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Effects of the 2021 expanded Child Tax Credit on parents' well-being and time use☆

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ABSTRACT

The American Rescue Plan Act of 2021 temporarily provided unconditional monthly cash benefits to most households with children to reduce child poverty during the COVID-19 pandemic. Using the American Time Use Survey and Well-Being Supplement, we examine the effects of the 2021 Child Tax Credit (CTC) expansion on well-being and time-use activities of households with children. We find that the CTC expansion was associated with improved parental well-being and health. The analysis also shows a significant increase in the time the parent spends with the child. The results are robust to several robustness checks and consistent with existing evidence.

1. Introduction

The COVID-19 pandemic has caused severe financial hardship for the U.S. population due to sharp increases in unemployment rates and declining incomes. Many studies have shown a significant deterioration in people's mental health and well-being during the pandemic (Ettman et al., 2020; Vindegaard and Benros, 2020). In response to these challenges, the American Rescue Plan (ARP) Act temporarily expanded the federal Child Tax Credit (CTC) in 2021. The benefit size was increased from $2000 per child to $3600 per child for children 0–5 years old and to $3000 per child for children 6–17 years old. Moreover, the credit was made fully refundable, and eligibility was extended to households with little or no earnings. The 2021 CTC increased access to the benefits for approximately 26 million children whose families had previously been partially or completely ineligible for the credit. The first monthly payment went to more than 59.3 million children in July 2021, while subsequent payments reached more than 61 million children, covering 90% of American families with children (Department of Treasury, 2021).

Existing literature on the effects of CTC expansion has shown a decrease in child poverty and food insecurity among parents (Pilkauskas et al., 2024a). Families also spend more on food, housing, and child-related goods and services (Schild et al., 2023). The CTC expansion improved parents' mental health outcomes (Cha et al., 2023), including fewer bad mental health days and fewer depressive and anxiety symptoms (Pignatti and Parolin, 2024). However, the reform had no significant effects on parents' employment or labor force participation (Ananat et al., 2024; Pac and Berger, 2024).

In this paper, we exploit data from the American Time Use Survey (ATUS) and Well-Being (WB) Supplement module to estimate the impact of the 2021 CTC reform on parents' well-being and time use. We impute a CTC value to proxy how much different families would likely benefit from the policy change and include this predicted CTC value in a difference-in-differences (DD) model. Specifically, in the model, CTC benefits vary by the number of children in the household, the children's ages, and the period studied.

This study contributes to the existing literature on child benefits in two main ways. First, to the best of our knowledge, this is the first study to provide an empirical analysis of the impact of CTC expansion on well-being using ATUS and WB Supplement data. The ATUS data contain measures of daily activities. In the WB Supplement module, the survey randomly selects three activities reported by each respondent. For each of the selected activities, respondents were asked how they felt along five dimensions (presented in a randomized order): happy, stress, pain, sad, and tiredness. Respondents were also asked questions about their general health. These data are widely used in the literature, but have not been used to study the effect of the 2021 CTC reform. Second, we also investigate the effects of the 2021 CTC reform on parental time use. To the best of our knowledge, this is the first study to

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provide an empirical analysis of the impact of unconditional monthly cash transfers on time-use activities among the U.S. population. This analysis is essential for developing a comprehensive evaluation of the reform and determining whether parents spend more time with and invest more resources in their children. Thus, we contribute to a small but growing collection of literature that seeks to investigate the effects of universal child benefits on parental well-being and time use in North America. Contrary to most other cash assistance programs in the U.S. (e.g., Earned Income Tax Credit), the 2021 reform temporarily transformed the CTC into a near-universal unconditional cash transfer to families with children for 2021. Our paper offers important implications for policymakers and could contribute to the ongoing debate regarding universal child benefits in the U.S. In addition, many studies have analyzed the association between extra family income and well-being/health, but few have examined causality. Here, we exploit exogenous policy-induced variations in family income to estimate the causal impact of income on several measures of well-being and time use.

We find that the CTC expansion led to a significant decrease in sadness for parents, but no significant changes in happiness, stress, pain, or tiredness measures. Estimates suggest that the reform improved parents' self-reported health. The analysis also shows a significant increase in the time parents spent with the child (called *childcare* in the paper later). Our findings are robust to several robustness checks and consistent with existing evidence on the relationship between CTC expansion and well-being.

The remainder of the paper is organized as follows. Section 2 describes the 2021 CTC expansion. Sections 3 and 4 describe, respectively, the sample data and the methodology. In Section 5, we present the empirical results. In Section 6, we discuss the results and conclude the paper.

2. Reform

Established in 1997, the CTC was intended to provide financial assistance to middle-income families.[1] Over time, the amount of the credit increased and, as of 2017, tax filers could receive a maximum CTC of $2000 per child per year. However, many children did not receive the full refundable amount or were ineligible for the benefit entirely. Indeed, families had to earn at least $2500 to claim the benefit. The CTC phased in at a rate of 15% for each dollar above $2500, up to a maximum of $1400. This was the maximum refundable amount of the CTC, which could be supplemented with a non-refundable portion of the tax credit, until reaching $2000. Thus, before the reform in 2021, the CTC was not fully refundable. As a result, one in three children were ineligible for the full benefit because their parents did not earn enough to be eligible. Children in single-parent families, in rural areas, from ethnic and racial minorities, and in large families were most likely to be ineligible for the full credit (Collyer et al., 2019; Curran and Collyer, 2020).

In 2021, the ARP temporarily transformed the CTC into a near-universal child benefit. Three fundamental changes took place: (i) the law extended eligibility to households with little or no income; (ii) the law made the benefit fully refundable and distributed monthly for the second half of 2021; and (iii) the maximum credit amount increased to an annual maximum of $3600 per child under age 6 and $3000 per child ages 6 to 17. The first monthly payment for the expanded CTC was made on July 15, 2021, and the final payment was made on December 15, 2021. During this 6-month period, the credit was distributed monthly—namely, up to $300 per child under the age of 6 and up to $250 per child 6–17 years old. Thus, the reform changed the frequency of benefits, with half of the annual amount as monthly payments from July 2021 to December 2021 and the other half as a lump-sum payment at tax time in spring 2022.

The full credit was available to households with modified adjusted gross income (AGI) lower than $112,500 for single households and $150,000 for married households for the 2021 tax year (including those with zero earned income). When income exceeded these thresholds, the credit was phased out. The first phase-out occurs when incomes exceed these thresholds but are below $200,000 for single filers and $400,000 for married filers. In this phase-out, the total benefit per child was reduced by $50 for each $1000 additional AGI. The credit cannot fall below $2000 (i.e., the same amount that eligible families received for under the previous law). The second phase-out applied to filers with AGI more than $200,000 and $400,000 for single and married filers, respectively. In this phase-out, the total benefit per child was reduced by $50 for each $1000 additional AGI. The credit could decrease below $2000 until reaching $0.

Fig. 1 shows the effect of the reform on the CTC benefit structure. The figure compares the 2021 expanded CTC with the pre-reform CTC by household income and age of children for single and married households.

In order to receive their monthly payments, eligible recipients had to have filed their taxes or registered for payments through the CTC portal hosted by the Internal Revenue Service (IRS). If bank account information was known, the funds were transferred directly to eligible recipients by the IRS. Evidence shows that, between 2020 and 2021, the child poverty rate decreased by about half (from 9.7% to 5.2%), with 90% of this decrease being attributed to the CTC according to the U.S. Census Bureau (Curran, 2022). The expanded CTC was implemented only for the 2021 tax year. Monthly benefits ended in December 2021, and households claimed the remainder of the benefits when filing taxes in spring 2022. This program was not renewed by Congress, and the CTC reverted to its original form as of the 2022 tax year.

3. Data

We use data from the 2021 ATUS and WB. The ATUS data are an annual cross-sectional survey that involves a representative sample of the U.S. population and is run by the Bureau of Labor Statistics (BLS). ATUS respondents are randomly selected from a subset of households that have completed the American Current Population Survey (CPS) survey and are only interviewed once. Using time diaries, the ATUS asks respondents how they spent every minute of the preceding 24-h day (i.e., yesterday). The activities within a person' s time diary are classified into hundreds of detailed activity categories and include with whom the person spent their time. ATUS interviews take place every day of the week (Monday through Sunday) to develop a representative portrait of daily life; BLS interviewers choose the day of the interview.

