 (66.62)	231.08 (66.67)	229.46 (66.54)	230.29 (67.41)
State EITC Rate (%)	0.15 (0.24)	0.15 (0.24)	0.15 (0.23)	0.16 (0.24)
Racial Equity Index	48.87 (10.22)	48.33 (10.30)	48.93 (10.16)	48.19 (10.32)
N	40.9 (41,654)	42.7 (16,278)	59.0 (60,002)	57.3 (21,809)

Note: Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. We further restricted the sample to income below $35,000 and non-missing outcomes. Abbreviations: PHQ-2: Patient Health Questionnaire-2; GAD-2: Generalized Anxiety Disorder-2; TANF: Temporary Assistance for Needy Families; (Figure caption continued on the next page.)

(Figure caption continued from the previous page.)
SNAP: Supplemental Nutritional Assistance Program; (WIC: Special Supplemental Nutrition Program for Women, Infants, and Children; EITC: Earned Income Tax credit. State caseload variables for SNAP and TANF were calculated by dividing the caseload by total state population and multiplied by 100. Medicaid beneficiaries were calculated in similar manner but multiplied by 1000.

Table 2.2. Quantitative evaluation of parallel trends assumption for outcomes.

	Mental health			Material hardship	
	Depressive symptoms	**Anxiety symptoms**	**Utilization of mental health services**	**Confident in ability to pay mortgage/rent**	**Food sufficiency**
Coefficient	-0.008	-0.002	-0.000	0.001	0.003
95% CI	[-0.030, 0.013]	[-0.025, 0.021]	[-0.005, 0.004]	[-0.005, 0.007]	[-0.003, 0.008]
p-value	(0.441)	(0.858)	(0.841)	(0.758)	(0.320)
Observations	57,296	57,307	57,283	40,412	57,441

Note: Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. The data set was restricted to the pre-expansion period for this analysis. The coefficients are derived from models in which primary exposure is an interaction between a binary variable representing adults with (or without) children and a continuous variable representing time.

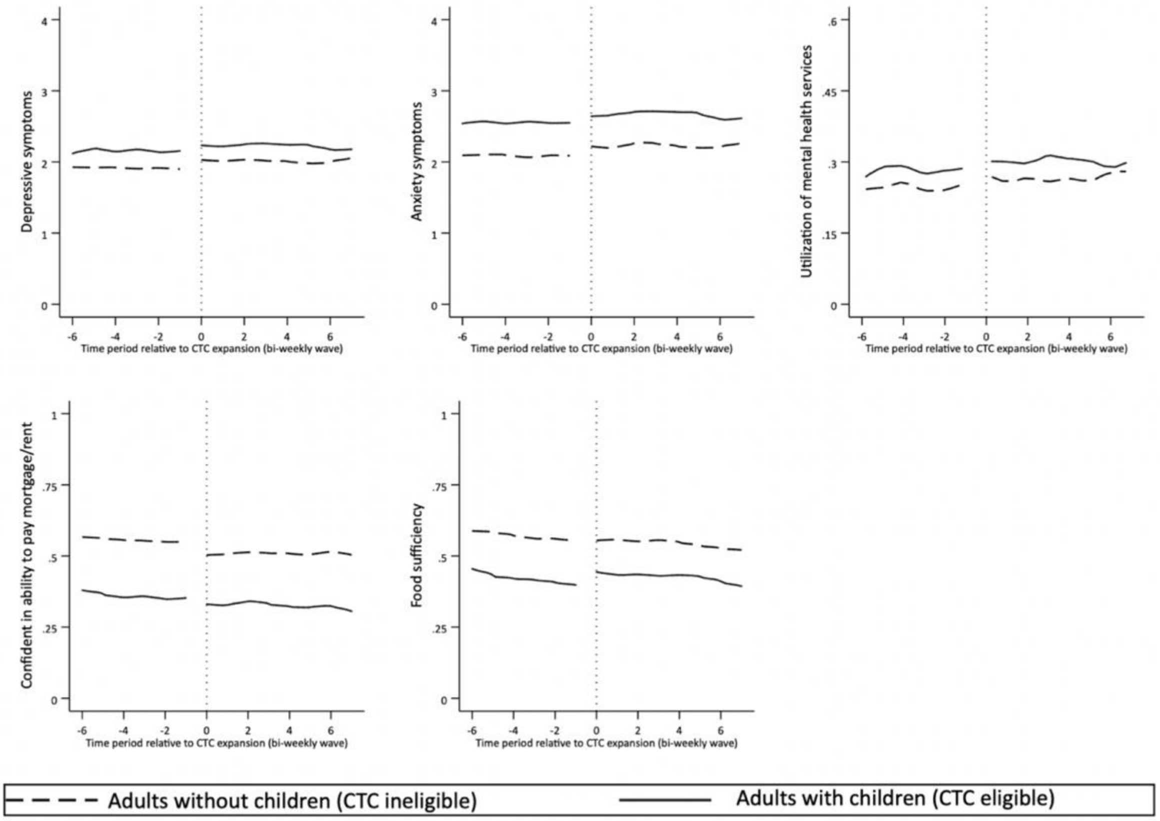

Figure 2.3. Qualitative evaluation of parallel trends assumption for outcomes.
Note: Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. A vertical dotted line indicates the first payment of the expanded Child Tax Credit (15 July 2021).

Table 2.3. Evaluation of differential compositional changes in treatment and control groups.

	Coefficient [95% CI] (p-value)
Age	0.720** [0.333, 1.106] (<0.001)
Male	-0.011 [-0.022 - 0.000] (0.055)
Marital Status	
Married	0.001 [-0.009, 0.011] (0.866)
Separated	0.008 [-0.004, 0.019] (0.205)
Never married	-0.009 [-0.020, 0.003] (0.131)
Less than high school or high school	-0.004 [-0.015 - 0.006] (0.402)
Race/Ethnicity	
Asian	0.006* [0.001, 0.010] (0.014)
Hispanic	0.001 [-0.007, 0.008] (0.897)
Non-Hispanic Black	0.000 [-0.007, 0.008] (0.966)
Non-Hispanic White	-0.005 [-0.016, 0.006] (0.348)
Other	-0.002 [-0.008, 0.004] (0.600)

Note: **p < 0.01, *p<0.05. Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. We further restricted the sample to income below $35,000 and non-missing outcomes. Coefficients are derived from models in which the primary exposure is an interaction term between a binary variable for adults with (versus without) children and an indicator for whether the interview occurred after (versus before) the CTC expansion. (Figure caption continued on the next page.)

(Figure caption continued from the previous page.)
The models examine whether differential compositional differences exist in the demographic characteristics of adults with and without children. A null result would indicate that there are no differential compositional changes in the treatment and control groups over time for a given covariate.

Table 2.4. Modification of the effect of the expanded CTC on mental health by state characteristics.

	Mental health			Material hardship	
Coefficient [95% CI] (p-value)	**Depressive symptoms**	**Anxiety symptoms**	**Utilization of mental health services**	**Confident in ability to pay mortgage/rent**	**Food sufficiency**
Composite social policy measure	-0.012	-0.012	-0.006	0.011**	0.006
	[-0.043, 0.019]	[-0.044, 0.020]	[-0.013, 0.000]	[0.003, 0.020]	[-0.002, 0.013]
	(0.446)	(0.459)	(0.068)	(0.009)	(0.155)
Racial Equity Index	0.002	0.005	0.001	0.000	-0.000
	[-0.003, 0.006]	[-0.000, 0.009]	[-0.000, 0.002]	[-0.001, 0.002]	[-0.002, 0.001]
	(0.461)	(0.060)	(0.263)	(0.737)	(0.407)

Note: **p < 0.01, *p < 0.05. Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. Coefficients are from the triple interaction between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing parents with children (versus adults without children), and a composite measure of state characteristics. All models were restricted to income below $35,000 and adjusted for individual characteristics like age, sex, race/ethnicity, marital status, and level of education as well as fixed effects for bi-weekly survey waves.

Table 2.5. Modification of the effect of the expanded CTC on mental health by single state policy generosity.

Coefficient [95% CI] (p-value)	Depressive symptoms	Anxiety symptoms	Utilization of mental health services	Confident in ability to pay mortgage/rent	Food sufficiency
TANF caseload	-0.024 [-0.096, 0.048] (0.513)	-0.046 [-0.121, 0.029] (0.229)	-0.011 [-0.027, 0.004] (0.161)	-0.013 [-0.033, 0.007] (0.217)	-0.005 [-0.023, 0.013] (0.580)
SNAP caseload	0.019 [-0.054, 0.092] (0.616)	-0.003 [-0.079, 0.074] (0.945)	-0.002 [-0.017, 0.014] (0.835)	-0.004 [-0.024, 0.017] (0.730)	0.003 [-0.015, 0.021] (0.723)
WIC participation	-0.051 [-0.124, 0.022] (0.168)	-0.051 [-0.127, 0.025] (0.185)	-0.015 [-0.030, 0.001] (0.064)	-0.003 [-0.023, 0.018] (0.792)	0.027** [0.009, 0.045] (0.003)
Medicaid beneficiaries	-0.075* [-0.147, -0.003] (0.042)	-0.025 [-0.100, 0.050] (0.519)	-0.002 [-0.018, 0.013] (0.778)	-0.015 [-0.035, 0.005] (0.145)	-0.003 [-0.020, 0.015] (0.783)
State EITC Rate	-0.052 [-0.252, 0.149] (0.613)	0.088 [-0.121, 0.297] (0.409)	0.005 [-0.038, 0.048] (0.813)	0.033 [-0.022, 0.088] (0.233)	-0.012 [-0.061, 0.038] (0.646)

Note: **p < 0.01 & *p < 0.05. Individual-level data were drawn from the U.S. Census Household Pulse Survey from week 28 to week 40 between April 14, 2021, to January 10, 2022. Coefficients are from the triple interaction between an indicator for whether the interview occurred after (versus before) the CTC expansion, a binary variable representing parents with children (versus adults without children), and a continuous measure of each state characteristic. All models were restricted to income below $35,000 and adjusted for individual characteristics like age, sex, race/ethnicity, marital status, and level of education as well as fixed effects for bi-weekly survey waves. Abbreviations: TANF: Temporary Assistance for Needy Families; SNAP: Supplemental Nutritional Assistance Program; WIC: Special Supplemental Nutrition Program for Women, Infants, and Children; EITC: Earned Income Tax credit. State caseload variables for SNAP and TANF were calculated by dividing the caseload by total state population and multiplied by 100. Medicaid beneficiaries were calculated in similar manner but multiplied by 1000.

