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# Effects of Child Tax Credit Design on Employment

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# Effects of Child Tax Credit Design on Employment


**Abstract**

Recent expansions of the Child Tax Credit (CTC) have generated interest in how the credit affects parental labor supply and child poverty. However, there is limited empirical evidence of the labor supply effects of the CTC outside of the context of the COVID-19 pandemic. We address this knowledge gap using 1997-2019 data from the Panel Study of Income Dynamics (PSID) to provide updated estimates of how low and middle-income parents' employment responds to changes in incentives for three groups of parents: unmarried mothers, married mothers, and married fathers. We find the most elastic employment response for unmarried and married mothers, with estimated elasticities of 0.39 and 0.38, respectively, in response to changes in the return to work. Married fathers are least sensitive, with an estimated elasticity of 0.07. We then estimate small but statistically significant income elasticities of -0.025 and -0.132 for unmarried and married mothers, respectively. We also examine how parameter estimates differ based on children's age and the parent's education or race-ethnicity, as well as test the possibility that individuals respond differently to a change in cash wages than they do to economically equivalent changes in tax and transfer program incentives. We use these estimated parameters to simulate the employment effects of eight CTC policy options. We estimate that restoring the fully refundable CTC benefit schedule in place during 2021 would reduce overall employment by about one percentage point, while a fully refundable CTC only for children under age two would reduce overall employment by about 0.1 percentage points. Other reforms to make the CTC more valuable for lower-income workers would modestly increase employment. Our results show that policymakers could expand access to the CTC - including for low-income workers and parents of very young children – while having little effect on parental employment. These are timely considerations as policymakers consider whether to renew the current law CTC provisions – which are set to expire after 2025 – or expand eligibility.



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## I. OVERVIEW

The American Rescue Plan Act of 2021 (ARPA) transformed the Child Tax Credit (CTC) for one year, substantially increasing the maximum credit and making the full credit available to low-income families with little or no income tax liability (a change often referred to as "full refundability"). This reform shifted the design of the CTC towards similarly designed child allowances in Great Britain, Canada, and South Korea, helping to drive a historic reduction in child poverty in 2021 (Burns and Fox 2023).

However, policy makers were concerned that eliminating any work requirement for low-income families to receive the benefit would discourage employment. Some research studies (e.g., Corinth et al. 2021) concluded that, had the 2021 changes to the CTC been made permanent, there would have been large reductions in labor force participation, thereby reducing the credit's impact on poverty and raising program costs. The estimates produced by these studies were largely based on various microsimulations models and the results, as others have noted, are sensitive to the assumptions of each model (Wielk et al. 2023; Bastian 2024). These models assume that households respond the same way to CTC tax incentives as they have to broader changes in household income, including changes in the generosity of the Earned Income Tax Credit (EITC), another refundable tax credit targeted to working parents of dependent children.

We test these assumptions using data from the Panel Study of Income Dynamics (PSID). This nationally representative, biannual panel dataset contains measures of various sources of household income that have been consistently measured across decades, including since the CTC's introduction in the late 1990s. Unlike prior work, instead of selecting labor supply elasticities from the existing literature on labor supply and social policy, we use the PSID to produce updated elasticity estimates, based on changes in the return to work (RTW) and nonlabor income between

1997-2019. We do so for three groups of parents: unmarried mothers, married mothers, and married fathers. We find the most elastic employment response for unmarried and married mothers, with estimated elasticities of 0.39 and 0.38, respectively, in response to changes in the RTW. Married fathers are least sensitive to the RTW, with an estimated elasticity of 0.07. We also test the possibility that individuals respond differently to a change in cash wages than they do to economically equivalent changes in tax and transfer program incentives. When we separate out the three major components of our RTW measure—changes in labor income, changes in taxes (including tax credits like the CTC and EITC) and changes in SNAP (i.e. "food stamp") benefits—we find evidence that married mothers are more responsive to changes in labor income than changes in tax liability. Finally, we estimate small but statistically significant income elasticities of -0.025 and -0.132 for unmarried and married mothers, respectively. Married fathers' employment does not appear to be responsive to increases in nonlabor income.

We also examine how parameter estimates differ based on children's age, and the parent's education or race-ethnicity. Consistent with prior work (Schanzenbach and Strain 2024), we find that unmarried mothers with a high school degree or less are most responsive to both changes in the RTW and nonlabor income. We also estimate greater elasticities for Non-Hispanic Black unmarried mothers and married fathers relative to Non-Hispanic Whites and Hispanics. Among married mothers, we find that Hispanics and those with very young children are most responsive to changes in the RTW and non-labor income.

We use these estimated parameters to simulate the employment effects of eight policy options, some of which were originally proposed by Maag (2023) and explored in Crandall-Hollick et al. (2025). The options include the CTC reform proposed in the 2024 Tax Relief for American Families and Workers Act (H.R.7024), often referred to as "Smith-Wyden" after its lead sponsors, which passed the House of Representatives with bipartisan support but failed in the Senate, and restoration of the ARPA CTC. We estimate that the H.R.7024 provision of making the refundable portion of the CTC available on a per-child basis would modestly increase overall employment, especially for unmarried mothers. The larger, fully refundable ARPA CTC would reduce overall employment by about one percentage point. Again, employment of unmarried mothers would be most responsive, falling by 2.65 percentage points. In contrast, the employment of married mothers and fathers is only estimated to fall by 0.67 and 0.39 percentage points, respectively. Earlier research also suggests these options would produce major reductions in child poverty.

If policymakers want to limit the cost or employment effects of CTC policy changes but also want to make the credit accessible to parents with modest earnings and very young children at home—for whom work-related costs such as childcare may serve as a particularly large barrier to labor force participation— we estimate that a fully refundable CTC for children under age two would reduce overall employment by about 0.08 percentage points, with most of the change occurring among unmarried mothers whose employment would fall by about 0.13 percentage points.

We start by reviewing the evidence on how refundable tax credits such as the CTC and EITC affect labor force participation. We then discuss the PSID and our models of the effect of changes in the RTW and nonlabor income on employment. We summarize the empirical evidence from our models, including how employment would respond to various CTC policy alternatives, and examine differences by age of children, education status, and race and ethnicity. The final section summarizes our main conclusions and plans for future research.

## II. BACKGROUND AND LIT REVIEW

### A. The Child Tax Credit and the Earned Income Tax Credit

The CTC provided $42.7 billion in benefits to families in 2022 (this includes both the nonrefundable portion of the credit, which reduces any income taxes owed, and the refundable portion of the credit).[1] Today, the CTC, along with the EITC, are the largest sources of cash assistance for low-income working families with children. According to the most recent Census Bureau data for 2023, these credits lifted 6.4 million people out of poverty, slightly more than half of whom (3.4 million) were children (Shrider 2024). Because of their importance in the social safety net, we begin by documenting how these credits were created and work today.

#### *1. Background and History of the EITC*

While not the primary focus of our analysis, as the larger and older refundable tax credit, the EITC informs a substantial amount of research and analysis of the CTC. The EITC was originally enacted in 1975, more than two decades before the CTC. Unlike the CTC, which was initially focused on middle-income families with children, the EITC was always targeted at low-income workers with children.[2] Because the credit is refundable, tax filers can get the full benefit of the credit even if they have little or no income tax liability.

The EITC is calculated as a percentage of the taxpayer's earnings up to a maximum amount. Once the credit reaches its maximum, it remains constant until earnings reach the phaseout point. After that point, the credit declines with each additional dollar of income until the credit equals zero. The credit is larger for families with children compared to those without and increases in size until families have three or more children, as illustrated in Figure 1.

The EITC has undergone numerous changes over the past 50 years, but the largest and most substantial occurred in 1993, when it was expanded with the goal that families with children working a minimum wage job would not be in poverty[3] (Crandall-Hollick 2022, Nichols and Rothstein 2016). With the exception of 2021, the average EITC has generally been larger than the CTC for low-income families (TPC 2022).

### 2. *Background and History of the CTC*

The CTC was first enacted in 1997. It was a relatively modest $400 credit per child age 16 and under that was nonrefundable in most cases (i.e., it could only be used to offset income taxes owed), increasing to $500 in 1999 (see Table 1). Since then, Congress has altered the credit in a myriad of ways –increasing the credit amount, allowing low-income taxpayers to receive part or all of the credit in the form of a refundable credit based on their earned income, and expanding the income range over which families are eligible (see Table 1). In 2022, almost 90 percent of families with children under age 17 benefited from the credit (TPC 2022). The following sub-sections provide more information on CTC changes over time.

#### a. Enactment to the TCJA (1997-2017)

When it was first enacted in 1997, the CTC was nonrefundable for most taxpayers.[4] In 2001, Congress modified the credit to allow a portion of it to be available as a refundable credit. That refundable portion of the credit—which the IRS calls the additional child tax credit or ACTC—was worth up to 10 percent of earnings above $10,000 not to exceed the maximum CTC amount, which at the time was $600 per child (Crandall-Hollick 2021). The refundable portion of the CTC was originally designed in 2001 to coordinate with the EITC. Once earnings reached $10,020 for families with two children in 2001, there was no further increase in the EITC. The earnings threshold for the refundable CTC was thus set at $10,000 so families could receive a subsidy for earnings in excess of that amount.[5] Concerned that the $10,000 threshold limited the CTC's value to lower-income working families, Congress reduced the earnings threshold to $8,500 in 2008 and then to $3,000 in 2009, severing the link between the CTC phase-in and the maximum EITC.

Congress also increased the maximum credit per child from $600 to $1,000 in 2003 and increased the phase-in rate for the refundable portion of the credit from 10 to 15 percent starting in 2004. Bringing these changes together, the "Pre-TCJA" line in Figure 2 shows the CTC benefit schedule in place from 2009-2017.

Congress made further changes to the credit as part of the Tax Cuts and Jobs Act (TCJA) that went into effect in 2018 and are scheduled to expire at the end of 2025 (see Figure 2). First, the TCJA increased the maximum credit per child to $2,000. The law also increased the refundable portion of the credit to $1,400. This maximum refundable portion of the credit was indexed for inflation (and is the only CTC parameter currently indexed for inflation).[6] Finally, the income level at which the credit begins to phase out was increased from $75,000 in AGI ($110,000 for married couples) to $200,000 ($400,000 married), in part to offset the loss of personal exemptions under the TCJA.[7]

b. ARPA (2021)

In 2021, Congress temporarily modified the credit for one year and effectively turned the CTC into a child allowance for low- and middle-income families as part of the American Rescue Plan Act (ARPA). The lowest income families with children, including those with no earnings, were eligible for the full credit for the first time.

ARPA expanded the CTC in three ways. First, the maximum credit increased to up to $3,000 per qualifying child ages six and over and $3,600 per child under age six.[8] Second, the maximum age for an eligible child was increased from 16 to 17. Third, the phase-in with earnings was eliminated, making the full or maximum credit available to the lowest income families, including those without any earned income to report on their tax return or taxes owed. This policy change became known as “full refundability” and is illustrated in Figure 2. The expansion also included a change in delivery method: almost all families eligible for the credit received half of it in monthly payments from July through December of 2021. They received the remainder when they filed their tax return the following year.

This temporary expansion provided the most substantial increase in benefits to children in poverty and lifted about 3 million children out of poverty. If ARPA had not been enacted, the CTC would have lifted fewer than a million children out of poverty, meaning two-thirds of the poverty reduction benefit of the credit in 2021 came from the ARPA changes (Burns and Fox 2023). Nonetheless, Congress declined to extend the ARPA expansion of the CTC past 2021. If the ARPA CTC had been in effect in 2023, researchers at Columbia University estimate that child poverty would have fallen by nearly half (47.8%) (Koutavas et al. 2024). In 2025, while most children live in families that receive the CTC, approximately 17 million children—a quarter of all children—live in families whose incomes are too low to receive the maximum benefit (Maag 2024).

c. ARPA Impacts Across Race and Ethnicity

A credit that phases in with earnings (like the EITC or the CTC outside of the ARPA changes) provides less benefits to the lowest-income households, which produces substantial disparities by race and ethnicity. Prior to 2021, three-quarters of white, non-Hispanic and Asian children were eligible for the full CTC, compared with only about half of Black and Hispanic children (Goldin and Michelmore 2022). The 2021 expansion closed those gaps and helped to more than halve the share of Black, Hispanic, and white non-Hispanic children in poverty (Creamer et al 2022, see Table B-2).[9][10] This is particularly significant given that Black and Hispanic children have long exhibited persistently higher levels of poverty and economic hardship than non-Hispanic white and Asian children (Creamer et al 2022, see Table B-2). At the regional level, the ARPA CTC delivered economic support and larger poverty reduction to states with higher poverty and lower costs, particularly helping families in southern states (Collyer, Hardy, and Wimer 2023). This – combined with the federal administration of CTC benefits – has important implications for racial economic inequality, given the concentration of Black (Martinez and Passel 2025) and, to a lesser degree, Hispanic families (Krogstad et al 2023) in states that generally provide lower levels of cash assistance via their welfare block grants (Hardy, Davis, and Samudra 2019; Parolin 2019) as well as lower levels of support for redistribution (Fording et al. 2007).[11]

### B. Responses to the EITC and CTC

Economic theory suggests that tax policies that change after-tax wages, like the EITC and the CTC, can be expected to change labor supply through two mechanisms: an income effect and a substitution effect. Underpinning this framework is the assumption that when a worker is deciding whether to work more or less, they are choosing between two goods: leisure (i.e., time spent outside of paid work) and other goods and services (purchased with after-tax dollars earned at work). The idea behind the income effect is that higher income allows more consumption of both goods and leisure. That is, the income effect on labor supply is negative because leisure is a "normal good" (i.e., demand for a good like leisure increases as income rises). The substitution effect says that when income adjusts to hold utility (well-being) constant, a consumer will tend to substitute away from a commodity when its price rises relative to other goods. A higher wage increases the price of leisure relative to other goods and services, meaning that the substitution effect on labor supply is positive. The net effect of an increase in after-tax wages on labor supply is theoretically ambiguous because the income and substitution effects have opposite signs (Killingsworth and Heckman, 1986).

The ultimate impact of the EITC or CTC on work thus depends on the relative size of these effects, which is an empirical question. These incentives also differ across the various sections of the EITC and CTC benefit schedules. If household earnings are in the phase-in range of a credit, there are both income and substitution effects. If earnings, however, place a household in the plateau region of the credit, there is only an income effect, and theory would predict a reduction in labor supply that could take the form of either leaving the paid workforce or reducing work hours. Finally, if a household is in the phase-out range of a credit, this reduces after-tax earnings (as each dollar of additional earnings reduces the amount of tax credit). In this case, both the substitution and income effects would predict a reduction in hours worked.

1. *Evidence from EITC Effects on Labor Force Participation*

The literature on the EITC has found little effect on hours worked, outside of those with self-employment income (CBO 2012, Meyer 2002, Saez 2010). Thus, much of the research on the impact of the tax credits like the EITC and CTC has tended to focus on decisions about whether to work or not (the extensive margin of labor force participation) as opposed to hours worked (the intensive margin).

This research finds that increases in EITC generosity increase unmarried mothers' labor force participation (Bastian 2020, Dahl et al. 2009, Eissa and Liebman 1996, Gelber and Mitchell 2012, Grogger 2003, Hoffman and Seidman 1990, Hoynes and Patel 2018, Meyer and Rosenbaum 2001, Schanzenbach and Strain 2021). Studies found the largest employment responses to EITC expansions during the 1990s. Studies based on more recent data have found smaller labor supply responses (Chetty et al. 2013, Hoynes and Patel 2018, Bastian and Jones 2021, Bastian 2023). Research has also found that income effects tend to be small and negative (Blundell and Macurdy 1999, Eissa and Hoynes 2004; 2006, Jones and Marinescu 2018, Baker et al. 2021).

Eissa and Hoynes (2004, 2006b) also examine the effect of the EITC on the employment of and hours worked by married couples. Consistent with theoretical expectations, as well as the fact that the labor supply of secondary earners is generally more elastic than that of primary earners, Eissa and Hoynes (2004) find that married mothers were 1.1 percentage points less likely to work in 1996 than in 1984, following various federal EITC expansions, and that these effects are concentrated among households on the plateau and phase-out ranges of the EITC benefit schedule. Newer research has found that married women's responsiveness to tax policy changes has declined over time (Blau and Kahn 2005, Elder et al. 2023).

