

The Effects of the Child Tax Credit on Labor Supply

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Abstract

The Child Tax Credit (CTC) is a major earnings subsidy in the US tax and transfer system, but its effects have received little research attention compared to the Earned Income Tax Credit. I identify the effects of the CTC on extensive margin labor supply using a difference-in-discontinuities design, exploiting the fact that parents lose eligibility for the credit when a child turns 17. Focusing on the credit's effects among lower-income households in the Survey of Income and Program Participation, I find that loss of the credit leads to an 8.4 percentage point reduction in the probability a child's parents are employed. The implied elasticity is at the upper bound of previous studies, consistent with an intertemporal substitution response.

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1 Introduction

The Child Tax Credit (CTC) has been a consistently favored tax policy for over twenty years, with all presidents since the Clinton administration adopting expansions to the credit. Both Republicans and Democrats have increased the CTC in recently tax reforms, and California legislators created a similar state credit in 2019 for families with young children (Matthews 2017, 2019). However, relatively little research has examined the effects of the CTC, despite its similarity as a wage subsidy to the Earned Income Tax Credit (EITC), a tax policy that has attracted considerable interest for its effects on labor supply, health, and educational outcomes (Nichols and Rothstein 2016; Hotz and Scholz 2003; Hoynes, Miller, and Simon 2015; Dahl and Lochner 2012). In this paper, I examine the labor supply effects of the CTC as an initial step towards understanding how this policy has affected behavior.

Because the EITC and CTC have similar policy designs, my results for the CTC provide further evidence of the effectiveness of tax policies structured to encourage work. Policymakers continue to express interest in expanding credits like the EITC that subsidize earnings, given their positive effects on a variety of dimensions, but most studies of the effects of the EITC were performed using data from the 1980s and 1990s (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Grogger 2003; Eissa and Hoynes 2004; Nichols and Rothstein 2016). There have been considerable changes in the economic and policy environment since that period, including welfare reform and the Great Recession, and some evidence suggests labor supply responses have declined for more recent tax reforms (Heim 2007; Kumar and Liang 2016). Furthermore, recent research has questioned the previous literature on the labor supply effects of the EITC (Kleven 2021). Thus, it is valuable for policymakers to know whether labor supply responses to expansions of refundable tax credits have remained as strong as has been found in the past. Since the CTC subsidizes wages similarly to the EITC, it can provide evidence on these responses using new econometric techniques.

Another area of recent policy interest has been the projected effects of making the CTC fully refundable, as was enacted for the single year of 2021 by the American Rescue Plan

(ARP). Different authors have found different conclusions of the labor supply effects of this policy, depending on assumptions about elasticities (Acs and Werner 2021; Corinth et al. 2022; Goldin, Maag, and Michelmore 2022). This study provides empirical results about labor supply responses that is directly relevant to these studies.

The CTC also provides evidence relevant to an ongoing debate in labor economics regarding the strength of structural elasticities. While most microeconomic evidence of labor supply responses to taxes find extensive margin elasticities in the range of 0.2 to 0.4 (Chetty et al. 2013), such results are typically identified from shocks that induce permanent changes in wages, and thus capture steady-state elasticities rather than the intertemporal substitution elasticities most useful for calibrating macroeconomic business cycle models. The CTC change I use reflects an anticipated temporary wage change, equivalent to the loss of a wage subsidy in a single year, so provides an ideal setting to measure intertemporal substitution effects from taxes for a large, low-income subpopulation.

To identify the labor supply effects of the CTC, I exploit the fact that credit eligibility is based on the number of children under age 17 in a tax unit as of the end of the tax year. For example, a family with a child born on January 1st, 1994 will receive a wage subsidy through the CTC for the entire year of 2010, while a family with a child born one day earlier on December 31st, 1993 will receive no wage subsidy for 2010. Other tax and transfer policies related to children typically change at ages 16, 18, or 19, and not age 17. If other characteristics of parents evolve smoothly around the age 17 cutoff, comparing the employment rates of parents with children just above and below the age 17 cutoff through a regression discontinuity (RD) design identifies the effect of the CTC on labor supply.

In practice, characteristics of children born in January and December differ due to seasonal factors, decisions related to timing of births, total years of exposure to the CTC policy, and incentives related to school starting ages (Bound and Jaeger 1996; Buckles and Hungerman 2013; LaLumia, Sallee, and Turner 2015), complicating a standard RD design. However, as long as these differences are consistent over time, it is possible to factor out such patterns

through a difference-in-regression discontinuities (DiRD) approach (Grembi, Nannicini, and Troiano 2016). This method combines a traditional RD and a difference-in-differences model, using changes in treatment over time between the treated and control groups (in this case, children above and below the December birth month cutoff) to identify causal effects.

I estimate a DiRD design using data from the Survey of Income and Program Participation (SIPP), focusing particularly on households in 2001 to 2016 with prior year incomes below \$20,000 (the range most subsidized by the CTC). My main results suggest an 8.4 percentage point fall in employment among low income households with children over the age 17 cutoff, giving an extensive margin elasticity with respect to the return to work of 1.04 for employment and 0.59 for labor force participation (LFP). These values are upper bounds due to intertemporal substitution effects, but a rough calibration exercise suggests these elasticities are consistent with steady-state values of 0.43 for employment and 0.47 for LFP. Thus, my results suggest that tax credits promoting employment for low-income workers continue to have substantial labor supply effects.

Section 2 discusses the history of the CTC and reviews prior literature on the credit. Section 3 examines the CTC's theoretical effects on labor supply. Section 4 describes the identification strategy and data. Section 5 provides results, and Section 6 concludes.

2 Background

2.1 History of the CTC

The Child Tax Credit arose out of tax reform in the 1980s (Crandall-Hollick 2018). Members of Congress believed that the falling real value of the dependent exemption no longer adjusted taxable income in line with a family's ability to pay, violating principles of equity; furthermore, observers noted that most other developed nations provided some form of child benefit to families (National Commission on Children 1991). Accordingly, several proposals were floated in the early 1990s to provide new tax deductions for families with children,

including an \$800 per child credit in Bill Clinton’s 1992 presidential platform (Woodward 1994)¹ and a \$500 per child credit in the Republican party’s 1994 Contract with America (Crandall-Hollick 2018). After the parties failed to reach an agreement in 1995, the issue remained on the Washington agenda, and the CTC became law as part of the Taxpayer Relief Act of 1997, which provided a credit of \$500 per child under age 17.² The original credit was effectively nonrefundable (i.e., unavailable to families that did not owe taxes), as it could only be claimed up to the amount of tax liability.³

A notable element of the introduction of the credit was the limitation of the tax break to families with children under 17. Negotiators in the House, Senate, and Clinton administration had proposed various age cutoffs for the credit, ranging from age 13 to age 18, but the final proposal adopted age 17 as a compromise (Esenwein and Taylor 1997; Stevenson 1997). Importantly, this age 17 cutoff is unlike the other age cutoffs for child benefits in the tax code; parents can claim the dependent exemption and the EITC until their children turn 19 (or 24 when the child is a full-time student). This institutional detail is key to my identification of the credit’s effects.

Four years after its enactment, the CTC expanded substantially as part of the Bush tax cuts of the early 2000s (Crandall-Hollick 2018). The credit value was increased to \$600 in 2001 and \$1,000 in 2003, and the 2001 tax cut also gave the CTC a refundable component increasing its potential subsidy to work. The credit’s refundable component, officially called the “Additional Child Tax Credit” (but referred to as the “refundable portion” of the CTC

¹Clinton’s campaign proposed this credit, but it was not pursued after his election due to budgetary concerns (Woodward 1994, p. 31 and 154). However, Clinton’s Treasury department proposed a \$300 CTC in response to the Republican credit (Crandall-Hollick 2018), suggesting the issue was not forgotten by either party.

²In the initial year of 1998, the credit was \$400, but could be claimed early through reduced withholding (Michel and Ahmad 2012).

³The original CTC was refundable for families with three or more children, but only to the extent that their payroll taxes exceeded the family’s EITC. Accordingly, refundable CTC amounts were only 3-5% of total CTC expenditures in the first few years of the credit (author’s calculations from Internal Revenue Service 2018). This alternate payment formula still exists in current law, but as a family with three or more children will almost always have EITC exceeding payroll taxes at low levels of income (as the EITC subsidizes earnings at a 34 to 45% rate, whereas payroll taxes are 7.65%), this formula is rarely used and is not a focus in this or other studies of the CTC.

in this paper), was adjusted so that taxpayers owing taxes less than their maximum CTC could claim the remaining CTC amount up to 10% of earnings above \$10,000, with the threshold adjusted for inflation.⁴ One rationale for this initial threshold was to counteract high marginal tax rates from the EITC at low levels of income; the policy intention was to have the CTC phase in as the EITC phased out, lowering overall work disincentives (Sawhill and Thomas 2001). Further expansions in this vein followed; the subsidy amount was increased to 15% in 2004, and the nominal threshold amount was lowered to \$3,000 in 2009 as part of economic stimulus legislation during the Great Recession (Crandall-Hollick 2018). The 2017 Tax Cuts and Jobs Act (TCJA) lowered the threshold slightly, to \$2,500, and increased the total amount of the credit available to \$1,400 per child (or \$2,000 when nonrefundable). Thus, the CTC has subsidized income at low levels in a manner similar to the EITC since 2001.⁵ (The main CTC parameters are listed in Table C.1).

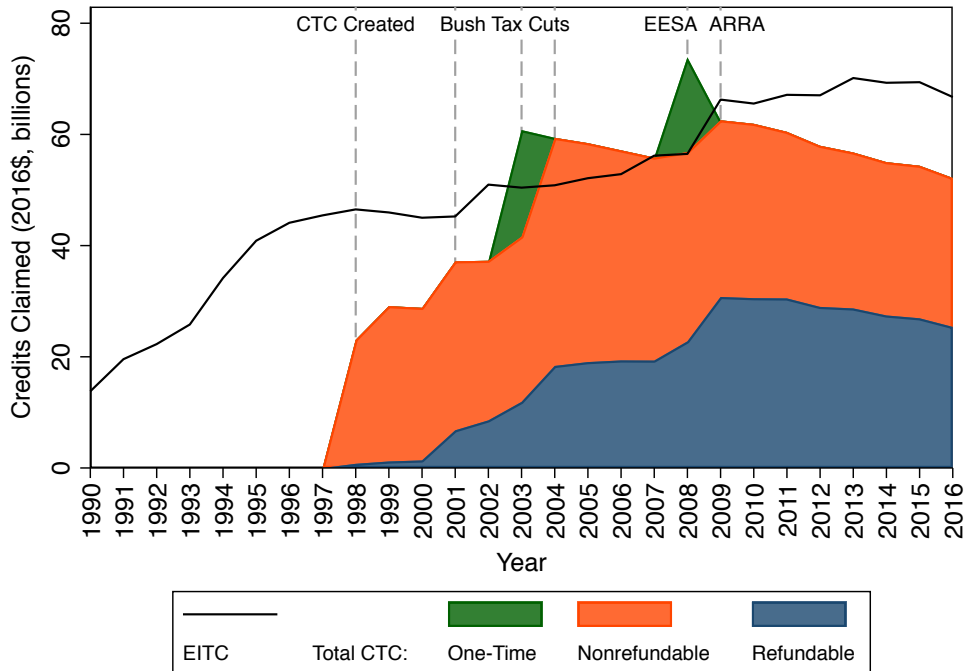
Because the maximum value of the CTC has never been indexed for inflation, the real value of the credit has fallen over time outside of legislated expansions. Figure 1 presents total spending on the CTC until 2016, as compared to the EITC; the discrete jumps in the level of the the CTC with legislated expansions are clear, as well as the gradually declining value of the credit in real terms. Notably, the total dollars distributed through the CTC surpassed the EITC briefly in 2003, and have remained at comparable levels ever since, indicating the importance of the CTC in the tax and transfer system.⁶

⁴As Steuerle (2004) notes, this proposal effectively adopted the CTC policy proposed by Sawhill and Thomas (2001).

⁵The 2021 CTC was made fully refundable due to changes in the American Rescue Plan, but that is outside the time period of this paper.

⁶The calculations for total CTC received in this figure differ in 2003 and 2008 from those in other sources, such as from the Tax Policy Center (2019) and Hoynes and Rothstein (2016), because the IRS Statistics of Income series on which such figures are based do not include either 1) the 2003 CTC expansion, which was distributed as a rebate to taxpayers based on their 2002 incomes in advance of the usual tax filing period (Internal Revenue Service 2005, p. 12), and 2) the Recovery Rebate Credit enacted by the Economic Stimulus Act of 2008, which gave a \$300 tax credit to nearly all taxpayers for each CTC eligible child (based on 2007 tax information), and was thus effectively a one-time expansion of the CTC. The 2003 one-time rebate is further discussed by Johnson, Parker, and Souleles (2009) and Michel and Ahmad (2012), while the 2008 rebate is discussed by Parker et al. (2013) and Shapiro and Slemrod (2009). Thus, Figure 1 adds the amount of CTC distributed early (\$14.7 billion 2003 dollars, as reported by the the Treasury Inspector General for Tax Administration (TIGTA) (2004, p. 16)) to the CTC in 2003, and the amount of the Recovery Rebate Credit that I calculate as accruing to families with children (computed as the total base CTC times

Figure 1: Expenditures on the CTC and EITC through 2016



Source: Author’s calculations from Internal Revenue Service (2018, Table A) and TIGTA (2004).

Notes: EESA refers to the 2008 tax year stimulus bills (both the Economic Stimulus Act and the Emergency Economic Stabilization Act), and ARRA is the 2009 Obama stimulus package (American Recovery and Reinvestment Act). “One-time” areas refer to expansions of the credit that lasted for only one year.

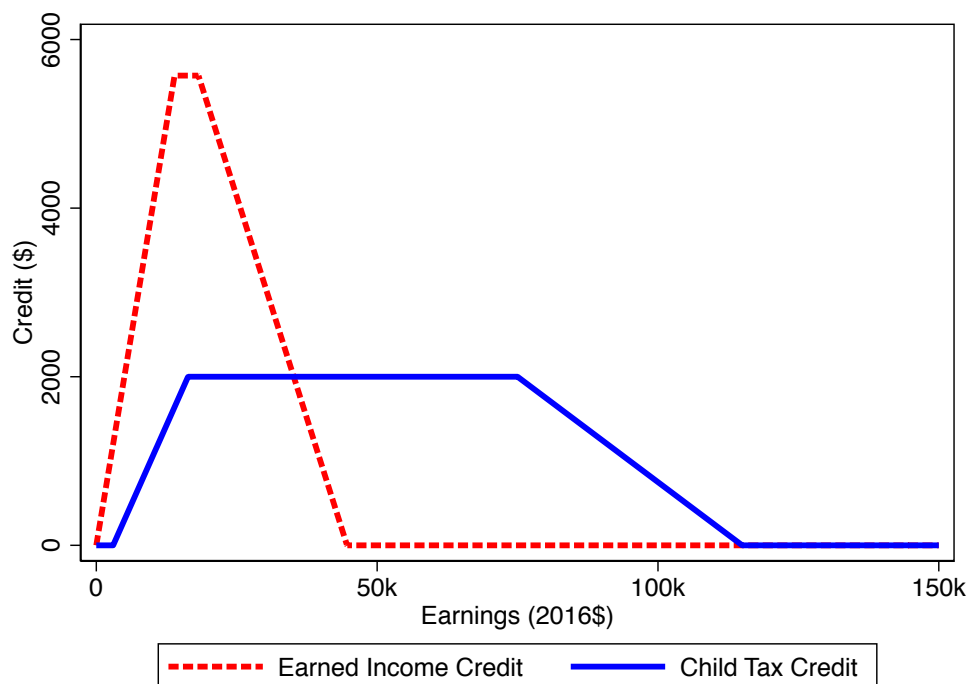
2.2 Design of the CTC

To illustrate how the Child Tax Credit affects a family’s tax liability during the period of my study, Figure 2 compares the 2016 amount of the CTC to the EITC for a single parent with two children and all income from wages, salaries, or self-employment (earned income), computed using the Taxsim model (Feenberg and Coutts 1993).⁷ The two credits have virtually identical designs in practice: each subsidizes earned income at a given rate (40% for a two-child EITC, and 15% for the CTC) up to a maximum level, remains constant with income for a plateau period, then is phased out (at a 21.06% and 5% rate, respectively) with increasing income. The CTC is smaller in value than the EITC, but extends to higher

0.3; this is a slight underestimate, as the credit also included more generous phase-ins and phase-outs than the regular CTC) to the CTC in 2008.

⁷Results are very similar for 1) married taxpayers, who face the same slopes but begin the phase-out for both the EITC and CTC at a higher income level, and 2) differing numbers of children, which rescales the maximum level of the credits but does not change the slopes.

Figure 2: Amount of CTC and EITC for a Single Parent with Two Children, 2016 Tax Law

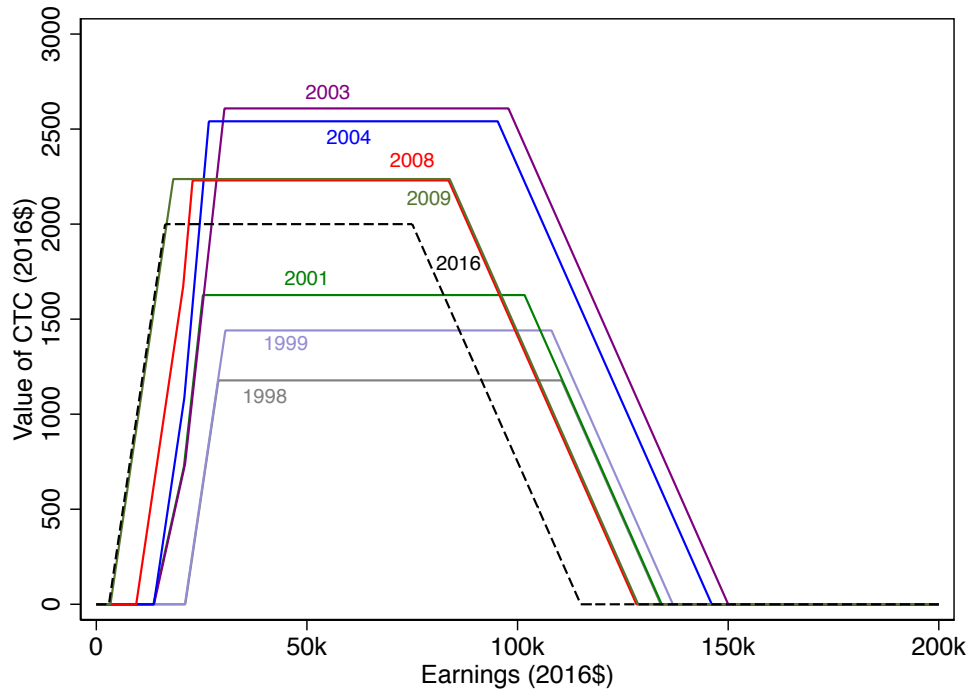


Source: Author's calculations using Taxsim. Calculations assume no unearned income or itemized deductions.

income levels, meaning it provides its subsidy to most families in the population; the CTC also subsidizes earnings over \$3,000 (or \$2,500 since 2018), rather than subsidizing from the first dollar as for the EITC.