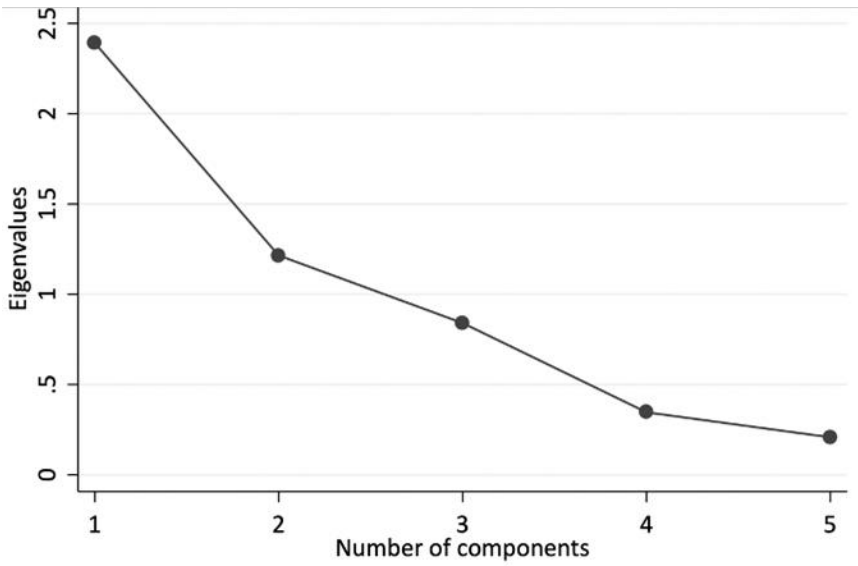

Figure 2.4. Scree plot of eigenvalues.
Note: Line plot of factor eigenvalues obtained from conducting principal component analysis based on five safety net policies of generosity in states using the study sample from the U.S Census Household Pulse Survey from April 14, 2021, to January 10, 2022.

Table 2.6. Principal component analysis: eigenvalues, proportion of variance explained, and cumulative proportion explained for each component.

Components	Eigenvalues	Proportion Explained	Cumulative Explained
1	2.39	0.48	0.48
2	1.21	0.24	0.72
3	0.84	0.17	0.89
4	0.35	0.07	0.96
5	0.21	0.04	1.00

Note: Eigenvalues, proportion of variance explained, and cumulative proportion explained for each component obtained from conducting principal component analysis using the study sample from the U.S Census Household Pulse Survey from April 14, 2021, to January 10, 2022.

Table 2.7. Safety net policies included in composite index to measure state generosity.

State characteristics	Variable Loading (Component 1)
TANF caseload	0.51
Food stamps caseload	0.42
WIC participation	0.26
Medicaid beneficiaries	0.56
State EITC Rate (%)	0.42

Note: Variable loadings presented above were obtained from conducting principal component analysis using the study sample from the U.S Census Household Pulse Survey from April 14, 2021, to January 10, 2022.

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Chapter 3: The Effects of Added Income on Postpartum Maternal Health: a Regression Discontinuity Analysis of Child-related Tax Benefits in the US

Abstract

Background: Poverty and financial hardship can negatively impact postpartum health. This study examined the impact of two child-related tax benefit policies that provide direct income support to low- and moderate-income parents on postpartum health. Both the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC) have been found effective in increasing after-tax income of targeted groups, reducing poverty, and increasing labor force participation. But there is limited evidence of child-related tax benefits' impacts on postpartum health.

Methods: We use Centers for Disease Control and Prevention (CDC) Pregnancy Risk Assessment Monitoring System (PRAMS) data from 2004 to 2020 to study the impact of child-related tax benefits on postpartum health outcomes like postpartum depressive symptoms, breastfeeding, and postpartum visits. We use regression discontinuity (RD) analysis, exploiting the advantage of discontinuity in eligibility for U.S. child-related tax benefits following the birth of the first child. The nuance of the tax system only allows parents with infants born on or before December 31 in a given year to receive substantial child-related tax benefits whereas those whose infants are born after December 31 must wait until the subsequent tax filing season.

Results: We were unable to reject the null hypothesis of no effect on the different postpartum health outcomes with additional child related tax compared to women who did not receive any additional child related tax, other than in the subgroup analyses. We found that with child related breastfeeding decreased (-0.032, 95% CI (-0.062, -0.002), p=0.035), and postpartum depression (0.029, 95% CI (0.004, 0.055), p=0.025) increased among Hispanic women, and postpartum visits were reduced for other minority racial subgroups (-

0.035, 95% CI (-0.062, -0.008), p=0.011). We found that postpartum visits were also reduced among unmarried respondents, and mothers older than 25.

Conclusion: Null results of child-related tax benefits on postpartum health may indicate that they may not have been sufficient to counteract the impacts of tax benefits conditioned on employment status and may have nullified the tax credit's positive effects. The counterintuitive findings on Hispanic women and other minority groups could be a result that these immigrant women are unable to receive child-related tax benefits as they cannot work. Future work is needed to understand the specific impacts of child-related tax benefits on different subgroups of women.

Introduction

During pregnancy and the postpartum period (which includes three months after delivery), women undergo hormonal, physical, emotional, and psychological changes that dramatically impact their health and wellbeing. It is recognized that poverty and financial hardship can negatively impact the health of new mothers and infants, with several hypothesized mechanisms (see conceptual diagram, Figure 3.1).(1, 2) For example, nearly one in eight new mothers experience symptoms of postpartum depression and this can adversely affect the woman and her children.(3-5) Children of women with postpartum depressive symptoms are more likely to experience developmental delays, emotional and behavioral problems, and attachment problems.(6, 7)

Another critical activity in the postpartum period is breastfeeding, for women who are able and choose to breastfeed. There is evidence on protective effects of longer duration of breastfeeding on both the woman and the, infant yet breastfeeding continuation has remained low.(8) But the most recent 2020 birth data from the Center for Disease Control's National Immunization Survey showed that breastfeeding rates at 6 months were improved to 58.2% in the US though exclusive breastfeeding at six months was only 25.41%.(9) Studies show that increased financial stress is associated with breastfeeding discontinuation.(20-22)

The postpartum period is also critical in terms of access to healthcare and preventive visits and plays a vital role in ensuring overall maternal health. However, research indicates postpartum visits attendance estimates vary substantially, from 24.9% to 96.5%, with a mean of 72.1%.(10-14) Research also shows that attendance rates vary based on insurance status and are a result of racial and socioeconomic disparities.(15-17)

Social and economic policies that can improve financial security of women have the potential to improve their access to healthcare, housing, nutrition, reduce anxiety and stress, and encourage them to incorporate better health practices into their lives (see conceptual diagram, Figure 3.1). Thus, policies that work to mitigate financial hardship among peripartum women may address social determinants of maternal health to improve health equity.(1, 25-28) For instance, paid maternity leave policies that improve access to paid leave, have shown to reduce socio-demographic disparities, and resulted in enhanced health benefits among postpartum women.(18, 23, 24) Prior studies have also shown that these policies disproportionately benefit more advantaged women in taking leave and breastfeeding as the low-income women rarely qualify for paid maternity leaves as they are partially paid.(18, 19) The result can be an earlier return to work, which may adversely affect their physical recovery and mother-child bonding. Thus, financial support through economic policies is needed in addition of better access to childcare and workplace support to ensure equitable access to maternity leave.

In this study, we examine the impact of two child-related tax benefit policies that provide direct income support for parents. In the United States, the earned income tax credit (EITC) and the child tax credit (CTC) are tax benefits that assist low- and moderate-income taxpayers. The EITC was established in 1975 and initially provided a modest tax credit of up to $400 for low-income working families with children.(29) Since then, state and federal legislations have expanded the EITC, making it the largest permanent federal antipoverty program in the country (Supplement Figure 3.5). EITC tax benefits are contingent on work status, income, marital status, and qualifying children. For example, in 2019, $3,618 is the maximum credit a household can expect if they have one child, compared to a household without qualifying children that can earn up to $543.(29) The exact amount varies according to household earned income and marital status (Supplement Figure 3.6).

Economic studies have found that EITC increased family income, increased labor market participation and reduced poverty and that effects are stronger among single mothers.(30-34) There is mixed evidence on the EITC receipts on physical health outcomes as it has found to be associated with increases in obesity and worsen metabolic markers like cholesterol. On the other hand, studies have shown improvements in birth outcomes, child development, and mental health.(32, 35-39) In our study, we use a more rigorous methodology with a more recent dataset to provide more comprehensive evaluation of the EITC effect on health outcomes.

Evidence shows that EITC can impact the social determinants of health, including improvements in food security, housing and paying off debts.(40-42) Other than the federal EITC, state-level EITC is also offered by more than half of states. These supplemental EITCs offered by states vary substantially among states. State-level EITC programs have received less attention in research and some studies have found improved child health outcomes, greater health insurance coverage as well as reduced poverty.(43-46)

Meanwhile, the CTC was enacted in 1997 and initially offered $400 per child to families to assist in raising children's costs. Toward the end of fiscal year 2020, CTC provided $2,000 in tax relief per qualifying child, with up to $1,400 of that amount refundable subject to a refundability threshold, phase-in, and phase-out demonstrated in Supplement Figure 3.7).(47) Unlike EITC, CTC not only provided more financial assistance to low-income households it also aided middle-income households.(48) As such, it allowed more families to benefit from the tax credit. The limited research on CTC shows that it promotes work, has an additional anti-poverty effect and support's child development, and reduces behavioral problems in children.(49-51) State-based EITCs and CTCs are not considered in our work.

