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DISCUSSION PAPER SERIES

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Clemente Pignatti
Bocconi University

Zachary Parolin
Bocconi University and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Effects of an Unconditional Cash Transfer on Mental Health in the United States

Mental health conditions have worsened in many countries in recent decades. 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 (US). Using data from the Behavioral Risk Factor Surveillance System, the largest health-related survey in the US, 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 reported bad mental health days. The effect materializes after the third monthly payment and disappears when the benefits are withdrawn.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.

JEL Classification: H51, I18, J18

Keywords: child tax credit, mental health, public policy

Corresponding author:
Zachary Parolin
Bocconi University
Via Roberto Sarfatti, 25
20100 Milano MI
Italy
E-mail: zachary.parolin@unibocconi.it

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 availability 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 status by alleviating financial constraints.

This study investigates how the provision of monthly and unconditional cash transfers affects mental health in the US. Specifically, we study how the temporary expansion of the Child Tax Credit (CTC) in 2021 affected the existence and intensity of mental health challenges among families with children. Even before this expansion, the CTC constituted the largest tax credit in the US, with an annual federal spending above $100 billion. 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 American Rescue Plan Act (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 $2,000 per child to $3,600 per child below age 6 and $3,000 per child between ages 6 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.

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.[1] However, the expanded CTC was in place only for the 2021 tax year and the US Congress did not renew the program for subsequent years. Monthly transfers ended in December 2021, and households claimed the remaining benefit amount when filing taxes in spring 2022. For the 2022 tax year, the CTC reverted back to its pre-ARPA form.

Though the effects of unconditional cash transfers on mental health have been studied extensively in low- and middle-income countries, they have received less focus in high-income countries, and the US in particular (Ridley et al., 2020). This is in part due to the longstanding absence of unconditional cash support in the US, where access to income transfers (including the pre-reform CTC) is often conditional on earnings. The 2021 expansion of the CTC thus provides a unique opportunity to study the consequences of the introduction of a nationally-available, unconditional cash benefit on mental health in a high-income country context.[2] This is interesting because differences in program characteristics (e.g. target population, size of the benefit), economic and social conditions (e.g. poverty rates, family ties) as well as other cultural traits (e.g. intra-household bargaining, stigma around mental illness) might lead to very different mental health effects of cash transfer programs between high- and low- and middle-income countries.

We produce causal estimates of the effects of the 2021 CTC expansion using 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 preferred specification adopts 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. 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 reported number of bad mental health days. In our preferred specification, an additional $300 over a six month window decreases the number of bad mental health days by 0.094 of a standard

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

[2] We acknowledge that some US-based randomized control trials, such as Baby's First Years, have evaluated the effects of cash payments on mental health outcomes, though generally in much smaller samples, in only a few cities, and often with smaller treatment intensities than the 2021 CTC.

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). Our treatment effects materialize a few months after the first transfer is received, and disappears 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 individuals who report at least some days of poor mental health in the previous month.

We rule out that the positive effects on 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 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 cash transfer affects mental health in part through the alleviation of financial worries.

This interpretation is also consistent with the results that emerge from our heterogeneous analysis. In particular, 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 in the previous year). 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.

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.

The paper contributes to three different debates in the literature. First, the paper 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 across different places and times, the direction of the causal relationship as well as its magnitude remain largely unknown (Ridley et al., 2020). Research has shown that mental health deteriorates following negative income shocks. Examples include studies that have examined the mental health effects of plant closure in Austria (Kuhn et al., 2009), the effects of a reduction in income due to extreme weather events among Indonesian farmers (Christian et al., 2019), and the effects of a fall in wages among US employees exposed to trade competition from China (Pierce and Schott, 2020).[3] While these papers exploit unusual shocks to income or living conditions, we contribute to the debate by exploring the effects of a well-defined and policy-induced change in income that affected millions of beneficiaries at the same time and can be easily replicated in other times and contexts.

For these reasons, the paper also contributes to the literature that examines the mental health effects of participation in cash-transfers or anti-poverty programs. These studies have generally found positive treatment effects, although point estimates vary quite substantially (Ridley et al., 2020). However, most of these estimates come from low- or middle-income countries, where these programs are more likely to be implemented. In the US, studies have examined the mental health effects of conditional cash transfers such as the Earned Income Tax Credit (EITC) and of other forms of social protection such as health or pension coverage (Aizer et al., 2016; Boyd-Swan et al., 2016; Collin et al., 2021; Evans and Garthwaite, 2014; Finkelstein et al., 2012; Jones et al., 2022).[4] However, effects of an unconditional cash transfer might be different compared to those of other types of programs (Haushofer and Shapiro, 2016). This refers, first, to differences in the target population, as unconditional cash transfers are more likely to reach low-income families. Additionally,

[3] A related literature examines the mental health effects of non-economic types of shocks, including the *in utero* exposure to maternal stress due to family ruptures (Persson and Rossin-Slater, 2018), access to social media (Braghieri et al., 2022) and moving to an affluent neighborhood (Ludwig et al., 2013).

[4] The few notable exceptions 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.

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, intra-household conflict on benefit use).[5]

Finally, the paper contributes to the recent literature that has examined the effects of the 2021 CTC expansion.[6] Previous research has found an increase in household spending (Parolin et al., 2022) and a reduction in child poverty and food insecurity (Parolin et al., 2021a) 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, or that suffered from sample-selection and attrition rates (e.g. online surveys).[7] Second, we adopt a new identification strategy that allows to isolate the causal effect of a change in benefit levels among eligible households. Previous contributions relied instead on identification strategies that required stronger assumptions (e.g. comparing parents and non parents) or lacked a clear source of treatment variation.[8] We argue that

[5]A related point refers to the fact that it is difficult for studies on other types of social protection to uncouple the income effect from the effect of any behavioral change due to program conditionalities.

[6]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).

[7]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 instead the Understanding America Study, which is a nationally representative panel at the University of Southern California. However, 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).

[8]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 and Garthwaite, 2014; Hotz and Scholz, 2006). The only difference

differences in data sources and identification are key for explaining our results.

Overall, our contribution is to study the relationship between income changes and mental health status in the context of one of the most salient policy changes in the US in recent years. We do so by exploiting policy-induced variations in benefit levels and relying on high-quality data from a large and nationally representative survey. The findings reveal the potential role that social protection can play in addressing the worsening of mental health conditions around the word, especially for low-income households.

2 Policy

The 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.[9]

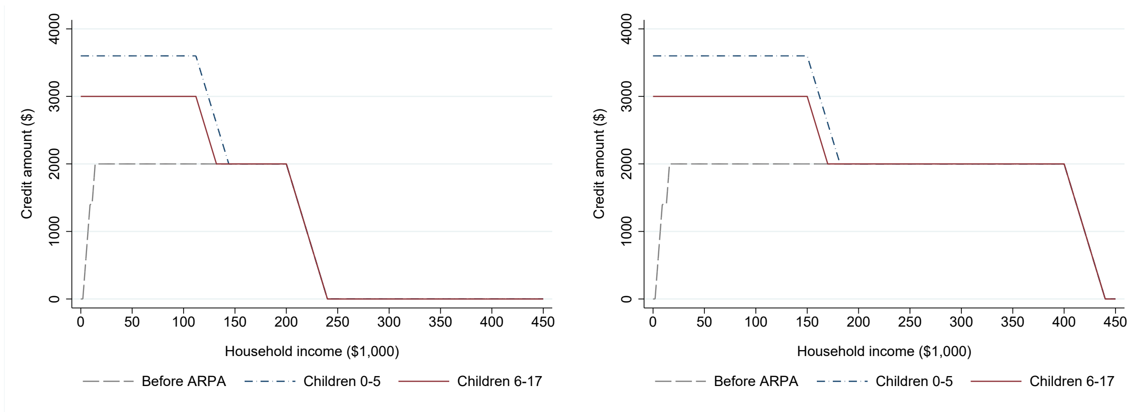
Before the ARPA, adults with children could receive a maximum of $2,000 per child per year from the CTC. However, many households did not receive the full benefit, or were entirely ineligible to the tax credit. This is because tax filers with an annual income below $2,500 did not qualify for the CTC. The benefit amount then started increasing at a rate of 15% of income above $2,500, until reaching the maximum of $1,400. This was the maximum refundable amount of the CTC, which could be complemented with a non-refundable part of the tax credit, until reaching $2,000. It is estimated that one in three children did not receive the full amount of CTC before the 2021 reform. 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 and Collyer, 2019).

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.

relates to the fact that, in these papers, the variation in benefit levels comes from differences in the number of children, rather than differences in the age of the child.

[9]In our preferred 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.

Figure 1: The CTC as a function of household income and marital status, before and after the 2021 ARPA



(a) Unmarried **(b)** Married

Notes: 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)

As detailed previously, ARPA increased the maximum benefit levels, made the benefit fully refundable and no longer conditional on earnings, and paid half the benefits in monthly installments starting in July 2021.

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 (panel A) and married (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 monthly payments are estimated to have reached 61 million households, with an almost full coverage of the eligible population (ranging between 64 and 67 million individuals, according to Parolin et al. (2021a)). 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 CTC expansion was not extended and the program reverted back to its pre-2021 period as of January 2022. 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 the 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.

3 Data

We use data from the BRFSS to estimate the effects of CTC receipt on 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.[10] The BRFSS is the largest health-related survey worldwide. The BRFSS 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 50 US states from the 2000s onward.

BRFSS is an ideal survey for the purpose of this study, as it 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.

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 3 January 2021 and 28 February 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

[10]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.

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 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: Less than 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 one 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 one 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	438693	

Notes: The table presents mean and SD 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.

Appendix Table A1 reports selected descriptive statistics from the 2021 BRFSS and the 2021 Current Population Survey (CPS). The two samples are very comparable with respect to their age composition, sex and 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). 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.[12] While these differences should be kept in mind, we do not see them as constituting direct threats to our analysis.

The BRFSS response rate in 2021 was 44%, in line with values reported in previous waves.[13] This is slightly lower than the response rate of the National Health and Nutri-

[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] 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 February 2021 to February 2022; while the CPS follows the standard calendar year.

[13] Unlike other surveys, the BRFSS did not experience any drop in response rates during the COVID-19

tion 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, Appendix 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%. Even in these cases, differences are generally small.

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 and Spitzer, 2002). Our measure of mental health from the BRFSS has also been used in previous research (Evans and Garthwaite, 2014), where it delivered results consistent with those obtained when measuring mental health using biomakers.

