

Estimating Income Effects on Earnings using the 2021 Child Tax Credit Expansion

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Abstract

We estimate income effects on earnings using the 2021 expanded Child Tax Credit. Children born in January 2016 were eligible for a credit of \$3,600, while children born in December 2015 were eligible for \$3,000. We use administrative tax data and a regression discontinuity design to estimate the earnings response to this temporary, one-year \$600 increase in non-labor income. We find that employment as reported by third parties decreased by 2.0% in 2021 and 1.1% in 2022 in low-income families, with no effect on families with higher incomes. However, this decrease in wages was offset by an equivalent increase in self-employment income reported on tax returns, implying either increased misreporting or no overall change in real employment.

Keywords: labor supply, income effect, Child Tax Credit, child allowance

JEL Codes: H2, J2

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1 Introduction

In recent years, numerous experts have proposed combating child poverty with a child allowance, or guaranteed monthly income transfer for children (H. Luke Shaefer et al. 2018; National Academies of Sciences, Engineering, and Medicine 2019; Garfinkel et al. 2021; Aizer, Hoynes, and Lleras-Muney 2022; Shah and Gennetian 2023). One enduring concern with implementing this policy is whether a child allowance would crowd out parental earnings.

In 2021, the United States introduced a temporary child allowance—the expanded Child Tax Credit (CTC). The expansion increased the generosity of the credit, provided monthly advance payments, and allowed families with no earned income to receive benefits. The effect of this policy—and of a child allowance in general—on employment and earnings is theoretically ambiguous. Static models of labor supply predict a negative income effect, while models that incorporate poverty traps, credit constraints, and fixed costs of work suggest positive income effects. The expansion provoked debates among economists. Some argued that if the expanded CTC were to be made permanent, the employment effects would be negligible¹; while others warned that as many as 1.5 million working parents would exit the labor force (Corinth et al. 2021). Our paper informs this discussion by estimating the effect of a temporary “child allowance”-like program, the expanded CTC, on the earnings of low-income families.²

We exploit a discontinuity in the CTC benefit schedule around a January 1 birth date cutoff and use the regression discontinuity (RD) framework to identify the causal effect of income on parental earnings. Children under age 6 at the end of tax year 2021 were eligible for \$3,600 of CTC, while children between the ages of 6 and 17 were eligible for \$3,000. Differences between families with children just above and below age 6 on December 31st, 2021 are plausibly random, allowing us to construct an unbiased causal estimate of responses to a temporary \$600 increase in CTC income.³

We use administrative tax data from the Internal Revenue Service (IRS) on all births registered with the Social Security Administration (SSA) to identify children born within one month of the age 6 cutoff, identifying over 600,000 families with children born in December 2015 and January 2016. We follow other work, such as Barr, Eggleston, and Smith (2022), in

1. See the “Open Letter to Congress about the Expanded CTC” signed by over 400 economists, discussed at <https://www.cnbc.com/2021/09/16/over-400-economists-letter-favor-extending-300-dollar-child-tax-credits.html>.

2. Our results focus on tax units, which consist of a taxpayer, spouse (if filing jointly) and their dependents. References to “families” or “households” in this paper technically refer to tax units.

3. The shock is temporary regardless of parental expectations about the permanency of the credit. Families at the age 6 discontinuity only experienced this credit difference of \$600 in a single year. If the expanded CTC had been made permanent, families with December and January children would both be eligible for \$3,000 next year since the children from both groups would be age 6 or older.

estimating results that exclude a “donut” of 8 days around the cutoff to rule out differences between December and January children caused by shifting of births around Christmas and New Year’s Day. We link children to tax units by searching among claimed dependents on pre-2021 tax forms (Form 1040), health insurance information returns (Form 1095-A/B/C), and the SSA birth records. We then use 2021 and 2022 tax returns and information returns to study the effect of additional CTC income on parental earnings.

The primary variation studied in this paper allows us to identify an income effect. The income effect is not the total employment response to the expanded CTC because the policy also eliminated the portion of the 2020 CTC where the credit increased with earnings (Lippold 2019). This increased marginal tax rates and introduced a substitution effect (see Corinth et al. (2021) and Goldin, Maag, and Micheltore (2022) for a full discussion on both the income and substitution effects and simulations of the employment response from converting the existing CTC into a child allowance). In our sample, families with children born in December 2015 and January 2016 faced the same change in marginal tax rates when the CTC was expanded. Our main results allow us to identify the likely labor supply consequences of a child allowance passed independently of the current (non-expanded) CTC.

For low-income families (after-tax income under \$30,000 in 2020), an additional \$600 of CTC benefits constituted a 3.0% increase in after-tax income. Our main finding is that eligibility for an additional \$600 decreased the probability of any adult in the household working for an employer in 2021 by 1.4 percentage points (2.0%), and by 0.8 percentage points (1.1%) in 2022 (not statistically significant). This translates to an elasticity of extensive margin earnings with respect to after-tax income of -0.50, which is larger than previously found in the literature. This means that, on average, a 10% increase in after-tax income from a temporary shock would decrease household labor market participation by 5%. Aggregating across all families within our sample, the elasticity is -0.34. However, we find a potentially offsetting effect from self-employment among income reported on tax returns. Low-income families increased the probability of reporting any self-employment income by 1.3 percentage points (4.9%) in 2021 and by 1.9 percentage points (6.3%) in 2022.

Results are robust to alternate specifications, including varying the donut size and the kernel weights, changing the definition of low-income, and using probit and logit regressions with the binary outcomes. Placebo tests provide additional support for our results. There are no earnings responses among higher income groups, which is expected since the \$600 CTC treatment at the cutoff is very small in proportion to their total income.

Furthermore, a second policy change at the discontinuity provides evidence about the possible employment effects of a child allowance policy that also increases the subsidy for work. Families in California with children under age 6 in 2021 were eligible for the state

Young Child Tax Credit (YCTC), which provided \$1,000 of benefits conditional on earning at least \$1. Thus, families with their youngest child at the age 6 cutoff in California faced two incentives – \$600 of additional income from the federal CTC unconditional on work and \$1,000 of subsidy income from the state YCTC that required work. Among families in California, we observe an income effect combined with a substitution effect. Though it is not possible to separately identify the income and substitution effect, this policy detail provides an opportunity to examine the effects of a child allowance combined with an increased subsidy to work, such as proposals to pair an increased CTC with an increased Earned Income Tax Credit (EITC) (Bastian 2023). Overall, this large incentive to work had no statistically significant effect (echoing findings by Unrath (2023) for the YCTC’s effects before 2021). Yet, among the low income families who are married, the additional subsidy increased employment and earnings of married men.⁴ That result highlights a possibly unintended implication of incentivizing work: the incidence of benefits falls mostly on married couples with the flexibility to increase employment.

We contribute to the literature on the labor supply consequences of child benefit programs by evaluating a similar, temporary policy in the United States. Our extensive margin income elasticity of -0.34 is below the elasticities of -0.5 to -0.81 (depending on the specification) reported by Mazar and Reingewertz (2023) in Israel. Other papers on labor supply and child allowances has studied programs in Argentina (Maurizio and Vázquez 2014), Canada (Schirle 2015), Poland (Magda, Kielczewska, and Brandt 2020; Gromadzki 2023), and South Africa (Bengtsson 2012). Our results are consistent with the prior work in that we also find a negative effect on labor supply.⁵

We estimate a key parameter needed in the literature that models the labor supply response to a permanent child allowance in the United States.⁶ In their simulations of the effect of a permanent child allowance, Corinth et al. (2021), National Academies of Sciences, Engineering, and Medicine (2019), and Goldin, Maag, and Michelmore (2022) use a labor force participation elasticity with respect to income of -0.085 for single parents, -0.05 for married men, and -0.12 for married women.⁷ These estimates originate from a detailed review of the literature conducted by Blundell and Macurdy (1999). We find that

4. The result does not hold in all specifications.

5. The magnitude of the effect is difficult to compare across studies since each program differs in scope and context.

6. There is some conceptual distinction between *labor supply* elasticities (measuring changes in hours of work) and *earnings* elasticities (which are a sufficient statistic for all behavioral responses, per Feldstein (1999)). In our data, we only observe earnings. However, assuming that zero earnings implies zero hours worked, we can compare our estimates of the extensive margin earnings elasticity to the extensive margin labor supply elasticity from other papers (as long as both are reported with respect to after-tax income).

7. Corinth et al. (2021) use a combined elasticity of -0.05 for married parents.

the extensive margin income elasticity among families with young children in the United States is -0.34 (driven by low-income families with an elasticity of -0.5). Our estimate is relevant because it is recent, estimated on the main population of interest (families with children), and uses variation in child benefits. More recently, the income effect has been studied using lotteries (Imbens, Rubin, and Sacerdote 2001; Golosov et al. 2021), inheritances (Holtz-Eakin, Joulfaian, and Rosen 1993), stimulus payments (Powell 2020), and cash welfare (Hoynes 1996; Gelber, Moore, and Strand 2017). The advantage of our paper is that we focus specifically on families with children, who are frequent targets of social safety net programs in the United States (Aizer, Hoynes, and Lleras-Muney 2022). The parameter we estimate is of great importance to calculating fiscal impacts of any program that includes unconditional cash transfers to families with children. For instance, our results can inform how earnings respond to increased generosity of a tax credit or welfare benefits.

The results of this paper contrast with prior work that concluded that the expanded CTC had no effects on labor supply by identifying a meaningful, negative employment response among an important subpopulation — low-income families. Numerous studies (Ananat et al. 2022; Hamilton et al. 2022; Lourie et al. 2022; Pilkauskas et al. 2022; Enriquez, Jones, and Tedeschi 2023) find that the 2021 expansion of the Child Tax Credit did not decrease labor supply. Our results are distinguishable from the current CTC literature because of our data source and empirical strategy. We use administrative tax records, and can observe the exact date of birth for each child, which allows us to employ the RD research design making comparisons between two extremely close groups. Our data includes all income earned in the formal labor market for all households. Since we use information returns, such as forms W-2 and 1099-NEC, our analysis is not limited to households that file taxes. We construct a panel of earnings before and after the expansion of the credit, while most surveys are limited to cross-sectional analysis; this allows us to focus on the pre-policy low-income households who have the strongest response to the CTC expansion. By using administrative tax records for the universe of children born near the benefit discontinuity, we estimate a less biased and more precise measure of the effect of the expanded CTC on employment and earnings.

The paper proceeds as follows. In section 2, we provide some background on the Child Tax Credit. In section 3, we present a framework that specifies the parameter of interest. In section 4, we describe our data and outcomes. In section 5, we present our empirical method and justify its validity. In section 6, we summarize the main results. In section 7, we conclude.

2 Background on the Child Tax Credit

In 1997, the Taxpayer Relief Act created the Child Tax Credit, which offered \$400 in tax reduction to households for each qualifying child under age 17 (Crandall-Hollick 2018). At its inception, the credit was not refundable (it could only be used to cover existing tax liabilities), making it unavailable to most low-income taxpayers. Over the next two decades, Congress increased the CTC benefit amount and expanded its reach to more households. By 2020, the credit was \$2,000 per child, and was available to more low-income families; the refundable portion of the credit phased in with earned income at a 15% rate for households earning over \$2,500.

In March 2021, the American Rescue Plan (ARP) passed. This pandemic aid package expanded the CTC, but only for tax year 2021. The expansion nearly doubled the credit and made it available to more families, including households who do not earn any income.⁸

Two features of the new 2021 expanded CTC schedule, which is illustrated in **Figure 1**, are key for this analysis. First, the ARP created two discrete levels of benefit that were a direct function of the child’s age. Children who were five years old or younger on December 31, 2021 were eligible for \$3,600. Children who were between age 6 and 17 (inclusive) on that date were eligible for \$3,000. A larger credit was available for young children based on research that they are more sensitive to the negative effects of poverty (Duncan et al. 1998; Duncan et al. 2012; National Academies of Sciences, Engineering, and Medicine 2019). Second, the benefit amount was constant (the flat “plateau” of the schedule) from \$0 to \$112,500 of income for unmarried parents who file taxes as head of household, and to \$150,000 for married parents who file jointly. These two features allow us to study the difference of \$600 in non-labor income for a large number of children.

The ARP also changed how the credit was distributed. Traditionally, to claim a tax credit for a given year, households need to file a tax return for that tax year, and then receive any refund after the return is processed (in February or later of the following tax year). In 2021, up to half of the expanded CTC was available in advance of the annual tax season through monthly payments of the “Advance CTC.” For families who filed a tax return for 2019 or 2020, or who used the non-filer portal on the IRS website, the IRS automatically issued a payment (by check or direct deposit to a bank account on file). Those monthly payments arrived from July 2021 until December 2021.⁹

8. **Figure A1** shows how the CTC expansion changed the benefit schedule relative to 2020.

9. Families who did not receive the payment had the opportunity to sign up to receive it on the IRS website. Families who received monthly payments but did not want them could also opt out on the IRS website. One reason why families may have opted out is they anticipated a tax liability at tax filing, and preferred to use the entire credit to cover that liability instead of paying for it out of pocket. A family may also have changed composition (such as a divorce or child moving out) and wanted to avoid the CTC being

2.1 California’s Young Child Tax Credit

Since 2019, the Young Child Tax Credit (YCTC), a California state credit, offered \$1,000 to families with a child under age 6 – the same age cutoff as the federal expanded CTC. The YCTC could be claimed by families who made between \$1 and 30,000 of earned income.¹⁰ Most importantly, families with no earned income could not claim the credit.

Thus, families with their youngest child born in January 2016 (or later) could claim the YCTC in California for an extra year relative to December 2015 children, but only if they worked. **Figure 2** shows the combined benefit of the federal CTC and the YCTC. This created an extra incentive to work in tax year 2021. The structure of the YCTC increases the return to any work (even \$1 of earned income) discontinuously, which lowers average tax rates and incentivizes work on the participation margin (Eissa, Kleven, and Kreiner 2008). We would expect that in California, where the YCTC creates an incentive to work that counteracts the negative income effect of the federal expanded CTC, the extensive margin earnings response would be less negative than in other parts of the country.

3 Framework

This section summarizes the predicted change to labor supply from an increase in unearned income (such as a child allowance or an increase to a nonrefundable tax credit). In a standard model of the consumption-labor tradeoff, households choose leisure $l \in [0, 1]$ and consumption $c \in [0, \infty]$ to maximize their utility:

$$U(l, c) \quad \text{subject to} \quad c = T + w(1 - l)$$

where w is wage and T is nonlabor income or wealth. The price of c is numeraire. The utility function $U(c, l)$ is differentiable, with $U'(c) > 0$ and $U'(l) > 0$.

The optimal bundle $(x^*(w, T)$ and $l^*(w, T))$ at any given w and T can be found by using the Lagrangian method of constrained optimization:

$$\begin{aligned} \frac{dU}{dc} &= \lambda \\ \frac{dU}{dl} &= \lambda w \end{aligned}$$

where λ is the Lagrangian multiplier, which is the shadow price or the marginal utility of

clawed back.

10. Households who claim the California EITC (CalEITC) could claim the YCTC if they had a qualifying child. In 2021, CalEITC was available to families if they made between \$1 and \$30,000 of earned income.

income. Without further assumptions on the utility function, there is no closed form solution for $x^*(w, T)$ and $l^*(w, T)$. However, we arrive at the standard result that the optimal bundle is located where the marginal rate of substitution is equal to w :

$$MRS_{lc} = -\frac{dc}{dl} = \frac{dU/dl}{dU/dc} = \frac{MU_l}{MU_c} = w$$

If the previous equality does not hold for any combination of (l, c) where $c = T + w(1 - l)$, the optimal bundle is located at a corner. In the case when $MRS_{lc} < w \forall l \in (0, 1)$, the household's optimal consumption bundle is $l^* = 0$ and $c^* = T + w$. In the case when $MRS_{lc} > w \forall l \in (0, 1)$, the household's optimal consumption bundle is $l^* = 1$ and $c^* = T$.

To summarize:

$$(l^*(w, T), c^*(w, T)) = \begin{cases} (1, T), & \text{if } MRS_{lc} > w \\ (0, T + w), & \text{if } MRS_{lc} < w \\ (l', c'), & \text{otherwise} \end{cases}$$

where $c' \in (T, w + T)$ and $l' \in (0, 1)$ are real numbers.