I next illustrate in Figure 3 the changes to the CTC's design over time, with all values adjusted to 2016 dollars. A few key factors stand out. First of all, the CTC has maintained a similar structure, with the value of the credit phasing in and out at similar rates; this held true even prior to the enactment of the refundable CTC, because the credit offsets taxes in the lowest rate bracket. Secondly, the credit has fluctuated in value over time; its real value was lowest at enactment in 1998, rose in the mid 2000s due to the Bush tax cuts, and fell from 2003 to 2016 due to inflation. Thirdly, a large percentage of the credit's benefits have shifted from wealthier families to low-income families, due to both 1) legislated reductions in the point where the refundable credit phases in for lower incomes, and 2) inflation lowering the real income level where the credit phases out (\$75,000 in nominal dollars for a single

Figure 3: Amount of CTC for a Single Parent with Two Children, Tax Laws for Various Years



Source: Author’s calculations using Taxsim. Calculations assume no unearned income or itemized deductions. Years listed are those in the study period where the main CTC parameters were changed by law, as well as 2016.

parent). The schedule of credits for other family arrangements are similar, and are shown in Appendix Figure C.1.⁸

An implication of these calculations is that the bulk of the CTC for most families comes through the refundable CTC formula of 15% of earnings over a threshold. While it is possible to receive a nonrefundable CTC that offsets taxes on unearned income, the standard deduction and exemptions for most taxpayers reduce taxable income to zero in the range where the CTC phases in, meaning there is no tax to offset. Nonrefundable credits are thus of little value to families with income under \$30,000, and most such families receive a CTC that is directly based on earnings.⁹

⁸Note that after my period of study, the 2018 credit was expanded considerably by the TCJA; however, the dependent exemption was also eliminated by that bill, so the new CTC is better compared to total tax benefits from the CTC plus dependent exemption in earlier years. Because this comparison is less straightforward, I do not yet examine the TCJA period in this paper. The American Rescue Plan also made the CTC fully refundable for 2021 only, but SIPP data for 2021 is not yet available.

⁹For example, Williams (2013) finds over 99.5% of tax units with children and incomes under \$30,000

Overall, the CTC functions very similarly to the EITC, providing a directly subsidy to earnings above a low threshold level. Theoretically, this means the CTC should encourage increased labor supply, as discussed in Section 3.

2.3 Prior Research on Labor Supply Effects of the CTC and Other Tax Credits

As noted by Marr et al. (2015), the effects of the CTC by itself are typically not identified in the literature; this occurs in part because the CTC and EITC phase in over similar income ranges, making it difficult to disentangle their effects. One of the few exceptions is Zheng (2020), which uses a difference-in-differences strategy with CTC expansions through 2013 and finds elasticity values similar to the ones in this paper.

The most relevant study on the CTC comes from Feldman, Katusčák, and Kawano (2016), which uses the loss of the CTC when children turn 17 as a source of exogenous variation (in the same manner as this paper). However, this study focuses on higher income households, for whom the loss of the CTC is a lump sum change in taxes (and thus should generate only an income effect on labor supply). The authors find that loss of the CTC leads to a 0.5 percentage point reduction in earnings growth for the *next* year (an implied intensive margin elasticity of 0.3), but no change in earnings in the year for which credit eligibility is lost. They interpret this effect as taxpayer “confusion” between marginal and average tax rates.¹⁰ While this is suggestive of no intensive margin response to the loss of the

owe no income tax in 2013. Those figures include the CTC, but my calculations with my 2001-2016 SIPP panel and Taxsim confirm that 96.0% of such families owe no income tax in this dataset, and 94.3% of such families would owe no tax even without the nonrefundable CTC.

¹⁰Notably, the authors do not discuss the potential role of average tax rates on labor supply; while they interpret their finding as a reduction in labor supply that is irrational, because marginal rates have not changed, the theoretical results presented in Section 3 suggest that it can be optimal to change labor supply in response to a change in *average* rates in the presence of fixed costs of work. Feldman et al.’s robustness checks also show that the effect on labor income is strongest for households with lower incomes (the \$30,000-\$55,000 range), which could be consistent with extensive margin responses by a portion of this group. Furthermore, because they examine labor supply in the year *after* CTC eligibility changes, their use of variation in eligibility for the CTC cannot be separated from the effect of children turning 18 in the tax year, which could have effects on labor supply through channels other than the CTC.

CTC, their paper only examines taxpayers with prior year income from \$30,000 to \$100,000, who are unlikely to be on the extensive margin. My study therefore expands on Feldman and coauthors by including lower income taxpayers and studying extensive margin changes, where EITC studies have shown stronger effects.¹¹ Because extensive margin responses are less subject to frictions (Chetty 2012), it is plausible that the intensive margin elasticities found in Feldman et al.'s paper will be a lower bound for the extensive margin effect of the credit.

Looney and Singhal (2006) examine changes in marginal tax rates associated with the loss of a dependent as children age (relative to families that do not lose a dependent).¹² While their method is similar to the one used in this study, exploiting changes in children's ages as a source of identification, their exogenous change in taxes uses the loss of dependent status rather than the loss of the CTC. A limitation of this approach is that children can be claimed as dependents until age 19 or age 24 if a full-time student; thus, the loss of the dependent exemption for a household can be correlated with endogenous college enrollment decisions. Also, their data only include the first two years of the CTC, when the credit was not refundable, and focuses on changes in the intensive margin (using marginal tax rates) rather than the extensive margin (with average tax rates). Their intensive margin elasticity estimate of 0.75 is close to those reported in studies of the EITC; this estimate includes individual fixed effects, and is thus an intertemporal labor supply elasticity (and therefore similar to the temporary wage variation I identify).

Another recent paper examines the effects of child tax benefits (including the CTC) on the labor supply of mothers immediately following their children's births, also using a discontinuity in child age around the end of the tax year (Wingender and LaLumia 2017). The authors find that an additional \$1,000 of child tax benefits from having a December rather than a January birth leads to a 5-6 percentage point reduction in maternal labor

¹¹These authors showed similar results to their 2016 study using SIPP data in a previous working paper (Feldman and Katusčák 2012); this study expands on those results by examining the extensive margin.

¹²Dokko (2007) provides a similar study that examines changes in labor supply in response to the aging of dependents.

supply in the third month following the birth. My study examines a different margin, the change in *prior year* incomes based on *fully anticipated* variation in the tax schedule. To the extent that the exact timing of a new birth could fall in either December or January, one would not necessarily expect mothers to anticipate their eligibility for tax benefits and alter their labor supply during pregnancy; indeed, as Wingender and LaLumia (2017) show in their Figure 2, mothers of newborns do *not* anticipate the change in eligibility for subsidies, as there is no discontinuity in their earnings in the year prior to birth. Thus, my result illustrates how parents respond to a fully anticipated tax change rather than an unanticipated income shock. Additionally, mothers of newborn children tend to have low levels of labor supply and unique needs in terms of childcare, meaning their responsiveness to taxes may be less representative of the overall population than the older parents that I study.

Prior research on the EITC has typically found strong labor supply elasticities, around 0.7 on the extensive margin and 0.1 on the intensive margin (Hotz and Scholz 2003; Nichols and Rothstein 2016; Meyer and Rosenbaum 2001). As a representative example, Grogger (2003) finds that a \$1,000 increase in the EITC is associated with a 3.6 percentage point increase in employment for single mothers, which roughly corresponds to an elasticity of 0.64.¹³ However, these estimates are typically identified from difference-in-differences approaches, which rely on unanticipated, permanent changes to the tax system (and thus identify steady-state elasticities, in the terminology of Chetty et al. (2013)); as intertemporal substitution Frisch elasticities are bounded below by steady-state Hicksian elasticities (Browning 2005), the results from this study may be expected to be higher than the EITC results.

A recent strand of research has questioned some of the earlier findings of strong extensive margin effects of the EITC. Chetty, Friedman, and Saez (2013) find a smaller effect using a more plausibly exogenous identification strategy based on differences in knowledge across neighborhoods; their extensive margin elasticity is around 0.19 (but 0.6 in the top decile

¹³This elasticity is computed based on an average income of \$12,300 in 1998 dollars and employment rate of 69 percent (per Table 1 in Grogger (2003)), and thus is somewhat overstated because it uses gross income rather than the return to work (Chetty et al. 2013).

of knowledge about the EITC). Kleven (2021) criticizes the inconsistent responses to the EITC in difference-in-differences studies and provides evidence that the extensive margin elasticity is closer to zero. More generally, Heim (2007) and Kumar and Liang (2016) have found declining population-wide labor supply elasticities over time; these may be related to increasing participation of women in the labor force and the effects of welfare reform, both of which could leave fewer workers on the extensive margin. Thus, it will be of interest to see if the measured effects of the CTC in a later time period and using a different identification strategy are of similar magnitude to those found for the EITC.

3 Theoretical Effects of the CTC on Labor Supply

The effects of the CTC on labor supply have not been thoroughly examined; as Marr et al. (2015) note, the CTC has not been studied extensively, due to its relatively recent introduction to the tax code. In this section I discuss the theoretical effects of the CTC on labor supply, drawing on a standard labor supply model, the fixed cost model of Eissa, Kleven, and Kreiner (2008), and a new dynamic fixed cost model.

3.1 Static Intensive Margin Effects

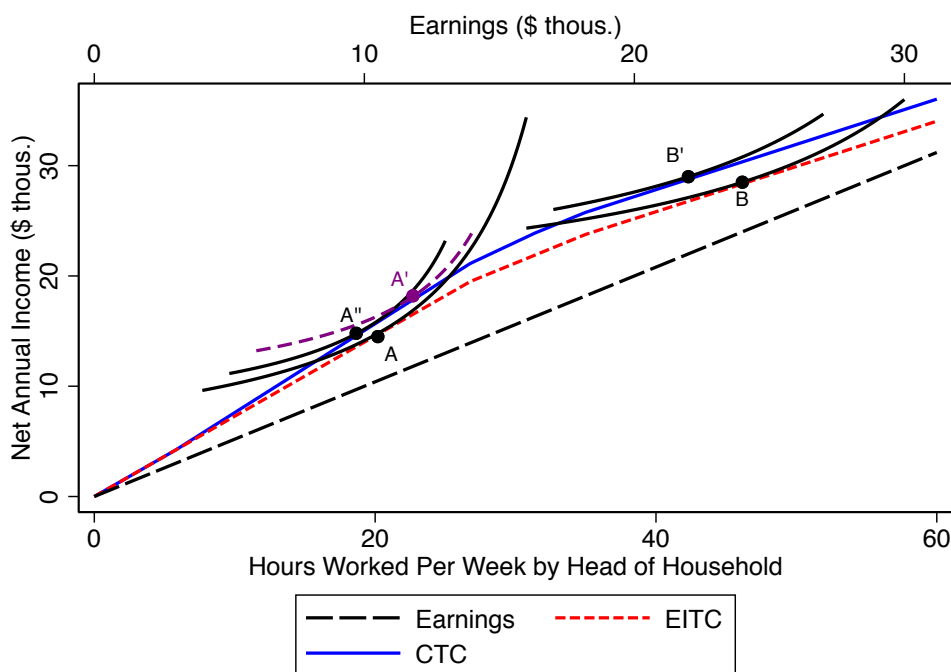
I first examine the CTC through the lens of a standard labor-leisure tradeoff, considering households that maximize utility $U(c, l)$ over bundles composed of hours of leisure l and consumption c . Household budget sets are given by earned income (an exogenous wage w times hours of work h), less possibly nonlinear taxes $T(wh)$, yielding

$$c = wh - T(wh)$$

while hours of leisure are equal to a time endowment e less hours of work h .

Notably, in the presence of an EITC and CTC, the budget set takes on the familiar “hump” shape seen in Nichols and Rothstein (2016, Figure 2.9). This shape is depicted in

Figure 4: Budget Set with EITC and CTC for a Single Worker with Two Children in 2016



Source: Author’s calculations using Taxsim. Household consists of a single parent with two children, working for 52 weeks at \$10.00 per hour.

Figure 4. To generate empirically relevant estimates, I compute results for a single worker with two children using Taxsim, assuming 52 weeks of work per year at a wage of \$10.00 per hour. Since hours of work reduce leisure, indifference curves are increasing to the upper left of this figure.

The EITC (red dashed line) generates a “hump” on the simple linear budget set *wh* (black line), first subsidizing income in the phase-in region, then remaining constant as a plateau, and finally phasing out with additional hours of work. The CTC (blue solid line) augments the effects of the EITC by adding a new wage subsidy above \$3,000 in earnings, which continues phasing in until about 31 hours of work per week. At this point, the CTC becomes a lump sum transfer; because the CTC does not phase out until a high income level, a low wage worker sees no increase in marginal tax rates at plausible values of hours worked.

Thus, the CTC effectively increases the subsidy provided by the EITC, providing differential effects on hours worked depending on whether a worker is in the subsidy or plateau region. In the subsidy region, the effect on labor supply is ambiguous, because the positive

substitution effect towards work could be counterbalanced by a negative income effect. If the substitution effect dominates, the worker would move to an indifference curve with higher work hours, as from point A to point A' ; if the reverse is true, the worker would move to an indifference curve that decreases work hours, such as A'' . However, in the plateau and phase-out regions, the labor supply effect of the CTC is theoretically predicted to be negative. On the plateau, the consumer faces the same wage rate as without a CTC, so experiences a pure income effect increasing leisure. The consumer should thus shift from an indifference curve like B to a curve like B' and reduce hours of work. For the phase-out range, this process is amplified by a negative substitution effect on hours.

3.2 Static Extensive Margin Effects

Given prior research on the EITC that finds the strongest effects on the extensive margin, it is important to simulate extensive margin behavior in detail to get an accurate picture of the CTC's effects. Therefore, I consider a static model incorporating fixed costs of work, following Eissa, Kleven, and Kreiner (2008). As these authors note, fixed costs of work are important for tax reforms to generate welfare effects on the extensive margin; if extensive margin responses arise only from continuous movements across a reservation wage distribution, any tax revenue (and thus welfare) effects will be infinitesimal for a small change in after-tax wages. This detail, along with the fact that low hours of work are rarely observed empirically, suggests a model with fixed costs is more appropriate than the standard model to assess the CTC.

To illustrate such a model graphically, I augment the simplified budget set in Figure 4 by adding fixed costs of work to the model. I also add the value of transfer programs and taxes, using the Urban Institute's Net Income Change Calculator (NICC, 2016).¹⁴ I use NICC to

¹⁴The NICC is a tool that allows users to input family characteristics and receive computations of the levels of taxes and transfer programs for those families. It has been used previously in the tax literature by Maag et al. (2012). The NICC uses 2012 tax and transfer rules, but to make the results more comparable to the present day, I augment the NICC calculations by adjusting SNAP benefits to remove the temporary increases of 2009 stimulus legislation, remove the temporary payroll tax cut in place from 2011 to 2012, and

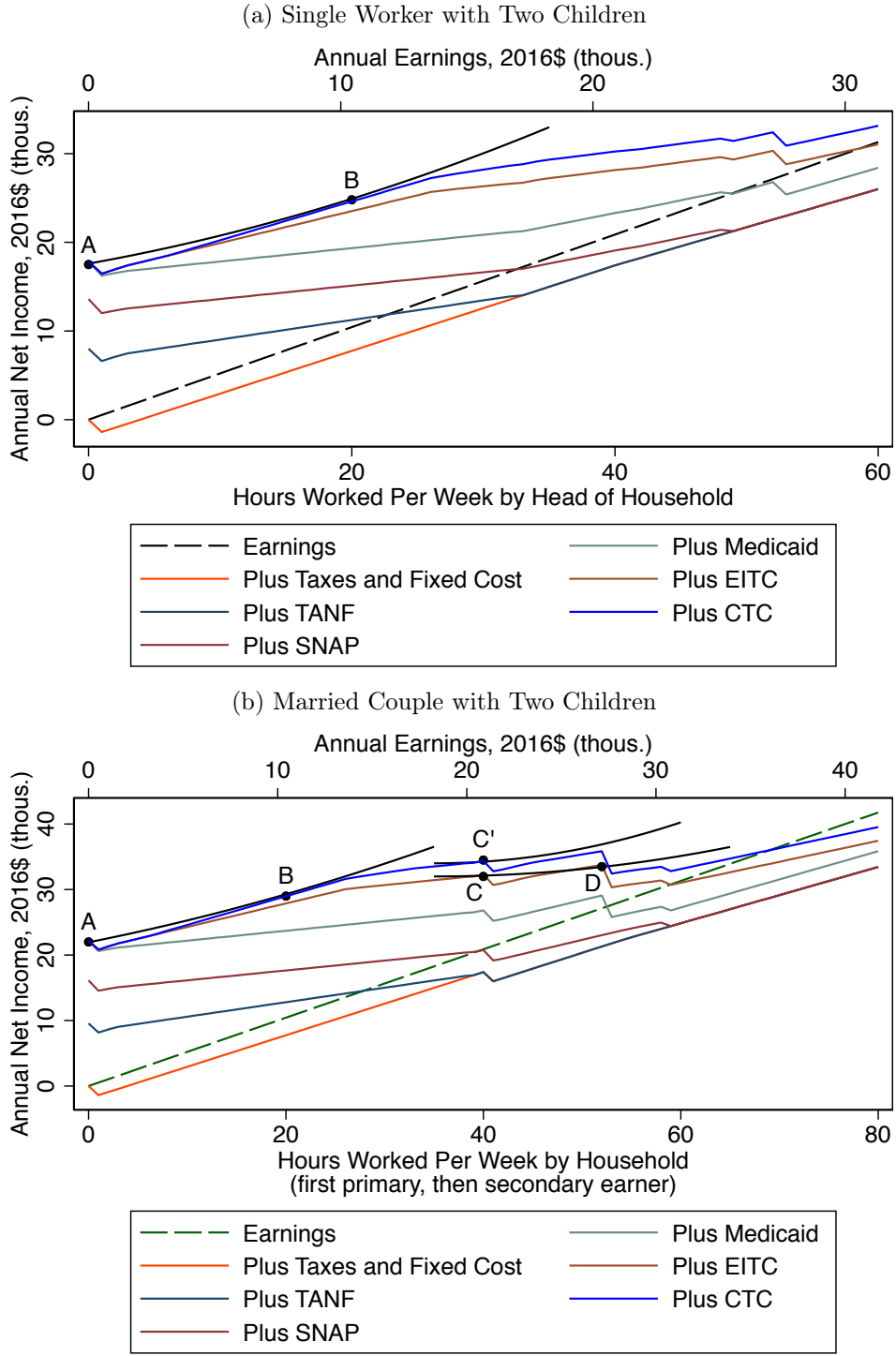
compute the value of welfare benefits (Temporary Assistance for Needy Families (TANF) and the Supplemental Nutrition Assistance Program (SNAP, formerly food stamps)) and income and payroll taxes for the example household, and assume that the worker will incur \$150 per month in fixed costs when working positive hours (which generates extensive margin effects). The results in Figure 5a indicate that the CTC should have positive extensive margin labor supply effects for single parents. While the budget set has numerous kinks and cliffs, the CTC (the topmost blue line) offsets fixed costs; as shown in the figure, a household located at a corner solution with no work (point *A*) could be induced to work positive hours (point *B*) by the CTC's subsidy.

