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RESEARCH ARTICLE

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The effects of an unconditional cash transfer on parents' mental health in the United States

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Abstract

The provision of unconditional cash transfers may be one effective policy strategy for improving mental health, but causal evidence on their efficacy is rare in high-income countries. This study investigates the mental health consequences of the 2021 child tax credit (CTC) expansion, which temporarily provided unconditional and monthly cash support to most families with children in the United States. Using data from the Behavioral Risk Factor Surveillance System, we exploit differences in CTC benefit levels for households with younger versus older children. More generous CTC transfers are associated with a decrease in the number of bad mental health days reported by the parents. The effect materializes after the third monthly payment and disappears when the benefits are withdrawn. The CTC's improvement of mental health is larger for more credit-constrained individuals, including low-income households, women, and younger respondents.

KEYWORDS
child tax credit, mental health, public policy

JEL CLASSIFICATION
H51, I18, J18

1 | INTRODUCTION

Mental health conditions have worsened in many countries in recent decades. Globally, one in every eight individuals lived with a mental health disorder in 2019 (IHME, 2019), and it is estimated that 12 billion working days are lost globally every year due to anxiety and depression (WHO, 2022). In the United States (US), 22.8% of the adult population reported having any mental illness in 2021 (up from 18.1% in 2014) and 5.5% reported a serious mental illness (up from 4.1% in 2014). Mental health concerns in the US are particularly acute among youth, women, minorities and low-income households (NSDUH, 2021). These trends have led to increased attention among policymakers on effective strategies for improving mental health outcomes. Yet, mental healthcare is under-resourced, compared to physical healthcare. This leads to a treatment gap of over 80% globally, despite the existence of cost-effective interventions (Ridley et al., 2020). Recently, increasing attention has been devoted to understanding the socio-economic causes of mental health distress, and to studying whether social policies can improve mental health by alleviating financial constraints.

This study investigates how the provision of monthly and unconditional cash transfers targeting children affects the parents' mental health. Specifically, we study the effects of the temporary expansion of the child tax credit (CTC) in 2021 in the US. The CTC was first introduced in 1997 to provide income support to families with children, and it has been repeatedly expanded since then. The 2021 reform made CTC receipt fully refundable and no longer conditional on earnings, increased benefit levels and modified the frequency of payments (i.e., from lump-sum payments at the moment of tax filing to monthly transfers). We adopt a difference-in-differences (DiD) approach that compares adults in households with children of different ages, exploiting differential changes in benefit generosity introduced by the policy change. We use data from the Behavioral Risk Factor Surveillance System (BRFSS), a nationally-representative survey that has been administered continuously in the US since 1984 and is the largest health-related survey worldwide. Our strategy relies on the assumption that mental health status would have evolved in a similar way among parents of young and older children, absent the policy change. We provide evidence in favor of this identification assumption, including by looking at the evolution in the outcomes of interest in the months before the policy announcement. We also relax this identification assumption in a series of alternative specifications and placebo tests.

Our findings indicate that a more generous CTC transfer decreased the number of bad mental health days reported by the parents. In our preferred specification, an additional $300 over a 6 months window decreases the number of bad mental health days by 0.094 of a standard deviation (SD) in the previous month. This is very close to the estimates reported in previous studies on the mental health effects of cash transfers (average treatment effect of 0.067 SD) or anti-poverty programs (average of 0.138 SD) (Ridley et al., 2020), although comparisons across studies should be interpreted with caution given that treatment intensity can also differ. Our treatment effects materialize a few months after the first transfer is received, and disappear immediately after the last monthly payment is made. Effects materialize on the intensive margin, decreasing the number of days individuals report being mentally sick among those individuals who report at least some days of poor mental health in the previous month. Treatment effects are particularly large for individuals reporting a high number of bad mental health days, suggesting that cash transfers can be effective even among individuals with severe mental health conditions.

We rule out that the positive effects on parents' mental health are driven by changes in labor market status, as effects on overall employment as well as on the types of jobs held are very small and statistically insignificant. Additionally, effects are not driven by changes in health insurance coverage or health care use. We also do not find that individuals spend the higher benefit to engage more frequently in unhealthy behavior (e.g., smoking or drinking). However, we do not observe in the BRFSS other important possible transmission channels, including expenditures. For these reasons, we interpret our results in light of the findings of other papers that have examined the 2021 CTC expansion, showing that it led to higher consumption levels and lower rates of child poverty and food insecurity (for a review, see Curran, 2022). This aligns with our assumption that the receipt of the child cash transfer affects parents' mental health in part through the alleviation of financial worries.

This interpretation is also consistent with the results that emerge from our heterogeneous analysis: we find that treatment effects are stronger for women compared to men, as well as among young adults (below the age of 40), low-educated individuals (with less than college degree) and people in low-income households (total household income below $35,000). We document that these groups traditionally report higher rates of mental health distress. Additionally, these groups are on average more likely to be financially distressed, and might therefore benefit relatively more from the receipt of a more generous transfer.[1]

Our baseline results of positive mental health effects hold under a series of placebo and robustness tests, including the adoption of a triple difference approach that uses income as an additional dimension in the analysis. However, treatment effects are close to zero and statistically non-significant when we use an alternative DiD strategy, comparing adults in households with and without children, as performed in some of the previous studies that have examined the CTC expansion. We believe that this alternative identification strategy relies on assumptions that are less likely to hold, as parents might have experienced increasing mental health concerns in the fall of 2021 (e.g., due to school closures or the risk of infection for un-vaccinated children). As such, the parallel trend assumption is less likely to hold for these groups. We provide suggestive evidence in favor of this interpretation, by also looking at trends in mental health between parents and non-parents in 2019, when the expanded CTC was not in place. For these reasons, we primarily rely on the comparison between adults with children of different ages to identify treatment effects, while trying to alleviate some possible concerns related to the use of this identification strategy.[2]

The paper primarily adds to the literature on the relationship between income and mental health. While a strong positive correlation between income and mental health has been documented extensively, the direction of the causal relationship remains widely debated (Ridley et al., 2020). Within this literature, studies have examined the mental health effects of participation in cash transfers or anti-poverty programs. In the US, research has investigated the mental health effects of conditional cash transfers, such as the Earned Income Tax Credit (EITC) (Boyd-Swan et al., 2016; Collin et al., 2021; Dow et al., 2020; Evans & Garthwaite, 2014; Jones et al., 2022; Lenhart, 2019; Morgan et al., 2021), and other forms of social protection (Aizer et al., 2016; Finkelstein et al., 2012; Rambotti, 2020). However, effects of an unconditional cash transfer might be different, compared to those of other types of welfare (Haushofer & Shapiro, 2016).[3] This refers, first, to differences in the target population, as unconditional cash transfers are more likely to reach low-income families. Additionally, money from unconditional cash transfers can be spent for any goods, including temptation goods that can decrease welfare. Moreover, policy design differences might influence the mental health effects of a given transfer amount (e.g., stigma from benefit receipt). Finally, it is difficult for studies on conditional cash transfers to uncouple the income effect from the effect of any behavioral change due to program conditionalities.

Additionally, the paper contributes to the recent literature that has examined the effects of the 2021 CTC expansion.[4] Previous research has found an increase in household spending (Parolin et al., 2024) and a reduction in child poverty and food insecurity (Parolin, Ananat, et al., 2021) following the policy change, with no significant effect on labor supply (Ananat et al., 2021). Relatively little is known on the mental health effects of the CTC expansion (Curran, 2022). The few papers that have examined this question have found conflicting results (Batra et al., 2023; Collyer et al., 2022; Glasner et al., 2022; Kovski et al., 2023). We complement these studies in two main ways. First, we use data from the largest health-related survey in the US. Previous studies used instead data from surveys that were either not nationally representative (e.g., focusing on Supplemental Nutritional Assistance Program [SNAP] recipients or the population of specific cities), or that suffered from sample-selection and attrition rates (e.g., Understanding America Study [UAS] or the Census's Household Pulse Survey).[5] Second, we adopt a new identification strategy that allows to isolate the causal effect of a change in benefit levels among eligible households, by exploiting differences in benefits among households with children of different age. Previous contributions relied mostly on the comparison of households with and without children, which requires identification assumptions that we show are unlikely to hold.[6]

2 | POLICY

The American Rescue Plan Act (ARPA) included a $1.9 trillion economic relief and stimulus package to support households and businesses during the COVID-19 pandemic. Interventions included a per-person stimulus check, the extension of unemployment benefits, an expansion of the EITC for childless adults, and emergency grants for small businesses. We focus here on the temporary expansion of the CTC that was included in the ARPA. While households eligible for the expanded CTC might have also benefited from other forms of economic support approved as part of the same legislative act, the CTC expansion was the intervention that most strongly and directly targeted households with children.[7]

Even before this expansion, the CTC constituted the largest tax credit in the US, with an annual federal spending above $100 billion. Before the ARPA, adults with children could receive a maximum of $2000 per child per year from the CTC. However, many households did not receive the full benefit, or were entirely ineligible to the tax credit. This is because tax filers with an annual income below $2500 did not qualify for the CTC. The benefit amount then started increasing at a rate of 15% of income above $2500, until reaching the maximum of $1400. This was the maximum refundable amount of the CTC, which could be complemented with a non-refundable part of the tax credit, until reaching $2000. It is estimated that one in three children did not receive the full amount of the CTC. Children of single parents, living in rural areas, of racial/ethnic minorities, and in larger families were more likely to be ineligible to the full benefit (Collyer et al., 2020; Curran & Collyer, 2019).

The expanded CTC marked an historic, albeit temporary, shift in the generosity and coverage of cash support provided to families with children in the US. Specifically, the 2021 reform, passed as part of the ARPA in March 2021, introduced three main changes to the CTC. First, the CTC became fully refundable and no longer conditional on earnings, effectively expanding the benefit to the lowest-income families who previously earned too little to qualify for full receipt. Second, the annual maximum benefit amount was increased from $2000 per child to $3600 per child below age six and $3000 per child between ages six and 17. Finally, the reform modified the frequency of benefit payment, paying half of the annual CTC value in monthly installments from July through December 2021, and the other half as a lump-sum payment at tax time in 2022. As a result, the ARPA temporarily transformed the CTC into a program resembling a national child allowance, a type of public support that is common in many high-income countries, but was absent in the US.

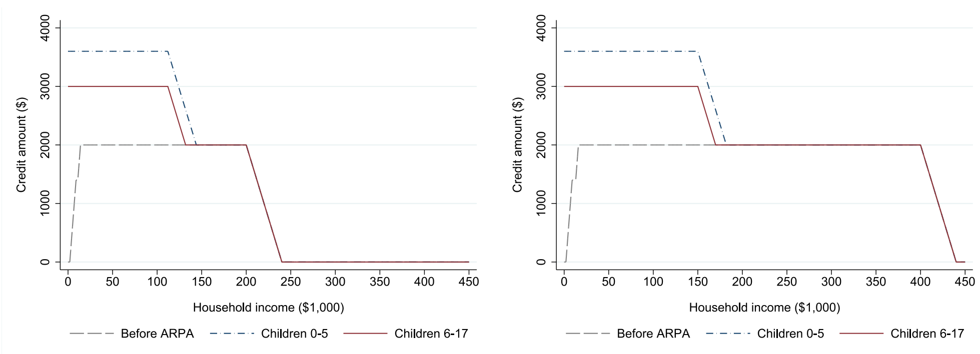
Figure 1 presents the effect of the reform on the schedule of benefit levels, plotting benefit level eligibility as a function of household income for unmarried (Figure 1, panel a) and married (Figure 1, panel b) individuals, both before and after the policy change. It shows that the reform has particularly benefited low-income households, while leaving unchanged benefit levels for households above a certain income threshold (that varies according to marital status and age of the child, ranging from $132,500 to $182,000). The figure also shows that the 2021 reform created a gap in CTC amount among households with the same income level and marital status, but with children of different ages (i.e., below or above the age of six). This component of the policy change will be exploited for identification.

The first monthly CTC payment was distributed to households of 59.3 million children in July 2021, for an estimated total disbursement of $15 billion. Subsequent payments reached households of 61 million children, covering more than 90% of families with children in the US.[8] Between 2020 and 2021, child poverty was approximately halved (falling from 9.7% to 5.2%), while the US Census Bureau estimates that 90% of this drop can be attributed to the CTC (Curran, 2022). However, the expanded CTC was in place only for the 2021 tax year and the Congress did not renew the program for subsequent years. Monthly transfers ended in December 2021, and households claimed the remaining benefit when filing taxes in spring 2022. For the 2022 tax year, the CTC reverted back to its pre-ARPA form.

There seems to have been a lot of uncertainty in late 2021 concerning the continuation of the expanded CTC, and partisan considerations played an important role in determining the final outcome. In particular, the US House of Representatives (where the Democratic Party had a majority) approved a continuation of the expanded CTC as part of the Build Back Better package in November 2021. However, the US Senate (which was equally split between Democrats and Republicans) did not take any action. Negotiations for extending the expanded CTC continued throughout the entire 2022, but the policy was not reinstated amidst concerns on the lack of work requirements to receive the transfer.

3 | DATA

We use data from the BRFSS to estimate the effects of CTC receipt on parents' self-reported mental health and other outcomes of interest. The BRFSS is an annual, state-based survey conducted over the telephone (both landline and cellphone) to collect information on health status and health behaviors of the US adult population. The survey is run by health departments in individual states and is later aggregated into an annual file by the Centers for Disease Control and Prevention, which is the national public health agency in the US.[9] The BRFSS is the largest health-related survey



(a) Unmarried (b) Married

FIGURE 1 The CTC as a function of household income, age of the child and marital status, before and after the 2021 ARPA. The figure reports the CTC credit amount (on the *y*-axis) as a function of household income (on the *x*-axis) before and after the temporary 2021 CTC expansion, for adults with children above or below the age of six. The information is reported separately for unmarried and married individuals (panels a and b, respectively). The figure is based on information provided in CSR (2021). ARPA, American Rescue Plan Act; CTC, child tax credit. [Colour figure can be viewed at wileyonlinelibrary.com] worldwide. Its sample size has increased from around 50,000 respondents covering 15 states when the survey was initiated in 1984, to more than 400,000 observations in all US states from the 2000s onward.

BRFSS collects detailed information on demographic characteristics and household composition, as well as a wide range of information on health outcomes, health care use, health habits, and employment status. The BRFSS also contains a module collecting information on a child selected at random in the household. For this child, the survey reports information on gender, race and relationship with the survey respondent. In the 2021 wave only, data is also released on the age of the child (in brackets), which we will use for identification. The survey is administered continuously throughout the year and it reports information on the exact date of the interview, allowing us to clearly identify individuals before and after the CTC expansion. Given the identification approach used in this paper, the BRFSS presents two main limitations. First, it allows to check for parallel trends only from the beginning of 2021, which is just a few months before the expansion of the CTC was announced. Second, the information on the child's age is reported only for one child chosen at random within the household, which could lead to measurement error of the treatment indicator. In order to overcome these limitations, we conduct complementary analysis using the UAS as well as the Current Population Survey (CPS).[10]

Table 1 presents descriptive statistics for selected variables for the main sample in the analysis, corresponding to the 2021 BRFSS wave. Interviews included in this survey wave were conducted between January 3, 2021 and February 28, 2022, although the first and last months in this window contain only few observations. The sample is well balanced between men and women as well as across the different age groups. 44% of respondents live in households with an annual income below $50,000, and half of them are married. Around 36% of respondents report the presence of a child in the household, with this child being the respondent's son/daughter in almost 80% of the cases. Children are equally distributed among the different age groups (i.e., 0–4, 5–9, 10–14 and 15–17) and in terms of their sex.

Table A1 reports selected descriptive statistics from the 2021 BRFSS and the 2021 CPS. The two samples are very comparable with respect to their age composition, sex, educational attainments.[11] Instead, the two surveys report larger differences when it comes to the racial composition of their samples (the BRFSS has a higher share of non-white individuals), the share of low-income households (the BRFSS has a higher share of households below $50,000) as well as the share of households with children (lower in the BRFSS compared to the CPS).[12] Differences between the two surveys have been also documented in previous studies (Arday et al., 1997), and can potentially be attributed to a series of survey characteristics.[13] While these differences should be kept in mind, we do not see them as constituting direct threats to our analysis.

TABLE 1 Descriptive statistics for selected variables, 2021 BRFSS sample.