While most research on the labor force impacts of the EITC has used a difference-in-differences (DiD) framework, some research has incorporated structural models to understand the impact of the EITC on labor supply decisions. Estimates derived from structural models can be particularly useful in predicting the employment impact of future policy changes, including as inputs to microsimulation models (see for example Blundell et al. 2000). Meyer and Rosenbaum (2001) incorporate a structural approach and find expansions to the EITC explained about 60 percent of the increase in the employment rate of single mothers from 1984 to 1996 and roughly 31 percent of the increase between 1992 to 1996. Keane and Moffit (1998) look at a wide range of social policy reforms including changes to the EITC and find credit expansions between 1984 and 1996 increased labor force participation by 16% from 65.4% to 76.1% among single mothers.[12]

Newer research has focused on taxpayers' knowledge of refundable tax credits. Mathematically, a wage subsidy like the EITC or CTC operates like an increase in after-tax earnings, but the credits are not reflected in periodic paychecks. Instead, the work subsidy comes in the form of an annual tax refund received many months after a decision to work has been made.

Some research suggests that people may not fully understand how tax credits like the CTC affect their marginal tax rate, and thus their return to work (Feldman et al., 2016; Kleven 2024).

*2. Research on Labor Force Participation Responses to the Child Tax Credit Prior to ARPA*

To date, a few working papers (Kang 2021, Lippold 2022, Zheng 2023) have examined the effect of the CTC on parental labor supply prior to the ARPA expansion. These studies find positive effects on employment, especially for less educated unmarried women (Kang 2021) and unmarried women with young children (Zheng 2023). This is consistent with the literature on the EITC, which finds that the employment effects of the EITC on unmarried mothers are concentrated among those with young children (Michelmore and Pilkauskas 2021).

Wiersma Strauss (2025a) goes beyond this focus on female labor supply to estimate how the TCJA CTC expansion affected employment and hours worked by married primary and secondary earners and unmarried mothers. This analysis looks at the heterogeneity of these effects across the different regions (phase-in versus plateau) of the CTC benefit schedule and finds that unmarried mothers and married parents in the phase-in range of the CTC benefit schedule, who experience a substitution effect from the TCJA CTC expansion, are more likely to be employed or increase their hours worked. However, those beyond the phase-in range of the CTC benefit schedule, who mainly experience an income effect from the TCJA CTC expansion, are estimated to maintain or reduce their labor supply. Wiersma Strauss (2025a) also finds that CTC benefit generosity has differential effects on labor supply by whether one has a young child, with secondary earners that have a young child most responsive to the income effect provided by greater CTC benefits.

*3. The Debate Around the ARPA CTC Expansion*

The short-run benefits from the expanded 2021 CTC went beyond poverty reduction to include reduced food hardship and food insecurity (Karpman et al. 2022, Moellman et al. 2024), improved child developmental outcomes (Aizer et al. 2024), and reduced housing instability (Pilkauskas et al. 2024). In the long run, there is evidence that cash assistance can dramatically improve educational, health, employment, and incarceration outcomes for children as they move into adulthood (Aizer et al. 2024; Hoynes and Schanzenbach 2018; Maag et al. 2023; Garfinkel et al. 2022). Nonetheless, Congress declined to extend the ARPA expansion of the CTC, ending the temporary reduction in child poverty (Burns et. al 2022).[13]

Much of the political pushback against reinstating the ARPA expansion to the CTC centered on concerns that making such an expansion permanent outside of the context of the COVID-19 pandemic would lead to substantial reductions in parental employment. For example, some critiques of the 2021 CTC compared the credit to the cash welfare program, Aid to Families with Dependent Children (AFDC), which was repealed in 1996 because it penalized work. (Weidinger 2021, 2023). Indeed, the theoretical implication of replacing a CTC that phases in with earnings with a larger unconditional credit is unambiguous. It eliminates the work incentive created for households in the phase-in range of the credit while also increasing income. Both the substitution and income effects generated by this type of policy change will tend to depress employment. The empirical question is by how much.

Some evidence from the one-year experiment in full refundability suggests little or no impact on employment. Schanzenbach and Strain (2024) summarize seven studies, all of which found statistically insignificant effects of the ARPA CTC payments on employment, using various data sources.[14] However, Schanzenbach and Strain (2024) extend these analyses to focus on the demographic groups most likely to be affected by the ARPA expansion, finding statistically significant reductions in employment for unmarried, non-college educated mothers with a child under the age of six.

While these studies provide valuable evidence, the temporary nature of the ARPA expansion combined with other disruptions during 2021 related to the global pandemic make the policy implications unclear. Prior research on the EITC had found that employment responses can increase over several years (Bastian 2020, Hoynes and Patel 2018), highlighting the importance of examining labor supply effects over longer time periods.

Researchers have thus estimated the potential impact on employment of a permanent extension of the ARPA CTC using different microsimulation models (see Table 2). However, given the lack of evidence on the labor supply effects of the CTC itself, especially prior to the ARPA reform, these papers rely on elasticity estimates from the wider social and tax policy literature, much of which is based on changes that occurred in the late twentieth century. For example, the income elasticities shown in Table 2 and selected by NASEM (2019), Corinth et al. (2021), and Goldin and Michelmore (2022) are all taken from Blundell and MaCurdy (1999). As shown in Table 2, many of these microsimulation models produced estimates suggesting between 150,000 and 400,000 individuals would exit the workforce. In contrast, Corinth et al. (2021) estimated that 1.46 million individuals would leave the workforce entirely if the ARPA expansion was made permanent. The differences in these estimates were largely driven by differing assumptions about labor supply responsiveness (see Table 2).[15]

Given the continued interest in understanding how different expansions of the CTC could affect parental employment, limited evidence on the labor supply effects of the CTC prior to the ARPA expansion, and the upcoming expiration of the TCJA CTC expansion, this paper provides new analysis of the responsiveness of parental employment to changes in tax policy. While much of the academic literature has focused on modelling a permanent expansion of the ARPA CTC, lawmakers have considered a variety of other options for the CTC. In 2024, the House of

Representatives passed legislation (H.R.7024) with broad bipartisan support that would have multiplied the phase-in rate for the refundable CTC by the number of qualifying children and raised the maximum refundable credit. The legislation failed in the Senate, however, after then-former President Trump opposed it.[16] Other proposals offered during the 2024 presidential election campaign and most recently by Representative Jason Smith (R-MO) would increase the maximum credit amount while retaining the current credit's phase-in structure.[17] Some analysts have proposed an approach that provides some of the benefit irrespective of earnings, and phases in the remainder (Bastian 2023, Edelberg and Kearney 2023, Maag 2023).

## III. DATA AND SAMPLE

Our empirical analysis is based on the Panel Study of Income Dynamics (PSID), which allows for study of how individuals and families respond to policies over time. The PSID is a nationally representative household survey that has followed households and their offspring since 1968, conducting annual interviews through 1997 and biannual interviews since. The sample has grown to more than 9,000 households and 26,000 individuals by 2019, with over 80,000 individuals participating over the course of the study. While the weighted sample is nationally representative, the initial design oversampled low-income families to facilitate investigations of poverty-related issues. The main advantage of using the PSID is that it provides panel data that covers the entire period since the CTC was enacted and includes detailed, consistently measured information on various sources of household income. The drawback is that the sample size is relatively small. However, its over-weighting of low-income households makes the survey especially well targeted to the group of households most affected by changes to the formula for CTC refundability.

We use the 1997-2019 PSID waves, which cover all the significant variations in federal CTC policy parameters except for the larger, fully refundable CTC made available as part of ARPA in

2021. We exclude that year because the extreme disruption of the global pandemic makes it difficult to isolate the separate effect of tax policy from the many other confounding factors at play that year. Our sample includes PSID reference persons with at least one child under 16 years old (N=33,678 person-years).[18] We exclude parents under the age of 19 or over age 55 (N=572 person-years). We also exclude unmarried fathers because the sample size is too small for meaningful analysis of this group (N=3,685 person-years). To focus on an able-bodied sample that could reasonably be expected to participate in the paid labor force, we also exclude those with a moderate to severe work-limiting disability (N=2,287 person-years) or who rate their health as poor (N=289 person-years). Finally, we drop those with missing demographic variable information (e.g., age, health, race and ethnicity, education; N=1,007 person-years).[19] The final sample includes 6,910 able-bodied, potentially CTC-eligible households. This includes 2,592 unmarried mothers, representing 8,971 person-years, and 4,318 married couples, representing 16,867 person-years of information about each member of the couple.

## IV. ESTIMATION METHODOLOGY

The CTC can affect employment in two ways.[20] First, the phase-in of the refundable tax credit with earnings is designed to encourage work by raising its after-tax return. In the phase-in range for the refundable CTC, every dollar of additional earnings produces a $1.15 increase in after-tax income including the tax credit. Second, those eligible for a tax credit even if they don’t work (including many secondary earners) experience an increase in household income, which could discourage work because leisure is a normal good. To capture these effects, we need to measure how the CTC affects both the return to work and nonlabor income.

We thus estimate an employment model that includes the effect of both labor and nonlabor income, as well as taxes, tax credits, and other common forms of cash or near cash assistance including SNAP. Our analysis focuses on the extensive margin (the decision to work) rather than the intensive margin (hours worked) for three main reasons. First, much of the debate about CTC policy design has been about how CTC design affects participation in the labor force. Second, as noted previously, evidence from the EITC suggests that the intensive margin is relatively insensitive to tax credits. Third, data on hours worked in publicly available surveys such as the PSID are quite noisy (because of missing values and measurement errors), making small changes in hours more difficult to precisely estimate.

### A. Estimating the Return to Work and Income

People are assumed to enter the work force if their net (after-tax) market wage at zero hours of work is greater than their unobserved reservation wage, which reflects the value of nonmarket activities such as leisure and unpaid activities such as childcare. Wages can be calculated as total earnings conditional on work divided by total hours. We define earnings as total labor income, including wage, self-employment, and farm income. As noted, measures of hours of work are imprecise, so we estimate the decision to work in terms of the total return to work (RTW). RTW is estimated as earnings conditional on working net of the change in taxes and transfers. This is similar to the marginal return to work measure used by Bastian (2023), but our measure captures the average return. This is appropriate because full-time workers often do not get to choose their hours of work, and the effect of taxes and transfers on net income is highly nonlinear. Importantly, the CTC for most of its history had zero marginal effect on the first hour of work for most single parents but increased with hours for individuals in the phase-in range for the refundable credit. Thus, a marginal return to work at zero hours would capture no effect of the CTC whereas a total (or average) return to work would reflect the considerable subsidy that applies in many cases.

To calculate the RTW, we create a measure of predicted earnings by using ordinary least squares (OLS) regression to estimate the relationship between current earnings and prior-wave earnings (with missing values replaced by zeroes), a dummy for zero prior-wave earnings, state and year fixed effects, and demographic and other control variables, including for state policies and economic conditions that change over time such as the minimum wage, unemployment rate, and maximum TANF benefits (see Appendix Table A1 for full estimates). We winsorize each of the income-related variables included in these regressions at the 99th percentile, to reduce the effect of outliers. As noted, the PSID data since 1997 have been biennial, so the lagged variables are usually two years prior to the current wave; we constrain our sample to those observed at most four years (two survey waves) prior. This constraint retains 96 percent of observations. Including the lagged dependent variable further drops from the sample the first person-year observed for each individual: the sample now includes 6,162 person-years for unmarried mothers, and 12,387 person-years each for married mothers and fathers (unweighted). Dollar values are indexed for inflation so all amounts are in constant 2019 dollars.

We estimate earnings separately for unmarried mothers, married mothers, and married fathers. Then we use the estimated coefficients to calculate the predicted (fitted) earnings, E*. These predicted earnings are highly correlated with actual earnings for those with earned income: The correlation is 0.78 for unmarried mothers, 0.85 for married mothers, and 0.82 for married fathers (see Figure 3). Based on the nominal value of these predicted earnings, we restrict our final sample to those at or below the original phaseout range for the CTC (i.e., unmarried mothers with E* less than $75,000 and married parents with E* that sums to less than $110,000) to focus our analysis on households most affected by changes to the formula for CTC refundability. In doing so we drop very few unmarried mothers (110 person-years) but close to one-third of the observations for married parents (3,823 person-years), resulting in a final sample of 1,914 unmarried mothers (6,052 person-years) and 2,803 married parents (8,564 person-years).

For each individual observation, we define the return to work (RTW) as follows:

$$RTW = E^* - (T(E^*, \Omega) - T(0, \Omega)) + SNAP(E^*, \Lambda) - SNAP(0, \Lambda)$$

T denotes federal and state income tax liability after credits, including the EITC and CTC. $T(E^*,\Omega)$ is income tax liability given earnings equals $E^*$; $T(0,\Omega)$ is income tax liability with zero individual earned income. Note that $T(E^*, \Omega)$ may be negative for low-income working people because of refundable tax credits. $\mathbf{\Omega}$ is a vector of variables other than the individual's own earnings that affect tax liability after credits (such as spousal earnings, other income,[21] deductions, number and age of children, state of residence, and year). We estimate T using NBER's TAXSIM program; [22] when doing so, we lag the survey year by one year. This allows our models to estimate how employment responds to changes in the generosity of CTC benefits that a PSID respondent is estimated to receive during the year in which they are observed.[23]

$SNAP(E^*, \Lambda) - SNAP(0, \Lambda)$ is the change in SNAP benefits attributable to earnings, which reduces benefits at a 30 percent rate at incomes ranging from 130 to 200 percent of the federal poverty level. This threshold, which is known as "broad-based categorical eligibility" or BBCE, varies by state and year. Thus, over a range of earned income, the SNAP phaseout amounts to a 30 percent marginal income surtax. $\Lambda$ is a vector of variables other than individual earnings that affect SNAP benefits (spousal earnings, household size, state, and year).[24]

We also calculate an after-tax and after-SNAP nonlabor income measure (Y) defined as:

$$Y = Y_0 - T(0, \Omega) + SNAP(0, \Lambda)$$

where $Y_0$ is nonlabor income, which includes spousal earnings, capital income included in taxable income (interest, dividends, rent, trusts), lagged transfer income (both private and from means-tested transfer programs other than SNAP),[25] and lagged Social Security (SS) income. We use the lagged value for transfer payments as a proxy for current transfer income because the latter is endogenous: current transfer income depends on earnings.[26] Y also includes SNAP(0, Λ) – T(0, Ω)—that is, SNAP benefits minus tax liability—both computed assuming zero individual earnings.

Table 3 provides descriptive statistics. Panel A is focused on the demographic and state-year control variables utilized in the regressions to predict earnings, while Panel B is focused on the income and tax variables just described. Table 3, Panel A shows that the sample of married parents is somewhat older and has more children on average compared with unmarried mothers. Married parents also have higher educational attainment, better self-reported health, and are more likely to be Non-Hispanic (NH) White. In contrast, unmarried mothers are disproportionately NH Black. Married parents are also more likely to have a non-school-aged child. Less than 3 percent of the sample includes an adult household family member other than a spouse.

Rates of employment and earnings are lowest among married mothers; 70 percent are employed (Table 3, Panel B). If unmarried mothers work, their estimated RTW is boosted by an average reduction in tax liability of $1,884, thanks to the refundable EITC and CTC. In contrast, married parents are estimated to be eligible for about $1,700 in CTC benefits on average, which offset, but do not eliminate, their income tax liability when working. The greatest source of nonlabor income for unmarried mothers is SNAP benefits, followed by transfer income. In contrast, spousal earnings account for the bulk of married parents' nonlabor income, and average transfer income is less than half that of unmarried mothers.

