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Effects of the Expanded Child Tax Credit on Household Expenditures during the Interview Survey Reference Period: Preliminary Evidence from the Consumer Expenditure Survey

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Preliminary Intent-to-Treat Estimates from the Consumer Expenditure Survey

[Will not be released until after the mid-year release of the micro-data]

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Abstract

The Child Tax Credit (CTC) was substantially expanded through the American Rescue Plan Act of 2021, making the benefit more generous, fully refundable, and more periodic. Early studies documented the positive impact of the expanded CTC on reducing poverty and food hardship, but there is no research on its impact on household spending, and particularly spending on children. We apply a series of difference-in-difference estimates using newly available data from the Consumer Expenditure Survey (CE) collected through September 2021 to examine whether the expanded CTC increased expenditures, both overall and for targeted categories such as spending on children's education and development. We note that this paper provides only a first preliminary look at expenditures using partial and incomplete data from the first two months of the expanded payments. Future iterations will examine the full effects of the payments on more complete data. We find positive and statistically significant intent-to-treat effects for spending on food, children's clothing, and childcare, although results vary across specifications. This study, particularly once data for the full period are available, will add to a growing body of evidence on the effects of the expanded CTC on U.S. households' economic well-being, and provide information as to its potential role in improving child well-being.

Introduction

The American Rescue Plan Act of 2021 (ARP), enacted in March 2021, significantly expanded the Child Tax Credit (CTC) in three major ways. First, it made the benefit more generous, increasing the maximum benefit size from $2,000 per child to $3,000 per child for children aged 6 to 17 years and $3,600 per child for children aged 0 to 5.[1] Included within this change was an increase in qualifying age from 16 and younger to 17 and younger. Second, it made the benefit "fully refundable," meaning tax filers were able to receive the full benefit of the credit regardless of their tax obligation.[2] Third, it converted the CTC's annual payment to payments delivered monthly from July to December of 2021.[3]

As a result of the expansions, from July through December 2021, most low- and middle-income households with children in the United States (U.S.) received monthly cash payments of $300 per child under age six and $250 per child between the ages of 6 and 17. Recipients had to have previously filed taxes, or alternatively enroll in the benefit using a government portal intended to allow non-filers (typically the lowest income households) to receive the benefit. The first monthly payment was distributed to the households of 59.3 million children in July 2021, while the last payment reached 61.2 million children in December 2021 (U.S. Department of Treasury, 2021).

Though now expired, the ARP-CTC is unprecedented in its reach and role in addressing the economic hardship of low-income households with children. Using new data from the Consumer Expenditure Survey (CE), this study examines the initial effects of the CTC expansion on consumer unit or household spending.[4] We stress that the results presented here are based on preliminary and incomplete CE data from just the first two months of payments. Our goal is not to interpret the results at present, but rather to lay out our approach and methodology for comment and revision. Nevertheless, for -

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[1] Before the ARP, eligible children included those under age 17 with a Social Security Number (SSN) who could be claimed as a dependent. Under the ARP, eligible children included those under age 18 with an SSN who could be claimed as a dependent.

[2] Previously, up to $1,400 of the $2,000 available through the CTC (per qualifying dependent) was refundable. Filers with income tax liability less than the refundable portion of the CTC would receive the difference as a refund. However, they would not benefit from the additional $600 available through the CTC that was non-refundable. Only filers with income tax liabilities larger than the refundable portion of the CTC were able to benefit. With the enacting of the ARP, there was no longer a minimum income at which a filer was able to receive the full benefit of the CTC.

[3] For most households, half of their annual payment was delivered in monthly installments between July and December of 2021, with the remaining half being delivered when they filed their taxes in 2022 for tax year 2021.

[4] The CE collects data on "Consumer Units," which in some cases differ from households. A consumer unit is defined as a group of people who "comprises either: (1) all members of a particular household who are related by blood, marriage, adoption, or other legal arrangements; (2) a person living alone or sharing a household with others or living as a roomer in a private home or lodging house or in permanent living quarters in a hotel or motel, but who is financially independent; or (3) two or more persons living together who use their income to make joint expenditure decisions. Financial independence is determined by the three major expense categories: Housing, food, and other living expenses. To be considered financially independent, at least two of the three major expense categories have to be provided entirely, or in part, by the respondent." See https://www.bls.gov/cex/csxgloss.htm#:~:text=Consumer%20unit%20%2D%20A%20consumer%20unit,in%20permanent%20living%20quarters%20in. The term consumer unit and household are often used interchangeably in the literature, although households are considered all people who live at an address and thus are not the same as consumer units. However, throughout the manuscript we refer to consumer units as households.

illustrative purposes, we present the preliminary results of our preferred methodology in the Results section.

Early studies established that the expanded CTC reduced material hardship (Parolin, Ananat, et al., 2021; Perez-Lopez, 2021), child poverty (Acs & Werner, 2021; Marr et al., 2021; Parolin, Collyer, et al., 2021), and childhood malnutrition (Waxman et al., 2021), while having no significant effect on parental employment (Ananat et al., 2022; Roll et al., 202a). Empirical research has not yet explored how household expenditures, and implicitly consumption, have changed due to the CTC expansion.

Our study adds to the literature on the impact of the CTC by examining overall expenditures on food, housing, and other key categories of spending. The consumption of goods and services is a primary indicator of living standards, and thus, a key component of measuring economic well-being (OECD, 2013). Moreover, parents' spending on children represents an important pathway to promote children's development and well-being (Jackson & Schneider, 2022; Kaushal et al., 2011; Kornrich & Furstenberg, 2013; Schneider et al., 2018). In particular, an increase in food consumption is known to improve children's nutrition and has long term positive impacts on economic well-being (Hamilton et al., 2022).

Additional income provided through cash transfers like the CTC may be characterized as improving child outcomes through two channels: family stress and family investments (Wimer and Wolf, 2020). The family stress model posits that economic hardship impairs family functioning, increasing parents' stress and undermining their mental health and ultimately children's development (Conger & Conger, 2002). Increased income can thus reduce family stress and improve child outcomes. The family investment model posits that increased income allows parents to purchase or invest in various things that enhance child development and well-being (e.g., books, toys, enriching activities, high-quality childcare, or nutritious food) (Yeung et al., 2002). This study focuses on the family investment model by investigating whether the expanded CTC affected households' expenditures on children as well as overall expenditures on categories such as food that contribute to child well-being.

Policymakers would like to know if households receiving the CTC are spending it in ways that benefit children, or if they are spending it on items that are not related to child well-being such as alcohol or tobacco. Thus, establishing how households spend the money is an important empirical question. A few studies have examined how households spend benefits similar to the CTC (Amorim, 2021; Jones et al., 2019; Gregg et al., 2005). Amorim (2021) examines the effects of income gains from cash transfers distributed from the Alaska Permanent Fund Dividend on child-related spending by exploiting exogenous variation in the values of the payment across time. The study uses CE data from the 1996–2015, and finds that immediately after receiving cash transfers, parents across the income distribution increase spending on clothing and durable goods that may improve children's material well-being. Using data from Canadian expenditure surveys, Jones et al. (2019) examined the effect of Canadian Child Benefits (CCB), very much like the ARP-CTC's payment design, on household expenditures and found that CCB increased the spending on basic needs such as transportation, childcare, and food at home, while decreased the expenditures on food in restaurants, alcohol and tobacco. Gregg et al. (2005) found that increased benefits for families with children in the UK led to increases in spending on children's clothing and footwear, books and toys, and fresh fruit and vegetables, and declines in spending on tobacco and alcohol; in addition, families receiving the increased benefits increased their ownership of cars and phones. However, we lack evidence on how families spent the CTC. While some studies have reported on how people receiving the CTC reported spending it (Karpman et al 2021; Pilkauskas & Cooney 2021; Rachidi 2021; RAPID-EC 2021; Roll et al 2021b; Zippel 2021), this study is the first to examine changes in spending patterns of households resulting from the CTC payments using a causal identification strategy.