	Mean	SD		Mean	SD
Men	0.487	0.500	Marital status: Unmarried couple	0.052	0.222
Age: 18–29	0.202	0.401	Presence of children in household	0.358	0.479
Age: 30–39	0.177	0.382	Number of children	0.922	1.893
Age: 40–49	0.152	0.359	Age of child: 0–4	0.244	0.430
Age: 50–59	0.160	0.366	Age of child: 5–9	0.251	0.434
Age: 60–69	0.156	0.363	Age of child: 10–14	0.276	0.447
Age: 70 and above	0.153	0.360	Age of child: 15–17	0.229	0.420
Income: <50k	0.440	0.496	Sex of child: Male	0.515	0.500
Race: White	0.724	0.447	Relationship to child: Parent	0.774	0.418
Ethnicity: Hispanic	0.173	0.378	At least 1 day not good mental health (0/1)	0.406	0.491
Marital status: Married	0.505	0.500	Number of days of not good mental health	4.640	8.561
Marital status: Divorced or separated	0.127	0.333	At least 1 day not good physical health (0/1)	0.321	0.467
Marital status: Widowed	0.068	0.252	Number of days of not good physical health	3.538	7.996
Marital status: Never married	0.248	0.432	Observations	438,693	

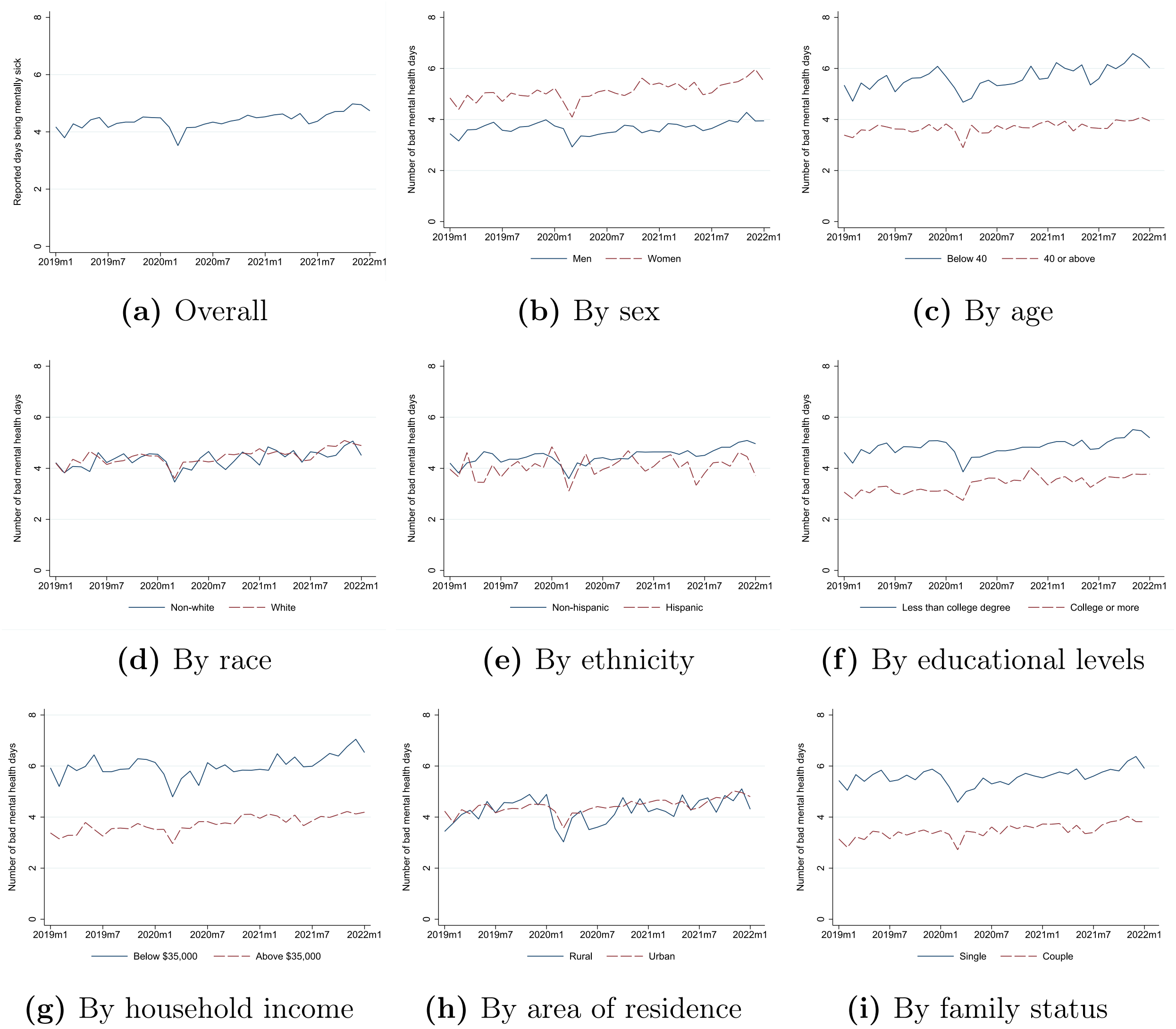
Note: The table presents means and SDs for selected individual and household characteristics, as measured in the 2021 wave of the BRFSS. The number of observations might vary across variables. Sampling weights are used to derive the estimates.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; SD, standard deviation.

The BRFSS response rate in 2021 was 44%, in line with values reported in previous waves.[14] This is slightly lower than the response rate of the National Health and Nutrition Examination Survey (51% for the interview sample and 46.9% in the examined sample between 2017 and 2020), but higher than the response rate of other widely used surveys such as the American Time Use survey (39.4% in 2021) and the California Health Interview Survey (11.2% in 2019). In order to alleviate concerns that the expansion of the CTC might have affected the composition of the sample, Table A2 shows selected descriptive statistics before and after the first monthly transfer was delivered on July 15, 2021. Out of the 23 selected variables, for only five we see statistically significant differences at the 10% level. Even in these five cases, differences are generally small in magnitude.

Our main outcome of interest will be the number of days an individual reports being not in good mental health over the previous 30 days. As stated in the survey question, this includes days in which the individual experienced “stress, depression, and problems with emotions.” The use of self-reported measures of mental health represents a standard practice in the literature (Braghieri et al., 2022). Research has also shown that self-reported measures of mental health status predict mental health diagnoses with an accuracy up to 90% (Kroenke & Spitzer, 2002). Our measure of mental health from the BRFSS has also been used in previous research (Evans & Garthwaite, 2014), where it delivered results consistent with those obtained when measuring mental health using biomakers.

Figure 2 plots the evolution of the number of days individuals report being mentally unhealthy between January 2019 and January 2022, for the overall sample (Figure 2, panel a) as well as by splitting the sample according to a



(a) Overall (b) By sex (c) By age

(d) By race (e) By ethnicity (f) By educational levels

(g) By household income (h) By area of residence (i) By family status

FIGURE 2 Average number of bad mental health days in the previous month, overall and by sub-groups (2019–2022). The figure reports the average number of bad mental health days reported by survey respondents in the previous month (2019–2022). Panel (a) presents the results for the overall sample, while panels from (b to i) present different results separately for different groups in the population. Sampling weights are used to derive the estimates. [Colour figure can be viewed at wileyonlinelibrary.com] number of characteristics (Figure 2, panels from b to i). While the series shows some month-to-month variation, the number of bad mental health days has clearly followed an upward trend over time.[15] In particular, the number of bad mental health days has increased from 4.17 in January 2019 to 4.74 in January 2022. This corresponds to a 13.7% increase, in line with trends reported from other sources (NSDUH, 2021). The number of mentally sick days has been traditionally higher among women than men (Figure 2, panel b), for young adults compared to older individuals (Figure 2, panel c), for low-educated and low-income individuals (Figure 2, panels f and g) as well as for singles compared to couples (Figure 2, panel i). Between 2019 and 2022, the number of bad mental health days has been instead similar among individuals of different race (Figure 2, panel d), ethnicity (Figure 2, panel e) and living in urban and rural areas (Figure 2, panel h).

4 | ESTIMATION STRATEGY

The purpose of the paper is to estimate the effects of the 2021 CTC expansion on parents' mental health outcomes, and to investigate its possible transmission mechanisms. We restrict the analysis to adults with children (i.e., all eligible to the CTC) and define treatment based on whether the child is above or below the age of 5. In doing so, we exploit policy-induced variations in benefit generosity based on the child's age.

Our main equation takes the following form:

$$Y_{isct} = \alpha + \beta_1 CTC_i + \beta_2 Time_t + \beta_3 CTC_i * Time_t + \beta_4 X_{isct} + c_s + t_c + \epsilon_{isct} \quad (1)$$

where Y_{isct} is the outcome of interest for individual i, living in state s in calendar month c, at time t before or after the CTC expansion. In the main analysis, this will correspond to the number of bad mental health days in the previous month, which we standardize to have mean zero and SD of one in the pre-treatment period. X_{ist} is a series of individual- and household-level characteristics. In particular, in the most complete specification, we control for state and month dummies, sex, age, marital status and number of children in the household, race, Hispanic ethnicity, educational attainments, and household income (in brackets). c_s is a set of state dummies, while t_c are calendar month dummies. In the baseline specification, $Time_t$ corresponds to a post-treatment dummy that takes the value of one after the policy change (see Section 5.1 for details on how this variable is constructed). However, we will also present results from event-study specifications at the monthly level, also to check for parallel trends in the pre-treatment period. CTC_i represents the treatment dummy. This takes the value of one if the respondent reports the presence of a child below the age of five in the household (and 0 for adults reporting a child between 5 and 17 years old).[16] The coefficient of interest in this DiD setting corresponds to β_3 in Equation (1), which is the coefficient of the interaction term between treatment status and the post-treatment dummy.

By comparing parents with children of different ages, we exploit policy-induced variations in CTC generosity introduced by the 2021 reform.[17] The exact amount of the benefit difference between parents with children above or below the age of six will depend on household income and marital status, but for most households this will correspond to $600 per year (see Figure 1).[18] Half of these $600 are delivered in monthly installments worth $50 in the period under consideration (from July to December 2021), while the other half is delivered as a lump-sum payment at the moment of tax filling in March 2022.

Figure B1 shows how the 2021 CTC expansion changed benefit levels for married and unmarried tax filers by the age of their child. For individuals claiming the maximum amount, the CTC expansion led to an increase in benefit levels equal to 80% if the child was below the age of six and 50% if the child was aged 6–17. These policy-induced variations in benefit levels are large. Consider an household with two children and an annual income of $55,000. Before the ARPA, this household was receiving a total CTC worth $4000 (or 7.27% of annual income irrespective of the child's age). After the reform, the CTC amount increased to $6000 if the two children were aged six or above (equal to 10.91% of total household income) and to $7200 if the children were below the age of six (corresponding to 13.09% of household income).

We confirm the existence of a first-stage relationship in the data by looking at the difference in the amount of CTC received between households with a child less than six, compared to households with an older child, before and after the CTC expansion. For this exercise, we use data from the 2022 and 2021 Annual Social and Economic Supplement (ASEC) of the CPS, where information on income sources for 2021 and 2020 is available. Specifically, we run a series of regressions using as dependent variable the average monthly value of CTC payments (see the note to Table 2 for details).

TABLE 2 First-stage relationship of the increase in monthly CTC benefits in 2021 for families with children below the age of 6 compared to families with children above age 6.

	(1) All	(2) Women	(3) Men	(4) Below age 40	(5) Above age 40	(6) Non-white	(7) White
Child under age 6 X 2021	51.02***	52.84***	48.75***	37.78***	57.98***	44.39***	52.27***
	(2.85)	(3.90)	(4.15)	(4.02)	(4.67)	(6.20)	(3.19)
	(8) Non-Hispanic	(9) Hispanic	(10) No college degree	(11) College degree	(12) Income < $75,000	(13) Income > $75,000	(14) Incomes >$200,000
Child under age 6 X 2021	46.91***	63.75***	62.45***	40.33***	46.04***	40.16***	2.95
	(3.15)	(6.43)	(5.13)	(3.29)	(4.20)	(3.75)	(9.00)

Note: Authors' analysis from the 2022 and 2021 Current Population Survey datasets, which measure income in calendar years 2021 and 2020. The table presents coefficient estimates and standard errors for different regressions, where the dependent variable is the average monthly value of CTC payments (in 2020, this is the lump-sum payments divided by 12; for 2021, we take the monthly value of the advance CTC payments, given that our empirical analysis later studies the effects of the advance payments). The sample is limited to adults between the ages of 18 and 64 with children in the household. All models include fixed effects for the number of children in the household. "Children Under Age 6" indicates that the household has a child under 6, while "2021" is a binary indicator of the reference period being 2021 rather than 2020. The control group is always represented by households with children aged 6–17. The larger monthly CTC payments in 2021 for children under 6 are the focus of our primary treatment effect in the baseline results on mental health, hence our focus on this independent variable in the models above.

Abbreviation: CTC, child tax credit.

***Significance at the 1% level.

In all specifications, we control for the number of children in the household (as it will be done in our baseline specification on mental health) and are interested in the effect of having a younger child (aged less than six) compared to an older one (6–17), after the CTC expansion. According to the functioning of the policy, this should lead to a monthly difference in CTC receipt equal to $50 on average. This is exactly what we find for the overall sample (column 1 of Table 2). Additionally, this effect is very similar across different sub-groups in the population for which we will later conduct the heterogeneous analysis. Instead, we do not find any first-stage relationship for households with a very high income (i.e., annual income above $200,000). This is also consistent with the functioning of the policy, as benefit generosity did not change for households in this income bracket, independently from the age of their children (see Figure 1 for details).

Table A3 reports selected descriptive statistics for individuals with children above or below the age of 5. We also report *p*-values of the *t*-tests for the equality of means for treated and control comparisons. We see that the two groups are very similar in terms of observable characteristics. Even when we find statistically significant differences (e.g., for the age of the respondent), they are generally small in magnitude.

By exploiting policy-induced differences in the change to benefit levels, our identification approach resembles the one adopted in previous work focusing on the 1993 EITC reform (Adireksombat, 2010; Evans & Garthwaite, 2014; Hotz & Scholz, 2006).[19] Similar identification strategies have been used extensively in the literature, including to study the impact of a child tax benefit expansion in Canada (Milligan & Stabile, 2011). Using this identification strategy also allows us to isolate the income effect from the effect of any other policy change that was implemented at the same time. This is key to examine the impact of income on mental health. In particular, and as documented above, the 2021 ARPA introduced a number of changes to the CTC, including changes to the system of benefit receipt (e.g., elimination of the phase-in component, monthly transfers of beneft). Our identification strategy holds constant all these other policy changes (i.e., they applied to all CTC recipients), and thus allows us to isolate the income effect of benefit receipt.

Even though we believe that comparing adults with children of different ages represents a better identification strategy, this is also subject to some potential threats.

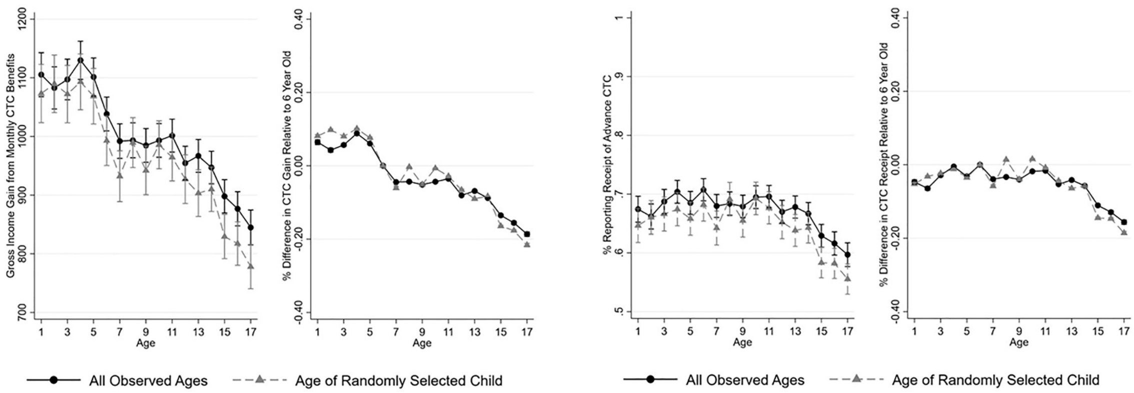
The first problem refers to the fact that the BRFSS does not report individual characteristics for all children in the household, but only for one child selected at random. This means that we have information on the total number of children (that we include as one of the controls), but details on personal characteristics (i.e., age, gender and race) only for one of these children. As a result, some households will be classified as treated (control) based on the age of the reported child, but might be in the control (treatment) group if instead we were observing another child of a different age in the same household. This can lead to a contamination of treatment across households, potentially leading to attenuation bias. In the results section, we will check the extent to which this problem can bias our estimates, by looking at households with only one child, for whom we can perfectly define treatment status, as well as by presenting results from a triple difference approach, which relies less on the measurement of the age of the child.