The WB module is a supplementary survey included in the ATUS to obtain information on respondents' emotional well-being. The WB module is available for three full years: 2010, 2012, and 2013. Another collection of the WB module was added in the ATUS from March 1, 2021, through December 31, 2021.

We use data from the 2021 ATUS and WB to estimate the 2021 CTC expansion on well-being and time use. Data from 2010 to 2013 are used for additional robustness checks at the end of the manuscript. In the WB module, three activities reported by each respondent are randomly selected; for each of the selected activities, the respondent is asked how he/she felt during each of his/her activities. Five emotions are asked, in random order, for each of these three activities. This method is known as the day reconstruction method and is well established in existing evidence (Clark et al., 2020).

We use the following outcomes: (1) happy; (2) stress; (3) pain; (4) sad; and (5) tiredness. Each emotion is rated on a scale from 0 to 6. A value of 0 denotes no emotion at all (for example, not happy at all) whereas a value of 6 denotes the highest emotion (for example, very happy). We also combine the negative emotions (stress, pain, sad, and

[1] For more information on the history of the CTC, see Garfinkel et al. (2016) and Crandall-Hollick (2018, 2021).

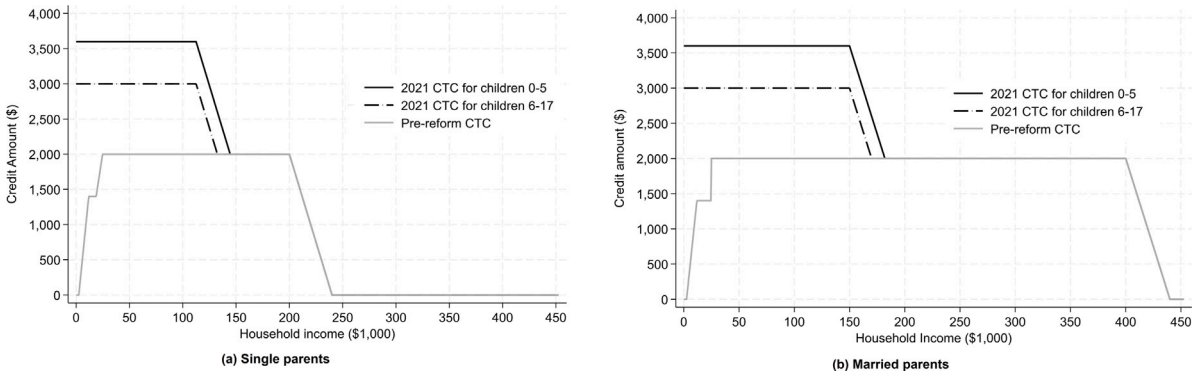


Fig. 1. Child tax credit, before and after the 2021 expansion.
Notes: Shows child tax credits amounts by adjusted gross income (AGI), age of children, and marital status, before and after the 2021 expansion. The figure illustrates the CTC credit amount as a function of household income before and after the 2021 expansion, for households with children aged 0–5 and/or 6–17 years. The information is shown separately for single parents and married parents (panels a and b, respectively). See more details in the main text.

tiredness) to create a measure of "average negative affect." We create an overall measure of well-being, "net affect," which is the difference between the happiness score and average negative affect score. For each ATUS–WB survey respondent and for each of the three activities (for which emotions were asked), we compute these seven measures of well-being. We then take the average across the three activities. These measures are very common in the experience well-being literature (Doyle et al., 2017). In addition, we use self-reported health status as an outcome variable to measure an individual's health with the question: "Would you say your health in general is . . . ? " Responses are coded on a 5-point Likert scale: 1 (excellent), 2 (very good), 3 (good), 4 (fair), and 5 (poor). Thus, for this health measure, higher values indicate worse health. We also construct two indicator variables: a dummy that equals 1 if the individual is in excellent or very good health and 0 otherwise, and a dummy that equals 1 if the individual reports fair or poor health and 0 otherwise. All these indicators are very common in the health economics literature (Lebihan, 2023). Details on well-being measures are presented in Appendix B.

We also study the impact of the 2021 CTC expansion on parents' time allocation. We analyze the following activities drawn from the ATUS time-use diary: (1) sleep; (2) paid work; (3) education; (4) home production; (5) leisure; (6) childcare; and (7) caregiving for adults. Details on the ATUS time use diary are presented in Appendix C.

For control variables, we use dummy variables for the sex of the respondent, age (18–29, 30–39, 40–49, 50–59, 60–64, 65 and older), highest level of education (less than a high school diploma, high school diploma, some college, bachelor's degree or more), and race/ethnicity (White, Black, Asian, Hispanic, other). Total household income is measured in 7 intervals (less than $25,000; $25,000–$34,999; $35,000–$49,999; $50,000–$74,999; $75,000–$99,999; $100,000–$149,999; $150,000 or more).[2] We use dummy variables for metropolitan areas and the presence of a partner in the household. We also include number of children fixed effects.

4. Methodology

We use a DD framework to estimate the effect of the CTC on well-being and time use. ATUS–WB data do not contain information about the CTC receipt. Moreover, if we were able to use data about CTC receipt, we would have an endogeneity problem because the receipt and amount of CTC benefits are correlated with other household characteristics that could be associated with well-being measures.

Rather, here, following the existing literature, we use a simulated measure of household benefits based on policy changes (Currie and Gruber, 1996). We calculate the monthly household CTC benefit as the sum of $300 multiplied by the number of children under 6 years old and $250 multiplied by the number of children 6 to 17 years old. For respondents surveyed before July 15, the monthly CTC amount is $0, regardless of the number and age of the children in the household. Therefore, the variations in the model come from two sources: (1) between-household differences in the number and age of the children (0–5 year old vs. 6–17 years) and (2) between-month differences (before and after CTC expansion). Our identification strategy is similar to the strategy employed by Pilkauskas et al. (2024b) and Pilkauskas et al. (2024a) to understand the effects of CTC expansion on economic well-being. We estimate the following model:

$$Y_{it} = \beta_0 + \beta_1 CTC_{itca} + \beta_2 X_{it} + \delta_s + \gamma_t + \theta_c + \varepsilon_{it} \quad (1)$$

where Y_{it} is an outcome variable for individual i in month t. CTC_{itca} is the predicted monthly benefit for a household according to the number of children c and the age of the children a. The parameter of interest, β_1, represents the effect of a $100 increase in predicted CTC benefits on well-being and time use. The term X_{it} represents the control variables listed in Section 3. Day fixed effects are also included. We include state fixed effects (δ_s) to control for time-invariant state-level characteristics that influence individuals. Month fixed effects (γ_t) are also included in the model. Because the number of children determines the CTC size and likely impacts both well-being and time use measures, we include fixed effects for the number of children (θ_c). Finally, ε_{it} is the error term.

This paper focuses on families with children. We restrict the sample to respondents living with children under 18 years old in the household. Some prior CTC research has used childless individuals as a comparison group (Batra et al., 2023; Ananat et al., 2024; Pac and Berger, 2024). However, as demonstrated in the next section, childless individuals are likely to be a poor counterfactual in this study. Indeed, several factors make it unlikely that the parallel trend assumption holds for these groups. For these reasons, in this paper, the identification strategy relies on variations in CTC benefits that parents receive according to the number and age of the children residing in the household. To avoid ambiguity, we also eliminate observations on the interview day, which was July 15, 2021.

In additional analyses, for the sake of completeness, we compare households with and without children. Similar to the literature (Kovski et al., 2023; Ananat et al., 2024), when we use childless households as a comparison group, we measure the CTC in two ways. The first

[2] In ATUS–WB data, income is reported in 16 income ranges (less than $5000; $5000–$7499; $7500–$9999; $10,000–$12,499; $12,500–$14,999; $15,000–$19,999; $20,000–$24,999; $25,000–$29,999; $30,000–$34,999; $35,000–$39,999; $40,000–$49,999; $50,000–$59,999; $60,000–$74,999; $75,000–$99,999; $100,000–$149,999; $150,000 or more).

method includes a pre/post-indicator interacted with an indicator for the presence of children in the household (binary measure):

$$Y_{it} = \beta_0 + \beta_1 Child017_i + \beta_2 Post_t + \beta_3 (Child017 \times Post)_{it} + \beta_4 X_{iy} + \delta_s + \gamma_t + \theta_c + \varepsilon_{it} \quad (2)$$

where $Child017_i$ is a binary variable equal to 1 if the respondent has at least one child 0–17 years old living in the household and 0 otherwise. The term $Post_t$ is a dummy variable that takes the value of 1 if the observation occurred after July 15 (post-treatment period) and 0 otherwise. Here, the parameter of interest is β_3, and represents the effect of having a child 0–17 years old living in the household after July 15.