It follows that each household has an indirect utility function which can be written as:

$$\begin{aligned} V(w, T) &= \{U(c, l) | c = T + w(1 - l)\} \\ &= U(c^*(w, T), l^*(w, T)) \end{aligned}$$

Thus when non-wealth income increases, the household will increase utility and locate at a higher indifference curve, as long as c is a normal good ($\frac{dc}{dT} \geq 0$) and l is a normal good ($\frac{dl}{dT} \geq 0$).

$$\frac{dU(c^*(w, T), l^*(w, T))}{dT} = \frac{dU}{dc} \frac{dc}{dT} + \frac{dU}{dl} \frac{dl}{dT}$$

Figure 3 illustrates the effect of a cash transfer (dT) on a household's budget constraint. An increase in income ($dT > 0$) shifts the budget constraint out, parallel to the old budget constraint since the wage rate is constant.

Households that chose not to work under the old budget constraint will also choose not to work under the new budget constraint. Recall, a household locates at point A under the old budget constraint iff $MRS_{lc} > w$. If indifference curves are convex, this shift would (strictly) decrease the $MRS_{lc}^{new} < MRS_{lc}$ since $MU_c^{new} < MU_c \forall c$ because of the diminishing marginal utility to consumption, and $MU_l^{new} = MU_l \forall l$. Thus, with the additional (reasonable)

assumption of diminishing marginal utility, a household that originally located at point A would locate at point B.

All agents who were previously working under the old budget constraint will increase their consumption of leisure, and some will stop working entirely. If consumption and leisure are both normal goods, a household with an optimal bundle located at point D would decrease consumption and leisure, again equating $MRS_{lc} = w$, to locate at a higher indifference curve at point E. An household that located at point C under the old budget constraint may completely stop working after the cash transfer. This would occur if the cash transfer was large enough to decrease the agent's $MRS_{lc}^{new} < MRS_{lc}$ such that $MRS_{lc}^{new} < w \forall l$ even though $MRS_{lc} = w$ at some bundle (l, c) .

Therefore, the standard model of the labor-leisure tradeoff predicts that there will be a decrease in labor supply on both the extensive and intensive margin.

4 Data description

Our primary data source is administrative tax records from the IRS. First, we create a dataset of all children born in December 2015 and January 2016 (and thus around the age 6 cutoff in 2021) with births registered with the SSA. Through the Enumeration At Birth program, the SSA assigns Social Security Numbers (SSNs) to most children born in the U.S., which are linked to the SSNs of their parents and transmitted to the IRS to monitor tax compliance. Using this data allows us to include children in our overall universe regardless of whether their parents file taxes. There are approximately 650,000 children born in December 2015 and January 2016, approximately 10,000 for each day of birth.

We then link parents to children by searching for that child among claimed dependents on 2020 tax returns (Form 1040). If found, the primary and secondary (if filing jointly) taxpayers on that tax return are linked to the child; this captures 91% of children. If not found, we search for taxpayers who claimed the child in earlier years (the most recent return from 2016-2019); this links 4% of children. If the linked parents could not be identified from a tax return, we search for the child among health insurance coverage information returns (Form 1095), indicating coverage and the SSN of the policyholder from 2016 to 2020. Searching among information returns is a natural next step since the IRS receives these forms even if a child is not claimed on a tax return; this links 1% of children.¹¹ If the child was never claimed on a tax return or health insurance information return during their lifetime, the SSNs of the mother (or father if mother is missing) that were reported on the

11. Form 1095 is the only information return that consistently contains data on children, who typically do not earn income.

birth certificate are linked to the child; this links 1.5% and 0.25% of children, respectively.¹² Once a parent is identified, we link parental information returns and tax returns to the child records. **Table 1** shows descriptive statistics for our sample.

We use the total amount of CTC claimed, the total amount of Advance CTC received as monthly payments, and a simulated instrument after-tax income as the “first stage” to confirm the validity of our empirical strategy. The total amount of CTC (including the Credit for Other Dependents, less repayments of the Advance CTC) and the amount received in monthly payments are taken directly from the administrative data. We calculate after-tax income by using tax and information returns from 2020, aging the dependents in a household up by one year, and applying the 2021 tax code (which includes the expanded CTC) using Taxsim (Feenberg and Coutts 1993).¹³ Using 2020 tax information as a simulated instrument, as in Gruber and Saez (2002), allows us to calculate the percentage increase in after-tax income from the CTC independent of behavioral responses to the 2021 tax code.

Our main outcomes are annual measures of total earnings (in dollars), employment (an indicator for any earnings), and logged earnings. Earnings are constructed from Form W-2 and Form 1099-NEC information returns. These third-party reported forms are valuable because they are less subject to selective reporting by taxpayers.

We also study outcomes reported on tax returns, including reported wages, business income from Schedule C, self-employment income reported on Schedule SE, and expenses claimed for the Child and Dependent Care Credit. The outcomes from tax returns are available only for families who filed taxes, and could be more subject to misreporting because they are not third-party verified. For these reasons, earnings as reported on Form W-2 and 1099-NEC are preferred as our primary outcome. We also study unemployment compensation earnings (reported by state unemployment offices to the IRS on Form 1099-G), which are available for non-filers and of particular interest given high rates of unemployment compensation receipt in 2021 due to the COVID-19 pandemic.

To focus on families with the strongest theoretical response to the expanded CTC, we divide our analysis sample into income groups based on 2020 after-tax income. **Figure 4** shows the distribution of after-tax income in 2020. We focus on low-income families (with income of \$0-\$30,000 in 2020), roughly the bottom quartile of income.

12. Children who are never claimed on a tax return or Form 1095 and have no parent SSN in the birth certificate data are dropped (1.75% of all recorded births). We also drop the small number of children who are linked to more than one tax return, as well as children who died before 2021.

13. Our measure of total household income includes all income sources reported on tax or information returns (including wages, business income, interest and dividends, capital gains, Social Security payments, pensions, and unemployment compensation). We do not observe nontaxable transfer income (such as SNAP, TANF, and SSI) that is not reported to the IRS, so these income sources are excluded from our measure.

5 Empirical Method

To estimate the causal effect of non-labor income on earnings, we use variation in the expanded CTC benefit schedule. Children who are born on or after January 1, 2016 were eligible for \$3,600 of the CTC in 2021; children born before that date are eligible for \$3,000. This variation based on birth date creates a natural experiment that allows us to study the effects of eligibility for additional income on employment and earnings.

5.1 Specification

Our preferred specification is a regression discontinuity (RD) design with a lagged dependent variable and pre-treatment covariates for precision. We estimate the following equation:

$$Y_{i,t} = \alpha + \beta_1 Treat_i + \beta_2 Age_i + \beta_3 Treat_i \times Age_i + \rho Y_{i,2020} + \mathbf{X}_{i,2020} + u_{i,t} \quad (1)$$

where $Y_{i,t}$ is the outcome for parents of child i in year t . $Treat_i$ is an indicator for whether child i was born on or after January 1, 2016. Age_i is the number of days between the child’s birth date and January 1, 2016. $\mathbf{X}_{i,2020}$ is a set of household-level covariates observed in 2020 that include state fixed effects, single (versus married filing jointly), presence of a female parent, number of children, quadratic ages of adults (separately by gender, set to zero if no adult of that gender), and indicators for whether an adult in the household was a student (received Form 1098-T), collected Social Security Disability Income (Form SSA-1099, excluding retirement benefits), had a mortgage (Form 1098), received dividend income (Form 1099-DIV), or received interest income (Form 1099-INT). $u_{i,t}$ is an additive error term.

The coefficient of interest in equation (1) is β_1 , the intent-to-treat estimator. It is the causal effect of eligibility for an additional \$600 of CTC on outcome $Y_{i,t}$. We use the logic of a sharp RD; since take-up of the CTC is endogenous, we estimate the effect of *eligibility* for additional income and do not scale coefficients by the proportion of families that received the CTC. We expect β_1 to be negative for all the main outcomes.

The main assumption needed to estimate a causal effect is that individuals do not have precise control over the treatment assignment. An implication of this assumption is that the difference between children born directly to the left and right of the cutoff is “as good as random” (Lee and Lemieux 2010). An important feature of the January 1 cutoff is the presence of Christmas and New Year’s Day, which empirically lead to some shifting of births away from those holidays. Additionally, Dickert-Conlin and Chandra (1999), Schulkind and Shapiro (2014), and LaLumia, Sallee, and Turner (2015) find evidence that parents schedule C-sections and inductions to take advantage of tax credits available to children who are born

in December that would not be available to children born in January until the following year. We follow other work, such as [Barr, Eggleston, and Smith \(2022\)](#) and [Rittenhouse \(2023\)](#), in estimating results that exclude a “donut” of 8 days around the cutoff to rule out differences between December and January children caused by these timing shifts.

To ensure identification of the causal effect of an additional \$600 of CTC, we limit our analysis sample to children who had no other discontinuities at the cutoff. Notable discontinuities that are concerning are kindergarten enrollment cutoffs and other tax credits that have age cutoffs. According to the National Center of Education Statistics, the statewide kindergarten attendance cutoff in Connecticut is “5 on or before January 1st”.¹⁴ Additionally, Massachusetts, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, and Vermont have kindergarten attendance cut-offs that are “up to local authorities”. Since access to childcare (through school) affects labor supply decisions, we exclude families who lived in Connecticut in 2020 or 2021 from our main analysis. In an additional check, we ensure that our results are robust to the exclusion of children living in the states where the kindergarten cutoff is up to local authorities. Because of the additional discontinuity of the YCTC, we exclude households who lived in California in 2020 or 2021 from our main analysis sample, but we do examine the effect of both programs combined in a separate specification.

We study annual labor supply responses in 2021 and 2022. Although households can anticipate with certainty the extra cash after March 2021, the credit is partially collected in 2021 (through monthly payments), and partially in 2022 (when filing a return for tax year 2021). In theory, the timing of a payment should not change behavior; households should optimize on information and not on realizations of changes to their budget constraint. If households are myopic and liquidity constrained, the timing of receipt of benefit payments and tax credits matters. For instance, consumption increases in the days after social security checks are disbursed ([Stephens 2003](#)), and the Earned Income Tax Credit, a tax credit for low-income taxpayers, increases consumption on both durable and non-durable goods during tax season when refund checks are delivered ([Barrow and McGranahan 2000](#)). In this paper, we anticipate behavioral responses to the expanded Child Tax Credit to be spread over two tax years, 2021 and 2022.

In addition to regressions that estimate the effect of eligibility for an additional \$600 on earnings by year, we estimate pooled regressions that estimate the *average* earnings responses in 2021 and 2022. We expect the effects of this relatively small, temporary shock to income to be concentrated in 2021 and 2022 since the policy was announced and realized during these two years. For the modal parent, half of the credit was claimed in 2021 and half of the credit was claimed in 2022, so averaging the effect across the two years is reasonable.

14. https://nces.ed.gov/programs/statereform/tab5_3.asp

Last, we explore heterogeneity of our results across parental and household characteristics. We use a difference in regression discontinuity design (DiRD) with a lagged dependent variable and pre-treatment covariates, and estimate the following equation:

$$\begin{aligned}
Y_{i,t} = & \alpha + \delta_1 Treat_i + \delta_2 Age_i + \delta_3 Treat_i \times Age_i \\
& + \delta_4 Treat_i \times D_i + \delta_5 Age_i \times D_i + \delta_6 Treat_i \times Age_i \times D_i \\
& + \delta_7 D_i + \rho Y_{i,2020} + \mathbf{X}_{i,2020} + u_{i,t}
\end{aligned} \tag{2}$$

where D_i is a heterogeneity variable of interest, and the other variables are defined the same as in equation (1). We interact D_i with Age_i and $Age_i \times Treat_i$ to allow the slope to vary flexibly for both groups on both sides of the cutoff.

The coefficient of interest in equation (2) is δ_4 . It captures the differential effect of eligibility for an additional \$600 among families with characteristic D_i . Like in the standard regression discontinuity design, our identifying assumption is that individuals in both groups do not have precise control over the treatment assignment.

We employ equation (2) to study the additional effect of California’s YCTC on the earnings response. In this case, D_i is an indicator for whether the family lived in California in 2020.¹⁵ Using the DiRD, we identify the differential earnings response to the incentives in California created by a double natural experiment: the YCTC and the federal expanded CTC. For the YCTC, children under age 6 who are the youngest child in that tax unit are eligible for an additional \$1,000 conditional on work. Under the assumption that the response to \$600 is the same in California as in the rest of the country, δ_4 identifies the earnings response to the Young Child Tax Credit, a policy that incentivizes work by decreasing average tax rates.¹⁶ We expect δ_1 to be negative and δ_4 to be positive, if leisure is a normal good.

In the next section, we argue that the use of the regression discontinuity design is relevant for our setting. Then, since the regression discontinuity approach is invalid if births can be manipulated precisely, we argue empirically for the validity of our regression discontinuity approach with a “donut” before proceeding to the results in Section 6.

15. We continue to exclude Connecticut from the regression.

16. The Young Child Tax Credit is available *per family*, not *per child*. For families with children younger than the focal child (25% of our sample in California), the policy is *not binding*—i.e families in the treatment and control group are both eligible for the Young Child Tax Credit and the \$600 differential in federal expanded CTC. Under the assumption that the earnings response among these families is the same, our estimate of δ_4 is attenuated towards zero. In an appendix table, we show the results of a triple DiRD with a full set of interactions between indicators for January births, California, and whether child i has younger siblings, but we have limited power to detect differential responses in these smaller subsamples.

5.2 Relevance

A discontinuity in CTC benefit amount is a crucial first stage for identification. According to the American Rescue Plan as written into law, children under age 6 were eligible for a credit of \$3,600 and children between age 6 to 17 were only eligible for \$3,000. **Figure 5** illustrates a clear discontinuity in the total amount of CTC claimed at the January 1st cutoff, among all households and low-income families in our data. The average household in our sample has 2.28 children, so the baseline amount of CTC claimed is over \$6,000. **Table 2** reports that the average family with a child born in December 2015 collected \$6,221 of CTC, while the average family with a child born just a few weeks later in January 2016 collected an additional \$483. According to the American Result Plan, half of the CTC was available through advance monthly payments; **Table 2** confirms that the average family with a child born in January collected \$263 more in advance credits.

We focus our analysis on low-income families, where eligibility for an additional \$600 represents a larger shock as a share of income. **Table 2** reports the average change in after-tax simulated income as a result of CTC eligibility, as computed using Taxsim with 2020 income and 2021 tax law. Low-income families with a child born in January are eligible for a 3.0% increase in after-tax income (\$622) relative to families with children born in December. **Appendix Table A1** shows results for other income groups; for middle-income families, the percentage difference in after-tax income is a statistically significant 1.2%, for higher income families the difference is 0.54%, and for very high-income families, the difference is small and not statistically significant.

The difference in amount of CTC claimed is less than the \$600 discontinuity proscribed by the legislation due to incomplete take-up and changes in eligibility. First, not all eligible families took up the credit. 17.7% of our low-income sample did not receive any advance monthly payments, and 17.9% of our sample did not file a tax return for 2021, which is how one would claim the second half of the expanded CTC. Additionally, not all families in our sample were eligible. We included all households with a child born within a month of the January 1st cutoff in our sample, but some families may be ineligible due to their high income.¹⁷ Other households may be ineligible because of changes in household composition. We constructed family units using 2020 information to avert endogeneity concerns, but the linked parents may no longer have been living with their children in 2021.¹⁸ We test whether

17. Households that made more than \$150,000 (or \$182,500 if married filing jointly) do not face a benefit schedule with an age discontinuity (see **Figure 1**)

18. For instance, if a child was never claimed on a tax return, we assume that the child lives with their birth mother, which could add measurement error. Similarly, if a child was claimed by a taxpayer in 2020 but then moves out of that household in 2021 (e.g. cases of foster children or divorce), we use the 2020 taxpayer's outcomes, but their tax unit is not eligible to claim the CTC for that child and would have to

there are any statistically significant differences in the linking procedure between families of children born in December and January. We do not find evidence for any differences across the cutoff; while our linking procedure may be noisy, it is noisy in the same way on both sides of the cutoff.