In the present study, we estimate the effects of child-related tax benefits on postpartum health based on data from the Centers for Disease Control and Prevention (CDC) Pregnancy Risk Assessment Monitoring System (PRAMS), a survey system that asks

mothers' questions about their pregnancy and postpartum.(52) We use quasi-experimental regression discontinuity (RD) analysis to evaluate the effects of child-related tax benefits on postpartum outcomes. The RD design is a rigorous non-experimental design with high internal validity and has been extensively employed to study health interventions' effects. To carry out this study, we exploit the predictability of tax season, which occurs between January 1 and April 15 each year, and a nuance in the tax system that households with infants born before December 31 in a given year receive substantial child-related tax benefits.
The findings of this study provide key insights into how child-related tax benefits impact postpartum health, at a time when federal and state policymakers are actively considering expanding and refining the design of these policies.

Methods

Data

Our study sample was drawn from the 2004-2021 waves of the Pregnancy Risk Assessment Monitoring Survey (PRAMS), an annual survey conducted by the Centers for Disease Control and Prevention in conjunction with states and other local agencies. PRAMS collects population-based data on sociodemographic information, health, maternal attitudes, and experiences prior to, during, and shortly after pregnancy, with participants sampled from birth certificates.(52) To invite potential participants, a pre-letter is sent, and invitations to respond to the PRAMS survey are sent up to three times. Survey data are linked with additional variables from birth certificates. Participants typically complete the survey four to seven months post-partum.(53)

We restricted our data to 2004 and after EITC expansions that distinguished between adults with/without children. Because of major changes in the EITC and CTC in 2021, we restricted the data to tax year 2020 and earlier. We restricted our study sample to nulliparous respondents, i.e., those with no prior children, since individuals without children are eligible for very small (if any) EITC refunds and no CTC. We also restricted the sample to those who met EITC & CTC eligibility criteria based on income and marital status (see sample flowchart, Figure 3.2). We further restricted our sample to those respondents who were interviewed after they were likely to have received the tax credits after filing their taxes.(54, 55) We assume all eligible respondents have filed taxes. Although prior studies have shown that about 80% of eligible households actually received their refunds, this strategy may result in measurement error.(56, 57) This strategy would give intent-to-treat estimates.(43, 58, 59)

Specifically, taxes are typically filed between January 1 to April 15 each year. Tax refunds with an additional child tax credit or EITC are issued by the Internal Revenue Service (IRS) after mid-February. Generally, e-filers receive their refunds within 21 days while a mailed return is expected to take between four and eight weeks. Individuals can expect a longer process if they file near the deadline.(60) Therefore, the sample was restricted to respondents with first newborns that were interviewed between March to July and our final size was 26,855.

We also restricted our analysis to observations with non-missing outcomes and non-missing covariates. The final sample size for each outcome was: ever breastfed N=24,347; breastfed greater than 1 month N=20,545; postpartum visit check-up N=18,384; postpartum depressive symptoms N=17,200. The differing number of observations for each outcome is because not all questions are asked in all waves or by all states.(61, 62)

Outcomes

We examined outcomes that are likely to change in the short term in response to an income boost as described in the conceptual framework. First, self-reported postpartum depressive symptoms were asked using two items 1) “How often have you felt depressed, hopeless, or down since your new baby was born?” and 2) “How often have you had little interest or pleasure in doing things since your new baby was born?” Each of the two items is scored from 0 (“never”) to 5 (“always”). The sum of the two items were categorized to construct a binary indicator of Postpartum depressive symptom.

We also examined two breastfeeding outcomes: 1) whether the child was ever breastfed and 2) whether the breastfeeding continued for greater than 4 weeks. Finally, we included whether the respondent attended a postpartum checkup visit, which typically occurs about 4-6 weeks after birth and includes a physical and mental health examination. The question in PRAMS was: “Since your new baby was born, have you had a postpartum checkup for yourself?”

Exposure

Our exposure of interest was receipt of the EITC during the postpartum period. We exploit a nuance of the tax system, in which parents with infants born on or before December 31 in a given year receive substantial child-related tax benefits a few months later when filing taxes, whereas those whose infants are born after December 31 must wait until the subsequent tax filing season one year later.(29, 63) Using a timeline figure (Supplement Figure 3.8) with an example of an exposed and control group, we explain how the exposed group receives the child-related tax benefits during the current tax filing season, while the non-exposed group does not. This difference in timing creates a short-term gap between the two groups, allowing us to measure the impact of the child-related tax benefits. This natural experiment enables us to study the effect of cash transfers to families with newborn children on postpartum health.

Covariates

Covariates included self-reported maternal characteristics: age (<25, 25-35, >35), education level (less than high school, high school, some college, college plus), marital status, and prenatal care insurance (Medicaid versus other sources). We also adjusted for self-reported race/ethnicity, categorized as Black, Hispanic, White, and other. The latter is a heterogeneous group that includes Asian/Pacific Islander (API), American Indian/Alaska Native, and others, since small cell sizes precluded us from including a more granular variable. As respondents are interviewed at different time points after their delivery, we also adjusted for the number of months between the delivery and interview (i.e., child age).

Statistical Analysis

To estimate the effects of additional income from the EITC and CTC on postpartum outcomes we implemented regression discontinuity (RD) analysis. RD design is a rigorous quasi-experimental approach suitable for program evaluation that assigns treatment status based on a cutoff or threshold. By comparing outcomes on either side of the threshold, researchers can make causal inferences about individuals or units just above or below a threshold.(64-66) To determine the feasibility of RD analysis, there must be a clear assignment rule and outcomes observable for both treatment and control groups.(64) In particular, we are able to implement a "sharp" RD design because of the clear cut-off in eligibility for child-rated tax benefits for those born on or before December 31 of each year, as similarly implemented in prior work.(63)

PRAMS data include information on the month of birth, which served as the treatment assignment variable. Of note, this treatment assignment variable is discrete and not continuous. Averages within arbitrary small neighborhoods of the cutoff point are no longer possible in the discrete case of RD even with an infinite amount of data, so the continuitybased local polynomial methods used in the case of a continuous treatment variable are not directly applicable.(67, 68) Literature suggests that researchers can select a particular functional form for the model relating the outcomes of interest to the treatment assignment variable as there is not a clear consensus on the modeling approach in this case.(67, 68) We performed estimation using global parametric regressions and also used non-parametric local estimation using the *rdrobust* package in Stata.

Global Parametric Estimation Equation

We estimated the effects of additional child-related tax benefits by using the following global parametric regression model with interactions:

$$Y = \alpha + \beta_1(D) + \beta_2 D * f(X - c) + \beta_3 f(X - c) + \beta_4 Covars + \beta_5 Year + \varepsilon$$

Y represents our outcomes of interest. The variable D is our treatment assignment rule that indicates if the respondents had a first birth on or before December 31. D is equal to 1 for respondents with first-born children during or before December whereas it's equal to 0 for those respondents with children born after December 31. $X - c$ is our centered running variable. In RD analyses, it is recommended to test multiple forms of f to evaluate the sensitivity of estimates of treatment effects to different model specifications.(64, 65, 69) We tested for different global parametric regression discontinuity estimate, including models formulated as linear, quadratic, cubic, or quartic as well as adding interaction terms. $Covars$ represents a vector of individual-level respondent characteristics determined before the treatment described above to improve precision. $Year$ represents indicator variables (i.e., fixed effects) for year of birth to account for any secular (i.e., underlying temporal) changes in the outcomes. To determine the closest functional form for our data, we would use *rdplot* Stata package.(68, 70)

For sensitivity analysis, we would also do global parametric regression model with interactions for other functional forms. For the robustness check, we re-estimated the models after restricting the sample to smaller bandwidths. In this analysis, we only compared the respondents with first-born children in the month of December (exposed) and respondents with children born in January (control). We select the linear or non-linear relationship based on the way of visualizing the relationship between the outcome and the treatment assignment variable using the *rdplots* package in Stata for different linear and polynomial functional forms. The cubic functional form was selected to make sure the functional form that is specified is as close as to correct functional form (Supplement Figure 3.4).

Local Polynomial Non-Parametric Estimation

We estimated the effects of additional child-related tax benefits using non-parametric approach (using the *rdrobust* package in Stata) that allows for local linear regression and involves selection of bandwidth, kernel and functional forms (using *rdplot* package in Stata).(64, 65, 69, 71) Global parametric models use all observations in the sample whereas local polynomial regressions are based on the subset of data points.(72)

Stratified Analyses

Additionally, we evaluated whether the EITC and CTC child-related tax benefits affect outcomes of interest among racial and ethnic subgroups that may benefit more. To do so, we estimated parametric and non-parametric models separately for each subgroup. We performed similar stratified analyses by marital status and mother's age-groups.

RD Assumptions

RD analysis relies on several assumptions to produce valid estimates. The first assumption is that treatment or control assignment cannot be manipulated. In our case, it would have been a concern if respondents were manipulating the month of birth to take advantage of child-related tax benefits. While this is unlikely given the many biological and clinical factors that affect timing of birth that would outweigh such tax-related preferences, we constructed histograms of the assignment variable (i.e., month of birth) to detect possible irregularities or potential manipulation around the December 31 cutoff. Possible manipulation could be indicated by bundling at the threshold in December.