This study applies a difference-in-difference approach, harnessing variation in the policy based on difference in time period, number of children in the household, and receipt/amount of the CTC. Newly available and comprehensive expenditure data from the CE allows us to examine household spending on (1) major categories, such as food, housing, alcohol and tobacco, leisure, etc., and (2) child-related spending including that for books, childcare, computers and tablets, and enrichment activities among households with children. This study seeks to contribute to a growing body of evidence on the effects of the expanded CTC on U.S. households' economic well-being and provide evidence as to its potential role in improving child health and education outcomes. In this paper, we use preliminary and incomplete data from the early months of the CTC expansion to put forward our preferred methodological approach to identifying causal effects of the policy, and then illustrate our approach using newly-available, though still preliminary and incomplete data.

The American Rescue Plan and Related Research

The ARP temporarily transformed the CTC into a nearly universal child allowance for 2021 through three fundamental changes:[5] (1) expanded eligibility, (2) higher credit amounts, and (3) delivery of credit in monthly installments for the second half of 2021. Prior to this temporary expansion to the CTC, tax filers could receive a maximum CTC of $2,000 per child per year at tax time.[6] However, the credit was not fully-refundable. Instead, it phased in with earnings, and tax filers claiming dependent

[5] The changes to the CTC in the ARP follow those outlined in the American Family Act (a bill first introduced in both the Senate and House of Representatives in 2017 and reintroduced in 2019) with one exception: in the AFA, the credit would begin to phase out for heads of household with earnings above $120,000 or and joint filers with Adjusted Gross Incomes (AGI) over $180,000. In the ARP, the credit began to phase out for families with AGIs above $112,500 or $150,000 per year, depending on filing status, but it only phased out until matching the credit values that a family would receive under prior law. This alteration was made because the Biden administration committed to not raising taxes for those with incomes below $400,000 per year

[6] See additional information on the history of the Child Tax Credit, see Crandall-Hollick (2021), Crandall-Hollick (2018), and Garfinkel et al. (2016).

children needed to earn a certain amount in order to qualify for the maximum credit.[7] Overall, one in three children did not receive the full benefit value because their parents did not earn enough to qualify. Children with single parents, those in rural areas, Black and Latino children, and those in larger households were disproportionately ineligible for the full credit (Curran and Collyer, 2020; Collyer, Harris, and Wimer, 2019).

The ARP expanded CTC eligibility to almost all children, including those in households with the lowest incomes who were previously excluded, by removing the earnings requirement and making the credit fully refundable. Second, it raised the maximum annual credit amounts to $3,000 for children ages 6-17 and $3,600 for children under age 6.[8] Third, beginning mid-July 2021, it delivered the credit in monthly installments of up to $250 per older child or up to $300 per younger child, for a period of six months.[9] Note that while the ARP expanded eligibility for the CTC, monthly payments did not reach all eligible households.[10] We take several steps to account for the imperfect coverage of the monthly CTC payment when evaluating the policy's effects spending, as discussed in our Data and Methods section.

Early research suggests the expanded CTC has generated large reductions in child poverty (Acs & Werner, 2021; Marr et al., 2021; Parolin, Collyer, et al., 2021) and reduced food insufficiency among households with children (Parolin, Ananat, et al., 2021; Perez-Lopez, 2021). A few studies have reported that households with children increased spending on childcare and school-related expenses (Roll, Hamilton, & Chun, 2021; Perez-Lopez & Mayol-Garcia, 2021). Other studies have asked parents directly how they spent their payments, finding that the most commonly reported expenses are on food, bills, and other necessities (Pilkauskas & Cooney, 2021). To date, no study has estimated the effect that the monthly CTC payments had on spending across the host of categories available in the CE data or used data on spending collected both before and after the delivery of the monthly CTC payments.

Data and Methods

Data:

This study uses micro-level data from the CE, a nationally representative survey sponsored by the U.S. Bureau of Labor Statistics that collects spending, demographics, and other financial information for households living in the U.S. The survey is divided into two methods of data collection, the Interview

[7] See Curran and Collyer, 2020 for details on how much a filer needed to earn to qualify for the full credit.
[8] Included within this change is an increase in the maximum qualifying after from 16 to 17.
[9] Because the payments began halfway through the year, families will receive half of the full amount of their credit in 2021 and the remainder when they file taxes in 2022. Note that children born in 2021 were not eligible for monthly payments. Their families can instead claim the entirety of the credit at tax time.
[10] Families who did not file taxes in the prior year, presumably due to having an income below the tax-filing threshold, generally needed to register with the Internal Revenue Service (IRS) in order to receive their monthly credit. Several estimates suggest that the total number of children in eligible tax units is around 64 to 67 million children (Parolin et al., 2021), more than the 60.9 million to whom the IRS distributed CTC payments to in August 2021 and to 61.2 million children in December 2021.

Survey and the Diary Survey. This research utilizes the data collected as part of the Interview portion of the CE since it covers a larger proportion of all spending by households. In addition, the Interview reference period is the three months prior to the interview as opposed to the Diary Survey, which is used to collect expenditures during a week. The CE sample is a rotating panel recruited every survey month, with households[11] to be interviewed once in each of four consecutive quarters.[12] Given this survey structure, we observe expenditures at overlapping three-month intervals. For example, data collected in August 2021 refer to expenditures from May to July 2021; data collected in September 2021 refer to expenditures from June to August 2021.

In response to the passage of the ARP, the BLS included a module of questions in the CE survey asking about the CTC starting with the October 2021 interviews. Although advanced CTC payments have not been previously issued, the BLS has included questions about the COVID-19 Economic Impact Payments, as well as previous recovery rebate programs. The CTC questions were developed based on the questions used in the recovery rebate modules. However, October data have not been released yet. So, we are unable to report on the responses to these questions.[13] Furthermore, without data on actual receipt of the CTC monthly payments, we are unable to estimate the treatment-on-treated effect. However, we have data through September, which means two interview months (August and September) have reference periods that (partially) overlap with the distribution of the monthly payments.[14]

Using TAXSIM estimates included in with CE microdata, we are able to calculate the additional CTC received as a result of the expansion.[15] First, we calculate the monthly value of the CTC that a tax unit would have been eligible for under the pre-ARP tax law. Then we calculate the monthly value of the CTC under the APR expansion (as described in detail earlier in the ARP and Related Research section)[16]

[11] Recall that when we are referring to households, the CE actually collects data for a unit dubbed the "consumer unit." Consumer unit is defined as "all members of a housing unit related by blood, marriage, adoption or some other legal arrangement; or two or more persons living together who are identified as making joint expenditure decisions; or a single person who is living with others but is financially independent" (BLS 2005).

[12] Due to survey non-response, CUs may participate int the CE for fewer than 4 times. Population weights are created to be applied to quarterly expenditures to produce estimates that are representative of the U.S. population.

[13] Once the full 2021 dataset is released by the BLS, we will update our analysis to cover the entire period the advanced CTC payments were distributed (i.e. through December 2021).

[14] The reference period for August is May, June, and July. The reference period for September is June, July, and August. The advanced CTC payments began being distributed in July and continued through December. This period of disbursement overlaps with one month in the August interview reference period and two months in the September interview reference period.

[15] Using data collected during the Interview, the BLS creates tax units and then employs the NBER TAXSIM model to provide imputations of adjusted gross income, and the number and age (i.e, 0-6, 6-17, 18+) of dependents at the tax unit level.