## B. Estimating Employment

### *1. RTW Model*

We estimate employment effects using two variants of the key RTW independent variable. The base model uses $RTW$ and $Y$ as described above. We estimate the function by logit maximum likelihood, which assumes there is an unobserved criterion function, which we will call W, such that the person works if and only if W>0. The latent criterion function may be written as:

$$W_{ist} = \beta_0 + \beta_1 RTW_{ist} + \beta_2 Y_{ist} + \beta_3 X_{ist} + \delta_s + \theta_t + \varepsilon \quad (1)$$

$RTW$ and $Y$ are as defined above, with subscripts for individual $i$, state $s$, and year $t$. $X$ is a vector of control variables and $\delta_s$ and $\theta_t$ are state and year fixed effects, respectively. ε is a random error term. If we assume that ε has a logistic distribution, we can estimate the parameters by logit maximum likelihood. In doing so, we use PSID individual weights and robust standard errors, clustered at the individual level.[27]

The vector of control variables, $X$, includes both taxable and transfer income attributable to other PSID family unit members,[28] and controls for age and age-squared, education (high-school degree or less; some college; four-year college degree), race-ethnicity (NH White, NH Black, Hispanic, Other/Don't Know), the number of CTC-qualifying children (1, 2, 3, or 4+),[29] self-reported health status (excellent, very good, good, or fair), as well as binary controls for whether the respondent has a child under age two or a child between two and five years old, presence of a cohabitating partner or another adult family member (e.g., a grandparent), whether the respondent lives in a metropolitan area, and whether the respondent reports a minor work-limiting disability. We also include three measures to control for employment-related conditions in the state of residence that vary over time: the minimum wage, maximum welfare benefits for a family of three, and the state unemployment rate.[30]

*2. Separate Components Model*

The RTW model assumes that people respond to tax incentives and transfer payments the same as they do to cash earnings and self-employment income. Our second model (Equation 2) allows those responses to differ. It splits $RTW_{ist}$ into separate variables for labor income ($E^*$), difference in estimated tax liability when working versus not (*Taxes*; equivalent to T(E*, Ω) – T(0, Ω)), and difference in estimated SNAP benefits when working versus not (*SNAP*; equivalent to SNAP(E*, Λ) – SNAP(0, Λ)). We expect the coefficients on $E^*$ ($\alpha_1$) and *SNAP* ($\alpha_3$) to be positive and the coefficient on $Taxes$ ($\alpha_2$) to be negative. Similarly, we split nonlabor income ($Y_{ist}$) into spousal earnings (*Spouse*) versus all other nonlabor income (*Other*). Note that *Other* includes the effect of taxes and tax credits on income if not working. Splitting the income components allows for employment to respond differently to spousal earned income than to other nonlabor income. The RTW model (Equation 1) is a special case of the separate components model (Equation 2), in which $\alpha_1$ = - $\alpha_2$ = $\alpha_3$ and $\alpha_4$ = $\alpha_5$. In contrast, the separate components model allows the absolute values of these coefficients to differ.

$$W_{ist} = \alpha_0 + \alpha_1 E *_{ist} + \alpha_2 Taxes_{ist} + \alpha_3 SNAP_{ist} + \alpha_4 Spouse_{ist} + \alpha_5 Other_{ist} + \alpha_6 X_{ist} + \delta_s + \theta_t + \varepsilon \qquad (2)$$

The magnitudes of $\alpha_2$ and $\alpha_5$ are now of particular interest, especially in comparison to the magnitudes of $\beta_1$ and $\beta_2$ from Equation 1 (RTW model), respectively, because they reflect how employment would respond to changes in the CTC. If the absolute value of the magnitude of these coefficients differ, then the use of these models for policy simulation purposes will result in differing predicted effects of CTC policy changes on employment. If the absolute values are similar, this instead indicates that people respond to tax and non-tax incentives in the same way, as predicted by theory. In this case, the constrained RTW model is consistent with the data.

## V. RESULTS

### A. RTW Model

Table 4 displays the results for our base RTW model. Panel A displays average marginal effects of a $1,000 *RTW* increase; Panel B displays corresponding elasticities[31] (see Appendix Table A2 for logit coefficient estimates for all model variables). Some of the control variables – including child age, race-ethnicity, and living in a metro area – have statistically significant effects on the probability of employment in addition to the key independent variables of interest (*RTW* and *Y*). The return to work has a positive and statistically significant estimated effect on employment for all three groups of parents (Table 4, Panel A). Unmarried mothers and married mothers have similarly elastic responses to changes in *RTW*, of 0.39 and 0.38, respectively (Table 4, Panel B). While primary earners also have a statistically significant positive response to *RTW*, their elasticity is an order of magnitude smaller, at 0.07. This is not surprising since almost all the fathers in this group—95 percent—are employed.

The employment of married mothers is estimated to be most responsive to changes in nonlabor income, with a statistically significant income elasticity of -0.13. This reflects the fact that most married mothers are secondary earners that have the most ability to take spells out of the work force, because their household and non-labor income includes spousal earnings. While unmarried mothers also have a statistically significant negative response to *Y*, their elasticity is much smaller, at -0.025, while married fathers' employment is not estimated to respond to *Y,* with an elasticity of 0.00. These estimated elasticities are within the range of prior work (see Table 2), with a smaller estimated response by single mothers compared to estimates generated from EITC policy variation during the 1990s and a somewhat more elastic estimated response by married mothers.

## B. Separate Components Model

We tested the unconstrained separate components model by estimating the parameters of Equation 2 (Table 5). Unmarried mothers and married fathers are estimated to respond similarly to changes in earnings and tax liability, although the effects of changes in earnings are more precisely estimated. In contrast, the employment of married mothers is estimated to be more responsive to changes in earnings than changes in tax liability; a Wald test rejects the hypothesis that the absolute values of the earnings and tax coefficients are the same (p=0.0115).[32] Dividing nonlabor income into spousal earnings versus other sources has less of an effect: both married mothers and fathers are estimated to respond similarly to these sources of nonlabor income.

Across all three groups of parents, the absolute value of the estimated average marginal effect for $Taxes_{ist}$ is smaller than that estimated for the $RTW_{ist}$, while similar magnitude coefficients are estimated for total nonlabor income ($Y_{ist}$) and nonlabor income other than spousal earnings ($Other_{ist}$; see Table 4, Panel A). This indicates that use of the separate components model will predict changes in employment in response to CTC policy reforms that are somewhat smaller than those predicted using the RTW model, as parents are estimated to be somewhat less responsive to changes in tax liability than they are to changes in the overall RTW.

## C. Policy Simulations

We use the parameter estimates from each of these models to predict how employment would change for each group of parents and in the aggregate in response to various CTC policy alternatives. The first set of policy alternatives that we simulate are various changes to the TCJA version of the CTC that increase low-income families' access to the refundable portion of the credit. We first lower the refundability threshold from $2,500 to zero. Second, we model the proposal from the 2024 Tax Relief for American Families and Workers Act (H.R.7024), which would allow the CTC to phase in on a per-child basis (15% multiplied by the number of CTC-eligible children). This reform means that for families with more children, less income is required for CTC benefits to fully phase-in than under the TCJA phase-in. Third, we increase maximum refundable CTC benefits to the full $2,000 per child. The TCJA limited the refundable credit to $1,400 per child (indexed for inflation). After modelling each of these options separately, we also model all three of these changes together. The fifth policy alternative is replacing the TCJA CTC with the larger, fully-refundable ARPA CTC. Our sixth policy alternative is a more limited version of this reform, which keeps the TCJA CTC in place for children aged 2 to 16 but introduces the fully-refundable $3,600 ARPA CTC for children under age 2. The seventh policy option that we model maintains the structure of the TCJA CTC but increases the maximum non-refundable credit to $5,000. Finally, our eighth policy option makes this new $5,000 maximum credit refundable.

As noted above, CTC policy can affect the change in tax attributable to working ($Taxes_{ist}$), and thus the return to work ($RTW_{ist}$) as well as net after-tax income at zero hours of work ($Y_{ist}$). We calculate each of these variables for each policy option (see Table 6 and Appendix Table A3). For example, replacing the current law TCJA CTC with the ARPA CTC would raise the maximum CTC benefit amount per child and make these expanded benefits fully refundable. This removes the ARPA CTC from the calculation of $RTW_{ist}$ and $Taxes_{ist}$, since the credit amount no longer depends on working, and instead adds the ARPA CTC to nonlabor income ($Y_{ist}$). In the RTW model, these changes decrease the return to work and increase nonlabor income, while in the separate components model, these changes increase both the difference in tax liability when working and other nonlabor income ($Other_{ist}$). Our parameter estimates imply that these changes would reduce employment.

Table 6 provides further details on these changes in RTW, tax liability, and nonlabor income for the ARPA CTC policy alternative. Full descriptive statistics for each policy alternative can be found in Appendix Table A3. For unmarried mothers, replacing the TCJA CTC with the ARPA CTC is estimated to decrease the average RTW by about 9% and increase average nonlabor income by 58%. For married mothers, replacing the TCJA CTC with the ARPA CTC decreases the average RTW by only 2%. This difference arises because most of the married couples in our sample qualify for the full CTC benefit based on solely the father's income. However, reinstating the ARPA CTC would increase married mothers' other nonlabor income by about 38% (see Table 6, Panel B). Finally, for married fathers, replacing the TCJA CTC with the ARPA CTC is estimated to decrease the average RTW by about 5% and increase other nonlabor income by about 37%.

Using the parameter estimates in Tables 4 (RTW model) and 5 (separate components model), Table 7 shows predicted employment levels in 2019 – the last year of our sample—for the selected policy alternatives in comparison with current law (the TCJA). The predicted employment effects from full refundability (ARPA CTC; Option 5) are modest except for unmarried mothers. The RTW model (Table 7, Panel A) predicts a 3.79 percentage point drop in employment for unmarried mothers, while the unconstrained separate components model (Panel B) estimates that unmarried mothers would have reduced employment by about 2.65 percentage points if the ARPA CTC were in effect in 2019. Even though married mothers have similar price and higher income elasticities than unmarried mothers (Table 4), they experience smaller changes in both the RTW and nonlabor income from this policy change (Table 6), so their employment is only predicted to drop by 0.67 to 0.96 percentage points, depending on the model (Table 7). Estimated employment effects for married fathers are small across all policy alternatives but largest in magnitude for the ARPA option (0.39 to 0.64 percentage points, depending on the model; see Table 7).

We estimate the aggregate change in employment by creating a weighted sum of predicted employment under each policy alternative as compared to the current law (TCJA) employment level. The weights are counts of individuals in each parental group in 2019 based on PSID individual sampling weights. Using the RTW model (Table 7, Panel A), we estimate that aggregate employment would have decreased by 1.50 percentage points if the ARPA CTC were in place in 2019. When using the separate components model, employment falls by 1.03 percentage points.

These estimates are about half as large as those estimated by Corinth et al. (2021) for at least two reasons. First, our estimated price and income elasticities for unmarried mothers and married fathers are somewhat smaller, although our estimated price and income elasticities for married mothers are larger. More importantly, these authors analyzed the impact of the ARPA expansion at the tax unit level and thus assumed that both spouses in a married couple remained in the workforce or both spouses exited the workforce. In doing so, the study does not account for the effect of spousal earnings on $RTW_{\mathrm{ist}}$ and $Y_{\mathrm{ist}}$. Thus, even though we find that married mothers are responsive to the RTW, we estimate a small employment response to the ARPA CTC policy option because their RTW changes little when the CTC is made fully refundable (see Table 6).

Our simulations show that if policy makers wanted to minimize employment effects while maximizing the benefit for parents with very young children, they could reinstate the ARPA CTC for children under age 2 only while maintaining the TCJA CTC benefit schedule for children ages 3 to 16 (Option 6 in Table 7). This option would also reduce the revenue loss because it only applies to young children. We estimate that making this change in 2019 would have reduced overall employment by only 0.12 percentage points (RTW model), with the largest effects (0.17 percentage points) for unmarried mothers.

The remaining options in Table 7 would tend to increase overall employment by a small amount because of their effect on the RTW. Reducing the beginning of the phase-in for the refundable CTC from $2,500 to 0 (Option 1), increasing the CTC phase-in rate on a per child basis (Option 2), or allowing the entire $2,000 credit to be refundable if earnings are high enough (Option #3) would all increase employment slightly for unmarried mothers and reduce it for married parents. This seemingly anomalous result arises because the proposals all increase the tax incentive to work for unmarried mothers, while generating much larger income than substitution effects on average for married parents given their higher average household earnings. If all three of these changes were in effect during 2019, we estimate under the RTW model that the employment of unmarried mothers would have been 1.34 percentage points higher (0.91 percentage points under the separate components model, see Table 7), while employment would fall by 0.19 to 0.38 percentage points for married mothers, depending on the model used. Finally, maintaining the current law (TCJA) CTC benefit schedule but increasing the maximum non-refundable benefit to $5,000 (Option 7) is only estimated to substantially affect the employment of married mothers; depending on the model used, we estimate a 0.24 to 0.54 percentage point increase in employment for this group if this policy change were in effect in 2019 (see Table 7). This is because only households with remaining positive income tax liability, who are on average married parent households (see Table 3), can benefit from these increased non-refundable benefits, In contrast, if this expanded maximum benefit amount was made refundable (Option 8), this would provide a stronger work incentive for unmarried mothers. We estimate a 0.66 to 0.99 percentage point (depending on the model used, see Table 7) increase in employment for this group during 2019 if this policy change was in effect.

### D. Heterogeneity Analyses

Our baseline models implicitly assume that economic responses do not vary based on characteristics that have been found to be relevant in prior work on the EITC and CTC, such as children's age (Michelmore & Pilkauskas 2021, Wiersma Strauss 2025a, 2025b, Zheng 2023), educational attainment (Kang 2021, Zheng 2023); and race-ethnicity (Goldin & Michelmore 2022, Hardy et al. 2022). We thus re-estimate our main models, allowing for different responses across these three parameters. For brevity, we focus on the RTW model in the following sub-sections; results for the separate components model are in Appendix Tables A4-A6.

#### *1. Differences by youngest child's age*

To examine heterogeneity by child age, we use PSID information on the ages of respondents' children to divide each group of parents into three categories: those whose youngest child is under age 2; those whose youngest child is between 2 to 5 years old; and those whose youngest child is school-aged (6 years and older). We then re-estimate Equations 1 and 2 by interacting each of the components of *RTW* and *Y* with dummies for these groups based on youngest child age.

Table 8 shows the results from the interacted RTW model (see Appendix Table A4 for the separate components model). Average marginal effects and elasticities are displayed for each sub-group of parents. Employment responses to changes in RTW and nonlabor income vary little by child age for unmarried mothers and married fathers; none of the differences across any of the youngest child age sub-categories are statistically significant. In contrast, the employment response of married mothers varies with youngest child age. Married mothers with a youngest child under age two have the most elastic responses to both changes in the RTW and nonlabor income. The elasticities for those with a very young child are significantly larger in magnitude than for those with a youngest child age 6 and older. The estimated income elasticity of -0.26 for married mothers with a youngest child under age two (see Table 8, Panel B) is double the overall income elasticity for married mothers of -0.13 (see Table 4), suggesting that the value of nonmarket time is highest for parents of infants. The cost of childcare also is higher for very young children than for older non-school aged children. All these factors would make parents more apt to stay home to care for their infants if they can afford to do so, with married mothers most likely to be in that situation given that they are in general secondary earners within their household.

*2. Differences by education level*

To examine differences by educational attainment across our wider sample of parents, we re-estimate Equations 1 and 2 by interacting each of the components of *RTW* and *Y* with the control indicators for education level. Table 9 shows the results from the interacted RTW model (see Appendix Table A5 for the separate components model). Average marginal effects and elasticities are displayed for each sub-group of parents.

Increases in the RTW are estimated to increase unmarried mothers' employment at all education levels, but the employment of those with the least education (high school degree or less) is most responsive, with an estimated elasticity of 0.49 (see Table 9, Panel B). Those with a high school degree or less are also the only sub-group of unmarried mothers with a statistically significant and negatively signed income effect, corresponding to an estimated elasticity of -0.12. These results align with prior work and the choice to use somewhat larger elasticities in some prior microsimulation models for low-income unmarried mothers (see Table 2).

In contrast, employment responses to changes in RTW and nonlabor income vary little by education for married parents. All married mothers, regardless of their educational attainment, have statistically significant estimated employment responses to changes in the RTW and nonlabor income. For married fathers, none of the differences across any of the education level sub-categories are statistically significant. Dividing married fathers by educational attainment also does not increase the precision of estimated responses to nonlabor income; those remain statistically insignificant, as in the base RTW model (Table 4).

*3. Differences by Race and Ethnicity*

To examine differences by race-ethnicity within our models, we re-estimate Equations 1 and 2 by interacting each of the components of $RTW$ and $Y$ with the control indicators for race-ethnicity. Given the low proportion of the sample within the "Other/Don't Know" category (see Table 3), we drop those in this category from the sample for this analysis only and instead examine differences across NH Whites, NH Blacks, and Hispanics. Table 10 shows the average marginal effects and elasticities from the interacted RTW model (see Appendix Table A6 for the separate components model) for each sub-group of parents.