The second assumption requires that baseline covariates are balanced above and below the threshold, i.e., that there are no compositional changes in births just around the threshold. While there are known seasonal differences in the composition of births, there is no evidence of dramatic changes on a shorter month-to-month basis.(73, 74) Nevertheless, we plotted baseline covariates before and after January to examine whether there is any such discontinuity in the covariates, and we performed regression analysis comparing covariates before and after the threshold. We included baseline covariates in the RD model to account for any potential confounding.

Another assumption is that there are no other programs using the same threshold at the same time. We are not aware of any other non-tax benefits based on the December 31 threshold. Nevertheless, such unobserved confounders are considered a limitation of any quasi-experimental analysis.

Results

Sample Characteristics

The analytical sample consisted of 11,275 children included in the exposed group (respondents with first newborns from July to December) and 15,580 in the control group (respondents with first newborns after 31st December from January to May). Most characteristics in the two groups were similar (Table 3.1), although there was an overrepresentation of White women (42.2 % vs. 50.1%) and underrepresentation of Hispanic women (21.4 % vs. 17.9%) in the exposed group. Also, among respondents of the control group, the average age of the infant at the time of the PRAMS interview was higher in the exposed group than that of the control group (4.8 vs. 3.6 months).

Evaluation of RD Assumptions

The distribution of the month of birth did not show any irregularities around the time of tax refunds using the entire data (Supplement Figure 3.1). Similarly, among respondents included in the RD analysis, there was no evidence of significant discontinuities other than the cut-off on 31st December (Supplement Figure 3.2). This allows us to interpret that factors other than the child tax related benefits will not influence the estimates from RD analysis. In addition, using graphical evaluation, we found there were no significant discontinuities in observed characteristics except for Hispanics and Whites women (Supplement Figure 3.3). Quantitative evaluation confirmed this, finding no statistically significant differences for most characteristics, suggesting there was no difference in composition between the exposed and control groups. However, there were statistically significant differences in composition among Whites (-0.030, 95% CI (-0.050, -0.010), p-value (0.003)) and Hispanics women (0.019, 95%) (Supplement Table 3.3). We adjusted for all baseline covariates in the RD model but cannot rule out the possibility of imbalance in relevant unobserved characteristics, which is a limitation of all quasi-experimental studies.

Effect of Child-related Tax Benefits on Postpartum Health

The effects of child-related tax benefits on our outcomes of interest using global parametric cubic regression with interactions were not statistically significant (Table 3.2). When we accounted for other multiple functional forms like linear and quadratic, our estimates slightly varied, and their signs remained the same (Supplement Table 3.1 & 3.2).

The effects of child-related tax benefits on our outcomes of interest using non-parametric local regressions were not statistically significant, similar to the global parametric regression results. (Table 3.3) These results could be indicative that child-related tax credits are insufficient as they are conditioned on working which may have nullified the tax credit's positive effects.

Subgroup Analyses

In stratified analyses, we found that among Hispanic women breastfeeding decreased (-0.032, 95% CI (-0.062, -0.002), p=0.035) and postpartum depression increased (0.029, 95% CI (0.004, 0.055), p=0.025). Postpartum visits were reduced for those in "other" racial/ethnic subgroups (-0.035, 95% CI (-0.062, -0.008), p=0.011). Hispanic women and other minority groups may be unable to receive child-related tax benefits because they are unable to work since they are more likely to be immigrants not eligible for the programs.

Stratified analyses by marital status showed that postpartum visits for unmarried respondents (-0.016, 95% CI (-0.030, -0.003), p=0.016) decreased (Table 3.5). For mother's age greater than 25, postpartum visits decreased (-0.014, 95% CI (-0.028, -0.001), p= 0.035)

(Table 3.6). This evidence is timely because policymakers are looking at extending postpartum Medicaid coverage as an alternative policy to encourage postpartum visits.(75)

Discussion

Peripartum women are highly vulnerable to financial hardship and policy interventions are needed to support their health and wellbeing.(1) We contribute to this growing literature by providing new evidence on the effects of timing of child-related tax credits provided during this period. The quasi-experimental RD analysis was used to examine the effects of child-related tax benefits on postpartum health, exploiting the clear cut-off in eligibility for child-rated tax benefits for those born on or before December 31 of each year as the treatment assignment rule for a sharp RD. We were unable to reject the null hypothesis of no effect on the different postpartum health outcomes, other than in the subgroup analyses. We found that breastfeeding decreased, and postpartum depression increased among Hispanic women, and postpartum visits were reduced for other minority racial subgroups, unmarried respondents, and mothers older than 25.

Our results could be indicative that child-related taxes are conditioned on employment status and may have nullified the tax credit's positive effects, since women may need to return to work to continue receiving benefits, especially in the absence of a national paid leave program. The null results could be a result of our limited final sample size or the possibility that the credit amount was not sufficient. These counterintuitive findings on Hispanic women and other minority groups may be indicative that these women are unable to receive the child-related tax benefits as they are unable to work. Since, Hispanic women are more likely to be immigrants not eligible for either program. Our results are not consistent with limited prior work that has been done in the past to study the impact of the child-related tax benefits on the pregnancy and postpartum health. Earlier studies shows that child related tax benefits result in increased infant birthweight, increased likelihood of breast feeding and better mental health. (36-38, 46, 75)

This study has several strengths. First, it used a large diverse data set with rich individual level including several relevant health outcomes. Secondly, it is based on a strong quasi-experimental design, as participants are unable to manipulate the assignment variable (i.e., month of birth).

This work also has limitations. First, we could not predict the exact child-related tax benefits amounts as we only have access to income categories in our dataset. There could also be measurement error because we do not know which participants filed their taxes to receive the benefits. This may have contributed to our null results, although this is analogous to an intent-to-treat design and represents policy effects at a population level. Furthermore, our work does not consider the impact of state based EITCs and CTCs. Secondly, our final sample size was too limited to estimate the effects of child-related tax benefits on postpartum health outcomes. Therefore, our estimates of child-related tax benefits were also highly variable across model specifications and had wide confidence intervals. Additionally, we had only month of birth data available in our data instead of date of birth, which was a precluded us from using non-parametric RD analysis easily and prevented us from considering a more granular treatment assignment variable. Finally, while the estimate of RD provides a local average treatment effect and is considered to have high levels of "internal validity," it does not have high levels of "external validity" and our results therefore cannot necessarily be generalized to other populations or other income interventions.

In conclusion, this research is important to explore the effects of child-related tax benefits, as it helps inform policymakers about the design of economic supports to families after childbirth. It adds to previous research demonstrating the importance of considering the broader social and economic context to improve postpartum maternal health outcomes. The implications of our research are particularly significant at a time when postpartum Medicaid benefits are being discussed as being extended for one year after delivery.(75) Future work is needed to understand the specific impacts of child-related tax benefits on different subgroups of women. This can help to identify strategies to ensure that benefits are targeted to the people who need them most. Additionally, policies that support women in the labor market, such as childcare subsidies, may be necessary to ensure that these benefits are effective. Further research is needed identify the most effective strategies, including exploring how they impact mothers' decisions to return to work.

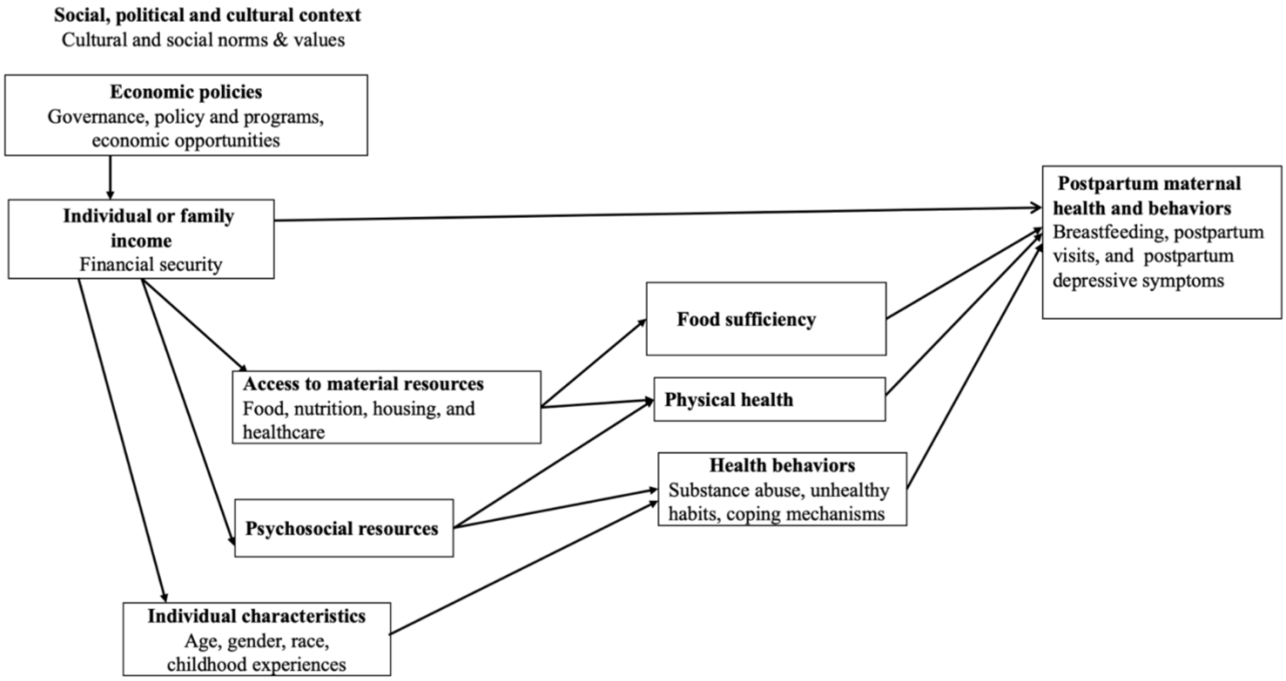


Figure 3.1. Conceptual framework linking economic policy, financial security, and postpartum maternal health.