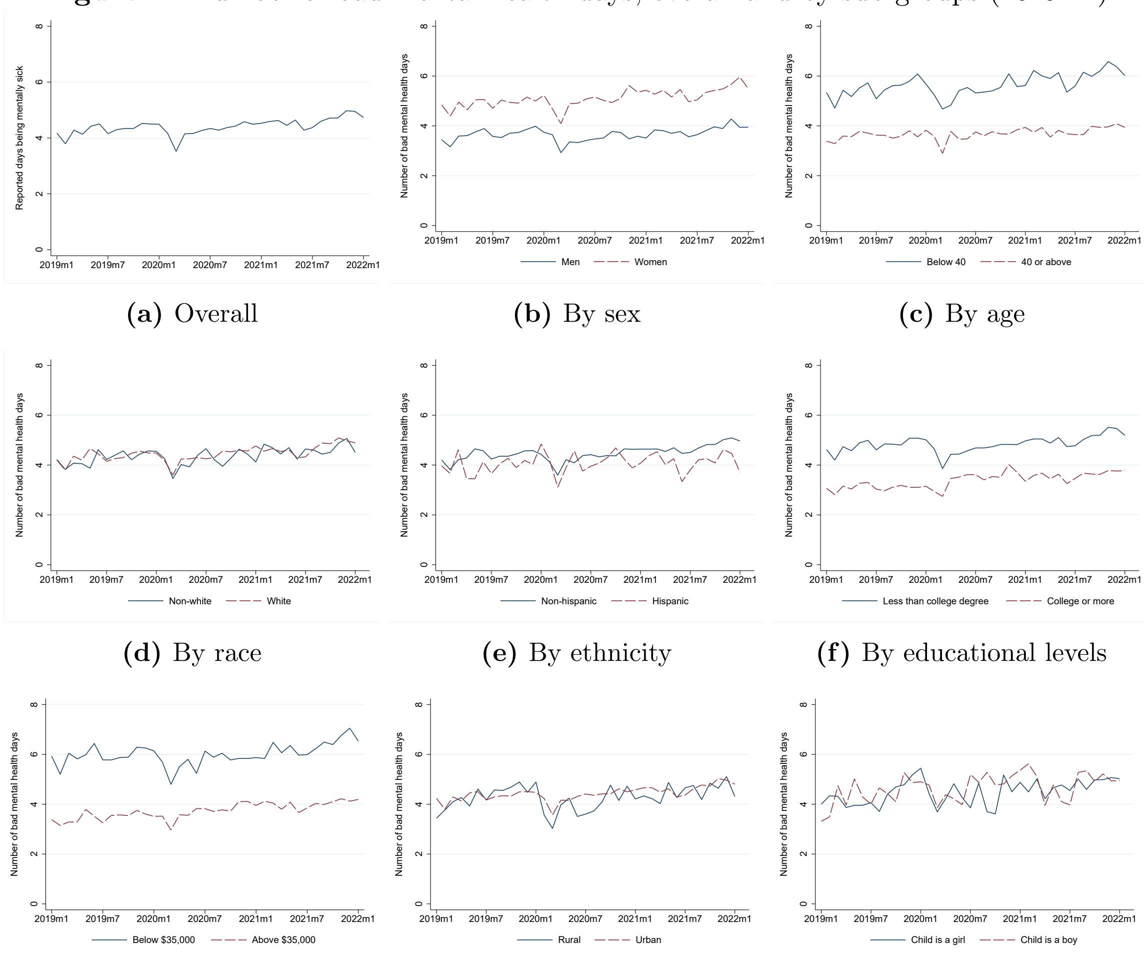
Figure 2 plots the evolution of the number of days individuals report being mentally unhealthy between January 2019 and January 2022, for the overall sample (panel A) as well as by splitting the sample according to a number of individual- and household-level characteristics (panels B to I). While the series shows some month-to-month variation, the number of mentally sick days has clearly followed an upward trend during the period of the analysis.[14] As a result, the number of mentally sick 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 (panel B), for young adults compared to older individuals (panel C) and for low-educated and low-income individuals (panels F and G). Differences in these groups have not evolved notably since 2019. At the same time, the

crisis, possibly due to its traditional reliance on telephone surveys (i.e. even before the pandemic).

[14]The only problematic variation 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 large drop takes place only in March 2020. While we cannot investigate the exact reasons behind this drop, 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.

number of bad mental health days was very similar before the pandemic among individuals of different race (panel D), ethnicity (panel E), living in urban and rural areas (panel H) and with children of different sex (panel I).

Figure 2: Number of bad mental health days, overall and by sub-groups (2019-22)



(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 sex of the child

Notes: The figure reports the average number of bad mental health days reported by survey respondent in the previous month (2019-22). Panel A presents the results for the overall sample, while panels from B to I present different results by sub-groups in the population. Sampling weights are used to derive the estimates.

4 Estimation strategy

The purpose of the paper is to identify and estimate the effects of the 2021 CTC expansion on mental health outcomes, and to investigate its possible transmission mechanisms. To do so, we adopt a DiD approach using two different definitions of treatment. In our first approach, we define a treatment variable equal to one if the respondent reports the presence of a child aged 0-17 in the households. This specification leverages the fact that households with children received monthly transfers between July and December 2021, while households without children did not receive the benefits. In our second approach, 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:

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

where Y_{isct} is the outcome of interest for individual i, living in state s in 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-level covariates. $Time_t$ represents a series of event time dummies, either in the form of a post-treatment dummy or in the form of monthly dummies from January 2021 to January 2022. c_s is a set of state dummies, while t_c are calendar month dummies.

CTC_i represents the treatment dummy. When using the first identification strategy, this dummy will take the value of one if the adult reports the presence of a child in the household and zero otherwise. Treatment therefore corresponds to eligibility for CTC benefit receipt, with the actual benefit amount varying based on household characteristics (e.g. income levels) as well as on the number and age of the children. In the second specification, this dummy takes the value of one if the respondent reports the presence of a child below the age of 5 in the household (and 0 for adults reporting a child between 5 and 17 years old). In this case, treatment corresponds to an additional increase in benefit levels based on the child's age. The exact amount of the benefit change will depend on household income (see Figure 1), but for most households this will correspond to an additional \$600 per year. Standard errors are clustered at the state level in all specifications.

Appendix Figure B1 shows in details how the 2021 CTC expansion changed the 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 six to 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 \$4,000 (or 7.27% of annual income irrespective of the child's age). After the reform, the CTC amount increased to \$6,000 if the two children were aged six or above (equal to 10.91% of total household income) and to \$7,200 if the children were below the age of six (corresponding to 13.09% of household income). The increase in CTC benefit due to the ARPA was thus equal to 3.64% of household income if the children were aged six or above, and 5.82% of household income if children were younger than six.

Appendix Table A3 reports selected descriptive statistics for our treatment and control groups defined using the two different identification strategies (i.e. separately for individuals with and without children and for those 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. As expected, we note that virtually all variables are statistically different when comparing adults with and without children. In particular, we find large difference in sex, age, race, ethnicity, income and marital status. When we instead compare adults with children below or above the age of 5, many of these differences are no longer statistically significant. Even when we still report statistically significant differences (e.g. for age groups), they are now substantially smaller in magnitude.

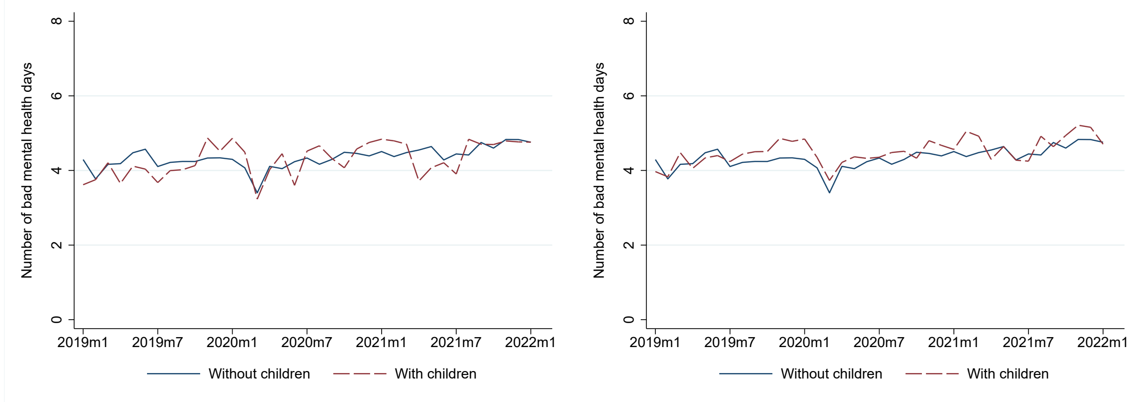
We will report baseline results for the effect of CTC receipt on mental health based on these two identification strategies. However, we note that our preferred specification is our comparison of adults with children of different ages, and we will present DiD results comparing adults with and without children only for comparison purposes. The preference for this identification approach is motivated by two main reasons.

First, there is a risk that adults with and without children would have experienced differential trends in mental health status in the fall of 2021 absent the policy change. This could have happened due to standard variations in mental health status over the year (e.g. increased stress due to school reopening), which could have been exacerbated in the fall of 2021. Indeed, COVID-19 vaccines were not yet available for children of most ages and school closures were still common at that point in time.[15]

We find some empirical evidence in line with this hypothesis. Figure 3 plots the evolution of the number of bad mental health days for adults with and without children. We

[15]Some of these concerns could apply also when comparing adults with children of different ages (e.g. COVID-19 vaccines became available for children above the age of 11 in the fall of 2021), potentially invalidating also our second identification approach. However, we believe that the hypotheses behind the parallel trend assumption are much more likely to hold in this case, and we conduct a series of tests to confirm this hypothesis.