Increases in the RTW are estimated to increase the employment of unmarried mothers regardless of race and ethnicity. Comparing across groups, the employment of NH Whites is least responsive to changes in the RTW, with an estimated elasticity of 0.29 compared to estimated elasticities of 0.47 and 0.44 for NH Black and Hispanic unmarried mothers, respectively. The larger RTW response among NH Black unmarried mothers is consistent with event-study evidence on employment responses to the EITC (Hardy et al. 2022). While the reported income elasticities across race and ethnicity are all highly inelastic, NH Blacks are the only sub-group of unmarried mothers with a statistically significant income elasticity, corresponding to an estimated elasticity of -0.13. These elasticities are similar to the statistically significant elasticities of NH White married mothers, at -0.11.

Married mothers across race-ethnicity exhibit a positive RTW and negative nonlabor income impact on employment. However, the magnitude of the response for Hispanics is both larger than and statistically distinct from other race-ethnic groups, corresponding to an estimated price elasticity of 0.61 and income elasticity of -0.25 (see Table 10, Panel B). Descriptively, Hispanic married mothers are much less likely to be in paid employment on average (56%) than NH Black (78%) and NH White (72%) married mothers, which could contribute to this greater responsiveness to changes in the return to work and nonlabor income.

Dividing married fathers by race-ethnicity does not increase the precision of estimated responses to nonlabor income; those remain statistically insignificant, as in the base RTW model (Table 4). However, NH Black fathers are estimated to be most responsive to changes in the RTW, corresponding to a positive and statistically significant price elasticity of 0.16 (see Table 10, Panel B). This is more than double the overall price elasticity for married fathers of 0.07 (see Table 4).

*4. Policy Simulations*

These heterogeneity analyses reveal that employment responds differentially to changes in the RTW and nonlabor income by the age of one's youngest child, educational attainment, and race-ethnicity. We thus re-estimate our CTC policy simulations based on these interacted models. Table 11 shows how the estimates from these interacted models compare with our baseline estimates for both the ARPA CTC and "ARPA CTC for children under age 2" policy alternatives; comparisons with other CTC policy alternatives can be found in Appendix Table A7. The predicted aggregate employment effects vary little from the base models when using the interacted models. The largest differences come from using the interacted RTW models by education level and race-ethnicity and are driven by unmarried mothers (see Table 11, Panel A). The differences are smaller across the various specifications of the separate components model (see Table 11, Panel B). Thus, while these heterogeneity analyses provide valuable knowledge and more detailed elasticity estimates across relevant socio-economic and demographic characteristics, they do not greatly affect the overall conclusions about employment effects.

## VI. ROBUSTNESS CHECKS

Our results are robust to using both the RTW and separate components models, as well as accounting for heterogeneity by age of one's youngest child, educational attainment, and race-ethnicity across these models. We further test the robustness of these results in various ways. Appendix Table A8 tests whether our baseline results are sensitive to empirical specification by testing various alternative, more stringent specifications. This includes adding state-by-year fixed effects; additional fixed effects for differences by education across states, over time, and by number of CTC-eligible children, as well as differences by number of CTC-eligible children across states and over time; and further leveraging the panel nature of the PSID by adding in a control for one's employment status during the prior PSID interview. We show these results as estimated elasticities alongside our main specification. Appendix Table A9 shows the predicted changes in employment for all CTC policy alternatives using the most stringent empirical specification with state-by-year fixed effects and the lagged dependent variable control. Results are similar to the main aggregate estimate from the separate components model.

Lastly, instead of using lagged transfer income to measure $Y_{ist}$, we instead predict transfer income, running an OLS regression similar to the regression used to predict labor income. This regresses current transfer income against prior-wave transfer income, a dummy for zero lagged transfers, state and year fixed effects, and demographic and other control variables. We find that this model only has predictive power for unmarried mothers (see Appendix Figure A1)[33] and that the RTW model for unmarried mothers using predicted transfer income to measure $Y_{ist}$ produces similar elasticities to the main results (see Appendix Table A10). Appendix Table A11 shows the predicted changes in employment for unmarried mothers for all CTC policy alternatives using this empirical specification with predicted transfer income. Aggregate employment is now predicted to drop by 4.37 percentage points if the ARPA CTC was in effect during 2019, as compared to 3.79 percentage points using the main RTW model (see Table 7).

## VII. CONCLUSIONS AND DIRECTIONS FOR FUTURE WORK

Using a panel data set that spans the history of the CTC, this paper updates estimates of how low and middle-income parents' employment responds to changes in incentives. We find that unmarried women and married parents (both sexes) are more likely to engage in paid work when the return to work increases, although the effect of this relationship is an order of magnitude smaller for married fathers (estimated elasticity of 0.07) than for unmarried and married mothers (estimated elasticities of 0.39 and 0.38, respectively). Married mothers are the most responsive to changes in nonlabor income, with a statistically significant estimated income elasticity of -0.13, while a smaller elasticity of -0.025 is estimated for unmarried mothers. No statistically significant income elasticity is estimated for married fathers, and the point estimate centers around zero. Together, these results are consistent with the notion that because of a spouse's income, married mothers – who are mostly secondary earners – are more able to choose to stay home with a child than unmarried mothers.

Unlike previous research, we test for the possibility that individuals respond differently to market wages than they do to economically equivalent economic incentives created by taxes, tax credits, and transfer programs. The point estimates in the separate components model suggest a smaller overall response to changes in tax liability than to labor income, but the difference is only statistically significant for married mothers. Based on those point estimates, the separate components model predicts smaller changes in employment in response to CTC policy reforms than using the RTW model.

Finally, we also build on previous research by testing for differences in parameters when our base models are further broken out by age of youngest child, education level, and race and ethnicity. These heterogeneity analyses reveal different employment responses to changes in the RTW and nonlabor income by the youngest child's age (with the largest response among married mothers with a child under age 2), educational attainment (largest response among less-educated unmarried mothers), and race-ethnicity (largest response among NH Black unmarried mothers and married fathers, and Hispanic married mothers).

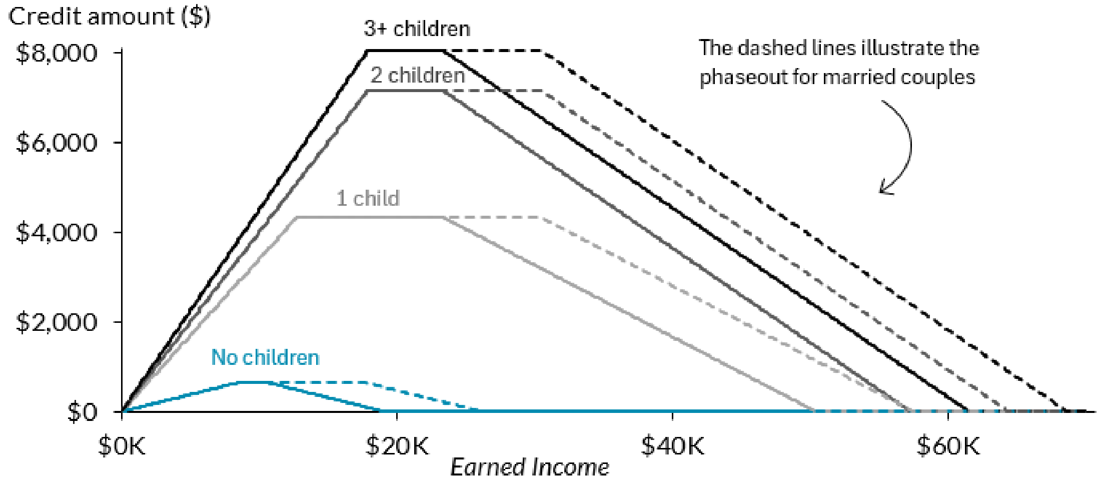
We used the estimated models to simulate the effect of eight different CTC policy options on parental employment. We estimate that aggregate employment would decline by between 1.03 to 1.70 percentage points, depending on model specification, if the ARPA CTC had been in effect during 2019 rather than current law (TCJA). We find that several other more targeted policy options could expand the benefits of the CTC with smaller employment effects. Removing the CTC refundability threshold, increasing the amount of the maximum refundable credit to match the amount of the maximum non-refundable credit, and making the refundable credit available on a per child basis, rather than per household, would have very little overall effect on parental employment and slightly increase employment of unmarried mothers. Alternatively, offering a fully refundable CTC for children under age 2 (i.e., ARPA CTC) while maintaining the TCJA CTC for children ages 3-16 would reduce overall employment by about 0.08 percentage points, with the largest response for unmarried mothers whose employment would fall by about 0.13 percentage points. Maintaining the structure of the TCJA CTC but increasing the maximum non-refundable credit to $5,000 would increase the employment of married mothers by about 0.24

percentage points, while making this $5,000 maximum credit refundable would increase the employment of unmarried mothers by about 0.66 percentage points.

Overall, these results provide updated estimates of parental employment elasticities for wider use by labor economists and policy analysts, as well as specific application of these elasticities in the case of CTC policy reform. With the current law CTC benefit schedule set to expire after 2025, these latter results are a timely input to inform the future of the CTC and its interaction with other existing income support policies.

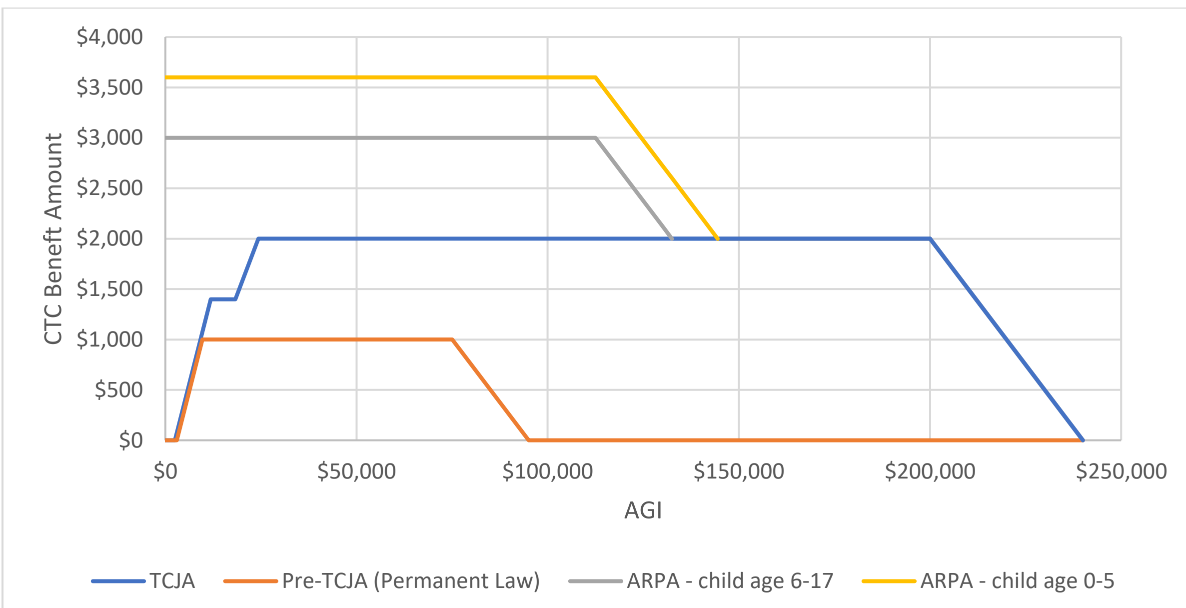
In future research, we plan to explore the wider time use and well-being implications of these findings for CTC recipients and their families. For example, for those parents that increase their labor supply in response to greater CTC benefit generosity, what types of activities do they substitute away from? Conversely, how long do spells out of the workforce persist for the parents that decrease their labor supply in response to greater CTC benefit generosity? Do these parents go on to spend more time with their children, or do they reallocate their time in other ways? These wider time use shifts are important to understand, as the welfare implications of exiting the work force for a limited time to engage in high social return activities such as investing in child development or the parent’s own human capital likely differ from simply consuming more leisure.

**Figure 1** EITC Amounts by Income and Number of Children, 2025



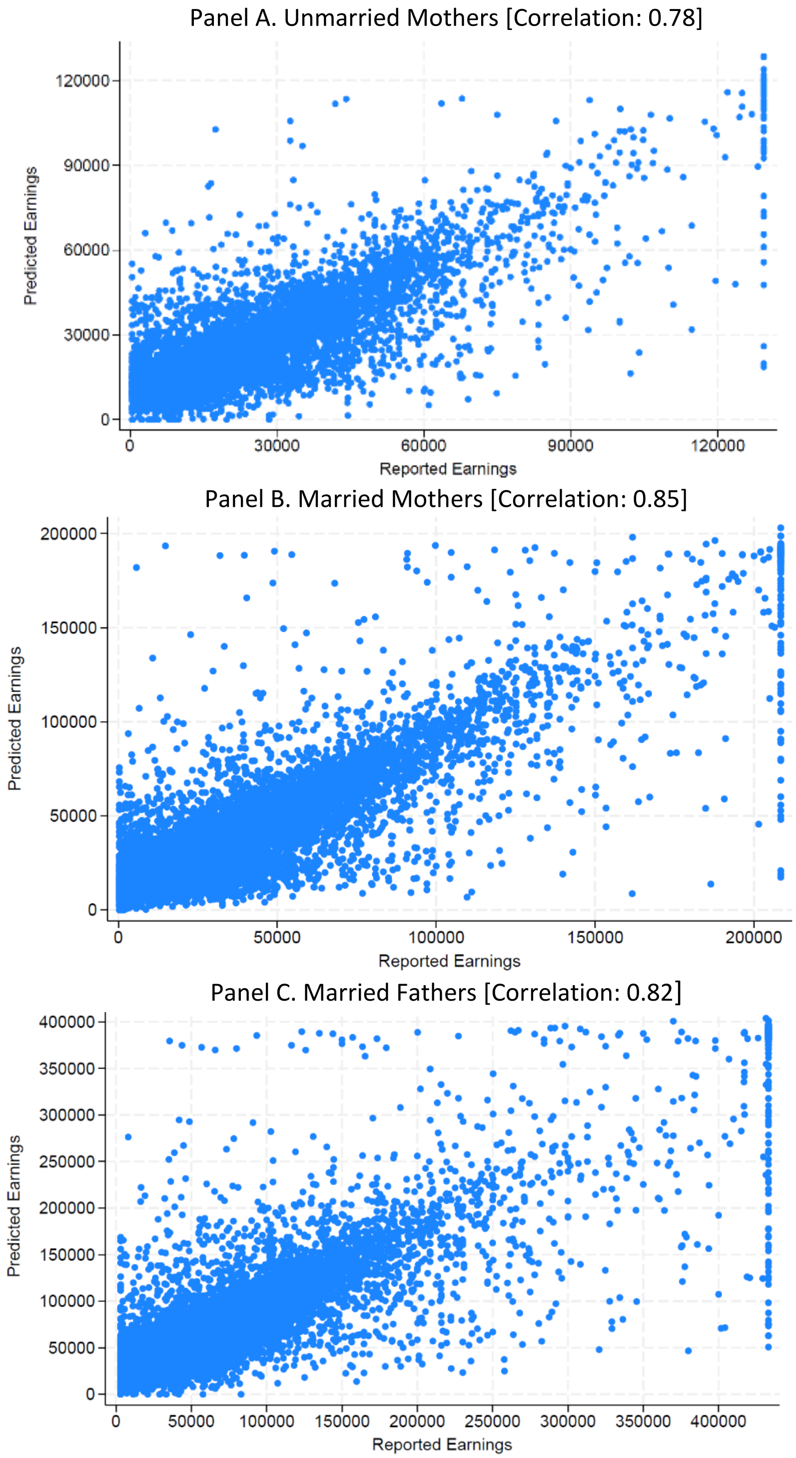

*Source*: Urban Institute.[34]
*Note*: The EITC phases out based on earned income or adjusted gross income, whichever is greater.