5.3 Validity

The biggest threat to our RD identification strategy is the manipulation of birth timing. It is widely accepted in the literature that births, though a natural biological process, can be manipulated. [LaLumia, Sallee, and Turner \(2015\)](#) and [Dickert-Conlin and Chandra \(1999\)](#) find evidence that parents schedule C-sections and inductions to take advantage of tax credits. Maternal characteristics are also correlated with season of birth; [Buckles and Hungerman \(2013\)](#) find that mothers of children born in December are 7.8% more likely to be married and 3.3% more likely to have a high school diploma compared to mothers of children born in January.¹⁹

Figure 6 is a histogram of the number of births in our data on each calendar day in the months of December 2015 and January 2016. The histogram is far from smooth—on some days there are 12,000 births, and on other days there are half as many.

Number of births per calendar day is volatile because the series varies predictably by the day of the week. **Table 3** shows that the day of the week can explain 80% of the variation in the number of births. Weekends have half the number of births as week days, demonstrating clear manipulation of birth timing away from weekends.²⁰ Days of the week and the holidays together can explain 97% of the variation in the number of births. This presents a challenge unique to this paper since we are exploiting a discontinuity in tax benefits for one year only. Prior work that has used date of birth and a regression discontinuity design to exploit the January 1st cutoff for tax benefits has used variation that existed for multiple birth cohorts ([Nichols, Sorensen, and Lippold 2012](#); [LaLumia, Sallee, and Turner 2015](#); [Mortenson et al. 2018](#); [Cole 2021](#); [Barr, Eggleston, and Smith 2022](#); [Rittenhouse 2023](#)). With many cohorts, the weekly seasonal pattern averages out.²¹ In our paper, we first must smooth the series of births, and then ensure that manipulation of birth timing is not a threat to

repay any advance payment.

19. These characteristics do not jump discontinuously at the cutoff, though they do become relevant as we use observations further away from the cutoff.

20. We are not concerned with manipulation of birth timing except for when it moves a birth from January to December or vice versa.

21. For example, in our data, December 15th is always Tuesday. For someone who averages together 10 years of data, December 15th is a different day of the week each year. One exception to this pattern of averaging is [Unrath \(2023\)](#), who also examines the California YCTC and finds no employment effects in 2019 or 2020.

identification.

We smooth out day-of-week variation and examine the residual for evidence of manipulation of births. We use the coefficients for each day of the week from the specification in column (5) of **Table 3**, which identifies day-of-week effects while excluding holidays.²² We calculate the predicted number of births each day using only the coefficients on the days of the week, and plot this residual on the bottom half of **Figure 6**. The residuals look significantly smoother following the removal of the day-of-week variation, except for large negative spikes that correspond to the Christmas and New Year’s holidays. We visually inspect the residual plot for evidence of manipulation and conclude that it is limited to the bars colored in grey on the figure (the 8 days before and after January 1st). Following prior work, we exclude those 8 days before and after the January 1st cutoff and estimate a “donut” specification.

We test whether families of children born in the “donut” of December and January are similar on observable characteristics. These results for our sample of low-income families are plotted in **Figure 7** and reported in **Table 4**.²³ None of the characteristics are discontinuous at the cutoff. In **Appendix Figure A2**, we also show that the continuity of our main variables of interest (earnings and employment) across the cut-off in 2020, before the expansion of the CTC, is not sensitive to our particular choice of a donut of 8 days.

6 Results

6.1 Reduced form response on earnings

Our main finding is that low-income families have a negative earnings response on the extensive margin to additional unearned income. **Figure 8** illustrates this result. The figure shows regression discontinuity plots for employment by child date of birth in 2020, 2021, and 2022. In 2020, prior to the expansion to the CTC, there was no statistically significant discontinuity in the probability of employment (any earnings reported on Forms W-2 or 1099-NEC) at the December to January cutoff. However, in 2021 and 2022, families eligible for additional CTC are less likely to be employed.

The regression discontinuity coefficients from our preferred specification (equation (1) above) on our main outcomes are shown in **Table 5**. Families eligible for an additional \$600 were 1.4 percentage points (2.0%) less likely to be employed in 2021, and 0.8 percentage points (1.1%) less likely to be employed in 2022 (though not statistically significant). They

22. For instance, since Christmas falls on a Friday, we would not want to include the number of births on Christmas day when calculating the average number of births on Fridays since including it would artificially deflate our average. The coefficient on Friday in column (5) of **Table 3** excludes the effect of holidays.

23. **Appendix Table A2** reports these tests for all families.

earned \$127 less on average in 2021, and \$349 less in 2022, although neither result is statistically significant. One explanation for the negative extensive margin result and null finding on earnings is that the marginal families who exited employment due to the additional income had relatively low earnings to begin with. Another explanation is that families who remained in the labor force increased their earnings on the intensive margin just enough to offset the negative earnings effect from those who dropped out.

Our result is consistent with the static labor supply model as summarized in the framework section. An increase in unearned income relaxes the household’s budget constraint. Since leisure is a normal good, households decrease their labor supply, and a portion of households on the margin reduce their hours of work to zero.

The negative extensive margin effect is stronger in 2021 than 2022. It is possible this reflects the differential response to monthly payments versus a lump sum transfer. Alternatively, the result could be driven by the mixed composition of our sample. Some portion optimizes based on expected future income and respond to the announcement of the expansion in 2021. Another portion is credit constrained and optimize in response to realization of additional income. Households that are credit constrained may spread their responses out over 2021 and 2022.

We also report the result of regressions on the log of earnings. Families eligible for additional income who remained working increased their earnings by 0.61% in 2021, and those who remained working in 2022 decreased their earnings by 5.5%. Families that did not earn income drop out of these regressions. Given the extensive margin result, it is difficult to interpret these findings on log earnings as causal.²⁴

The regression discontinuity coefficients from our preferred specification (equation (1) above) on employment among middle, high, and very-high income families are shown in **Table 6**. Among these other income groups, families eligible for an additional \$600 do not decrease employment (and the confidence intervals generally rule out an effect larger than 0.5 percentage points). These results are a placebo test since we would not expect households who had more than \$30,000 in after-tax income to change their employment status in response to \$600. **Appendix Table A4** and **Appendix Table A5** show the

24. The main concern with causally interpreting these results is that selection is driving the negative coefficient on logged earnings. However, we argue that the results are consistent with eligibility for additional income reducing earnings on the intensive margin. First, selection would have to be positive for the coefficient on logged earnings to be negative, like we observe. Positive selection would mean that families who earned relatively *more* exited employment in response to the additional income, which is unlikely. Second, the coefficient on logged earnings is more negative in 2022. This is consistent with [Chetty et al. \(2012\)](#) who argue that the extensive margin is easier to adjust in the short run because of indivisible labor, while intensive margin adjustments are possible only in the medium and long run. Also, it is worth pointing out that the negative employment effect is not statistically significant in 2022, which is where we see the strongest effect on logged earnings.

results on the other main outcomes: earnings and logged earnings. There is no evidence that earnings or logged earnings in higher-income families changed in response to eligibility for an additional \$600.

In **Appendix Table A3** and **Appendix Figure A3**, we break down the reduced form household-level response to additional income into individual-level responses. **Appendix Table A3** reports that single parents are 1.2 percentage points (1.7%) less likely to be employed and earn \$410 less in 2021, and 1.2 percentage points (1.6%) less likely to be employed (not statistically significant) and earn \$623 less in 2022. **Appendix Figure A3** shows that single parents are less likely to have W-2 employment, and less likely to have 1099-NEC employment, although this result is not statistically significant. None of the results on married individuals are statistically significant, despite the large coefficients in some instances, because the regressions are not well powered — only 15% of the low-income sample is married. Married men are 0.9 percentage points (1.8%) less likely to be employed in 2021 and 3.3 percentage points (6.3%) more likely to be employed in 2022. Married women are 0.5 percentage points (1.2%) less likely to be employed in 2021 and 2.1 percentage points (5.2%) less likely to be employed in 2022. Point estimates in **Appendix Figure A3** show that the negative employment result is not driven by W-2 nor 1099-NEC employment among married individuals, but the confidence intervals are wide and contain most of the reasonable range.

We next present the results of equation (2) for four heterogeneity variables of interest: an indicator for 1) whether the household worked in 2020, 2) whether the household received unemployment compensation in 2021, 3) whether the focal child born has younger siblings, and 4) whether the household had multiple children at the cutoff (twins, triplets, etc.). **Table 7** summarizes the results. First, in column (1) we test for differential responses among families who worked in 2020. We find that families eligible for additional income were equally likely to stop working as they were to delay returning to employment after not working in 2020. Families who were employed in 2020 were 1.1 percentage points (1.6%) less likely to be employed in 2021, and 2.3 percentage points (3.2%) more likely to be employed in 2022 (neither result is statistically significant).

In column (2), we find that there is evidence to support the hypothesis that families eligible for additional income combined the CTC with unemployment benefits, or substituted monthly advance payments for unemployment, to continue not working.²⁵ Among those receiving unemployment benefits in 2021, families eligible for additional CTC income were 4.3 percentage points (5.9%) less likely to be employed in 2021 (significant at the 5% level),

25. Pandemic-era unemployment insurance benefits expired in September 2021, so this concern was especially prominent among some policymakers in 2021.

and 1.8 percentage points (2.4%) less likely to be employed in 2022 (not significant).

Column (3) tests whether families with young children responded more elastically to the additional CTC income. Families with young children have more caregiving responsibilities than families where all children are school-aged. Our results are in the direction of our hypothesis that families where the child born near the age 6 cutoff has younger siblings responded more elastically to additional income, but the difference is not statistically significant.

Last, we test in column (4) whether families with more than one child with a birthday near the cutoff respond more strongly to the discontinuity in benefits since they were eligible for more additional income. The results show that families eligible for additional income who had multiples decreased employment by an additional 2 percentage points, but the result is not statistically significant.

Table 8 summarizes the elasticities implied by our reduced form results. The household extensive margin elasticity with respect to after-tax income is -0.34 among all families with children born near the cutoff, and -0.50 for low-income families with children. This means that among low-income families, eligibility for a 1% increase in after-tax annual income decreased employment by 0.50%. The elasticity is calculated by dividing the percent change in employment by the percent change in simulated after-tax income. It captures an “intent to treat” response since the denominator is defined as the change in eligibility, not the *actual* realized change in after-tax income.

The intensive margin elasticity with respect to after-tax income is -0.79 for the full sample, and -0.77 for low-income families. This means that among low-income families, eligibility for a 1% increase in after-tax annual income decreases earnings by 0.77%. This elasticity is calculated by dividing the percent change in logged earnings by the percent change in simulated after-tax income.

Our results are broadly consistent with other work on child benefit programs which finds negative employment effects. [Milligan and Stabile \(2009\)](#) find that an increase in the generosity of a Canadian child allowance program decreased employment among low-income families. [Mazar and Reingewertz \(2023\)](#) used a decrease in the generosity of the child allowance in Israel to calculate an income effect elasticity between -0.5 to -0.8 (depending on the specification).

6.2 Other reduced form results

Table 9 shows that the negative employment result is driven by a decrease in W-2 employment. Families eligible for additional income were 1.4 percentage points (2.2%) less likely to have a W-2 (significant at the 5% level) and 0.3 percentage points (2.0%) less likely to have

a 1099-NEC (not statistically significant) in 2021. In 2022, families eligible for additional income were 1.1 percentage points (1.7%) less likely to have W-2 income and 0.6 percentage points (4.1%) less likely to have a 1099-NEC, though neither result is statistically significant.

We next turn to results for outcomes reported on the tax return (Form 1040). These outcomes are only available for families who filed taxes in the corresponding year. Although the results are not available for the full sample and thus may be biased, due to the richness of the data and potential for uncovering interesting outcomes, we report the results in **Table 9**.²⁶ We would expect W-2 employment to closely follow wages reported on the tax return.²⁷ Indeed, we find that this is the case. Low-income families who filed taxes in 2021 are 0.8 percentage points (1.1%) less likely to report having any wage income (not statistically significant). Families who filed taxes in 2022 are 1.8 percentage points (2.1%) less likely to report having any wage income that year.

Families that filed increased self-employment (SE). According to **Table 9**, eligibility for additional income increased the probability of reporting any SE income by 1.3 percentage points (4.9%), significant at the 10% level, and increased total SE income reported by \$222 (not statistically significant) in 2021. In 2022, eligibility increased the probability of reporting SE income by 1.9 percentage points (6.3%) and increased total SE income by \$461 (significant at the 5% level). These results suggest that low-income families decreased W-2 employment and increased self-employment in response to eligibility for additional income.

The increase in self-reported SE may indicate a substitution of W-2 employment for SE, but it may also reflect misreporting to minimize tax liability. On the one hand, SE allows for more flexibility in work schedules, which may be particularly attractive to families with children. Prior work finds that additional income increases the probability of becoming an entrepreneur ([Holtz-Eakin and Rosen 1994](#)). Entrepreneurship is risky, and additional income allows families to self-insure while pursuing employment. However, the increase in self-reported SE may be the result of misreporting. Families with children who drop out of the labor force stand to lose thousands of dollars in tax credits from the EITC. Prior work found that tax units manipulate self-employment income to maximize the EITC they can claim ([Chetty, Friedman, and Saez 2013](#); [Chetty and Saez 2013](#)). In most cases, it is not possible to verify self-employment earnings. Individuals who are self-employed may be issued a form 1099-NEC, but most are not.²⁸

26. We expect the subsample of filers to be positively selected from all tax units.

27. In 2020, 3.4% of low-income families report wages on their tax return but did not receive a W-2. An example of income reported as wage on Form 1040 that would not have a corresponding W-2 are taxable scholarships. Meanwhile, 5.7% received a W-2, but report no wages on their tax return. The W-2s may have been issued as a result of a clerical error or the family could have made adjustments to their wage income (see Form 1040 for details).

28. In our sample, 36% of tax units that reported self-employment income in 2020 also had a 1099-NEC

We test whether the increase in self-employment completely offsets the negative employment response we observe on third party information returns. We construct two alternate measures of employment, and report results on those dependent variables in **Table 10**. First, we use an indicator for whether the filer reports any wage income or self-employment income (any 1040 earnings). We find a null effect on employment in 2021 and 2022. The limitation of this measure is that it is only available for filers and it is self-reported. Next, we combine information about employment from third party information returns and self-employment from the tax return. We assume that nonfilers have no self-employment income, unless it is reported on a 1099-NEC information return. For this measure, in 2021, employment decreased by 1 percentage point (1.3%), but the result is not statistically significant. This result is in line with what we observe from the information returns. In 2022, employment increased by 0.1 percentage points, which was not statistically significant. In sum, there is some evidence to suggest when we incorporate information about self-employment from tax returns, employment did not decrease among low-income families.

Our results on self-employment, if indicative of a switch away from W-2 employment, help reconcile our findings with the conclusion of other papers that have studied labor supply responses to the advance monthly payments. [Enriquez, Jones, and Tedeschi \(2023\)](#), [Ananat et al. \(2022\)](#), and [Pilkauskas et al. \(2022\)](#) conclude that there was no negative employment effect to the advance monthly payments. We find a negative effect on employment as reported from third party information returns, but no employment effect when including self-employment as reported on tax return.

Appendix Table A6 shows that eligibility for additional income does not change the probability of reporting expenses for the purpose of claiming the Child and Dependent Care Credit (CDCC). It slightly lowered the amount of childcare expenses reported in 2021 (by \$91), which may be because families were less likely to work in W-2 employment, and thus less likely to utilize formal childcare arrangements. In 2022, there is no statistically significant effect on childcare expenses. This may be because low income families had less incentive to claim the CDCC since it was no longer refundable.²⁹

Appendix Table A7 reports results on unemployment compensation. Families eligible

form filed by a third party. This is expected. For example, if an attorney works as an independent contractor for a firm, the firm would issue them a 1099-NEC that summarizes the total compensation they earned for their services that year. If the attorney works as an independent contractor and provides legal services to private individuals, they would not receive a 1099-NEC from that client, but would report the income earned on the tax return.

29. The Child and Dependent Credit was made refundable for one year in 2021, before revering back to its original non-refundable version in 2022. Non-refundable tax credits decrease total tax liability, but are not paid out if total tax liability is negative. Low-income families, who usually have low tax liability, cannot take advantage of most non-refundable tax credits.

for an additional \$600 are not more likely to collect unemployment benefits, and do not collect more total unemployment benefits.