I also illustrate a similar budget set for a married couple with two children in Figure 5b. This model assumes both individuals work at the same wage, and that one individual (the primary earner) first works up to 40 hours per week, after which the secondary earner begins work up to 40 hours per week. Work expenses are incurred after each individual begins to work. In this scenario, the CTC (topmost blue line) again provides a positive substitution effect on the primary earner's labor supply, potentially moving the family from no work (point *A*) to work by the primary earner (point *B*). However, the CTC is fully phased in when the secondary earner begins to work, and thus only has an income effect for this worker. This raises the reservation wage, and could potentially move a secondary earner who was indifferent about working (between points *C* and *D*, so working enough to locate at the Medicaid cliff described in Yelowitz (1995)) to not working at all (point *C'*). Since this response only comes through an income effect, the magnitude is likely small (as income effects tend to be dominated by substitution effects for secondary earners, who have positive

add the 2014 value of Medicaid and CHIP under the Affordable Care Act (valuing at the cost per enrollee from Kaiser Family Foundation (2019), multiplied by 0.5 to reflect valuation of Medicaid below cost, as discussed in Finkelstein, Hendren, and Luttmer (2019)). Thus, the transfer system depicted in the figures roughly reflects the taxes and transfer system in 2016.

I assume that the example family lives in California, has no income or assets besides labor income, pays \$600 in monthly rent, and works for 52 weeks at a wage of \$9.60 per hour in 2012 dollars (which makes each hour increase in work increase annual income by roughly \$500, and is roughly equal to the \$9.57 deflated value of the \$10 2016 California minimum wage). Note that TANF and Medicaid policies in California are typically more generous than in other states, although the other tax and transfer policies used for this exercise are mostly consistent across the country.

Figure 5: Budget Sets with Fixed Costs Illustrating Extensive Margin Effects



Source: Urban Institute (2016) and author's calculations (see text for details). Household consists of a single parent or married couple with two children, working for 52 weeks at roughly \$10.00 per hour. Assumes \$150 per month in work expenses, incurred once when the primary earner begins to work, and again when the secondary earner begins to work (after the primary earner works 40 hours).

compensated elasticities (Blundell and Macurdy 1999)). Thus, the implication of this model is that the CTC should encourage labor supply by the primary earner in low-income families, but have little or negative effects for secondary earners in those families.

Thinking intuitively about these findings, the CTC lowers the average tax rate of working for primary earners but not secondary earners (as it has typically already phased in for secondary earners). As Eissa, Kleven, and Kreiner (2008) discuss, the relevant parameter for extensive margin labor supply decisions is the average tax rate rather than the marginal tax rate. Because the CTC lowers average tax rates on work, it makes work unambiguously more attractive to primary earners.

3.3 A New Model of Dynamic Extensive Margin Effects

The nature of the variation in this study means I must also consider intertemporal substitution effects. My design compares families with children just over age 17 to those just under age 17 at the end of a tax year. Thus, the “control group” children in the natural experiment of the RD design will turn 17 and lose the credit next year; the two groups only face different tax rates for the one year period of “treatment.” This design is similar to that used by Fehr and Goette (2007), who randomly increased the wages of Swiss bicycle messengers in two alternating periods to investigate intertemporal substitution. In both cases, a group with a net wage change is compared to a control group that will have a similar wage change in the next period, creating transitory and anticipated income variation. My identification strategy thus provides estimates of an intertemporal substitution elasticity.

It is well known that for intensive margin elasticities with additively separable utility, one can link the steady-state (static) elasticity to the intertemporal substitution elasticity using the formula (Browning, Deaton, and Irish 1985; Chetty 2012):

$$\epsilon_S = \epsilon_I - \rho (\omega)^2 \frac{A}{wh} \tag{1}$$

which relates the compensated steady-state intensive elasticity ϵ_S to the intertemporal substitution intensive elasticity ϵ_I via the elasticity of intertemporal (consumption) substitution ρ , the income effect ω , the level of assets A , and earnings wh . Note that the intertemporal substitution elasticity is (weakly) greater than the steady-state elasticity.

To understand how the elasticity estimates from this study compare to steady-state estimates from other studies, I derive a similar formula to equation (1) relating the two elasticities in the extensive margin case. To do so, I develop a dynamic model of discrete labor force participation extending the work of Eissa, Kleven, and Kreiner (2008). A summary of the model follows; full details are given in Appendix B.

Consider a continuum of households who live for τ periods indexed by $t = 1, \dots, \tau$ and maximize a concave, continuous utility function $U [C_t]$ over consumption C_t to solve

$$\begin{aligned} V [A_t, \theta_t] &= \max_{p_t, A_{t+1}} U [C_t] - Fp_t + \beta E_t [V [A_{t+1}, \theta_{t+1}]] \\ \text{s.t. } A_{t+1} &= (1 + r_t) (A_t + Y_t - C_t) \\ Y_t &= N_t + G_t + p_t w_t (1 - a_t) \end{aligned}$$

where p_t is an indicator for labor force participation, A_t are assets, Y_t is non-asset income, and β is a discount rate. Wages w_t , non-labor income N_t , government transfers G_t , and interest rates r_t are given exogenously in each period. The parameter $\theta_t = \{w_t, G_t, N_t, \nu_t, r_t\}$ is a vector of the exogenous state variables.

The key extensions of the model relative to a standard dynamic labor supply model (such as Macurdy 1981) are the inclusion of fixed costs of work and taxes. Households are identical except for a varying disutility of participating in work F distributed according to a generic CDF $\phi [F]$, drawn once and then held fixed for each household. I define a_t as the average tax rate faced when going to work (due to taxes paid and transfers forgone). Given these parameters, households choose whether to participate in work and how much they will save for the next period.

To simplify notation, I define variables with a superscript as those evaluated at the superscript level of p_t . For example, U^1 (or U^0) is utility evaluated at the working (nonworking) state in that period. I then define $\Delta x = x^1 - x^0$ as the difference between variables in the work and non-work states.

The solution to the model involves an Euler equation pinning down an optimal savings level in each work state, $A_{t+1}^{*p_t}$ (depending on marginal utility in each state λ^{p_t}) and a cutoff condition

$$\Delta V^* = \Delta U + \beta E_t [\Delta V_{t+1}] \geq F \quad (2)$$

As noted by Blundell and Macurdy (1999), extensive margin labor supply models in a dynamic setting do not typically have simple closed-form solutions, because the amount of assets saved in each period will depend on past and future work decisions (unlike models of intensive margin labor supply, where the savings decision and work decision can be considered independently). To make the model tractable, I consider the case where individuals are making decisions in response to small changes in tax rates. Thus, we can approximate optimal savings with a first order Taylor expansion,

$$\Delta A_{t+1}^* = \alpha \Delta Y_t = \alpha w_t (1 - a_t)$$

where $\alpha = \frac{\partial A_{t+1}^{*0}}{\partial Y_t^0}$ is the marginal propensity to save out of current period income when not working.

I then simplify the model further by taking a second order Taylor expansion of equation (2). (Note that a second order expansion is needed, rather than first order, to account for consumption smoothing.) This approximation leads to an expression for the probability of labor force participation in each period:

$$P_t = E[p_t] = \phi[\Delta V^*]$$

and gives the intertemporal elasticity of substitution ϵ_I with respect to the next-of-tax rate (for an anticipated temporary tax change) as

$$\begin{aligned}\epsilon_I &= \frac{\partial P_t}{\partial(1-a_t)} \cdot \frac{1-a_t}{P_t} \\ &= \frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \lambda^0 w_t (1-a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1-a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)\end{aligned}$$

where the last term (depending on α) reflects the degree of desired consumption smoothing as a function of the marginal propensity to save and return on assets.

The steady-state elasticity ϵ_S (for an anticipated permanent tax change) will involve no consumption smoothing motive, so $\alpha = 0$ in this case. Using this fact, and recognizing that many of the terms can be rewritten in terms of empirically measurable parameters (which can be taken as constant over a small tax change), I compute the relationship between the two elasticities as

$$\epsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t} \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \epsilon_S \quad (3)$$

This gives an expression for the ratio of the two elasticities that depends on the coefficient of relative risk aversion γ , the marginal propensity to save α , the interest rate on assets r_t , the savings rate s_t , and the percentage change in post-tax income when working W_t . I then use these results to compare my elasticity estimates to steady-state values from other studies in Section 5.4.

Note that for plausible values of the parameters (as shown in Figure B.1), the value of the multiplier term in equation (3) is positive, meaning the intertemporal substitution elasticity will be larger than the steady-state elasticity (consistent with the intensive margin case discussed above).

3.4 Summary

Overall, Figure 4 indicates that the CTC could increase intensive margin labor supply relative to the EITC, depending on the relative magnitude of the income and substitution effects for each worker. Importantly, prior research on the EITC has suggested substitution effects of tax changes strongly outweigh income effects in magnitude (Nichols and Rothstein 2016); this would suggest the CTC’s labor supply effects are positive on net for low-wage workers. As the model in Section 3.2 further shows, the extensive margin labor supply effects of the CTC are unambiguously positive for primary earners, as the CTC always reduces average tax rates and thus lowers reservation wages. Secondary earners should see little effect. Lastly, the model in Section 3.3 shows that the effects I find should be larger than those found in prior studies, reflecting the short-term change in incentives identified in this study that leads to intertemporal substitution.

4 Method

4.1 Data

I use the 1984 to 2014 panels of the Survey of Income and Program Participation, focusing on the 2001 to 2016 period where the refundable CTC was in effect, but before the TCJA changed the CTC’s structure.¹⁵ The SIPP is ideal for my purposes because it provides a large nationally representative panel of US households, is available both before and after the CTC was enacted, and provides information on month of birth for children, allowing construction of the running variable in an RD design. While the data collected by the SIPP has changed over the years (particularly for the 1996 and 2014 panels), it is broadly consistent for most key variables.

After importing the SIPP data (using Nichols 2008), I collapse all SIPP monthly char-

¹⁵Because of the SIPP’s change to use overlapping samples in the 2018-2020 panels, I have not yet analyzed later years.

acteristics to an annual level, using December characteristics for demographic information and rolling up income across months of the year.¹⁶ I follow Looney and Singhal (2006) in imputing incomes for missing months based on the average for the rest of the year; this allows me to approximate characteristics of households as they are treated by the tax system (which uses calendar year incomes and determines marital status and age cutoffs based on December values). I generate information on tax liabilities and the amount of CTC received by each tax unit using the NBER Taxsim calculator (Feenberg and Coutts 1993), subject to several assumptions detailed in Appendix A. My main income measure is post-tax income, which includes labor income, non-labor income, taxes, and cash transfers, but I also use Adjusted Gross Income from the tax code (AGI, which excludes most cash transfers and taxes) to identify households most subsidized by the credit. I standardize ages within panels, using household and person IDs to compare ages for each record, and take the earliest reported (non-imputed) modal age where values differ.

I define the broad analysis sample as all children who are dependents of a tax unit and have ages within 6 months of turning age 14 to 17 at the end of the year. I also require at least one of the children's parents to be observed in the SIPP for 8 or more months in both the current and prior year (to ensure tax liabilities and labor supply reflect full year values).¹⁷

My primary sample is a low-income subset of the overall SIPP. While results in the broader population provide some information about labor supply responses, most households with children have incomes that place them beyond the phase-in region of the CTC, where labor supply effects should theoretically be strongest. Thus, to focus on families who are likely making extensive margin decisions, I exploit the panel nature of the data and focus on the subsample of children with prior year parental AGI of \$0 to \$20,000 in 2016 dollars (roughly the bottom 25% of all households with age 14-17 children). While current year

¹⁶In the case of attrition before December, I use characteristics from the last observed month in the year.

¹⁷I exclude two other small groups: children whose parents had no tax dependents (due to inconsistencies in the SIPP data) or had invalid ages (negative or only imputed values).

income is endogenous to CTC eligibility, the use of prior year income helps isolate low income households using a predetermined characteristic. This subsample thus consists of lower income families who face the greatest incentive to work due to the CTC, and functions similarly to the restrictions to single parents or workers with low education in studies of the EITC's effects (Meyer and Rosenbaum 2001; Grogger 2003; Eissa and Hoynes 2004; Kleven 2021).

I focus on two primary outcomes. The first is whether either of a child's parents are employed during the calendar year, as measured by an indicator for positive labor income (wages or self-employment, which are the income sources subsidized by the CTC). This measure is comparable to previous studies of the effects of tax credits on labor supply (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Eissa and Hoynes 2004), which typically use indicators for any work last year. However, while such employment measures are often referred to as "labor force participation" (LFP) in the existing literature, a more comprehensive definition of participation would include unemployed parents, who are willing to supply labor (as measured by looking for work) but have not found jobs. To the extent that unemployment reflects time for a search and matching process to take place, the CTC should subsidize both work and efforts to find work (as it raises the expected value of working). I thus also examine whether children's parents are ever in the labor force (employed or unemployed) during the year.

The unit of analysis for this study is a child, as each child's age determines their parents' eligibility for that child's CTC subsidy. A child's parents' tax unit can be composed of either two individuals (if parents are married) or one individual (if the child lives with only one parent or guardian, or if the parents are unmarried). My measures of outcomes examine behavior for the "first mover" in each tax unit (i.e., the presence of any employment or participation), following the analysis of Figure 5b, which suggested secondary earners will typically not face greater incentives to work due to the CTC.

Table 1 presents descriptive statistics for all households with children in the studied age

Table 1: Descriptive Statistics for All Households and Primary Sample

	All Households	Primary Sample
Share of tax units with earnings	0.91	0.63
Share of tax units in labor force	0.93	0.72
Either parent is non-white	0.43	0.61
Parents are married	0.66	0.27
<i>Highest Parent Education</i>		
< High School	0.11	0.27
HS Grad	0.19	0.29
Some College	0.36	0.36
College Grad	0.20	0.06
Advanced Degree	0.14	0.02
Post-tax income (2016\$)	58,286.64 (50,780.30) [-193,947.39—797,144.44]	19,629.09 (17,573.99) [-77,207.22—258,318.23]
Amount of CTC received (2016\$)	1,401.55 (1,321.77) [0.00—11,426.12]	652.44 (964.07) [0.00—9,114.30]
Child Age as of end of tax year	15.47 (1.16) [13.50—17.42]	15.46 (1.15) [13.50—17.42]
Parent Age (max)	45.65 (7.67) [16.00—88.00]	44.27 (9.52) [16.00—88.00]
Tax unit number of dependents	2.45 (1.24) [1.00—14.00]	2.55 (1.35) [1.00—11.00]
Observations	41619	9443

Notes: Values are means, with standard deviations in parentheses and minimum/maximum in brackets. Statistics are presented for all children in the 2001 to 2014 SIPP panels who are age 13.5 to 17.5 at the end of the year, live with their parents, whose parents have valid (non-imputed) ages and dependents for federal taxes, have non-imputed birth months, and have at least one parent observed for 8+ months in the current and prior year. Primary sample is further restricted to children whose parents have AGI in the prior year below \$20,000 in 2016 dollars. Due to the need for full-year and lagged data, excluded years are 2001, 2004, and 2008.

range of 13.5 - 17.5 years (as detailed below), and those in the primary sample of lower-income households (with average income of about \$19,600 in 2016 dollars). As the table shows, the primary sample reflects a disadvantaged group of parents; 73% are single parents, 27% have less than a high school degree (and only 8% have a college degree), and 61% are racial minorities.

4.2 Identification Strategy

To investigate the effects of the Child Tax Credit on labor supply, I exploit the age cutoff for the credit. A convenient feature of the CTC for research purposes is that it provides a credit for each child in the tax unit aged 16 or below; thus, a child who turns 17 on January 1st of a year will entitle his or her parents to a credit for the previous year, but an otherwise identical child born one day earlier on December 31st would not provide a credit for that year. Thus, performing a regression discontinuity using dates of birth for families with children near age 17 in the previous year can potentially provide internally valid estimates of the effects of the CTC on parental employment for the population of families with older children.¹⁸

Using the age 17 cutoff is valuable for several reasons. The cutoff is unrelated to all the major tax and transfer benefits available to families with children, which sunset when a child turns 18 or 19. In particular, the dependent exemption and EITC end at age 19 (or age 24 if the child is a full-time student); the child care credit and child care subsidies end at age 13; and children receive special treatment under SSI and SNAP until age 18. TANF rules change at age 16, 18 or 19 depending on the state and child's student status, Medicaid and CHIP end at age 19, and WIC benefits end at age 5. (All parameters taken from Urban Institute (2019)). Additionally, other major changes in children's living situations (such as leaving home or graduating high school) tend to change at ages other than 17. This implies that changes at age 17 are likely to reflect the effect of the CTC rather than the loss of other programs or lifecycle factors.

Caveats to Identification: Birth Timing and Seasonality

A natural concern about the use of an age cutoff is the possibility of parents manipulating birth timing to gain eligibility for the CTC. Prior research has established that some parents do alter the timing of their births around end of year or other date cutoffs to gain earlier

¹⁸As noted above, this source of variation has been used previously by Feldman, Katuščák, and Kawano (2016).