The second method uses predicted CTC amounts (continuous measure):

$$Y_{it} = \beta_0 + \beta_1 CTC_{itca} + \beta_2 X_{it} + \delta_s + \gamma_t + \theta_c + \varepsilon_{it} \quad (3)$$

where CTC_{itca} is the predicted monthly benefit for a household according to the number of children c and the age of the children a. The monthly household CTC benefit is calculated as in the first equation, with the additional information that, for households without children, the CTC amount is always set to 0.

The ATUS–WB data do not provide information on actual CTC receipt. Instead, we define treatment by CTC eligibility. The estimates reported are intention-to-treat effects. Administrative records show that the temporarily expanded CTC covered 90% of families with children in the U.S. (Ananat et al., 2024). This means that treatment effects are unlikely to be driven by individuals not taking CTCs.

We use linear probability models for binary dependent variables and ordinary least squares (OLS) for continuous dependent variables. We also use the weights available in the ATUS–WB data. Standard errors are clustered at the state level.

Summary statistics for well-being and time use outcomes are shown in Tables A1 and A2, respectively. We also present the summary statistics for the independent variables in Table A3 of the Appendix.

Table 1
Effects of the 2021 CTC on well-being.

Variables	CTC (1)	CTC (2)	CTC (3)	N
Happy	0.014	0.044	0.044	2040
	(0.014)	(0.033)	(0.029)	
Stress	0.019	−0.006	−0.009	2044
	(0.012)	(0.022)	(0.027)	
Pain	−0.016	−0.020	−0.008	2046
	(0.014)	(0.018)	(0.018)	
Sad	−0.017*	−0.044**	−0.040**	2047
	(0.009)	(0.019)	(0.019)	
Tiredness	0.020	0.021	0.028	2045
	(0.018)	(0.026)	(0.031)	
Negative affect	0.001	−0.010	−0.005	2036
	(0.010)	(0.015)	(0.018)	
Net affect	0.012	0.055	0.045	2029
	(0.020)	(0.039)	(0.040)	
Health	−0.030***	−0.055***	−0.049***	2057
	(0.009)	(0.014)	(0.012)	
Excellent/very good health	0.011*	0.020***	0.018**	2057
	(0.006)	(0.007)	(0.007)	
Fair/poor health	−0.012**	−0.021***	−0.018***	2057
	(0.005)	(0.007)	(0.005)	
State fixed effects	No	Yes	Yes	
Month fixed effects	No	Yes	Yes	
Day fixed effects	No	Yes	Yes	
Number of children fixed effects	No	Yes	Yes	
Individual X's	No	No	Yes	

Notes: For each dependent variable, we report the estimated policy effects (β_1 of Eq. (1) shown). The coefficients represent the effect of an additional $100 in monthly 2021 CTC benefits. The sample is restricted to households with at least one child younger than 18. Three specifications are reported: (1) no control variables; (2) only state, month, day, and number of children fixed effects; and (3) the addition of the individual control variables listed in Section 3 (i.e., gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household). Standard errors (in parentheses) are clustered at the state level.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

5. Results

This section is arranged as follows. First, we report the estimates of CTC expansion on adults' well-being and time use following our baseline specification that compares adults with children. Second, we present additional results using an alternative identification strategy that compares adults with children to adults without children. Third, we present results from a series of robustness checks for our preferred identification strategy. Fourth, we explore any heterogeneous effects.

5.1. Baseline results

Table 1 presents the estimated effects of the 2021 CTC expansion on the well-being of adults with children. Three specifications are reported: (i) no control variables; (ii) only state, month, day, and number of children fixed effects; and (iii) the addition of the individual control variables listed in Section 3. In the three specifications, the results are generally consistent. Thus, the remainder of this study focuses on the models with a full set of controls. Estimates suggest that an additional $100 in monthly CTC benefits was associated with a decrease in parental sadness among households with children. More specifically, the reform produced a 0.040 decrease in sadness (i.e., a decrease of 7.05% relative to the mean score of 0.53), which is 3.21% of a standard deviation. For the happiness, stress, pain, and tiredness measures, there was no evidence of a significant change following the 2021 CTC reform. Similarly, the decrease in negative affect and the increase in net affect were not statistically significant. However, an additional $100 in monthly CTC benefits showed a beneficial and significant effect on the health status of individuals (about a 4.76% of a standard deviation).[3] Estimates also showed a 1.8 percentage point increase in the likelihood of being in excellent/very good health and a 1.8 percentage point decrease in the likelihood of being in fair/poor health. Taken together, these results suggest that the 2021 CTC reform may have improved the well-being and health of parents.

Table 2 presents results on the time spent on sleep, paid work hours, education, home production, leisure, childcare, and caregiving for adults. The reform had no significant effect on any of these activities, with the exception of childcare hours (Column 3). Indeed, an additional $100 in monthly CTC benefits was associated with a 5% increase in childcare relative to the mean score of 1.38, which is 3% of the standard deviation.[4]

5.2. Additional findings comparing households with and without children

This paper focuses on households with children younger than 18 years old, exploiting variation in the generosity of CTC benefits by the number and age of children in the households. We also present an alternative identification strategy that compares households with

[3] As indicated in the data section, responses related to the health status are coded on a 5-point Likert scale: 1 (excellent), 2 (very good), 3 (good), 4 (fair), and 5 (poor). Higher values indicate worse health. Here, the reform produced a decrease in health measure, so lower values indicate better health for the parents following the CTC expansion.

[4] In Table A4 of the Appendix, we divide the childcare hours into several categories. We find a significant increase in time spent on caring for and helping children. However, the reform had no significant impact on time spent for activities related to children's education and health.

Table 2
Effects of the 2021 CTC on time use.

Variables	CTC (1)	CTC (2)	CTC (3)	N
Sleep	−0.061***	−0.052*	0.010	2840
	(0.019)	(0.027)	(0.031)	
Paid work	0.054	0.070	−0.032	2840
	(0.040)	(0.052)	(0.058)	
Education	−0.037***	−0.076**	−0.031	2840
	(0.011)	(0.030)	(0.025)	
Home production	0.057*	0.036	0.018	2840
	(0.029)	(0.037)	(0.038)	
Leisure	−0.122***	−0.150***	−0.039	2840
	(0.021)	(0.041)	(0.048)	
Childcare	0.108***	0.157***	0.063***	2840
	(0.017)	(0.020)	(0.022)	
Caregiving for adults	−0.001	0.007	0.009	2840
	(0.005)	(0.007)	(0.007)	
State fixed effects	No	Yes	Yes	
Month fixed effects	No	Yes	Yes	
Day fixed effects	No	Yes	Yes	
Number of children fixed effects	No	Yes	Yes	
Individual X's	No	No	Yes	

Notes: For each dependent variable, we report the estimated policy effects (β_1 of Eq. (1) shown). The coefficients represent the effect of an additional $100 in monthly 2021 CTC benefits. The sample is restricted to households with at least one child younger than 18. Three specifications are reported: (1) no control variables; (2) only state, month, day, and number of children fixed effects; and (3) the addition of the individual control variables listed in Section 3 (i.e., gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household). Standard errors (in parentheses) are clustered at the state level.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

children under 18 (treated group) with households without children under 18 (control group).