**Figure 2** CTC Benefit Schedule Examples (Unmarried Taxpayer with One Child)




*Source:* Author's calculations based on Crandall-Hollick (2022).
*Notes:* Assumes all income is earned income. The displayed "TCJA" schedule was effective for 2018. The maximum refundable portion of the TCJA CTC benefit is indexed for inflation for the remaining years that the TCJA CTC benefit schedule is in effect (2019-2020 and 2022-2025 tax years). "Pre-TCJA" schedule was effective for the 2009-2017 tax years and is set to return in 2026. "ARPA" benefit schedules were in place for 2021 tax year.

**Figure 3. Fitted Versus Actual Earnings, by Family Status**




*Source:* 1999-2019 PSID data.

## Table 1. Major Changes in the Child Tax Credit, 1998 - 2025

| Year | Maximum credit | Refundability (Additional Child Tax Credit) Rate | Threshold | Limit | Phase-out Threshold [1] |
|---|---|---|---|---|---|
| 1998 | $400 | N/A [2] | | | $75,000 for single and head of household (HOH) and $110,000 for married couples filing jointly (MFJ). |
| 1999 | $500 | | | | |
| 2000 | | | | | |
| 2001 | $600 | Up to 10 percent of earnings over | $10,000 | $600 | $75,000 for single and HOH and $110,000 for MFJ. |
| 2002 | | | $10,350 | | |
| 2003 | $1,000 | | $10,500 | $1,000 | |
| 2004 | | Up to 15 percent of earnings over | $10,750 | | |
| 2005 | | | $11,000 | | |
| 2006 | | | $11,300 | | |
| 2007 | | | $11,750 | | |
| 2008 | | | $8,500 | | |
| 2009 | $1,000 | Up to 15 percent of earnings over | $3,000 | $1,000 | $75,000 for single and HOH and $110,000 for MFJ. |
| 2010 | | | | | |
| 2011 | | | | | |
| 2012 | | | | | |
| 2013 | | | | | |
| 2014 | | | | | |
| 2015 | | | | | |
| 2016 | | | | | |
| 2017 | | | | | |
| 2018 | $2,000 | Up to 15 percent of earnings over | $2,500 | $1,400 [3] | $200,000 for single and HOH; $400,000 for MFJ |
| 2019 | | | | $1,400 | |
| 2020 | | | | $1,400 | |
| 2021 | $3,000 per child 6 - 17 / $3,600 per child 0 - 5 | Fully refundable [4] | | | TCJA thresholds in effect for basic credit. The ARPA increase phases out starting at $75,000 for singles, $112,500 for HOH, and $150,000 for MFJ |
| 2022 | $2,000 | Up to 15 percent of earnings over | $2,500 | $1,500 | Single: $200,000 ; Others: $400,000 |
| 2023 | | | | $1,600 | |
| 2024 | | | | $1,700 | |
| 2025 | | | | $1,700 | |

Source: Crandall-Hollick (2021) based on: IRS, Instructions for Form 1040, Form 2441, and Form 8812, various years; H.R. 1, American Recovery and Reinvestment Act of 2009; and IRS Revenue Procedure 2011-12.

Note : The child tax credit is available for qualifying children age 16 and under. ARPA increased the age limit to 17 for a single year, 2021.

[1] Credit phases out at a rate of 5% of modified AGI over these thresholds. Modified AGI for the child tax credit is AGI plus a) any amount excluded from income due to exclusion of income from Puerto Rico; b) foreign earned income; c) foreign earned income exclusion and d) exclusion of income for bona fide residents of American Samoa. The Modified AGI is equal to AGI for taxpayers without income from any of the above four sources. If the difference between the Modified AGI and the phase-out threshold is not a multiple of $1,000, it is increased to the next multiple of $1,000.

[2] Prior to 2001, the credit was nonrefundable for families with fewer than 3 children. The credit was partially refundable based on a complex formula for families with 3 or more children.

[3] The Tax Cuts and Jobs Act (TCJA) capped the refundable tax credit at $1,400, but indexed the maximum credit for inflation, in $100 increments.

[4] Full refundability allows low-income families to qualify for the maximum credit with no phase in or minimum earnings requirement.

**Table 2. Elasticities Used in Selected Analyses of the Effect of Making the 2021 ARPA CTC Permanent**

| | Substitution Elasticities | Income Elasticities | Change in Employment (Parents Exiting the Workforce) |
|---|---|---|---|
| National Academies of Sciences 2019 | N/A | -0.085 single mothers<br><br>-0.12 married mothers<br><br>-0.05 all other tax units | -149,000 |
| Corinth et al. 2021 | 0.75 low income single mothers<br><br>0.25 all other tax units | -0.085 low income single mothers<br><br>-0.05 all other tax units | -1,460,000 |
| Goldin et al. 2022 | 0.2 single mother and all fathers<br><br>0.3 married mothers | -0.085 single mothers<br><br>-0.12 married mothers<br><br>-0.05 all other tax units | -386,000 |
| Brill et al. 2021 | 0.25 all earners | -0.05 all earners | -296,000 |
| Bastian 2023 (preferred elasticities) | 0.4 low income single mothers<br><br>0.2 all other mothers<br><br>0.05 all other tax units | N/A | -354,000 |

*Source:* Adapted from Wielk et al. (2023).

**Table 3. Descriptive Statistics**

| | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **Number of Observations [weighted]** | 6,049 | 8,542 | 8,542 |
| **PANEL A. DEMOGRAPHIC AND STATE-YEAR CONTROL VARIABLES** | | | |
| **Number of CTC-eligible children** | 1.73 | 1.93 | |
| 1 | 51% | 35% | 35% |
| 2 | 33% | 44% | 44% |
| 3 | 12% | 16% | 16% |
| 4+ | 5% | 5% | 5% |
| **Age** | 35.99 | 36.99 | 38.94 |
| **Education** | | | |
| HS or less | 54% | 41% | 48% |
| Some College | 32% | 33% | 27% |
| 4-yr College Degree | 14% | 27% | 25% |
| **Race-Ethnicity** | | | |
| Non-Hispanic White | 39% | 72% | 70% |
| Non-Hispanic Black | 43% | 7% | 8% |
| Hispanic | 16% | 18% | 18% |
| Other/Don't Know | 2% | 3% | 3% |
| **Self-Rated Health** | | | |
| Excellent | 16% | 22% | 25% |
| Very Good | 36% | 40% | 41% |
| Good | 37% | 32% | 28% |
| Fair | 10% | 6% | 6% |
| **Minor Work-Limiting Disability** | 3% | 3% | 4% |
| **Has child under age 2** | 7% | 15% | |
| **Has child ages 2-5** | 28% | 44% | |

| | | |
|---|---|---|
| **Lives with adult family member** | 3% | 2% |
| **Lives with cohabitating partner**[35] | 3% | N/A |
| **Lives in metro area** | 76% | 64% |
| **State Characteristics** | | |
| Average minimum wage | $8.20 | $8.08 |
| Average max welfare benefits (family of 3) | $496 | $533 |
| Average unemployment rate | 5.92% | 5.71% |

**PANEL B. WORK AND TAX / INCOME VARIABLES**

| | **Unmarried Mothers** | **Married Mothers** | **Married Fathers** |
|---|---|---|---|
| **Employed** | 80% | 70% | 95% |
| **Earnings** | $29,055 | $23,821 | $58,762 |
| **Predicted Earnings (E*)** | $30,897 | $27,849 | $59,237 |
| **RTW** | $28,438 | $21,114 | $45,651 |
| Difference in Tax Liability (working versus not) | -$1,884 | $6,107 | $9,967 |
| Difference in SNAP benefits (working versus not) | -$4,343 | -$628 | -$3,619 |
| **CTC Benefits (estimated, if working)** | $1,222 | $1,683 | $1,716 |
| **Nonlabor Income** | $11,557 | $60,203 | $33,215 |
| Spousal earnings | N/A | $58,762 | $23,821 |
| Non-Wage Taxable Income | $454 | $2,191 | $2,191 |
| Lagged Transfer Income | $4,582 | $1,922 | $1,922 |
| Lagged Social Security Income | $836 | $156 | $156 |
| SNAP(0, $\Lambda$) | $5,685 | $780 | $3,772 |
| T(0,$\Omega$) | $0.20 | $3,609 | -$1,352 |

*Source:* 1999-2019 PSID data.
*Notes:* Sample is restricted to parents (of dependent children) ages 19 to 55 years old with less than $75,000 ($110,000 married) in predicted earnings who do not report a moderate to severe work-limiting disability or poor health, have non-missing demographic information, and were observed during the prior two PSID survey waves. All dollar amounts are real CPI-adjusted 2019 dollars. CTC benefits are calculated using TAXSIM based on predicted earnings, non-labor income, year, marital status, state, and number of own household children. Results are weighted using PSID sampling weights.

**Table 4. Employment Effects for RTW Base Model (All Variables)**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **PANEL A. AVERAGE MARGINAL EFFECTS** | | | |
| **RTW** | 0.0124*** | 0.0148*** | 0.0020*** |
| | (0.0012) | (0.0007) | (0.0003) |
| **Nonlabor income (Y)** | -0.0014* | -0.0011*** | -0.0001 |
| | (0.0006) | (0.0002) | (0.0001) |
| ***Control Variables*** | | | |
| **Income of other PSID family unit members** | 0.0005 | -0.0015 | 0.0003 |
| | (0.0007) | (0.0014) | (0.0005) |
| **Education** | | | |
| Some College | -0.0119 | 0.0319 | 0.0087 |
| | (0.0181) | (0.0169) | (0.0075) |
| 4-Year College Degree | -0.0319 | 0.0071 | -0.0052 |
| | (0.0360) | (0.0216) | (0.0139) |
| **Has child under age 2 (0/1)** | -0.0095 | -0.1099*** | -0.0093 |
| | (0.0340) | (0.0162) | (0.0079) |
| **Has child ages 2-5 (0/1)** | -0.0009 | -0.0717*** | -0.0034 |
| | (0.0216) | (0.0140) | (0.0067) |
| **Number of CTC Qualifying Children** | | | |
| Two | -0.0409* | -0.0061 | 0.0051 |
| | (0.0184) | (0.0151) | (0.0074) |
| Three | 0.0178 | -0.0148 | 0.0061 |
| | (0.0234) | (0.0205) | (0.0074) |
| Four | -0.0674 | -0.1039** | 0.0124 |
| | (0.0396) | (0.0341) | (0.0173) |
| **Race-Ethnicity** | | | |
| NH Black | -0.0533* | -0.0192 | -0.0463** |
| | (0.0251) | (0.0266) | (0.0155) |
| Hispanic | 0.0455 | -0.0482 | -0.0062 |
| | (0.0282) | (0.0248) | (0.0096) |

| | | | |
|---|---|---|---|
| Other/Don't Know | 0.0480 | -0.0971* | -0.0827* |
| | (0.0541) | (0.0440) | (0.0379) |
| **Age** | -0.0107 | 0.0101 | 0.0030 |
| | (0.0114) | (0.0098) | (0.0039) |
| **Age$^2$** | 0.0002 | -0.0001 | -0.0000 |
| | (0.0002) | (0.0001) | (0.0001) |
| **Adult Family Member (0/1)** | 0.0293 | 0.0327 | -0.0251 |
| | (0.0313) | (0.0353) | (0.0195) |
| **Mild work-limiting disability (0/1)** | 0.0271 | -0.0124 | -0.0195 |
| | (0.0313) | (0.0307) | (0.0158) |
| **Self-Reported Health** | | | |
| Very Good | -0.0129 | -0.0020 | -0.0023 |
| | (0.0215) | (0.0162) | (0.0071) |
| Good | -0.0278 | 0.0066 | -0.0188* |
| | (0.0220) | (0.0180) | (0.0078) |
| Fair | -0.0199 | -0.0114 | -0.0136 |
| | (0.0313) | (0.0293) | (0.0117) |
| **Lives in metro area (0/1)** | -0.0126 | -0.0355* | -0.0007 |
| | (0.0206) | (0.0166) | (0.0078) |
| **State Minimum Wage** | 0.0044 | -0.0082 | 0.0048 |
| | (0.0120) | (0.0086) | (0.0048) |
| **Max Welfare Benefits** | 0.0003 | -0.0002 | -0.0001 |
| **For a Family of 3** | (0.0002) | (0.0002) | (0.0001) |
| **State Unemployment Rate** | 0.0031 | 0.0121 | -0.0060 |
| | (0.0089) | (0.0080) | (0.0037) |
| **PANEL B. ELASTICITIES** | | | |
| **RTW** | 0.394*** | 0.383*** | 0.072*** |
| | (0.039) | (0.021) | (0.011) |
| **Nonlabor income (Y)** | -0.025* | -0.132*** | -0.004 |
| | (0.011) | (0.030) | (0.004) |
| **Baseline Employment** | 0.80 | 0.70 | 0.95 |
| N[a] | 5,985 | 8,510 | 8,440 |
| **Pseudo-R$^2$** | 0.170 | 0.200 | 0.151 |
| **Log Likelihood** | -26,112 | -72,416 | -24,546 |

* p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.
*Source*: 1999-2019 PSID data.
*Notes*: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Elasticities are calculated using margins(eyex) command in Stata. Model controls for state and year fixed effects (not shown). See Appendix Table A2 for logit coefficient estimates for all model variables. There are a few excluded categories in the following categorical variables: Education (High School or Less (excluded), Some College, Bachelor's Degree or More), Race (NH White (excluded), NH Black, Hispanic, Other/Don't Know), Number of CTC-eligible children (1 (excluded), 2, 3, 4+), and Self-reported Health (Excellent (excluded), Very Good, Good, Fair). Results are weighted using PSID individual weights.

**Table 5: Employment Effects for Separate Components Model (Key Variables; Average Marginal Effects)**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **RTW** | | | |
| *E** | 0.0100*** | 0.0131*** | 0.0019*** |
| | (0.0019) | (0.0009) | (0.0003) |
| *Taxes* | -0.0080 | -0.0066* | -0.0012 |
| | (0.0047) | (0.0032) | (0.0009) |
| *SNAP* | -0.0035 | 0.0056 | 0.0031 |
| | (0.0097) | (0.0055) | (0.0019) |
| **Nonlabor Income** | | | |
| *Spouse* | | -0.0012*** | -0.0003 |
| | | (0.0003) | (0.0002) |
| *Other* | -0.0013* | -0.0012 | -0.0001 |
| | (0.0006) | (0.0006) | (0.0001) |
| **Baseline Employment** | 0.80 | 0.70 | 0.95 |
| N[a] | 5,985 | 8,510 | 8,440 |
| **Pseudo-$R^2$** | 0.171 | 0.201 | 0.152 |
| **Log Likelihood** | -26,081 | -72,312 | -24,508 |

p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Note:* Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Results are weighted using PSID individual weights.

**Table 6. RTW, Tax Liability, and Non-Labor Income Under Current Law (TCJA) and the ARPA CTC Policy Alternative, 2019**

| | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **PANEL A: RTW Model** | | | |
| *RTW* | | | |
| Current Law (TCJA) | $29,149 | $21,130 | $40,368 |
| ARPA | $26,570 | $20,708 | $38,212 |
| | | | |
| *Y* (Nonlabor Income) | | | |
| Current Law (TCJA) | $9,661 | $57,110 | $32,178 |
| ARPA | $15,247 | $59,887 | $36,712 |
| **PANEL B: Separate Components Model** | | | |
| *Taxes* | | | |
| Current Law (TCJA) | -$3,381 | $5,037 | $5,351 |
| ARPA | -$801 | $5,459 | $7,507 |
| | | | |
| *Other* (Nonlabor Income) | | | |
| Current Law (TCJA) | $9,661 | $7,306 | $12,384 |
| ARPA | $15,247 | $10,083 | $16,918 |
| Baseline Employment | 81.43% | 61.12% | 92.03% |
| N (unweighted) | 626 | 632 | 632 |

*Source:* 1999-2019 PSID data.
*Notes:* Estimates are weighted using PSID individual weights.