[16] To calculate the pre-ARP CTC in the CE data, we first calculate the federal income tax liability of tax units in the data (according to the 2021 tax brackets and marginal tax rates, see Tax Foundation, 2022), and then calculate the value of the CTC they would have received under prior law. For those with AGIs below pre-ARP CTC phaseout thresholds, this was calculated as the lesser of: (1) $2,000 per qualifying dependent, and (2) the sum of 15% of their earned income and their tax liability. For those with AGIs above the phaseout thresholds, we calculate the maximum credit for their family size and then reduced in at a rate 5% for each dollar over the phaseout threshold. We also used data on tax units to calculate the credit the received under the ARP expansion. This expansion granted all families with adjusted gross incomes below $112,500 (joint filers) or $75,000 (heads of household) a credit of $3,000 per child ages 6-17 and $3,600 per child under age 6, half of which was paid out in monthly installments between July and December 2021. Above these thresholds, the credit phased out at a rate of 5% for each dollar in

and take the difference between this and value of the pre-ARP CTC. This monthly difference is then multiplied by the number of months within the reference period that monthly payments could have been received, which yields a 3-month reference period value of the additional CTC received for each tax unit. We then aggregate the 3-month reference period additional payments for all tax units within a household to get a household-level measure of the additional CTC received.[17] Table 1 shows the average dollar value of the expected additional CTC received for August and September Interviews. With this measure, we are able to estimate the intent-to-treat effect (ITT). The models and the identification strategy we use are described in the Methods section.

***Table 1**: Average Addition Expected 3-Month Reference Period Value of the CTC to be Received per Household*

	Additional Expected 3-Month Reference Period CTC Received	
	All Households	With children < 17 y/o
Total	$104.85	$365.88
	(5.81)	(16.51)
Income Category		
Under $25,000	$113.97	$752.24
	(15.16)	(72.25)
$25,000 to $50,000	$107.74	$385.92
	(11.99)	(29.69)
$50,000 to $100,000	$110.69	$342.94
	(8.37)	(17.37)
$100,000+	$87.84	$214.72
	(6.48)	(12.82)
Race		
White, non-Hispanic	$74.79	$312.65
	(4.80)	(15.19)
Black, non-Hispanic	$165.75	$515.94
	(29.96)	(79.27)
Hispanic	$174.17	$414.54
	(16.49)	(30.74)
Asian, non-Hispanic	$103.30	$276.38
	(15.53)	(31.91)

AGI, until reaching the credit that families received under prior law. The policy parameters then followed those of previous law, beginning the final phaseout of the credit for joint filers with AGIs over $400,000 and heads of household with AGIs over $200,000.

[17] Ideally, we would be able to conduct our analysis at the tax unit level, but CE expenditure data are reported at the household level, which can contain multiple tax units. Therefore, our measure of the additional CTC payment received needs to be aggregated to the household level.

Other, non-Hispanic	$184.16	$447.99
	(46.33)	(88.05)

Notes: Averages are calculate using data from August and September Interviews only. The additional CTC received is reported at the household-level. All averages are weighted using FINLWT21 and the 44 replicate weights.

Expenditure categories:

The CE provides comprehensive and detailed information on household spending. To examine the ARP-CTC's effects on household expenditures, both overall and child-related, we construct variables for 17 outcome categories.

First, we use data on ten major expenditure categories: housing and utility; food; alcohol and tobacco; clothing; transportation; health; leisure; personal care; education and reading; miscellaneous; and total expenditures. Second, using CE data on detailed item expenditures, we construct the following seven categories of child-related expenditures: children's clothes; books and toys; childcare; school items; computers and tablets; enrichment activities; and sports items. Details on the measures of each expenditure category are presented in Appendix Tables 1 (major categories) and 2 (child-related categories). Table 2 shows the average 3-month reference period total expenditures for households with and without children, pre- and post-distribution of the monthly CTC payments.

Table 2*: Average 3-Month Reference Period Total Expenditure*

	3-Month Reference Period Total Expenditures			
	With children < 17 y/o		Without children < 17 y/o	
	Pre	Post	Pre	Post
Total	$20,019.34	$21,208.95	$13,549.96	$14,636.23
	(628.86)	(617.48)	(160.97)	(293.21)
Income Category				
Under $25,000	$10,259.68	$10,675.38	$8,507.72	$8,742.39
	(321.96)	(663.97)	(148.88)	(255.09)
$25,000 to $50,000	$12,217.95	$12,463.79	$10,323.07	$10,604.53
	(286.25)	(968.75)	(215.94)	(376.81)
$50,000 to $100,000	$18,392.40	$17,394.71	$14,660.62	$14,317.27
	(1906.27)	(723.01)	(367.68)	(403.10)
$100,000+	$28,473.31	$31,599.51	$23,737.87	$27,166.02
	(767.83)	(1151.33)	(449.51)	(869.97)
Race				
White, non-Hispanic	$22,435.58	$24,908.68	$14,129.97	$15,091.11
	(587.26)	(901.49)	(207.66)	(359.77)

Black, non-Hispanic	\$17,589.95	\$15,796.76	\$11,043.90	\$10,449.23
	(3661.53)	(1339.28)	(352.19)	(472.90)
Hispanic	\$15,181.90	\$15,258.64	\$12,037.12	\$13,807.25
	(376.12)	(987.92)	(304.58)	(707.24)
Asian, non-Hispanic	\$21,188.21	\$24,907.17	\$15,948.02	\$20,619.31
	(883.58)	(2545.84)	(821.06)	(2076.11)
Other, non-Hispanic	\$17,867.94	\$14,565.66	\$12,199.82	\$17,623.68
	(1408.42)	(1428.53)	(752.87)	(3116.16)

Notes: "Total Expenditures" is based on the CE definition of total spending. "Pre" includes data collected during interviews from January 2021 through July 2021. "Post" includes data collected during interviews from August 2021 through September 2021. All averages are weighted using FINLWT21 and the 44 replicate weights.

Methods:

Our empirical models all rely on a difference-in-difference framework. We are interested in understanding how the expansion of the CTC affected expenditures. Analyzing the difference in pre-CTC expansion and post-CTC expansion spending for treated units would yield a first difference estimate of the treatment effect. However, a first difference specification will not control for variation in spending due to month-to-month differences ("monthly" variation) and changes in supply and demand of goods and services due to COVID-19 restrictions, which may be correlated with when the monthly payments began being distributed. Variation due to the pandemic can be separated into two subcategories. The first type of variation we will refer to as "pre-pandemic" variation, and is the variation in spending that occurs between pre- and post-start of the pandemic. The second type of variation is "within pandemic" variation and is the variation in spending that occurs between months due to changes in COVID-19 restrictions.

To control for these various sources of variation, we first estimate the difference between the pre- and post-CTC expansion spending for households with and without children. Households without children do not benefit from the expansion of the CTC, and therefore, act as our control units. Equation (1) shows the model that will be estimated.

$$E_{it} = \beta_0 + \beta_1 * Child_{it} + \beta_2 * Child_{it} * 1[CTC_t > 0] + \nabla X_{it} + \alpha_t + e_{it} \quad (1)$$

We first evaluate the CTC expansion using a binary treatment indicator (i.e., the post-CTC expansion period), which is represented as the indicator function $1[CTC(t) > 0]$. As described in the data section, our dependent variable is three-month expenditures for various spending categories. We only use data from 2021 in order to control for "pre-pandemic" variation in spending.[18] Expenditures are reported by household i and collected in interview month t.[19] $Child\_it$ is a binary variable equal to 1 if the household has at least one child under age 18 and 0 otherwise. Vector X_{it} represents a set of household characteristics, namely age of the reference person, this person's gender, race/ethnicity, and education, and for the household the total income, state of residence, and the number of people in the household. α_t represents a full set of controls for the interview month, which account for seasonal variation.