6.3 Reduced form in California

Table 11 shows the results of equation (1) on families in California for our main outcomes, employment and earnings as reported by third party information returns, in 2021 and 2022. Low-income families in California eligible for additional income from the CTC and the YCTC were 0.7 percentage points (1.0%) less likely to be employed in 2021, and 0.4 percentage points (0.5%) less likely to be employed in 2022 (neither result is statistically significant). These extensive margin results are notable—the null results stand in contrast to the negative employment result in the rest of the country (from **Table 5**). Families eligible for CTC and YCTC earned \$3,513 more on average in 2021, and \$3,279 more in 2022. Although the YCTC provides an incentive to work for the first \$1 of income, families respond by significantly increasing their earnings beyond the first dollar. This could be because an individual would need to work more to recover the costs associated with finding and starting a job. After fixed costs of entering the labor force are realized, the decision to work more hours is relatively less costly. The null household-level extensive margin result and positive earnings result mask a substantial amount of heterogeneity on the individual level.

In **Table 12**, we break out the reduced form results by demographic group. We find that low-income married men are most responsive to the combined policies in California. In 2021, employment among married men in California increased by 9.8 percentage points (21%), not statistically significant, and by \$8,003. In 2022, employment increased by 16.4 percentage points (34%) and earnings increase by \$8,359. Even though 2022 saw no additional incentive for continuing to work, we identify a persistent effect among married men in 2022. Overcoming fixed costs to work in 2021 (for example, buying a car, the job search) would also lower the cost to work in 2022, allowing married men to continue to work after the expiration of the subsidy. Among married women, we see no statistically significant effects on employment or earnings. For single parents, there were no significant effects on employment in 2021 or 2022. Earnings increased by \$2,155 in 2021 and \$2,244 in 2022 (not statistically significant). Single parents may face childcare constraints that they are not able to overcome even with the potential for an additional \$1,000 of income from the YCTC. However, for those who were already working, the additional income may have allowed them to increase work on the intensive margin.

We study the differential response in California compared to the rest of the country by presenting heterogeneity results using equation (2) above. By studying the employment and earnings responses in California, we can provide suggestive evidence about the employment

effects of a child allowance policy that also increases the subsidy for work. The YCTC in California created a unique incentive to work for at least \$1 of earned income in 2021.³⁰ In **Table 13**, we present the results in 2021 from equation (2) where the heterogeneity variable (D_i) is an indicator for whether the family lived in California in 2020. Each row in the table represents a regression. The first column shows the difference between families with children born in January and children born in December in all of the United States excluding Connecticut (the treatment is additional unconditional income). The second column shows the additional difference between children born in January and children born in December for California families (where the treatment is additional income combined with an incentive to work). We expect the coefficient in the first column to be (weakly) negative if leisure is a normal good, and the coefficient in the second column to be positive since the \$1,000 conditional on working increases the return to work.

In **Table 13**, we see that among all low-income families, unconditional income decreased employment by 1.4 percentage points (2%) and decreased earnings by \$114 (not statistically significant). Conditional income that subsidized work increased employment by 0.7 percentage points (0.9%), as expected, but the coefficient is not significant. Conditional income did increase earnings by \$3,510 (significant at the 1% level).

Among married men, unconditional income had no effect on employment or earnings, while conditional income increased employment by 11 percentage points (21%), significant at the 10% level, and earnings by \$6,644, significant at the 5% level. For married women, unconditional and conditional income did not effect employment or earnings. For single parents, unconditional income did decrease employment by 1.3 percentage points (1.8%) and earnings by \$405. The incentive to work slightly increased employment, but this result was not statistically significant, and increased earnings by \$2,528.

Appendix Table A8 presents results for a triple difference-in-difference model and takes into account the fact that families with children born in December aged out of eligibility for the YCTC only if there were no younger children in the family. Since this further decreases our sample size, in this specification, we cannot reject the null hypothesis that income conditional on work has no effect on employment or earnings.

6.4 Robustness

We test the robustness of our main results among low-income families. In **Table 14**, we show the results of our main robustness checks. Our negative result on employment among

30. This differential incentive between our treatment and control group did not exist in 2022 since the children born in December 2015 and January 2016 would be aged 6 and 7, and would not be eligible for the YCTC anymore.

low-income families are robust to a non-linear specification, alternate kernel weights, and the exclusion of families who lived in states with ambiguous kindergarten cutoffs.

We also show that our main employment result is robust to alternative donut sizes. **Figure 9** shows the coefficient plot for specifications that omit births within a varying number of days evenly around the January 1st cutoff. The effect is relatively stable across donut size. Specifications with smaller excluded regions near the cutoff show some attenuation, which is consistent with some amount of birth manipulation directly near January 1st. Specifications that exclude a relatively larger region have wide confidence intervals as the sample size decreases, but the coefficient remains negative.

Next, we show that our results on employment, earnings, and logged earnings are robust to the exclusion of the lagged dependent variable and the covariates. These results can be found in **Appendix Table A9**, **Appendix Table A10**, and **Appendix Table A11**.

Finally, we test robustness to our definition of low-income. **Appendix Figure A4** shows the coefficient plot for our preferred specification across different definitions of low-income. Each coefficient represents a slightly broader definition of low-income. The coefficient is relatively stable. As the definition of low-income includes more households of relatively higher income, the treatment effect fades. This is consistent with an extensive margin response among the most vulnerable families.

7 Conclusion

In 2021, the American Rescue Plan temporarily expanded the CTC. For many policymakers, the expansion was a long awaited trial run of a first-of-its-kind child allowance program in the United States. However, many were concerned that the expanded CTC was too generous, and parents would respond by dropping out of the labor force or decreasing the number of hours they work ([Winck 2021](#)).

In this paper, we answer how earned income responds to a cash transfer for a population targeted by child allowance policies. We use plausibly exogenous variation in unearned income from the CTC expansion. We precisely estimate the causal effect of \$600 of CTC on earnings and employment, and derive an earnings elasticity with respect to after-tax income—the key parameter for evaluating the fiscal consequences of a child allowance.

We show that eligibility for an additional \$600 of CTC decreased the probability of employment as reported on third party information returns. Among low-income families, the probability that any adult in the household was employed decreased by 1.4 (2.0%) percentage points in 2021 (significant at the 5% level) and by 0.8 (1.1%) percentage points in 2022 (not statistically significant). We derive an extensive margin elasticity with respect to after-tax

income of -0.5. This means that a 10% increase in the after-tax income of low-income families would on average reduce employment by 5%.

We add to prior work that has used variation from lotteries (Imbens, Rubin, and Sacerdote 2001; Golosov et al. 2021), inheritances (Holtz-Eakin, Joulfaian, and Rosen 1993), stimulus payments (Powell 2020), and cash welfare (Hoynes 1996; Gelber, Moore, and Strand 2017). We quantify the likely labor supply response from variation in child benefits among a relevant population that will be the primary target of any future child allowance policy — low-income families with children. However, our results have two important limitations to external validity. First, we identify the response to a small, temporary shock to after-tax income. The behavioral response to a temporary shock is likely to be a lower bound on the responses we would expect from a permanent child allowance. Second, the expansion of the CTC occurred during a global pandemic. The labor-leisure trade-off experienced by families with children was unique during this time. Daycare and school closings made time at home especially valuable, and the threat of illness from COVID-19 increased the non-pecuniary costs of working. Some may be concerned that our elasticities are too large as a result of the pandemic, but in fact, the elasticity is on the lower end of elasticities from a child benefit program in Israel (Mazar and Reingewertz 2023).

Additionally, we find some evidence to suggest that low-income families substitute from formal, W-2 employment into self-employment. Low-income families increased the probability of reporting any self-employment income by 1.3 percentage points (4.9%) in 2021 (significant at the 10% level) and by 1.9 percentage points (6.3%) in 2022 (significant at the 5% level). This is consistent with recent evidence that self-employment increased during the pandemic (Gregory, Harding, and Steinberg 2022). It is also consistent with a reporting response rather than a real response, since we do not find evidence of increased reporting of independent contract work among third party information returns (Chetty, Friedman, and Saez 2013; Chetty and Saez 2013). Future work will further examine this margin of response.

We utilize variation from the Young Child Tax Credit (YCTC) in California to study the likely consequences of a generous tax credit that also incentivizes work. We find that the YCTC and the federal CTC combined did not decrease employment as reported on third party information returns—a null finding that stands in direct contrast to the negative employment response to eligibility for \$600 of additional non-labor income. In fact, we find that among low-income married men, employment increased by 9.8 percentage points (21%) and earnings increased by \$8,002 in 2021, and by 16 percentage points (33.3%) and \$8,359 in 2022. Among low-income single parents, earnings increased by \$2,155. We find evidence that pairing a child allowance policy with a policy that increases the subsidy to work may be a viable method for mitigating the negative employment response from a child allowance

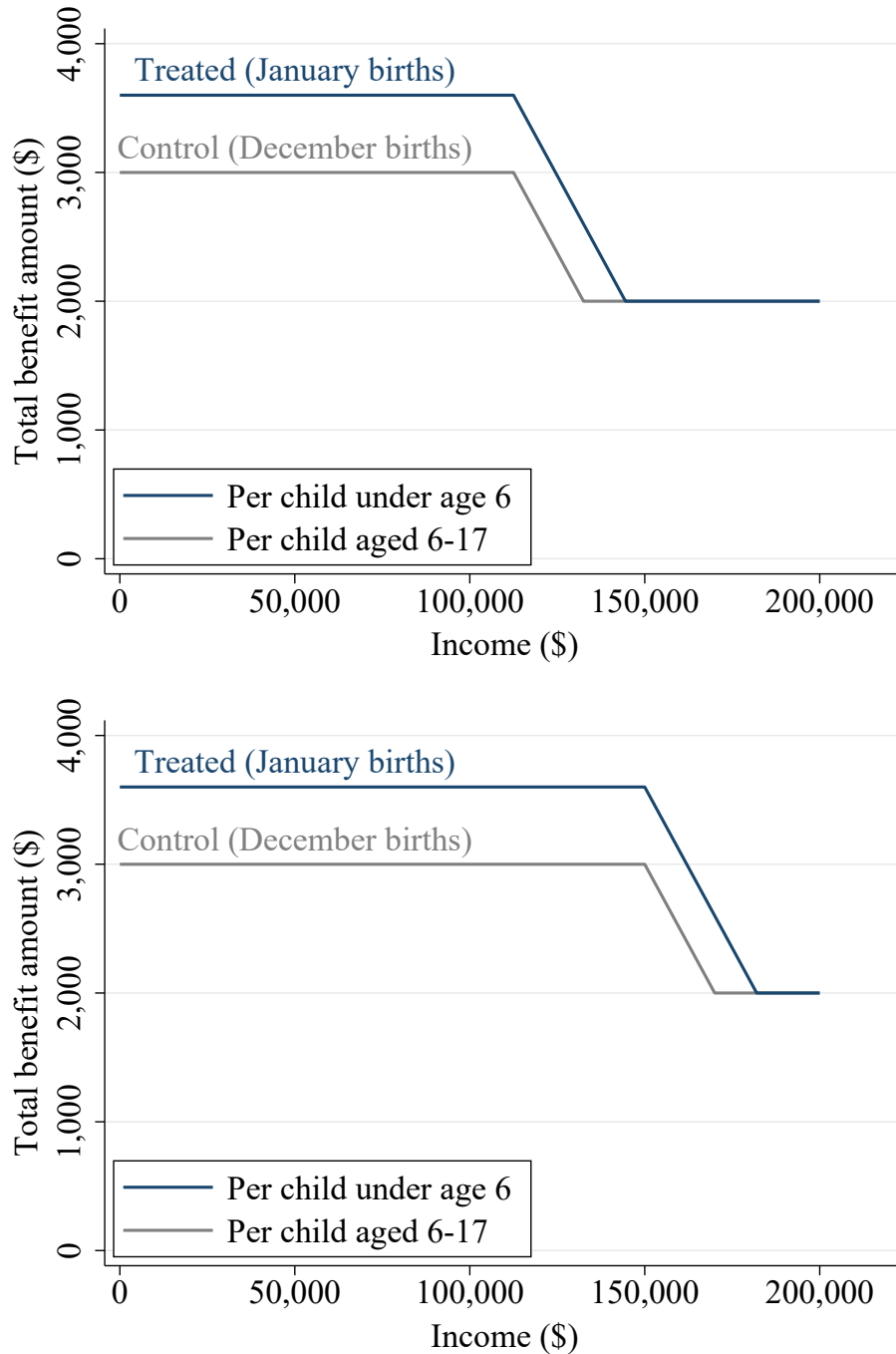
program.

Our results could be used to inform the likely behavioral responses to new state credits. After the expiration of the expanded Child Tax Credit, many states passed their own versions of the Child Tax Credit. In 2023, seven states will implement fully refundable tax credits for children modeled after the expanded CTC (McCabe 2023).³¹ For families with income below the phase out region, the increase in after-tax income from these new credits will induce a pure income effect. This paper shows that employment could decrease by at least 1-2% in response to these policies.

31. Prior to 2022, only California had a state version of the Child Tax Credit, the YCTC. In 2022, California converted the YCTC into a fully refundable credit.

8 Tables and Figures

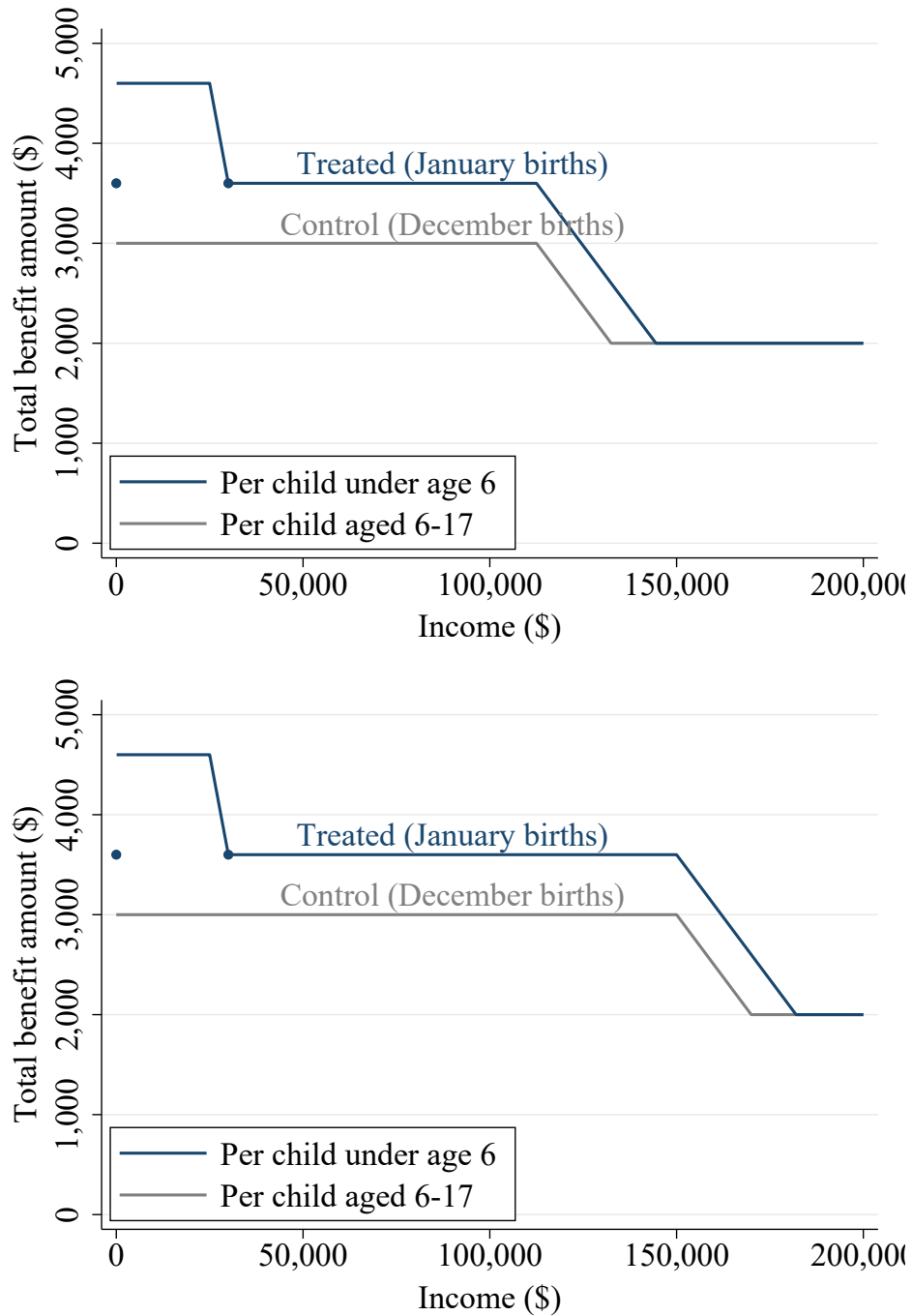
Figure 1: Background - Benefit schedule of the 2021 expanded Child Tax Credit



Source: Author's calculations using TAXSIM v35 (<https://taxsim.nber.org/>).