In Tables A5 and A6 of the Appendix, we present results comparing households with and without children in terms of well-being and time use measures, respectively. We measure the CTC in two ways: (i) with a pre/post-indicator interacted with an indicator for the presence of children in the household (binary measure) and (ii) with predicted CTC amounts (continuous measure). In the binary method, the effect of the reform is modeled only by the presence of children in the household after the reform, whereas the continuous measure exploits variations in CTC amounts according to the number (including 0) and age of the children. When we use the binary measure, many of the effects on well-being and time use become non-significant. However, when the continuous measure is used, we continue to find evidence of improvements in sadness and health (Table A5). Similarly, we find a significant increase in childcare (Table A6). These results are hardly surprising using this alternative identification strategy. Previous studies have reported that effects turn statistically non-significant when comparing households with and without children (Kovski et al., 2023; Pignatti and Parolin, 2024).[5]

We are reluctant to rely on models that use childless individuals as a comparison group because childless individuals are likely to be a poor counterfactual in this study for several reasons. First, the crucial hypothesis for DD methodology is the common trend assumption before the policy change. More precisely, the trends between the treatment and control groups should be parallel before the reform. The parallel trend assumption between the two groups is unlikely to hold during the pre-reform period as parents are generally more stressed and tired before the policy compared to households without children. Households with children also do not have the same uses of time during a typical day as childless households (less leisure time, more time devoted to childcare, etc.). To test this hypothesis, we present the trends of all outcomes for households with children versus households without children in Figures A1–A2. For all measures of well-being and time use, the trends between the two groups are not parallel during the pre-reform months.[6] Second, demographic characteristics differ between households with and without children. As Table A7 shows, households without children were more likely to be older, educated, and White and less likely to have a spouse. Third, other threats to the difference-in-differences model are events or policy changes affecting one group but not the other during the study period. During the COVID-19 period, the two groups were likely to react differently to shocks. For example, parents may have faced more emotional difficulties in the fall of 2021 related to the reopening of schools at that time. COVID-19 vaccines were generally not yet available for children during this period, and the risk of infection was therefore greater for unvaccinated children. Furthermore, school closures were still frequent during 2021, increasing stress for parents and disrupting their schedules. For all these reasons, our preferred strategy excludes households without children from the control group.

5.3. Robustness checks

For the rest of the analysis, we return to our baseline specification, where we focus only on households with children.

Our identification strategy relies on a combination of variations in CTC benefits in terms of both the age and number of the children before and after the policy change. A key identification assumption of our model is the common time trend assumption. In order to examine whether this is the case, we perform a series of event study models. In Fig. 2, we report event study plots from regressions of our well-being outcomes on all control variables[7] as well as interactions of month indicators with the number of child indicators (1, 2, or 3 or more). We do the same for time use measures in Fig. 3. The figures plot the interaction terms to test whether significant differences occur in outcome trends among families of different sizes before the reform. The reference category is parents with one child (omitted category). Gray lines represent respondents with two children. Black lines represent respondents with three children or more. In Tables D1 and D2 (Appendix D), we report estimates associated with Figs. 2 and 3, respectively. We show the interaction terms and test whether significant differences in outcome trends emerge among families of different sizes before the reform. Using three specifications, we test whether all pre-reform coefficients are equal to 0. The first specification tests whether the interaction terms associated with respondents with two children (*2child*) before the reform are significantly different from 0. The second specification tests whether the interaction terms associated with respondents with three or more children (*3ormchild*) before the reform are significantly different from 0. The third specification tests whether all interaction terms before the reform (regardless of the number of children) are significantly different from 0. For the well-being measures, we show no significant differences (at the 5 percent level) in the months prior to the policy change (Fig. 2 and Table D1). For the time use measures, we generally find no significant differences (at the 5 percent level) in the months prior to the reform. Some exceptions are observed, such as sleep and childcare (Fig. 3 and Table D2).

In Fig. 4, we report event study plots from regressions of our well-being outcomes on all control variables[8] as well as interactions of

[5] Existing evidence indicates that the lack of significance is more present for the binary measure and, to a lesser extent, for the continuous measure.

[6] We also visually inspected trends for both groups by restricting the analysis to households aged 18–64 (Figures A3 and A4 for well-being and time use, respectively). Again, we show that, for all measures of well-being and time use, the pre-reform trends are not parallel between the two groups.

[7] The control variables are gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects.

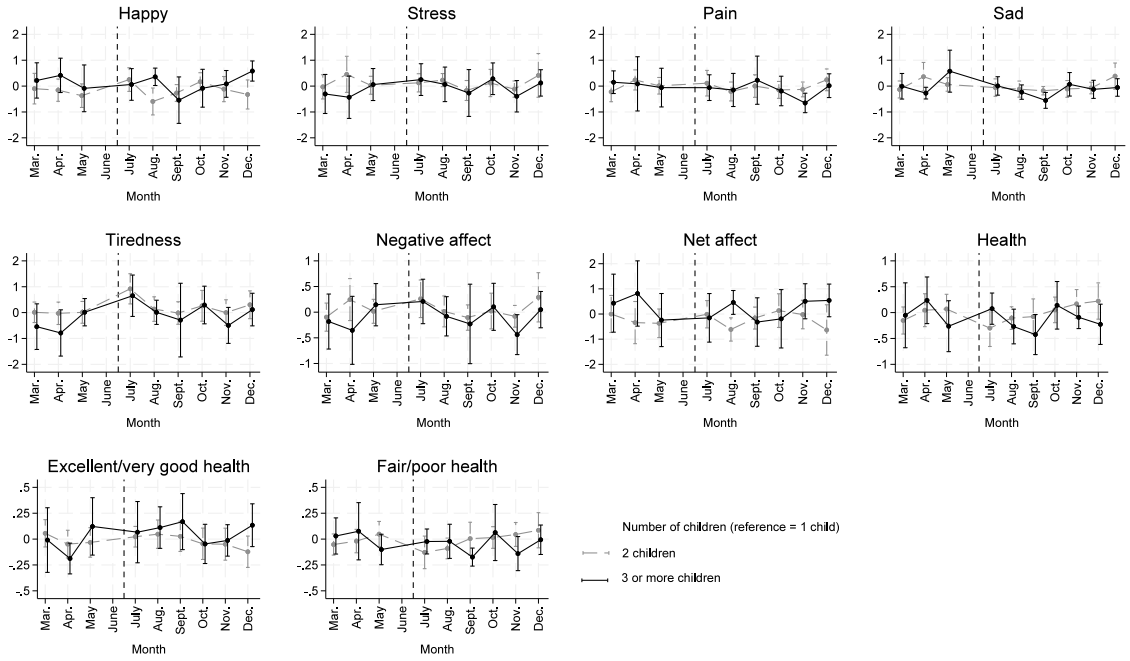


Fig. 2. Event study plots of parallel trends by number of children for well-being measures.
Notes: Event study plots from regressions of our well-being outcomes on all control variables (gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects) as well as interactions of month indicators with number of child indicators (1, 2, or 3 or more). The figures plot the interaction terms to test whether significant differences in well-being outcome trends emerge between families of different sizes before the reform. Gray lines represent respondents with two children. Black lines represent respondents with three children or more. The reference category is parents with one child (omitted category).

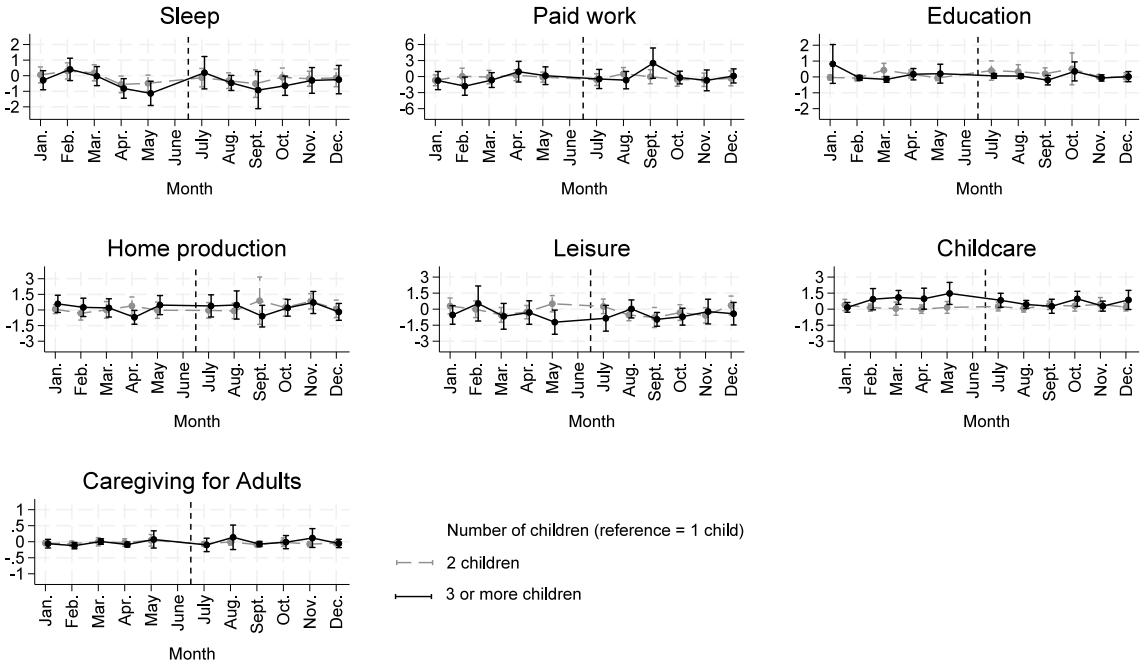


Fig. 3. Event study plots of parallel trends by number of children for time use measures.
Notes: Event study plots from regressions of our time use outcomes on all control variables (gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects) as well as interactions of month indicators with number of child indicators (1, 2, or 3 or more). The figures plot the interaction terms to test whether significant differences in time use outcome trends emerge between families of different sizes before the reform. Gray lines represent respondents with two children. Black lines represent respondents with three children or more. The reference category is parents with one child (omitted category).