With a binary treatment, we are using only the variation in spending between possible recipients and non-recipients to identify the treatment effect. Possible recipients are represented by households interviewed in August or September that have at least one child aged 17 or less. We have two types of non-recipients, households who have at least one child, but were not interviewed in August or September and households without a child aged 17 or less. Variation in spending for pre- and post-treatment is controlled for with interview month fixed effects (α_t). We control for "monthly" variation and "within pandemic" variation by including a dummy variable for the presence of a child. The coefficient (β_2) on the interaction term provides the difference-in-difference estimate of the association between the expanded CTC and expenditures by households with children. Recall, we do not have data on receipt of the monthly payments. Instead, we assume all households with children receive a monthly payment, so β_2 represents the Intent-to-Treat (ITT) effect.[20]

Expanded CTC payments were first sent out on July 15, 2021, and the last payments (as of this writing) were sent out in mid-December of 2021. Ideally, we would measure the effect of expanded CTC payments on monthly expenditures, but CE data are collected at the 3-month reference period level.[21] A respondent who is interviewed in August will report expenditures for May, June, and July. Since part of the reference period occurs after the CTC expansion, this respondent is considered "treated" under the binary specification even though only one month of expenditures occur post-expansion. A similar "partial treatment" also occurs for interviews occurring in September 2021, where only two months of expenditures (July and August) occur post-expansion. A potential shortcoming of using equation (1) to estimate the treatment effect is that it treats households interviewed in August and September as if they received the same treatment intensity, but they did not. As noted earlier a respondent who is interviewed in August 2021 will report expenditures for May, June, and July. Only during one month of this reference

[18] All households are reporting expenditures from after the start of the pandemic, so there is no difference in spending due to spending pre- or post-start of the pandemic.
[19] Recall, the reference period for expenditures is the three months prior to the interview.
[20] When the remaining quarters of 2021 CE data are made available, we will apply a two-stage least squares methodology to estimate the treatment-effect on the treated. For this approach, we use the expected benefit (based on the calculation described above) as an instrumental variable and the observed receipt (as reported by families in the CEX surveys) as the endogenous variable.
[21] Some of the expenditures for the CE interview are collected at the monthly level, but a majority are only reported for the en tire three-month reference period, which is why we use the data at the (overlapping) three-month frequency.

period could a household have received a monthly payment. In contrast, a respondent interviewed in September 2021 will have a reference period of June, July, and August, which means this respondent would have been treated for two months. By treating CUs interviewed in August and September as having the same treatment intensity we are diluting our treatment effect. We attempt to correct for this potential bias, as well as improve the statistical power of our model, by exploiting the variation in the amount of the monthly CTC payment.[22]

Equation (2), shown below, is identical to equation (1) with the only difference being the indicator function, $1[CTC(t) > 0]$, has been replaced with $CTC(t)$, which represents the dollar value of the additional CTC that is expected to be received after the expansion. By using the variation in the expected amount to be received we are implicitly controlling for the variation in treatment intensity that occurs between August and September interviews. Ceteris paribus, $CTC(Aug) < CTC(Sept)$ simply because August interviewees will only receive one month of the payment whereas September interviews will be assigned two months. Moreover, this specification allows us to use not only variation between recipients and non-recipients, but also variation in spending by households who are expected to receive different size payments. Again, β_2 represents our estimate of the ITT effect.

$$E_{it} = \beta_0 + \beta_1 * Child_{it} + \beta_2 * Child_{it} * CTC_t + \nabla X_{it} + \alpha_t + e_{it} \quad (2)$$

Both equations (1) and (2) use families without children to control for any "monthly" variation in spending, as well as to control for "within pandemic" variation due to changes in supply and demand of goods and services resulting from changes in COVID-19 restrictions. For these models to lead to an unbiased estimator, households with and without children need to have similar "monthly" and "within-pandemic" variation in spending. However, it could be the case that households with children have different month-to-month trends in spending and are impacted differently when COVID-19 restrictions change. For example, households with children will have expenditures for school and childcare that are different from month to month and not incurred by households without children and unrelated to the receipt of monthly payments. Additionally, changes in policies about school openings and closing will impact households with children but not households without children. As a result, the estimate of the ITT effect in equation (1) and (2) could be biased. We account for this difference in seasonal spending by restricting our sample to only those households with children.

[22] Since the variation in treatment intensity can be described as a discrete measure, we could separate in binary treatment variable into three categories of treatment intensity (e.g., CTC=0.33 in August 2021 and March 2022; CTC=0.67 in September 2021 and February 2022; CTC=1 in other months after August 2021) and estimate equation (1) using this alternative specification. This option will be explored while revising this work.

If we simply restrict our sample to only households with children, we reduce our model to a first difference. Therefore, when we restrict our sample to only households with children, we also pull in data from January 2019 to September 2019 interviews. Equation (3a) shows the new specification with a binary treatment variable. Equation (3b) shows the continuous treatment specification.

$$E_{it} = \beta_0 + \beta_1 * 1[CTC_t > 0] + \gamma_t + \lambda_t + \nabla X_{it} + e_{it} \quad (3a)$$

$$E_{it} = \beta_0 + \beta_1 * CTC_t + \gamma_t + \lambda_t + \nabla X_{it} + e_{it} \quad (3b)$$

Unlike in equations (1) and (2), our treatment and control groups are dictated by the year in which the interview took place rather than the presence of children. γ_t represents this year fixed effect, taking a value of one if the household was interviewed in 2021. This fixed effect controls for "pre-pandemic" variation, variation in spending between 2021 (treatment) and 2019 (control). It also controls for the month-to-month ("monthly") variation in spending that occurs for households with children that is the same between 2021 and 2019. To control for the pre- and post- treatment variation, we include a full set of fixed effects for the interview month (λ_t). The ITT effect with an indicator for when a household is expected to receive a monthly payment during their reference period (i.e., August and September 2021). In equation (3b) the treatment variable is the dollar value of the expected monthly payment received during the reference period. All other variables are the same as in equation (3a). The ITT effect is represented by β_1. Our identification strategy is similar to the identification strategy used to estimate the ITT effect in equation (1) and (2).

Unlike with equations (1) and (2), we are not concerned about differences in "monthly" variation due to the presence of children. However, one might still be concerned about the potential for changes in COVID-19 restrictions to affect spending ("within pandemic" variation). Most of the impact on spending from these restrictions were felt during 2020. By 2021 many of the restrictions that were constraining consumption during 2020 had already been relaxed and there was not much variation between the pre- and post-treatment periods. Therefore, we do not believe the pandemic era will cause much of a bias in equations (3a) and (3b), but we can use a triple-difference framework to help account for the any impact changes in restrictions had on spending.[23] We will provide a detailed description of this model in future iterations of this paper. A more detailed description of the technical details underlying the identification strategy is provided in the Technical Appendix.

[23] See Berck and Villas-Boas 2016 for an explanation of triple difference modeling.

Results

This section presents the ITT estimates for the models detailed above in the Methods section. Note all the models were estimated using the survey weight (FINLWT21). Additionally, the dollar value of the additional CTC received is scaled by $100. So, a 1 unit increase in the continuous model is equivalent to a $100 increase in the CTC monthly payment. As noted earlier, these results are meant only to provide an illustration of our approach, and should not be used yet to interpret the causal effects of the expanded CTC. In future iterations, when full data across the treatment period become available, we will be able to present and interpret results more definitively.