Note: The figure shows the Child Tax Credit (CTC) benefit schedule by income level for those filing as head of household (top panel) and married, filing jointly (bottom panel). For head of household, the expanded CTC benefit begins phasing out at \$112,500 (\$150,000 for married, filing jointly).

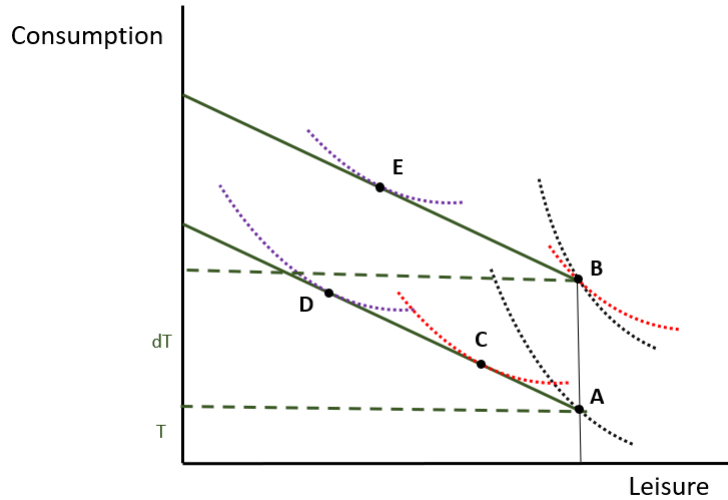
Figure 2: Background - Benefit schedule of the 2021 expanded Child Tax Credit and California's Young Child Tax Credit



Source: Author's calculations using TAXSIM v35 (<https://taxsim.nber.org/>).

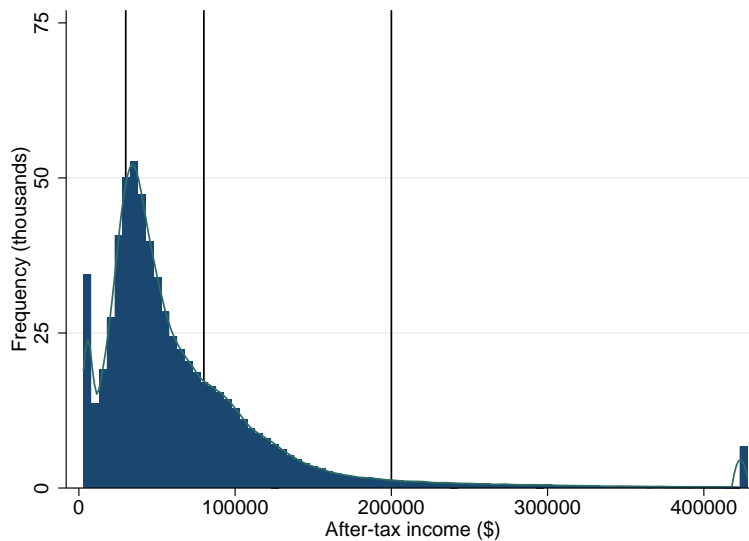
Note: The figure shows the Child Tax Credit (CTC) benefit schedule combined with the California Young Child Tax Credit by income level for those filing as head of household (top panel) and married, filing jointly (bottom panel). For head of household, the expanded CTC benefit begins phasing out at \$112,500 (\$150,000 for married, filing jointly).

Figure 3: Framework - Effect of cash transfer on individual demand for consumption and leisure



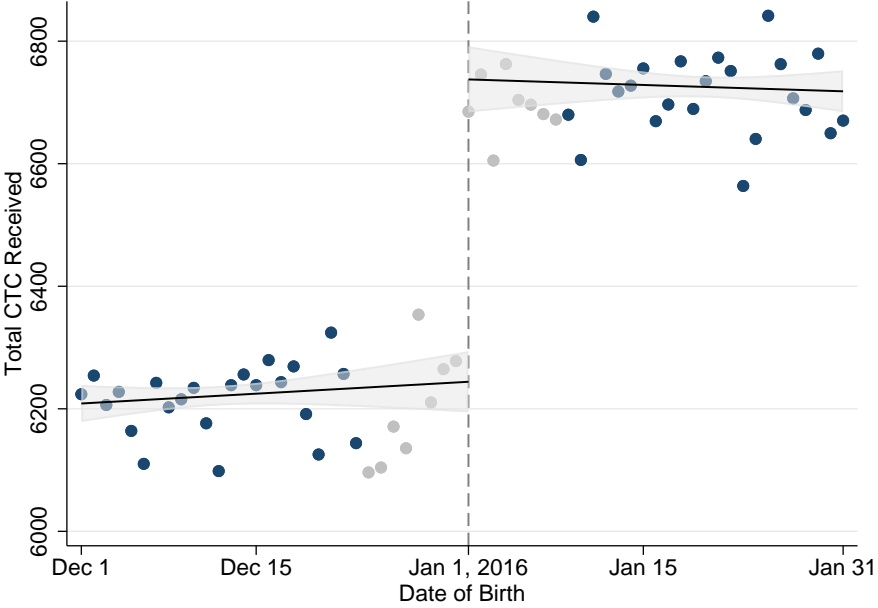
Note: This figure shows an increase to the non-labor income of a household, and illustrates the resulting re-optimization problem faced by different households. The household with purple indifference curves moves from point D to E. The household with red indifference curves moves from point C to B. The household with black indifference points moves from point A to B.

Figure 4: Distribution of total after-tax income in 2020

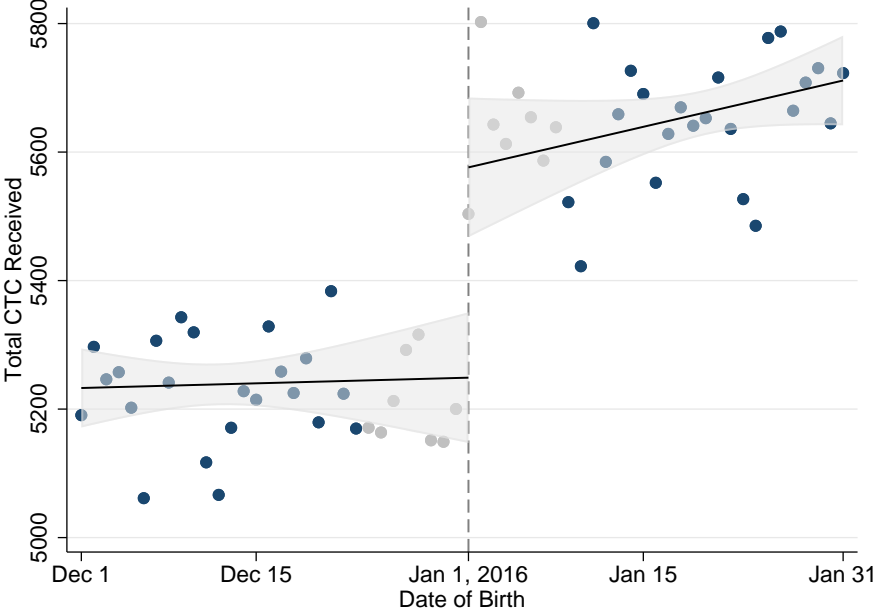


Note: This figure shows a histogram of after-tax income in 2020. After-tax income is winsorized at the 1st and 99th percentile. The vertical lines are at \$30,000, \$80,000, and \$200,000 and represent the thresholds between the low-income group, the middle-income group, the high-income group, and the very high-income group.

Figure 5: Relevance - CTC amount claimed for families with children born near the cutoff



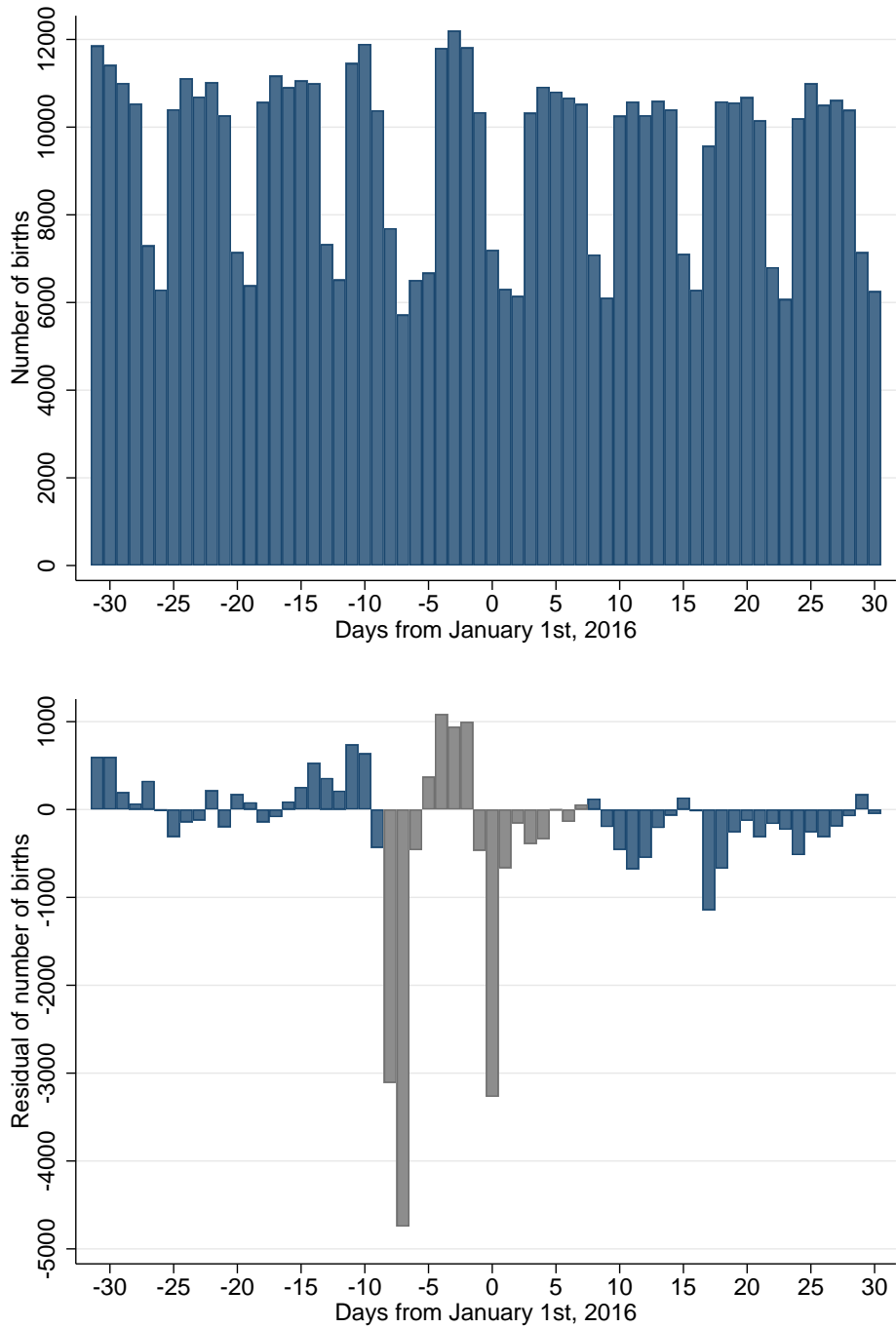
(a) Full sample



(b) Low-income families

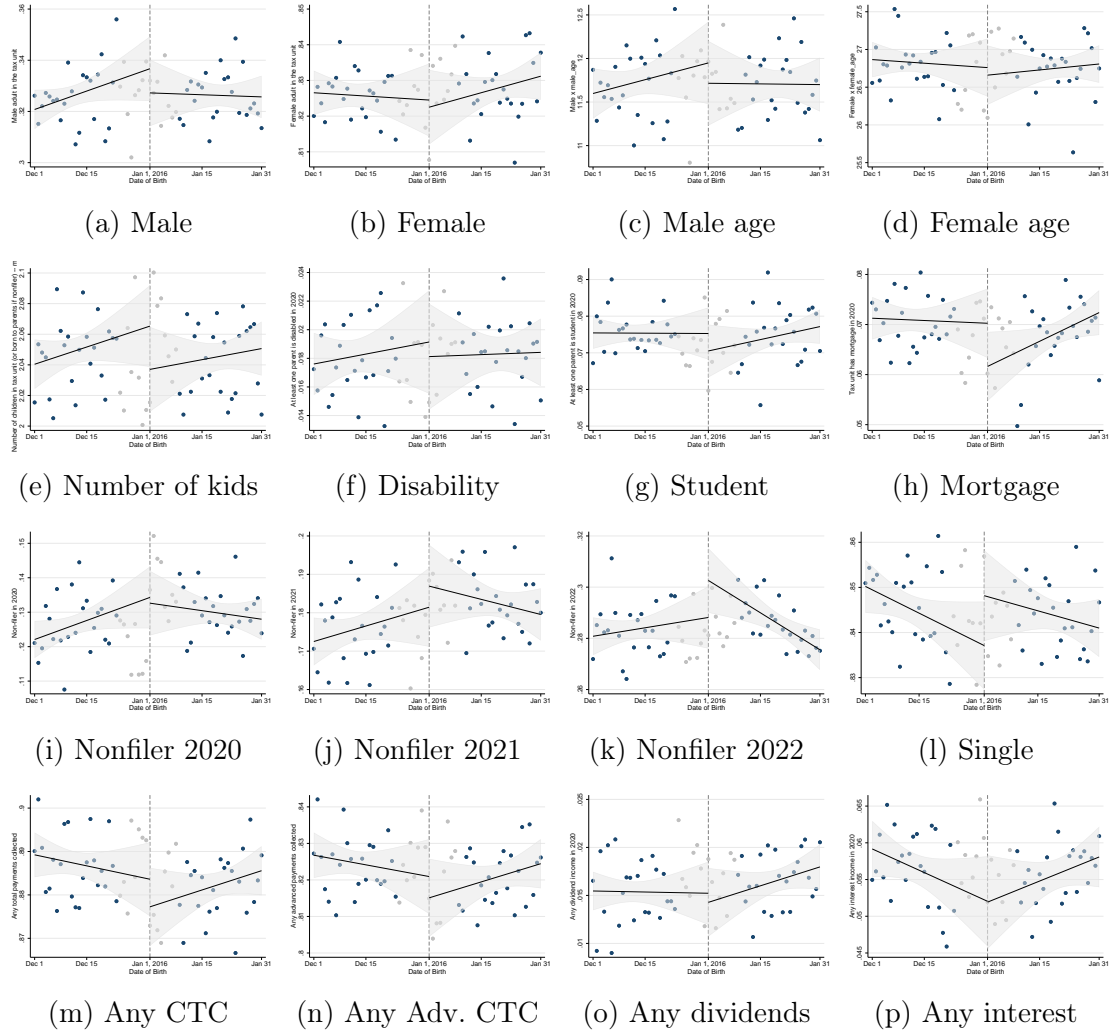
Note: This figure plots the average amount of Child Tax Credit (CTC) received by families with children born on each calendar day within 30 days of January 1st, 2016. Birth dates to the right of January 1st were eligible for \$600 more of CTC. Blue dots represent days that are outside of the 8 day “donut” and are included in our analysis sample. Gray dots represent birth dates that are excluded. Low-income is defined as after-tax income of \$0-30k in 2020.