month indicators with the age of child indicators (children 0–5 only, children 0–5 and 6–17, or children 6–17 only). We do the same for time use measures in Fig. 5. The figures plot the interaction terms to test whether significant differences occur in outcome trends among families with children of different ages before the reform. The reference category is parents with children aged 6–17 only (omitted category). Gray lines represent respondent parents with children aged 0–5 only. Black lines represent parents with children aged 0–5 and 6–17. In Tables D3 and D4 (Appendix D), we report estimates associated with Figs. 4 and 5, respectively. We show the interaction terms to test whether significant differences in outcome trends emerge among families of different ages before the reform. Using three specifications, we test whether all pre-reform coefficients are equal to 0. The first specification tests whether the interaction terms associated with respondents with children aged 0–5 only (*05only*) before the reform are significantly different from 0. The second specification tests whether the interaction terms associated with respondents with children aged 0–5 and 6–17 (*05–617*) before the reform are significantly different from 0. The third specification tests whether all interaction terms before the reform (regardless of the age of the children) are significantly different from 0. For the well-being measures, we generally show no significant differences (at the 5 percent level) in the months prior to the policy change (Fig. 4 and Table D3). The only exceptions are happy and net affect. For the time use measures, some significant differences (at the 5 percent level) in the months prior to the reform are observed (education, home production, leisure, and childcare; Fig. 5 and Table D4). We are aware that we must be cautious in our conclusions for these variables. As these results indicate, overall, there is little evidence of significant pre-trends. However, we caution that these graphs do not perfectly capture the variations we use in our models, as we use variations in both the number and age of children in the household.

In Table A8, we present estimates from a number of robustness checks for well-being measures. Each panel represents a different regression. The original estimates are shown in the first row. Panel A excludes the first month of CTC payment (July). Indeed, existing evidence shows that families are less likely to receive the benefit in the first month of the reform (Kovski et al., 2023). Excluding July, the results are even more compelling. A significant decrease in pain, sadness, and the likelihood of being in fair/poor health occurs. Additional CTC benefits are also associated with a significant improvement in health and an increase in the likelihood of being in excellent/very good health. Panel B excludes the last month of the observation window (December) to test whether families react differently when payments are about to end. Although the effects on emotions become non-significant, the beneficial effects on health remain significant. In Panel C, we exclude households with children aged 11–17 in order to account for the fact that the COVID-19 vaccine became available in the fall of 2021. The results are similar when excluding them. We also find a significant increase in happiness and net affect. Panel D adds employment status as a covariate into the model and shows that the results are similar to the original estimates.[9] Panel E adds as a control an indicator for the presence of any children aged 0–5 in the household and shows that the results remain similar to the original estimates. In Panel F, we include fixed effects for the age of the youngest child in the household in the model. We show that the results are similar to the original estimates. We perform the same robustness checks for time use measures and show that our results are robust (Table A9). Additional CTC benefits are significantly associated with an increase in childcare, but no significant effect is found for other time use measures.

In Tables A10 and A11 of the Appendix, we include 2010, 2012, and 2013 in the pre-CTC comparison period according to different specifications for well-being and time use measures, respectively.[10] In Panel A, we include 2010, 2012, and 2013 in the pre-CTC comparison. In Panel B, we exclude the period from March 1, 2021, to July 14, 2021, and compare the months when the expansion took place in 2021 with the years 2010, 2012, and 2013. Indeed, the first payment took place on July 15, 2021, but the reform was enacted in March 2021. We therefore want to rule out the hypothesis of household anticipation. In Panel C, we limit the sample to July 16 through December 31 of each year in order to compare the months when the CTC expansion took place in 2021 with the same months in 2010, 2012, and 2013. In Panels D through L, we perform the same restrictions but detail and compare each year (2010, 2012, and 2013) one by one with the year 2021.[11] Regardless of the specification, we find that the results remain similar to the original estimates for well-being measures (Table A10). For time use measures, we continue to find evidence of an increase in childcare for all specifications. We also find a significant decrease in leisure time for some specifications (Table A11).

In Tables A12 and A13 of the Appendix, we allow the policy effects to vary by month (July to December) for well-being and time use measures, respectively. We also test whether the effect is different from 0 as well as the equality of policy effects over time. For the majority of the well-being outcome (pain, sadness, net affect, negative affect, health, excellent/very good health, fair/poor health), the null hypothesis of the equality of effects between the months is rejected, suggesting that the effects differ over time (Table A12). More generally, by observing each variable, we find that the beneficial effects of the reform appeared mainly from the month of August (i.e., when more households are affected) and persisted until December. Turning to the time use measures, we report that the reform increased childcare from the month of August and the effects persisted until December (Table A13). We find no significant effects for the other time use outcomes. However, the sample size may be too small to detect any significance of the effects by month.

In Table A14 of the Appendix, we use ordered probit regressions for ordered variables (happiness, stress, pain, sadness, tiredness, net affect, negative affect, health) and probit marginal effects for binary variables (excellent/very good health, fair/poor health). We show that our results are robust to these specifications.

Tables A15 and A16 of the Appendix restrict the sample to households aged 18–64 for well-being and time use measures, respectively. We show that the results are similar to our main estimates.

According to Kling et al. (2007), aggregating multiple measures into an index in a particular domain (e.g., emotional well-being) improves statistical power. So, following Hoynes et al. (2016), we estimate two different summary standardized indexes of emotional well-being. The first index includes four measures: stress, pain, sadness, and tiredness. An increase in the first index indicates a worse outcome. The second include five measures: happiness, stress, pain, sadness, and tiredness. We convert the happiness variable so that an increase of the second

[8] The control variables are gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects.

[9] We do not control for employment status in the main estimates because the 2021 CTC reform could have had effects on labor market outcomes. Later, we test this and show that the reform had no significant impact on paid work activities. The lack of effect on employment is consistent with existing studies (Ananat et al., 2024; Pac and Berger, 2024). The fact that the effects on well-being and health remain similar when controlling for employment status suggests that other transmission channels must be at play.

[10] As previously mentioned, the WB module is available for three full years: 2010, 2012, and 2013. Another collection of the WB module was included in the ATUS from March 1, 2021, through December 31, 2021.

[11] For time use measures, we add a specification that excluded January and February 2021 (Panel M of Table A11). Indeed, well-being measures are available for the period from March 2021 to December 2021, while time measures are available from January 2021 to December 2021. We show that, by restricting the analysis to the March 2021–December 2021 period for time use measures, the results remain identical.