Figure 3: Number of bad mental health days, by presence of children in the household (2019-22)



(a) All adults with children

(b) Parents only

Notes: The figure plots the average number of bad mental health days reported by survey respondents in the previous month (2019-22). Panel A reports this information separately for adults with and without children, independently from the relationship with the child. Panel B presents the same differentiation, but focusing only on parents among adults with children. Sampling weights are used to obtain the estimates.

find that households with children report higher variability in the number bad mental health days, with a deterioration of mental health in the fall of both 2019 and 2020. This is true when looking at all adults with children (panel A) and restricting the attention to parents (panel B). The increase in the number of bad mental health days is instead substantially smaller in the fall of 2021. This provides suggestive evidence for the fact that the CTC expansion might have alleviated concerns that normally affect adults with children around the time of school opening, thereby improving their mental health.[16]

Second, restricting the analysis to adults with children 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 (i.e. monthly transfers rather than the full provision of the benefit at tax time). When comparing adults with and without children, we cannot isolate the effects of the more generous benefit from the effects of other policy changes which might also affect mental health (e.g. more timely and regular access to cash can be preferred to lump-sum payments among credit-constrained individuals, even holding benefit amount constant). This is instead feasible when we compare adults with children of different ages,

[16]Unfortunately, information on the age of the child is reported only in the 2021 BRFSS wave, which means that we cannot plot trends in the outcome of interest based on the age of the child in the years before the CTC expansion. In the results section, we will in any case check for parallel trends in 2021.

holding constant any other changes in the system of benefit receipt.[17]

By exploiting policy-induced differences in the change to benefit levels, our preferred identification approach resembles the one adopted in previous work focusing on the 1993 EITC reform (Evans and Garthwaite, 2014; Hotz and Scholz, 2006; Adireksombat, 2010). Similar identification approaches have been used extensively in the literature, including to study the impact of a child tax benefit expansion in Canada (Milligan and Stabile, 2011). 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, in the present context, the variation in benefit levels is determined by the age of the child, while the 1993 reform generated differences in benefit levels based on the number of children.

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 is that 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 6. 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). This will generate some attenuation bias, that should be kept in mind while discussing the results. We will assess the extent to which this can change our results in the robustness tests, by excluding children aged 5-9 from the analysis.

The second 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 and only in the 2021 survey wave.[18] This means that we have information on the total number of children, but details on personal characteristics (i.e. age, gender and race) only for one of these children. This means that some of our 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 within households, potentially leading to attenuation

[17]We also note that observable characteristics are much more comparable between the treatment and control groups when using the second identification approach (see Appendix Table A3 and discussion above). While lack of balance in observable characteristics does not represent a a direct threat to the identification strategy in a DID setting, it can lead to a failure of the parallel trends assumption if individuals with different observable characteristics respond differently to the same time-varying shock.

[18]Details on characteristics of a random child in the household were collected also in previous waves, but are made available only starting in 2021.

bias. We will check the extent of this problem in the results section, by also looking at households with only one child, for whom we can perfectly define treatment status.

Before moving to the results section, three final considerations are worth mentioning. These apply irrespective of the identification strategy adopted, and can affect both the robustness of our results and also how to interpret them.

First, we do not have information on actual CTC receipt in the BRFSS, and will define treatment based on CTC eligibility (i.e. presence and age of children). This means that our estimates should be interpreted as intention-to-treat (ITT) effects. However, we do not believe that this represents a serious problem for two main reasons. 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%). This means that our treatment effects are unlikely to be driven by individuals who did not take up the CTC.

The second point concerns the fact that, in the two identification strategies, we rely on the exogeneity of the presence or age of the child in the household, respectively. This assumption has been frequently used in the EITC literature in the US, but it might be violated if individuals fertility decisions are affected by fiscal incentives, as it has been repeatedly found in the literature (see, for instance, Milligan (2005)). However, we consider this possibility unlikely in the present context. This is because of the temporary and unexpected policy change at the centre of the study, which makes it impossible for individuals to respond to the policy within the period of the analysis.

The final point concerns how to interpret our results, given the temporary nature of the CTC expansion. If we believe that the benefits of income on mental health increase with time individuals receive the support, our results should be interpreted as lower bounds of the effects that would be obtained if the policy was permanently implemented. If we instead believe that individuals rapidly adjust their expectations following an increase in expected income, then our estimates should be close to those obtained for a more permanent intervention. In any case, it is worth highlighting that, until late in 2021, it was not yet clear to benefit recipients whether the expanded CTC would have been extended (see Section 2).

5 Results

This section presents the main results of the analysis. In particular, Section 5.A presents the baseline results on the effects of the CTC on the number of of bad mental health days for the overall sample; Section 5.B presents a large battery of robustness and placebo tests to confirm the validity of these results; Section 5.C explores the heterogeneity of treatment effects across groups in the population; while Section5.D discusses possible mechanisms.

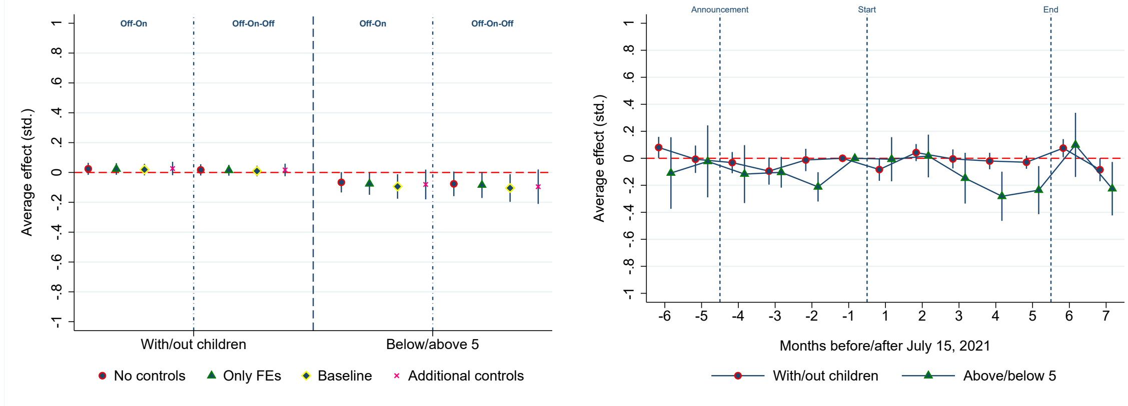
A. Baseline results

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. Panel A presents the results of a simple DiD specification, using the two identification strategies introduced above. The post-policy dummy is also 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 14 August 2021, and the value of one from 15 August until 15 December 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 15 August in the DiD specification (i.e. one month after the first CTC transfer).[19] In the "Off-On-Off" specification, the dummy is constructed in the same exact way until 15 December 2021, but then is set to zero (rather than missing) from 16 December 2021 until the end of the survey interviews in February 2022. For each identification strategy and 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").

Results are very similar irrespective of the definition of the post-treatment dummy and the choice of the covariates, while they differ substantially between the two different identification approaches. In particular, all estimates obtained by comparing adults with

[19] 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 we also refer the reader to the time event specification which does not rely on these assumptions.

Figure 4: Effects of the receipt of the expanded CTC on the number of bad mental health days



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

Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). In both panels, the results are reported for the two identification strategies described in the text. In the first one (denoted as "With/out children"), we compare adults in households with and without children. In the second one (denoted as "Below/above 5), 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 our results with both identification strategies. In the same panel, we also denote with vertical dashed bars the time of policy announcement ("Announcement", corresponding to 11 March 2021), the time of the policy introduction ("Start", corresponding to 15 July 2021) when the first CTC monthly transfer was disbursed, and the time of policy withdrawal ("End", corresponding to 15 December 2021) when the last CTC monthly payment was made.

and without children report a precisely estimated zero effect on the number of bad mental health days. By contrast, all the specifications that compare households of children below or above the age of five report that a more generous transfer improves self-reported mental health. This mostly comes from a reduction in the number of bad mental health days within the population reporting at least one bad mental health day (Appendix Figure B2).

In our preferred specification in Panel A of Figure 4, having access to a more generous benefit decreases the number of bad mental health days 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).[20] 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).[21] The mental health benefits that we obtain are around half of those that have been estimated from the provision of public health insurance in the US (Finkelstein et al., 2012).

[20]Ridley et al. (2020) report an average improvement in mental health outcomes equal to 0.067 standard deviations for cash transfer programs and of 0.138 for multifaceted anti-poverty programs.

[21]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).

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. Appendix Figure B3 shows the results of this exercise, following the same structure of Figure 4 above, but reporting only treatment effects for the comparison of households with children above or below the age of five. 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 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 and Garthwaite, 2014).

Panel B of Figure 4 shows the results from our time-event specification, where we substitute the post-treatment dummy with a full set of monthly dummies centred around the first CTC transfer on 15 July 2021. We present results with our two identification strategies, but using only the covariates included in our baseline model introduced above. When comparing adults with and without children, we find that the zero treatment effects are rather constant over time. Moving to our preferred identification strategy, we find instead substantial heterogeneity in treatment effects over time. In particular, we note that adults with children below or above the age of five were on parallel trends before the policy announcement that took place in March 2021. Afterwards, adults with children below the age of five started experiencing a relative improvement in their mental health status. This anticipation effect peaks two months before the policy change, when trends between treatment and control groups return parallel. This situation continues until three months after the first CTC monthly transfer, when mental health starts improving again among households receiving a more generous CTC.[22] However, the positive effects of the more generous benefit

[22]The fact that positive mental health effects appear only after the third CTC monthly payment is very much 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).

immediately vanish when benefits are withdrawn in mid-December 2021.

B. Robustness tests

We now present a series of robustness tests. To start with, we should understand why we obtain zero effects when we compare adults with and without children. We have argued that this might be due to the fact that parents experience higher stress around the time of school re-opening, especially during a pandemic (see Figure 3 for some initial evidence on this). Panel A of Appendix Figure C1 presents the results of the same DiD specification introduced above, but separately for 2021 (left side of the panel, replicating results presented in panel A of Figure 4) and 2019. While in 2021 we have precisely estimated treatments effects around zero, 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 C1 replicates instead the event-study estimates presented in panel B of Figure 4, but this time adding data for 2019 to control for within-year variations in mental health status constant across years. Using this specification, we find a decrease in the number of days of bad mental health even with this identification strategy.[23]

Similarly, we should test if our preferred identification strategy is affected by similar problems. For instance, it could be that the mental health status of adults with younger children was improving simply because of their lower exposure to school closures. We present a series of tests to check this hypothesis. To start with, 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. This includes unmarried individuals with income above $150,000 and married individuals with an income above $200,000 (see Figure 1). If our positive treatment effects on mental health were driven by other confounding factors, we should see a positive effect also for these individuals. Instead, treatment effects are precisely estimated around zero (Figure C2). We also conduct a triple difference exercise, using income as additional dimension in the analysis. Here, the identification comes by comparing adults with children below and above 5 in relatively poorer versus richer households.[24] We find a reduction in the number

[23] It is worth noting that the treatment effects that we obtain when comparing adults with and without children in 2019 and 2021 (panel B of Figure C1) are smaller in magnitude compared to the baseline estimates we obtain when comparing adults with children above or below the age of 5 (Figure 4). This might be due to a series of reasons, including differences in the population of interest and differences in treatment definition (i.e. here it is not possible to distinguish between the effects of the changes in benefit levels from other policy-induced changes, such as the introduction of monthly payments).