Figure 6: Validity - Birth patterns near January 1, 2016



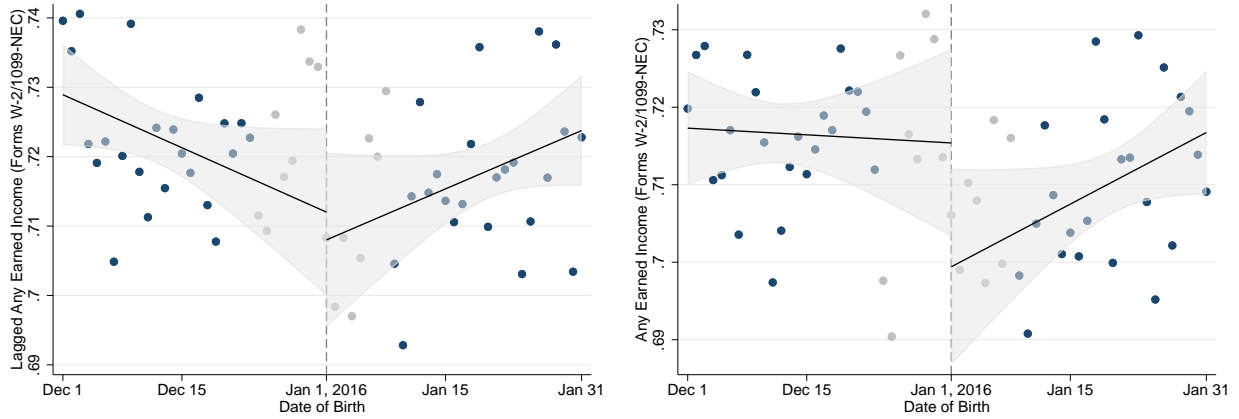
Note: The figure on the top shows a histogram of the number of births on each calendar day in the full sample. The figure on the bottom plots the residuals of the number of births when the coefficients on day of the week only from specification (5) in Table 3 are used to predict the number of births each calendar day. Day +/- 8 days from January 1st are shown in gray.

Figure 7: Validity - continuity of covariates at the cutoff



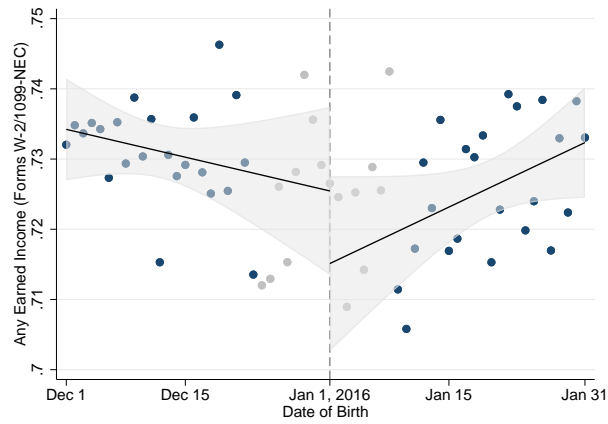
Note: This figure shows regression discontinuity plots on household and parent characteristics. Calendar days +/- 8 days from January 1st are shown in gray. Male (female) indicates if there is an adult male (female) in the tax unit. Disability is whether any parent collected Social Security Disability Income as reported on Form SSA-1099. Student is whether any parent was a college or graduate student as reported on Form 1098-T. Mortgage is whether the tax unit paid mortgage interest reported on Form 1098. Single is whether the parent linked with the child had no spouse in 2020, or on the latest tax return if did not file in 2020. Any CTC is whether the tax unit claimed any Child Tax Credit for tax year 2021 (including monthly payments). Any Advance CTC is whether the tax unit received any advance monthly payments in 2021. Any dividend is whether the tax unit received any dividend income as reported on Form 1099-DIV. Any interest is whether the tax unit received any interest income as reported on Form 1099-INT.

Figure 8: Results - regression discontinuity plots of employment



(a) Employment in 2020

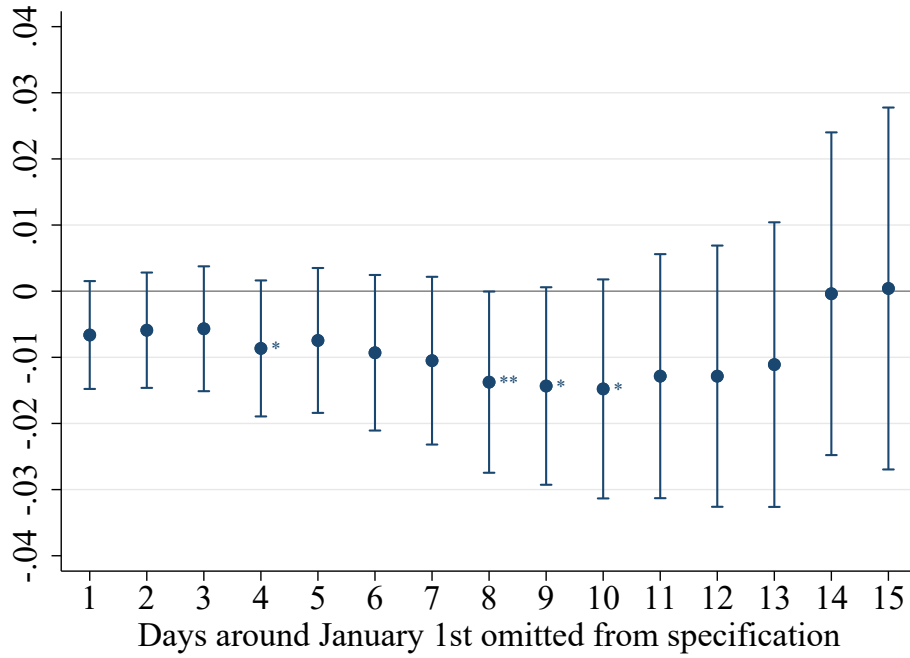
(b) Employment in 2021



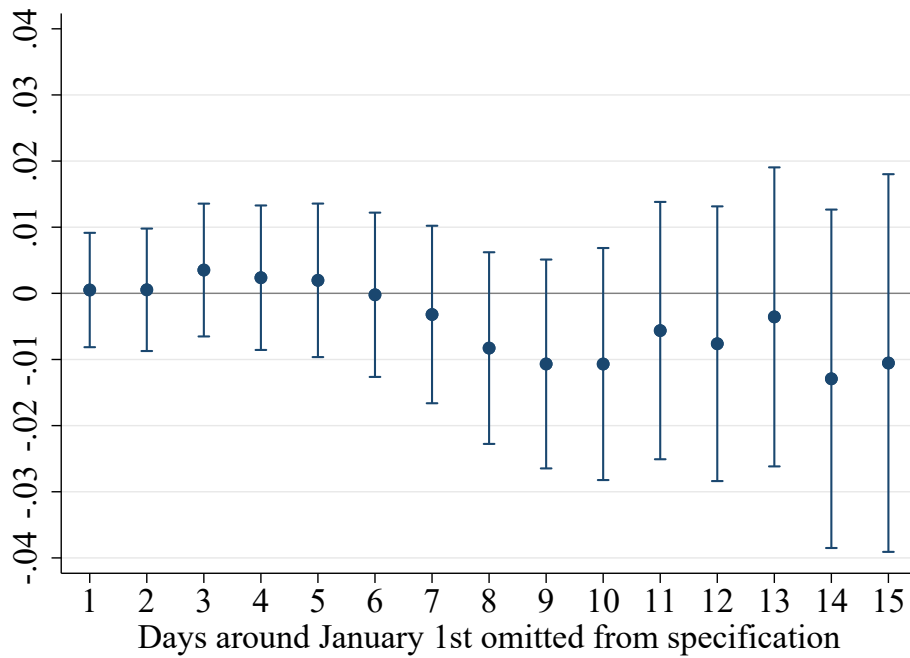
(c) Employment in 2022

Note: This figure shows regression discontinuity plots for whether households in our low-income sample had any earnings reported on Forms W-2 and 1099-NEC. Calendar days +/- 8 days from January 1st are shown in gray, and are not included in the line of best fit. 95% confidence intervals shown.

Figure 9: Robustness - robustness of main result by donut size



(a) Employment (2021)



(b) Employment (2022)

This figure shows a coefficient plot of the RD coefficient from the preferred specification (see note in Table 5 for details) where the dependent variable is an indicator for any earnings reported on Forms W-2 or 1099-NEC. The horizontal axis indicates births outside a donut with a varying number of days evenly around the January 1st cutoff. Standard errors are clustered by household.

Table 1: Descriptive Statistics for Sample

	Full sample		Low-income	
	Mean	SD	Mean	SD
After-Tax Income in 2020	67,172	(63,955)	18,313	(8,583)
.. share: \$0 < \$30,000	0.23		1.00	
.. share: \$30,000 < \$80,000	0.49		0.00	
.. share: \$80,000 < \$200,000	0.23		0.00	
.. share: over \$200,000	0.04		0.00	
.. share: negative income	0.01		0.00	
Earnings:				
2020	71,611	(85,223)	10,967	(15,654)
2021	77,504	(91,402)	14,419	(20,101)
2022	84,549	(97,147)	18,330	(24,392)
Employment:				
2020	0.91		0.72	
2021	0.90		0.71	
2022	0.90		0.73	
Household characteristics:				
Number of kids	2.28	(0.95)	2.05	(0.99)
Age of youngest child	3.52	(1.75)	3.70	(1.72)
Age of oldest child	8.14	(4.80)	7.73	(4.82)
Age (female parent)	34.56	(7.11)	32.40	(7.91)
Age (male parent)	37.32	(7.48)	35.90	(8.87)
2020 characteristics:				
Non-filer	0.05		0.13	
Single (i.e., not filing jointly)	0.45		0.84	
Female parent	0.87		0.83	
Male parent	0.67		0.33	
Form 1098 (mortgage interest)	0.43		0.07	
Form 1098-T (tuition)	0.08		0.08	
SSA-1099 with disability income	0.01		0.02	
Form 1099-INT (interest)	0.21		0.06	
Form 1099-DIV (dividends)	0.12		0.02	
N	437,000		103,000	

Sample is linked parents of all children born in December 2015 to January 2016, outside of an 8-day donut around the discontinuity, and not living in Connecticut or California (see text for details). Earnings are income reported on Forms W-2 and 1099-NEC, winsorized at the 1st and 99th percentile. Employment is an indicator for any positive income reported on these forms. Low-income is defined as after-tax income of \$0-30k in 2020. Sample size rounded to the nearest thousand for disclosure reasons.

Table 2: Relevance - Discontinuity in benefit amount for families with children

	Total CTC	Advance CTC	After-tax simulated income	Log after-tax simulated income
Full sample				
January birth	483.2*** (24.8)	262.6*** (13.0)	534.1*** (18.9)	0.012*** (0.0011)
<i>N</i>	437,000	437,000	437,000	437,000
Control mean	6221.2	2944.5	70437.2	10.9
Low-income				
January birth	401.3*** (57.4)	205.8*** (28.2)	622.3*** (17.6)	0.030*** (0.0017)
<i>N</i>	103,000	103,000	103,000	103,000
Control mean	5238.5	2398.1	23952.4	10.0

Table reports the RD coefficient of interest. After-tax simulated income is calculated by applying 2021 tax policy to 2020 income. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Validity - predicting the number of births per day

	(1)	(2)	(3)	(4)	(5)
January	-592.8 (517.0)			-468.4*** (95.30)	
Saturday		-3847.3*** (426.5)		-3795.3*** (168.6)	-3847.3*** (203.6)
Sunday		-4513.6*** (426.5)		-4461.5*** (168.6)	-4513.6*** (203.6)
Monday		-244.4 (439.6)		-108.6 (179.9)	-101.2 (217.6)
Tuesday		440.0 (426.5)		440.0** (168.3)	440.0** (203.6)
Thursday		-408.8 (426.5)		48.31 (180.3)	-11.17 (217.6)
Friday		-1241.3*** (426.5)		-291.3 (180.3)	-350.7 (217.6)
Christmas Eve			-1814.4 (2049.3)	-3378.4*** (385.5)	-3110.7*** (461.6)
Christmas Day			-3789.4* (2049.3)	-5013.8*** (385.5)	-4746.1*** (461.6)
New Year's Eve			828.6 (2049.3)	-735.4* (385.5)	-467.7 (461.6)
New Year's Day			-2312.4 (2049.3)	-3068.4*** (383.8)	-3269.1*** (461.6)
MLK Day			60.65 (2049.3)	-878.0** (385.5)	-1145.7** (461.6)
Constant	9692.4*** (365.6)	10816.9*** (301.6)	9509.4*** (269.1)	11025.1*** (126.3)	10816.9*** (143.9)
N	62	62	62	62	62
R^2	0.0214	0.823	0.0901	0.975	0.963
Adj. R^2	0.00513	0.803	0.00888	0.969	0.955

Dependent variable is the number of births per calendar day. Regression includes all calendar days in December 2015 and January 2016. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Validity - balance of 2020 covariates among low-income families

	Non-filer	Single	Number of kids	Female in household	Female \times age
January birth	-0.0017 (0.0066)	0.011 (0.0071)	-0.028 (0.020)	-0.0020 (0.0074)	-0.10 (0.28)
N	103,000	103,000	103,000	103,000	103,000
	Male in household	Male \times age	Has mortgage	Parent student	SSDI income
January birth	-0.0094 (0.0092)	-0.23 (0.35)	-0.0086* (0.0050)	-0.0048 (0.0052)	-0.0010 (0.0026)
N	103,000	103,000	103,000	103,000	103,000
	Has interest income	Has dividend income	Employment	Total earnings	Log earnings
January birth	-0.00011 (0.0045)	-0.00095 (0.0025)	-0.0040 (0.0089)	70.0 (308.4)	0.034 (0.028)
N	103,000	103,000	103,000	103,000	74,000

Table reports the RD coefficient on regressions where the dependent variable is a tax unit/household characteristic. All characteristics were observed in 2020, prior to the expansion of the CTC. All regressions exclude births within 8 days of January 1st. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Results - main earnings outcomes among low-income families

	Employment		Earnings		Log earnings	
	2021	2022	2021	2022	2021	2022
January birth	-0.014** (0.0070)	-0.0083 (0.0074)	-127.3 (264.7)	-349.4 (386.0)	0.0061 (0.026)	-0.055** (0.026)
<i>N</i>	103,000	103,000	103,000	103,000	65,000	65,000
Control mean	0.72	0.73	14499.2	18444.2	9.43	9.70

This table shows RD coefficients from the preferred specification, using a running variable of child age relative to the January 1, 2016 cutoff. RD specification is linear with a uniform kernel. Models include a lagged dependent variable and the following covariates: state fixed effects, the number of children, interactions of (quadratic) age with an indicator for a female adult in the household, indicator for filing jointly, and whether the primary taxpayer or spouse received information returns for a mortgage, student status, Social Security disability income, dividend income, or interest income. Earnings are income reported on Forms W-2 and 1099-NEC, winsorized at the 1st and 99th percentile. Employment is an indicator for any positive income reported on these forms. Logged earnings is a log transformation of winsorized earnings (households with no earnings in 2020 or the year of the regression drop out). Sample includes all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Results - employment by medium-, high-, and very high- income families

	Middle (\$30-80k)		High (\$80-200k)		Very high (> \$200k)	
	2021	2022	2021	2022	2021	2022
January birth	-0.00046 (0.0025)	-0.0017 (0.0029)	-0.0011 (0.0017)	-0.00033 (0.0022)	-0.0073 (0.0059)	-0.0018 (0.0075)
<i>N</i>	214,000	214,000	101,000	101,000	17,000	17,000
Control mean	0.95	0.94	0.98	0.98	0.97	0.96

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and description of the outcome variables). Income groups defined based on after-tax income in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Heterogeneity - employment among low-income families

	(1)		(2)		(3)		(4)	
	2021	2022	2021	2022	2021	2022	2021	2022
January birth	-0.005 (0.017)	-0.017 (0.016)	-0.004 (0.008)	-0.003 (0.009)	-0.029** (0.013)	-0.022 (0.014)	-0.013* (0.007)	-0.010 (0.007)
January birth × Employed in 2020	-0.011 (0.018)	0.023 (0.017)						
January birth × Unemployment in 2021			-0.043** (0.018)	-0.018 (0.016)				
January birth × Youngest					0.021 (0.016)	0.020 (0.016)		
January birth × Multiples							-0.020 (0.065)	0.073 (0.070)
<i>N</i>	103,000	103,000	103,000	103,000	103,000	103,000	103,000	103,000
Control mean	0.72	0.73	0.72	0.73	0.72	0.73	0.72	0.73

The table shows the RD coefficient from the preferred specification interacted with a heterogeneity variable of interest. Employment is an indicator for any positive income reported on Forms W-2 and 1099-NEC in 2020. Youngest is an indicator for whether the child at the cutoff is the youngest or the only child. Unemployment is an indicator for whether anyone in the household collected unemployment in 2021 as reported on Forms 1099-G. Multiples is an indicator for whether the household has two or more children born in December or January. The sample includes only low income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Results - derived household earnings elasticities