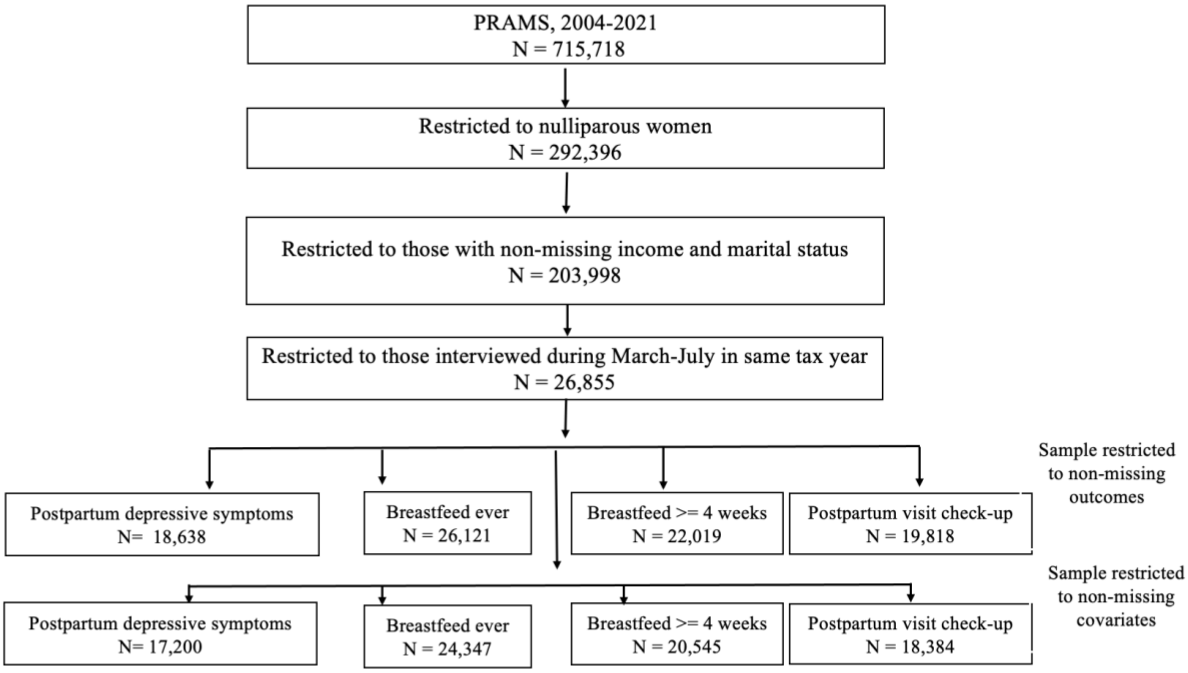


Figure 3.2. Study sample flow chart.

Note: Data drawn from the 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control.

Table 3.1. Sample Characteristics.

	Respondents with first-born from July to December (exposed) N= 11,275		Respondents with first-born from January to May (control) N = 15,580	
VARIABLES	Mean /N	SD / %	Mean /N	SD / %
Outcome Variables				
Postpartum				
Postpartum depressive symptoms	1351	16.80	1740	16.42
Breastfeed ever	9,259	84.35	12,917	85.29
Breastfeed >= 4 weeks	6,442	69.94	9,093	70.99
Postpartum visit check-up	7,710	89.13	10,172	91.08
Socio-demographic characteristics				
Mother's age (years)				
<25	6,644	58.93	8,354	56.83
25-35	4,049	35.91	5,880	37.74
35+	582	5.16	846	5.43
Mother married (%)	4,278	37.94	6,692	42.94
Race				
Black	2,342	21.22	2,560	17.02
Hispanic	2,366	21.44	2,701	17.95
White	4,656	42.19	7,651	50.83
Other	1,673	15.16	2,139	14.21
Mother's Education				
Less than high school	1,662	14.90	2,095	13.58
High school	4,107	36.82	5,375	34.83
Higher education	3,836	34.31	5,473x`	35.47
College +	1,549	13.89	2,488	16.12
Prenatal care insurance paid by Medicaid versus others	7,026	64.84	9,356	61.78
Child's age	4.77	1.40	3.59	0.90

Note: Sample was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS).

Table 3.2. Main results- global parametric estimation: Effect of income credits after the first child is born in the household on postpartum maternal health.

	Postpartum depressive symptoms	Breastfeed ever	Breastfeed >= 4 weeks	Postpartum visit check-up
Born before December	-0.048	-0.011	-0.056	0.040
95% CI	[-0.129, 0.034]	[-0.073, 0.051]	[-0.146, 0.035]	[-0.021, 0.101]
p-value	(0.252)	(0.729)	(0.227)	(0.196)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. Coefficients represent the results from global parametric cubic regression discontinuity analysis and represents the effect of increased credits on the outcomes of interest. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, race/ethnicity, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Table 3.3. Local polynomial non-parametric estimation: Effect of income credits after the first child is born in the household on postpartum maternal health.

	Postpartum depressive symptoms	**Breastfeed ever**	**Breastfeed >= 4 weeks**	**Postpartum visit check-up**
Born before December	-0.009	-0.027	-0.017	0.011
95% CI	[-0.041, 0.023]	[-0.058, 0.003]	[-0.052, 0.018]	[-0.018, 0.041]
Robust 95% CI	(-0.092, 0.027)	(0.113, 0.183)	(-0.090, 0.042)	(0.515, 0.577)
Robust p-value	0.282	0.000	0.474	0.000
Polynomials	1	2	1	2

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. Coefficients represent the results from non-parametric regression discontinuity analysis using *rdrobust* packages and represents the effect of increased credits on the outcomes of interest. Package *rdrobust* allowed us to do robustness check by providing robust standard errors and confidence intervals. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, race/ethnicity, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Table 3.4. Stratified analyses- global parametric estimation: Effect of income credits after the first child is born in the household on postpartum maternal health by racial subgroups.

Born before December 95% CI P-value	**Postpartum depressive symptoms**	**Breastfeed ever**	**Breastfeed >= 4 weeks**	**Postpartum visit check-up**
Black	-0.006	-0.005	0	-0.003
	[-0.036, 0.024]	[-0.032, 0.021]	[-0.033, 0.034]	[-0.025, 0.019]
	(0.686)	(0.687)	(0.985)	(0.787)
Hispanic	0.029*	0.003	-0.032*	-0.017
	[0.004, 0.055]	[-0.015, 0.020]	[-0.062, -0.002]	[-0.040, 0.006]
	(0.025)	(0.772)	(0.035)	(0.155)
White	0.008	-0.007	-0.016	-0.002
	[-0.011, 0.028]	[-0.022, 0.009]	[-0.038, 0.005]	[-0.016, 0.012]
	(0.390)	[0.400]	(0.138)	[0.757]
Other races	0.014	-0.007	-0.012	-0.035*
	[-0.021, 0.049]	[-0.030, 0.015]	[-0.046, 0.022]	[-0.062, -0.008]
	(0.423)	(0.513)	(0.492)	(0.011)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The models were rerun for each racial sub-group Black, Hispanic, White, and other racial groups combined. Coefficients represent the results from global parametric linear regression discontinuity analysis and represents the effect of increased credits on the outcomes of interest for each racial category. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Table 3.5. Stratified analyses- global parametric estimation: Effect of income credits after the first child is born in the household on postpartum maternal health by mother's marital status.

Born before December 95% CI P-value	**Postpartum depressive symptoms**	**Breastfeed ever**	**Breastfeed >= 4 weeks**	**Postpartum visit check-up**
Married	0.014	-0.01	-0.018	-0.002
	[-0.005, 0.034]	[-0.023, 0.003]	[-0.038, 0.002]	[-0.016, 0.012]
	(0.144)	(0.139)	(0.082)	(0.767)
Not married	0.008	-0.001	-0.015	-0.016*
	[-0.009, 0.025]	[-0.015, 0.013]	[-0.035, 0.005]	[-0.030, -0.003]
	(0.379)	(0.915)	(0.130)	(0.016)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The models were rerun for mother's marital status. Coefficients represent the results from global parametric linear regression discontinuity analysis and represents the effect of increased credits on the outcomes of interest for each racial category. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, race/ethnicity, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Table 3.6. Stratified analyses- global parametric estimation: Effect of income credits after the first child is born in the household on postpartum maternal health by mother's age.

Born before December 95% CI P-value	**Postpartum depressive symptoms**	**Breastfeed ever**	**Breastfeed >= 4 weeks**	**Postpartum visit check-up**
Mother's age < 25	0.016	0.001	-0.018	-0.008
	[-0.002, 0.034]	[-0.013, 0.015]	[-0.038, 0.002]	[-0.022, 0.006]
	(0.085)	(0.922)	(0.071)	(0.290)
Mother's age >25	0.004	-0.012	-0.014	-0.014*
	[-0.014, 0.023]	[-0.026, 0.002]	[-0.033, 0.006]	[-0.028, -0.001]
	(0.646)	(0.095)	(0.180)	(0.035)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The models were rerun for each mother's age category age less than 25 and age greater than 25. Coefficients represent the results from global parametric linear regression discontinuity analysis and represents the effect of increased credits on the outcomes of interest for each mother's age category. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like race/ethnicity, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Appendix

Appendix Table 3.1. Sensitivity analysis - global parametric estimation: Effect of income credits after the first child are born in the household on postpartum maternal health.