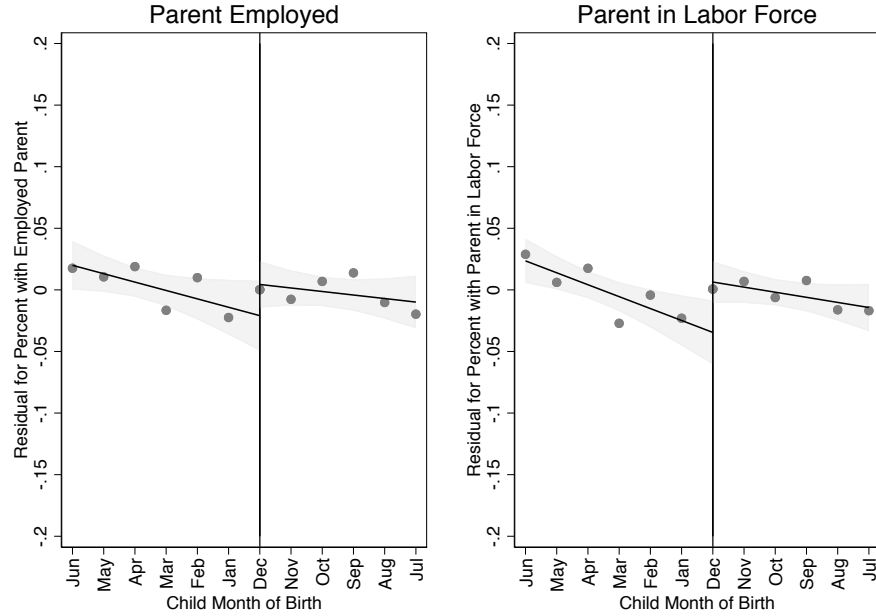
eligibility for tax credits (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Schulkind and Shapiro 2014). If parents are able to change when their children are born using elective Cesarean sections or induced labor, this would imply that parents can manipulate their eligibility for the CTC, meaning that births would nonrandomly fall in December or January and thus potentially bias estimates of the CTC's effect.

These concerns may be well founded. A recent study by LaLumia, Sallee, and Turner (2015) using all income tax returns in the US suggests that birth timing effects are present (although small in magnitude); they estimate that \$1,000 of child tax benefits (including the CTC) are associated with a 1 percentage point shift of births from early January to late December. Similarly, Schulkind and Shapiro (2014, table 1) find that mothers giving birth in December and January differ in several demographic characteristics (although by less than 1 percentage point). While the bias from birth timing appears relatively small, it will be addressed explicitly in this analysis, as discussed below.

In addition to the concern of manipulation of birth timing, the RD results could be influenced by seasonal trends in birth outcomes. A long literature has noted that children born in the winter tend to have worse later economic outcomes than children born in the summer (Buckles and Hungerman 2013; Schulkind and Shapiro 2014; Bound and Jaeger 1996). In particular, Buckles and Hungerman (2013) find that while children born to parents who were not planning to have children display no seasonal patterns in parental characteristics, parents who wanted to have children disproportionately give birth in the spring and summer. To the extent that this makes parents not demographically similar across the December / January cutoff, the RD results will be biased.

These seasonal effects could be compounded in my data because the SIPP only includes date of birth at the monthly level, hiding daily variation that could show more smoothness across the cutoff. Furthermore, for most of my sample period, December children receive one fewer years of exposure to the CTC than January children (since the credit was introduced after their birth, and the December children age out sooner). This means the two groups

Figure 6: Preexisting Discontinuities in Year *before* Loss of CTC Eligibility



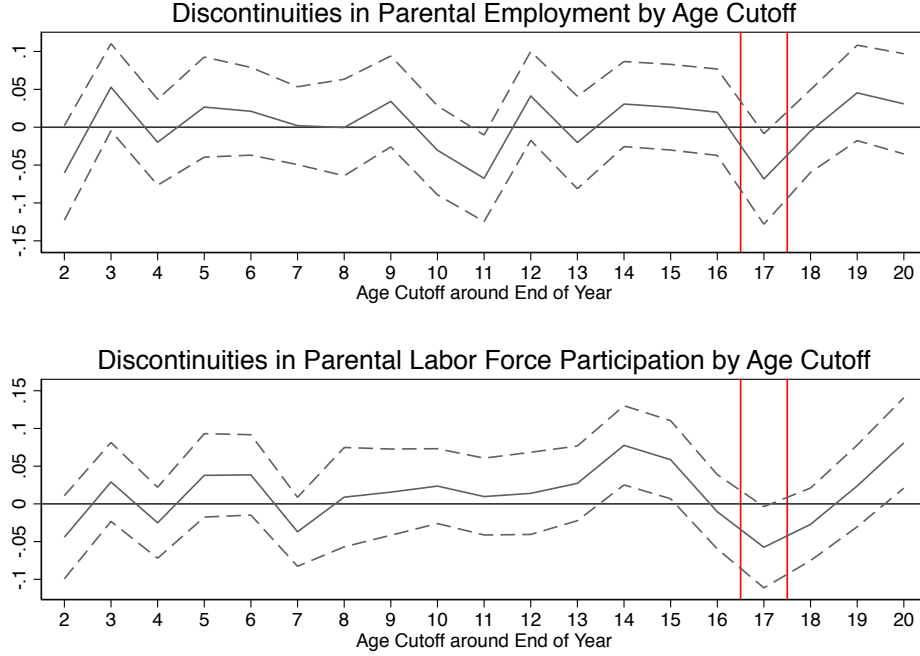
Notes: This figure presents regression discontinuity estimates among children aged 13.5 to 16.5 (i.e., around the age 14 to age 16 end-of-year cutoffs). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

differ in terms of lifetime wealth.

Some evidence for these biases is apparent in Figure 6, which displays an RD design for the jump at December for the dependent variables of interest (parental extensive margin labor supply and labor force participation) among children in the primary sample for the age ranges *before* the CTC cutoff occurs, around the age cutoffs from 14 to 16. In general, low-income parents of children born in December are more likely to be employed or in the labor force than parents of children born in January, even when both are eligible for the CTC. (The estimate for employment is not statistically significant, but the point estimate is positive).

This seasonal pattern is fairly consistent over time. Figure 7 illustrates how this seasonality influences estimated discontinuities around various potential age cutoffs (e.g., an age cutoff at age 16 compares children turning 16 in December to those turning 16 in January) within a window of 6 months on either side of the cutoff. While the results are imprecise

Figure 7: Discontinuities in Labor Supply at Multiple Age Cutoffs



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Dashed lines are 90 percent confidence intervals. Age 1 is excluded due to imprecision. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text). Results without controls are presented in Figure C.6.

due to low sample sizes for each individual age, and any given discontinuity could reflect either seasonal factors or responses to policies at other age cutoffs, the point estimates of discontinuities are more often positive for children aged 2 to 16 (especially for labor force participation). This suggests the seasonal variation in outcomes is persistent. The drop in the discontinuities at age 17, where parents lose CTC eligibility, represents a break from the trend.

To address this consistent difference in characteristics, I make use of a difference-in-regression discontinuities (DiRD) design (Grembi, Nannicini, and Troiano 2016). The motivation for this design is that while there may be seasonal differences in demographics (observed and unobserved) between parents of children born in different months, those seasonal difference should be constant over time. The estimated discontinuity for children at ages before 17 can thus identify the seasonal and birth timing effects, which are then dif-

ferenced out from the CTC’s effect at age 17. Under the assumptions that the seasonal differences are constant and that the effect of the CTC does not itself depend on seasonality (that is, parents with children born in different months respond similarly to the incentives, as discussed by Grembi, Nannicini, and Troiano (2016)), the DiRD design will identify the causal effect of the loss of the CTC at age 17 on labor supply.

One potential concern about the age 17 cutoff is that some states set their minimum school-leaving ages at 17, meaning factors besides the CTC could be changing at that age. Such a response would require parents to change their labor supply in advance of their children actually leaving school (as their child would age out of school in December of the tax year, but labor supply is measured from the previous January to December), which seems implausible, but I investigate this factor explicitly in robustness checks.

4.3 Empirical Model

My preferred reduced form specification is given in equation (4), which represents a DiRD design.

$$y_{it} = \alpha + \delta D_{it} + \phi_1 A_{it} D_{it} + \phi_2 A_{it} (1 - D_{it}) + (\gamma + \beta D_{it} + \phi_3 A_{it} D_{it} + \phi_4 A_{it} (1 - D_{it})) T_{it} + \theta X_{it} + \epsilon_{it} \quad (4)$$

I define y_{it} as the outcome variable of interest (an indicator for employment or labor force participation) for the tax unit of child i for year t ; the model is thus a linear probability model. The running variable A_{it} , capturing age, is the birth month of each child relative to December of year t (normalized so that the December cutoff is zero, November is +1, January is -1, and so forth, until June is -6 and July is +5).

As is standard for a regression discontinuity design, I define D_{it} as an indicator for the discontinuity (equal to one if a child is born in December through July), and include both D_{it}

and interactions of D_{it} with A_{it} to estimate the slopes of the running variable on both sides of the cutoff. This corresponds to a local linear regression with a uniform kernel within my estimation window. The baseline coefficient δ captures the discontinuity at the December-January birth month cutoff for ages prior to the loss of the CTC (estimated by pooling among all the ages before 16.5 included within the regression).

I then incorporate the “difference” aspect of the design by interacting the initial RD variables with T_{it} , an indicator for the child being age 16.5 or above. Thus, the coefficient of interest β indicates the jump in outcomes when the running variable crosses the threshold (i.e., the child turns 17 in December of year t) relative to the jump in outcomes for December births at earlier ages. The interactions with functions of A_{it} allow changes in slopes of the running variable around age 17 as well.

Equation (4) includes additional variables X_{it} beyond a standard RD framework to increase precision. I include the lagged dependent variable $y_{i(t-1)}$ and other covariates X_{it} in some specifications, namely year and state fixed effects, parental race (non-white), education (5 categories), age (of the oldest parent, quadratic), number of dependents, marital status (an indicator for married parents), and indicators for residence in a metropolitan area and the current and lagged number of months observed. I use the SIPP monthly weights for December in my analysis, and follow Nielsen et al. (2009) and Seay and Nielsen (2012) in clustering standard errors for my results by SIPP panel, variance stratum code, and half-sample code to account for the clustered survey design.

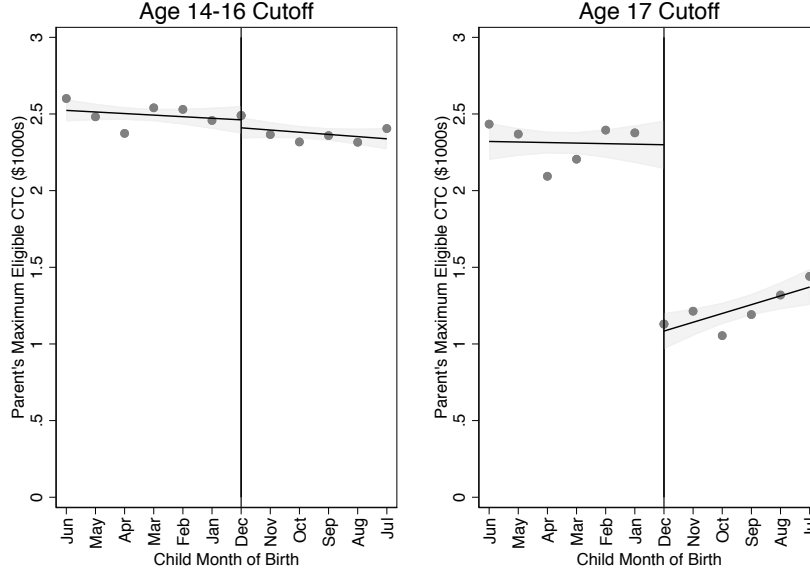
A key element of an RD design is the choice of the estimation window. In the case of this design, there are two windows to consider: the bandwidth for the RD design around each cutoff, and the number of age cutoffs prior to 16.5 to pool when estimating the baseline seasonal discontinuity D_{it} . Because of the cyclical nature of the data, where an age A_{it} value that is far to the right of one December cutoff is inherently located to the left of the next December cutoff, my bandwidth cannot exceed 6 months without having overlapping observations (preventing separate estimation of linear trends). Thus, I choose a 6 month

bandwidth as the default to maximize power (Figure 15 shows the sensitivity of results to other bandwidths). Similarly, I consider multiple windows of ages to include as “pre-treatment” ages, and adopt 13.5 - 16.5 as the preferred period based on the fact that such children are all around high school age and past the age 13 discontinuity in eligibility for a different tax policy, the Child and Dependent Care Tax Credit (Figure 13 shows the estimates are quite stable regardless of the age range chosen).

To directly link the effects of the CTC to the change in outcomes at age 17, I also estimate an intent-to-treat (ITT) estimate modeled after a fuzzy RD design (Lee and Lemieux 2010), comparing the jump in the maximum value of CTC received by parents at the cutoff to the jump in outcomes. To do so, I estimate equation (4), but estimate a first stage coefficient β_c by replacing the dependent variable with C_{it} , the maximum potential value of the CTC received by the parents of child i as of December of year t , with the CTC value in 2016 dollars inflated by the Consumer Price Index. This CTC maximum value depends only on the number of CTC-eligible children in the family, and will thus mechanically fall at the cutoff due to the loss of a CTC eligible child. I then simultaneously estimate the reduced form and first stage using seemingly unrelated regression. The resulting ratio of the two discontinuities, β/β_c , represents an ITT estimate of the change in probability of employment per dollar of CTC eligibility lost at the cutoff. When rescaled by the mean levels of employment and the gain in post-tax income from employment in the sample, this estimate provides an elasticity measure comparable to other studies.

My hypothesis is that the CTC increases parental labor supply; thus, I expect extensive margin labor supply and labor force participation to fall after the loss of the credit at age 17. The effects of gaining the CTC could differ from the effects of losing the CTC; in particular, individuals who are induced to enter the labor force by the credit could find it easier to remain employed even after the credit expires. However, the counterfactual of gaining the CTC cannot be cleanly identified, as only the age 17 cutoff differs between the CTC and other tax and transfer benefits. My results also reflect a temporary (one year) anticipated

Figure 8: First Stage RD Results for CTC Eligibility of Child’s Parents



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

shock to labor supply due to the CTC, as the parents whose children are located to the left of the cutoff will automatically lose CTC eligibility for their children in the following year.

5 Results

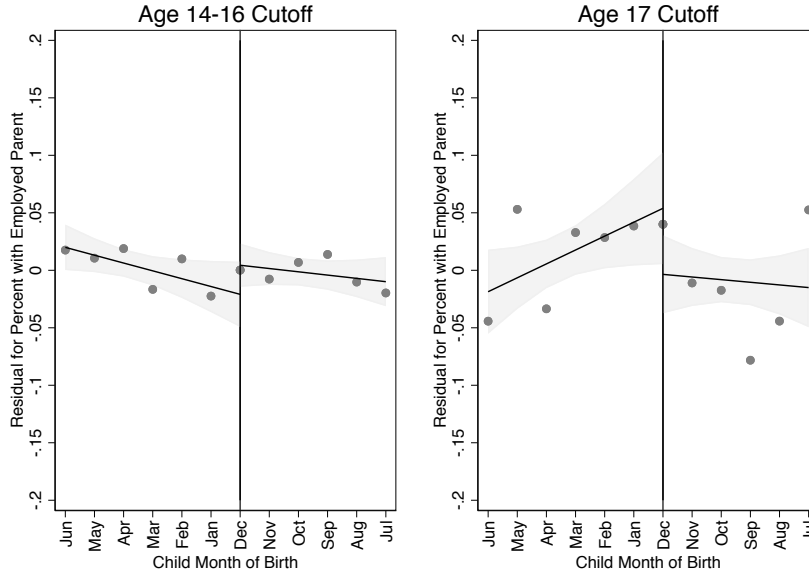
5.1 Graphical Results

I first present reduced form graphical results, illustrating the presence of discontinuities at the age 17 and earlier cutoffs in my estimation window for the time period of interest (2001 and later, when the CTC was refundable). Each point in these graphs represents outcomes for children in a given month of birth (the lowest level of birthdate aggregation available in the SIPP).¹⁹

I first show that the maximum CTC does drop at the cutoff, as expected, in Figure 8. Because Taxsim automatically computes the value of the CTC using the number of under-17 children in each tax unit, the large discontinuity seen here is a mechanical result of the tax

¹⁹Additional figures are provided in Appendix C, Figures C.2 to C.5.

Figure 9: RD Results for Parental Employment



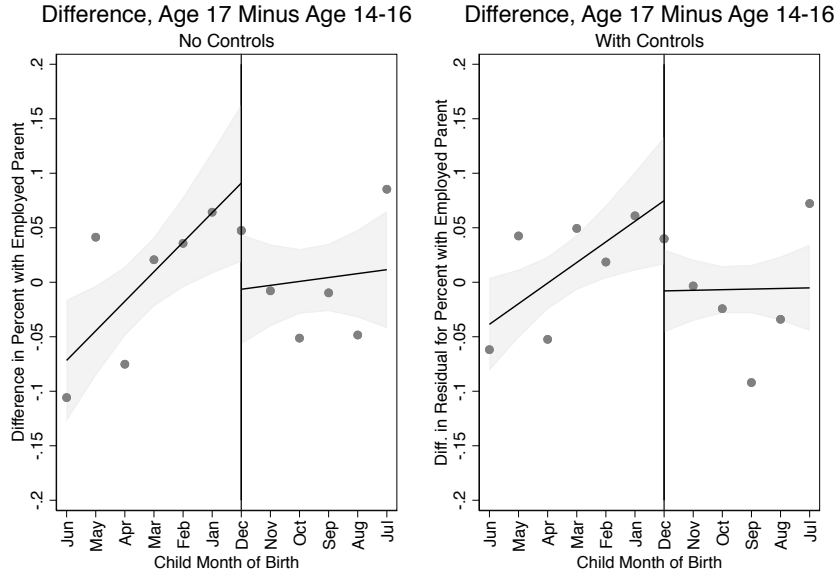
Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

model. However, it is reassuring that Taxsim generates a large drop in the amount of CTC eligibility at the age 17 cutoff, with no discernible discontinuity in the credit received at ages 14 - 16. (Note the value of the credit does not drop to zero at age 17, because many parents with a child turning 17 have other younger children who remain eligible for the credit). Thus, the first stage (indicating the credit is reduced by the cutoff) appears valid.

Turning to the primary reduced form results, Figure 9 shows a standard linear RD design. The data suggest a discontinuity in the expected direction, with a drop in the proportion of children in tax units with working parents at the age 17 cutoff (although the results are imprecise). However, the discontinuity at ages 14 -16 has the opposite sign, suggesting that seasonal factors (leading parents with December children to have higher levels of employment than parents of January children) are partially offsetting the drop in outcomes at age 17.