[24] The assumption is that richer households are either not affected by the policy change or, even if they

of bad mental health days even under this specification (Appendix Figure C3).[25]

We interpret the evidence presented so far by drawing two conclusions. First, the zero treatment effects that we obtain in our baseline specification when comparing adults with and without children comes from two opposite forces: a "standard" deterioration in mental health status for adults with children around the time of school re-opening and a positive effect from the expanded CTC. Second, the identification strategy that compares adults with children above and below the age of five is unlikely to suffer from similar problems and it instead captures the causal effects of a more generous CTC. For these reasons, in the continuation of the paper we will present results only from this second specification.

We conclude this section by presenting a series of robustness tests on our preferred identification strategy. First, we replicate the baseline results but excluding children aged 10 to 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 C4). Additionally, we exclude kids aged from 5 to 9 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. Even in this case, results remain largely unchanged (Figure C5). We also restrict the analysis to households with only one child, to avoid mis-measurement of treatment status in the absence of information on the age of other children in the household (Figure C6). Results are smaller in magnitude in the DiD specification, but the event study analysis shows that they are very similar (although less precisely estimated). We then compare results obtained using all adults with children and then restricting the analysis only to parents (Figure C7). We also augment the specification by including additional controls for child's race, ethnicity and sex (Figure C8). All these tests confirm the main results discussed above.

Finally, we conduct a placebo test by using the number of bad physical health days as an alternative outcome of interest. The assumption is that a more generous transfer should not have any immediate direct effect on physical health. Figure C9 confirms this intuition and shows that treatment effects are precisely estimated around zero, both for the

experience an increase in benefit levels, this plays a smaller role compared than for poorer households. 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.

[25] Point estimates are larger in magnitude in this triple difference specification, but, as expected, confidence intervals also become bigger (especially in the time event model). It is also worth highlighting that the timing of treatment effects slightly changes compared to our baseline specification, with no effect after the policy announcement and instead a positive effect on mental health already after the first monthly payment.

overall sample (panel A) and for different sub-groups in the population (panel B).

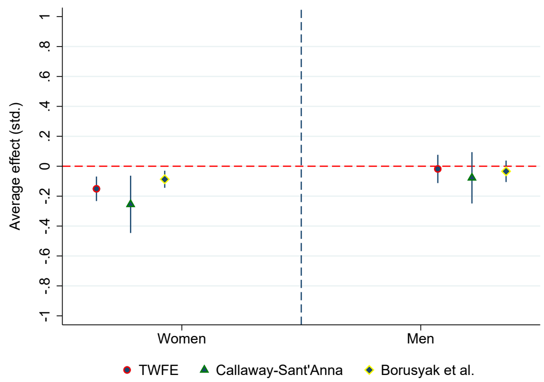
C. Heterogeneous effects

We now explore heterogeneous treatment effects across groups in the population. In particular, we replicate the main analysis but splitting the sample according to some individual and household characteristics. Figure 5 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 on treatment effects homogeneity (Sun and Abraham, 2021). While the timing of treatment is common among all groups in the present analysis, treatment intensity varies (i.e. based on household income, even in our preferred identification strategy). Additionally, previous evidence has shown that the mental health effects of cash transfers are different across groups in the population (Ridley et al., 2020).

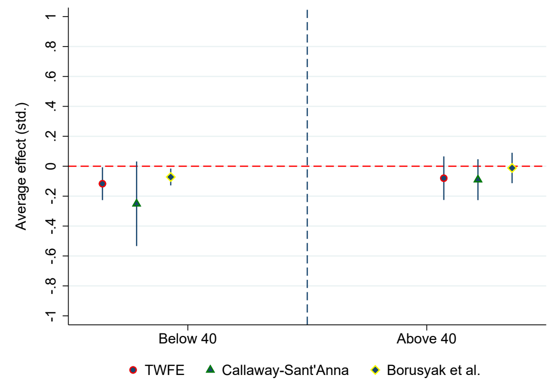
The results show that the positive effect on mental health status is reported among women, but not for men (panel A). The effect is also stronger for individuals below the age of 40, compared to older individuals (panel B). Instead, we do not find any notable difference in treatment effects between white and non-white individuals (panel C). 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. However, the positive effect on mental health materializes only for individuals of Hispanic origin (panel D). 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) (panels E and F, respectively). 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 individuals in rural areas (panel G). Finally, we find differences in treatment effects based on child's sex, but these are not constant across model specifications (panel H).

The results from the heterogeneous analysis are largely in line with those of previous studies. For instance, the literature on the EITC in the US has consistently reported larger effects for women compared to men (Evans and 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. This is also consistent with evidence from Appendix Figure B2, showing that

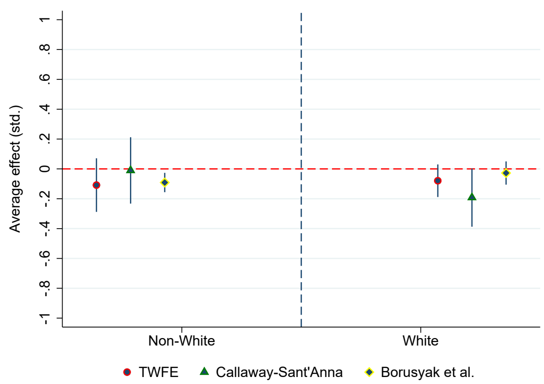
Figure 5: Heterogeneity in treatment effects



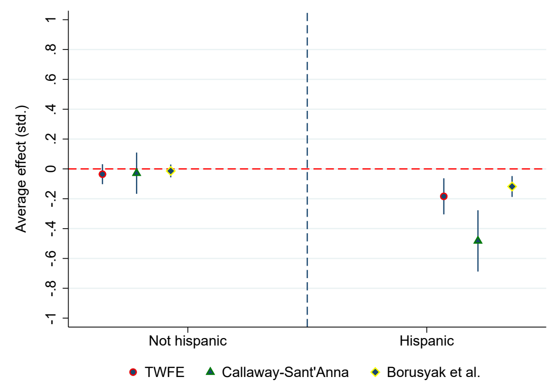
(a) By sex



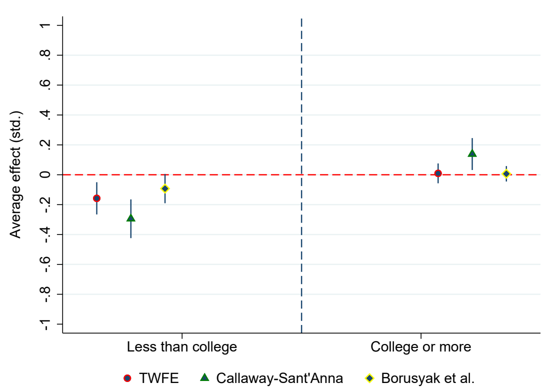
(b) By age



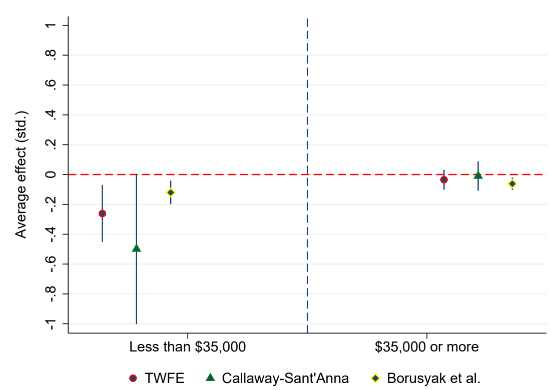
(c) By race



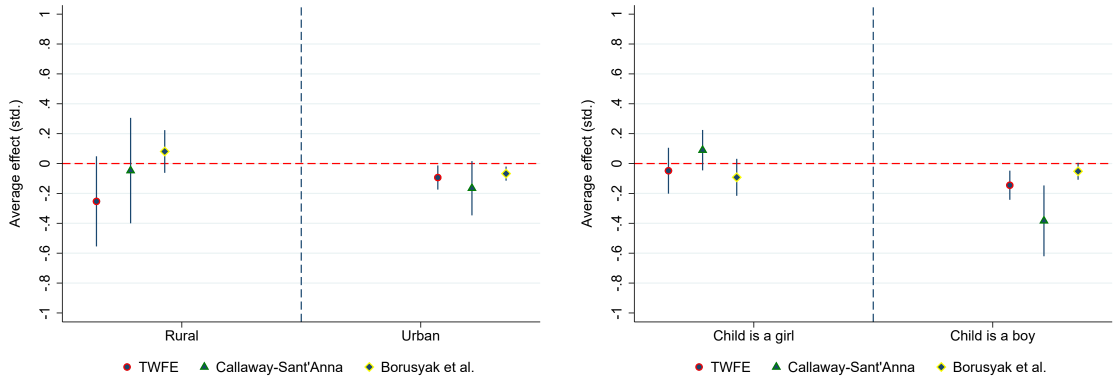
(d) By ethnicity



(e) By educational levels



(f) By household income



(g) By area of residence

(h) By child's sex

Notes: 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. 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. 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 results obtained using estimations proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2021).

the positive effect on mental health status materializes only on the intensive margin (i.e. reducing the number of bad mental health days among individuals who experience at least one bad mental health day during the month).

D. Potential mechanisms and discussion

Armed with these results, we now 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 centres, personal care establishments and grocery stores (Parolin et al., 2022). As a result, it is estimated that the CTC expansion reduced child poverty rate by as much as 40% (Parolin et al., 2021b) and decreased material hardship (Parolin et al., 2021a). 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 consumption 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.

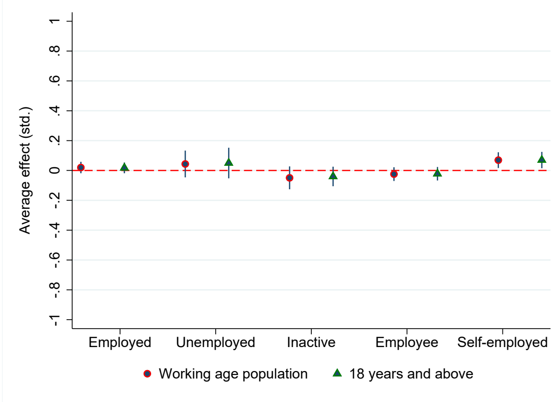
In particular, we will look at treatment effects on (i) labor market outcomes, (ii) health care coverage, and (iii) health care use. These results will be presented for the overall population in the main text (panel A of Figures from 6 to 8) and for different sub-groups of the population in Appendix B. We will then account for potential mediators by replicating the results on mental health for the different groups in the population while also including among the covariates the possible transmission channels (panel B of Figures from 6 to 8). 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.[26] 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 6). 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 (Appendix Figure B4). This is in line with previous evaluations of the CTC expansion, documenting lack of disincentive effects on labor supply and, if anything, small positive employment effects for some groups (Ananat et al., 2021). At the same time, we find a small shift from dependent employment to self-employment. We then return to our baseline model using the number of mental health days as the outcome of interest, but augmenting it with controls for employment status and status in employment. We find that the positive treatment effects documented above remain in place, implying that they are not driven by changes in employment status or composition (panel B of Figure 6).