	Employment	Log Earnings	Log after-tax simulated income
Full Sample			
January birth	-0.0037* (0.0019)	-0.0096 (0.0060)	0.012*** (0.0011)
<i>N</i>	874,000	760,000	874,000
Control mean	0.90	10.9	10.9
% Change	-0.0041	-0.0096	0.012
Extensive margin elasticity	-0.338		
Intensive margin elasticity		-0.789	
Low-income			
January birth	-0.011* (0.0062)	-0.023 (0.022)	0.030*** (0.0017)
<i>N</i>	205,000	130,000	205,000
Control mean	0.72	9.56	10.0
% Change	-0.015	-0.023	0.030
Extensive margin elasticity	-0.503		
Intensive margin elasticity		-0.773	

This table presents RD coefficients using the specification of Table 5, but estimated by pooling 2021 and 2022 data. “% Change” is the coefficient of interest divided by the control mean (for employment) or just the coefficient (for logged outcomes). Elasticities are computed as the percent change in the outcome (first two columns) divided by the percent change in income (third column). Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Other results - effect on other tax outcomes among low-income families

	2021							
	Filer				Information return			
	Any 1040 wages	1040 wages	Any 1040 SE income	1040 SE income	Any W-2 wages	W-2 wages	Any 1099-NEC income	199-NEC income
January birth	-0.008 (0.008)	-59.381 (277.448)	0.013* (0.008)	221.712 (191.467)	-0.014** (0.007)	28.651 (237.068)	-0.003 (0.006)	-175.664 (132.517)
<i>N</i>	81,000	81,000	81,000	81,000	103,000	103,000	103,000	103,000
Control mean	0.73	15150.6	0.27	3620.4	0.65	12268.4	0.16	2230.9
	2022							
	Filer				Information return			
	Any 1040 wages	1040 wages	Any 1040 SE income	1040 SE income	Any W-2 wages	W-2 wages	Any 1099-NEC income	199-NEC income
January birth	-0.018** (0.008)	-851.462** (420.411)	0.019** (0.009)	460.552** (227.509)	-0.011 (0.008)	-400.028 (362.155)	-0.006 (0.007)	13.330 (157.742)
<i>N</i>	71,000	71,000	71,000	71,000	103,000	103,000	103,000	103,000
Control mean	0.82	20979.2	0.30	4428.5	0.67	16196.0	0.15	2248.2

Table reports the RD coefficient for the preferred specification (see note in Table 5 for details) for dependent variables: any wage earnings reported on Line 1 of Form 1040 (indicator), total wage earnings (dollars), any schedule SE (self-employment) income reported on Form 1040 (indicator), total schedule SE income (dollars), any earnings reported on Form W2 (indicator), total earnings reported on Form(s) W2 (dollars), any earnings reported on Form 1099-NEC (indicator), and total amount of earnings reported on Form(s) 1099-NEC (dollars). In this first four columns, the sample is low-income families who filed taxes in the respective year. In the last four columns, the sample is all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Other results - alternate definitions of employment among low-income families

	Any 1040 earnings		Any W-2, 1099, or SE income	
	2021	2022	2021	2022
January birth	0.002 (0.006)	0.002 (0.003)	-0.010 (0.006)	0.001 (0.007)
<i>N</i>	81,000	71,000	103,000	103,000
Control mean	0.90	0.98	0.79	0.80

Table reports the RD coefficient for the preferred specification (see note in Table 5 for details) for dependent variables: an indicator for any earnings reported on Form 1040 (wage + self-employment) and an indicator for any W-2, 1099, SE income (indicator), assuming that nonfilers have no self-employment income. In this first two columns, the sample is low-income families who filed taxes in the respective year. In the last two columns, the sample is all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: California - main earnings outcomes of low-income families

	Employment		Earnings		Log earnings	
	2021	2022	2021	2022	2021	2022
January birth	-0.0068 (0.025)	-0.0036 (0.026)	3512.7*** (1093.8)	3278.7** (1556.6)	0.15 (0.11)	0.15 (0.12)
<i>N</i>	10,000	10,000	10,000	10,000	5,000	5,000
Control mean	0.67	0.69	14668.2	18531.2	9.40	9.60

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and a description of the dependent variables). The sample in these regressions are low-income families who lived in California in 2020. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: California - main earnings outcomes of low-income families by demographics

	Employment		Earnings		Log earnings	
	2021	2022	2021	2022	2021	2022
Married men						
January birth	0.098 (0.060)	0.16*** (0.062)	8002.8*** (2867.4)	8358.9** (3524.1)	0.027 (0.33)	0.13 (0.32)
<i>N</i>	2,000	2,000	2,000	2,000	1,000	1,000
Control mean	0.46	0.48	15642.8	18507.1	9.77	9.99
Married women						
January birth	0.0096 (0.049)	-0.027 (0.054)	416.0 (1622.9)	-1473.2 (2173.2)	-0.13 (0.40)	-0.063 (0.52)
<i>N</i>	2,000	2,000	2,000	2,000	.	.
Control mean	0.29	0.35	5235.7	7567.6	9.23	9.36
Single						
January birth	-0.032 (0.028)	-0.019 (0.028)	2154.7** (989.5)	2244.2 (1571.6)	0.19 (0.12)	0.11 (0.13)
<i>N</i>	8,000	8,000	8,000	8,000	5,000	5,000
Control mean	0.69	0.71	12840.0	16281.3	9.30	9.51

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and description of the outcome variables). Missing *N* indicates sample size less than 500. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: California - DiRD in 2021

	Unconditional income	Incentive to work
	January birth	January birth × California
Employment		
All low-income	-0.014** (0.0070)	0.0066 (0.026)
Married men	-0.0091 (0.020)	0.11* (0.063)
Married women	-0.0039 (0.019)	0.013 (0.053)
Single	-0.013* (0.0075)	-0.018 (0.029)
Earnings		
All low-income	-113.9 (264.7)	3510.1*** (1125.0)
Married men	1165.9 (1020.9)	6643.5** (3030.3)
Married women	264.9 (454.6)	75.2 (1670.9)
Single	-404.6* (236.8)	2527.8** (1015.1)

Each row in the table represents a regression. The first column shows the difference between low-income families with children born in January and children born in December in all of the United States excluding Connecticut. The second column shows the additional difference between children born in January and children born in December for California families. All regressions also include a lagged dependent variable and covariates. Employment is an indicator for any positive income reported on Forms W-2 and 1099-NEC. Sample is limited to low-income families. Max N=112,000; 18,000 married families, 94,000 single families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Results - robustness checks

	2021				
	(1)	(2)	(3)	(4)	(5)
	Probit	Logit	Triangular weights	Epanechnikov weights	Exclude ambiguous kindergarten cutoff
January birth	-0.062** (0.031)	-0.11** (0.056)	-0.018** (0.0083)	-0.017** (0.0080)	-0.012 (0.0075)
<i>N</i>	103,000	103,000	98,000	98,000	86,000
Control mean	0.72	0.72	0.72	0.72	0.72

Table reports the RD coefficient on employment on a sample of low-income families. Column 1 reports results of a probit regression. Column 2 reports results of a logit regression. Column 3 reports regressions results using triangular weights. Column 4 reports regression results using epanechnikov weights. Column 5 reports regressions results on sample of families who live in states where there is no ambiguity that the kindergarten cutoff is not January 1st. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9 References

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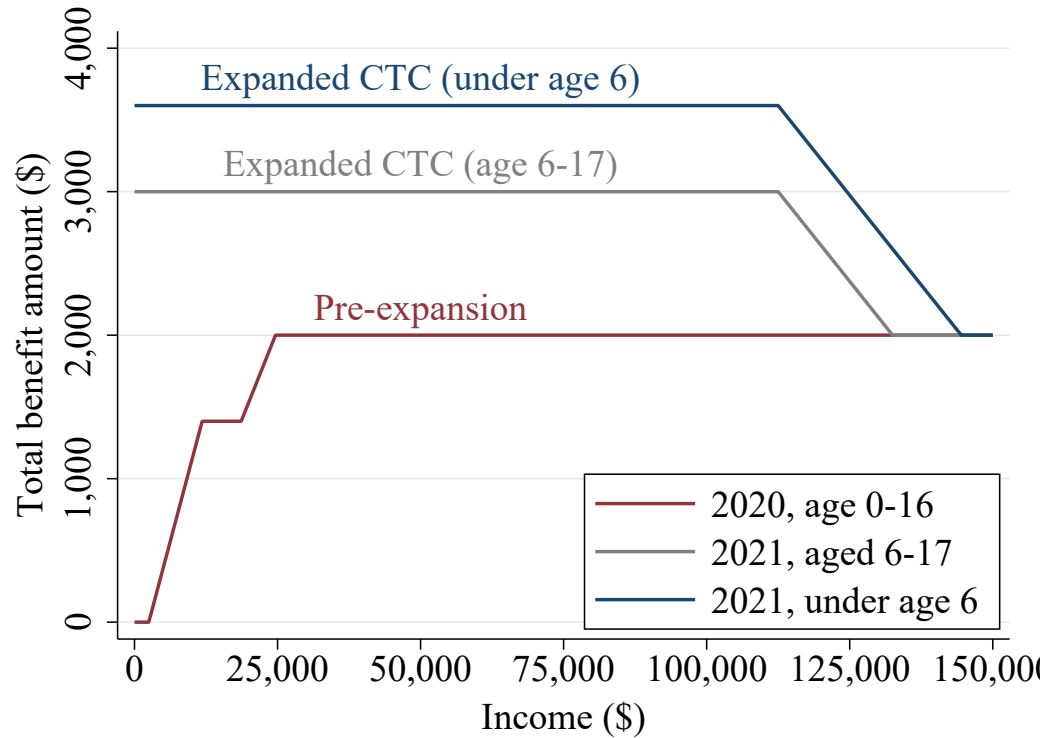
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A Appendix Tables and Figures

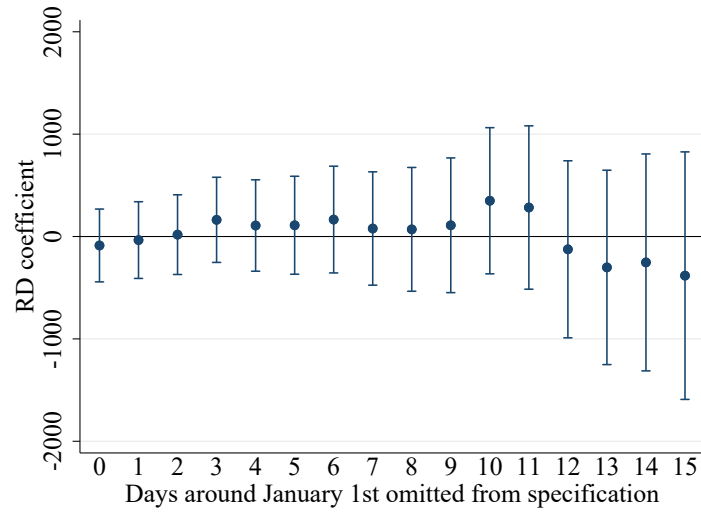
Figure A1: Background - CTC benefit schedule for head of household 2020, 2021



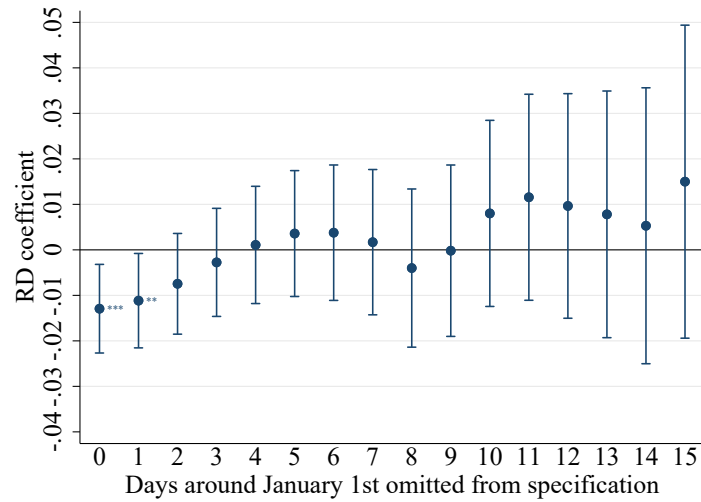
Source: Author's calculations using TAXSIM v35 (<https://taxsim.nber.org/>).

Note: The Figure shows the Child Tax Credit (CTC) benefit schedule in 2020 and 2021 by income level for those filing as head of household.

Figure A2: Validity - robustness of pre-treatment (2020) outcome to donut size



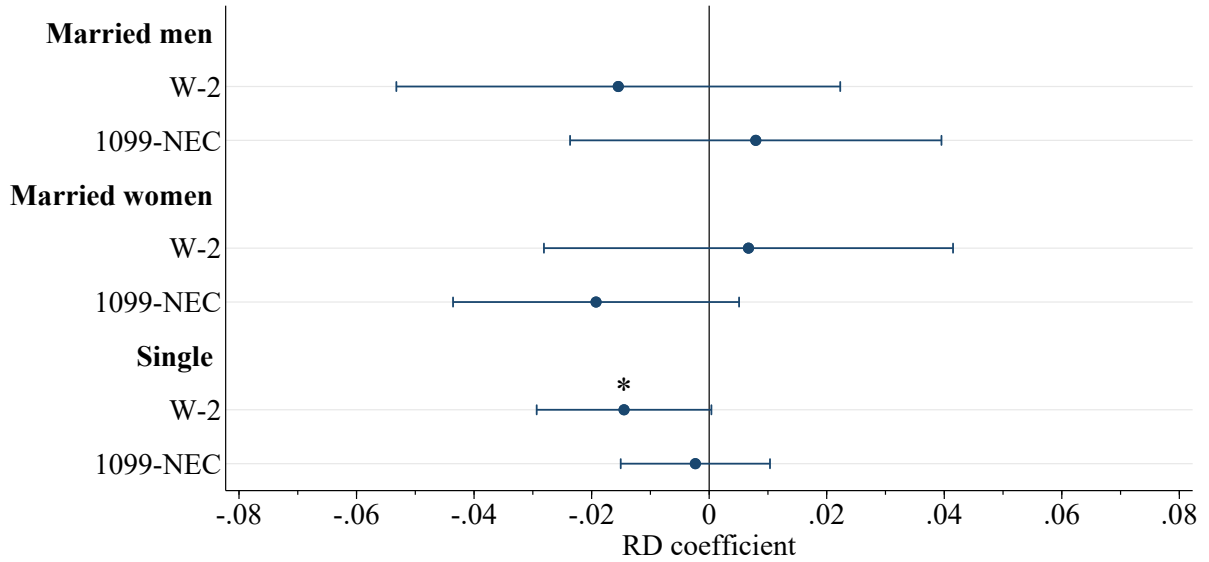
(a) Earnings (low-income sample)



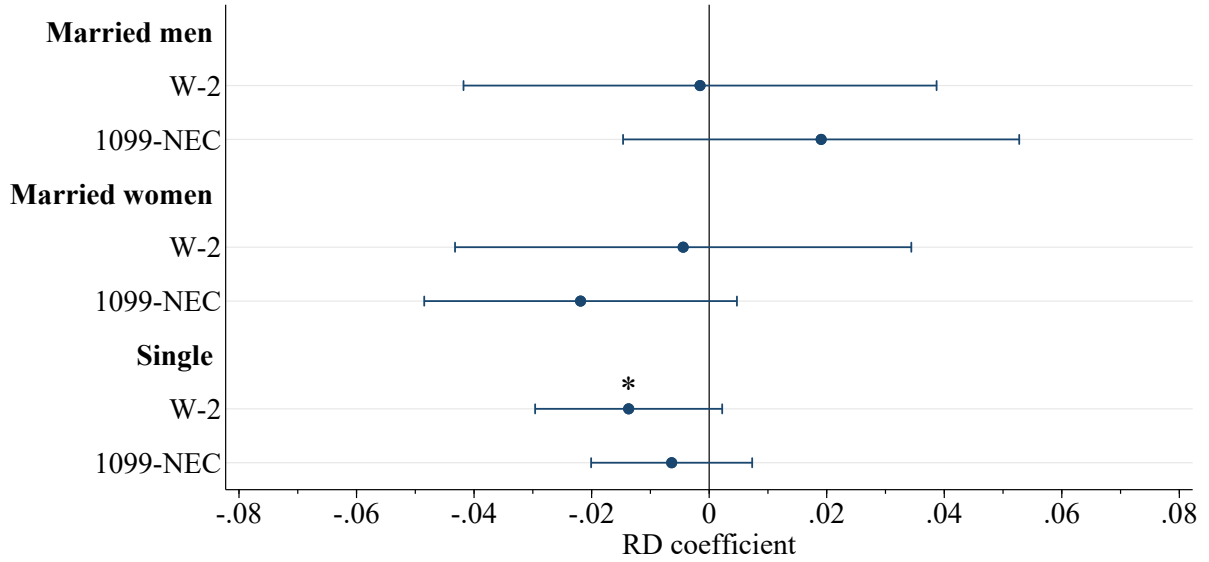
(b) Employment (low-income sample)

Figure shows a coefficient plot of the RD coefficient from the preferred specification (see note in Table 5 for details). The dependent variable is the total earnings (\$) reported on Forms W2 or 1099-NEC in 2020 (Panel A), and an indicator for any earnings reported on Forms W2 or 1099-NEC in 2020 (Panel B). The horizontal axis indicates the sample members omitted from the regression by the number of calendar days between January 1st and that child's date of birth. Standard errors are clustered by household.