**Table 7. Predicted percentage point change in employment relative to TCJA baseline for CTC policy options, 2019**

| PANEL A: RTW MODEL | Aggregate Effect | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|---|
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| (1) No refundability threshold | -0.01 | 0.14 | -0.07 | -0.03 |
| (2) Per child refundability | 0.09 | 0.68 | -0.11 | -0.07 |
| (3) 2K refundable max | -0.04 | 0.08 | -0.13 | -0.03 |
| (1) + (2) + (3) | 0.11 | 1.34 | -0.38 | -0.15 |
| (5) ARPA CTC | -1.50 | -3.79 | -0.96 | -0.64 |
| (6) ARPA CTC for children under age 2 | -0.12 | -0.17 | -0.13 | -0.08 |
| (7) 5K CTC non-refundable | 0.26 | 0.04 | 0.54 | 0.11 |
| (8) 5K CTC refundable | 0.29 | 0.99 | 0.05 | 0.09 |

| PANEL B: SEPARATE COMPONENTS MODEL | Aggregate Effect | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|---|
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| (1) No refundability threshold | 0.00 | 0.10 | -0.03 | -0.02 |
| (2) Per child refundability | 0.07 | 0.46 | -0.05 | -0.04 |
| (3) 2K refundable max | -0.01 | 0.05 | -0.06 | -0.01 |
| (1) + (2) + (3) | 0.11 | 0.91 | -0.19 | -0.08 |
| (5) ARPA CTC | -1.03 | -2.65 | -0.67 | -0.39 |
| (6) ARPA CTC for children under age 2 | -0.08 | -0.13 | -0.08 | -0.04 |
| (7) 5K CTC non-refundable | 0.13 | 0.04 | 0.24 | 0.07 |
| (8) 5K CTC refundable | 0.16 | 0.66 | -0.05 | 0.06 |
| | | | | |
| Weights (% of weighted PSID individuals in each group in 2019) | 100% | 23.56% | 38.22% | 38.22% |

*Source:* 1999-2019 PSID data.
*Notes:* Predictions sample includes those observed during 2019, while model parameters are estimated using the full sample.. Estimates are weighted using PSID individual weights.

**Table 8. Employment Effects by Age of Youngest Child, RTW Model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **PANEL A: AVERAGE MARGINAL EFFECTS** | | | |
| ***RTW*** | | | |
| Under 2 | 0.0142*** | 0.0148*** | 0.0023*** |
| | (0.0034) | (0.0012) | (0.0005) |
| 2-5 | 0.0148*** | 0.0151*** | 0.0022*** |
| | (0.0020) | (0.0011) | (0.0004) |
| 6 and Older | 0.0114*** | 0.0152*** | 0.0018*** |
| | (0.0013) | (0.0010) | (0.0004) |
| ***Y* (Nonlabor Income)** | | | |
| Under 2 | 0.0011 | -0.0017** | -0.0001 |
| | (0.0063) | (0.0005) | (0.0001) |
| 2-5 | -0.0026 | -0.0018*** | -0.0003** |
| | (0.0021) | (0.0004) | (0.0001) |
| 6 and Older | -0.0012* | -0.0007**[b] | -0.0000 |
| | (0.0006) | (0.0002) | (0.0001) |
| **PANEL B: ELASTICITIES** | | | |
| ***RTW*** | | | |
| Under 2 | 0.4516*** | 0.4755*** | 0.0861*** |
| | (0.1082) | (0.0432) | (0.0177) |
| 2-5 | 0.4571*** | 0.4036*** | 0.0788*** |
| | (0.0677) | (0.0298) | (0.0133) |
| 6 and Older | 0.3663*** | 0.3474***[a] | 0.0641*** |
| | (0.0427) | (0.0253) | (0.0129) |
| ***Y* (Nonlabor Income)** | | | |
| Under 2 | 0.0217 | -0.2588** | -0.0030 |
| | (0.1225) | (0.0880) | (0.0059) |
| 2-5 | -0.0491 | -0.2172*** | -0.0140* |
| | (0.0410) | (0.0501) | (0.0057) |
| 6 and Older | -0.0206 | -0.0781**[ab] | -0.0009 |
| | (0.0106) | (0.0281) | (0.0048) |

| | | | |
|---|---|---|---|
| **Baseline Employment** | | | |
| Under 2 | 0.65 | 0.54 | 0.95 |
| 2-5 | 0.76 | 0.66 | 0.95 |
| 6 and Older | 0.83 | 0.76 | 0.95 |
| **N**[c] | 5,920 | 8,510 | 8,440 |
| **Pseudo-$R^2$** | 0.178 | 0.203 | 0.153 |
| **Log Likelihood** | -25,672 | -72,160 | -24,487 |

** p<0.05, ** p<0.01, *** p<0.001*

[a] Different from "Under 2" at *p<0.05*

[b] Different from "2-5" at *p<0.05*

[c] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Notes*: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Elasticities are calculated using margins(eyex) command in Stata. Results are weighted using PSID individual weights.

**Table 9. Employment Effects by Education Level, RTW model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **PANEL A: AVERAGE MARGINAL EFFECTS** | | | |
| ***RTW*** | | | |
| High School or Less | 0.0156*** | 0.0158*** | 0.0026*** |
| | (0.0017) | (0.0009) | (0.0004) |
| Some College | 0.0098***[a] | 0.0137***[a] | 0.0015*** |
| | (0.0016) | (0.0007) | (0.0004) |
| Bachelor's Degree or More | 0.0064***[a] | 0.0146*** | 0.0014* |
| | (0.0017) | (0.0009) | (0.0006) |
| ***Y* (Nonlabor Income)** | | | |
| High School or Less | -0.0061** | -0.0010*** | -0.0003 |
| | (0.0020) | (0.0003) | (0.0003) |
| Some College | 0.0005[a] | -0.0011*** | -0.0000 |
| | (0.0014) | (0.0002) | (0.0001) |
| Bachelor's Degree or More | 0.0044*[a] | -0.0012*** | -0.0000 |
| | (0.0019) | (0.0003) | (0.0001) |
| **PANEL B: ELASTICITIES** | | | |
| ***RTW*** | | | |
| High School or Less | 0.4857*** | 0.3912*** | 0.0830*** |
| | (0.0556) | (0.0248) | (0.0132) |
| Some College | 0.3173***[a] | 0.3622***[a] | 0.0574*** |
| | (0.0529) | (0.0201) | (0.0138) |
| Bachelor's Degree or More | 0.2629***[a] | 0.4001***[b] | 0.0675** |
| | (0.0716) | (0.0281) | (0.0256) |
| ***Y* (Nonlabor Income)** | | | |
| High School or Less | -0.1215** | -0.1046*** | -0.0135 |
| | (0.0450) | (0.0290) | (0.0107) |
| Some College | 0.0074[a] | -0.1335*** | -0.0014 |
| | (0.0209) | (0.0295) | (0.0048) |
| Bachelor's Degree or More | 0.0635**[a] | -0.1856***[a] | -0.0026 |
| | (0.0240) | (0.0468) | (0.0039) |

| | | | |
|---|---|---|---|
| **Baseline Employment** | | | |
| High School or Less | 0.74 | 0.64 | 0.94 |
| Some College | 0.85 | 0.72 | 0.96 |
| Bachelor's Degree or More | 0.91 | 0.76 | 0.97 |
| **N**[c] | 5,985 | 8,510 | 8,440 |
| **Pseudo-$R^2$** | 0.178 | 0.201 | 0.151 |
| **Log Likelihood** | -25,864 | -72,331 | -24,531 |

* p<0.05, ** p<0.01, *** p<0.001

[a] Different from "High School or Less" at *p<0.05*

[b] Different from "Some College" at *p<0.05*

[c] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Notes*: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Elasticities are calculated using margins(eyex) command in Stata. Results are weighted using PSID individual weights.

**Table 10. Employment Effects by Race-Ethnicity, RTW model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **PANEL A: AVERAGE MARGINAL EFFECTS** | | | |
| ***RTW*** | | | |
| NH White | 0.0091*** | 0.0136*** | 0.0015*** |
| | (0.0015) | (0.0008) | (0.0003) |
| NH Black | 0.0145***[a] | 0.0097***[a] | 0.0046***[a] |
| | (0.0016) | (0.0016) | (0.0010) |
| Hispanic | 0.0143*** | 0.0223***[ab] | 0.0026*** |
| | (0.0028) | (0.0018) | (0.0006) |
| ***Y*** **(Nonlabor Income)** | | | |
| NH White | 0.0007 | -0.0009*** | -0.0000 |
| | (0.0012) | (0.0002) | (0.0001) |
| NH Black | -0.0065**[a] | -0.0004 | -0.0003 |
| | (0.0019) | (0.0007) | (0.0008) |
| Hispanic | -0.0002 | -0.0020*** | -0.0005 |
| | (0.0029) | (0.0006) | (0.0004) |
| **PANEL B: ELASTICITIES** | | | |
| ***RTW*** | | | |
| NH White | 0.2885*** | 0.3381*** | 0.0583*** |
| | (0.0488) | (0.0219) | (0.0106) |
| NH Black | 0.4746***[a] | 0.2696*** | 0.1559***[a] |
| | (0.0546) | (0.0474) | (0.0335) |
| Hispanic | 0.4404*** | 0.6149***[ab] | 0.0824***[b] |
| | (0.0994) | (0.0683) | (0.0182) |
| ***Y*** **(Nonlabor Income)** | | | |
| NH White | 0.0117 | -0.1144*** | -0.0018 |
| | (0.0212) | (0.0328) | (0.0028) |
| NH Black | -0.1303**[a] | -0.0264 | -0.0145 |
| | (0.0428) | (0.0543) | (0.0408) |
| Hispanic | -0.0035[b] | -0.2484**[b] | -0.0173 |
| | (0.0429) | (0.0768) | (0.0156) |

| | | | |
|---|---|---|---|
| **Baseline Employment** | | | |
| NH White | 0.86 | 0.72 | 0.97 |
| NH Black | 0.73 | 0.78 | 0.90 |
| Hispanic | 0.83 | 0.56 | 0.93 |
| **N[c]** | 5,886 | 8,284 | 8,199 |
| **Pseudo-$R^2$** | 0.174 | 0.201 | 0.149 |
| **Log Likelihood** | -25,455 | -70,066 | -22,869 |

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

[a] Different from "NH White" at *p<0.05*

[b] Different from "NH Black" at *p<0.05*

[c] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Notes*: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Elasticities are calculated using margins(eyex) command in Stata. Results are weighted using PSID individual weights.

**Table 11. Predicted percentage point change in employment relative to TCJA baseline and base models for selected CTC policy options, 2019**

| **PANEL A: RTW MODEL** | **Aggregate Effect** | **Unmarried Mothers** | **Married Mothers** | **Married Fathers** |
|---|---|---|---|---|
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| ***ARPA CTC, Base Model*** | ***-1.50*** | ***-3.79*** | ***-0.96*** | ***-0.64*** |
| ARPA CTC, By Youngest Child Age Model | -1.52 | -3.70 | -1.02 | -0.67 |
| ARPA CTC, By Education Model | -1.70 | -4.55 | -0.95 | -0.69 |
| ARPA CTC, By Race-Ethnicity Model | -1.68 | -4.44 | -0.94 | -0.63 |
| ***ARPA CTC for children under age 2, Base Model*** | ***-0.12*** | ***-0.17*** | ***-0.13*** | ***-0.08*** |
| ARPA CTC for children under age 2, By Youngest Child Age Model | -0.09 | -0.04 | -0.14 | -0.08 |
| ARPA CTC for children under age 2, By Education Model | -0.16 | -0.31 | -0.14 | -0.08 |
| ARPA CTC for children under age 2, By Race-Ethnicity Model | -0.13 | -0.25 | -0.13 | -0.06 |
| | | | | |
| **PANEL B: SEPARATE COMPONENTS MODEL** | **Aggregate Effect** | **Unmarried Mothers** | **Married Mothers** | **Married Fathers** |
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| ***ARPA CTC, Base Model*** | ***-1.03*** | ***-2.65*** | ***-0.67*** | ***-0.39*** |
| ARPA CTC, By Youngest Child Age Model | -1.05 | -2.76 | -0.68 | -0.37 |
| ARPA CTC, By Education Model | -1.20 | -3.03 | -0.67 | -0.49 |
| ARPA CTC, By Race-Ethnicity Model | -1.08 | -2.72 | -0.58 | -0.43 |
| ***ARPA CTC for children under age 2, Base Model*** | ***-0.08*** | ***-0.13*** | ***-0.08*** | ***-0.04*** |
| ARPA CTC for children under age 2, By Youngest Child Age Model | -0.02 | 0.06 | -0.08 | -0.01 |
| ARPA CTC for children under age 2, By Education Model | -0.12 | -0.28 | -0.08 | -0.05 |
| ARPA CTC for children under age 2, By Race-Ethnicity Model | -0.09 | -0.20 | -0.06 | -0.04 |
| | | | | |
| Weights (% of weighted PSID individuals in each group in 2019)[a] | 100% | 23.56% | 38.22% | 38.22% |

[a] Weights slightly differ for the by race-ethnicity model, given that those in the “Other/Don’t Know” category are dropped for this analysis (24.48% weight for unmarried mothers, 37.76% weights for married parents).

*Source:* 1999-2019 PSID data.

*Notes:* Predictions sample includes those observed during 2019, while model parameters are estimated using the full sample.. Estimates are weighted using PSID individual weights.

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## APPENDIX

### Table A1. Predicted Labor Income, Full Regression Results

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **Lagged labor income** | 0.722*** | 0.843*** | 0.832*** |
| | (0.028) | (0.014) | (0.018) |
| **Spousal earnings** | -- | 0.003 | 0.003 |
| | | (0.005) | (0.015) |
| **Non-wage income subject to tax** | 0.013+ | -0.030+ | -0.024 |
| | (0.007) | (0.017) | (0.024) |
| **Transfer income** | -0.032 | -0.014 | -0.063+ |
| | (0.025) | (0.008) | (0.034) |
| **Income subject to tax, other family members** | 0.061* | -0.012 | 0.049 |
| | (0.026) | (0.028) | (0.049) |
| **Transfer income, other family members** | -0.094 | -0.024 | 0.227 |
| | (0.105) | (0.130) | (0.285) |
| **Household Social Security Income** | -0.106 | 0.005 | -0.071 |
| | (0.095) | (0.119) | (0.143) |
| **Self-employment income (0/1)** | -651.87 | -993.55* | 571.17 |
| | (1,511.05) | (425.01) | (922.57) |
| **Farm Income (0/1)** | - | -2,511.16 | 11,737.15+ |
| | | (2,304.50) | (7,086.69) |
| **Child support payments (0/1)** | -431.17 | 2,613.70** | -2,170.81 |
| | (704.23) | (948.63) | (1,712.48) |
| **Age** | 589.71 | 491.13 | 1,635.15** |
| | (429.26) | (386.38) | (531.63) |
| **$Age^2$** | -6.64 | -5.80 | -23.12** |
| | (5.93) | (5.06) | (6.79) |
| **Race-Ethnicity** | | | |
| NH Black | -3,186.97*** | 156.15 | -7,072.98*** |
| | (792.96) | (728.08) | (1,080.37) |

| | | | |
|---|---|---|---|
| Hispanic | -3,577.67** | -1,426.78 | -5,819.93*** |
| | (1,209.06) | (915.50) | (1,381.06) |
| Other/Don't Know | -5,185.28* | 5,090.42** | -779.43 |
| | (2,106.58) | (1,694.15) | (2,655.58) |
| **Education** | | | |
| Some College | 2,418.57** | 936.54+ | 3,622.64*** |
| | (695.61) | (521.47) | (919.10) |
| 4-Year College Degree | 10,575.25*** | 5,371.21*** | 12,680.17*** |
| | (1,533.23) | (638.01) | (1,170.10) |
| **Has child under age 2 (0/1)** | -2,236.57* | -2,261.60* | -2,055.96+ |
| | (963.71) | (941.34) | (1,166.37) |
| **Has child ages 2-5 (0/1)** | -226.40 | 439.12 | 1,053.00 |
| | (733.88) | (628.10) | (993.17) |
| **Number of CTC-eligible children** | | | |
| 2 | 1,363.90+ | -228.23 | -162.53 |
| | (711.00) | (557.97) | (1,056.49) |
| 3 | -1,248.15 | -930.86 | 1,498.12 |
| | (937.62) | (811.50) | (1,471.40) |
| 4 | -2,142.63 | -1,292.78 | -48.88 |
| | (1,316.69) | (1,343.08) | (2,350.85) |
| **Adult Family Member (0/1)** | -1,639.30 | 762.18 | -1,226.12 |
| | (1,560.58) | (1,598.08) | (1,608.76) |
| **Co-habitating Partner (0/1)** | -1,871.76 | | |
| | (1,995.65) | | |
| **Mild work-limiting disability (0/1)** | -3,252.78* | -929.57+ | -3,979.19+ |
| | (1,622.64) | (1,215.98) | (2,358.53) |
| **Lives in metro area (0/1)** | 1,480.11* | 713.51 | 4,227.78*** |
| | (686.09) | (583.88) | (989.75) |
| **Self-Reported Health** | | | |
| Very Good | 570.20 | -1264.24+ | -1,257.78 |
| | (897.29) | (707.92) | (1,164.37) |
| Good | -1,508.67+ | -2,040.19** | -1,965.03 |
| | (885.23) | (751.56) | (1,239.28) |

| | | | |
|---|---|---|---|
| Fair | -4,095.02*** | -3,205.35** | -3,127.77+ |
| | (1,085.13) | (1,163.24) | (1,601.50) |
| **State Minimum Wage** | -812.08+ | 202.03 | -282.15 |
| | (433.14) | (349.16) | (601.68) |
| **Max Welfare Benefits** | 3.83 | -12.71+ | 8.52 |
| | (8.55) | (6.71) | (10.26) |
| **State Unemployment Rate** | 178.97 | 134.14 | 422.27 |
| | (403.86) | (352.82) | (566.95) |
| **Zero prior wave earnings (0/1)** | 8,722.49*** | 6,859.91*** | 26,955.78*** |
| | (1,781.04) | (1,051.48) | (6,741.75) |
| **N**[a] | 5,485 | 9,973 | 12,105 |
| **$R^2$** | 0.7086 | 0.7421 | 0.7041 |

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations differs from the total sample for each group of parents, as regression sample only includes those with labor income and is not restricted to those making less than $75,000 ($110,000 married) in nominal dollars. Labor income is then predicted for the full sample based on these regression results, and the nominal amount of this prediction is used to restrict the final sample to those making less than $75,000 ($110,000 married).