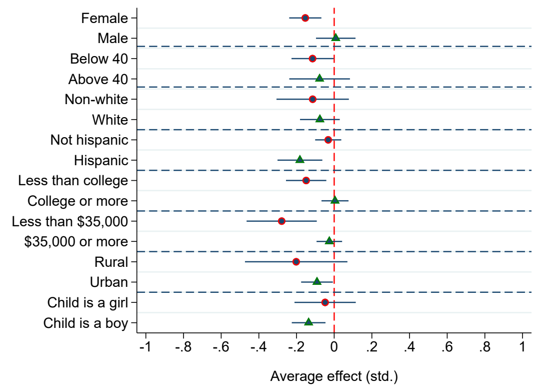
Positive treatment effects on health care coverage would materialize if individuals use the more generous CTC to buy some form of insurance. As such, any possible positive effect should materialize trough privately provided schemes. In turn, increased health coverage could improve mental health, as also 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 A of Figure 7)) 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

[26]Here and in the rest of this section, we will be using only our preferred identification strategy that compares adults in households with children above or below the age of five. Additionally, 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 14 August 2021 and value of one from 15 August until 15 December 2021 ("Off-On" specification in the language introduced above). Finally, all models will include the set of covariates corresponding to our baseline model (see above in this section for details).

Figure 6: Employment



(a) Treatment effects on labor market status



(b) Controlling for potential mediators

Notes: 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. 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, we instead use as outcome of interest the number of bad mental health days, but augment our baseline specification with dummies for employment and status in employment (equal to one if the individual is in self-employment).

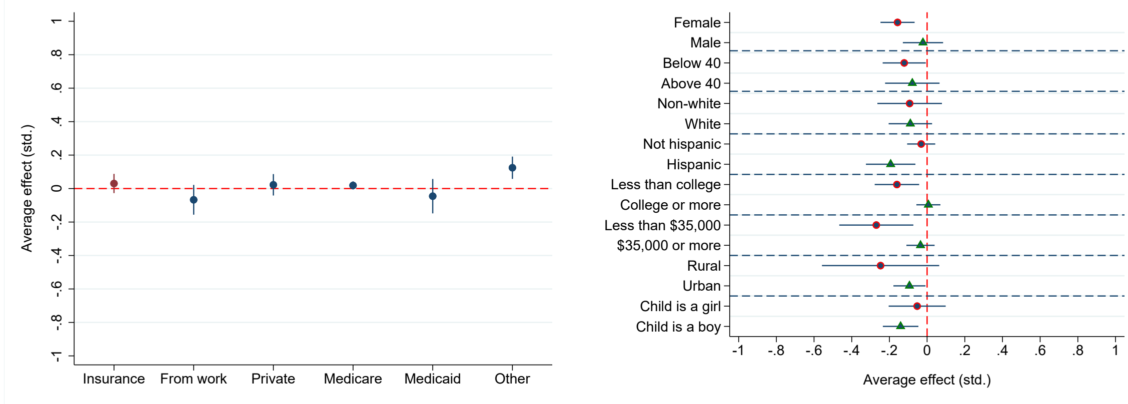
of being covered by public health insurance schemes such as Medicare and Medicaid, but we also find zero effects on other forms of private insurance (i.e. from work or other private schemes). We find a positive effect on other miscellaneous categories of health insurance.[27] However, the share of individuals being covered by these miscellaneous programs is small and, as a result, the overall effect on health coverage is zero.[28] Panel B of Figure 7 shows the results of the mediation analysis, where we augment our baseline specification with dummies for health care coverage and the type of coverage. Consistent with the results documented above, we find that positive treatment effects on mental health remain in place.

As a final hypothesis, we test whether the CTC expansion led to any changes in health care use, and if this is behind the positive effects on mental health documented above. In particular, the BRFSS asks respondents whether they have a doctor that they consider being their personal health care provider ("Doctor" in panel A of Figure 8), if they missed a doctor appointment within the last 12 months because they could not afford it ("Could 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

[27] 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.

[28] Appendix Figure B5 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.

Figure 7: Health insurance



(a) Treatment effects on health insurance

(b) Controlling for potential mediators

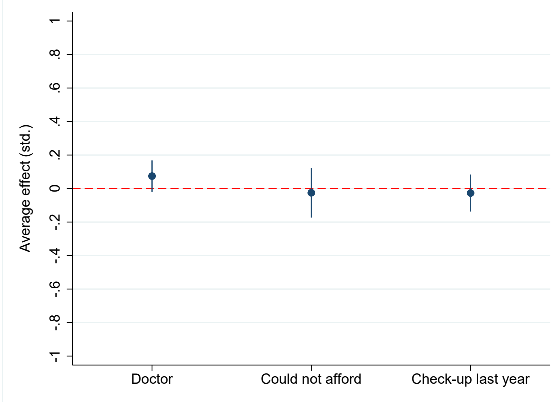
Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). The identification strategy adopted is the one comparing adults with children below or above the age of five. In Panel A, the outcomes of interest are dummies equal to one if the individual reports having health insurance coverage, overall (red dot) and by type of coverage (blue dots). In Panel B, we instead use as outcome of interest the number of bad mental health days, but augment our baseline specification with dummies for health insurance coverage and the type of insurance (equal to one if the individual has private insurance).

by sub-groups using the presence of a personal doctor as the main outcome of interest (Appendix Figure B6) and find zero effects across the board, although estimates for low-income individuals are positive and relatively large in magnitude.[29] We then augment our baseline specification to also control for the three indicators of health care use introduced above. Panel B of Figure 8 presents the results of this mediation analysis, where we again find that the positive effects of the more generous CTC on mental health remain in place even after including these additional controls.

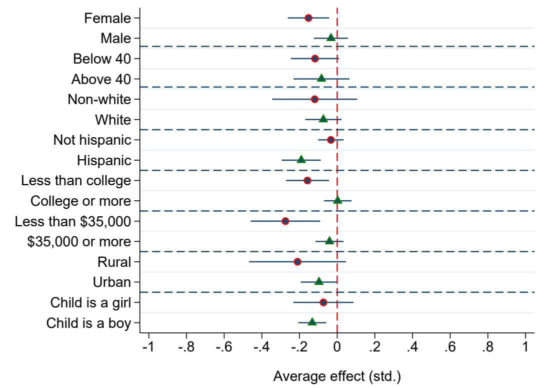
In the absence of data to test alternative hypotheses in the BRFSS, we can only speculate that the positive effects on mental health materialise due to the increase in consumption and the reduction in poverty and material hardship that has been documented in previous studies on the CTC expansion. Once 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 some temptation

[29] 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.

Figure 8: Health care use



(a) Treatment effects on health care use



(b) Controlling for potential mediators

Notes: 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. In Panel A, 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 B, we instead use as outcome of interest the number of bad mental health days, but augment our baseline specification with dummies corresponding to the three outcomes of interest in panel A.

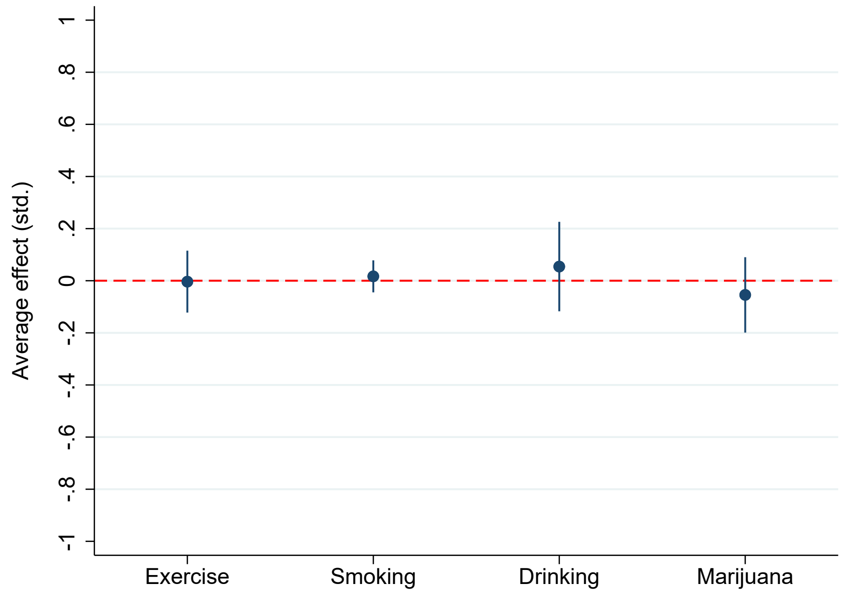
goods. For instance, Parolin et al. (2022) find an increase in overall household expenditure after the first CTC monthly payments, but not for items such as alcohol, tobacco and gambling. Consistent, with these findings, we report zero treatment effects on the probability of smoking cigarettes, drinking alcohol or using Marijuana (Figure 9, see note to the figure for exact definitions of the outcomes of interest). At the same time, we do not find any effect on the probability of exercising.

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. We use data from the BRFSS and exploit policy-induced variations in benefit generosity for adults with children of different age. 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, esti-

Figure 9: Healthy and unhealthy behavior



Notes: 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. 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 six days in the past month ("Marijuana").

mates 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, with treatment effects largely in line with previous evidence on the mental health effects of cash transfers in emerging and developing countries. Treatment effects materialize a few months after the first transfer, and disappear as soon as the benefit is withdrawn. We also find larger effects among women, young adults as well as low-income and low-educated individuals. We interpret these findings in light with 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.

Our findings then point to potential welfare gains from the rule-out of a nationwide 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., 2022, 2023), 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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Appendices

A Appendix: 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 (p-value)
	Mean	SD	Mean	SD	
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: Less than 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
Observations	438693		1002272		

Notes: The table presents mean and SD for selected individual and household characteristics, measured in the 2021 CPS and the 2021 wave of the BRFSS. The table also reports p-values of the t-tests of equality of means. Sampling weights are used to derive the estimates.