Table 3 shows the estimates of the ITT effect (β_2) from equation (1) and (2). The ten different expenditure categories correspond to the ten major expenditure categories described in the Data section. Panel I in the table presents the treatment effect for the binary specification. Panel II presents the results for the continuous specification. In general, the results are insignificant, which is not surprising given the data only contained two interview months of treated observations and those treated observations received a relatively small "dose" of the treatment.

That being said, we do see positive and weakly significant treatment effects under the binary specification for food and clothing. The positive coefficient on food is in line with other research that has found the CTC expansion has helped reduce food insecurity (Parolin, Ananat, et al., 2021). We also find a statistically significant negative treatment effect for transportation when using the continuous treatment. This result contradicts our hypothesis that the CTC would have a positive effect on spending. However, this could be a result of differences in seasonal spending between consumer units with and without children. Specifically, the treatment period reflected July and August spending which corresponds to when schools are closed for the summer. Thus, consumer units with children may be spending less on transportation because it is the summer, which is not a decrease that is likely to be experienced by consumer units without children.

We can test this theory by looking at Table 4. This table presents the estimates when using equations (3a) and (3b), which restricts the sample to only consumer units with children and pulls in expenditures from 2019. When using the binary treatment, the treatment effect on food and clothing are still positive and weakly significant. We also see the treatment effect on personal care becoming statistically significant. Turning to the continuous treatment (panel II), the treatment effect on transportation is still negative, but the magnitude of the effect has reduced, and it is not statistically significant. Both changes suggest there was some difference in the seasonal spending patterns of consumer units with and without children. However, the coefficient is still negative, but this could be a result of differences in pandemic era spending. The triple-difference specification will help us disentangle what is going on with this treatment effect.

***Table 3**: Difference-in-differences estimate of effect of expanded CTC on spending on major expenditure categories among households with children compared to households without children, binary and continuous treatment*

I: Binary Treatment	Total	Food	Housing	Transport	Health	Clothing	Alcohol & Tobacco	Leisure	Personal Care	Misc.
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	439.92	159.17	134.07	8.08	23.83	57.11	0.76	163.24	-2.32	14.55
	(836.20)	(75.79)	(220.60)	(183.90)	(72.86)	(29.92)	(19.26)	(141.30)	(8.32)	(46.96)
II: Continuous Treatment	**Total**	**Food**	**Housing**	**Transport**	**Health**	**Clothing**	**Alcohol & Tobacco**	**Leisure**	**Personal Care**	**Misc.**
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	-238.52	-2.30	-34.75	-57.30	-16.15	4.70	0.05	17.96	-2.10	5.99
	(124.11)	(12.06)	(27.79)	(17.63)	(9.95)	(4.90)	(3.57)	(18.99)	(1.22)	(4.40)
N observations	***15,432***	***15,432***	***15,431***	***15,432***	***15,432***	***15,432***	***15,432***	***15,431***	***15,432***	***15,432***

Notes: Standard errors clustered at the household level are reported in parenthesis. All estimates are calculated with FINLWT21 survey weights. The coefficients in Panel I can be interpreted as the dollar increase in spending over the three months during which the household is expected to have received the CTC. The coefficients in Panel II can be interpreted as the increase in spending per $100 of additional CTC received over the 3-month reference period.

***Table 4**: Difference-in-differences estimate of effect of expanded CTC on spending on major expenditure categories among households with children in 2021 compared to those in 2019, binary and continuous treatment*

I: Binary Treatment	Total	Food	Housing	Transport	Health	Clothing	Alcohol & Tobacco	Leisure	Personal Care	Misc.
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	1022.30	286.17	417.52	245.46	28.31	60.31	2.46	119.04	27.04	84.09
	(873.01)	(85.29)	(267.04)	(183.86)	(78.63)	(37.52)	(20.24)	(129.12)	(9.47)	(56.87)
II: Continuous Treatment	**Total**	**Food**	**Housing**	**Transport**	**Health**	**Clothing**	**Alcohol & Tobacco**	**Leisure**	**Personal Care**	**Misc.**
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	-166.45	6.08	-19.00	-21.16	-9.07	6.44	-1.32	7.30	0.06	8.15
	(106.12)	(13.07)	(29.46)	(16.30)	(10.06)	(5.25)	(3.52)	(15.96)	(1.16)	(4.45)
N observations	***8,955***	***8,955***	***8,954***	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***

Notes: Standard errors clustered at the household level are reported in parenthesis. All estimates are calculated with FINLWT21 survey weights. The coefficients in Panel I can be interpreted as the dollar increase in spending over the three months during which the household is expected to have received the CTC. The coefficients in Panel II can be interpreted as the increase in spending per $100 of additional CTC received over the 3-month reference period.

Finally, Table 5 presents the estimates of the ITT effects on child related expenditures using equations (3a) and (3b). Looking at the binary treatment in panel I, the treatment effect for children's clothes is positive and statistically significant. When we compare the magnitude of this treatment effect to the treatment effect we saw for clothing generally, it appears a majority of the additional spending on clothing generally is attributable to spending on children's clothes. The binary treatment effects for the rest of the spending categories are insignificant.

Turning to the continuous treatment effects in panel II, we see a positive and statistically significant coefficient on children's clothing. Consumer units who we expect to receive the advanced CTC payment spend $0.09 out of every $1 on children's clothing. Based on the average predicted gain in CTC payment ($365.88) recipients will increase spending on children's clothing by $31.54, which is within the margin of error for the binary treatment effect. In addition to children's clothing, spending on childcare has a positive and statistically significant coefficient.

While these results are promising, for the same reasons mentioned above about why large standard errors are not a big concern for us at this point, we do not want to read too much into the statistical significance we find. The results we present in here will likely change when we incorporate data from the rest of 2021.

***Table 5**: Difference-in-differences estimate of effect of expanded CTC on spending on child-related items and childcare among households with children in 2021 to those in 2019, binary and continuous treatment*

I: Binary Treatment							
	Children's clothes	**Books and toys**	**Computers, tablets, and tech.**	**School related items**	**Sports items**	**Childcare**	**Enrichment activities**
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	48.07	6.24	14.67	96.77	12.88	40.66	3.19
	(17.82)	(14.69)	(16.88)	(75.88)	(22.93)	(45.86)	(54.57)
II: Continuous Treatment							
	Children's clothes	**Books and toys**	**Computers, tablets, and tech.**	**School related items**	**Sports items**	**Childcare**	**Enrichment activities**
Estimation method	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*	*ITT*
	8.62	0.63	2.18	-14.15	-0.10	8.10	-5.83
	(3.10)	(1.91)	(2.13)	(10.10)	(3.16)	(4.82)	(4.34)
N observations	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***	***8,955***

Notes: Standard errors clustered at the household level are reported in parenthesis. All estimates are calculated with FINLWT21 survey weights. The coefficients in Panel I can be interpreted as the dollar increase in spending over the three months during which the household is expected to have received the CTC. The coefficients in Panel II can be interpreted as the increase in spending per $100 of additional CTC received over the 3-month reference period.

Conclusion

Households with children faced significant economic challenges during the pandemic as a result of lost jobs, school closures, and a lack of childcare. The expansion of the CTC provided more generous benefits, modified the rules to allow more households to receive the credit, and implemented monthly payments in hopes of alleviating some of this burden. Early studies have established the expanded CTC has reduced material hardship, improved food security, and reduced child poverty. However, none of these studies have looked at the direct impact of the CTC expansion on spending. This paper helps fill this gap by beginning to explore how household expenditures have changed due to the CTC expansion using data from the Consumer Expenditure Survey. Future iterations of the paper will revisit these preliminary and incomplete results as fuller data across the treatment period become available.