*Source*: 1999-2019 PSID data.

*Note*: OLS regression with robust standard errors in parentheses, clustered at individual level. Dollar amounts scaled to $2019. Model controls for state and year fixed effects (not shown). There are a few excluded categories in the following categorical variables: Education (High School or Less (excluded), Some College, Bachelor's Degree or More), Race (NH White (excluded), NH Black, Hispanic, Other/Don't Know), Number of CTC-eligible children (1 (excluded), 2, 3, 4+), and Self-reported Health (Excellent (excluded), Very Good, Good, Fair). Results are weighted using PSID individual weights.

**Table A2. Logit Coefficient Estimates, RTW Base Model (All Variables)**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **RTW** | 0.092*** | 0.090*** | 0.046*** |
| | (0.009) | (0.005) | (0.006) |
| **Nonlabor income (Y)** | -0.010* | -0.007*** | -0.002 |
| | (0.004) | (0.001) | (0.002) |
| **Income of other family members** | 0.004 | -0.009 | 0.007 |
| | (0.005) | (0.008) | (0.013) |
| **Education** | | | |
| Some College | 0. 091 | 0.195+ | 0.212 |
| | (0.138) | (0.103) | (0.188) |
| 4-Year College Degree | -0. 227 | 0.042 | -0.111 |
| | (0.245) | (0.130) | (0.290) |
| **Has child under age 2 (0/1)** | -0. 070 | -0.634*** | -0.203 |
| | (0.247) | (0.090) | (0.163) |
| **Has child ages 2-5 (0/1)** | -0. 007 | -0.430*** | -0.078 |
| | (0.161) | (0.084) | (0.152) |
| **Number of CTC-Eligible Children** | | | |
| 2 | -0.298* | 0.037 | 0.121 |
| | (0.133) | (0.092) | (0.173) |
| 3 | 0.143 | -0.089 | -0.131 |
| | (0.192) | (0.123) | (0.236) |
| 4 | -0.474+ | -0.597** | -0.256 |
| | (0.258) | (0.191) | (0.335) |
| **Race-Ethnicity** | | | |
| NH Black | -0.381* | 0.120 | -0.876*** |
| | (0.183) | (0.168) | (0.240) |
| Hispanic | 0.390 | -0.286* | -0.158 |
| | (0.249) | (0.145) | (0.237) |
| Other/Don’t Know | 0.413 | -0.563* | -1.315** |
| | (0.512) | (0.246) | (0.417) |

| | | | |
|---|---|---|---|
| **Age** | -0.080 | 0.062 | 0.068 |
| | (0.085) | (0.059) | (0.091) |
| **Age$^2$** | 0.001 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) |
| **Adult Family Member (0/1)** | 0.229 | 0.204 | -0.484 |
| | (0.257) | (0.226) | (0.323) |
| **Self-Reported Health** | | | |
| Very Good | -0.100 | -0.012 | -0.061 |
| | (0.169) | (0.099) | (0.188) |
| Good | -0.210 | 0.040 | -0.425* |
| | (0.170) | (0.110) | (0.183) |
| Fair | -0.153 | -0.069 | -0.319 |
| | (0.239) | (0.176) | (0.264) |
| **Mild work-limiting disability (0/1)** | 0.203 | -0.076 | -0.391 |
| | (0.288) | (0.187) | (0.279) |
| **Lives in metro area (0/1)** | -0.095 | -0.217* | 0.015 |
| | (0.157) | (0.102) | (0.179) |
| **State Minimum Wage** | 0.033 | -0.050 | 0.111 |
| | (0.089) | (0.052) | (0.111) |
| **Max Welfare Benefits For a Family of 3** | 0.002 | -0.001 | -0.002 |
| | (0.002) | (0.001) | (0.002) |
| **State Unemployment Rate** | 0.023 | 0.074 | -0.137 |
| | (0.067) | (0.049) | (0.085) |
| **N[a]** | 5,985 | 8,510 | 8,440 |
| **Pseudo-$R^2$** | 0.1697 | 0.1997 | 0.1505 |
| **Log Likelihood** | -26,112 | -72,416 | -24,546 |

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source*: 1999-2019 PSID data.

*Note*: Standard errors in parentheses and clustered at the individual level. Logit coefficient estimates correspond to the average marginal effects and elasticities reported in Table 5. Dollar amounts scaled to thousands of $2019. Model controls for state and year fixed effects (not shown). There are a few excluded categories in the following categorical variables: Education (High School or Less (excluded), Some College, Bachelor's Degree or More), Race (NH White (excluded), NH Black, Hispanic, Other/Don't Know), Number of CTC-eligible children (1 (excluded), 2, 3, 4+), and Self-reported Health (Excellent (excluded), Very Good, Good, Fair). Results are weighted using PSID individual weights.

**Table A3. 2019 Tax Year Changes in RTW, Tax Liability, and Non-Labor Income: CTC Policy Alternatives**

**PANEL A: *RTW***

| | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| Current Law (TCJA) | $29,149 | $21,130 | $40,368 |
| (1) + (2) + (3) | $29,923 | $20,908 | $39,866 |
| ARPA CTC | $26,570 | $20,708 | $38,212 |
| ARPA CTC for children under age 2 | $29,057 | $21,063 | $40,141 |
| 5K CTC non-refundable | $29,361 | $21,580 | $40,802 |
| 5K CTC refundable | $30,398 | $21,215 | $40,732 |
| N (unweighted) | 626 | 632 | 632 |

**PANEL B: *Y* (Nonlabor Income; RTW Model)**

| | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| Current Law (TCJA) | $9,661 | $57,110 | $32,178 |
| (1) + (2) + (3) | $9,661 | $57,350 | $32,723 |
| ARPA CTC | $15,247 | $59,887 | $36,713 |
| ARPA CTC for children under age 2 | $9,991 | $57,361 | $32,609 |
| 5K CTC non-refundable | $9,661 | $57,213 | $32,209 |
| 5K CTC refundable | $9,661 | $57,991 | $32,763 |
| N (unweighted) | 626 | 632 | 655 |

*Source:* 1999-2019 PSID data.
*Notes:* Estimates are weighted using PSID individual weights.

**Table A4. Employment Effects by Age of Youngest Child , Separate Components Model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **Separate Components Marginal Effects** | | | |
| ***E**** | | | |
| Under 2 | 0.0012 | 0.0131*** | 0.0018** |
| | (0.0061) | (0.0018) | (0.0006) |
| 2-5 | 0.0159*** | 0.0136*** | 0.0018*** |
| | (0.0045) | (0.0015) | (0.0004) |
| 6 and Older | 0.0083*** | 0.0132*** | 0.0019*** |
| | (0.0020) | (0.0014) | (0.0005) |
| ***Taxes*** | | | |
| Under 2 | -0.0068 | -0.0065 | -0.0001 |
| | (0.0147) | (0.0064) | (0.0019) |
| 2-5 | -0.0181+ | -0.0078 | -0.0004 |
| | (0.0096) | (0.0056) | (0.0013) |
| 6 and Older | -0.0050 | -0.0056 | -0.0020 |
| | (0.0053) | (0.0046) | (0.0014) |
| ***SNAP*** | | | |
| Under 2 | -0.0604+ | 0.0107 | -0.0000 |
| | (0.0360) | (0.0121) | (0.0038) |
| 2-5 | 0.0195 | 0.0079 | -0.0015 |
| | (0.0227) | (0.0095) | (0.0026) |
| 6 and Older | -0.0096 | 0.0025 | 0.0079** |
| | (0.0103) | (0.0076) | (0.0029) |
| ***Spouse*** | | | |
| Under 2 | | -0.0019** | -0.0003 |
| | | (0.0006) | (0.0003) |
| 2-5 | | -0.0019*** | -0.0003 |
| | | (0.0004) | (0.0003) |
| 6 and Older | | -0.0007* | -0.0005 |
| | | (0.0003) | (0.0003) |

| | | | |
|---|---|---|---|
| ***Other*** | | | |
| Under 2 | 0.0022 | -0.0017 | -0.0000 |
| | (0.0061) | (0.0012) | (0.0002) |
| 2-5 | -0.0026 | -0.0017 | -0.0003** |
| | (0.0020) | (0.0007) | (0.0001) |
| 6 and Older | -0.0011* | -0.0010 | 0.0000 |
| | (0.0005) | (0.0007) | (0.0001) |
| **Na** | 5,985 | 8,510 | 8,440 |
| **Pseudo-R2** | 0.1729 | 0.2041 | 0.1573 |
| **Log Likelihood** | -26,010 | -72,015 | -24,350 |

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Notes*: Standard errors in parentheses and clustered at the individual level. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Results are weighted using PSID individual weights.

**Table A5. Employment Effects by Education Level, Separate Components model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| ***E**** | | | |
| High School or Less | 0.0122*** | 0.0159*** | 0.0023*** |
| | (0.0030) | (0.0015) | (0.0005) |
| Some College | 0.0053* | 0.0101*** | 0.0012* |
| | (0.0026) | (0.0017) | (0.0005) |
| Bachelor's Degree or More | 0.0091* | 0.0124*** | 0.0007 |
| | (0.0037) | (0.0016) | (0.0005) |
| ***Taxes*** | | | |
| High School or Less | -0.0079 | -0.0075 | -0.0022 |
| | (0.0063) | (0.0050) | (0.0015) |
| Some College | 0.0050 | 0.0007 | 0.0004 |
| | (0.0065) | (0.0059) | (0.0014) |
| Bachelor's Degree or More | -0.0165 | -0.0114* | 0.0013 |
| | (0.0109) | (0.0058) | (0.0014) |
| ***SNAP*** | | | |
| High School or Less | -0.0033 | 0.0016 | 0.0017 |
| | (0.0159) | (0.0086) | (0.0030) |
| Some College | -0.0087 | 0.0082 | 0.0074* |
| | (0.0145) | (0.0101) | (0.0032) |
| Bachelor's Degree or More | 0.0158 | 0.0066 | -0.0016 |
| | (0.0135) | (0.0096) | (0.0027) |
| ***Spouse*** | | | |
| High School Degree or Less | | -0.0008 | -0.0004 |
| | | (0.0005) | (0.0004) |
| Some College | | -0.0020*** | -0.0008* |
| | | (0.0005) | (0.0003) |
| Bachelor's Degree or More | | -0.0008* | -0.0003 |
| | | (0.0004) | (0.0002) |

| | | | |
|---|---|---|---|
| ***Other*** | | | |
| High School Degree or Less | -0.0063** | -0.0006 | -0.0013* |
| | (0.0020) | (0.0015) | (0.0005) |
| Some College | 0.0007 | -0.0025+ | -0.0000 |
| | (0.0014) | (0.0013) | (0.0001) |
| Bachelor's Degree or More | 0.0042* | -0.0010 | -0.0001 |
| | (0.0017) | (0.0006) | (0.0001) |
| **Na** | 5,985 | 8,510 | 8,440 |
| **Pseudo-R2** | 0.1804 | 0.2025 | 0.1595 |
| **Log Likelihood** | -25,776 | -72,156 | -24,286 |

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Notes*: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Results are weighted using PSID individual weights.

**Table A6. Employment Effects by Race and Ethnicity, Separate Components model**

| Variable | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|
| **Separate Components Marginal Effects** | | | |
| ***E**** | | | |
| NH White | 0.0087*** | 0.0123*** | 0.0015*** |
| | (0.0024) | (0.0011) | (0.0003) |
| NH Black | 0.0102*** | 0.0080*** | 0.0026* |
| | (0.0025) | (0.0020) | (0.0012) |
| Hispanic | 0.0074 | 0.0194*** | 0.0028** |
| | (0.0046) | (0.0024) | (0.0008) |
| ***Taxes*** | | | |
| NH White | -0.0084 | -0.0078* | -0.0012 |
| | (0.0060) | (0.0038) | (0.0008) |
| NH Black | -0.0066 | -0.0007 | 0.0049 |
| | (0.0064) | (0.0057) | (0.0042) |
| Hispanic | 0.0024 | -0.0065 | -0.0042 |
| | (0.0103) | (0.0086) | (0.0030) |
| ***SNAP*** | | | |
| NH White | 0.0060 | 0.0152* | 0.0040* |
| | (0.0135) | (0.0064) | (0.0018) |
| NH Black | -0.0133 | 0.0074 | -0.0006 |
| | (0.0130) | (0.0127) | (0.0076) |
| Hispanic | -0.0235 | -0.0181 | 0.0016 |
| | (0.0231) | (0.0137) | (0.0054) |
| ***Spouse*** | | | |
| NH White | | -0.0011*** | -0.0003 |
| | | (0.0003) | (0.0002) |
| NH Black | | -0.0007 | -0.0014 |
| | | (0.0007) | (0.0009) |
| Hispanic | | -0.0015* | -0.0001 |
| | | (0.0006) | (0.0005) |

| | | | |
|---|---|---|---|
| ***Other*** | | | |
| NH White | 0.0007 | -0.0010 | -0.0000 |
| | (0.0012) | (0.0006) | (0.0001) |
| NH Black | -0.0065** | 0.0002 | -0.0027* |
| | (0.0019) | (0.0021) | (0.0012) |
| Hispanic | -0.0005 | -0.0018** | 0.0006 |
| | (0.0029) | (0.0007) | (0.0009) |
| **N[a]** | 5,886 | 8,284 | 8.199 |
| **Pseudo-$R^2$** | 0.1762 | 0.2038 | 0.1546 |
| **Log Likelihood** | -25,383 | -69,828 | -22,716 |

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] Number of observations is less than the total sample for each group of parents, as observations in states with no variation in employment are automatically dropped from the sample when running the logistic regression.

*Source:* 1999-2019 PSID data.

*Note*s: Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Marginal effects are an estimate of the weighted average value of F'(x) for continuous variables and ΔF(x) for the change in the indicator variable from 0 to 1 for the discrete variables. Results are weighted using PSID individual weights.