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		t-test (p-value)
	Mean	SD	Mean	SD	
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: Less than 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	242821		152521		

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

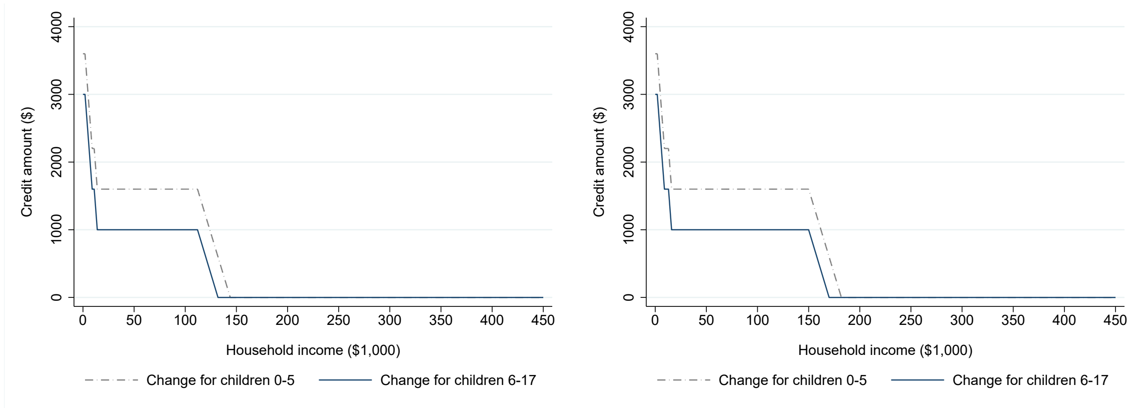
Table A3: Descriptive statistics and t-tests of equality of means, for selected variables for different populations based on treatment status

	Households with no children		Households with children			Households with children aged 5 or above		Households with children aged less than 5		
	(1)		(2)			(3)		(4)		
	Mean	SD	Mean	SD	t-test (p-value, (1)-(2))	Mean	SD	Mean	SD	t-test (p-value, (3)-(4))
Men	0.504	0.500	0.428	0.495	0.000	0.410	0.492	0.422	0.494	0.384
Age: 18-29	0.194	0.396	0.119	0.323	0.000	0.058	0.234	0.313	0.464	0.000
Age: 30-39	0.100	0.300	0.379	0.485	0.000	0.327	0.469	0.538	0.499	0.000
Age: 40-49	0.084	0.278	0.353	0.478	0.000	0.422	0.494	0.129	0.335	0.000
Age: 50-59	0.179	0.383	0.133	0.339	0.000	0.172	0.378	0.018	0.132	0.000
Age: 60-69	0.218	0.413	0.015	0.120	0.000	0.018	0.132	0.001	0.037	0.000
Age: 70 and above	0.224	0.417	0.002	0.043	0.000	0.002	0.046	0.000	0.019	0.004
Income: Less than 50k	0.460	0.498	0.365	0.481	0.000	0.358	0.480	0.389	0.488	0.033
Race: White	0.752	0.432	0.710	0.454	0.000	0.719	0.449	0.697	0.460	0.108
Ethnicity: Hispanic	0.132	0.338	0.261	0.439	0.000	0.259	0.438	0.272	0.445	0.349
Marital status: Married	0.456	0.498	0.700	0.458	0.000	0.695	0.460	0.686	0.464	0.476
Marital status: Divorced or separated	0.136	0.343	0.107	0.309	0.000	0.124	0.330	0.062	0.241	0.000
Marital status: Widowed	0.093	0.291	0.011	0.103	0.000	0.013	0.113	0.004	0.067	0.002
Marital status: Never married	0.268	0.443	0.116	0.320	0.000	0.106	0.307	0.158	0.365	0.000
Marital status: Unmarried couple	0.046	0.209	0.067	0.250	0.000	0.062	0.241	0.089	0.285	0.000
Presence of children in household	0.000	0.000	1.000	0.000	0.000	1.000	0.000	1.000	0.000	.
Number of children	0.000	0.000	1.987	1.069	0.000	2.004	1.092	1.943	1.026	0.027
Age of child: 0-4	.	.	0.261	0.439	.	0.000	0.000	1.000	0.000	0.000
Age of child: 5-9	.	.	0.264	0.441	.	0.358	0.479	0.000	0.000	0.000
Age of child: 10-14	.	.	0.276	0.447	.	0.374	0.484	0.000	0.000	0.000
Age of child: 15-17	.	.	0.199	0.399	.	0.269	0.443	0.000	0.000	0.000
Sex of child: Male	.	.	0.511	0.500	.	0.512	0.500	0.511	0.500	0.942
Relationship to child: Parent	.	.	1.000	0.000	.	1.000	0.000	1.000	0.000	.
At least one day not good mental health (0/1)	0.394	0.489	0.426	0.494	0.000	0.424	0.494	0.466	0.499	0.003
Number of days of not good mental health	4.576	8.576	4.480	8.240	0.326	4.538	8.352	4.715	8.155	0.437
At least one day not good physical health (0/1)	0.336	0.472	0.285	0.451	0.000	0.284	0.451	0.293	0.455	0.493
Number of days of not good physical health	3.959	8.526	2.451	6.322	0.000	2.561	6.539	2.187	5.686	0.017
Observations	317258		44049			29749		8518		

Notes: The table presents mean and SD 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, following the two identification approaches used in the paper. These are (i) adults in households with no children (column 1, corresponding to the control group in the first identification strategy), (ii) adults in households with children (column 2, corresponding to the treatment group in the first identification strategy), (iii) adults with children aged 5 or above (column 3, corresponding to the control group in the second identification strategy), and (iv) adults with children aged less than 5 (column 4, corresponding to the treatment group in the second identification strategy). 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.

B Appendix: Additional figures

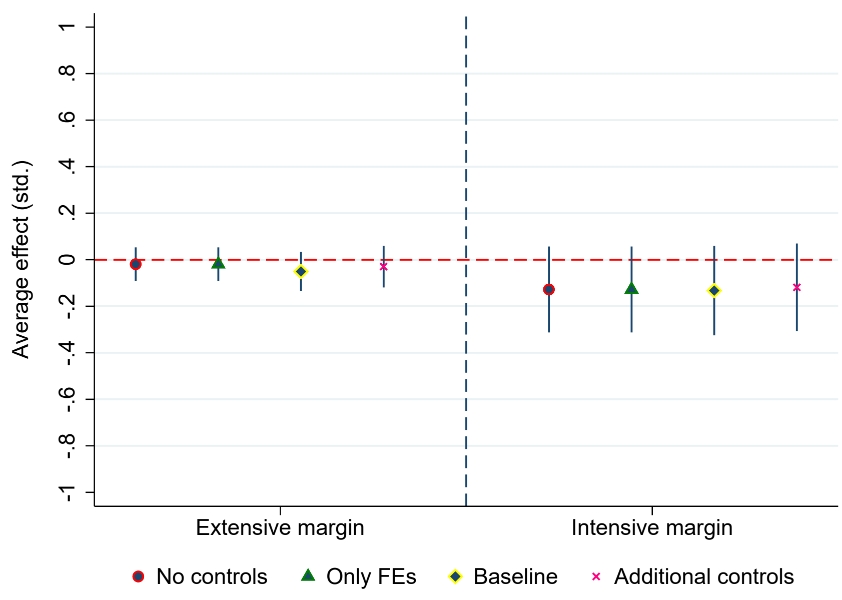
Figure B1: Change in benefit levels for married and unmarried individuals due to the 2021 CTC expansion, by age of the child



(a) Unmarried **(b)** Married

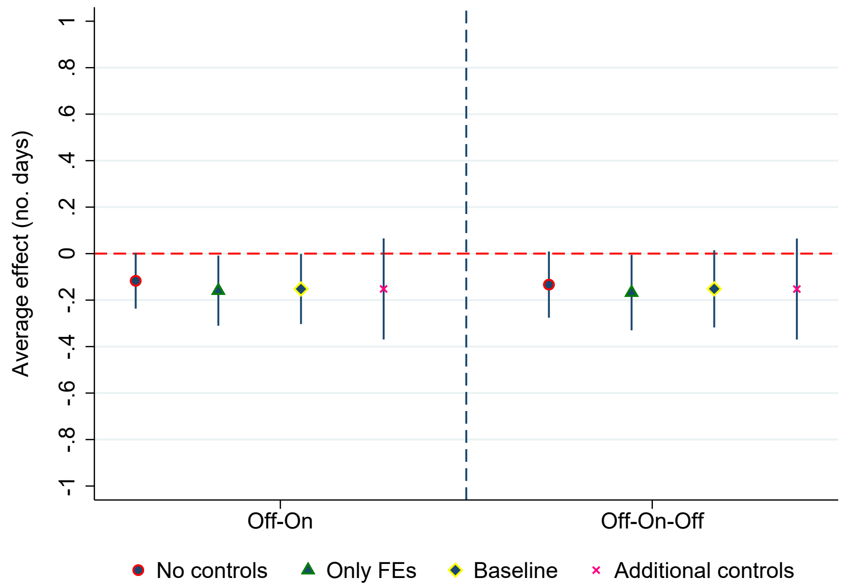
Notes: The figure reports the change in benefit levels that resulted from the 2021 temporary CTC expansion, for adults with children above or below the age of six. The information is reported separately for unmarried and married individuals (panels A and B, respectively). The figure is based on information provided in CSR (2021).