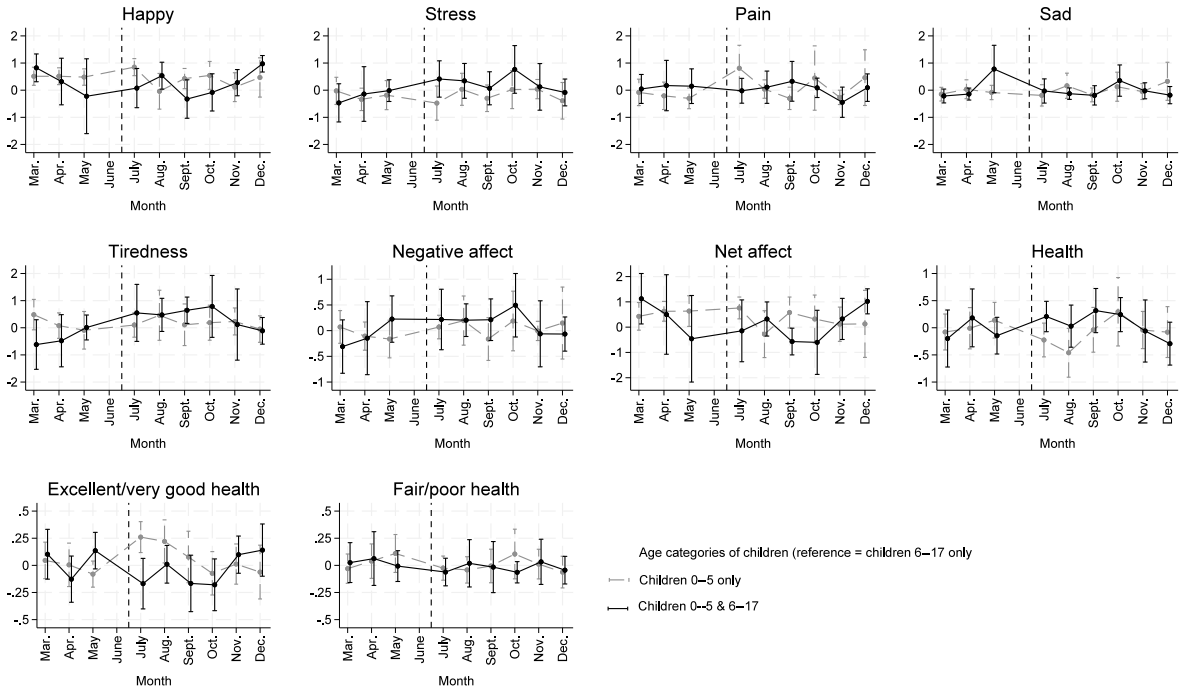


Fig. 4. Event study plots of parallel trends by age of children for well-being measures.
Notes: Event study plots from regressions of our well-being outcomes on all control variables (gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects) as well as interactions of month indicators with age of child indicators in the household (children aged 0–5 only, children aged 0–5 and children aged 6–17, and children aged 6–17 only). The figures plot the interaction terms to test whether significant differences in well-being outcome trends emerge between families with children from different age categories before the reform. Gray lines represent respondents with only children aged 0–5 in the household. Black lines represent respondents with children aged 0–5 and 6–17 in the household. The reference category is parents with only children aged 6–17 in the household (omitted category).

index also indicates a worse outcome. The summary index is the simple average of the standardized z-scores measured for each component. The z-score is calculated by subtracting the mean and dividing by the standard deviation.[12] Table A17 of the Appendix presents the results for the two indexes. For the first index, the coefficient estimate is similar to that obtained for the negative affect outcome in Table 1, and there is no evidence of a significant change following the 2021 CTC reform. For the second index (including happiness), we show that the effects are also insignificant.[13]

5.4. Heterogeneous effects

In Table 3, we evaluate the heterogeneous effects of the policy for well-being measures. We study how effects vary across gender, education, income, race, marital status, and child's age. In the baseline specification (Eq. (1)), we add an interaction term between the subgroup and the predicted benefit. Results in Panel A reveal that the effects are similar regardless of the respondent's sex. The only exception is sadness, where we observe that the effects are only significant for women. In Panel B, we study how effects vary across education. Low-educated individuals include those with a high school diploma or less. Estimates show that the effects on well-being measures are similar, regardless of education. In Panel C, we study how effects vary across income. Low-income respondents include individuals with an annual family income less than $50,000. Estimates show that poor individuals are less tired after the policy change. The beneficial effect of the reform on health is also larger for low-income respondents. In Panels D and E, the policy effects on well-being are generally similar regardless of the race and marital status. In Panel F, we test how the policy effects vary with children of different ages. In particular, we want to know whether the effects differ depending on whether the household has at least one child aged 0–5 or not. Indeed, childcare needs and time vary depending on the child's age. For well-being measures, the effects are similar regardless of the presence or not of at least one child aged 0–5 in the household.[14]

Table 4 presents the heterogeneous effects of the reform on time use measures. The results in Panel A reveal that the effects are similar regardless of the respondent's sex. The only exceptions are education and leisure, where we observe a significant decrease in education and an increase in leisure for women. In Panel B, estimates report a significant increase in paid work and a significant decrease in leisure for low-educated respondents. In Panel C, we study how effects vary across income. We found that, for low-income individuals, time in home production increased while time in childcare decreased. The results in Panel D show that the policy effects on well-being are generally similar regardless of the race, except for sleep. In Panel E, estimates show a significant increase in paid work and a significant decrease in leisure for single mothers. In Panel F, we report a significant increase in childcare

[12] Like Hoynes et al. (2016), we use the mean and standard deviation of the pre-reform period.

[13] It is normal that the estimated coefficient for the second index in Table A17 is different from that estimated for the net affect variable in Table 1. In Table A17, the happiness variable has been converted to unhappiness, where a higher score is a worse outcome. This is to be consistent with the other negative emotions (stress, pain, sadness, tiredness). In Table 1, the net affect variable is the difference between the (untransformed) happiness score and the average negative affect score (negative emotions).

[14] For Panel F, we also include in the model an indicator for any children aged 0–5 in the household. If this variable is not included, the significance of the effects obtained is larger than without it.

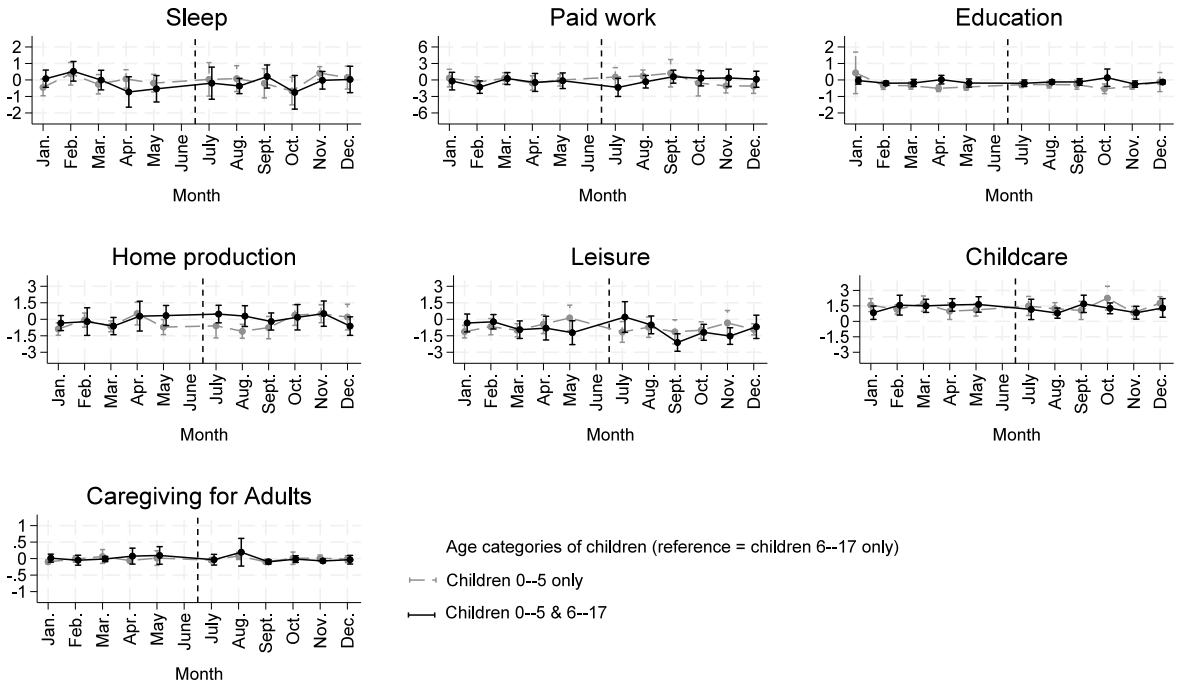


Fig. 5. Event study plots of parallel trends by age of children for time use measures.
Notes: Event study plots from regressions of our time use outcomes on all control variables (gender, age, education, income, metropolitan area, race/ethnicity, presence of a partner in the household, state, and day fixed effects) as well as interactions of month indicators with age of child indicators in the household (children aged 0–5 only, children aged 0–5 and children aged 6–17, and children aged 6–17 only). The figures plot the interaction terms to test whether significant differences in well-being outcome trends emerge between families with children from different age categories before the reform. Gray lines represent respondents with only children aged 0–5 in the household. Black lines represent respondents with children aged 0–5 and 6–17 in the household. The reference category is parents with only children aged 6–17 in the household (omitted category).