However, we can investigate already now the extent to which having access to information on the age of just one child at random leads to measurement error in the first-stage relationship. We do this by looking at the first stage relationship between the age of the child and CTC receipt. According to the rules introduced by the 2021 reform, parents with children below the age of six should receive relatively more generous transfers, a pattern that we have just confirmed above. Ideally, we would like this relationship to hold even if we observe only one child selected at random within the household. We can test both hypotheses using the 2022 ASEC of the CPS, which reports information on all children in the household (including their age) as well as self-reported data on CTC receipt for 2021. In line with our expectations, we see that CTC transfers are higher for households with younger children (Figure 3, panel a). This is not driven by differences in policy take-up around the age six threshold (Figure 3, panel b). Importantly, both results are very similar irrespective of whether we take into account the age of all children in the household (black lines in Figure 3) or if we randomly select a child in the household (gray line).

The second potential problem relates to the fact that, by relying on age differences between children, we restrict our attention to the 2021 BRFSS wave only (i.e., this is the only BRFSS wave for which this information was publicly released), which limits the possibility to check for parallel trends over a longer period of time. Recall that the first CTC monthly payment was disbursed in July 2021, but the CTC expansion was announced already in March 2021. This implies that, using data from the 2021 BRFSS, we have only 2 months to check for parallel trend before the policy announcement.

To address this concern, we present complementary evidence from the UAS. This survey has some limitations, including high attrition rates and the fact that it is conducted online. Additionally, respondents receive a monetary compensation for participating. All these features, together with the smaller sample size, explain why we are not using the UAS as the main data source for the analysis.[20] However, the UAS COVID survey, launched in March 2020, collects information on parents' mental health status and the age of the child in the household, thus allowing to check for parallel trends for a longer period of time before the announcement of the CTC expansion. Additionally, the UAS, unlike the BRFSS, is a panel survey. This allows us to check whether our conclusions concerning the parallel trend assumption would differ, if we were to add individual fixed effects.

We present these results in Figure B2 (using UAS data as repeated cross-sections) and 13 (adding individual fixed effects). As outcomes of interest, we use dummy indicators equal to one if the respondent showed always, often or generally symptoms of depression (Figure B2, panel a), anxiety (Figure B2, panel b), worry (Figure B2, panel c) or little interest (Figure B2, panel d).[21] We find that (i) trends in the different outcomes of mental health were parallel between



(a) Benefit levels from advance CTC payments by age of children

(b) Self-reported receipt of advance CTC payments by age of children

FIGURE 3 CTC benefit levels and benefit take-up by age of the child. Panel (a) plots the relationship between per-child monthly CTC payments and child's age, both in absolute terms (left part of the panel) and relative to the gains for children aged six (right part of the panel). Panel (b) plots instead the relationship between CTC take-up and child's age, both in absolute terms (left part of the panel) and relative to the gains for children aged six (right part of the panel). In both panels, we plot the relationship using information on the age of all children in the household (black line) and then using only information on one child selected at random (gray line). CTC, child tax credit.

parents with children below or above the age of six in the months before the announcement of the CTC expansion, and (ii) the inclusion of individual fixed effects does not affect this conclusion. This provides evidence of the fact that the parallel trend assumption is likely to hold when comparing adults with children of different ages. We will provide further evidence supporting this assumption in Section 5, when we will present event-study estimates using the BRFSS for the few months before the policy announcement for which we have the necessary information (i.e., January and February 2021).

5 | RESULTS

This section presents the main results of the analysis. More specifically, Section 5.1 presents results on the effects of the CTC on the number of bad mental health days for the overall sample following our baseline specification that compares adults with children of different ages; Section 5.2 presents additional results using an alternative identification strategy that compares adults with and without children; Section 5.3 presents a series of robustness tests to our preferred identification strategy; Section 5.4 explores the heterogeneity of treatment effects across groups in the population; Section 5.5 discusses possible mechanisms for our main findings.

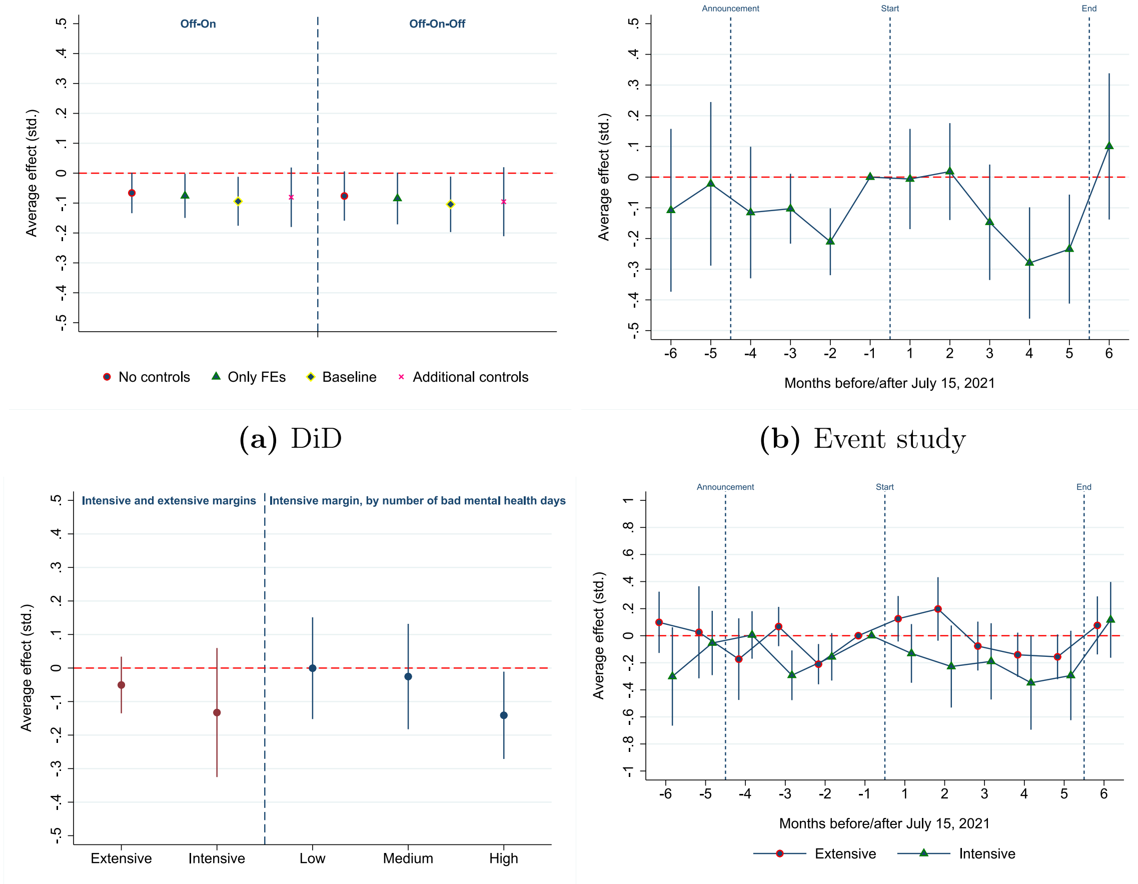
5.1 | Baseline results

Figure 4 presents estimates of β_3 in Equation (1). The outcome of interest is the number of bad mental health days an individual reports over the previous month, normalized to have mean of zero and SD of one in the pre-treatment period.

Figure 4, panel a presents the results of a simple DiD specification. The post-policy dummy is constructed in two alternative ways. In the "Off-On" specification, the dummy takes the value of zero from the start of the survey interviews in January 2021 until August 14, 2021, and the value of one from August 15 until December 15, 2021. The survey questionnaire elicits information on the number of bad mental health days in the previous month, which is why the post-treatment period starts on August 15 in the DiD specification (i.e., 1 month after the first CTC transfer).[22] In the "Off-On-Off" specification, the dummy is constructed in the same exact way until December 15, 2021, but then is set to zero (rather than missing) from December 16, 2021 until the end of the survey interviews in February 2022. For each definition of the post-treatment dummy, we present results from different specifications, with (i) no covariates ("No controls" in Figure 4), (ii) with only state and month dummies ("FEs"), (iii) with also some individual-level covariates for sex, age, marital status and number of children in the household ("Baseline") and (iv) with additional controls for race, Hispanic ethnicity, educational attainment, and household income ("Additional controls").

The results that we obtain are very similar irrespective of the definition of the post-treatment dummy as well as the choice of the covariates. In particular, we find that a more generous transfer for their children improves self-reported mental health of the parents. In our preferred specification in panel a of Figure 4 (i.e., corresponding to the baseline model, where we control, among others, for the number of children in the household), having access to a more generous benefit decreases the number of bad mental health days for the parents by 0.094 SD units. Our estimates are very much in line with those reported in previous studies examining the mental health effects of unconditional cash transfers in developing countries (Ridley et al., 2020), although it is not clear the extent to which findings from these contexts could be generalized to the US.[23] As a matter of comparisons, the effect of a more generous CTC is similar in magnitude to the (negative) effect on mental health from the introduction of Facebook in US colleges (Braghieri et al., 2022) and around 25% of the average effect of job loss on mental health reported in the meta-analysis by Paul and Moser (2009).[24] The mental health benefits that we find are around half of those that have been estimated from the provision of public health insurance in the US (Finkelstein et al., 2012).

Although estimates from our preferred specification are in most cases only marginally significant, they represent large responses to the CTC expansion. To give a better sense of the magnitude of treatment effects, we re-run our baseline DiD specifications using the dependent variable in its count format. In this specification, we adopt a negative binomial regression model to account for over-dispersion of the dependent variable. Figure B4 shows the results of this exercise, following the same structure of panel a of Figure 4. In our baseline specification, a more generous CTC reduces the number of days of bad mental health by 15%. We also know that families with children below the age of five have received a maximum annual increase in the CTC amount worth $600, compared to families with older children. Assuming universal policy take-up, this implies a lower bound estimate according to which increasing benefit amount



(a) DiD **(b)** Event study

(c) DiD on different margins **(d)** Event study on different margins

FIGURE 4 Effects of the receipt of the expanded CTC on the number of bad mental health days. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). In all panels, we restrict the analysis to adults with children and compare those whose child is below or above the age of 5. In panel (a), we additionally report results using (i) two different definitions of the post-treatment variable ("Off-On" and "Off-On-Off," see text for details) and (ii) different specifications that vary according to the types of covariates included in the analysis (see text for details). In panel (b), we use the controls included in the "Baseline" specification to derive event-study estimates. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. In panel (c), we present, on the left part of the figure, DiD results separately for the "Intensive" and "Extensive" margins (see text for details). Additionally, we further investigate the intensive margin by presenting, on the right part of the figure, results separately for groups of individuals with a "Low" (≥5), "Medium" (≥10) or "High" (≥20) number of bad mental health days during the previous month. Finally, panel (d) replicates the event study estimate of panel (b), but this time analyzing separately results for the extensive and intensive margins (i.e., in the latter case, without differentiating by the number of bad mental health days reported during the month, as it is instead done in the right part of panel (c)). Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]

by \$500 would decrease the number of mental bad days by 12.5%. This is a steep income-health gradient, in line with previous estimates from the US on the EITC (Evans & Garthwaite, 2014). Additionally, we note that, if anything, the estimates that we just discussed should be interpreted as lower bounds of the true treatment effects. This is because the lack of information on the age of all children in the household likely leads to measurement error of the treatment variable, which would generate attenuation bias of treatment effects.

Panel b of Figure 4 shows the results from our event-study specification, where we substitute the post-treatment dummy with a full set of monthly dummies, with the omitted dummy corresponding to the one for the month before the CTC expansion. We present results using only the covariates included in the baseline model introduced above. We find substantial heterogeneity in treatment effects over time. In particular, adults with children below or above the age of five were on parallel trends before the policy announcement that took place in March 2021. Afterward, adults with children below the age of five started experiencing a relative improvement in their mental health status. This anticipation effect peaks 2 months before the policy change, when trends between treatment and control groups return parallel. This situation continues until 3 months after the first CTC monthly transfer, when mental health starts improving again among households receiving a more generous CTC. However, the positive effects of the more generous benefit vanish when benefits are withdrawn in December 2021. The fact that positive mental health effects appear only after the third CTC monthly payment is in line with findings in Kovski et al. (2023), who look at the effects of the CTC expansion on anxiety and depression in a sample of SNAP beneficiaries. They interpret the timing of treatment effects in line with a dosage effect (i.e., a certain treatment dosage is needed before change is observed).[25]

We also note that the announcement effect that we obtain is almost as large as the effect of benefit receipt. This is consistent with the fact that the CTC expansion might have generated a psychological benefit that goes beyond the financial alleviation it provided. This is very plausible in the present context, given that individuals had accumulated savings during the first phases of the pandemic (e.g., due to forced savings because of business closures as well as the expansion of other social security programs). Accordingly, checking and savings account balances were substantially above their pre-pandemic levels for households at all income levels (Wheat et al., 2023). This implies that the mental health effects from actual CTC receipt that we obtain for the months after the program start are probably at the lower bound of estimates that could be obtained in contexts in which credit constraints play a more important role.

Finally, we want to investigate whether the positive effect on mental health materializes because of a reduction in the share of individuals who report any bad mental health day (so-called extensive margin), or, rather, due to a decrease in the number of bad mental health days reported by individuals who report at least one bad mental health day per month (so-called intensive margin). In panel c of Figure 4, we present the DID results on the intensive and extensive margins separately (left part of the Panel). We see that, despite the coefficients being imprecisely estimated, most of the positive effect on mental health comes from a reduction in the number of bad mental health days within the population reporting at least one bad mental health day. In panel d of Figure 4, we present the event study estimates corresponding to these DiD results. They show that the intensive margin follows a similar trend than the overall results described overall in Figure 4, panel b, while the extensive margin does not present any clear trend over the entire period (i.e., it shows some variation over time, but this is small in magnitude and generally not statistically significant).

Returning to panel c of Figure 4, we want to further understand whether the intensive margin result is driven by the population of individuals reporting a low, medium or high number of bad mental health days (corresponding to $\geq$5, $\geq$10 or $\geq$20 days, respectively). This has important policy implications, as individuals with a high number of bad mental health days per month are likely more difficult to treat and require more intense health care. We find that our effects are driven by individuals who report a high number of bad mental health days. This suggests that the provision of cash transfer can be effective in improving mental health even among individuals with severe mental health conditions.

5.2 | Additional results comparing households with and without children

We now present results from an alternative specification strategy, which is the one comparing households with and without children, as done in some previous contributions on the 2021 CTC expansion. In this case, treatment corresponds to the receipt of the expanded CTC, with the actual benefit amount varying according to individual and household characteristics. The results obtained under this alternative specification are presented in Figure C1, showing the simple DiD estimates (Figure C1, panel a) and the event-study results (Figure C1, panel b). According to these results, we do not find any effect of the CTC expansion on parents' mental health. This is in line with the evidence presented in some of the earlier contributions on the mental health effects of the 2021 CTC expansion (Batra et al., 2023; Collyer et al., 2022; Glasner et al., 2022). However, we do not find these results as invalidating our main findings as discussed above, and present them only for comparison purposes.

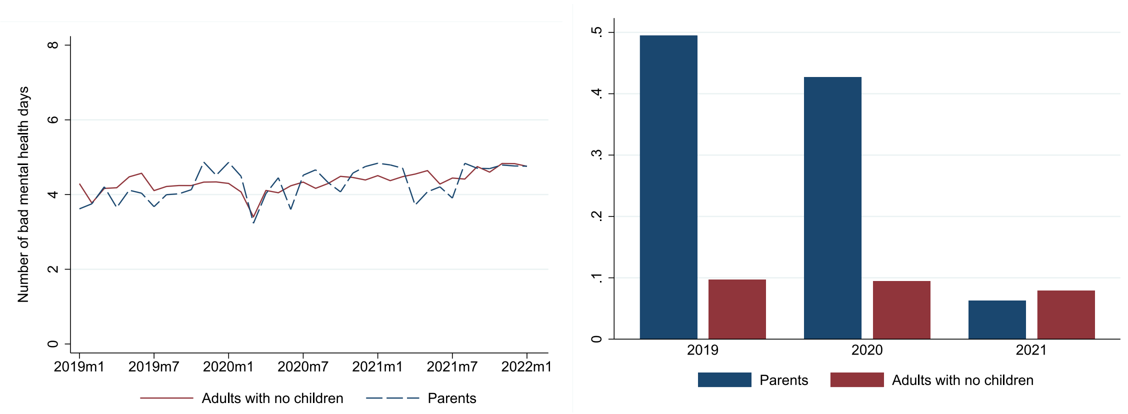
This is because the specification that compares adults with and without children presents at least two limitations, compared to the one we used in the paper.