Next, Figure 10 indicates the effect of accounting for seasonality via the DiRD design. Each point in the figure represents the difference between the results estimated around the age 17 cutoff and the age 14 - 16 pooled cutoff. As the figure shows, the difference in

Figure 10: DiRD Results for Parental Employment



Notes: Each point in the figures represents the difference in mean outcomes by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

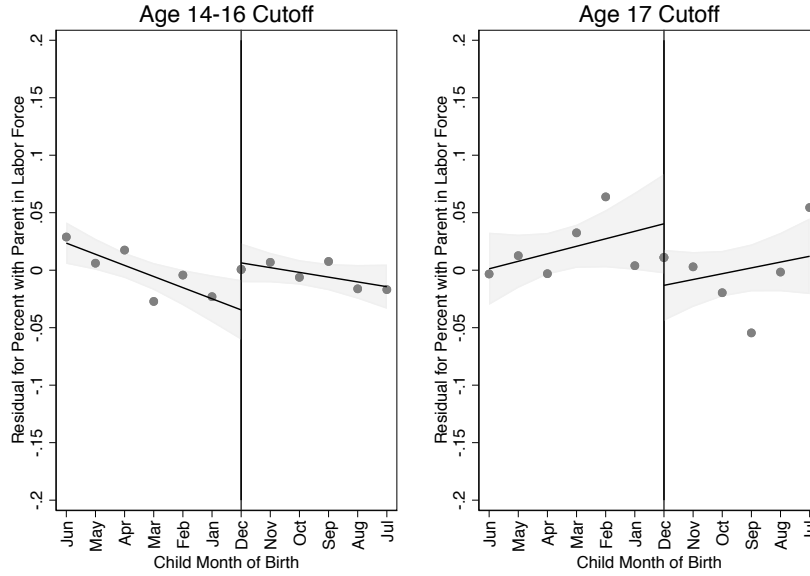
discontinuities at age 17 is higher in magnitude than the age 17 discontinuity alone, and estimating the results with controls reduces the variance with little change to the point estimate. The results thus indicate a significant and negative discontinuity at age 17.

I show similar results for parental labor force participation in Figures 11 and 12. The results are similar in direction and magnitude to the results for parental employment. Here, the preexisting discontinuity at ages 14 - 16 is much larger in magnitude, underscoring the need to use a difference in discontinuities to correct for persistent seasonality.

5.2 Regression Results

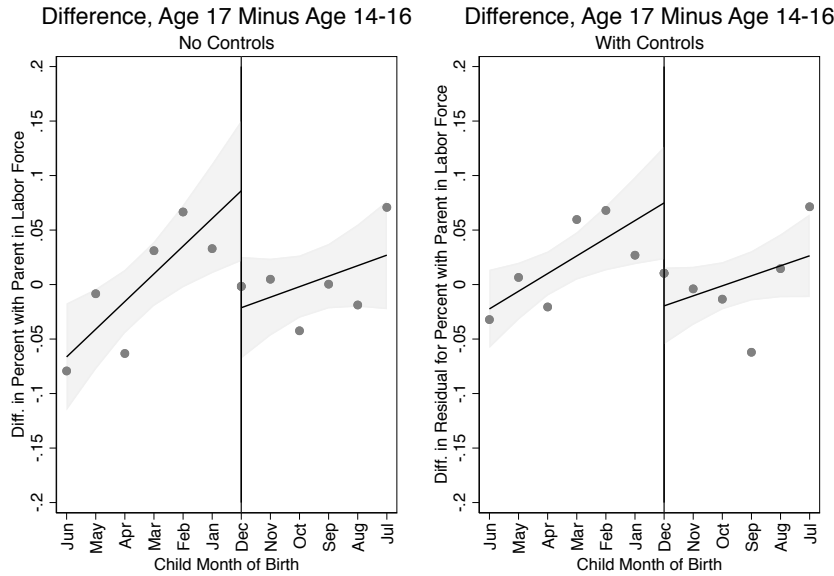
Building on the results illustrated by the figures above, I next present my main DiRD models. Table 2 shows reduced form regression results for a basic DiRD model for parental employment with no controls in column 1, adding the lagged outcome in column 2, and including controls for parental characteristics in column 3 (with a similar structure for labor

Figure 11: RD Results for Parental Labor Force Participation



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 12: DiRD Results for Parental Labor Force Participation



Notes: Each point in the figures represents the difference in mean outcomes by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Table 2: DiRD Results for Primary Sample

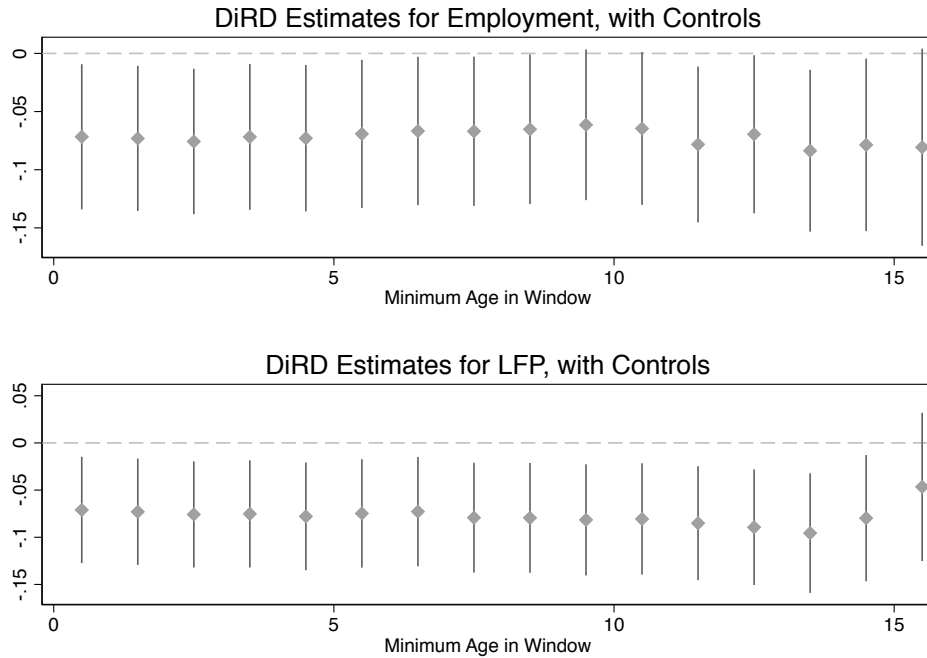
	Parent Employed			Parent in Labor Force		
	(1)	(2)	(3)	(4)	(5)	(6)
Diff in Disc.	-0.097* (0.054)	-0.087* (0.044)	-0.084** (0.042)	-0.107** (0.048)	-0.091** (0.040)	-0.096** (0.039)
Age 16.5+ (Post)	0.091** (0.044)	0.067* (0.038)	0.076** (0.035)	0.086** (0.039)	0.067** (0.034)	0.076** (0.032)
December Disc.	-0.007 (0.036)	0.021 (0.021)	0.026 (0.021)	0.025 (0.034)	0.034* (0.018)	0.042** (0.019)
Lagged DV		Yes	Yes		Yes	Yes
Controls			Yes			Yes
N	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

force results in columns 4-6). The table shows the difference in discontinuities estimator as well as the change in outcomes for children aged 16.5 or older (the “post-policy” period in this context for the DiRD model), and the discontinuity for December in the “pre-policy” ages of 14 - 16. In the simplest specification, the results are large and significant; including lagged outcomes reduces the effect size slightly (reflecting heterogeneity in responses by prior year employment status, as discussed below), but the coefficients remain nearly identical when adding further controls. Column 3, my preferred specification, suggests an 8.4 percentage point fall in the proportion of parents working at the cutoff, relative to the 2.6 percentage point seasonal difference between December and January at earlier ages. This result is significant at the 5 percent level. The labor force results follow the same pattern, with a preferred estimate of a 9.6 percentage point fall in labor force participation at the cutoff (significant at the 5 percent level).

These results are consistent across different age windows used for estimation. Figure

Figure 13: DiRD Results by Age Window Used to Estimate Pre-Treatment Discontinuity



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around the December age cutoff. Each age window extends from the minimum age indicated up to age 16.5. Spikes indicate 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

13 shows the specifications with controls from Table 2, with the pre-treatment December discontinuity estimated in windows from various minimum ages up to age 16.5. Thus, the rightmost value in the figure (15.5) represents the minimum window length, while points further to the left reflect longer windows. As the figure shows, the DiRD estimates are quite stable over windows of various widths. (The estimate does shrink for a very short one year window for labor force participation, but this result is also imprecise).

I also examine the implications of these findings for labor supply in the full population, not limiting to the primary sample of parents with incomes below \$20,000. Table 3 displays the results for the entire sample, as well as by lagged household income categories. I find no effect on employment or labor force participation among households with AGI above \$20,000. It is reassuring that the effect of the CTC on labor supply appears limited to households where the credit should provide the largest change in average taxes (and thus the largest

Table 3: DiRD Results for All Households by Prior Year Income

	Parent Employed				Parent in Labor Force			
	(1) All Hhlds	(2) <\$20k	(3) \$20k- <\$30k	(4) \$30k+	(5) All Hhlds	(6) <\$20k	(7) \$20k- <\$30k	(8) \$30k+
Diff in Disc.	-0.014 (0.011)	-0.084** (0.042)	0.049 (0.032)	0.001 (0.007)	-0.017* (0.010)	-0.096** (0.039)	0.016 (0.023)	0.003 (0.005)
Age 16.5+	0.013 (0.009)	0.076** (0.035)	-0.022 (0.026)	-0.003 (0.005)	0.016** (0.008)	0.076** (0.032)	-0.012 (0.018)	-0.001 (0.004)
Dec. Disc.	0.004 (0.005)	0.026 (0.021)	-0.002 (0.017)	-0.001 (0.003)	0.008* (0.005)	0.042** (0.019)	0.011 (0.012)	-0.002 (0.003)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	41,619	9,443	3,988	28,188	41,619	9,443	3,988	28,188
Clusters	1,141	1,034	897	1,130	1,141	1,034	897	1,130

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units, classified by prior year AGI. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

subsidy encouraging employment).

Next, I test whether results vary by whether households are entering or exiting employment; while theoretically both types of households face the same potential tax schedule, if there are state-dependent costs to finding employment, households with different levels of labor force attachment could respond differently to the CTC. Table 4 thus shows results by the subgroups of households that were not employed or participating in the prior year (the potential entrants), as compared to those that were already employed or participating (the potential exiters). While results are not as precise for these subgroups, the estimates are 50-70% smaller for exiters. This implies that the loss of the CTC operates more strongly by reducing the incentive for non-workers to begin work, rather than leading current workers to stop working (although the response for current workers could be attenuated, as they would need to stop working at the very start of the year to have an employment change in

Table 4: DiRD Results for Households by Entry or Exit Status

	Employed		In Labor Force	
	(1) Entry	(2) Exit	(3) Entry	(4) Exit
Diff in Disc.	-0.108 (0.080)	-0.046 (0.041)	-0.182** (0.090)	-0.060* (0.034)
Age 16.5+ (Post)	0.117* (0.066)	0.037 (0.033)	0.164** (0.070)	0.037 (0.028)
December Disc.	0.026 (0.042)	0.005 (0.022)	0.060 (0.045)	0.022 (0.019)
Controls	Yes	Yes	Yes	Yes
N	3,695	5,748	2,609	6,834
Clusters	810	931	696	971

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

my model). Thus, an important channel of the CTC's effects is to encourage households to move from nonparticipation into job search and employment.

Examining heterogeneity, Table 5 finds similar results across marital status and number of children in a tax unit. This implies the effects of the CTC are fairly consistent across household compositions (conditional on having low prior year income). The point estimates are slightly lower (but noisier) for households with one child as compared to two or more children, which could reflect higher fixed costs in families with more children that leave them closer to the margin of employment.

I run other specifications of my main results in Table 6. As an alternative to my prior year income cutoff, I run results limiting to single parents with education levels of high school or less. The results in columns 2 and 6 are similar to my main results, but are slightly smaller in magnitude and not precisely estimated. I include results in columns 3 and 7 that drop states where children can leave school at age 17 (as this provides a potential alternative

Table 5: DiRD Results for Households by Marital Status and Number of Children

	Parent Employed				Parent in Labor Force			
	(1) Single	(2) Married	(3) 1 Kid	(4) 2+ Kids	(5) Single	(6) Married	(7) 1 Kid	(8) 2+ Kids
Diff in Disc.	-0.095* (0.052)	-0.064 (0.062)	-0.041 (0.072)	-0.101** (0.051)	-0.089* (0.049)	-0.129*** (0.049)	-0.064 (0.066)	-0.098** (0.046)
Age 16.5+	0.101** (0.044)	0.012 (0.050)	0.070 (0.050)	0.075* (0.044)	0.073* (0.040)	0.092** (0.039)	0.079* (0.046)	0.061 (0.039)
Dec. Disc.	0.015 (0.025)	0.032 (0.041)	-0.000 (0.044)	0.031 (0.024)	0.033 (0.024)	0.048 (0.032)	-0.020 (0.038)	0.054** (0.022)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6,862	2,581	2,168	7,275	6,862	2,581	2,168	7,275
Clusters	951	689	716	959	951	689	716	959

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

factor that changes at the age 17 cutoff).²⁰ These results are substantially similar. I also test whether measuring earnings using survey measures reporting employment status by month (as opposed to the presence of positive earned income) changes the results for employment, and find very similar results in column 4. These results thus support my main specification.

5.3 Robustness Checks

For any RD design, it is critical to check the robustness of the results, as there are typically many researcher degrees of freedom (Lee and Lemieux 2010). I first provide a test that the discontinuity in earnings at age 17 is not spurious by examining whether other demographic variables show jumps at the cutoff. As Table 7 shows, there is no significant jump at the cutoff in terms of the education of children’s parents (defined as the proportion with a

²⁰School leaving ages are drawn from the Digest of Education Statistics (various years), <https://nces.ed.gov/programs/digest/>

Table 6: Further Robustness Checks for DiRD Results

	Parent Employed				Parent in Labor Force		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Base	Single Low Educ.	Leave School ≠ 17	LFP Measure	Base	Single Low Educ.	Leave School ≠ 17
Diff in Disc.	-0.084** (0.042)	-0.055 (0.040)	-0.091* (0.047)	-0.075* (0.042)	-0.096** (0.039)	-0.041 (0.035)	-0.107** (0.044)
Age 16.5+	0.076** (0.035)	0.072** (0.031)	0.069* (0.038)	0.057 (0.036)	0.076** (0.032)	0.047* (0.027)	0.062* (0.036)
Dec. Disc.	0.026 (0.021)	0.008 (0.020)	0.034 (0.023)	0.025 (0.020)	0.042** (0.019)	0.008 (0.019)	0.050** (0.021)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	6,420	7,659	9,443	9,443	6,420	7,659
Clusters	1,034	943	963	1,034	1,034	943	963

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

college degree), in racial composition (defined as the percentage with non-white parents), in the proportion with married parents, age of the oldest parent, the number of dependents claimed by parents, or in the value of parental outcomes in the previous year (which provides a check that the discontinuity does not derive from pre-existing differences). To maximize the power of these tests, I also test for differences in a standardized index of all these variables in column 8 (which remains insignificant)²¹ and perform a χ^2 test on joint significance of each coefficient in a seemingly unrelated regression ($p = 0.32$). This implies covariates are smooth at the cutoff; the main results are also robust to controlling for all these factors, as shown in columns 3 and 6 of Table 2.

I also test for discontinuities in children's characteristics. To address the concern of

²¹The index is constructed by standardizing each control variable from columns 1-7, then taking the average of the resulting variables.

Table 7: DiRD Results for Parental Characteristics

	(1) Educ (Coll+)	(2) Race (Non- White)	(3) Married	(4) Age (max)	(5) Num Dep.	(6) Lag Emp.	(7) Lag LFP	(8) Index
Diff in Disc.	-0.041 (0.048)	-0.087 (0.055)	0.042 (0.050)	-0.723 (0.970)	-0.153 (0.152)	-0.016 (0.056)	-0.023 (0.053)	-0.071 (0.132)
Age 16.5+	0.042 (0.044)	0.054 (0.046)	-0.077* (0.042)	2.062** (0.803)	-0.174 (0.130)	0.035 (0.045)	0.028 (0.043)	0.064 (0.123)
Dec. Disc.	-0.016 (0.023)	0.039 (0.038)	-0.024 (0.034)	0.834 (0.692)	-0.034 (0.109)	-0.042 (0.036)	-0.013 (0.033)	-0.137* (0.072)
N	9,443	9,443	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034	1,034	1,034
Mean DV	0.08	0.61	0.27	44.27	2.55	0.61	0.72	0.41
χ^2 p -value								0.32

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Index refers to standardized index of all other columns. χ^2 p -value is for test of first 7 columns being jointly different from zero in seemingly unrelated regression.

Table 8: DiRD Results for Children's Characteristics

	(1) Enrolled in School	(2) Highest Grade Completed	(3) Attrition	(4) Future Months Obs.
Diff in Disc.	0.026 (0.024)	-0.015 (0.110)	-0.004 (0.050)	-0.210 (0.589)
Age 16.5+ (Post)	-0.041** (0.018)	0.805*** (0.091)	0.015 (0.038)	0.220 (0.456)
December Disc.	-0.022 (0.017)	-0.086 (0.089)	-0.008 (0.025)	-0.155 (0.307)
N	4,691	4,686	7,167	7,167
Clusters	938	937	997	997
Mean DV	0.96	9.56	0.13	8.80

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. (Except columns 1 and 2 are estimated in window of 15.5 to 17.5 year old children).

Table 9: DiRD Results for Receipt of Other Income Sources

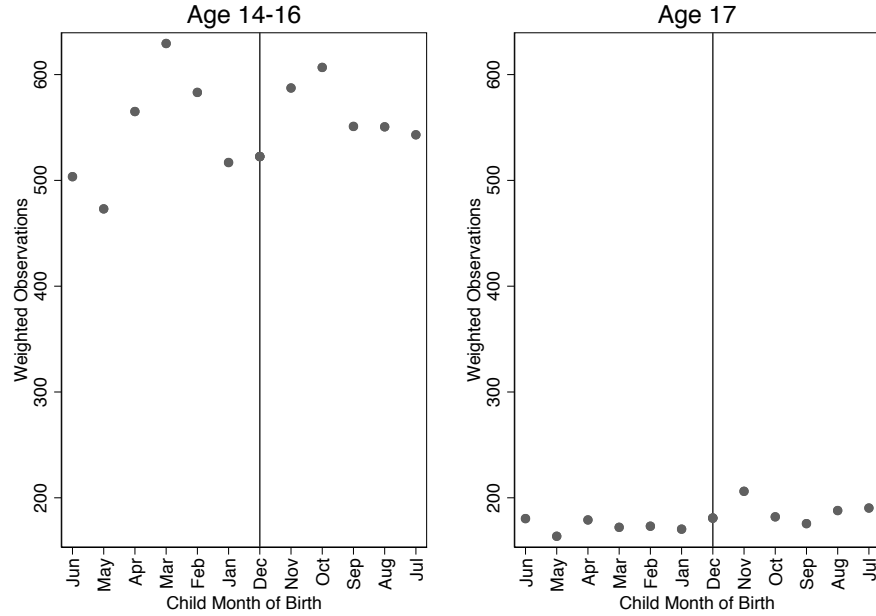
	(1) Dividends	(2) Property	(3) Pensions	(4) Soc. Sec.	(5) Transfers	(6) UI
Diff in Disc.	-0.001 (0.011)	0.005 (0.010)	-0.006 (0.017)	-0.031 (0.029)	0.015 (0.032)	-0.001 (0.022)
Age 16.5+	-0.000 (0.010)	-0.004 (0.009)	0.001 (0.014)	0.005 (0.023)	0.011 (0.023)	-0.012 (0.016)
Dec. Disc.	-0.010 (0.007)	-0.005 (0.005)	-0.000 (0.008)	0.033** (0.014)	-0.018 (0.017)	0.006 (0.011)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

minimum school-leaving ages occurring at 17, I check if children's school enrollment changes at the cutoff. The SIPP data only include educational information for respondents at the age of 15 or above, meaning the sample for these tests is restricted to ages 15.5 to 17.5. However, children just above and below the discontinuity are no more likely to be enrolled in school or in a higher grade (see Table 8), suggesting that the discontinuity in parental earnings does not stem from their children's educational attributes. Columns 3 and 4 show children are no more likely to drop out of the SIPP sample in the next year (or be observed for fewer months) when they are just above the cutoff.