only for parents with at least one child aged 0–5 in the household. Clearly, there is a significant difference between younger children and older children.[15]

As previously mentioned, the CTC benefits begin to phase out at $112,500 for single parents and $150,000 for married parents. In Tables A18 and A19, we report several alternative samples for well-being and time measures, respectively. In Panel A, we exclude respondents whose household income is $150,000 or more.[16] This approach allows us to focus exclusively on families that were most heavily impacted by the 2021 CTC reform (based on income distribution, the credit size increased most for low-income families) and whose health and well-being were most negatively impacted by the pandemic. The results are similar to the original estimates. Thus, we observe a significant decrease in sadness and in the likelihood of being in poor/fair health (Table A18). Estimates also show that health improved and the likelihood of being in excellent/very good health increased. Panel B restricts the sample to households with a family income of $150,000 or more, which can be considered a placebo group because these households were not affected by the reform or, even if they experienced an increase in benefits, the effect should be smaller compared to poorer households. Reassuringly, we find no significant impact of the reform on health in this group. Some significant effects are observed on pain, but they are in the opposite direction as expected (i.e., emotions got worse). Only for happiness do we find a beneficial effect, but the effect is only significant at 10% (Table A18).

Turning to the time use measures (Table A19), the results are similar to the original estimates. Families with incomes below $150,000 are the ones most affected by the policy change and are seeking a significant increase in childcare. In contrast, for families with high income ($150,000 or more), there is no significant change (at a significance level of 5% or less) in the time use measures.

6. Discussion and conclusion

This paper presents new empirical evidence on the effects of the 2021 CTC expansion on well-being and time-use activities of parents. This paper shows that the reform decreased parents' sadness and improved their health. In Table A20, we present a summary table comparing our results with related literature on the CTC expansion's effects on parental health. The beneficial effects of the policy on overall health, as found in this paper, are consistent with existing evidence. For example, according to Rook et al. (2023), CTC-eligible adults experienced a 3.0 percentage point increase in the likelihood of being in excellent/very good health—an estimate close to ours. Several studies also show that CTC expansion had a beneficial effect on parental mental health. Specifically, existing evidence reports a decrease depressive and anxiety symptoms (Batra et al., 2023; Cha et al., 2023; Kovski et al., 2023; Pignatti and Parolin, 2024). Our data do not include information on depressive and anxiety symptoms, but we find that the CTC expansion led to a significant decrease in sadness for parents. Sadness is considered one of the key symptoms of depression and is strongly correlated with anxiety and depression (Mouchet-Mages and Bayle, 2008; Zaid et al., 2025). Both mental health and physical health

[15] For Panel F, we also include in the model an indicator for any children aged 0–5 in the household. If this variable is not included, the significance and the size of the effects obtained are larger than without it.

[16] In the ATUS–WB data, income is reported in income ranges (less than $25,000, $25,000-$34,999, $35,000-$49,999, $50,000-$74,999, $75,000-$99,999, $100,000-$149,999, $150,000 or more). We cannot distinguish the phase-out threshold of $112,500 for single parents. Instead, we use the same criterion of $150,000 for single parents and married parents.

Table 3
Heterogeneous effects on well-being.

Variables	Happy	Stress	Pain	Sad	Tiredness	Negative affect	Net affect	Health	Excellent/very good health	Fair/poor health
Panel A: By sex										
CTC*Female	−0.008	0.005	0.002	−0.030**	0.027	0.002	−0.004	0.015	0.003	0.015**
	(0.029)	(0.045)	(0.029)	(0.013)	(0.035)	(0.020)	(0.039)	(0.014)	(0.008)	(0.007)
CTC	0.048	−0.011	−0.009	−0.024	0.012	−0.006	0.048	−0.057***	0.016*	−0.027***
	(0.033)	(0.047)	(0.026)	(0.020)	(0.040)	(0.025)	(0.048)	(0.013)	(0.009)	(0.006)
Panel B: By education										
CTC*LowEduc	−0.017	0.047	0.005	0.001	0.026	0.020	−0.040	−0.014	−0.006	−0.008
	(0.026)	(0.031)	(0.031)	(0.014)	(0.033)	(0.021)	(0.035)	(0.020)	(0.010)	(0.008)
CTC	0.055*	−0.041	−0.011	−0.041**	0.010	−0.019	0.073**	−0.039*	0.022**	−0.013**
	(0.029)	(0.032)	(0.028)	(0.020)	(0.032)	(0.018)	(0.035)	(0.021)	(0.011)	(0.006)
Panel C: By income										
CTC*Less50k	−0.005	−0.000	−0.018	−0.024	−0.103**	−0.036	0.029	−0.039*	0.011	−0.003
	(0.032)	(0.030)	(0.041)	(0.021)	(0.046)	(0.028)	(0.051)	(0.021)	(0.013)	(0.008)
CTC	0.045	−0.009	−0.000	−0.030*	0.071**	0.011	0.033	−0.032**	0.014	−0.017***
	(0.029)	(0.027)	(0.025)	(0.017)	(0.028)	(0.019)	(0.037)	(0.015)	(0.009)	(0.006)
Panel D: By race										
CTC*White	0.010	−0.050	−0.067	−0.027	−0.054	−0.051	0.063	−0.011	0.005	0.010
	(0.042)	(0.044)	(0.051)	(0.030)	(0.059)	(0.038)	(0.070)	(0.018)	(0.010)	(0.007)
CTC	0.035	0.034	0.049	−0.018	0.074	0.038	−0.008	−0.040*	0.014	−0.026***
	(0.047)	(0.053)	(0.053)	(0.030)	(0.065)	(0.039)	(0.076)	(0.020)	(0.011)	(0.007)
Panel E: By marital status										
CTC*Single	0.009	0.079*	0.071	0.036	−0.026	0.041	−0.032	−0.006	−0.003	0.005
	(0.053)	(0.042)	(0.047)	(0.036)	(0.046)	(0.035)	(0.074)	(0.031)	(0.014)	(0.009)
CTC	0.042	−0.021	−0.019	−0.046**	0.032	−0.011	0.051	−0.048***	0.019**	−0.019***
	(0.029)	(0.027)	(0.017)	(0.018)	(0.029)	(0.017)	(0.038)	(0.012)	(0.007)	(0.005)
Panel F: By age of the child										
CTC*Anychild05	0.022	0.056	0.034	0.008	0.035	0.035	−0.009	0.031*	−0.017	−0.000
	(0.028)	(0.036)	(0.034)	(0.023)	(0.031)	(0.022)	(0.040)	(0.018)	(0.011)	(0.007)
CTC	0.029	−0.048	−0.030	−0.045*	0.005	−0.029	0.052	−0.069***	0.030***	−0.018***
	(0.035)	(0.038)	(0.032)	(0.024)	(0.038)	(0.025)	(0.050)	(0.017)	(0.010)	(0.007)

Notes: Shows the heterogeneous effects of the policy for well-being measures. Each panel represents a different regression (by sex, education, income, race, marital status, and child's age). In the baseline specification (Eq. (1)), an interaction term between the subgroup and the predicted benefit is added. The coefficients represent the effect of an additional $100 in monthly 2021 CTC benefits. The sample is restricted to households with at least one child younger than 18. All models include state, month, day, and number of children fixed effects. They also include the individual control variables listed in Section 3 (gender, age, education, income, metropolitan area, race/ethnicity, and presence of a partner in the household). Low-educated respondents include those with a high school diploma or less. For Panel F, we also include in the model an indicator for any children aged 0–5 in the household. Standard errors (in parentheses) are clustered at the state level.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

are correlated with measures of subjective well-being (Dolan et al., 2008). Our paper therefore shows that the reform has an impact on not only health, but also subjective well-being. Clearly, the effects are consistent with prior research on CTC and health/well-being.