First, comparing adults with and without children does not allow to isolate the income effects of the 2021 CTC policy change. As reviewed in Section 2, ARPA modified many other policy characteristics (e.g., elimination of phase-in portion of the CTC, introduction of the monthly transfer), in addition to changing benefit levels. This means that this identification strategy would allow to estimate only the combined effect of a series of policy changes, which might have potentially had opposite effects on parents' mental health. Additionally, this policy change took place in a context in which parents were unsure about the possible extension of the expanded CTC beyond 2021, and might have therefore not considered the additional benefits they were receiving as representing a permanent change in their households' disposable income. Under this uncertainty, this identification strategy is likely to under-estimate the positive mental health effects of the policy change.

Second, the parallel trend assumption is less likely to hold when comparing adults with and without children. In particular, this could have happened due to standard variations in mental health status over the year (e.g., increased stress for parents due to school reopening in the fall), which could have been exacerbated in the fall of 2021. Indeed, COVID-19 vaccines were not yet available for children of most ages around the time of the CTC expansion and school closures were still common at that point in time. If this was true, this identification strategy would, once again, underestimate the true effects of the policy. In particular, the zero effects we find as part of this specification might result from a positive treatment effect and a negative trend in parents' mental health around the same point in time, absent the policy change. We now briefly provide some evidence in favor of this hypothesis.

To start with, Figure 5 plots the evolution of the number of bad mental health days for adults with and without children between 2019 and 2022 (Figure 5, panel a). We see that parents report higher variability in the number bad mental health days, compared to adults without children. In Figure 5, panel b, we specifically investigate trends in the number of bad mental health days between parents and non-parents in the fall (i.e., September to December, a period that includes the time of the 2021 CTC expansion). We observe that parents experience a sharper deterioration in mental health status between September and December in both 2019 and 2020, compared to non-parents. The increase in the number of bad mental health days is instead substantially smaller in the fall of 2021.

Panel a of Figure C2 presents the results of the DiD specification comparing parents with adults without children, but for 2019. While in 2021 we had precisely estimated treatments effects around zero (see panel a of Figure C1), in 2019 adults with children (both parents and non-parents) experience a deterioration in their mental health status around the time of the (placebo) policy change. Panel b of Figure C2 brings together these results by presenting the estimates of a triple-difference approach, using data for both 2019 and 2021 to control for within-year variations in mental health status constant across years.[26] Although results are imprecisely estimated, all coefficients are negative in sign, suggesting that the CTC expansion might have led to an improvement in mental health status among parents.



(a) Average number of bad mental health days in the previous month, by presence of children in the household (2019-22)

(b) Difference in the number of bad mental health days between December and September of each year, by presence of children in the household (2019-22)

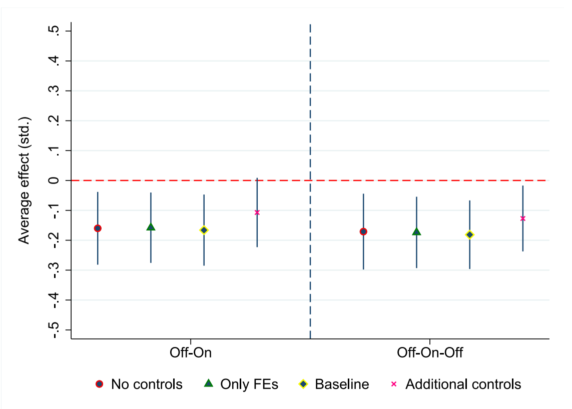
FIGURE 5 Evolution in the number of bad mental health days, by presence of children in the household (2019–2022). Panel (a) plots the average number of bad mental health days reported by survey respondents in the previous month (2019–2022), separately for parents and adults without children. Panel (b) presents the difference in the number of bad mental health days between December and September of each year (i.e., December–September) between 2019 and 2021, again separately for parents and adults without children. [Colour figure can be viewed at wileyonlinelibrary.com]

5.3 | Robustness tests

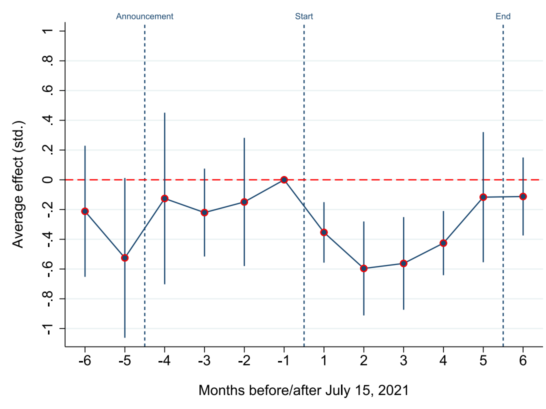
We now return to our baseline specification (i.e., comparing parents with children of different ages) and present some robustness tests. One of the main concerns is related to the fact that we observe children's characteristics (including age) only for one child selected at random within the household. Since we rely on this information to define treatment status, this can lead to measurement error of our treatment indicator (e.g., some households being classified as treated based on the characteristics of the child selected at random would be classified as control based on the characteristics of another child). This could lead to attenuation bias of treatment effects due to mis-measurement of the treatment indicator.

To start with, we present results of specifications that do not rely exclusively on the comparison of children of different ages, but use instead household income as another dimension in the analysis in a triple difference approach. This estimation exploits the fact that the 2021 CTC change did not affect benefit levels for households above a certain income level (which varied based on marital status, see Figure 1). Identification comes by comparing parents with children below or above the age of five in relatively poorer versus richer households.[27] The assumption is that richer households are either not affected by the policy change or, even if they experience an increase in benefit levels, this plays a smaller role compared than for poorer households. As far as the measurement error problem is similar among relatively poorer and richer households (an assumption that should be satisfied by the random selection of the child at the moment of the interview), these results should not be affected by measurement error of the treatment variable. Reassuringly, we find a reduction in the number of bad mental health days even under this specification (Figure 6). Point estimates are larger in magnitude compared to our baseline results, in line with the fact that our baseline results might be affected by attenuation bias. However, confidence intervals are also larger, as it is often the case in a triple difference approach.

As a second exercise to test the extent to which mis-measurement of the treatment variable can affect our baseline results, we focus on households that report only one child, for which the information on the child picked at random necessarily corresponds to all the relevant information for the definition of the treatment status.[28] Figure 7 presents the results of this robustness tests, separately for the DiD estimates (Figure 7, panel a) and the event-study approach (Figure 7, panel b). All DiD estimates are negative in sign (as our baseline estimates), but small in magnitude and not statistically significant. However, the event study results show very similar findings compared to our baseline

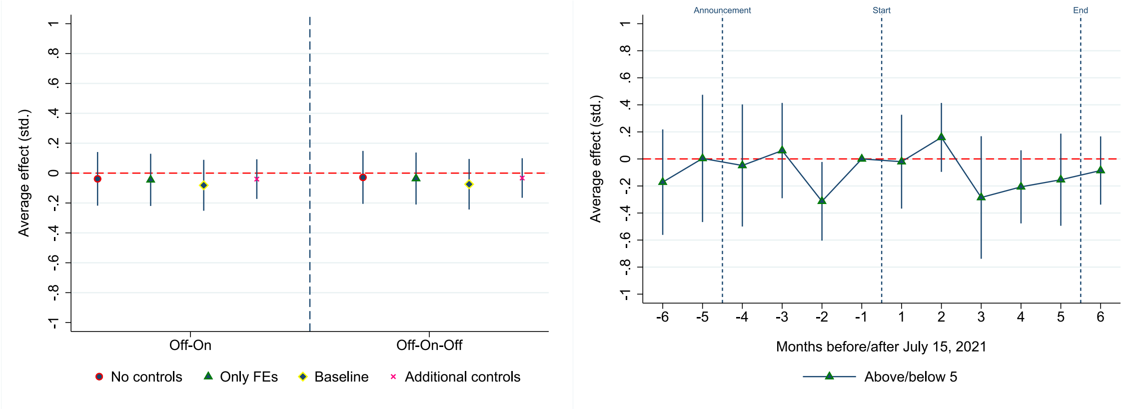


(a) DiD



(b) Event study

FIGURE 6 Triple difference with household income. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Here, the analysis adopts a triple-difference identification approach, comparing individuals (i) with children below or above the age of five, (ii) before and after the policy change, (iii) with an annual income below or above \$75,000. In panel (a), we report estimates that vary depending on the definition of the post-treatment period ("Off-On" and "Off-On-Off," see text for details) as well as for the set of covariates included. The event-study results in panel (b) use instead the set of covariates corresponding to our "Baseline" model (see text for details). In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit. [Colour figure can be viewed at wileyonlinelibrary.com]



(a) DiD (b) Event study

FIGURE 7 Only households with one child. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). The identification strategy adopted is the one comparing adults with children below or above the age of five. Differently from the baseline results, we exclude households with more than one child from both the treatment and control groups. In panel (a), we report simple DiD estimates that vary depending on the definition of the post-treatment period ("Off-On" and "Off-On-Off," see text for details) as well as for the set of covariates included (see text for details). The event-study results in panel (b) use instead the set of covariates corresponding to our "Baseline" model. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]

specification. In particular, we find a decrease in the self-reported number of bad mental health days starting from 3 months after the receipt of the first CTC monthly transfer.

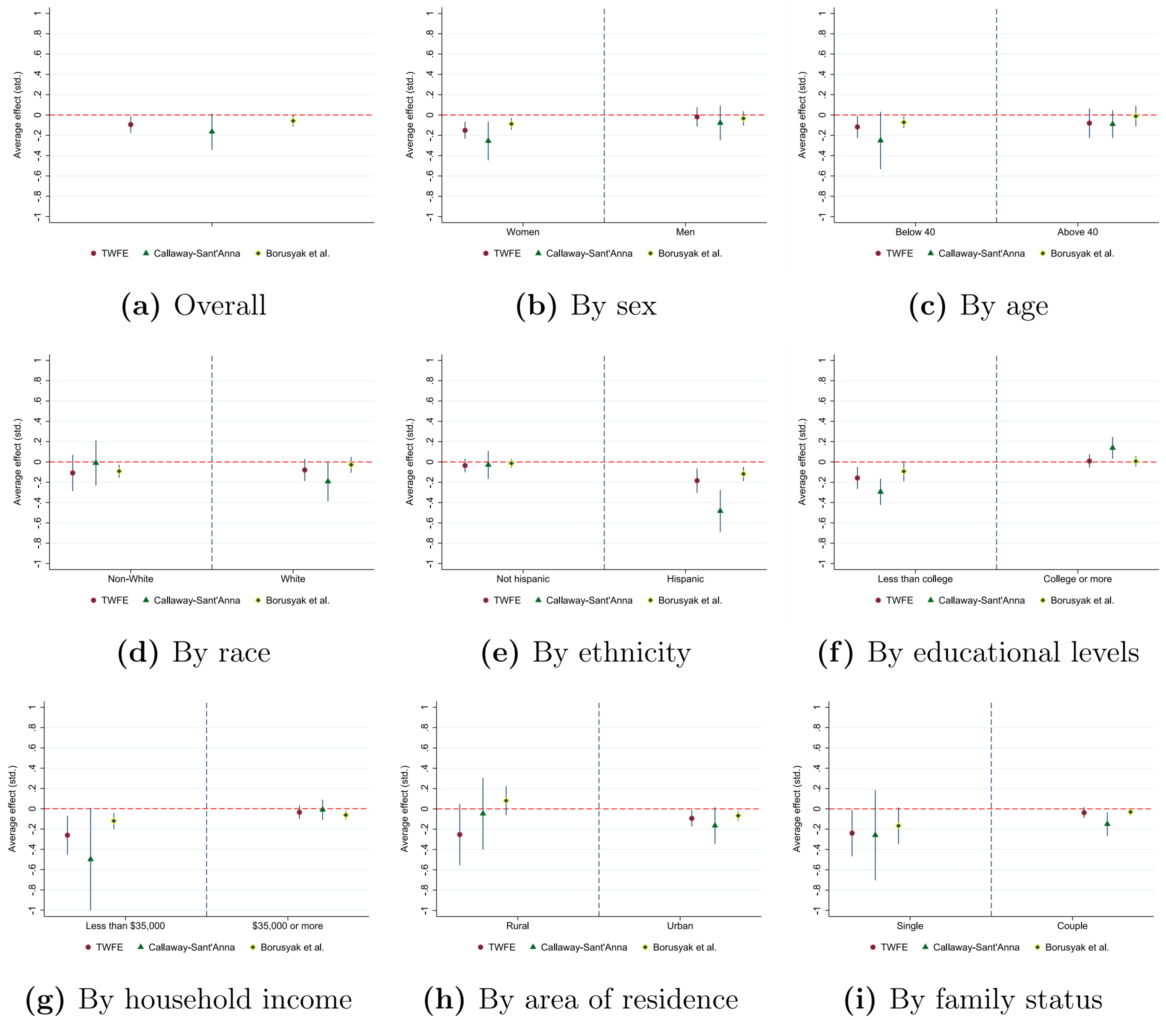
We conclude this section by presenting a series of miscellaneous robustness and placebo tests. First, we replicate the baseline results but excluding children aged 10–17 from the control group. This is to account for the fact that COVID-19 vaccines started becoming available for children of this age in the fall of 2021. However, results are very similar when using this smaller control group (Figure C3). Additionally, we exclude kids aged from five to nine from the control group. This is to account for the fact that some of these individuals (i.e., those aged 5) should be considered as treated following the policy rules.[29] Even in this case, results remain largely unchanged (Figure C4). We then compare results obtained using all adults with children and then restricting the analysis only to parents (Figure C5). We also augment the specification by including additional controls for child's race, ethnicity and sex (Figure C6). Once again, results that are very similar to those from our baseline model. Finally, we compare adults with children below and above the age of five, but for whom the policy did not generate any change in the CTC.[30] If our treatment effects were driven by other confounding factors (e.g., lower exposure to school closures among parents with younger children), we should see an effect also for these individuals. Instead, treatment effects are precisely estimated around zero (Figure C7).

5.4 | Heterogeneous effects

We now explore heterogeneous treatment effects across groups in the population. In particular, we replicate the main analysis, but dividing the sample according to some individual and household characteristics. This allows us to study heterogeneity in treatment effects because, given the identification strategy that we adopt, treatment intensity does not vary across different sub-groups (as we have also confirmed this finding with the first-stage results in Table 2). We believe that this exercise is interesting, given that we have seen that mental health conditions differ substantially across groups in the population (Section 3). Additionally, research has shown that poverty can affect mental health through a variety of mechanisms that can affect different groups differently (Ridley et al., 2020). Accordingly, previous studies have shown that the mental health benefits from cash transfers are not equally distributed in the population. This includes studies on the mental health effects of the 2021 CTC expansion (Batra et al., 2023; Kovski et al., 2023).

Figure 8 presents the results of this exercise. To confirm their validity, we present them using both the two-way fixed-effect (TWFE) model as well as using alternative estimations proposed by Borusyak et al. (2021) and Callaway and Sant'Anna (2021). This is because the standard TWFE model delivers consistent estimates only under relatively strong assumptions (Sun & Abraham, 2021). We present the results for the overall sample in Figure 8, panel a, and the results of the heterogeneous analysis in Figure 8, panels b–i.