Figure A3: Results - employment by income source and demographics for low-income families



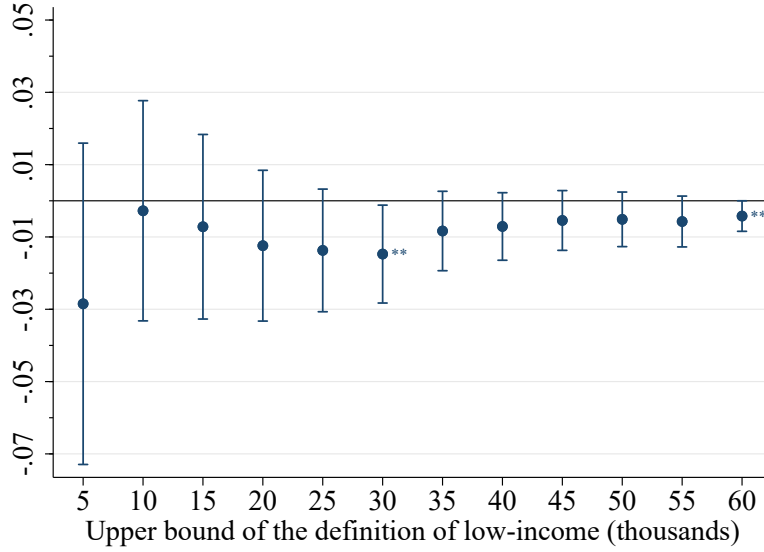
(a) Employment by income source in 2021



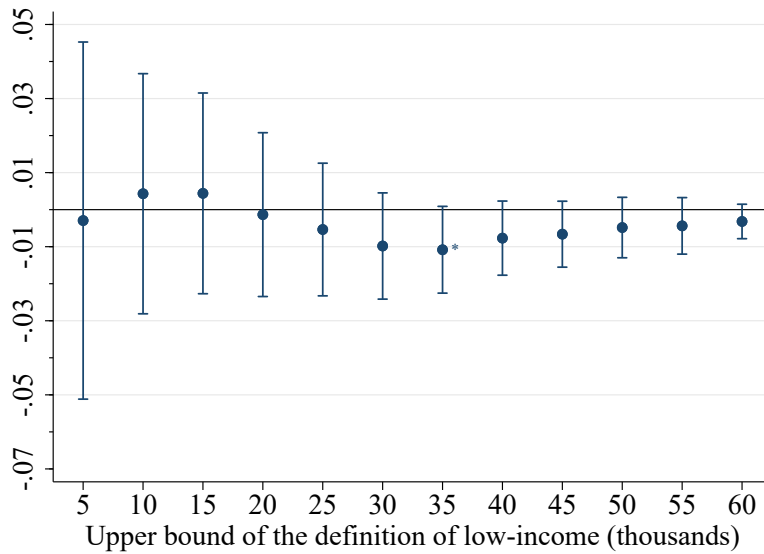
(b) Employment by income source in 2022

Note: This figure shows a coefficient plots of the RD coefficient from the preferred specification (see note in Table 5 for details). The dependent variable is an indicator for reported on Forms W2 or 1099-NEC. Regressions run separately for each demographic group. Standard errors are clustered by household.

Figure A4: Robustness - alternate definition of low-income



(a) Employment in 2021



(b) Employment in 2022

Figure shows a coefficient plot of the RD coefficient from the preferred specification (see note in Table 5 for details) where the dependent variable is an indicator for any earnings reported on information returns W2 or 1099-NEC. The horizontal axis indicates the upper bound of after-tax income when defining “low-income”. The lower bound is \$0 of after-tax income. Standard errors are clustered by household.

Table A1: First stage - Discontinuity in benefit amount for families with children

	Total CTC	Advance CTC	After-tax simulated income	Log after-tax simulated income
Middle (\$30-80k)				
January birth	600.4*** (32.1)	319.2*** (16.8)	583.1*** (19.8)	0.012*** (0.00047)
<i>N</i>	214,000	214,000	214,000	214,000
Control mean	6857.9	3246.3	53005.9	10.8
High (\$80-200k)				
January birth	410.3*** (42.4)	223.5*** (24.3)	574.5*** (35.7)	0.0054*** (0.00032)
<i>N</i>	101,000	101,000	101,000	101,000
Control mean	6533.5	3137.7	115685.6	11.6
Very high (> \$200k)				
January birth	68.9 (106.7)	31.2 (59.0)	7.30 (23.1)	0.000029 (0.000080)
<i>N</i>	17,000	17,000	17,000	17,000
Control mean	2466.3	1354.5	306299.7	12.6

Table reports the RD coefficient from the preferred specification (see note in Table 5 for details). Income group defined based on after-tax income in 2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Validity - balance of 2020 covariates in full sample

	Non-filer	Single	Number of kids	Female in household	Female \times age
January birth	-0.0023 (0.0020)	0.0010 (0.0048)	-0.0017 (0.0091)	-0.00084 (0.0032)	-0.064 (0.13)
<i>N</i>	437,000	437,000	437,000	437,000	437,000
	Male in household	Male \times age	Has mortgage	Parent student	SSDI income
January birth	-0.0016 (0.0045)	-0.023 (0.18)	0.00067 (0.0047)	-0.0024 (0.0026)	0.00076 (0.0010)
<i>N</i>	437,000	437,000	437,000	437,000	437,000
	Has interest income	Has dividend income	Employment	Total earnings	Log earnings
January birth	-0.00087 (0.0039)	0.0017 (0.0031)	-0.0018 (0.0028)	-1039.5 (821.1)	-0.0033 (0.013)
<i>N</i>	437,000	437,000	437,000	437,000	397,000

Table reports the RD coefficient on regressions where the dependent variable is a tax unit/household characteristic. All characteristics were observed in 2020, prior to the expansion of the CTC. All regressions exclude births within 8 days of January 1st. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Results - main earnings outcomes among low-income families by demographics

	Employment		Earnings		Log earnings	
	2021	2022	2021	2022	2021	2022
Married men						
January birth	-0.0094 (0.020)	0.033 (0.022)	1116.7 (1021.1)	1676.8 (1459.4)	-0.051 (0.085)	-0.11 (0.096)
<i>N</i>	16,000	16,000	16,000	16,000	6,000	6,000
Control mean	0.51	0.52	15797.9	19496.3	9.80	9.97
Married women						
January birth	-0.0046 (0.019)	-0.021 (0.021)	247.4 (454.5)	-276.4 (724.2)	0.083 (0.12)	0.037 (0.12)
<i>N</i>	16,000	16,000	16,000	16,000	4,000	4,000
Control mean	0.37	0.41	6459.7	8750.4	9.29	9.53
Single						
January birth	-0.012* (0.0075)	-0.012 (0.0079)	-409.5* (236.8)	-623.3* (338.4)	0.0039 (0.027)	-0.054* (0.028)
<i>N</i>	87,000	87,000	87,000	87,000	57,000	56,000
Control mean	0.73	0.74	13093.7	16669.9	9.36	9.63

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and description of the outcome variables). Separate regressions are run by demographic group. Sample includes all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Results - earnings by medium-, high-, and very high- income families

	Middle (\$30-80k)		High (\$80-200k)		Very high (> \$200k)	
	2021	2022	2021	2022	2021	2022
January birth	128.5 (257.3)	105.6 (367.9)	562.4 (722.6)	870.0 (1012.7)	-2248.4 (3800.2)	2946.6 (5048.1)
<i>N</i>	214,000	214,000	101,000	101,000	17,000	17,000
Control mean	51252.0	57001.3	153303.3	166340.9	334134.1	339480.8

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and description of the outcome variables). Income groups defined based on after-tax income in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Results - logged earnings by medium-, high-, and very high- income families

	Middle (\$30-80k)		High (\$80-200k)		Very high (> \$200k)	
	2021	2022	2021	2022	2021	2022
January birth	-0.0035 (0.0084)	-0.0053 (0.0098)	-0.000032 (0.0066)	0.00098 (0.0087)	-0.031 (0.022)	0.0024 (0.027)
<i>N</i>	199,000	197,000	99,000	99,000	17,000	17,000
Control mean	10.7	10.8	11.8	11.9	12.6	12.6

The table shows the RD coefficient from the preferred specification (see note in Table 5 for details and description of the outcome variables). Income groups defined based on after-tax income in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Other results - childcare expenses among low-income families

	2021		2022	
	Any childcare expense	Childcare expense	Any childcare expense	Childcare expense
January birth	-0.004 (0.007)	-91.436** (46.184)	-0.000 (0.006)	-0.893 (20.443)
<i>N</i>	81,000	81,000	71,000	71,000
Control mean	0.15	705.0	0.080	227.3

Table reports the RD coefficient on whether any childcare expenses reported to claim the Child and Dependent Care Credit (CDCC) on Form 1040 (indicator), and amount of childcare expenses reported when claiming the CDCC (dollars) in 2021 and 2022. See note in Table 5 for details about the specification. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Other results - unemployment among low-income families

	2021		2022	
	Collects unemployment	Unemployment benefit	Collects unemployment	Unemployment benefit
January birth	-0.003 (0.006)	-17.536 (106.065)	0.005 (0.004)	21.607 (21.728)
<i>N</i>	103,000	103,000	103,000	103,000
Control mean	0.22	2847.1	0.039	133.2

Table reports the RD coefficient on whether any unemployment was claimed (indicator) and the total benefit claimed (in dollars) in 2021 and 2022. See note in Table 5 for details about the specification. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: California - triple difference-in-differences with youngest

	January birth	January birth × Youngest	January birth × CA	January birth × Youngest × CA
Employment				
All low-income	-0.029** (0.013)	0.054 (0.053)	0.021 (0.016)	-0.065 (0.061)
Married men	-0.043 (0.033)	0.13 (0.11)	0.056 (0.042)	-0.035 (0.13)
Married women	-0.0024 (0.031)	0.037 (0.092)	-0.0015 (0.039)	-0.034 (0.11)
Single	-0.023 (0.015)	0.021 (0.061)	0.014 (0.017)	-0.052 (0.069)
Earnings				
All low-income	359.8 (480.1)	1809.2 (2057.4)	-667.6 (573.8)	2291.0 (2451.8)
Married men	285.9 (1605.5)	4775.5 (4910.1)	1382.8 (2071.3)	2497.4 (6200.2)
Married women	283.3 (726.6)	-2368.6 (2024.7)	-25.9 (927.9)	3431.9 (2990.4)
Single	306.9 (416.2)	1404.0 (2021.1)	-972.1* (504.5)	1495.4 (2337.8)

Each row in the table represents a regression. The first column shows the difference between low-income families with children born in January and children born in December in all of the United States excluding Connecticut. The second column shows the additional difference between children born in January and children born in December among families where the focal RD child has no younger siblings in all of the United States excluding Connecticut. The third column shows the difference between low-income families with children born in January and children born in December for California families. The fourth column shows the additional difference between children born in January and children born in December among families where the focal RD child has no younger siblings in California, and is the variable of interest. All regressions also include a lagged dependent variable and covariates. Employment is an indicator for any positive income reported on Forms W-2 and 1099-NEC. Sample is limited to low-income families. Max N=112,000; 18,000 married families, 94,000 single families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Robustness - Response on employment among low-income families

	Dependent variable: employment					
	2021			2022		
	(1)	(2)	(3)	(1)	(2)	(3)
January birth	-0.016*	-0.014*	-0.014**	-0.010	-0.008	-0.008
	(0.009)	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)
Age (days from Jan 1st)	-0.000	0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
January \times Age	0.001	-0.000	-0.000	0.001**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lagged depvar	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
N	103,000	103,000	103,000	103,000	103,000	103,000
Control mean	0.717	0.717	0.717	0.731	0.731	0.731
adj. R^2	0.000	0.371	0.387	0.000	0.274	0.297

Table reports the RD coefficients where the dependent variable is an indicator for whether any earnings is reported on Forms W-2 and 1099-NEC. Age is the relative number of days between the child's date of birth and January 1st. Controls include a lagged dependent variable and the following covariates: state fixed effects, the number of children, interactions of (quadratic) age with an indicator for a female adult in the household, indicator for filing jointly, and whether the primary taxpayer or spouse received information returns for a mortgage, student status, Social Security disability income, dividend income, or interest income. Sample includes all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Robustness - Response on earnings among low-income families

	Dependent variable: earnings					
	2021			2022		
	(1)	(2)	(3)	(1)	(2)	(3)
January birth	-104.2 (389.9)	-168.9 (268.6)	-127.3 (264.7)	-384.1 (477.8)	-446.1 (393.4)	-349.4 (386.0)
Age (days from Jan 1st)	-17.7 (12.8)	-1.1 (8.9)	-2.5 (8.8)	-16.4 (15.9)	-0.5 (13.2)	-0.9 (13.1)
January \times Age	33.0* (19.0)	11.2 (13.1)	11.1 (13.0)	41.4* (23.1)	20.5 (18.9)	14.4 (18.8)
Lagged depvar	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
<i>N</i>	103,000	103,000	103,000	103,000	103,000	103,000
Control mean	14499.2	14499.2	14499.2	18444.2	18444.2	18444.2
adj. R^2	0.000	0.519	0.535	0.000	0.323	0.353

Table reports the OLS coefficients where the dependent variable is earnings reported on Forms W-2 and 1099-NEC. Age is the relative number of days between the child's date of birth and January 1st. Controls include a lagged dependent variable and the following covariates: state fixed effects, the number of children, interactions of (quadratic) age with an indicator for a female adult in the household, indicator for filing jointly, and whether the primary taxpayer or spouse received information returns for a mortgage, student status, Social Security disability income, dividend income, or interest income. Sample includes all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Robustness - Response on log earnings among low-income families

	Dependent variable: log earnings					
	2021			2022		
	(1)	(2)	(3)	(1)	(2)	(3)
January birth	0.020 (0.030)	0.001 (0.026)	0.006 (0.026)	-0.029 (0.029)	-0.058** (0.027)	-0.055** (0.026)
Age (days from Jan 1st)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)
January \times Age	0.002 (0.001)	0.001 (0.001)	0.002 (0.001)	0.002* (0.001)	0.002 (0.001)	0.002 (0.001)
Lagged depvar	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
N	73,000	65,000	65,000	75,000	65,000	65,000
Control mean	9.4	9.4	9.4	9.6	9.7	9.7
adj. R^2	-0.000	0.283	0.304	0.000	0.168	0.197

Table reports the OLS coefficients where the dependent variable is logged earnings reported on Forms W-2 and 1099-NEC. Age is the relative number of days between the child's date of birth and January 1st. Controls include a lagged dependent variable and the following covariates: state fixed effects, the number of children, interactions of (quadratic) age with an indicator for a female adult in the household, indicator for filing jointly, and whether the primary taxpayer or spouse received information returns for a mortgage, student status, Social Security disability income, dividend income, or interest income. Sample includes all low-income families. Low-income is defined as after-tax income of \$0-30k in 2020. All regressions exclude births within 8 days of January 1st. Sample size rounded to the nearest thousand for disclosure reasons. Standard errors, clustered by household, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$