We implement a difference-in-difference approach and use variation between recipients and non-recipients as well as variation due to differences in the CTC amount a household receives to identify the treatment effect. Because the full year of 2021 data is not yet available and the CE only started to ask about receipt of the CTC in October 2021, our analysis is limited to estimating the intent-to-treat effects. Estimates of these effects suggest receipt of the CTC is most likely to impact spending related to children. Our results show an increase in spending on children's clothing and childcare in response to the CTC. Spending on food also was shown to increase. Spending on categories unrelated to children, such as alcohol and tobacco, did not appear to be impacted by the CTC. However, our dataset includes only two Interview Survey reference months that can be considered treated, and even so, the treatment is relatively small for these observations. Results vary across specifications, and our estimates are intent-to-treat only. Therefore, we want to caveat the results presented in this paper. When we update our analysis to use the full 2021 year of data, we will have many more treated observations with larger treatments and we will be able to estimate the treatment-on-treated effect, so we fully expect our results to change.

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Appendix Table 1: Major Expenditure Categories

Category	Description
Housing and utility	Shelter cost; utility cost; household operations; house furnishings and equipment
Food	Food at home and away from home (including meals as, and not as, pay)
Alcohol and tobacco	Alcoholic beverages and tobacco and smoking supplies
Clothing	Clothing and footwear for men, women, boys, and girls, and other apparel products and services
Transportation	Cars and trucks, other vehicles, gasoline and motor oil, maintenance and repairs, vehicle insurance, rental, leases, licenses, and public transportations
Health	Health insurance, medical services, prescription drugs, and medical supplies
Leisure	Fees and admissions to entertainment activities, televisions, radios, and sound equipment, pets, toys, and playground equipment, and other entertainment
Personal care	Wigs, hairpieces, or toupees, electric personal care appliances, and personal care services for males and females, including haircuts
Education and reading	Tuition, schoolbooks, supplies, and equipment for college, elementary and high school, day care center, and other schools, and other school-related expenses; newspapers and magazines and books
Miscellaneous	Miscellaneous expenditures including funeral, cash contributions, insurance and pension
Total	Total of above

Appendix Table 2: Child-related Expenditure Categories

Category	Description
Children's clothes	Infant and children clothing and footwear
Books and toys	Books and toys including infant furniture and equipment
Childcare	Babysitting and childcare and day care
School items	School related items including tuition, schoolbooks, supplies, and school lunch
Computers and tablets	Computers, tablets, digital book readers, and other related software and accessories including CD
Enrichment activities	Enrichment activities and outings including trips club membership tickets to events fees for lessons musical instruments
Sports items	Sports item including athletic gear, bicycles, and camping items

Technical Appendix

The purpose of this section is to provide a detailed technical explanation for the estimation strategy described in the Data & Methods section of the paper. It should be noted, the model described in this section does not exactly match the model presented in the main paper. The model presented in this section is a simplified version of model in the main paper. We use a simplified version of the model in order to make the explanation of the identification strategy and potential biases easier to understand. However, everything discussed in this section can be extended to the model we estimate,

$$Exp_{ict} = \alpha_c + \alpha_i + \beta_i t + \gamma_i CTC_{ict} + \delta_i y_{ict} + \eta_i hsize_{ict}$$

- i = pre- or post-pandemic (i.e. 2019 or 2020)
- c = presence of qualifying dependent
- t = time period (pre- or post-July, when the CTC advanced payments began)
- Exp_{ict} expenditure for an observation in time period t, with children c, and in year i
- α_c an indicator for whether or not the observation has a qualify dependent
- α_i an indicator for whether or not the observation occurs during the pandemic era
- CTC_{ict} the Child Tax Credit
- y_{ict} income
- $hsize_{ict}$ the number of people living within the observation

First difference (time): compare pre-ARP (t = 0) to post-ARP (t = 1)
** All observations occur during the pandemic era (i=1) **

$$Exp_{i,c,t=1} - Exp_{i,c,t=0} = a_c + \alpha_i + \beta_i(t=1) + \gamma_i CTC_{i,c,t=1} + \delta_i y_{i,c,t=1} + \eta_i hsize_{i,c,t=1} - [a_c + \alpha_i + \beta_i(t=0) + \gamma_i CTC_{i,c,t=0} + \delta_i y_{i,c,t=0} + \eta_i hsize_{i,c,t=0}]$$

$$\Delta_t Exp_{ict} = \beta_i t + \gamma_i\left(CTC_{i,c,t=1} - CTC_{i,c,t=0}\right) + \delta_i\left(y_{i,c,t=1} - y_{i,c,t=0}\right) + \eta_i\left(hsize_{i,ct,=1} - hsize_{i,c,t=0}\right)$$

- $\Delta_t Exp_{ict}$ the change in expenditures between pre- and post-July
- a_c drops because (on average) the presence of qualifying dependent does not change
- α_i drops because all observations occur during the pandemic era (i=1)
- $\beta_i t$ effect on expenditures from being post July
- $\left(CTC_{i,c,t=1} - CTC_{i,c,t=0}\right)$ the additional child tax credit received pre-July and post-July
- $\left(y_{i,c,t=1} - y_{i,c,t=0}\right)$ the change income pre-July and post-July
- $\left(hsize_{i,ct,=1} - hsize_{i,c,t=0}\right)$ the change in household size pre-July and post-July

Second difference (children): compare observations with (c=1) and without (c=0) children

$$\begin{aligned}\Delta_t Exp_{i,c=1,t} - \Delta_t Exp_{i,c=0,t} &= \beta_i t + \gamma_i\left(CTC_{i,c=1,t=1} - CTC_{i,c=1,t=0}\right) + \delta_i\left(y_{i,c=1,t=1} - y_{i,c=1,t=0}\right) \\ &+ \eta_i\left(hsize_{i,c=1,t=1} - hsize_{i,c=1,t=0}\right) \\ &- \left[\beta_i t + \gamma_i\left(CTC_{i,c=0,t=1} - CTC_{i,c=0,t=0}\right) + \delta_i\left(y_{i,c=0,t=1} - y_{i,c=0,t=0}\right)\right. \\ &\left. + \eta_i\left(hsize_{i,c=0,t=1} - hsize_{i,c=0,t=0}\right)\right]\end{aligned}$$

$$\Delta_{tc}Exp_{ict} = \gamma_i\left[\left(CTC_{i,c=1,t=1} - CTC_{i,c=1,t=0}\right) - \left(CTC_{i,c=0,t=1} - CTC_{i,c=0,t=0}\right)\right]$$
$$+ \delta_i\left[\left(y_{i,c=1,t=1} - y_{i,c=1,t=0}\right) - \left(y_{i,c=0,t=1} - y_{i,c=0,t=0}\right)\right]$$
$$+ \eta_i\left[\left(hsize_{i,c=1,t=1} - hsize_{i,c=1,t=0}\right) - \left(hsize_{i,c=0,t=1} - hsize_{i,c=0,t=0}\right)\right]$$

$$\Delta_{tc}Exp_{ict} = \gamma_i\left(CTC_{i,c=1,t=1} - CTC_{i,c=1,t=0}\right)$$
$$+ \delta_i\left[\left(y_{i,c=1,t=1} - y_{i,c=1,t=0}\right) - \left(y_{i,c=0,t=1} - y_{i,c=0,t=0}\right)\right]$$
$$+ \eta_i\left[\left(hsize_{i,c=1,t=1} - hsize_{i,c=1,t=0}\right) - \left(hsize_{i,c=0,t=1} - hsize_{i,c=0,t=0}\right)\right]$$