We also estimated the effect of the reform on time use activities. Our estimates show that the policy had generally no significant impact on work time. These results are consistent with existing evidence on the effects of CTC expansion on employment outcomes (Ananat et al., 2024; Pac and Berger, 2024). Similarly, we do not find that the reform had a significant effect on time spent on health care, which is consistent with previous evidence of the null effects of the reform on health care use (Pignatti and Parolin, 2024). Therefore, we can exclude the possibility that the beneficial effects on parental well-being were due to changes in employment or changes in health care use. Rather, here, it is hypothesized that the introduction of unconditional supplementary income reduced poverty and alleviated financial worries, which was beneficial for parental health and well-being (Rook et al., 2023; Pignatti and Parolin, 2024). An additional hypothesis would be that parents spending more time with their children would be beneficial for the parents' well-being (Musick et al., 2016).

Our findings are consistent with prior evidence on cash transfer programs, such as the Earned Income Tax Credit (EITC), which has been associated with improvements in parental well-being and health. For example, Boyd-Swan et al. (2016) showed that eligible mothers experienced an increase in happiness and self-esteem as well as a decrease in depression symptomatology. Similarly, Evans and Garthwaite (2014) reported improvements in the self-reported health of eligible mothers. Another study by Gangopadhyaya et al. (2020) found that a $1000 increase in maximum EITC payments decreased the number of days not in good mental health by 0.21–0.27 days. Other cash transfers programs have also been shown to increase money and time spent on and with infants (Gennetian et al., 2022). By exploiting child tax benefit expansion in Canada, Milligan and Stabile (2011) found positive effects on parents' mental health. Although the design of the programs differs from the expanded CTC, all of these studies show the beneficial effect of child benefits on parents' well-being, which is consistent with the results of our paper.[17]

The heterogeneous effects show that the effects of the 2021 CTC expansion are larger for low-income households in terms of well-being and health. This is consistent with existing evidence on CTC expansion and more generally with child benefits (Pignatti and Parolin, 2024). These people are most vulnerable to financial constraints and more likely to have poorer health as well as high marginal returns of benefits. This result has important implications for policymakers because it shows that, even if it is temporary and during a particular period (COVID-19), receiving extra income is beneficial to the health of the most vulnerable. This type of policy could lead to a reduction in health disparities. Our findings also show that the effects are not significantly higher for single parents or low-educated households. This result might

[17] The EITC is conditioned on work. The Baby's First Year is a large-scale multi-site randomized controlled trial where the new mothers received monthly unconditional cash transfers for several years (Gennetian et al., 2022).

Table 4
Heterogeneous effects on time use.

Variables	Sleep	Paid work	Education	Home production	Leisure	Childcare	Caregiving for adults
Panel A: By sex							
CTC*Female	−0.043	−0.062	−0.029*	−0.052	0.160***	0.031	−0.006
	(0.032)	(0.076)	(0.015)	(0.049)	(0.043)	(0.033)	(0.011)
CTC	0.036	0.004	−0.014	0.049	−0.133**	0.044**	0.012
	(0.042)	(0.084)	(0.021)	(0.049)	(0.062)	(0.022)	(0.012)
Panel B: By education							
CTC*LowEduc	−0.015	0.156*	−0.007	0.059	−0.162***	−0.018	−0.001
	(0.041)	(0.083)	(0.014)	(0.054)	(0.045)	(0.035)	(0.008)
CTC	0.019	−0.134*	−0.026	−0.020	0.066	0.075**	0.010*
	(0.023)	(0.073)	(0.026)	(0.043)	(0.058)	(0.032)	(0.005)
Panel C: By income							
CTC*Less50k	0.035	−0.083	0.008	0.089*	−0.011	−0.056*	0.015
	(0.034)	(0.088)	(0.020)	(0.053)	(0.049)	(0.028)	(0.013)
CTC	−0.005	0.004	−0.034	−0.020	−0.035	0.087***	0.002
	(0.029)	(0.063)	(0.025)	(0.040)	(0.053)	(0.029)	(0.005)
Panel D: By race							
CTC*White	0.114**	−0.154	−0.012	0.067	−0.008	−0.015	0.019
	(0.044)	(0.107)	(0.030)	(0.070)	(0.053)	(0.044)	(0.011)
CTC	−0.084*	0.094	−0.021	−0.036	−0.033	0.075	−0.007
	(0.048)	(0.121)	(0.040)	(0.058)	(0.073)	(0.046)	(0.010)
Panel E: By marital status							
CTC*Single	−0.020	0.234**	−0.034	−0.027	−0.191***	0.022	−0.001
	(0.050)	(0.103)	(0.045)	(0.055)	(0.054)	(0.037)	(0.011)
CTC	0.013	−0.068	−0.026	0.022	−0.011	0.059**	0.009
	(0.030)	(0.059)	(0.023)	(0.039)	(0.050)	(0.023)	(0.008)
Panel F: By age of the child							
CTC*Anychild05	−0.005	0.013	−0.004	−0.034	−0.036	0.059*	0.003
	(0.044)	(0.067)	(0.019)	(0.043)	(0.045)	(0.033)	(0.010)
CTC	0.013	−0.049	−0.028	0.042	−0.017	0.032	0.007
	(0.049)	(0.075)	(0.030)	(0.052)	(0.058)	(0.029)	(0.008)

Notes: Shows the heterogeneous effects of the policy for time use measures. Each panel represents a different regression (by sex, education, income, race, marital status, and child's age). In the baseline specification (Eq. (1)), an interaction term between the subgroup and the predicted benefit is added. The coefficients represent the effect of an additional $100 in monthly 2021 CTC benefits. The sample is restricted to households with at least one child younger than 18. All models include state, month, day, and number of children fixed effects. They also include the individual control variables listed in Section 3 (gender, age, education, income, metropolitan area, race/ethnicity, and presence of a partner in the household). Low-educated respondents include those with a high school diploma or less. For Panel F, we also include in the model an indicator for any children aged 0–5 in the household. Standard errors (in parentheses) are clustered at the state level.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

be surprising because it is not consistent with those of Pignatti and Parolin (2024) and Cha et al. (2023). Indeed, these authors report a larger beneficial effect on mental health for more credit-constrained individuals (including low-educated households, low-income families, and single families). Several reasons could explain this discrepancy. First, our sample size is small (approximately 2040 observations for well-being measures in the overall sample). Standard errors increase mechanically when performing subgroup analyses, thereby limiting our ability to draw strong conclusions from these results. Second, there could be differences in the measurements. Indeed, Pignatti and Parolin (2024) focused on the number of bad mental health days and Cha et al. (2023) on psychological distress symptoms. In our study, these are measures of general well-being and health (health, happiness, sadness, etc.). As the measures can be very broad and include both mental and physical well-being, the effects obtained may be different. Nevertheless, we find that the beneficial effects of the reform are larger for low-income families. Income here appears to be a better indicator of family vulnerability, especially as the CTC benefit eligibility criterion is based on income (before and after the expansion). All of our results by income[18] are consistent with the fact that the families most likely to be affected by the reform are those who see the largest effects on well-being and time use (households lower than $112,500 for single households and $150,000 for married households).

Considering all the results, this paper shows that the 2021 CTC reform improved parents' well-being and health, but several factors limit the ability to draw broader conclusions about the effect of monthly universal cash transfers on well-being and time use. Indeed, this reform was temporary and lasted only six months. This program was also implemented during a particular period—namely, the COVID-19 pandemic. During this period, sharp increases in unemployment rates and declining incomes, as well as a deterioration in people's mental health and well-being, were found. Thus, the effects of the CTC reform may have been particularly beneficial during this atypical period, yet it may limit the generalizability of results. Some seemingly eligible families did not receive the benefits, and some payments were delayed (Pilkauskas et al., 2024b). Nevertheless, Michelmore and Pilkauskas (2023) showed that more than 80% of low-income families received at least one payment. The data used in this paper ended in 2021, so future research should examine whether the effects of the CTC reform persist. Although this is less of a concern given that the expanded CTC was temporary, it is difficult to generalize our results to a permanent expansion, and individuals may respond differently to a more permanent change in terms of employment, behavior, and health. It would also be interesting to see the effects when the program is returned to its original pre-2021 form. Finally, the sample sizes are small, making it difficult to measure some effects. Nevertheless, the results found in this paper are consistent with the existing literature on the CTC expansion. These findings have important implications for policymakers and contribute to the ongoing debate on the introduction of a permanent universal child benefit for the U.S. population.

[18] See Tables 3, 4, A18, and A19.

CRediT authorship contribution statement

Laetitia Lebihan: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.ehb.2025.101533.

Data availability

Data will be made available on request.

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