The results show that the positive effect on mental health is reported among women, but not for men (Figure 8, panel b). The effect is also stronger for individuals below the age of 40, compared to older individuals (Figure 8, panel c). Instead, we do not find any notable difference in treatment effects between white and non-white individuals (Figure 8, panel d). In particular, both groups see a reduction in the number of bad mental health days, but the magnitude of the effect varies across model specifications.[31] However, the positive effect on mental health materializes only for individuals of Hispanic origin (Figure 8, panel e). Similarly, treatment effects appear only for low-educated individuals (i.e., with less than a college degree) as well as for individuals who live in poorer households (i.e., annual income below $35.000) (Figure 8, panels f and g, respectively).[32] We do not find reach any firm conclusion with respect to the heterogeneity in treatment effects for individuals living in urban versus rural areas, possibly due to the small sample of



(a) Overall (b) By sex (c) By age
(d) By race (e) By ethnicity (f) By educational levels
(g) By household income (h) By area of residence (i) By family status

FIGURE 8 Heterogeneity in treatment effects. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). In all panels, we use the post-treatment dummy corresponding to the "Off-On" model in Figure 4 and the set of covariates included in our "Baseline" model (see text for details). Each panel presents results for groups in the population defined by different individual or household characteristic (e.g., by sex, age groups, race). For each group, we present DiD results from the TWFE model as well as from alternative models proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2021). Standard errors are always clustered at the state level. DiD, difference-in-differences; TWFE, two-way fixed-effect. [Colour figure can be viewed at wileyonlinelibrary.com] individuals in rural areas (Figure 8, panel h). Finally, we find that effects are stronger for single parents, compared to couples (Figure 8, panel i).

Overall, CTC receipt seems to have been particularly beneficial for certain groups (e.g., low-income and unmarried women) that are more likely to be suffering from mental health conditions and also to report larger financial constraints. These results are line with those of previous studies analyzing the effects of other social protection programs in the US. For instance, the literature on the EITC has consistently reported larger effects for women compared to men (Evans & Garthwaite, 2014). Additionally, these results echo our understanding of the functioning of the policy and its potential impact on mental health. In particular, Figure 2 has already shown that women, individuals below the age of 40 as well as low-income and low-educated individuals tend to report a higher number of bad mental health days. By relaxing financial constraints, the more generous CTC might have reduced economically-induced concerns and contributed to better mental health among these groups.

5.5 | Potential mechanisms and discussion

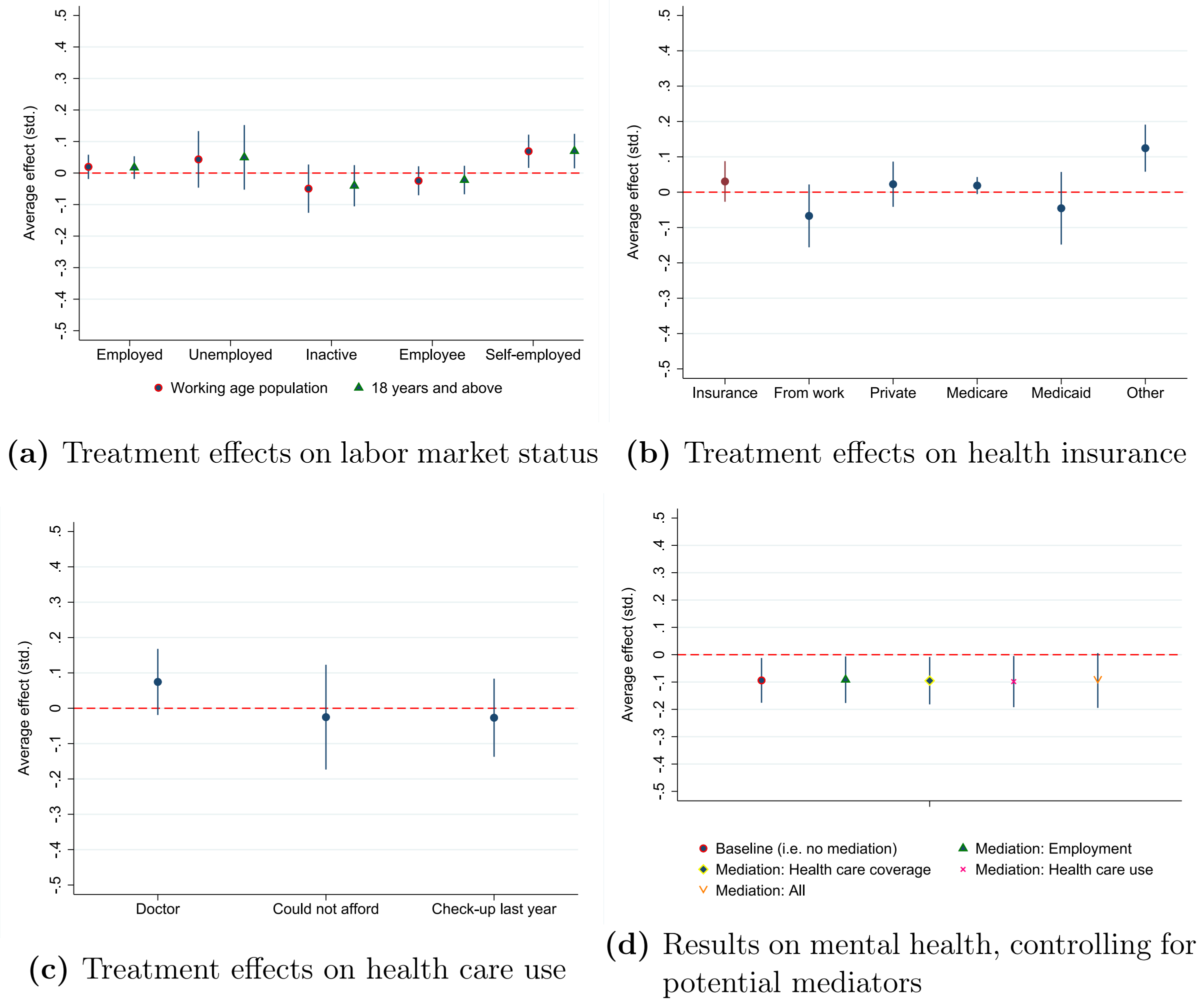
We finally explore possible transmission mechanisms from CTC receipt to mental health. Previous work on the 2021 CTC expansion has shown that it reduced material deprivation along a number of dimensions. In particular, the CTC reduced food insufficiency by 20% and reduced the likelihood that households were behind on rent by 10% (Parolin et al., 2023). Research has also shown that the CTC monthly transfers increased spending in child care centers, personal care establishments and grocery stores (Parolin et al., 2024). As a result, it is estimated that the CTC expansion reduced child poverty rate by as much as 40% (Parolin, Collyer, et al., 2021) and decreased material hardship (Parolin, Ananat, et al., 2021). All this is likely to generate positive effects on mental health, especially among those groups for which we find larger treatment effects (e.g., low-income individuals).

Unfortunately, the BRFSS does not report information on total consumption expenditures or the use of certain goods and services (e.g., food, child care). This means that we cannot directly test whether the mental health effects documented above arise as a result of this channel. In the rest of this section, we will test alternative hypotheses to confirm findings from previous studies and/or rule out other transmission mechanisms. In particular, we will look at treatment effects on (i) labor market outcomes, (ii) health care coverage, and (iii) health care use. These results will be presented for the overall population in the main text (panels a–c of Figure 9) and for different sub-groups of the population in Appendix B. We will then account for potential mediators by replicating the results on mental health, but now also including, among the covariates, the possible transmission channels mentioned above (panel d of Figure 9). If treatment effects are still present, this means that other mechanisms drive our results.

We start by looking at the effects of the CTC expansion on labor market outcomes.[33] In particular, we look at effects on employment status (i.e., employment, unemployment and inactivity) and also status in employment (i.e., dependent employment or self-employment). We present results separately for the working age population (18–64 years old) as well as for the entire population above 18. We find zero effects on overall employment, unemployment and inactivity (panel a of Figure 9). Zero effects on employment are common to almost all groups in the population, although certain categories (e.g., men and older individuals) report small positive employment effects (Figure B5). This is in line with previous evaluations of the CTC expansion, documenting lack of disincentive effects on labor supply (Ananat et al., 2021). At the same time, we find a small shift from dependent employment to self-employment, but we do not investigate its causes.

Positive treatment effects on health care coverage would materialize if individuals use the more generous CTC to buy some form of insurance. As such, any possible positive effect should materialize trough privately provided schemes. In turn, increased health coverage could improve mental health, as also documented in previous research (Finkelstein et al., 2012). In order to explore this channel, we look at treatment effects on overall health care coverage (red dot in panel b of Figure 9) as well as for different types of health insurance schemes (blue dots in the same panel). As expected, we find zero effects on the likelihood of being covered by public health insurance schemes such as Medicare and Medicaid, but we also find zero effects on other forms of private insurance (i.e., from work or other private schemes). We find a positive effect on other miscellaneous categories of health insurance.[34] However, the share of individuals being covered by these miscellaneous programs is small and, as a result, the overall effect on health coverage is zero.[35]

As a further hypothesis, we test whether the CTC expansion led to any changes in health care use. The BRFSS asks respondents whether they have a doctor that they consider being their personal health care provider ("Doctor" in panel c of Figure 9), if they missed a doctor appointment within the last 12 months because they could not afford it ("Could



(a) Treatment effects on labor market status

(b) Treatment effects on health insurance

(c) Treatment effects on health care use

(d) Results on mental health, controlling for potential mediators

FIGURE 9 Potential mechanisms. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). In panel (a), the outcomes of interest are dummies for employment status (i.e., employment, unemployment and inactivity) or status in employment (i.e., dependent employment or self-employment). Each time, we run the analysis for the overall sample and then restricting to the working age population (18–64). Dummies for status in employment are set to zero for individuals who are not employed. In panel (b), the outcomes of interest are dummies equal to one if the individual reports having health insurance coverage, overall (red dot) and by type of coverage (blue dots). In panel (c), the outcomes of interest are dummies equal to one if the individual reports having a doctor that considers as his/her personal health care provider ("Doctor"), if the individual missed a doctor appointment in the last 12 months because it could not afford it ("Could not afford") and if the individual had at least one regular health check-up within the last year ("Check-up last year"). In panel (d), we conduct mediation analysis by adding controls for, respectively, (i) employment status (equal to one if the individual is employed) and status in employment (equal to one if the individual is in self-employment) (i.e., Mediation: Employment), (ii) health insurance coverage (equal to one if the individual has any type of insurance) and the type of insurance (equal to one if the individual has private insurance) (i.e., Mediation: Health care coverage), and (iii) the presence of a health care provider, missing a doctor appointment because it could not afford and having undertaken a health check-up in the last year (i.e., Mediation: Health care use). The last set of results in panel (d) are obtained by including all the potential mediators at once (i.e., Mediation: All). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

not afford" in the same panel) and whether they had the last doctor routine check-up within the last 12 months ("Check-up last year" in the same panel). We see that treatment effects on all these dimensions are not statistically significant. We conduct the heterogeneous analysis by sub-groups using the presence of a personal doctor as the main outcome of interest (Figure B7) and find zero effects across the board, although estimates for low-income individuals are positive and relatively large in magnitude.[36]

Finally, we add these variables capturing labor force status, health care coverage and health care use as additional covariates in our baseline models (i.e., when the outcome of interest is the number of bad mental health days). Having shown that CTC receipt had no effect on this potential transmission channels, we should not expect our baseline treatment effects on mental health to substantially change when we include these variables as controls. Panel d of Figure 9 confirms this hypothesis. In particular, we first replicate our baseline result on the number of bad mental health days (first estimate to the left). All the subsequent estimates plot treatment effects obtained by adding to our baseline specification one set of potential mediators at time (i.e., for employment, health care coverage and health care use). Finally, the last estimate to the right includes all these potential mediators at once. The treatment effects on the number of self-reported bad mental health days remain remarkably similar across specifications, suggesting that other transmission channels must be at play.

In the absence of data to test alternative hypotheses in the BRFSS, we can only speculate that the positive effects on mental health materialize due to the increase in consumption and the reduction in poverty and material hardship documented in previous studies. Once again, this is consistent with larger treatment effects among credit-constrained individuals such as women, young adults and low-income households. While we cannot directly observe consumption expenditures or material deprivation in the BRFSS, we have access to information on certain healthy or unhealthy behaviors. This can be useful to benchmark our findings with those of previous papers that have examined consumption effects of the CTC expansion, including on temptation goods. Parolin et al. (2024) find an increase in overall household expenditure after the first CTC monthly payments, but not for items such as alcohol, tobacco and gambling. Consistent with these findings, we report zero treatment effects on the probability of smoking cigarettes, drinking alcohol or using Marijuana (Figure 10). At the same time, we do not find any effect on the probability of exercising. Accordingly, we do not find any effect on the self-reported number of days of bad physical health.

6 | CONCLUSIONS

The paper has examined the mental health effects of receipt of an unconditional cash transfer. Specifically, we ask whether receipt of the expanded CTC in the fall of 2021 in the US decreased the number of bad mental health days among parents. We use data from the BRFSS and exploit policy-induced variations in benefit generosity for adults with

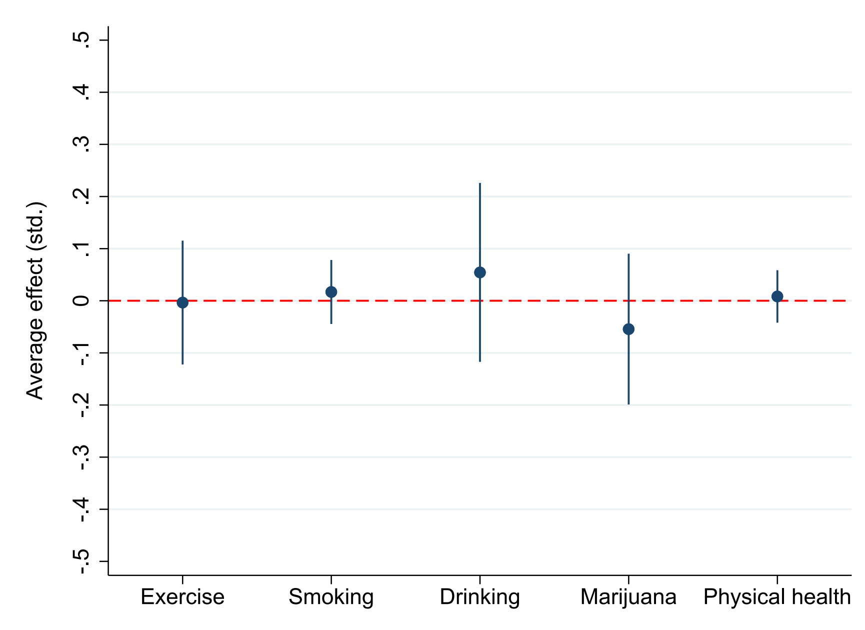


FIGURE 10 Healthy and unhealthy behavior and physical health. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Outcomes of interest correspond to dummies equal to one if (i) the individual participated in any physical activity over the last month ("Exercise"), (ii) smoked cigarettes some days ("Smoking") (iii) drank alcohol at least once in the last month ("Drinking"), and (iv) smoked Marijuana for at least 6 days in the past month ("Marijuana"). We also look at treatment effects on (v) the standardized number of bad physical health days reported in a month. The set of covariates included in our "Baseline" model (see text for details). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

children of different ages. Our primary contribution is to provide one of the first available estimates of the mental health effects of the receipt of a nationally-provided, unconditional cash transfer in the US, where this type of policy has traditionally not been in place. Accordingly, previous studies have examined the impact of other types of social protection and income transfer schemes (e.g., EITC, pension or health coverage).

We argue that our contribution is important for at least two reasons. First, estimates on the effects of unconditional cash transfers from developing and emerging countries cannot be easily generalized to a high-income context, due to differences in program functioning and other economic and societal characteristics. Second, estimates on other types of social protection schemes from the US cannot be used to draw conclusions on the mental health effects of an unconditional cash transfer, due to differences in target population as well as the absence of policy conditionalities. Our aim is understand whether unconditional cash transfers can be used to address the growing concerns over mental health conditions.

We find that a more generous CTC reduces the number of bad mental health days reported in the last month. Treatment effects materialize a few months after the first transfer, and disappear as soon as the benefit is withdrawn. Effects appear mostly on the intensive margin, by decreasing the number of bad mental health days reported by individuals who report at least one bad mental health day. We find larger effects among women, young adults as well as low-income and low-educated individuals. We interpret these findings in light of the fact that the receipt of the more generous benefit might have alleviated financial concerns among credit-constrained individuals, in line with the stated policy objectives. However, we note that credit constraints affected a relatively small share of the population at the time of the CTC expansion. This means that we can expect even larger effects on mental health from unconditional cash transfers implemented under more standard circumstances.