- $\Delta_{tc}Exp_{ict}$ the difference in expenditures between pre- and post-July for an observation with children compared to the difference in expenditures between pre-and post-July for an observation without children
  - The level difference between observations with children and without children of the level different between pre- and post-July
  - Did expenditures between pre- and post-July for observations with children change at a different rate than observations without children
- $\left(CTC_{i,c=1,t=1} - CTC_{i,c=1,t=0}\right)$ the additional child tax credit received pre-July and post-July
  - $\left(CTC_{i,c=0,t=1} - CTC_{i,c=0,t=0}\right)$ drops out of the equation because $C_{i,c=0,t=1} = CTC_{i,c=0,t=0} = 0$
- $\left[\left(y_{i,c=1,t=1} - y_{i,c=1,t=0}\right) - \left(y_{i,c=0,t=1} - y_{i,c=0,t=0}\right)\right]$ is the change in pre- and post-July income for observations with children compared to the change in pre- and post-July income for observations without children
  - If you believe there is zero change pre- and post-July income for both observations with children and observations without children then this effect will drop out of the diff-in-diff equation
  - If you believe the change pre- and post-July income for observations with children is the same as the change for observations without children then this effect will drop out of the diff-in-diff equation
  - If you believe the change pre- and post-July income for observations with children is different from the change for observations without children then this effect will not drop out of the diff-in-diff equation and the treatment effect estimate will be biased
- $\left[\left(hsize_{i,c=1,t=1} - hsize_{i,c=1,t=0}\right) - \left(hsize_{i,c=0,t=1} - hsize_{i,c=0,t=0}\right)\right]$ is the change in pre- and post-July household size for observations with children compared to the change in pre- and post-July household size for observations without children
  - If you believe there is zero change pre- and post-July household size for both observations with children and observations without children then this effect will drop out of the diff-in-diff equation
  - If you believe the change pre- and post-July household size for observations with children is the same as the change for observations without children then this effect will drop out of the diff-in-diff equation
  - If you believe the change pre- and post-July household size for observations with children is different from the change for observations without children then this effect will not drop out of the diff-in-diff equation and the treatment effect estimate will be biased

- If you believe δ_i and/or η_i do not drop out of the diff-in-diff equation then the treatment effect will be biased
  - The bias is a result of observations with children being fundamentally different than observations without children
  - To address this, we can alter the diff-in-diff to compare only observations with children

First difference (time): compare pre-ARP (t = 0) to post-ARP (t = 1)

** This is the same as before except all observations have a qualifying dependent (c=1) **

$$Exp_{i,c,t=1} - Exp_{i,c,t=0} = a_c + \alpha_i + \beta_i(t=1) + \gamma_i CTC_{i,c,t=1} + \delta_i y_{i,c,t=1} + \eta_i hsize_{i,c,t=1} - [a_c + \alpha_i + \beta_i(t=0) + \gamma_i CTC_{i,c,t=0} + \delta_i y_{i,c,t=0} + \eta_i hsize_{i,c,t=0}]$$

$$\Delta_t Exp_{ict} = \beta_i t + \gamma_i\left(CTC_{i,c,t=1} - CTC_{i,c,t=0}\right) + \delta_i\left(y_{i,c,t=1} - y_{i,c,t=0}\right) + \eta_i\left(hsize_{i,ct,=1} - hsize_{i,c,t=0}\right)$$

- $\Delta_t Exp_{ict}$ the change in expenditures between pre- and post-July
- a_c drops because all observations have a qualifying dependent (c=1)
- α_i drops because (on average) the observations in pandemic era do not change
- $\beta_i t$ effect on expenditures from being post July
- $\left(CTC_{i,c,t=1} - CTC_{i,c,t=0}\right)$ the additional child tax credit received pre-July and post-July
- $\left(y_{i,c,t=1} - y_{i,c,t=0}\right)$ the change income pre-July and post-July
- $\left(hsize_{i,ct,=1} - hsize_{i,c,t=0}\right)$ the change in household size pre-July and post-July

Second difference (pandemic era): compare observations in the pandemic era (i=1) to observations in the pre-pandemic era (i=0)

$$\begin{aligned}\Delta_t Exp_{i=1,c,t} - \Delta_t Exp_{i=0,c,t} &= \beta_i t + \gamma_i\left(CTC_{i=1,c,t=1} - CTC_{i=1,c,t=0}\right) + \delta_i\left(y_{i=1,c,t=1} - y_{i=1,c,t=0}\right) \\ &+ \eta_i\left(hsize_{i=1,c,t=1} - hsize_{i=1,c,t=0}\right) \\ &- \left[\beta_i t + \gamma_i\left(CTC_{i=0,c,t=1} - CTC_{i=0,c,t=0}\right) + \delta_i\left(y_{i=0,c,t=1} - y_{i=0,c,t=0}\right)\right. \\ &\left.+ \eta_i\left(hsize_{i=0,c,t=1} - hsize_{i=0,c,t=0}\right)\right]\end{aligned}$$

$$\begin{aligned}\Delta_{tc} Exp_{ict} = \gamma_i&\left[\left(CTC_{i=1,c,t=1} - CTC_{i=1,c,t=0}\right) - \left(CTC_{i=0,c,t=1} - CTC_{i=0,c,t=0}\right)\right] \\ &+ \delta_i\left[\left(y_{i=1,c,t=1} - y_{i=1,c,t=0}\right) - \left(y_{i=0,c,t=1} - y_{i=0,c,t=0}\right)\right] \\ &+ \eta_i\left[\left(hsize_{i=1,c,t=1} - hsize_{i=1,c,t=0}\right) - \left(hsize_{i=0,c,t=1} - hsize_{i=0,c,t=0}\right)\right]\end{aligned}$$

$$\begin{aligned}\Delta_{tc} Exp_{ict} = \gamma_i&\left(CTC_{i=1,c,t=1} - CTC_{i=1,c,t=0}\right) \\ &+ \delta_i\left[\left(y_{i=1,c,t=1} - y_{i=1,c,t=0}\right) - \left(y_{i=0,c,t=1} - y_{i=0,c,t=0}\right)\right] \\ &+ \eta_i\left[\left(hsize_{i=1,c,t=1} - hsize_{i=1,c,t=0}\right) - \left(hsize_{i=0,c,t=1} - hsize_{i=0,c,t=0}\right)\right]\end{aligned}$$