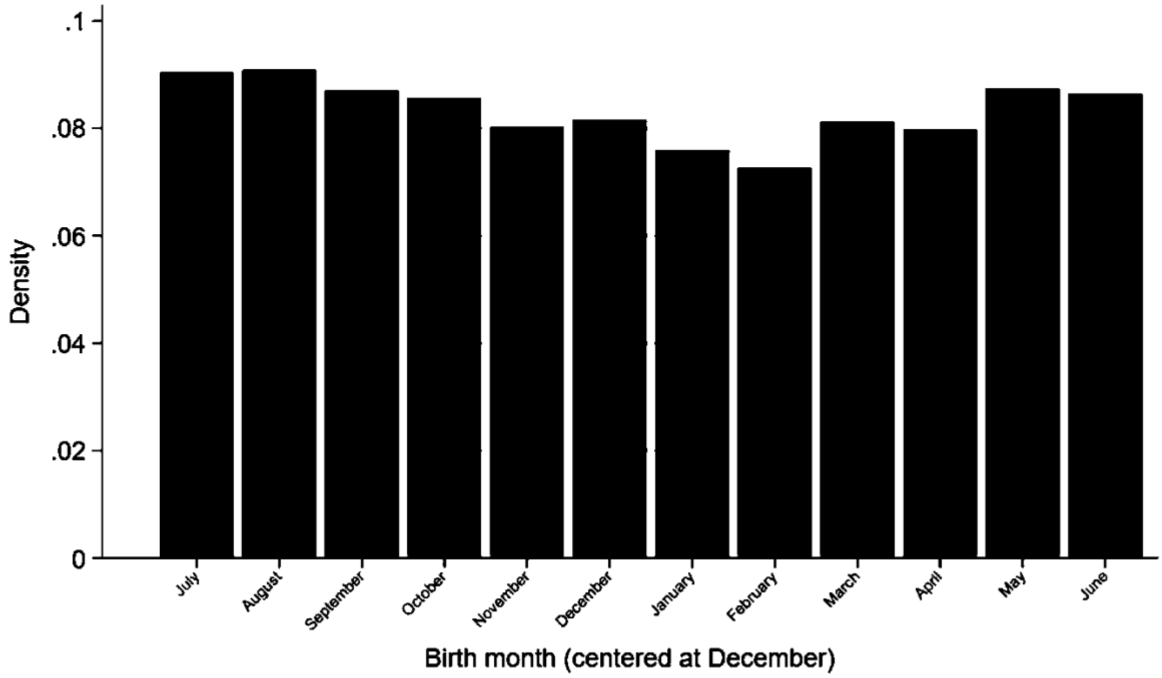
	Linear	Quadratic	Quartic
Postpartum depressive symptoms	0.009	0	-0.114
	[-0.011, 0.029]	[-0.038 - 0.037]	[-0.355, 0.127]
	(0.392)	(0.992)	(0.355)
Breastfeed ever	-0.008	-0.029*	-0.066
	[-0.024, 0.007]	[-0.058, -0.001]	[-0.252, 0.120]
	(0.305)	(0.045)	(0.486)
Breastfeed >= 4 weeks	-0.015	-0.008	0.031
	[-0.037, 0.008]	[-0.049, 0.033]	[-0.232, 0.295]
	(0.196)	(0.706)	(0.816)
Postpartum visit check-up	-0.003	0.01	0.113
	[-0.018, 0.013]	[-0.019, 0.038]	[-0.064, 0.291]
	(0.712)	(0.505)	(0.209)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. Each cell shows a coefficient estimated from different global parametric regression discontinuity estimate (models formulated as linear, quadratic, or polynomial (of power 4)) from a separate regression and represents the effect of increased credits on the outcomes of interest. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, race/ethnicity, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.

Appendix Table 3.2. Restrictive parametric estimation -Effect of income credits after the first child is born in the household on postpartum maternal health.

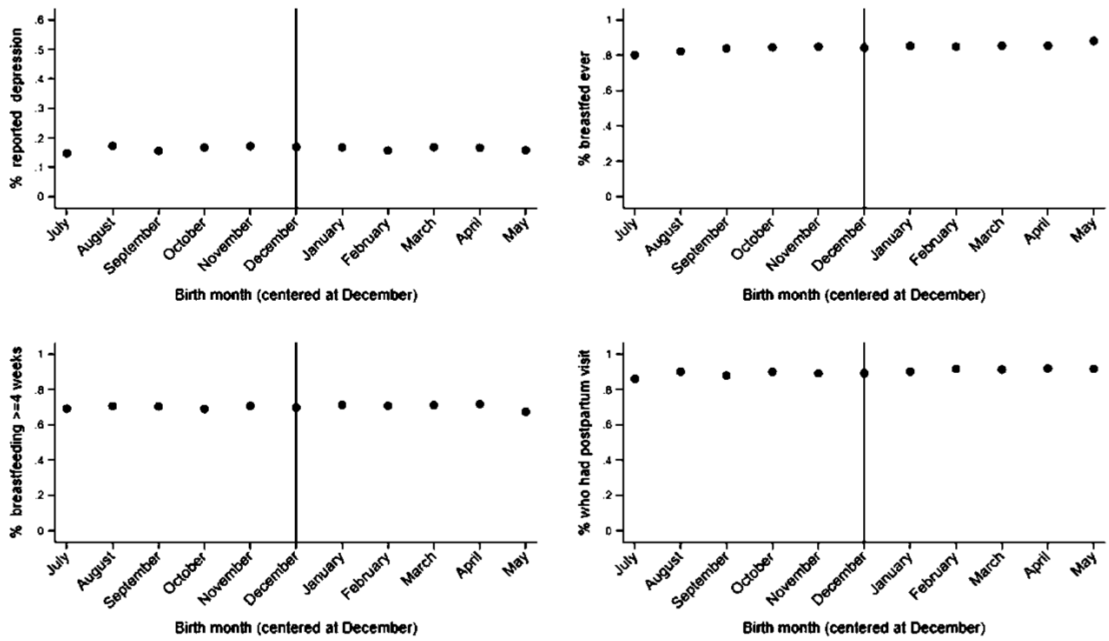
	Postpartum depressive symptoms	**Breast feed ever**	**Breastfeed >= 4 weeks**	**Postpartum visit check-up**
Born in Dec vs. Jan	0.003	-0.013	-0.013	-0.002
[95% CI]	[-0.016, 0.022]	[-0.027, 0.002]	[-0.033, 0.008]	[-0.017, 0.012]
P-value	(0.737)	(0.082)	(0.225)	(0.739)

Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). The sample is restricted to the respondents with first-born children in the month of December as exposed whereas those respondents with children born in January as control group. Coefficients represent the results from linear regression discontinuity analysis and represents the effect of increased credits on the outcomes of interest. All models were restricted to sample with EITC income eligibility and adjusted for individual mother's characteristics like age, race/ethnicity, marital status, and level of education as well as fixed effects for years. We also controlled for time gap between birth and interview in months.



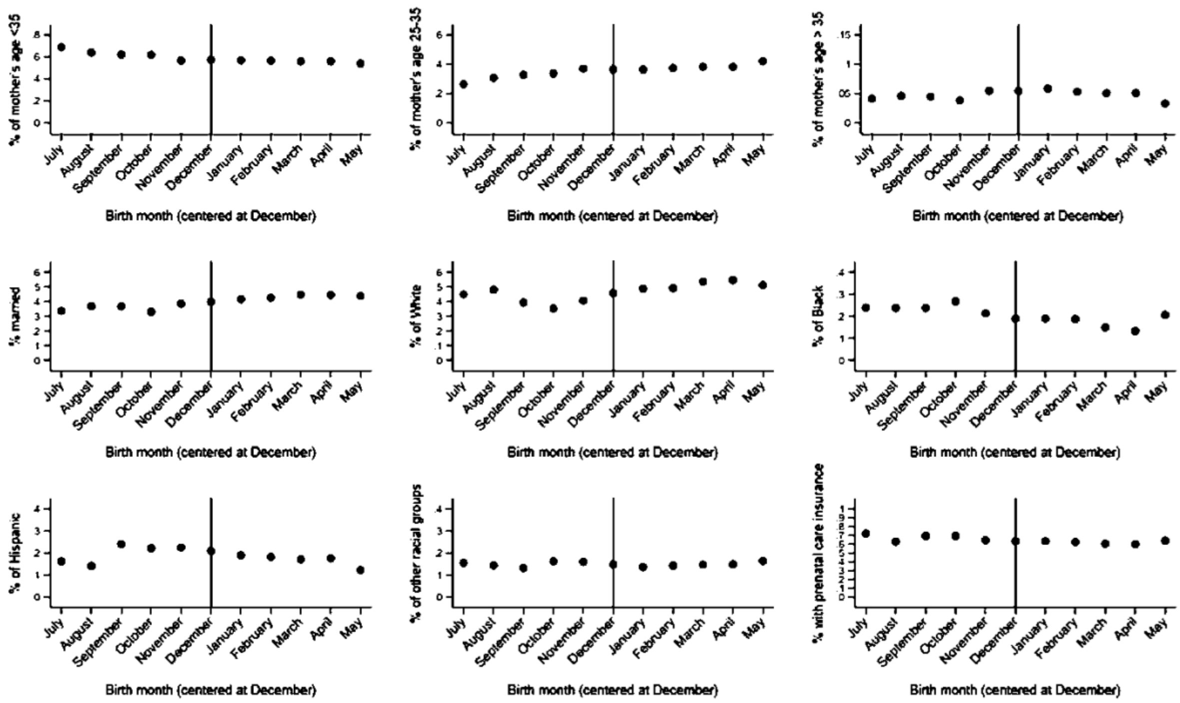
Appendix Figure 3.1: Distribution of children’s month of birth using PRAMS 2004-2021.

Note: Individual level data was drawn from the 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). We used the entire sample to calculate the histogram for children's month of birth distribution and centered on December.