Our findings point to potential welfare gains from the rule-out of a nation-wide unconditional cash transfer program. This complements available evidence on the same policy experiment, which has documented sizable improvements in terms of consumption and a reduction in material deprivation and child poverty (Parolin et al., 2023, 2024), with no negative effects on employment (Ananat et al., 2021). While we believe that the observed effects are at the lower bound of those that would emerge if the policy was made permanent, we cannot directly test this hypothesis, as the policy was discontinued in late 2021. Future work should address the effects of the long-term receipt of an unconditional cash transfer.

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CONFLICT OF INTEREST STATEMENT

The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available in BRFSS at https://www.cdc.gov/brfss/annual_data/annual_data.htm. These data were derived from the following resources available in the public domain: BRFSS, https://www.cdc.gov/brfss/annual_data/annual_data.htm.

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ENDNOTES

[1] Note, however, that checking and salving account balances were rather high at the time of the CTC expansion for households at all income levels, potentially due to forced savings accumulated during the first phases of the COVID-19 pandemic as well as other stimulus packages (Wheat et al., 2023). This implies that credit constraints were rather low from an historical perspective, meaning that the mental health effects of the benefits might be even larger under normal circumstances.

[2] In particular, and as it will be discussed in details below, in the BRFSS we observe children's characteristics, including age, only for one child picked at random within the household. This can lead to measurement error of the treatment indicator when adopting the

identification strategy that compares households with children of different ages. We will test the extent to which measurement error can affect our results, by also relying on additional data sources, and provide evidence that this is unlikely to be a concern in our setting.

[3] The few notable exceptions of studies analyzing the mental health effects of unconditional cash transfers in the US are Jones and Marinescu (2022) who study the labor market effects of unconditional transfers from the Alaska permanent fund, Akee et al. (2010) who study the impact of casino lottery payments on children's educational attainments, and ongoing work by Gennetian et al. (2022) who report experimental evidence on the effects of a cash transfer on family time and children's investments. Instead, there is a growing literature on the mental health effects of unconditional cash transfers in developing and emerging countries (Ridley et al., 2020). However, it is not clear how much findings from these studies can be generalized to the US.

[4] There is also a small literature on the effects of the standard CTC (i.e., before its 2021 temporary expansion). This includes studies on the effects of CTC receipt on maternal health (Kang, 2022a), female labor supply (Kang, 2022b; Lippold, 2022) and children educational attainments (Kang, 2022c).

[5] More in details, Batra et al. (2023) use data from the Census's Household Pulse Survey. This is nationally representative, but it is Internet based and it was launched in 2020 to provide updated information during the pandemic. As such, it does not meet the standard quality requirements of other Census's surveys and its response rate is generally below 10%. Glasner et al. (2022) use the Understanding America Study, which is a nationally representative panel at the University of Southern California. The survey is conducted online and individuals receive a monetary compensation for filling the survey, thereby generating concerns of sample selection. Collyer et al. (2022) use two panel surveys conducted in New York. However, the specific geographical coverage as well as the specific sampling population of the two surveys (i.e., targeting low-income individuals and the children's caretakers) limit the external validity of the results, especially if treatment effects are heterogeneous across groups. Finally, Kovski et al. (2023) look at a sample of beneficiaries of the Supplemental Nutritional Assistance Program (SNAP) who use a mobile application to manage their benefits. This is not representative of SNAP beneficiaries (nor of the overall US population). Accordingly, sample characteristics are heavily unbalanced (e.g., 95% of individuals in the sample are female).

[6] Studies that have examined the mental health effects of the CTC expansion have either compared adults in households with and without children or used a continuous measure of benefit change estimated based on observable characteristics (e.g., number and age of child, number of adults, income). Our estimation strategy is instead similar to the one adopted by studies that have examined the impact of the 1993 expansion of the EITC (Adireksombat, 2010; Evans & Garthwaite, 2014; Hotz & Scholz, 2006). The only difference relates to the fact that, in the EITC papers, the variation in benefit levels comes from differences in the number of children, while here variation in benefit generosity comes from differences in the age of the child.

[7] In our empirical specification, we will also restrict the analysis to households with children of different ages, further reducing the risk of contamination with other forms of treatment.

[8] Estimates indicate that the total number of children in eligible tax units in the US is between 64 and 67 million (Parolin, Collyer, et al., 2021).

[9] States need to ask the core component questions without modification in wording, but can decide whether to administer the optional modules and also add state-specific additional modules.

[10] The BRFSS is not the only possible survey available to conduct this analysis. For instance, an alternative option could be to use the National Health Interview Survey (NHIS), which also reports information on mental health status. However, the NHIS is not representative at the state level (hence, we do not have information on the state of residence) and it has a much lower sample size than the BRFSS (i.e., in 2021, the adult population in the NHIS would consist of around 29,000 observations, compared to almost 430,000 observations in the BRFSS). Given that treatment effects can be quite small in magnitude in the present context, a large sample size is necessary to detect them with adequate precision.

[11] Some of these variables report statistically significant differences in the *t*-tests for equality of means, but these differences are very small in magnitude and their statistical significance is due to the large sample size.

[12] Note that the age composition of children in the household is very similar between the two surveys.

[13] Survey design differences between the CPS and the BRFSS include their different interview methods (i.e., only phone interviews in the BRFSS compared to a combination of in-person visits and phone interviews in the CPS), differences in the way in which the surveys are administered (e.g., BRFSS is run and managed at the state level) and differences in their response rates (i.e., higher in the CPS). Additionally, the values for the 2021 BRFSS refer to the 2021 wave of the survey, which spans from January 2021 to February 2022; while the CPS follows the standard calendar year and we use information for the entire 2021.

[14] Unlike other surveys, the BRFSS did not experience any drop in response rates during the COVID-19 pandemic, possibly due to its traditional reliance on telephone surveys (i.e., even before the pandemic).

[15] The only exception is the one registered at the onset of the pandemic, when the BRFSS reports a decrease in the number of poor mental health days. However, it is worth noting that the drop takes place only in March 2020. While we cannot investigate the exact reasons behind this trend, we note that the number of interviews conducted per month does not show any significant change. The 2020 BRFSS response rate is also in line with values of previous years. However, it is possible that some other survey characteristics (e.g., distribution of

respondents across states or sample composition) might have temporarily changed in March 2020. In any case, in the main analysis we use only data from the 2021 BRFSS wave.

[16] Since we do not have information on actual CTC receipt, we will define treatment based on CTC eligibility. This means that our estimates should be interpreted as intention-to-treat (ITT) effects. However, we do not believe that this represents a problem. First, benefit eligibility is exogenous while benefit receipt (which relies on the filing of taxes) is not. Second, data from administrative records shows that almost all eligible adults claimed the expanded CTC (coverage rate around 90%, see (Parolin, Ananat, et al., 2021)). This means that our treatment effects are unlikely to be driven by individuals who did not take up the CTC. Also, note that age of the child is available in the BRFSS only in bandwidths, which do not perfectly correspond to the age cutoff of the 2021 CTC reform. We will investigate in details the extent to which this measurement error can affect our estimates.

[17] By controlling for the number of children in the household, we look at the effect of having the same number of children, but of different ages. However, it is worth noting that in the BRFSS, we do not have access to information on the age of all children in the household, but only for one that is chosen at random.

[18] Note that, in the most complete specification, we will control, among others, also for household income and marital status. This means that any remaining variation in benefit level between treated and control units comes from the policy-induced change around the age of six.

[19] The size of the benefit change we exploit here is also comparable to the one generated by the 1993 EITC reform. The only difference is that, here, the variation in benefits is determined by the age of the child, while the 1993 EITC reform generated differences in benefits based on the number of children.

[20] It is also worth noting that the UAS has new waves in each month between March 2020 and June 2021, but after June 2021 the survey becomes much more discontinuous (i.e., the following waves are in September 2021 and February 2022). This makes the UAS inadequate to capture treatment effects that can vary even quickly with time, as we will show in the main results with the BRFSS.

[21] For ease of exposition, we look at the data from August 2020, corresponding to 14 survey waves before March 2021.

[22] Individuals may be more likely to assign higher importance to the most recent days/weeks within the previous month. We will investigate the sensitivity of the results to differences in the definition of the post-treatment period, but also refer to the event-study specification which does not rely on these assumptions.

[23] Ridley et al. (2020) report an average improvement in mental health outcomes equal to 0.067 SDs for cash transfer programs and of 0.138 for multifaceted anti-poverty programs. However, it is important to note that comparisons of treatment effects across studies are difficult to make, given that treatment intensity (i.e., the amount of cash transfer received) as well as treatment delivery method (e.g., monthly or lump-sum transfers) can also vary. In any case, we note that the cost of the median multi-faceted anti-poverty intervention in Ridley et al. (2020) is equal to $1188.5 (PPP), while the median cost of cash transfer programs that they analyze is equal to $787 (PPP). As a reminder, treatment here corresponds to a $600 yearly difference in the generosity of the CTC.

[24] This is obtained by looking only at estimates from quasi-experimental studies (e.g., examining the effects of mass layoffs), as suggested by Braghieri et al. (2022).

[25] As noted in Section 4, the UAS has only one wave during the period of the CTC expansion (wave 350, launched in September 2021). This makes it inadequate to measure treatment effects of the CTC expansion, if these effects vary quickly from 1 month to another, as shown in Figure 4. Additionally, even in the BRFSS, we do not observe any effect from the CTC expansion in September 2021 (i.e., the only month for which we have also UAS data). Nevertheless, we acknowledge that the results presented in Figures B2 and B3 using the UAS do not show any evidence of an announcement effect (unlike those presented in panel b of Figure 4 obtained with the BRFSS). However, we do not see this as a particularly problematic inconsistency, in light of the different methodological limitations affecting the UAS (see Section 4 for details).

[26] Here the coefficient of interest is the one of the interaction term between the dummy for 2021, the dummy for the months between August 15 and December 15, and the presence of children in the household.

[27] In order to make the two samples comparable in size, poor (rich) households are defined as those with an annual income below (above) $75,000. However, results are consistent when using other income thresholds.

[28] The main shortcomings of this approach relate to the fact that (i) it limits considerably sample size, and (ii) it skews the sample toward parents with younger children (i.e., these households are more likely to have only one child, compared to households that report older children).

[29] Indeed, the BRFSS does not report the exact age of the child, and the available age brackets (i.e., 0–4, 5–9, 10–14, and 15–17) do not perfectly overlap with the discontinuity in benefit levels generated by the policy change, which applies to children above or below the age of six. This means that some individuals that should be considered as treated using this identification approach, will instead be included in the control group (i.e., adults with children aged exactly 5).

[30] This includes unmarried individuals with income above $150,000 and married individuals with an income above $200,000 (see Figure 1).

[31] This is different from previous evidence on the 2021 CTC expansion, with some studies that had found larger mental health benefits for non-white individuals (Batra et al., 2023; Kovski et al., 2023).

[32] Larger treatment effects among low-income individuals are particularly noteworthy, as the discussion in Section 4 has shown that the first stage relationship might be slightly smaller for these individuals, at least in absolute terms (see Table 2).

[33] Here and in the rest of this section, we present only results from our DiD model where the post-treatment dummy takes the value of zero from the beginning of the 2021 survey period until August 14, 2021 and value of one from August 15 until December 15, 2021 ("Off-On" specification in the language introduced above). Additionally, all models will include the set of covariates corresponding to our "Baseline" model (see above in this section for details).

[34] This category includes (i) the Children's Health Insurance Program, (ii) Military related health care, (iii) the Indian Health Service, (iv) state sponsored health plans, and (v) other government programs.

[35] Figure B6 shows that the zero effects on overall health coverage materialize for all sub-groups, with the exception of low-income households, for whom there is positive treatment effect.

[36] This is consistent with the result presented above of positive treatment effects on health care coverage for low-income individuals. We interpret this as evidence of the fact that the larger CTC might have led credit-constrained individuals to increase spending in health insurance, also in line with previous studies on the 2021 CTC expansion.

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APPENDIX A

ADDITIONAL TABLES

TABLE A1 Descriptive statistics and *t*-tests of equality of means, for selected variables measured in the BRFSS and the CPS.

	BRFSS		CPS		*t*-test
	Mean	SD	Mean	SD	(*p*-value)
Age: 18–24	0.123	0.328	0.113	0.317	0.000
Age: 25–29	0.079	0.269	0.087	0.282	0.000
Age: 30–34	0.099	0.298	0.090	0.286	0.000
Age: 35–39	0.079	0.269	0.085	0.279	0.000
Age: 40–44	0.087	0.281	0.081	0.272	0.000
Age: 45–49	0.066	0.248	0.076	0.265	0.000
Age: 50–54	0.082	0.274	0.080	0.272	0.204
Age: 55–59	0.078	0.268	0.083	0.275	0.000
Age: 60–64	0.087	0.282	0.083	0.276	0.000
Age: 65–69	0.069	0.253	0.072	0.259	0.000
Age: 70–74	0.062	0.240	0.060	0.238	0.057
Age: 75–79	0.043	0.203	0.041	0.197	0.000
Age: 80 and above	0.048	0.214	0.049	0.216	0.180
Sex: Men	0.487	0.500	0.483	0.500	0.015
Education: At least some college	0.605	0.489	0.616	0.486	0.000
Race: White	0.724	0.447	0.774	0.418	0.000
Income: <50	0.440	0.496	0.369	0.482	0.000
Child in household: At least one	0.358	0.479	0.483	0.500	0.000
Age of child: 0–4	0.244	0.430	0.251	0.434	0.485
Age of child: 5–9	0.251	0.434	0.247	0.431	0.269
Age of child: 10–14	0.276	0.447	0.277	0.448	0.463
Age of child: 15–17	0.229	0.420	0.225	0.417	0.765
Observations	438,693		1,002,272		

Note: The table presents means and SDs for selected individual and household characteristics, measured in the 2021 CPS and the 2021 wave of the BRFSS. In order to compare the two surveys with respect to the age of the child within the household, we selected in the CPS one child at random for each household that reported the presence of at least one child. The table also reports *p*-values of the *t*-tests of equality of means. Sampling weights are used to derive the estimates.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; CPS, Current Population Survey; SD, standard deviation.

TABLE A2 Descriptive statistics and *t*-tests of equality of means, for selected variables measured in the BRFSS, before and after the 2021 CTC expansion.

	Before CTC expansion		After CTC expansion		
	Mean	**SD**	**Mean**	**SD**	***t*-test (*p*-value)**
Men	0.485	0.500	0.491	0.500	0.139
Age: 18–29	0.200	0.400	0.202	0.401	0.677
Age: 30–39	0.174	0.379	0.180	0.384	0.030
Age: 40–49	0.151	0.358	0.153	0.360	0.491
Age: 50–59	0.161	0.367	0.157	0.364	0.223
Age: 60–69	0.158	0.365	0.156	0.362	0.275
Age: 70 and above	0.156	0.363	0.152	0.359	0.133
Income: <50k	0.438	0.496	0.441	0.496	0.603
Race: White	0.726	0.446	0.721	0.448	0.245
Ethnicity: Hispanic	0.164	0.370	0.184	0.388	0.000
Marital status: Married	0.504	0.500	0.505	0.500	0.827
Marital status: Divorced or separated	0.126	0.332	0.126	0.332	0.952
Marital status: Widowed	0.070	0.255	0.068	0.252	0.407
Marital status: Never married	0.249	0.432	0.246	0.431	0.461
Marital status: Unmarried couple	0.051	0.219	0.054	0.226	0.058
Presence of children in household	0.352	0.478	0.343	0.475	0.008
Number of children	0.897	1.855	0.667	1.125	0.020
Age of child: 0–4	0.247	0.431	0.243	0.429	0.718
Age of child: 5–9	0.251	0.433	0.253	0.435	0.855
Age of child: 10–14	0.275	0.446	0.271	0.445	0.704
Age of child: 15–17	0.227	0.419	0.233	0.423	0.587
Sex of child: Male	0.515	0.500	0.516	0.500	0.952
Relationship to child: Parent	0.772	0.419	0.776	0.417	0.709
Observations	242,821		152,521		

Note: The table presents means and SDs for selected individual and household characteristics, measured in the 2021 wave of the BRFSS before or after the first CTC monthly payment (on July 15, 2021). The table also reports *p*-values of the *t*-tests of equality of means. Sampling weights are used to derive the estimates.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; CTC, child tax credit; SD, standard deviation.

TABLE A3 Descriptive statistics and *t*-tests of equality of means, for selected variables for different populations based on treatment status.