- $\Delta_{tc} Exp_{ict}$ the difference in expenditures between pre- and post-July for an observation in the pandemic era compared to the difference in expenditures between pre-and post-July for an observation in the pre-pandemic era
    - The level difference between observations in the pandemic era and pre-pandemic era of the level different between pre- and post-July
    - Did expenditures between pre- and post-July for observations in the pandemic era change at a different rate than observations in the pre-pandemic era
- $\left(CTC_{i=1,c,t=1} - CTC_{i=1,c,t=0}\right)$ the additional child tax credit received pre-July and post-July
    - $\left(CTC_{i=0,c,t=1} - CTC_{i=0,c,t=0}\right)$drops out of the equation because $C_{i=0,c,t=1} = CTC_{i=0,c,t=0} = 0$
- $\left[\left(y_{i,c=1,t=1} - y_{i,c=1,t=0}\right) - \left(y_{i,c=0,t=1} - y_{i,c=0,t=0}\right)\right]$ is the change in pre- and post-July income for observations in the pandemic era compared to the change in pre- and post-July income for observations in the pre-pandemic era
    - If you believe there is zero change pre- and post-July income for both observations in the pandemic era and observations in the pre-pandemic era then this effect will drop out of the diff-in-diff equation
    - If you believe the change pre- and post-July income for observations in the pandemic is the same as the change for observations in the pre-pandemic era then this effect will drop out of the diff-in-diff equation
    - If you believe the change pre- and post-July income for observations in the pandemic era is different from the change for observations in the pre-pandemic era then this effect will not drop out of the diff-in-diff equation and the treatment effect estimate will be biased
- $\left[\left(hsize_{i=1,c,t=1} - hsize_{i=1,c,t=0}\right) - \left(hsize_{i=0,c,t=1} - hsize_{i=0,c,t=0}\right)\right]$ is the change in pre- and post-July household size for observations in the pandemic era compared to the change in pre- and post-July household size for observations in the pre-pandemic era
    - If you believe there is zero change pre- and post-July household size for both observations in the pandemic era and observations in the pre-pandemic era then this effect will drop out of the diff-in-diff equation
    - If you believe the change pre- and post-July household size for observations in the pandemic is the same as the change for observations in the pre-pandemic era then this effect will drop out of the diff-in-diff equation
    - If you believe the change pre- and post-July household size for observations in the pandemic era is different from the change for observations in the pre-pandemic era then this effect will not drop out of the diff-in-diff equation and the treatment effect estimate will be biased
- If you believe δ_i and/or η_i do not drop out of the diff-in-diff equation then the treatment effect will be biased
    - The bias is a result of observations in the pandemic era being fundamentally different than observations in the pre-pandemic era
    - We could try to correct this by using only data from the pandemic era, but this is what we did in first diff-in-diff model.
    - There does not appear to be another way we can use on pandemic era data and create a treatment and control group without using the presence of a qualifying dependent

- Therefore, we can turn to the triple difference

$$
\begin{aligned}
(\Delta_t Exp_{i=1,c=1,t} - \Delta_t Exp_{i=1,c=0,t}) - (\Delta_t Exp_{i=0,c=1,t} - \Delta_t Exp_{i=0,c=0,t})& \\
= \{\beta_{i=1} t + \gamma_{i=1}(CTC_{i=1,c=1,t=1} - CTC_{i=1,c=1,t=0}) + \delta_{i=1}(y_{i=1,c=1,t=1} - y_{i=1,c=1,t=0})& \\
+ \eta_{i=1}(hsize_{i=1,c=1,t=1} - hsize_{i=1,c=1,t=0})& \\
- [\beta_{i=1} t + \gamma_{i=1}(CTC_{i=1,c=0,t=1} - CTC_{i=1,c=0,t=0}) + \delta_{i=1}(y_{i=1,c=0,t=1} - y_{i=1,c=0,t=0})& \\
+ \eta_{i=1}(hsize_{i=1,c=0,t=1} - hsize_{i=1,c=0,t=0})]\}& \\
- \{\beta_{i=0} t + \gamma_{i=0}(CTC_{i=0,c=1,t=1} - CTC_{i=0,c=1,t=0}) + \delta_{i=0}(y_{i=0,c=1,t=1} - y_{i=0,c=1,t=0})& \\
+ \eta_{i=0}(hsize_{i=0,c=1,t=1} - hsize_{i=0,c=1,t=0})& \\
- [\beta_{i=0} t + \gamma_{i=0}(CTC_{i=0,c=0,t=1} - CTC_{i=0,c=0,t=0}) + \delta_{i=0}(y_{i=0,c=0,t=1} - y_{i=0,c=0,t=0})& \\
+ \eta_{i=0}(hsize_{i=0,c=0,t=1} - hsize_{i=0,c=0,t=0})]\}&
\end{aligned}
$$

$$
\begin{aligned}
\Delta_{tc} Exp_{i=1,c,t} - \Delta_{tc} Exp_{i=0,c,t}& \\
= \gamma_{i=1}[(CTC_{i=1,c=1,t=1} - CTC_{i=1,c=1,t=0}) - (CTC_{i=1,c=0,t=1} - CTC_{i=1,c=0,t=0})]& \\
+ \delta_{i=1}[(y_{i=1,c=1,t=1} - y_{i=1,c=1,t=0}) - (y_{i=1,c=0,t=1} - y_{i=1,c=0,t=0})]& \\
+ \eta_{i=1}[(hsize_{i=1,c=1,t=1} - hsize_{i=1,c=1,t=0}) - (hsize_{i=1,c=0,t=1} - hsize_{i=1,c=0,t=0})]& \\
- \{\gamma_{i=0}[(CTC_{i=0,c=1,t=1} - CTC_{i=0,c=1,t=0}) - (CTC_{i=0,c=0,t=1} - CTC_{i=0,c=0,t=0})]& \\
+ \delta_{i=0}[(y_{i=0,c=1,t=1} - y_{i=0,c=1,t=0}) - (y_{i=0,c=0,t=1} - y_{i=0,c=0,t=0})]& \\
+ \eta_{i=0}[(hsize_{i=0,c=1,t=1} - hsize_{i=0,c=1,t=0}) - (hsize_{i=0,c=0,t=1} - hsize_{i=0,c=0,t=0})]\}&
\end{aligned}
$$

- $\beta_{i=1} t$ and $\beta_{i=0} t$ are both added and subtracted, so they drop out

$$
\begin{aligned}
\Delta_{tci} Exp_{i,c,t} = \gamma_{i=1}(CTC_{i=1,c=1,t=1} - CTC_{i=1,c=1,t=0})& \\
+ (\delta_{i=1} - \delta_{i=0})[(y_{i,c=1,t=1} - y_{i,c=1,t=0}) - (y_{i,c=0,t=1} - y_{i,c=0,t=0})]& \\
+ (\eta_{i=1} - \eta_{i=0})[(hsize_{i,c=1,t=1} - hsize_{i,c=1,t=0})& \\
- (hsize_{i,c=0,t=1} - hsize_{i,c=0,t=0})]&
\end{aligned}
$$

- $\Delta_{tci} Exp_{i,c,t}$ is the difference in expenditures pre- and post-July controlling for the difference in expenditures pre- and post-July for observations with and without qualifying dependents and controlling for expenditure pre- and post-July for observations in the pandemic and pre-pandemic era
- $(CTC_{i=1,c=1,t=1} - CTC_{i=1,c=1,t=0})$ the additional child tax credit received pre-July and post-July
    - $CTC_{i=1,c=0,t=1} = CTC_{i=1,c=0,t=0} = CTC_{i=0,c=1,t=1} = CTC_{i=0,c=1,t=0} = CTC_{i=0,c=0,t=1} = CTC_{i=0,c=0,t=0} = 0$
- $(\delta_{i=1} - \delta_{i=0})$ represents the difference between the pandemic era change in pre- and post-July income for observations with children compared to the pandemic era change in pre- and post-July income for observations without children and the pre-pandemic era change in pre- and post-July income for observations with children compared to the pre-pandemic era change in pre- and post-July income for observations without children
    - If you believe the difference for the pandemic era is the same as the difference in the pre-pandemic era then this term will drop out of the triple difference equation

    - This means you believe the difference in income across the year between observations with and without qualifying dependents during the pandemic era is the same as the difference in income across the year between observations with and without qualifying dependents during the pre-pandemic era
- $(\eta_{i=1} - \eta_{i=0})$ represents the difference between the pandemic era change in pre- and post-July household size for observations with children compared to the pandemic era change in pre- and post-July household size for observations without children and the pre-pandemic era change in pre- and post-July household size for observations with children compared to the pre-pandemic era change in pre- and post-July household size for observations without children
    - If you believe the difference for the pandemic era is the same as the difference in the pre-pandemic era then this term will drop out of the triple difference equation
    - This means you believe the difference in household size across the year between observations with and without qualifying dependents during the pandemic era is the same as the difference in household size across the year between observations with and without qualifying dependents during the pre-pandemic era