I also test whether other sources of income change at the cutoff. As Table 9 shows, households at the cutoff are no more likely to have income from any source aside from earnings; they also do not have significantly more income in dollars from any source (results not shown). This implies that households do not offset their changes in labor supply by increasing other income sources or relying on transfers, but simply lower their income overall

Figure 14: Distribution of Children in Tax Units by Age



Notes: Each point in the figures represents the weighted sum of observations by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5).

after losing the CTC's subsidy.

Another consideration for an RD design is whether individuals are able to manipulate the value of the running variable to fall on a given side of the cutoff. I present evidence on this test (McCrary 2008) in Figure 14, which displays the monthly weighted distribution of the running variable in my sample. The distribution appears smooth, implying that manipulation of birth timing is not driving the results.

Table 10 displays the sensitivity of the results to different RD estimation methods, including a triangular kernel, a quadratic polynomial, and a simple average across quarters of the year (effectively a difference-in-differences model at a quarterly level). The results are fairly noisy, but all estimates have the same sign and are close in magnitude to the baseline case (except the quadratic specification, which has the largest confidence interval).

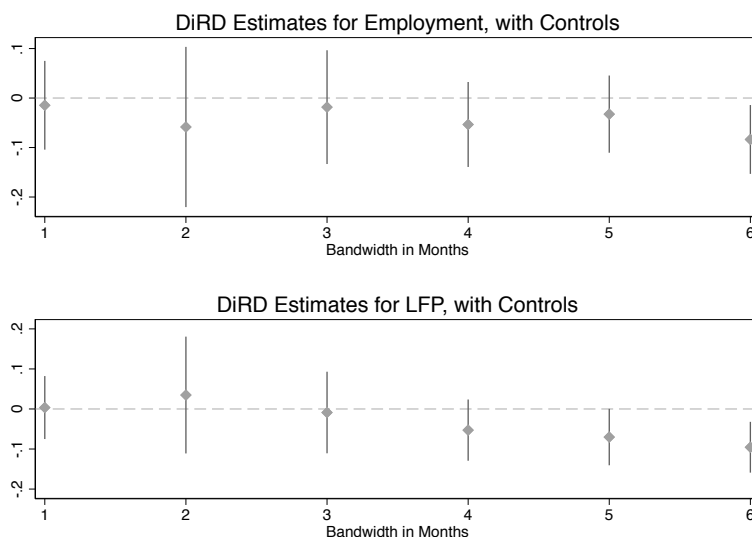
The main results are also consistent when estimated across a range of bandwidths. Figure 15 shows the main DiRD estimates for both outcomes in an age 13.5 to 17.5 window when changing the bandwidth of the local linear regression to different values. The first value

Table 10: DiRD Results by RD Estimation Method

	Employment				LFP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Diff in Disc.	-0.084** (0.042)	-0.049 (0.048)	-0.007 (0.075)	-0.037 (0.026)	-0.096** (0.039)	-0.062 (0.043)	-0.009 (0.066)	-0.050** (0.025)
Age 16.5+ (Post)	0.076** (0.035)	0.073* (0.041)	0.064 (0.071)	0.043** (0.020)	0.076** (0.032)	0.063* (0.037)	0.032 (0.062)	0.052*** (0.018)
December Disc.	0.026 (0.021)	0.022 (0.023)	0.023 (0.034)	0.009 (0.014)	0.042** (0.019)	0.035* (0.021)	0.024 (0.031)	0.018 (0.012)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	9,443	9,443	4,840	9,443	9,443	9,443	4,840
Clusters	1,034	1,034	1,034	899	1,034	1,034	1,034	899
Degree	1	1	2	0	1	1	2	0
Kernel	Uni	Tri	Uni	Uni	Uni	Tri	Uni	Uni
Bandwidth	6	6	6	3	6	6	6	3

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Results are estimated for varying degree local linear regressions with uniform or triangular kernels. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

Figure 15: DiRD Estimates by Bandwidth



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in varying bandwidths centered around the December age cutoff. Each regression is estimated among children in the primary sample, ages 13.5 to 17.5. Spikes indicate 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Table 11: Placebo DiRD Results for Period Before CTC was Refundable

	1984-1999				1990-1999			
	(1) Emp.	(2) Emp.	(3) LFP	(4) LFP	(5) Emp.	(6) Emp.	(7) LFP	(8) LFP
Diff in Disc.	0.074 (0.086)	0.044 (0.065)	-0.011 (0.078)	0.010 (0.063)	0.075 (0.086)	0.044 (0.065)	-0.011 (0.079)	0.010 (0.063)
Age 16.5+	-0.069 (0.066)	-0.092* (0.053)	-0.001 (0.064)	-0.043 (0.053)	-0.069 (0.066)	-0.092* (0.053)	-0.001 (0.064)	-0.043 (0.053)
Dec. Disc.	-0.053 (0.061)	-0.043 (0.031)	-0.005 (0.058)	-0.012 (0.030)	-0.053 (0.061)	-0.043 (0.031)	-0.005 (0.058)	-0.012 (0.030)
Lagged DV		Yes		Yes		Yes		Yes
Controls		Yes		Yes		Yes		Yes
N	7,467	7,466	7,467	7,466	5,830	5,830	5,830	5,830
Clusters	1,396	1,395	1,396	1,395	826	826	826	826

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed). Note that full-year data for 2000 is not included in SIPP data. "Emp." is employment, "LFP" is labor force participation.

(for a bandwidth of 1) is identical to a difference-in-differences estimator, treating December as the treated group and January as the control group. The results are quite noisy, but generally have the same sign (except for bandwidths of less than 2 months for labor force participation). Thus, the use of a 6 month bandwidth to maximize precision does not appear to bias the results.

I also perform a placebo test by examining results from years before the CTC was refundable and provided a subsidy to labor force participation. Using the 1984 to 1996 panels of the SIPP, I extend the analysis in my primary sample to the 1980s and 1990s period, when the CTC did not exist or was effectively not refundable.²² The results, in Table 11 (and Figure C.7), show that estimated discontinuities in this period are insignificant, with

²²As previously noted, while the CTC was refundable from 1998 to 2000 for families with three or more children and payroll taxes exceeding their EITC, these conditions provide a very minimal subsidy in practice.

some opposite signs (and differing signs of the pre-existing seasonal pattern). This implies that some factor changed between the 1990s and 2000s that encouraged parents with age 17 children to leave the labor force; the CTC provides a plausible explanation for this factor.

5.4 Elasticity Estimates

Building on the results in my primary specification, I estimate labor supply elasticities in Table 12. The results are presented in four panels. Panel A provides the previously computed reduced form estimates for the drop in outcomes at the age 17 cutoff, while Panel B shows the corresponding first stage drop in the maximum eligible CTC. The ratio of these two estimates gives an intent-to-treat (ITT) estimate of an 8.0 percentage point increase in employment and 9.1 percentage point increase in labor force participation for every increase of \$1,000 in CTC eligibility for the primary sample.

Next, I rescale the ITT estimates to compute overall elasticities with respect to the change in return to employment caused by the CTC.²³ To compute the return to work (panel D), I calculate the total post-tax income accruing to workers and non-workers (or labor force participants and non-participants) in the primary sample, taking the difference in means for lagged income as representative of the expected income growth for a given member of this population when beginning to work (or enter the labor force). Combined with figures on the mean proportion of parents working (or in the labor force) in each group (panel C), I can generate an elasticity estimate at the average according to the standard formula:

$$\begin{aligned}\varepsilon &= \frac{\Delta y_{it}}{\Delta R_{it}} \cdot \frac{E(R_{it})}{E(y_{it})} \\ &= \frac{\beta}{\beta_c} \cdot \frac{E(R_{it})}{E(y_{it})}\end{aligned}$$

where R_{it} is the return to work (or participation) for a given child's parents at time t , and the change in the CTC ($\Delta C_{it} = \beta_c$) is the only change in returns to work at the cutoff.

²³This measure of income is the relevant one for assessing extensive margin labor supply, as shown in Section 3.2 and Chetty et al. (2013, p. 33).

Table 12: ITT and Elasticity Estimates

	(1) Employed	(2) In Labor Force
(A) Parent Employed / In LF Diff in Disc.	-0.084** (0.042)	-0.096** (0.038)
(B) Maximum Eligible CTC (\$1,000s) Diff in Disc.	-1.049*** (0.065)	-1.048*** (0.065)
(C) Percent Working / LFP (lag) Mean	0.608*** (0.009)	0.723*** (0.009)
(D) Return to Work / LFP (lag, \$1,000s) Mean	7.928*** (0.396)	4.683*** (0.437)
ITT Estimate (= A/B)	0.080* (0.041)	0.091** (0.038)
Elasticity at Average (= $(A/C)/(B/D)$)	1.040* (0.539)	0.591** (0.251)
N	9,443	9,443
Clusters	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Return to work is computed as difference in post-tax income between working and non-working parents (and likewise for LFP). Fuzzy RD and elasticity standard errors are computed using the delta method. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

The resulting elasticity estimate in Table 12 is 1.04 for employment and 0.59 for labor force participation in the primary sample of low-income parents. This reflects an intertemporal substitution response, as the loss of the credit is temporary (relative to the control group of children born in January, who will lose the credit next year) and can be anticipated. Theory thus predicts stronger labor supply responses in this context due to a lack of wealth effects.

The implied employment elasticity for the population of interest is higher than that found in other studies of the intertemporal effects of changes in CTC eligible children or dependents

on the intensive margin (Feldman, Katusčák, and Kawano 2016; Looney and Singhal 2006). This confirms the theoretical prediction that the CTC would have stronger extensive than intensive margin effects.

To derive a steady-state measure comparable to studies on the EITC, I perform a calibration exercise. Using formula (3), we have that

$$\varepsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t} \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \varepsilon_S$$

where the relationship depends on the coefficient of relative risk aversion γ , the marginal propensity to save α , the interest rate on assets r_t , the savings rate s_t , and the percent change in post-tax income when working W_t . To calibrate the model, I set:

- $\gamma = 1$ (following Chetty (2006))
- $\alpha = 0.75$ (following Johnson, Parker, and Souleles (2009), who find $(1 - \alpha) = 0.25$ in a study based on a response to the Child Tax Credit)
- $r_t = 0.073$, $s_t = -0.02$ (using Saez and Zucman (2016), Appendix Tables B30 and B33, with values for the bottom 90% of households averaged for the 2000-09 and 2010-12 periods)
- $W_t = 0.80$ for employment and $W_t = 0.41$ for labor force participation (computed based on the mean changes in post-tax income when working in my sample, computed for the prior year).

We thus have for employment

$$\begin{aligned}\varepsilon_I^{emp} &= \frac{\left(1 - \frac{(1)(0.8)}{1-(-0.02)} \left(1 - \frac{2(0.75)}{1+.05} + \frac{(2+.05)(0.75)^2}{(1+.05)^2}\right)\right)}{\left(1 - \frac{(1)(0.8)}{1-(-0.02)}\right)} \varepsilon_S^{emp} \\ &= 2.42 * \varepsilon_S^{emp} \\ \Rightarrow \varepsilon_S^{emp} &= \frac{\varepsilon_I^{emp}}{2.42} = \frac{1.04}{2.42} = 0.43\end{aligned}$$

In an analogous calculation for labor force participation, I find

$$\begin{aligned}\varepsilon_I^{lfp} &= 1.26 * \varepsilon_S^{lfp} \\ \Rightarrow \varepsilon_S^{lfp} &= \frac{\varepsilon_I^{lfp}}{1.26} = \frac{0.59}{1.26} = 0.47\end{aligned}$$

Thus, my intertemporal substitution elasticities are consistent with steady-state elasticities of 0.43 for employment and 0.47 for labor force participation. (Figure B.1 shows the sensitivity of this calculation to different parameter values). These values are in the range of results found for the EITC from the earlier difference-in-differences studies (Eissa and Liebman 1996; Meyer and Rosenbaum 2001, as corrected by Chetty et al. 2013) as well as studies using other identification strategies (Chetty, Friedman, and Saez 2013).

6 Conclusion

The results presented here suggest that the Child Tax Credit increases labor supply by 8.4 percentage points among low-income parents of older children, and increases labor force participation by 9.6 percentage points. This implies that the original proponents of the refundable CTC were correct that the credit would increase incentives to work (Sawhill and Thomas 2001). These results also imply that although some studies have found labor supply elasticities that are lower in the 2000s than in the 1990s, or cast doubt on the EITC's extensive margin effects, the Child Tax Credit has labor supply effects comparable to the

EITC—with an estimated steady-state employment elasticity of 0.43, very similar to EITC studies (Chetty et al. 2013). Thus, my results confirm that tax credits play an important role in encouraging individuals to enter the labor force.

One limitation of these results is that the power of the SIPP sample is not large, despite using 16 years of data. This limits the range of hypotheses that can be tested in my sample. Thus, a primary topic for future research is to examine whether the effects shown here replicate in other datasets, such as population level tax data. Such cross-validation would help confirm the evidence reported here that the Child Tax Credit encourages labor supply.

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Appendix A Details on Taxsim Calculations

To prepare the SIPP data for use with Taxsim (using the Taxsim27 program, <http://users.nber.org/~taxsim/taxsim27/stata.html>), I made the following assumptions:

1. I assumed all tax units filed as single, head of household, married filing jointly, or as dependent filers (following Taxsim procedures, which assume no units file as married filing separately or qualifying widow[er]s). To construct tax units, I grouped individuals with their spouses (with the person listed first in the SIPP household roster treated as the “primary” filer), treated records with no spouse but with dependents as heads of household, and counted other records as single filers.
2. While the SIPP topical modules include some information on claiming of dependents, I found this data to be poor quality in recent years (with many non-responses). Therefore, I constructed counts of dependents for each household using information available in all years of the panel. Following tax rules, I counted potential dependent children as those who are under age 19 (or under age 24 if a full-time student), unmarried, and living in the same household as their parents or guardians. I also included unmarried parents who have income less than that of their children (but only if their AGI is below the filing threshold for dependents), to account for the fact that many non-child dependents are elderly parents. I only allowed children and parents to be claimed as dependents; other individuals in the household (i.e., “qualifying relatives” for tax purposes) were not captured, as well as children who lived with their parents during the year but not in the last observed month (usually December).²⁴
3. The SIPP data on itemized deductions in topical modules is also limited, so I attempted to use as much information as was available to capture these deductions. My method broadly follows Coey (2010); I computed mortgage interest as the reported amount of

²⁴For consistency over time, I excluded the rule in 2005 and later that allows disabled children of any age to be claimed as dependents.

the mortgage on a primary residence times the reported interest rate, computed real estate taxes as the midpoint of the available bins for property taxes, included state and local income taxes as part of Taxsim's standard methods, and counted medical expenses as a deduction when they exceeded a certain percentage of AGI. Charitable donations and other miscellaneous deductions were not captured.

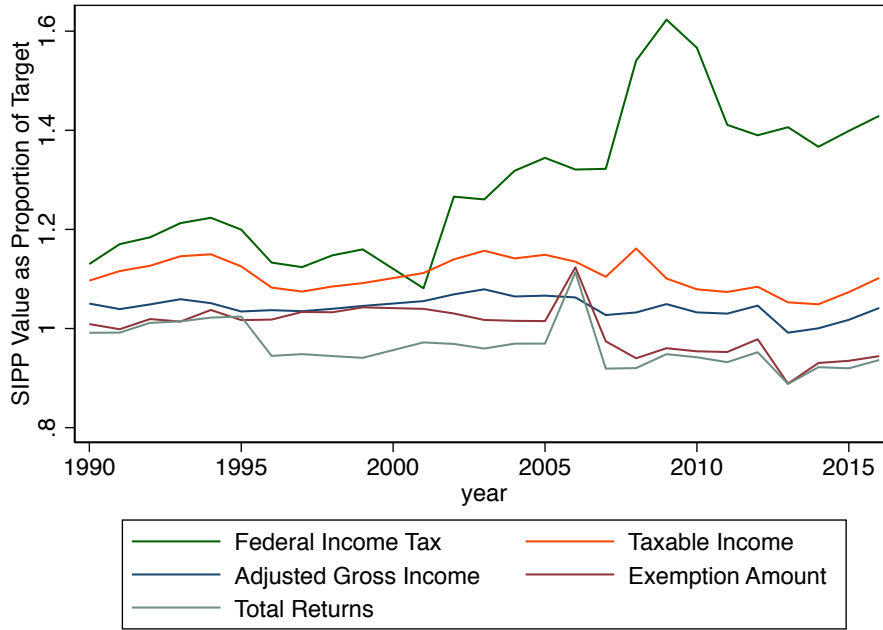
4. The information in the SIPP topical modules (on deductions, capital gains, medical expenses, rent paid, and child care expenses) are only asked in some years of each panel. To impute the missing years, I carried forward values from prior topical modules within each panel, and inflated items using the CPI-U.²⁵

Figure A.1 compares the totals for several variables between the SIPP and the official aggregates from the IRS Statistics of Income for this period, focusing on households with AGI less than \$100,000 in nominal terms (to avoid issues with topcoding of income in the SIPP) who are likely to file returns (having positive earnings, or non-zero calculated taxes). While there are some discrepancies (for example, the number of dependents tends to be too low in the SIPP, and estimated federal income tax tends to be too high), most SIPP totals are within 10% of the administrative value (Figure A.1a). Figure A.1b shows how estimates of the CTC compare to IRS totals; the SIPP consistently finds too large an amount of nonrefundable CTC and too low an amount of refundable CTC, consistent with other datasets such as the Current Population Survey. However, the total amount of CTC captured is about 90% of target.