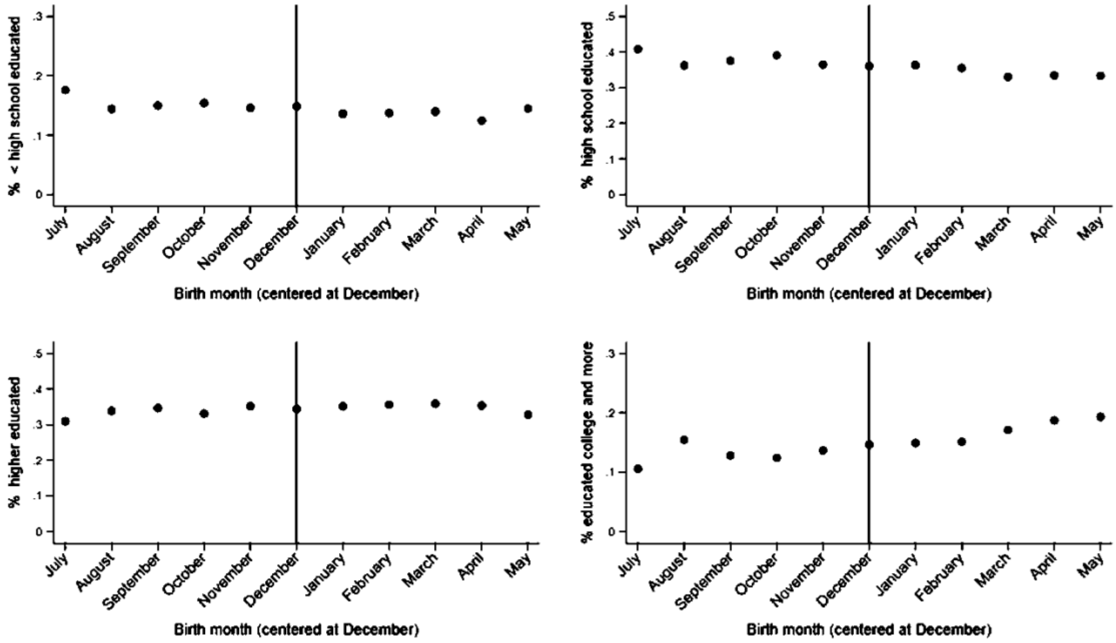


Appendix Figure 3.2. Graphical test to see any discontinuities in outcomes.

Note: Scatter plot to test discontinuities in outcome according to the month of birth of the first newborn and centered on December. The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The dots in the scatter plot represent the monthly mean of each variable and the values to the left of the black vertical line represents the respondents with the first newborn before December whereas the right of the vertical line represents January month onwards.



Appendix Figure 3.3. Graphical test to see any discontinuities in covariates.
Note: Scatter plot to test discontinuities in covariates according to the month of birth of the first newborn and centered on December. The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The dots in the scatter plot represent the monthly mean of each variable and the values to the left of the black vertical line represents the respondents with the first newborn before December whereas the right of the vertical line represents January month onwards.



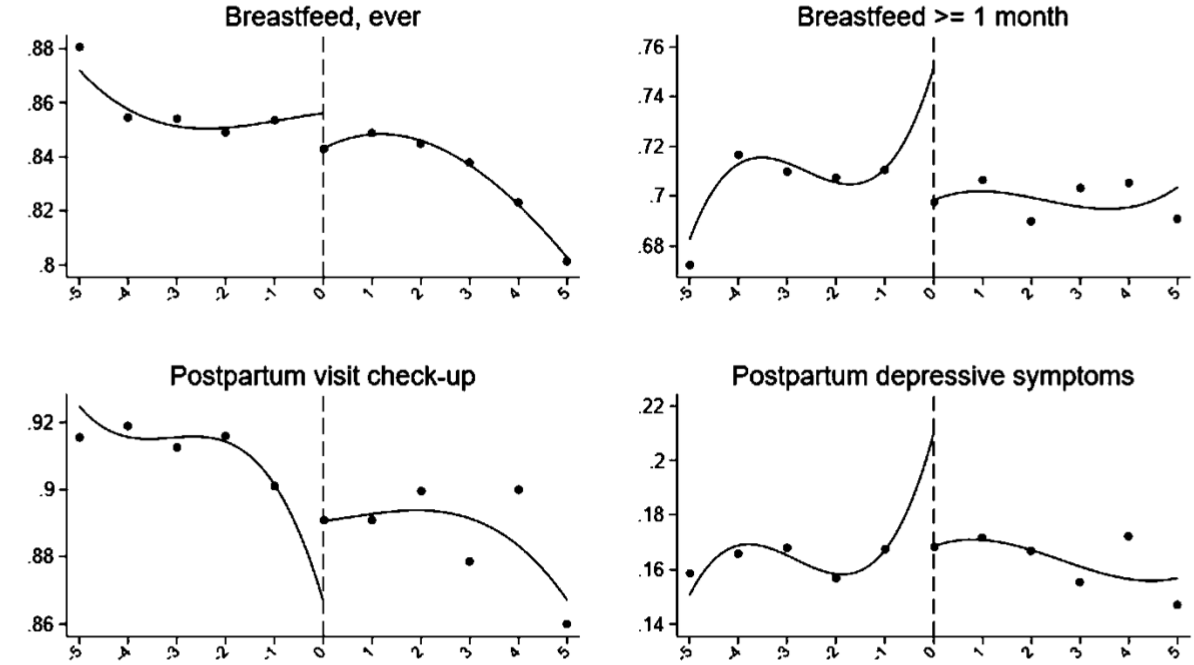
Appendix Figure 3.3. Graphical test to see any discontinuities in covariates, continued.
Note: Scatter plot to test discontinuities in covariates according to the month of birth of the first newborn and centered on December. The sample is restricted to the respondents whose first-borns were born from July to December as exposed, and those whose first-borns were born after 31st December ranging from January to May as control. The dots in the scatter plot represent the monthly mean of each variable and the values to the left of the black vertical line represents the respondents with the first newborn before December whereas the right of the vertical line represents January month onwards.

Appendix Table 3.3. Balance on baseline characteristics.

Variables	Born in Dec 95% CI P-value
Mother's age <25	0.003 [-0.016, 0.023] (0.740)
Mother's age 25-34	0.001 [-0.018, 0.019] (0.942)
Mother's age <35+	-0.004 [-0.013, 0.005] (0.391)
Mother married (%)	-0.017 [-0.036, 0.002] (0.083)
Mother's race	
Black	-0.001 [-0.016, 0.015] (0.905)
Hispanic	0.019* [0.004, 0.035] (0.016)
White	-0.030** [-0.050, -0.010] (0.003)
Other	0.012 [-0.002, 0.026] [0.095]
Mother's Education	
Less than high school	0.012 [-0.001 - 0.026] [0.077]
High school	-0.003 [-0.021 - 0.016] [0.790]
Higher education	-0.007 [-0.026 - 0.011] [0.447]
College +	-0.003 [-0.016 - 0.011] [0.721]
Prenatal care insurance paid by Medicaid versus others	-0.001 [-0.020 - 0.018] [0.903]

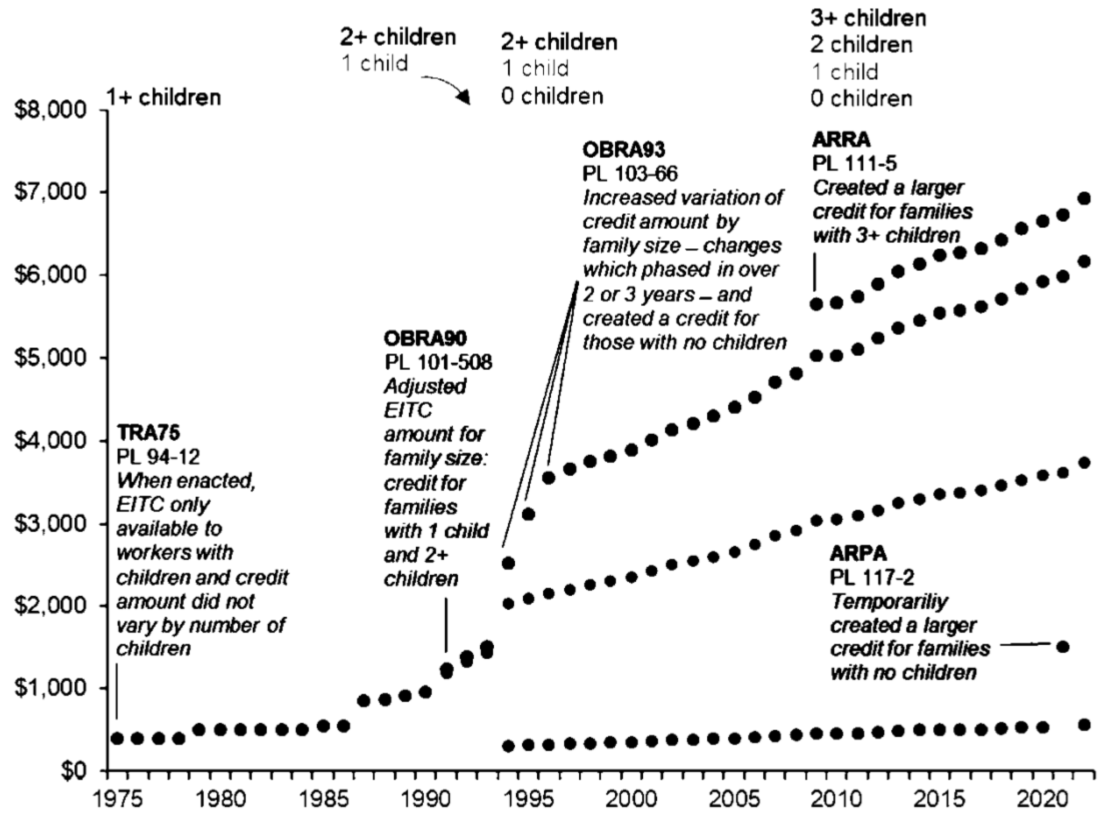
Note: **p < 0.01, *p < 0.05. Individual level data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS). (Figure caption continued on the next page.)