Figure B2: Results on the extensive and intensive margin



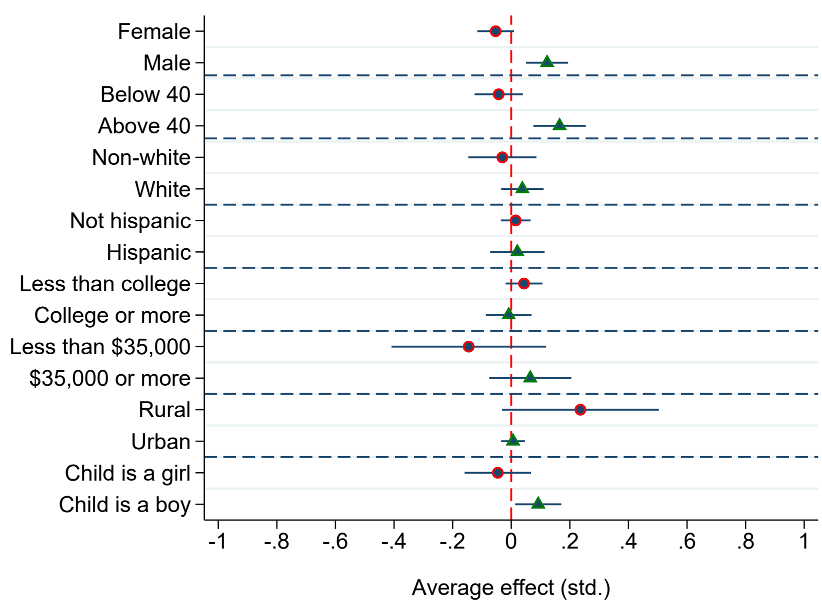
Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). The identification strategy adopted is the one comparing adults with children below or above the age of five. In the left part of the figure, the outcome of interest is a dummy equal to one if the individual experiences at least one bad mental health day in the previous month ("Extensive margin"). In the right part of the figure, the outcome of interest is the number of bad mental health days experienced among individuals who have at least one bad mental health day within the month ("Intensive margin"). For each outcome of interest, we present results from different types of specifications where we vary the set of covariates included (see text for details).

Figure B3: Negative binomial regression model



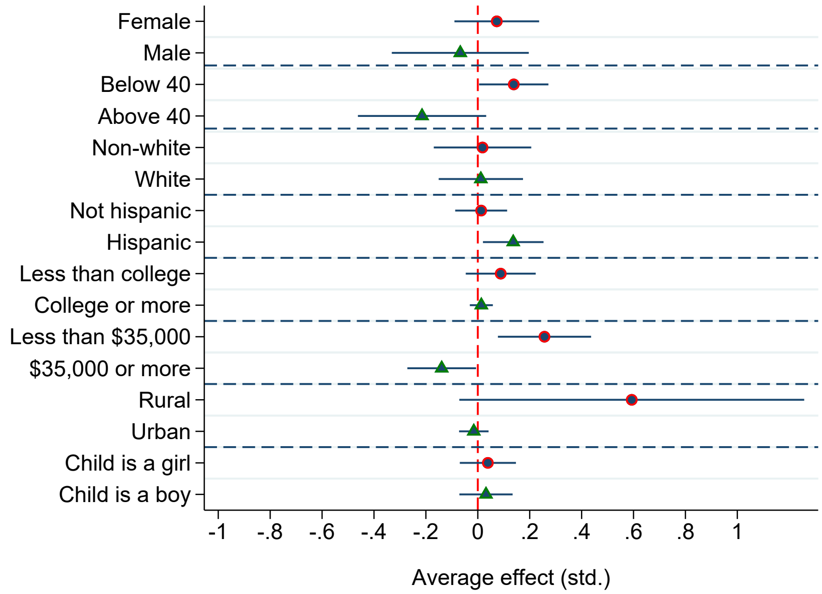
Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). The identification strategy adopted is the one comparing adults with children below or above the age of five. Compared to the results presented in Figure 4, here the outcome of interest is not normalized to have mean of zero and SD of one in the pre-treatment period, while we account for its 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).

Figure B4: Heterogeneous results for employment



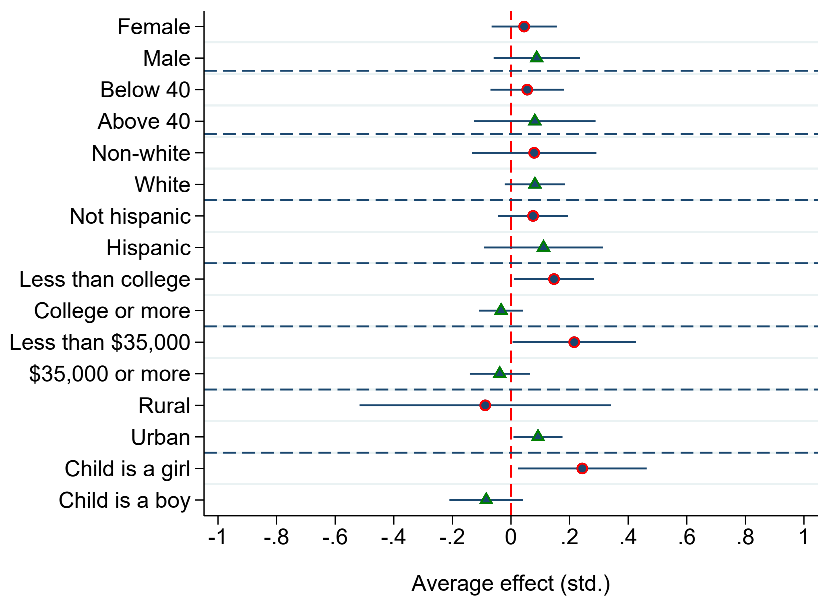
Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). The identification strategy adopted is the one comparing adults with children below or above the age of five. Results are presented separately for different groups in the population. The outcome 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).

Figure B5: Heterogeneous results for health care coverage



Notes: 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. 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).

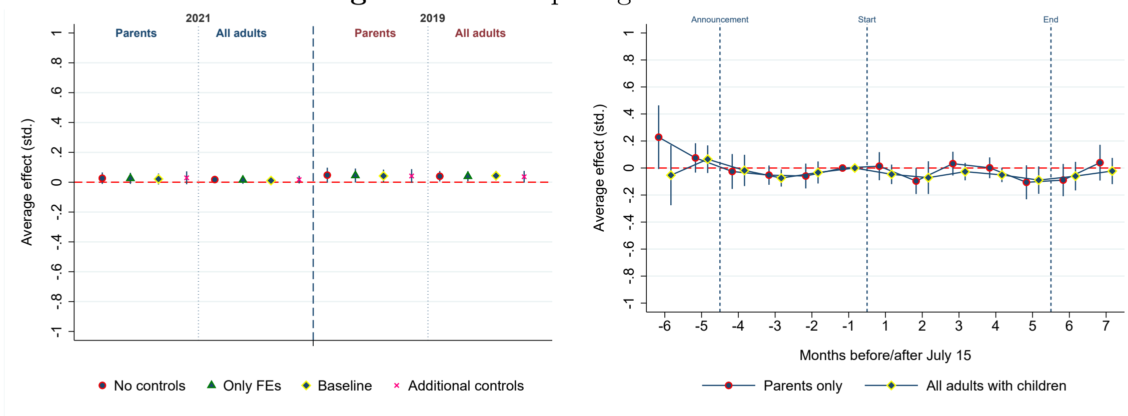
Figure B6: Heterogeneous results for health care use



Notes: 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. 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).

C Appendix: Robustness tests

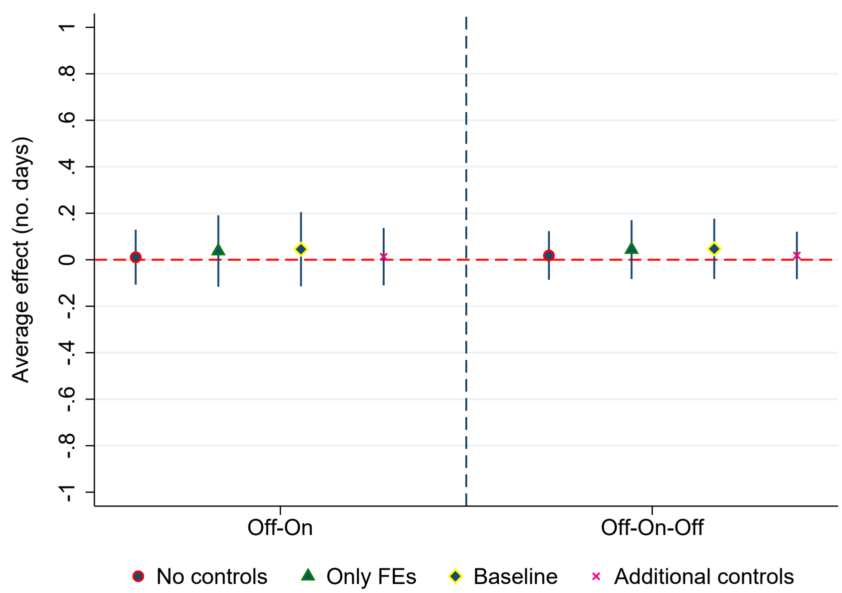
Figure C1: Comparing 2019 with 2021



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

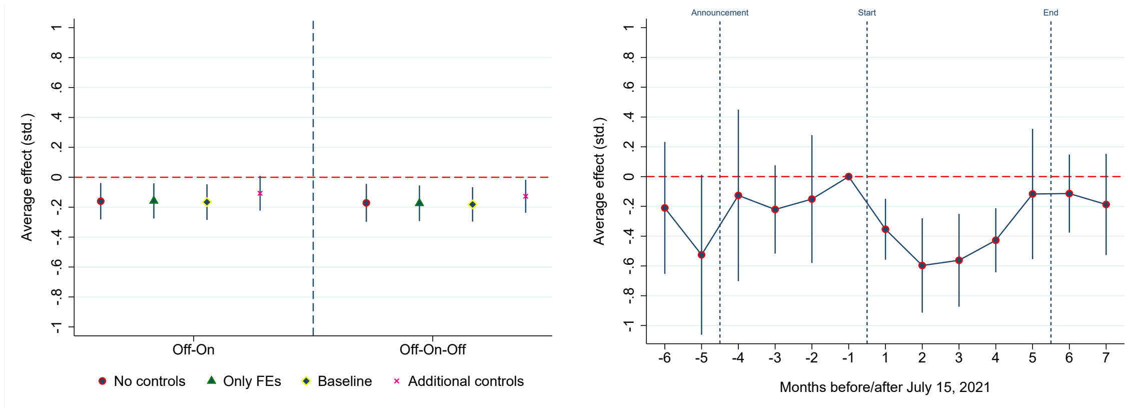
Notes: The figure reports point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). The identification strategy adopted is the one comparing adults with and without children. The outcome of interest is always the number of bad mental health days reported over the previous month, normalized to have mean of zero and SD of one in the pre-treatment period. In panel A, the anlaysis is conducted separately for 2021 (replicating findings in Figure 4 in the main text) and for 2019 (placebo policy change) as well as for parents and all adults with children and using different sets of covariates. In panel B, we use the covariates included in our baseline model and perform an event-study approach using 2019 as an additional time period in the analysis (i.e. in addition to 2021, with the inclusion of year dummies). Results are presented separately for specifications that include all adults with children and only parents.