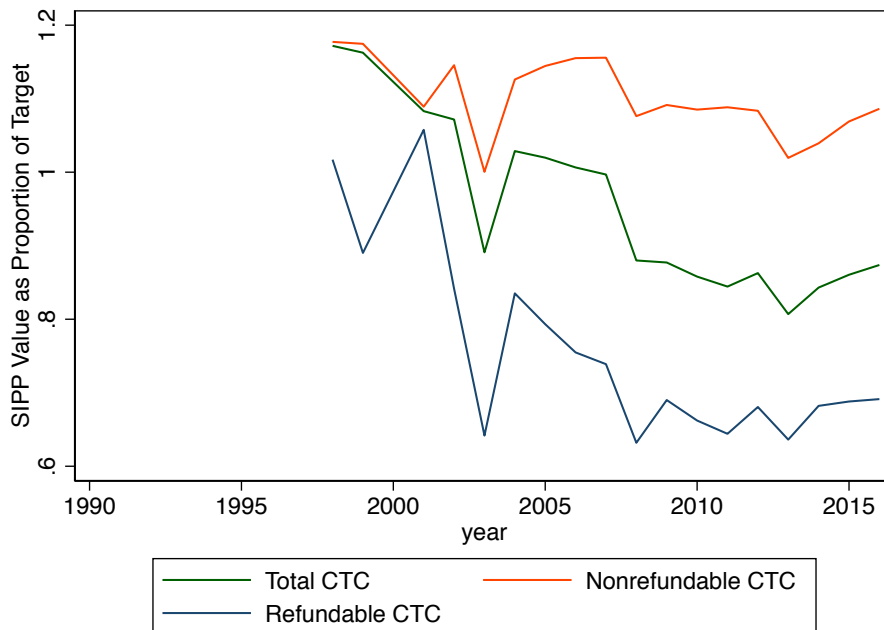
²⁵I did not inflate the value of mortgage interest paid, as this is usually fixed in nominal terms, or of capital gains or property taxes, which are taken as the mean of intervals and thus imprecise.

Figure A.1: Calibration of SIPP Total Tax Values to Administrative Targets

(a) Tax Totals



(b) CTC Totals



Notes: Targets taken from IRS Statistics of Income (various years). Values are for non-dependent tax filers with Adjusted Gross Income under \$100,000. Only households observed in December in the SIPP are included.

Appendix B Mathematical Appendix: Dynamic Model of Discrete Labor Force Participation with Taxes

In this section, I develop a dynamic model to illustrate the differences between steady-state and intertemporal substitution elasticities of labor force participation. This clarifies how the CTC affects intertemporal incentives when it is lost at the age 17 cutoff. The model draws heavily on the fixed work costs framework of Eissa, Kleven, and Kreiner (2006; 2008), but provides a new extension of their results to a dynamic setting (in the vein of Macurdy (1981)).

Consider a continuum of households who live for τ periods indexed by $t = 1, \dots, \tau$ and maximize a concave, continuous utility function $U [C_t]$ over consumption C_t to solve

$$\begin{aligned}
 V [A_t, \theta_t] &= \max_{p_t, A_{t+1}} U [C_t] - Fp_t + \beta E_t [V [A_{t+1}, \theta_{t+1}]] \\
 \text{s.t. } A_{t+1} &= (1 + r_t) (A_t + N_t + (1 - p_t) G_t + w_t p_t - T [w_t, \nu_t] p_t - C_t)
 \end{aligned}$$

where p_t is an indicator for labor force participation, A_t are assets, and β is a discount rate. Wages w_t , non-labor income N_t , and interest rates r_t are given exogenously in each period. Households are identical except for a varying disutility of participating in work F distributed according to a generic CDF $\phi [F]$, drawn once and then held fixed for each household. The parameter $\theta_t = \{w_t, G_t, N_t, \nu_t, r_t\}$ is a vector of the exogenous state variables. Note this model assumes additive intertemporal separability of utility, additive separability of fixed costs, complete capital markets (to borrow and save), rational expectations, and unitary household decision-making.²⁶ I use square brackets to denote arguments of a function, and

²⁶The problem could equivalently be considered as the work decision of the primary earner in each household, which is the margin examined by my empirical analysis.

abstract from intensive margin decisions by assuming indivisible labor, so households in each period can either earn the wage w_t or earn 0.²⁷ Households then choose whether to participate in work and how much they will save for the next period.

Households face a tax-transfer system $T[w_t, N_t, \nu_t]$ if they have earned income w_t , where ν_t indexes parameters of the tax system. T can be negative if households receive net transfers, and I define $G = -T[0, N_t, \nu_t]$ as the value of transfers when not working. I then define

$$a_t = a_t[w_t, G_t, N_t, \nu_t] = \frac{T[w_t, N_t, \nu_t] + G_t}{w_t} \quad (\text{B.1})$$

as the average tax rate faced when going to work (due to taxes paid and transfers forgone).

We can thus simplify the household's problem to

$$\begin{aligned} V[A_t, \theta_t] &= \max_{p_t, A_{t+1}} U[C_t] - Fp_t + \beta E_t[V[A_{t+1}, \theta_{t+1}]] \\ \text{s.t. } A_{t+1} &= (1 + r_t)(A_t + Y_t - C_t) \\ Y_t &= N_t + G_t + p_t w_t (1 - a_t) \end{aligned}$$

for current period (non-asset) income Y_t .

Finally, to simplify notation, define variables with a superscript as those evaluated at the superscript level of p_t . For example, U^1 (or U^0) is utility evaluated at the working (nonworking) state in that period. We then define

$$\Delta x = x^1 - x^0$$

as the difference between variables in the work and non-work states.

²⁷As is standard for dynamic models (Blundell and Macurdy 1999), the intensive margin choice of hours worked conditional on participation involves intratemporal decisions only, and is not affected by the separable fixed cost. Thus, if labor is divisible by hours h_t , we can define an optimal h_t^* that will be chosen conditional on work, which would then carry through all the equations and generate the same result relating the two elasticities of interest.

In each period, available consumption levels with or without work are:

$$C_t^1 = A_t + N_t + G_t - \frac{A_{t+1}^1}{1 + r_t} + w_t(1 - a_t)$$

$$C_t^0 = A_t + N_t + G_t - \frac{A_{t+1}^0}{1 + r_t}$$

(where the desired savings level A_{t+1} may also vary with work status, as work affects available income).

We can first optimize over A_{t+1} , and then over p_t . In each of the two working states, the first order condition for optimization of savings is the standard Euler equation

$$U_c [C_t^1] = (1 + r_t) \beta E_t \left[\frac{\partial V [A_{t+1}^1, \theta_{t+1}]}{\partial A_{t+1}} \right] = \lambda^1 \quad (\text{B.2})$$

$$U_c [C_t^0] = (1 + r_t) \beta E_t \left[\frac{\partial V [A_{t+1}^0, \theta_{t+1}]}{\partial A_{t+1}} \right] = \lambda^0$$

which pins down the optimal asset values in work and non-work, A_{t+1}^{1*} and A_{t+1}^{0*} .

We can approximate optimal assets with a first order Taylor series as follows:

$$A_{t+1}^{1*} [A_t, \theta_t] \approx A_{t+1}^{0*} [A_t, \theta_t] + \alpha \Delta Y$$

$$= A_{t+1}^{0*} + \alpha w_t (1 - a_t) \quad (\text{B.3})$$

$$\Rightarrow \Delta A_{t+1}^* = \alpha w_t (1 - a_t)$$

where $\alpha = \frac{\partial A_{t+1}^{0*}}{\partial Y_t^0}$ is the marginal propensity to save out of current period income when not

working. With this expression, we have

$$\begin{aligned}
\Delta C &= C_t^1 - C_t^0 \\
&= w_t(1 - a_t) - \frac{\Delta A_{t+1}^*}{1 + r_t} \\
&\approx w_t(1 - a_t) - \frac{\alpha w_t(1 - a_t)}{1 + r_t} \\
&= w_t(1 - a_t) \left(1 - \frac{\alpha}{1 + r_t} \right)
\end{aligned} \tag{B.4}$$

Each person then works according to a cutoff condition that arises once assets are chosen optimally by (B.2):

$$\begin{aligned}
V^1 [A_t, \theta_t] &\geq V^0 [A_t, \theta_t] \\
\Rightarrow U^1 - F + \beta E_t [V [A_{t+1}^{1*}, \theta_{t+1}]] &\geq U^0 + \beta E_t [V [A_{t+1}^{0*}, \theta_{t+1}]] \\
\Rightarrow \Delta U + \beta E_t [\Delta V_{t+1}] &\geq F
\end{aligned} \tag{B.5}$$

To clarify equation (B.5), I note that by taking a second order Taylor expansion, we have

$$\begin{aligned}
U [C_t^1] &= U [C_t^0] + U_c^0 \cdot (\Delta C) + \frac{1}{2} U_{cc}^0 \cdot (\Delta C)^2 + R_2 \\
V [A_{t+1}^{1*}, \theta_{t+1}] &= V [A_{t+1}^{0*}, \theta_{t+1}] + V_A^0 \cdot (\Delta A_{t+1}^*) + \frac{1}{2} V_{AA}^0 \cdot (\Delta A_{t+1}^*)^2 + R_2
\end{aligned}$$

(where R_2 represents the third and higher order Taylor series terms). Note that a second order expansion is needed (rather than first order) to account for consumption smoothing in the dynamic model. Call the marginal utility of consumption in the non-work state λ^0 ,

where its derivative is $\lambda_c^0 = U_{cc}^0$. Then we have (by substituting from (B.2), (B.3), and (B.4)):

$$\begin{aligned}\Delta U &\approx U_c^0 \cdot (C_t^1 - C_t^0) + \frac{1}{2} U_{cc}^0 \cdot (C_t^1 - C_t^0)^2 \\ &= \lambda^0 w_t (1 - a_t) \left(1 - \frac{\alpha}{1 + r_t}\right) + \frac{1}{2} \lambda_c^0 \left(w_t (1 - a_t) \left(1 - \frac{\alpha}{1 + r_t}\right)\right)^2\end{aligned}\quad (\text{B.6})$$

and also

$$\begin{aligned}\Delta V_{t+1} &\approx V_A^0 \cdot (\Delta A_{t+1}^*) + \frac{1}{2} V_{AA}^0 \cdot (\Delta A_{t+1}^*)^2 \\ \Rightarrow E_t[\Delta V_{t+1}] &= \frac{\lambda^0}{\beta(1+r_t)} (\Delta A_{t+1}^*) + \frac{\lambda_c^0}{2\beta(1+r_t)} (\Delta A_{t+1}^*)^2 \\ &= \frac{\lambda^0 \alpha w_t (1 - a_t)}{\beta(1+r_t)} + \frac{\lambda_c^0 (\alpha w_t (1 - a_t))^2}{2\beta(1+r_t)}\end{aligned}\quad (\text{B.7})$$

Putting the expressions (B.5), (B.6), and (B.7) together gives

$$\begin{aligned}F &\leq \lambda^0 w_t (1 - a_t) \left(1 - \frac{\alpha}{1 + r_t}\right) + \frac{1}{2} \lambda_c^0 \left(w_t (1 - a_t) \left(1 - \frac{\alpha}{1 + r_t}\right)\right)^2 \\ &\quad \dots + \beta \left(\frac{\lambda^0 \alpha w_t (1 - a_t)}{\beta(1+r_t)} + \frac{\lambda_c^0 (\alpha w_t (1 - a_t))^2}{2\beta(1+r_t)}\right) \\ &= \lambda^0 w_t (1 - a_t) \left(1 - \frac{\alpha}{1 + r_t} + \frac{\alpha}{(1 + r_t)}\right) + \frac{1}{2} \lambda_c^0 w_t^2 (1 - a_t)^2 \left(\left(1 - \frac{\alpha}{1 + r_t}\right)^2 + \frac{\alpha^2}{1 + r_t}\right) \\ &= \lambda^0 w_t (1 - a_t) + \frac{1}{2} \lambda_c^0 w_t^2 (1 - a_t)^2 \left(1 - \frac{2\alpha}{1 + r_t} + \frac{\alpha^2}{(1 + r_t)^2} + \frac{\alpha^2}{1 + r_t}\right) \\ &= \lambda^0 w_t (1 - a_t) + \frac{1}{2} \lambda_c^0 w_t^2 (1 - a_t)^2 \left(1 - \frac{2\alpha}{1 + r_t} + \frac{(2 + r_t) \alpha^2}{(1 + r_t)^2}\right) \\ &= \Delta V^*\end{aligned}$$

Considering this equation intuitively, the term depending on α reflects the household's desire to smooth consumption in the dynamic model. To the extent that marginal utility is decreasing, the household is better off moving assets from a current higher wage period (where the household works) to a future period.

In each period, the probability of participation is thus

$$P_t = E[p_t] = \phi[\Delta V^*]$$

We can define

$$\frac{\partial P_t}{\partial(1-a_t)} = \phi'[\Delta V^*] \left(\lambda^0 w_t + \lambda_c^0 w_t^2 (1-a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)$$

and the intertemporal elasticity of substitution with respect to the next-of-tax rate (for an anticipated temporary change) is thus

$$\begin{aligned} \varepsilon_I &= \frac{\partial P_t}{\partial(1-a_t)} \cdot \frac{1-a_t}{P_t} & (B.8) \\ &= \frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \left(\lambda^0 w_t (1-a_t) + \lambda_c^0 w_t^2 (1-a_t)^2 \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right) \\ &= \frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \lambda^0 w_t (1-a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1-a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right) \end{aligned}$$

Now consider the labor force participation elasticity in the steady-state (i.e. in response to an anticipated permanent change in taxes). In this case, a_{t+1} and future values change in the same way as a_t , so marginal utility will increase by the same amount in all periods. This implies there is no need to smooth consumption due to the wage change, or equivalently, $\alpha = 0$. Thus, we have the steady-state elasticity of participation with respect to the average tax rate as

$$\varepsilon_S = \frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \lambda^0 w_t (1-a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1-a_t) \right) \quad (B.9)$$

Comparing equations (B.9) and (B.8) we have

$$\frac{\varepsilon_I}{\epsilon_S} = \frac{\frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \lambda^0 w_t (1 - a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)}{\frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]} \lambda^0 w_t (1 - a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \right)}$$

We can cancel most terms, and get

$$\begin{aligned} \frac{\varepsilon_I}{\epsilon_S} &= \frac{\left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)}{\left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \right)} \\ \Rightarrow \varepsilon_I &= \frac{\left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)}{\left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1 - a_t) \right)} \epsilon_S \end{aligned}$$

We can now rewrite several of these quantities in terms of empirically measurable parameters (which can be taken as constant over a small tax change). Note that $\frac{\lambda_c^0}{\lambda^0}$ is related to the coefficient of relative risk aversion γ and the savings rate $s_t = \frac{A_{t+1}^{0*} - A_t}{Y_t^0}$ as follows:

$$\begin{aligned} \gamma &= \frac{-C_t^0 U_{cc}^0}{U_c^0} = - (A_t + N_t + G_t - A_{t+1}^{0*}) \frac{\lambda_c^0}{\lambda^0} \\ &= - (Y_t^0 + (A_t - A_{t+1}^{0*})) \frac{\lambda_c^0}{\lambda^0} \\ &= - (Y_t^0 - s_t Y_t^0) \frac{\lambda_c^0}{\lambda^0} \\ \Rightarrow \frac{\lambda_c^0}{\lambda^0} &= \frac{-\gamma}{Y_t^0 (1 - s_t)} \end{aligned}$$

We can then define the percentage return to work (i.e. the percent change in post-tax income when entering work)

$$W_t = w_t (1 - a_t) / Y_t^0$$

So we have

$$\varepsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t} \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \epsilon_S \quad (\text{B.10})$$

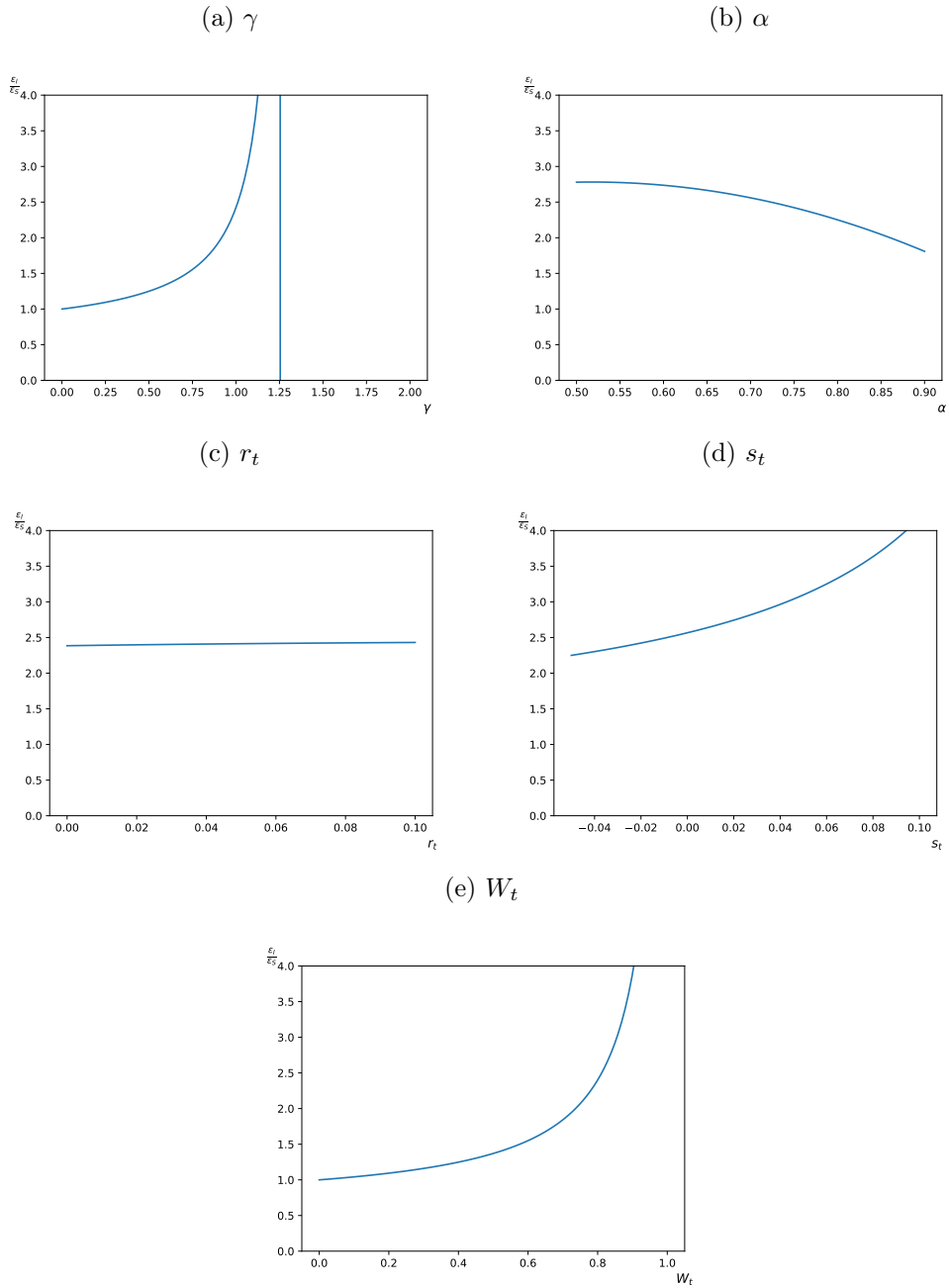
This gives an expression for the ratio of the two elasticities that depends on several parameters:

- The coefficient of relative risk aversion γ
- The marginal propensity to save α (equal to $1 - \mu$, where μ is the marginal propensity to consume)
- The interest rate on assets r_t
- The savings rate s_t
- The percent change in post-tax income when working W_t

I then use formula (B.10) to compare the two elasticities in the context of my study, using the calibration discussed in Section 5.4.