(Figure caption continued from the previous page.)
The sample is restricted to the respondents with first-born children in the month of December as exposed whereas those respondents with children born in January as control group. Coefficients are obtained from the basic linear regression analysis with a separate regression for each baseline characteristic in the model added as a dependent variable. To ensure that there is a balance in the baseline covariates between exposed and controlled group.



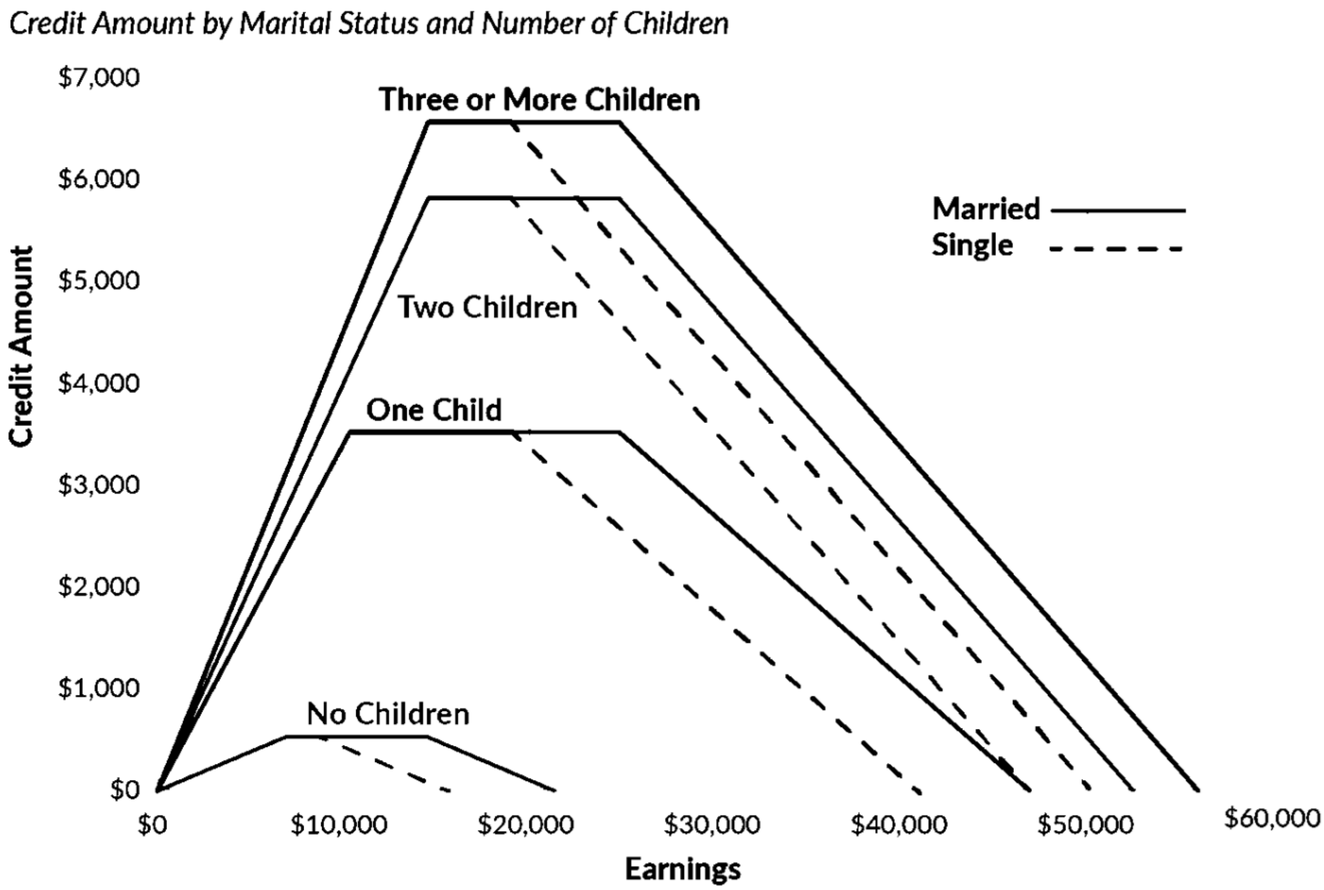
Appendix Figure 3.4. ***Rdplots*** **for cubic functional form.**

Note: *Rdplots* to find the functional form for the outcomes. The x-axis shows months centered on December. On the left side that ranges from July to December shows the exposed group and on the right side that ranges from January to May shows the control group. Data was drawn from 2004-2021 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS).

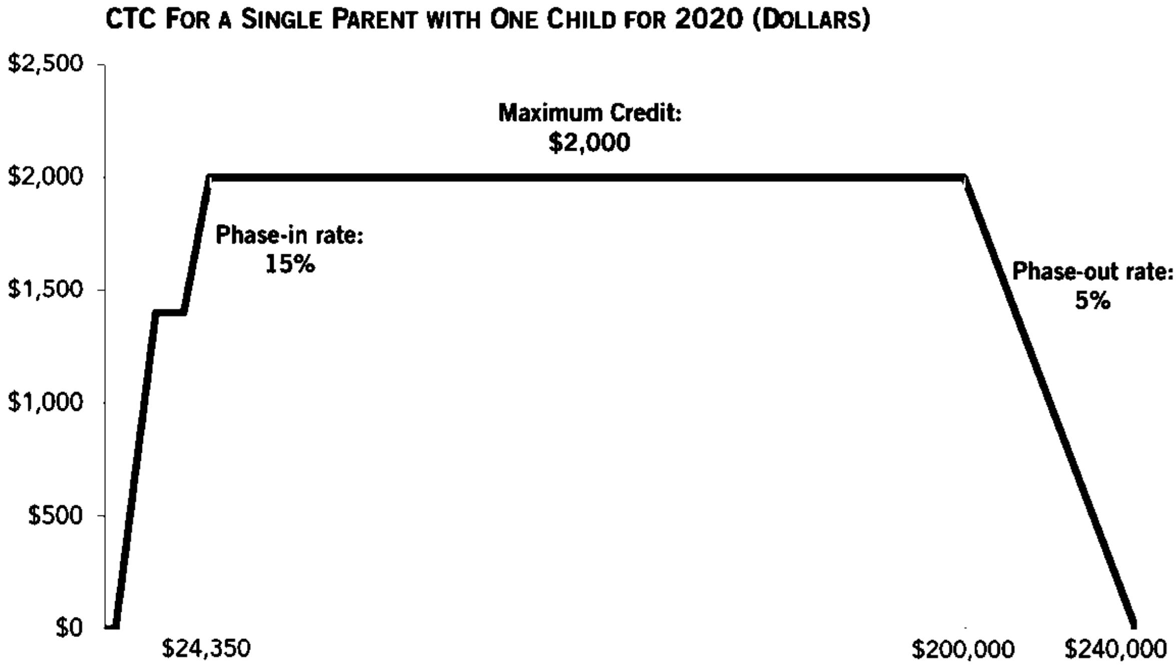


Appendix Figure 3.5. Maximum EITC Amount by Number of Qualifying Children, 1975-2022.

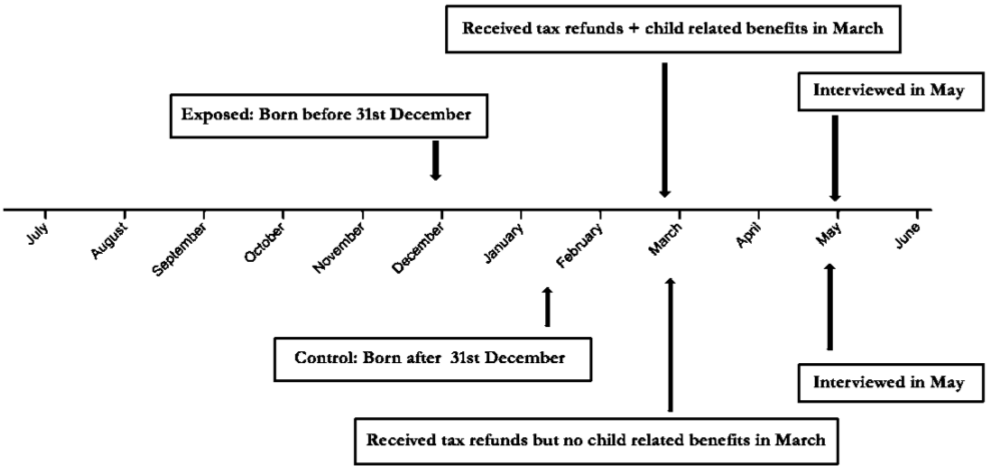
Note: Maximum EITC Amount by Number of Qualifying Children, 1975-2022. Reprinted from "The Earned Income Tax Credit (EITC): A Brief Legislative History," by M. Crandall-Hollick, 2022, Congressional Research Service, 1. Abbreviations: EITC Earned income tax credit; ARPA: American Rescue Plan Act; OBRA: Omnibus Budget Reconciliation Act; TRA: Tax Reform Act.



Appendix Figure 3.6. The Phase-In and Phaseout of the EITC.
Note: The Phase-In and Phaseout of the EITC Reprinted from "The Earned Income Tax Credit (EITC): A Primer," by Robert Bellafiore, 2019, Tax Foundation. Abbreviations: EITC Earned income tax credit.



Appendix Figure 3.7. CTC amount by income, 2020.
Note: CTC amount by income, 2020 Reprinted from “What is the child tax credit?” 2020, Peter G. Peterson Foundation. Abbreviations: CTC Child tax credit.



Appendix Figure 3.8. Understanding the timeline for exposed and control groups.

Note: Understanding the timeline using an example of exposed groups above the line and control groups example below the line. This figure illustrates how the exposed group receives child-related tax benefits during the current tax filing season, while the control group does not.

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