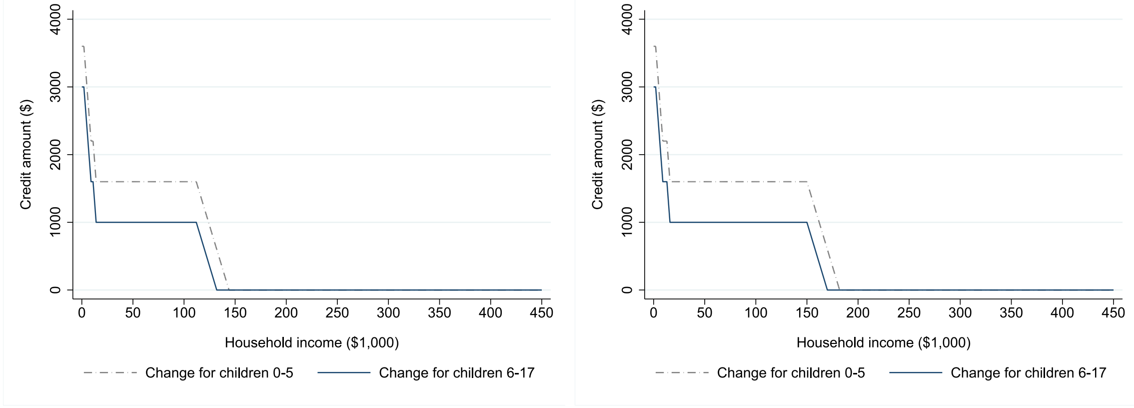
	Households with children aged 5 or above (1)		Households with children aged <5 (2)		*t*-test (*p*-value, (1)–(2))
	Mean	SD	Mean	SD	
Men	0.410	0.492	0.422	0.494	0.384
Age: 18–29	0.058	0.234	0.313	0.464	0.000
Age: 30–39	0.327	0.469	0.538	0.499	0.000
Age: 40–49	0.422	0.494	0.129	0.335	0.000
Age: 50–59	0.172	0.378	0.018	0.132	0.000
Age: 60–69	0.018	0.132	0.001	0.037	0.000
Age: 70 and above	0.002	0.046	0.000	0.019	0.004
Income: <50k	0.358	0.480	0.389	0.488	0.033
Race: White	0.719	0.449	0.697	0.460	0.108
Ethnicity: Hispanic	0.259	0.438	0.272	0.445	0.349
Marital status: Married	0.695	0.460	0.686	0.464	0.476
Marital status: Divorced or separated	0.124	0.330	0.062	0.241	0.000
Marital status: Widowed	0.013	0.113	0.004	0.067	0.002
Marital status: Never married	0.106	0.307	0.158	0.365	0.000
Marital status: Unmarried couple	0.062	0.241	0.089	0.285	0.000
Number of children	2.004	1.092	1.943	1.026	0.027
Age of child: 0–4	0.000	0.000	1.000	0.000	0.000
Age of child: 5–9	0.358	0.479	0.000	0.000	0.000
Age of child: 10–14	0.374	0.484	0.000	0.000	0.000
Age of child: 15–17	0.269	0.443	0.000	0.000	0.000
Sex of child: Male	0.512	0.500	0.511	0.500	0.942
At least 1 day not good mental health (0/1)	0.424	0.494	0.466	0.499	0.003
Number of days of not good mental health	4.538	8.352	4.715	8.155	0.437
At least 1 day not good physical health (0/1)	0.284	0.451	0.293	0.455	0.493
Number of days of not good physical health	2.561	6.539	2.187	5.686	0.017
Observations	29,749		8518		

Note: The table presents means and SDs for selected individual and household characteristics, measured in the 2021 wave of the BRFSS. These descriptive statistics are presented separately for the groups that are used as treatment and control groups in the main analysis. These are (i) adults with children aged 5 or above (column 1), and (ii) adults with children aged <5 (column 2). The table also reports *p*-values of the *t*-tests of equality of means between treatment and control groups. Sampling weights are used to derive the estimates.

Abbreviations: BRFSS, Behavioral Risk Factor Surveillance System; SD, standard deviation.

APPENDIX B

ADDITIONAL FIGURES



(a) Unmarried (b) Married

FIGURE B1 Change in benefit levels for married and unmarried individuals due to the 2021 CTC expansion, by age of the child. The figure reports the change in benefit levels that resulted from the 2021 temporary CTC expansion, for adults with children above or below the age of six. The information is reported separately for unmarried and married individuals (panels a and b, respectively). The figure is based on information provided in CSR (2021). CTC, child tax credit. [Colour figure can be viewed at wileyonlinelibrary.com]

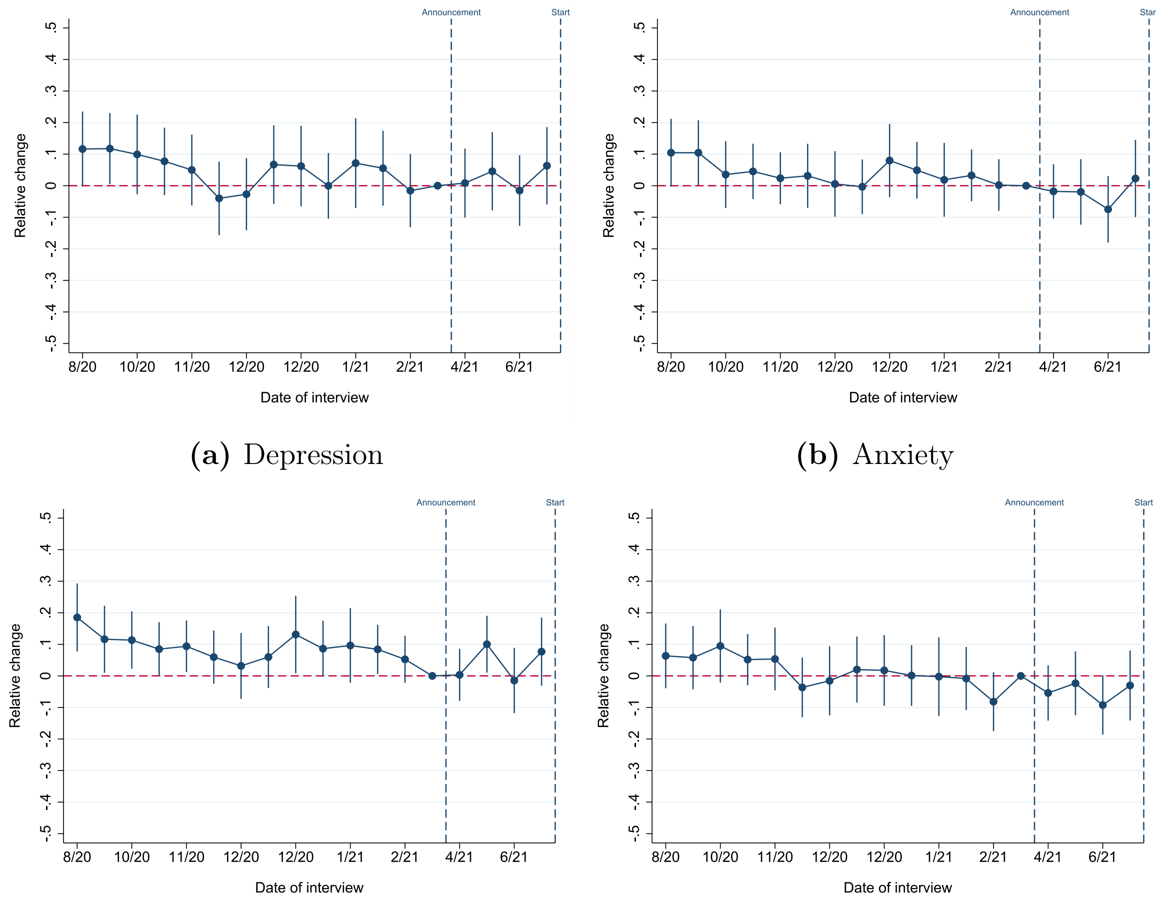
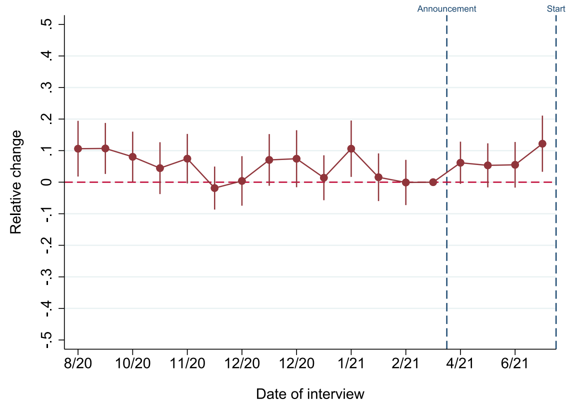
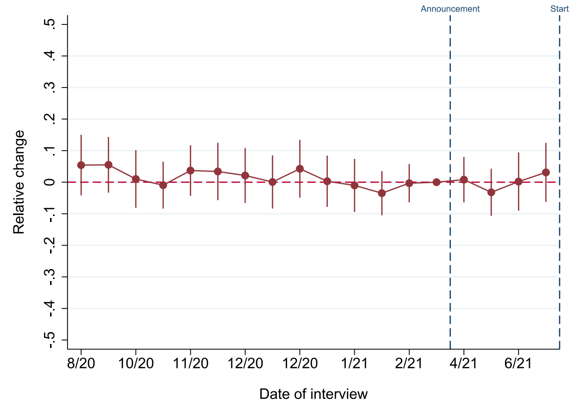


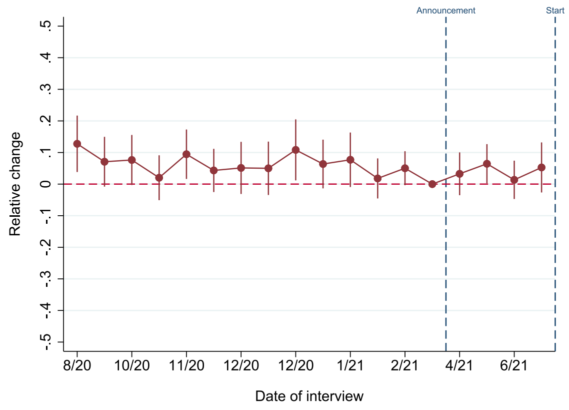
FIGURE B2 Trends in mental health using the UAS, repeated cross-sections. The figure reports point estimates and 90% confidence intervals for tests of parallel trends, using data from the UAS. The analysis is conducted using different UAS survey waves from August 2020 to June 2021. The treatment group corresponds to individuals with children aged <6, while the control group is constituted by individuals with children aged 6 and above. The outcome variables correspond to dummy indicators equal to one if the respondent showed always, often or generally symptoms of depression (panel a), anxiety (panel b), worry (panel c) or little interest (panel d). Observations are used as repeated cross sections. Standard errors are clustered at the state level. UAS, Understanding America Study. [Colour figure can be viewed at wileyonlinelibrary.com]



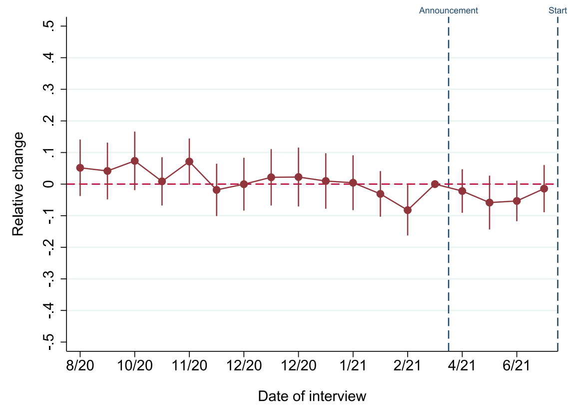
(a) Depression



(b) Anxiety



(c) Worried



(d) Little interest

FIGURE B3 Trends in mental health using the UAS, with individual fixed effects. The figure reports point estimates and 90% confidence intervals for tests of parallel trends, using data from the UAS. The analysis is conducted using different UAS survey waves from August 2020 to June 2021. The treatment group corresponds to individuals with children aged <6, while the control group is constituted by individuals with children aged 6 and above. Dummy indicators equal to one if the respondent showed always, often or generally symptoms of depression (panel a), anxiety (panel b), worry (panel c) or little interest (panel d). All specifications include individual fixed effects. Standard errors are clustered at the household level. UAS, Understanding America Study. [Colour figure can be viewed at wileyonlinelibrary.com]

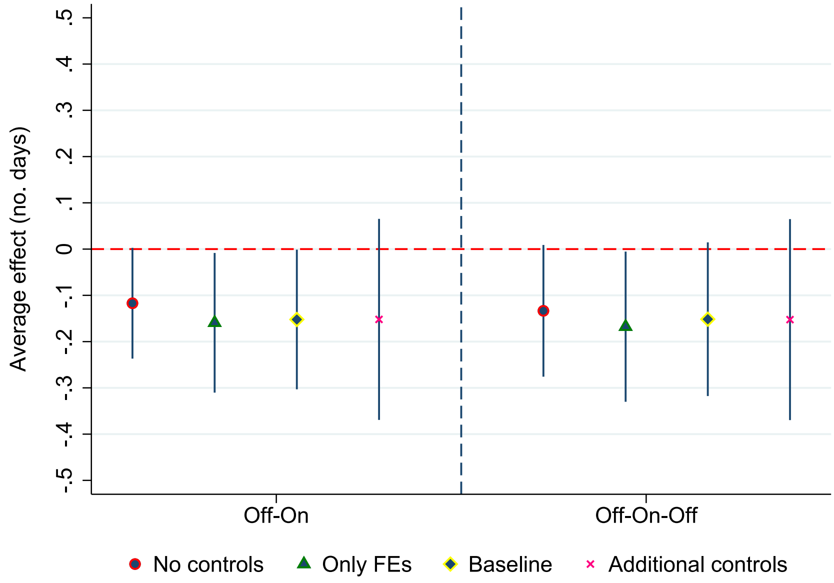


FIGURE B4 Negative binomial regression model for the number of bad mental health days. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the results presented in Figure 4, here the outcome of interest is not normalized to have mean of zero and standard deviation of one in the pre-treatment period, while we account for overdispersion by using a negative binomial regression model. We present results from specifications using two definitions of the post-treatment variable ("Off-On" and "Off-On-Off," see text for details) as well as for specifications that vary the set of covariates included (see text for details). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

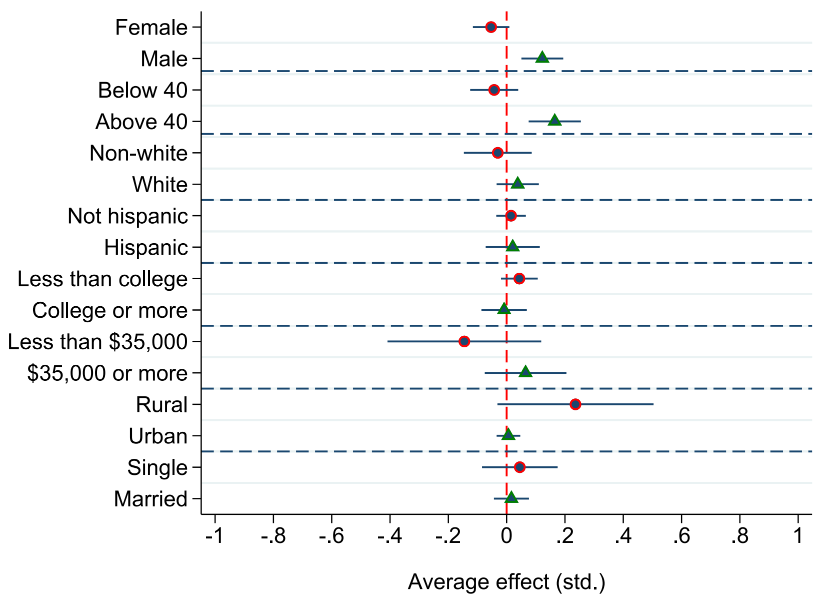


FIGURE B5 Heterogeneous results for employment. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Results are presented separately for different groups in the population. The outcome of interest is always a dummy equal to one if the individual is employed, and zero otherwise. All results are obtained using the covariates included in our "Baseline" model, and with the post-treatment dummy defined as in our "Off-On" specification (see text for details). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