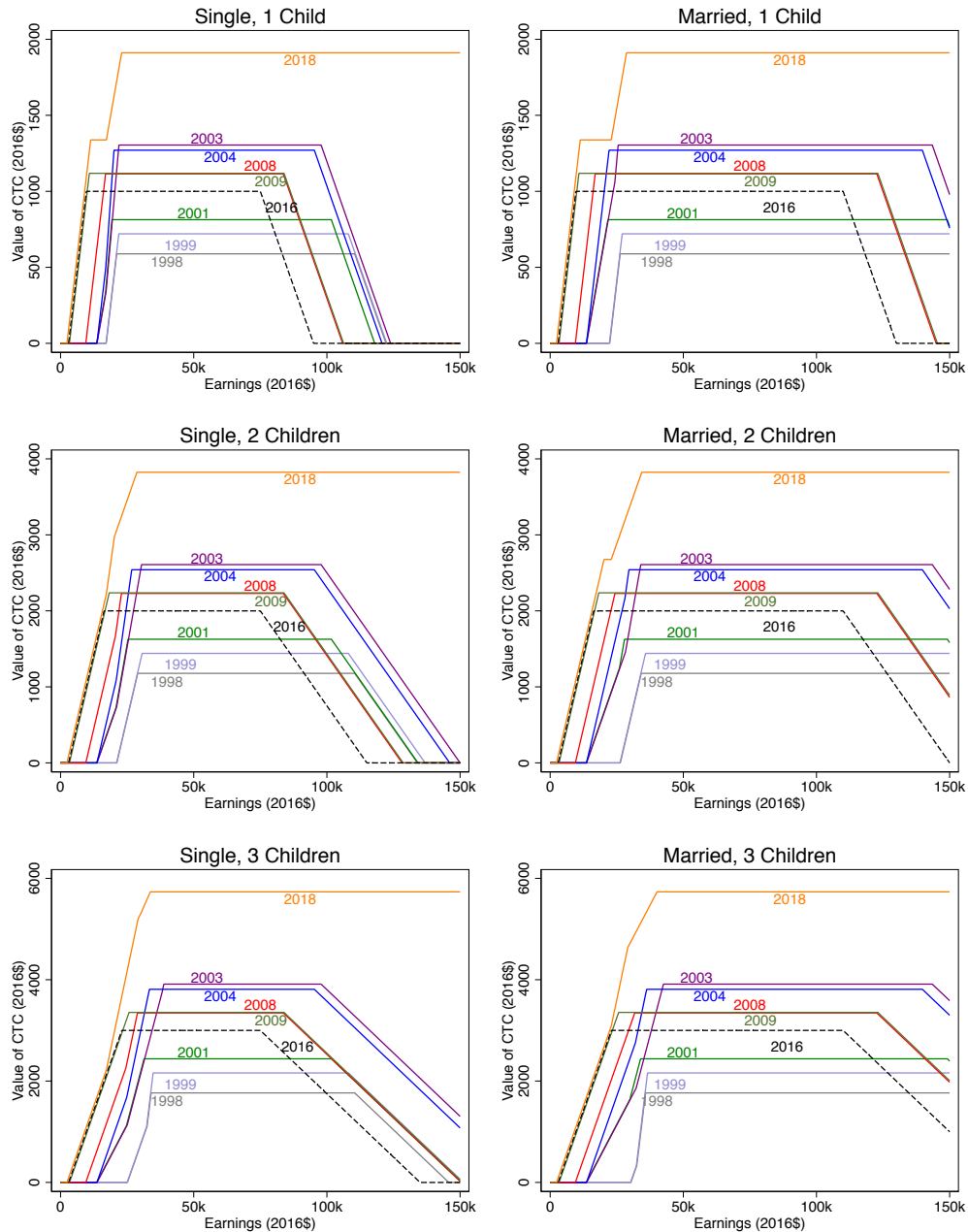
Figure B.1 displays how the ratio between the two elasticities depends on the values of the parameters (holding other parameters constant at their calibrated value). The relationship is quite stable across reasonable values of r_t and α , varies more based on s_t and W_t , and is quite sensitive to γ (with a discontinuity around a value of 1.25).

Figure B.1: Sensitivity of Elasticity Ratio Calculation to Parameter Values



Appendix C Additional Figures and Tables

Figure C.1: Amount of CTC by Family Type, Tax Laws for Various Years



Source: Author's calculations using Taxsim. Calculations assume no unearned income or itemized deductions. Years listed are those where the main CTC parameters were changed by law, as well as 2016.

Table C.1: Parameters of the CTC Over Time

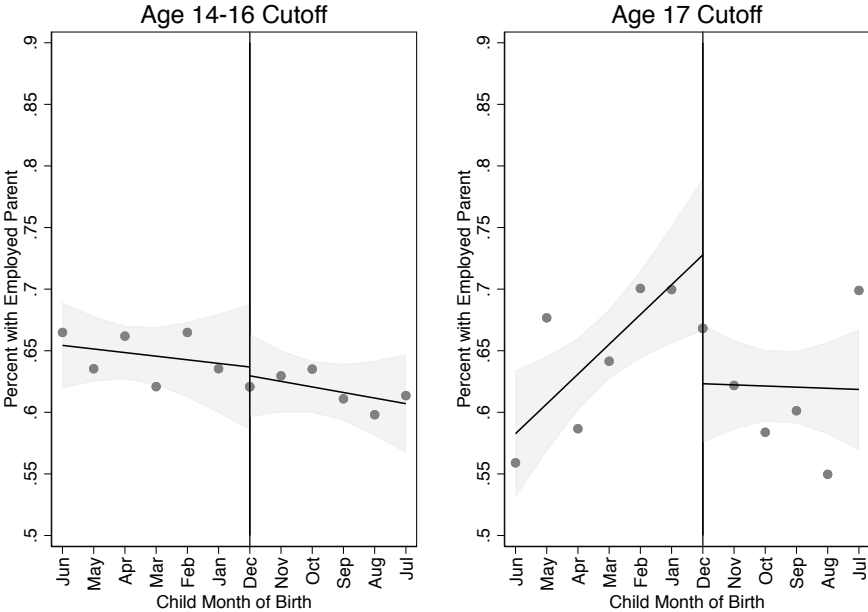
Year	Refundability Phase-in		Max Credit per Child		Phase-Out Start	
	Start	Rate	Refundable	Total	Single/HoH	Joint
1998	N/A	N/A	\$0	\$400	\$75,000	\$110,000
1999	N/A	N/A	\$0	\$500	\$75,000	\$110,000
2000	N/A	N/A	\$0	\$500	\$75,000	\$110,000
2001	\$10,000	10%	\$600	\$600	\$75,000	\$110,000
2002	\$10,350	10%	\$600	\$600	\$75,000	\$110,000
2003	\$10,500	10%	\$1,000	\$1,000	\$75,000	\$110,000
2004	\$10,750	15%	\$1,000	\$1,000	\$75,000	\$110,000
2005	\$11,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2006	\$11,300	15%	\$1,000	\$1,000	\$75,000	\$110,000
2007	\$11,750	15%	\$1,000	\$1,000	\$75,000	\$110,000
2008	\$8,500	15%	\$1,000	\$1,000	\$75,000	\$110,000
2009	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2010	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2011	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2012	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2013	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2014	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2015	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2016	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2017	\$3,000	15%	\$1,000	\$1,000	\$75,000	\$110,000
2018	\$2,500	15%	\$1,400	\$2,000	\$200,000	\$400,000
2019	\$2,500	15%	\$1,400	\$2,000	\$200,000	\$400,000
2020	\$2,500	15%	\$1,400	\$2,000	\$200,000	\$400,000
2021*	\$0	100%	\$3,000	\$3,000	\$200,000	\$400,000
2022	\$2,500	15%	\$1,500	\$2,000	\$200,000	\$400,000

Notes: Refundable credit excludes special calculation of credit for 3+ kids. Refundable phase-in start refers to minimum level of earned income (from wages or self-employment). Phase-out start refers to level of AGI. Phase-out rate is 5% in all years.

* In 2021, many parameters of the CTC changed; the credit was fully refundable, was available until age 18, there was an additional 5% phase-out of the portion over \$2,000 at \$75,000 or \$150,000 of AGI, and the credit was increased by \$600 for children under 6.

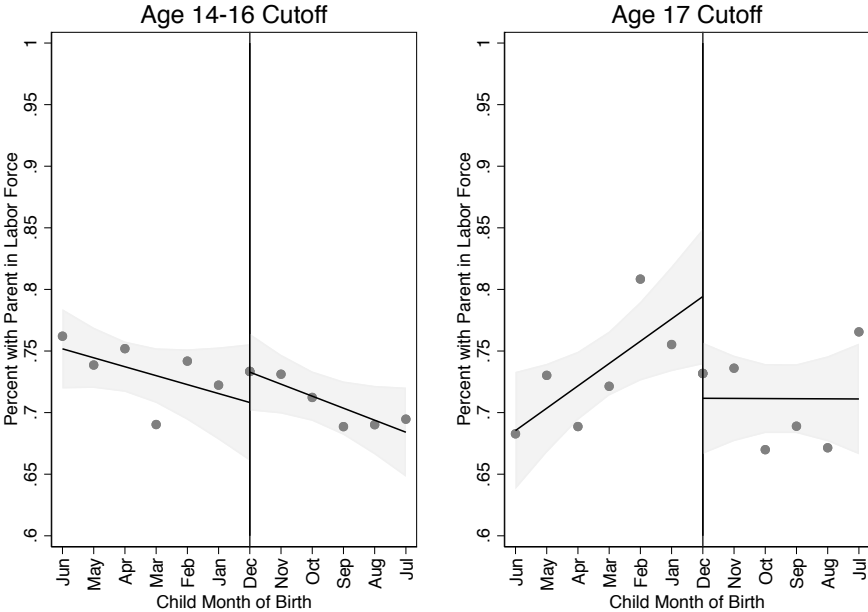
Source: Crandall-Hollick (2018), Urban Institute (2019), and Tax Policy Center (2022). 2022 refundable credit value from IRS Revenue Procedure 2021-45.

Figure C.2: RD Results for Parental Employment, No Controls



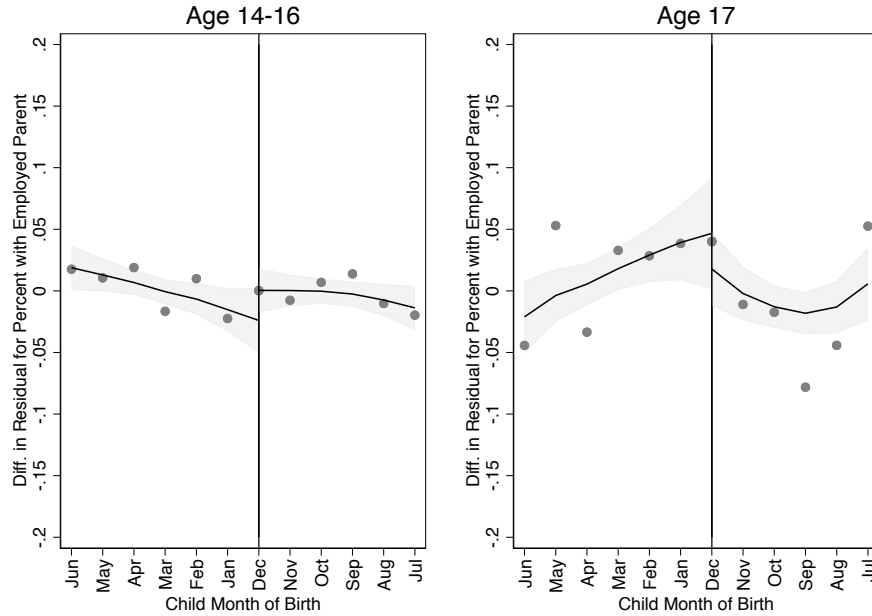
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure C.3: RD Results for Parental Labor Force Participation, No Controls



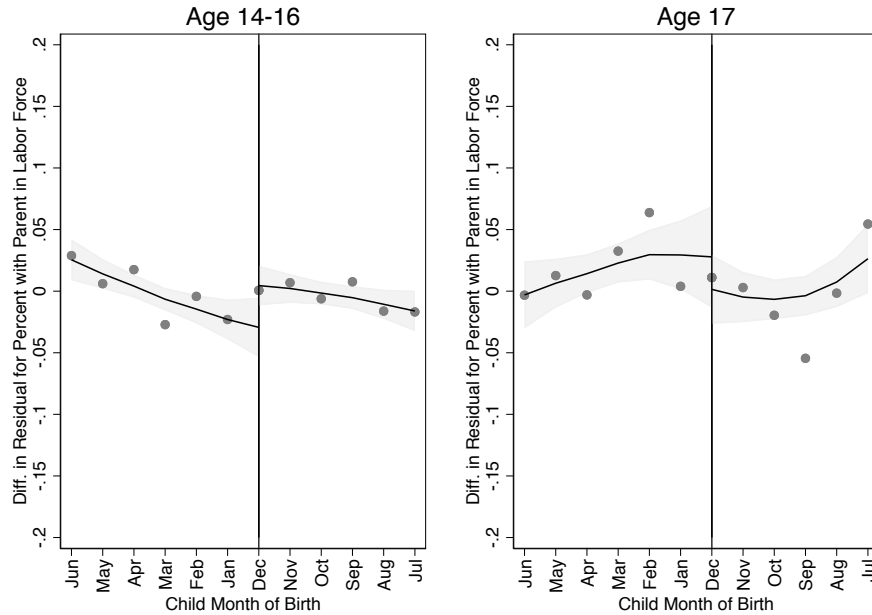
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure C.4: RD Results for Parental Employment, Local Polynomial Fit



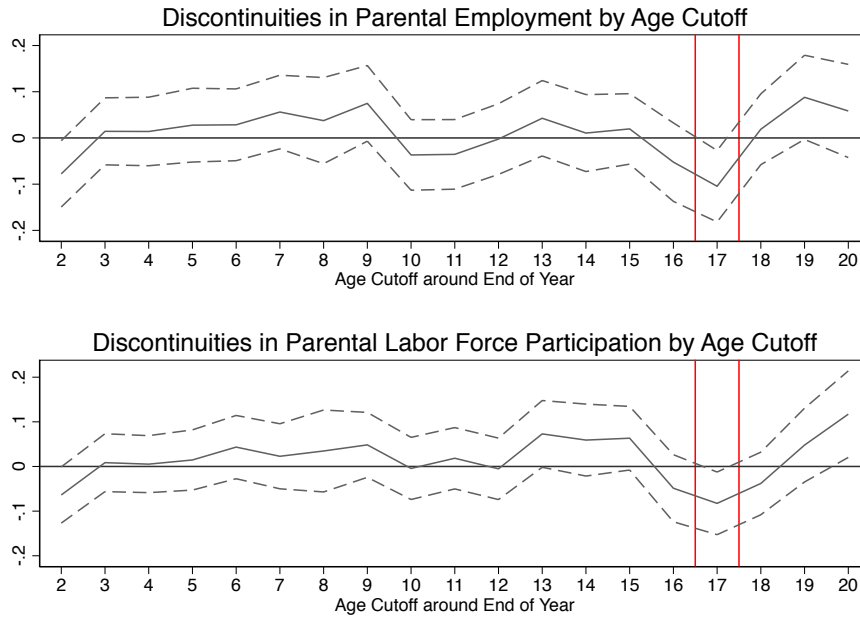
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure C.5: RD Results for Parental Labor Force Participation, Local Polynomial Fit



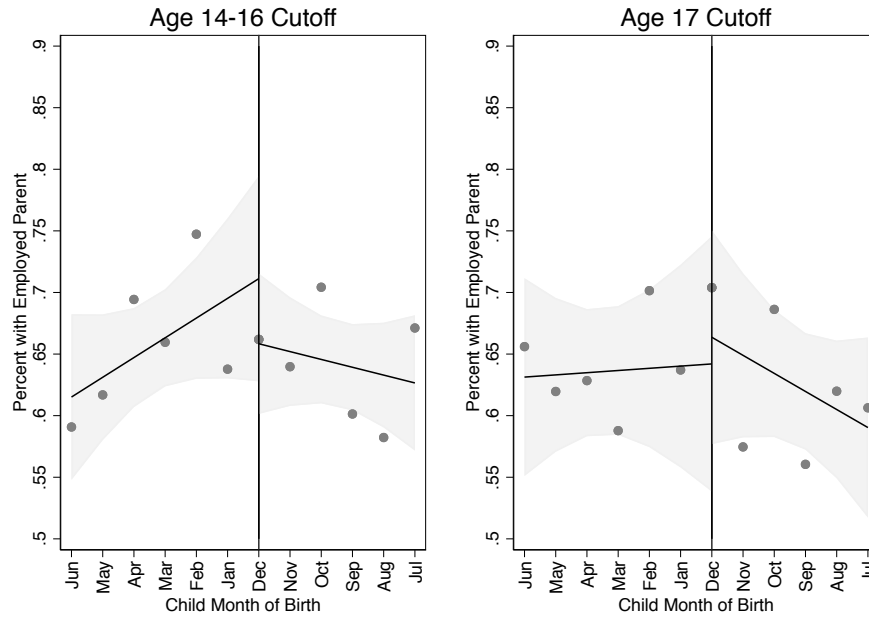
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure C.6: Discontinuities by Age Cutoff, No Controls



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Dashed lines are 90 percent confidence intervals. Age 1 is excluded due to imprecision.

Figure C.7: Placebo: RD Results for Parental Employment, Period Before CTC was Refundable



Notes: Results are for the 1984-2000 period, before CTC was a refundable credit. Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.