Figure C2: Placebo analysis using very rich households



Notes: 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, here 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.

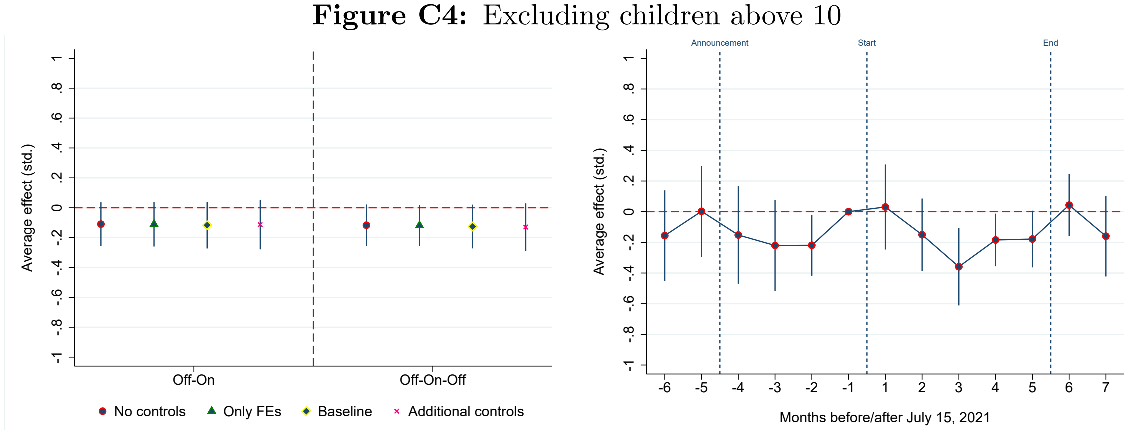
Figure C3: Triple difference



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

Notes: 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).

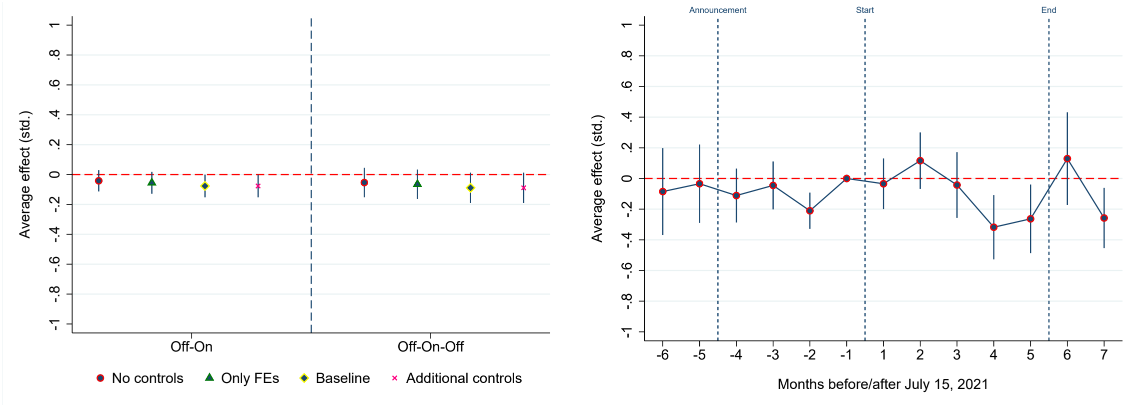
Figure C4: Excluding children above 10



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

Notes: 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 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. The event-study results in panel B use instead the set of covariates corresponding to our baseline model (see text for details).

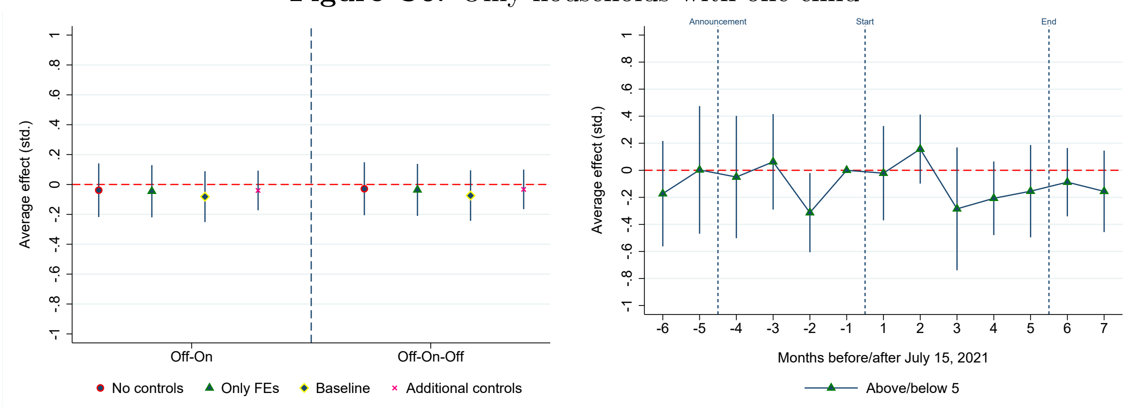
Figure C5: Excluding children 5-9



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

Notes: 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 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. The event-study results in panel B use instead the set of covariates corresponding to our baseline model (see text for details).

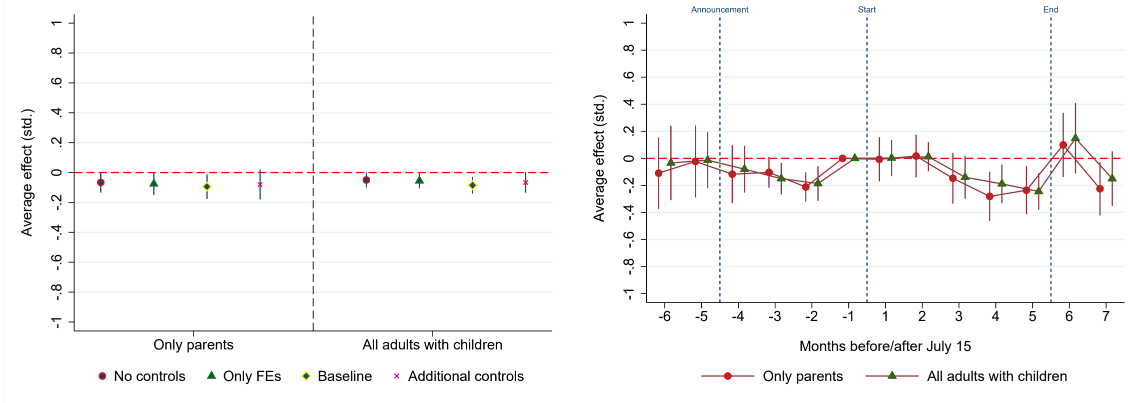
Figure C6: Only households with one child



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

Notes: 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. The event-study results in panel B use instead the set of covariates corresponding to our baseline model (see text for details).

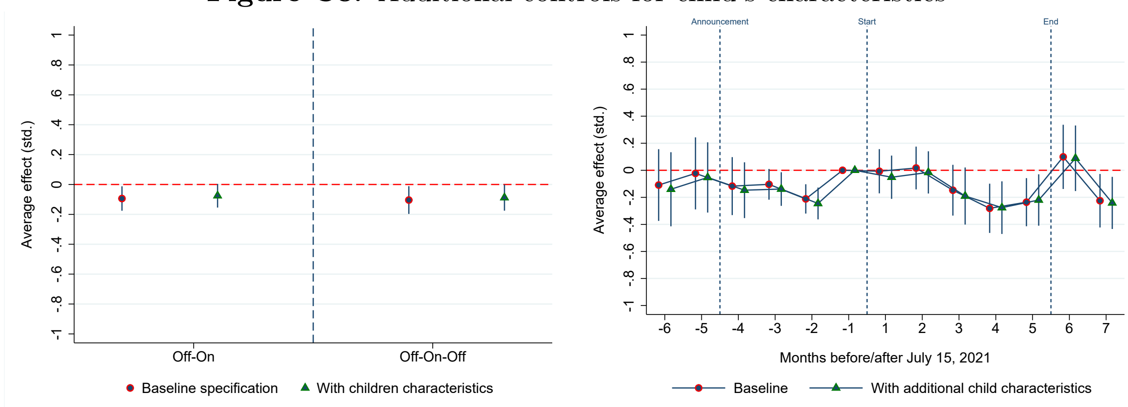
Figure C7: Parental and non-parental adults



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

Notes: 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. 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. The event-study results in panel B use instead the set of covariates corresponding to our baseline model (see text for details), while again comparing all adults with children with only parents.

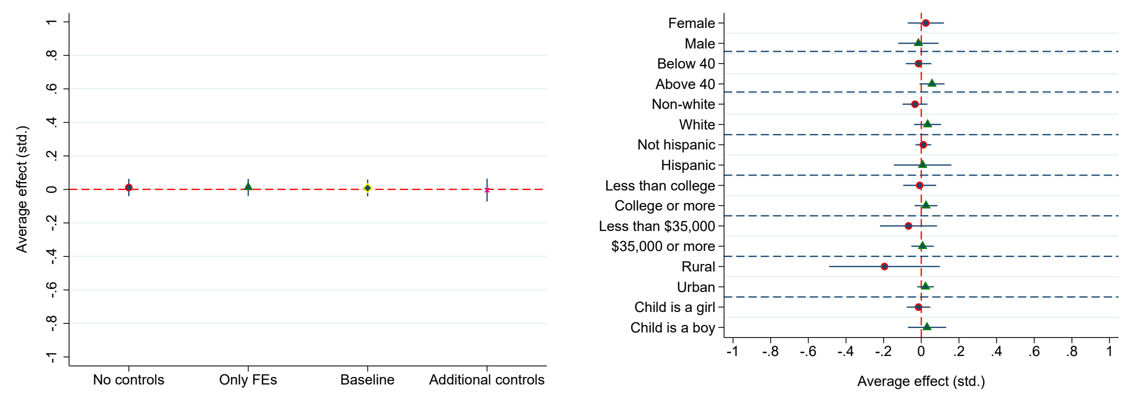
Figure C8: Additional controls for child's characteristics



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

Notes: 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 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.

Figure C9: Treatment effects on the number of bad physical health days



(a) DiD **(b)** Heterogeneous analysis

Notes: 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. However, the outcome of interest corresponds to the number of bad physical health days reported in the previous month. In panel A, we report simple DiD estimates that vary depending the set of covariates included (see text for details). In panel B, we present results using the set of covariates included in our baseline model, separately for different groups in the population.