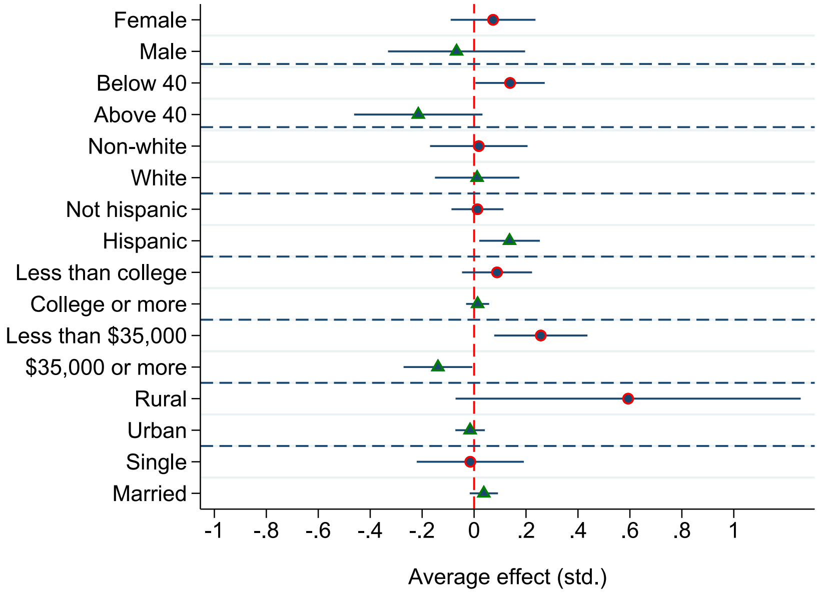


FIGURE B6 Heterogeneous results for health care coverage. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Results are presented separately for different groups in the population. The outcome of interest is always a dummy equal to one if the individual has health care coverage (from any source), and zero otherwise. All results are obtained using the covariates included in our "Baseline" model, and with the post-treatment dummy defined as in our "Off-On" specification (see text for details). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

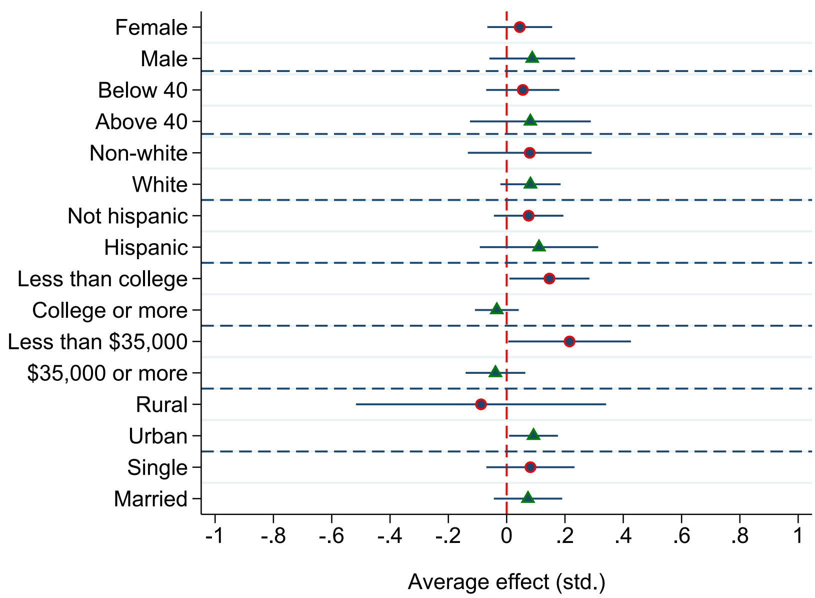
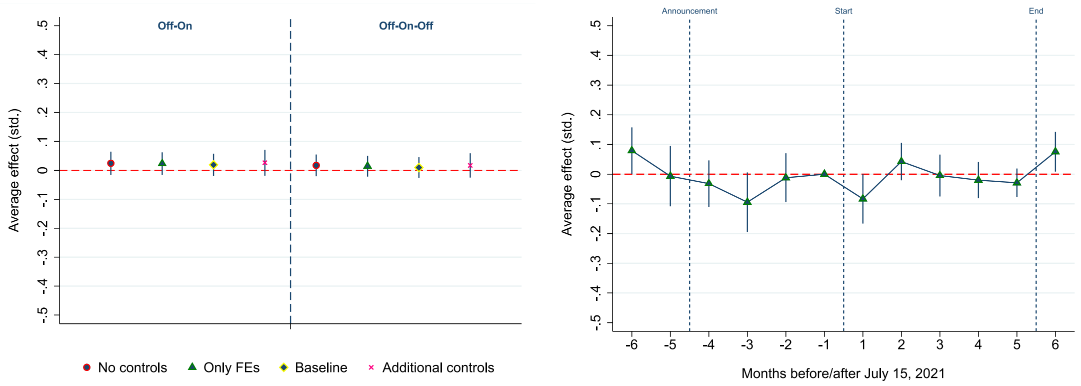


FIGURE B7 Heterogeneous results for health care use. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Results are presented separately for different groups in the population. The outcome of interest is always a dummy equal to one if the individual has a doctor that considers his/her personal health care provider, and zero otherwise. All results are obtained using the covariates included in our "Baseline" model, and with the post-treatment dummy defined as in our "Off-On" specification (see text for details). Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]

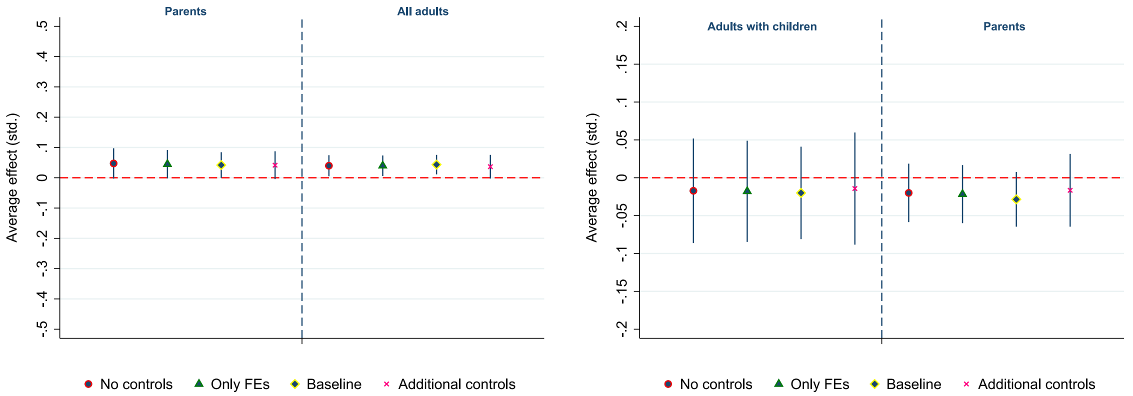
APPENDIX C

ROBUSTNESS TESTS



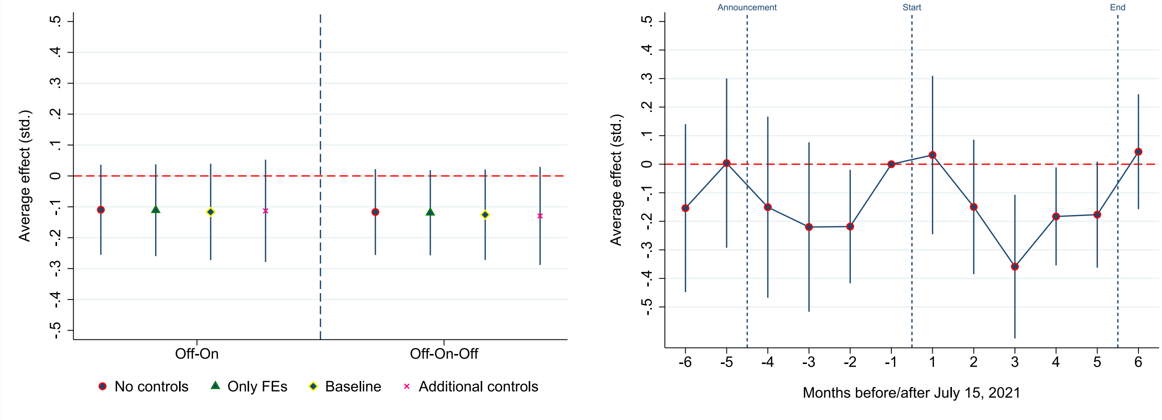
(a) DiD (b) Event study

FIGURE C1 Effects of the receipt of the expanded CTC on the number of bad mental health days, using an alternative identification strategy. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, here we compare parents (treated) with adults without children (control). In panel (a), we additionally report results using (i) two different definitions of the post-treatment variable ("Off-On" and "Off-On-Off," see text for details) and (ii) different specifications that vary according to the types of covariates included in the analysis (see text for details). In panel (b), we use the controls included in the "Baseline" specification to derive event-study estimates. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit. [Colour figure can be viewed at wileyonlinelibrary.com]



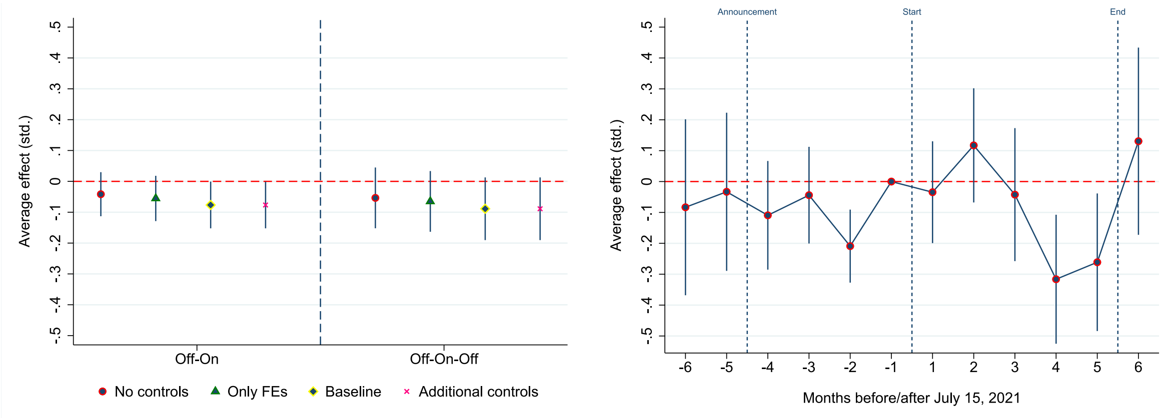
(a) DiD on 2019 (b) DDD (2021 vs 2019)

FIGURE C2 Comparing 2019 with 2021. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, here we compare parents or adults with children (treated) with adults without children (control). The outcome of interest is always the number of bad mental health days reported over the previous month, normalized to have mean of zero and standard deviation of one in the pre-treatment period. In panel (a), the analysis is conducted for 2019 (placebo policy change), separately for parents and all adults with children (and using different sets of covariates, see text for details). In panel (b), we present the estimates of a triple difference approach, using data for 2019 as a pre-treatment year and data for 2021 as the treatment year. Results are presented separately for specifications that include all adults with children and only parents and using different sets of covariates. Standard errors are always clustered at the state level. [Colour figure can be viewed at wileyonlinelibrary.com]



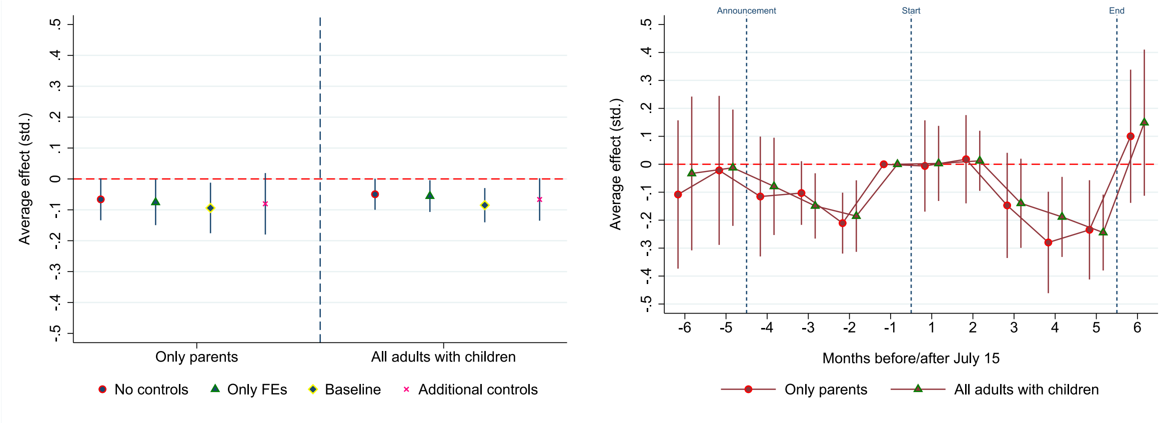
(a) DiD (b) Event study

FIGURE C3 Excluding children above 10. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, we exclude children aged 10–17 from the control group. In panel (a), we report simple DiD estimates that vary depending on the definition of the post-treatment period ("Off-On" and "Off-On-Off," see text for details) as well as for the set of covariates included (see text for details). The event-study results in panel (b) use instead the set of covariates corresponding to our "Baseline" model. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]



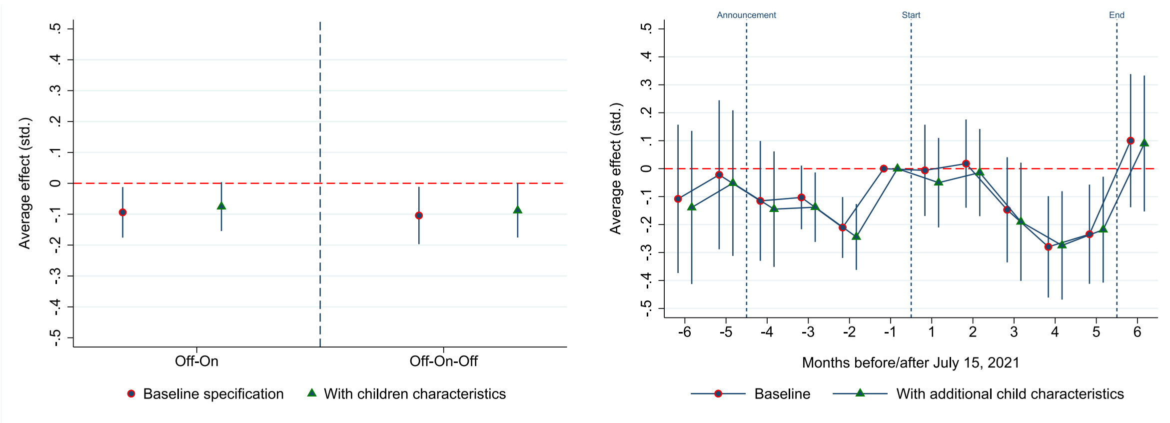
(a) DiD (b) Event study

FIGURE C4 Excluding children 5–9. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, we exclude children aged 5–9 from the control group. In panel (a), we report simple DiD estimates that vary depending on the definition of the post-treatment period ("Off-On" and "Off-On-Off," see text for details) as well as for the set of covariates included (see text for details). The event-study results in panel (b) use instead the set of covariates corresponding to our "Baseline" model. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]



(a) DiD (b) Event study

FIGURE C5 Parental and non-parental adults. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, we look separately at the samples of parents and all adults with children. In panel (a), we report simple DiD estimates that vary depending on whether we include only parents or all adults with children, as well as for the set of covariates included (see text for details). The event-study results in panel (b) use instead the set of covariates corresponding to our "Baseline" model, while again comparing all adults with children with only parents. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]



(a) DiD (b) Event study

FIGURE C6 Additional controls for child's characteristics. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, we include additional controls for child's sex, race and ethnicity. In panel (a), we report simple DiD estimates that vary depending on the definition of the post-treatment period ("Off-On" and "Off-On-Off," see text for details) as well as for the set of covariates included (see text for details). The event-study results in panel (b) compares results obtained with our "Baseline" model and this augmented specification. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to March 11, 2021), the time of the policy introduction ("Start", corresponding to July 15, 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to December 15, 2021) when the last CTC monthly payment was made. Standard errors are always clustered at the state level. CTC, child tax credit; DiD, difference-in-differences. [Colour figure can be viewed at wileyonlinelibrary.com]

FIGURE C7 Placebo analysis using very rich households. The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in Equation (1). Compared to the baseline results presented in the main text, we restrict the attention to individuals for whom the CTC benefit amount was not affected by the 2021 reform. This includes unmarried individuals with an income above \$150,000 and married individuals with an income above \$200,000. Standard errors are always clustered at the state level. CTC, child tax credit. [Colour figure can be viewed at wileyonlinelibrary.com]