**Table A7. Predicted percentage point change in employment relative to TCJA baseline and base models for all non-ARPA CTC policy options, 2019**

| PANEL A: RTW MODEL | Aggregate Effect | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|---|
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| ***(1) + (2) + (3), Base Model*** | ***0.11*** | ***1.34*** | ***-0.38*** | ***-0.15*** |
| (1) + (2) + (3), By Youngest Child Age Model | 0.10 | 1.31 | -0.38 | -0.16 |
| (1) + (2) + (3), By Education Model | 0.10 | 1.33 | -0.39 | -0.16 |
| (1) + (2) + (3), By Race-Ethnicity Model | 0.13 | 1.38 | -0.41 | -0.14 |
| ***5K CTC non-refundable, Base Model*** | ***0.26*** | ***0.04*** | ***0.54*** | ***0.11*** |
| 5K CTC non-refundable, By Youngest Child Age Model | 0.27 | 0.06 | 0.57 | 0.11 |
| 5K CTC non-refundable, By Education Model | 0.26 | 0.03 | 0.54 | 0.11 |
| 5K CTC non-refundable, By Race-Ethnicity Model | 0.28 | 0.04 | 0.60 | 0.11 |
| ***5K CTC refundable, Base Model*** | ***0.29*** | ***0.99*** | ***0.05*** | ***0.09*** |
| 5K CTC refundable, By Youngest Child Age Model | 0.31 | 1.02 | 0.10 | 0.09 |
| 5K CTC refundable, By Education Model | 0.28 | 0.97 | 0.06 | 0.08 |
| 5K CTC refundable, By Race-Ethnicity Model | 0.29 | 0.92 | 0.08 | 0.10 |
| **PANEL B: SEPARATE COMPONENTS MODEL** | **Aggregate Effect** | **Unmarried Mothers** | **Married Mothers** | **Married Fathers** |
| TCJA | 0.00 | 0.00 | 0.00 | 0.00 |
| ***(1) + (2) + (3), Base Model*** | ***0.11*** | ***0.91*** | ***-0.19*** | ***-0.08*** |
| (1) + (2) + (3), By Youngest Child Age Model | 0.15 | 1.06 | -0.18 | -0.07 |
| (1) + (2) + (3), By Education Model | -0.02 | 0.42 | -0.21 | -0.08 |
| (1) + (2) + (3), By Race-Ethnicity Model | 0.03 | 0.58 | -0.17 | -0.12 |
| ***5K CTC non-refundable, Base Model*** | ***0.13*** | ***0.04*** | ***0.24*** | ***0.07*** |
| 5K CTC non-refundable, By Youngest Child Age Model | 0.14 | 0.05 | 0.24 | 0.10 |
| 5K CTC non-refundable, By Education Model | 0.11 | 0.06 | 0.19 | 0.04 |
| 5K CTC non-refundable, By Race-Ethnicity Model | 0.14 | 0.05 | 0.26 | 0.07 |
| ***5K CTC refundable, Base Model*** | ***0.16*** | ***0.66*** | ***-0.05*** | ***0.06*** |
| 5K CTC refundable, By Youngest Child Age Model | 0.17 | 0.61 | -0.04 | 0.10 |
| 5K CTC refundable, By Education Model | 0.10 | 0.53 | -0.08 | -0.03 |
| 5K CTC refundable, By Race-Ethnicity Model | 0.13 | 0.40 | -0.03 | 0.11 |

| | | | | |
|---|---|---|---|---|
| Weights (% of weighted PSID individuals in each group in 2019)[a] | 100% | 23.56% | 38.22% | 38.22% |

[a] Weights slightly differ for the by race-ethnicity model, given that those in the "Other/Don't Know" category are dropped for this analysis (24.48% weight for unmarried mothers, 37.76% weights for married parents).
*Source:* 1999-2019 PSID data.
*Notes:* Predictions sample includes those observed during 2019, while model parameters are estimated using the full sample.. Estimates are weighted using PSID individual weights.

***Table A8.*** *Estimates Robust to Various Sets of Controls*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | **Unmarried Mothers** | | | | |
| RTW | 0.336*** | 0.395*** | **0.394*****| 0.429*** | 0.405*** | 0.501*** | 0.289*** | 0.368*** |
| | (0.034) | (0.039) | **(0.039)** | (0.041) | (0.041) | (0.049) | (0.036) | (0.044) |
| Nonlabor income | -0.024* | -0.025* | **-0.025*** | -0.031* | -0.025* | -0.038** | -0.002 | -0.013 |
| | (0.011) | (0.011) | **(0.011)** | (0.012) | (0.012) | (0.013) | (0.018) | (0.017) |
| | | | | **Married Mothers** | | | | |
| RTW | 0.365*** | 0.384*** | **0.383***** | 0.392*** | 0.387*** | 0.427*** | 0.201*** | 0.231*** |
| | (0.020) | (0.021) | **(0.021)** | (0.021) | (0.021) | (0.022) | (0.018) | (0.019) |
| Nonlabor income | -0.126*** | -0.133*** | **-0.132***** | -0.139*** | -0.138*** | -0.140*** | -0.094** | -0.096*** |
| | (0.030) | (0.030) | **(0.030)** | (0.033) | (0.032) | (0.029) | (0.030) | (0.028) |
| | | | | **Married Fathers** | | | | |
| RTW | 0.071*** | 0.072*** | **0.072***** | 0.082*** | 0.082*** | 0.112*** | 0.050*** | 0.076*** |
| | (0.010) | (0.011) | **(0.011)** | (0.012) | (0.012) | (0.017) | (0.009) | (0.014) |
| Nonlabor income | -0.004 | -0.004 | **-0.004** | -0.008 | -0.004 | -0.005 | -0.004 | -0.005 |
| | (0.004) | (0.004) | **(0.004)** | (0.006) | (0.004) | (0.005) | (0.003) | (0.004) |
| **Controls** | | | | | | | | |
| Demographic Controls | X | X | **X** | X | X | X | X | X |
| Other family member inc. | X | X | **X** | X | X | X | X | X |
| State, Year FEs | | X | **X** | X | X | X | X | X |
| State-Year Controls | | | **X** | X | X | | X | |
| Education Interactions (w/ State, Year, and # of CTC-eligible kids) | | | | X | | | | |
| # of CTC-eligible kids Interactions (w/ State, Year) | | | | | X | | | |
| State x Year FEs | | | | | | X | | X |
| Lagged Dependent Variable Control | | | | | | | X | X |

* p<0.05, ** p<0.01, *** p<0.001

*Source:* 1999-2019 PSID data.

*Notes*: Results are reported as estimated average elasticities in response to a $1,000 increase in the return to work/nonlabor income. Standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Elasticities are calculated using margins(eyex) command in Stata. Results are weighted using PSID individual weights. Main specification denoted in bold.

**Table A9. Predicted change in employment relative to TCJA baseline for CTC policy options, 2019, RTW model with state by year FEs and lagged employment control**

| | Aggregate Effect | Unmarried Mothers | Married Mothers | Married Fathers |
|---|---|---|---|---|
| TCJA | 0.00% | 0.00% | 0.00% | 0.00% |
| (1) No refundability threshold | 0.00% | 0.11% | -0.03% | -0.03% |
| (2) Per child refundability | 0.10% | 0.58% | -0.04% | -0.06% |
| (3) 2K refundable max | -0.02% | 0.05% | -0.07% | -0.02% |
| (1) + (2) + (3) | 0.15% | 1.13% | -0.18% | -0.12% |
| (5) ARPA CTC | -1.05% | -2.71% | -0.51% | -0.57% |
| (6) ARPA CTC for children up to age 2 | -0.08% | -0.11% | -0.07% | -0.07% |
| (7) 5K CTC non-refundable | 0.14% | 0.04% | 0.24% | 0.11% |
| (8) 5K CTC refundable | 0.22% | 0.80% | -0.02% | 0.09% |
| Weights (% of weighted PSID individuals in each group in 2019) | 100% | 23.56% | 38.22% | 38.22% |

*Source:* 1999-2019 PSID data.
*Notes:* Predictions sample includes those observed during 2019, while model parameters are estimated using the full sample.. Estimates are weighted using PSID individual weights.

**Table A10.** **Estimates Robust to Predicting Transfer Income**

| | **Unmarried Mothers** ***(Lagged Transfer Income)*** | **Unmarried Mothers** ***(Predicted Transfer Income)*** |
|---|---|---|
| RTW | **0.394*** ** | 0.394*** |
| | **(0.039)** | (0.039) |
| Nonlabor income | **-0.025*** | -0.042* |
| | **(0.011)** | (0.021) |
| Baseline Employment | 0.80 | 0.80 |
| N | 5,985 | 5,985 |
| Pseduo-$R^2$ | 0.170 | 0.171 |
| Log likelihood | -26,112 | -26,067 |

* p<0.05, *** p<0.001
*Source:* 1999-2019 PSID data.
*Notes*: Results are reported as estimated average elasticities with standard errors in parentheses. Dollar amounts scaled to thousands of $2019. Elasticities are calculated using margins(eyex) command in Stata. Results are weighted using PSID individual weights. Main specification denoted in bold.

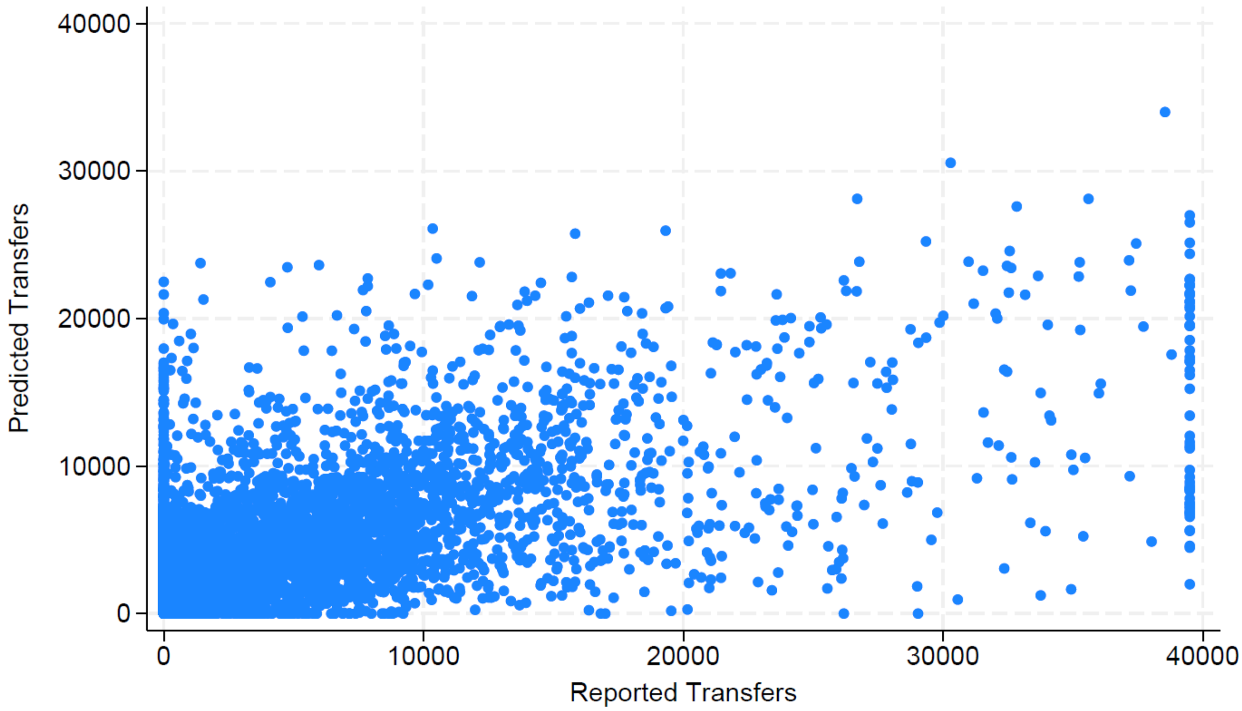
**Table A11. Predicted change in employment relative to TCJA baseline for CTC policy options, 2019, RTW model with predicted transfer income**

| **RTW MODEL** | **Unmarried Mothers (*Lagged Transfer Income*)** | **Unmarried Mothers (*Predicted Transfer Income*)** |
|---|---|---|
| TCJA | **0.00%** | 0.00% |
| (1) No refundability threshold | **0.14%** | 0.15% |
| (2) Per child refundability | **0.68%** | 0.69% |
| (3) 2K refundable max | **0.08%** | 0.08% |
| (1) + (2) + (3) | **1.34%** | 1.35% |
| (5) ARPA CTC | **-3.79%** | -4.37% |
| (6) ARPA CTC for children under age 2 | **-0.17%** | -0.21% |
| (7) 5K CTC non-refundable | **0.04%** | 0.05% |
| (8) 5K CTC refundable | **0.99%** | 0.99% |

*Source:* 1999-2019 PSID data.
*Notes:* Predictions sample includes those observed during 2019, while model parameters are estimated using the full sample. Estimates are weighted using PSID individual weights. Main specification denoted in bold.

**Appendix Figure A1. Fitted Versus Actual Transfer Income, Unmarried Mothers**

[Correlation: 0.58]




*Source:* 1999-2019 PSID data.

[1] Low-income taxpayers with AGI under $25,000 received $14.3 billion from the child credit for 2022. For more information, see Table 3.3 https://www.irs.gov/statistics/soi-tax-stats-individual-statistical-tables-by-size-of-adjusted-gross-income.

[2] Since 1993, the EITC has also included a small credit for workers without children.

[3] For more information on the legislative history of the EITC, see Crandall-Hollick (2022).

[4] The credit was refundable for taxpayers with three or more qualifying children using what is commonly referred to as the "alternative formula." The refundable credit under the alternative formula is calculated as the excess of a taxpayer's payroll taxes (including one-half of any self-employment taxes) over their earned income tax credit (EITC), not to exceed the maximum credit amount. However, lower-income taxpayers will often pay less in payroll taxes than they will receive in the EITC. This is because payroll taxes are equal to 7.65% of earnings, while the EITC equals up to 45% of earnings at lower incomes. Thus, in practice the credit was generally nonrefundable for most low-income families.

[5] Like the earning threshold amounts for the EITC, the $10,000 earnings threshold was indexed for inflation.

[6] The law also enacted a $500 nonrefundable credit for each dependent ineligible for the CTC.

[7] See https://taxpolicycenter.org/briefing-book/key-elements-of-the-u.s./taxes-and-the-family/how-did-the-tcja-change for more information.

[8] These higher credit amounts began to phase out when a married couples' income exceed $150,000 ($112,500 for single parents; see Figure 2).

[9] These estimates are derived using the supplemental poverty measure or SPM. See Creamer et al. (2022) for more information.

[10] The share of Asian children in poverty also decreased, albeit to a lesser degree, by 24%, while the share of American Indian and Alaskan Native children in poverty decreased by more than 50% (Creamer et al., 2022; see Table B-2).

[11] In general, states have substantial design and implementation authority over government support programs and labor market policies. For example, the traditional cash assistance block grant for low-income families is largely devolved to states, and states have the authority to raise their own minimum wages and supplement the federal EITC with their own state-level EITC. Even SNAP – a federal entitlement – allows for state policy choices that intervene to make it easier or more difficult to access benefits.

[12] A confounding factor is that the welfare reforms at both the federal and state levels enacted around the time of the largest EITC expansion, in 1993, were intended to increase employment among low-income workers. Kleven (2024) argues that welfare reform explains the employment effects attributed to the EITC; Schanzenbach and Strain (2021) adopt a different empirical specification and conclude that the 1993 EITC expansion increased employment independent of changes to welfare policy.

[13] See for example, https://www.cbpp.org/press/statements/record-rise-in-poverty-highlights-importance-of-child-tax-credit-health-coverage .

[14] Pac and Berger's (2024) results for single female caregivers with one child aged 0-5 are negative and statistically significant (2.7 percentage point reduction in employment). However, estimates for married female caregivers with children aged 0-5 are positive.
[15] The estimates in Corinth et al. (2021) are also sensitive to other empirical choices. For example, the study analyzed the impact of the ARPA expansion at the tax unit level and thus assumed that either both spouses in a married couple remained in the workforce or both spouses exited the workforce. See Wielk et al. (2023) for a discussion. For a non-technical summary of the disagreement, see https://www.washingtonpost.com/politics/2021/11/08/battle-over-bidens-child-tax-credit-its-impact-poverty-workers/.
[16] Kaia Hubbard, "Senate fails to advance major tax bill that would expand Child Tax Credit," *CBS News*, August 1, 2024, https://www.cbsnews.com/news/child-tax-credit-senate-vote/.
[17] See https://taxpolicycenter.org/comparing-child-tax-credit-legislation-2025-tcja-debate for more information.
[18] We do not count 16-year-olds as dependent children for CTC purposes as it is possible that they turn 17 during the year and then become CTC ineligible.
[19] The PSID did not specifically asked about "hispanicity" during the 1997-2003 period (see the PSID Family Public Data Index (Demographic/Race and Ethnicity/hispanicity) at https://simba.isr.umich.edu/DC/i.aspx for more information). We leverage the panel nature of the data to impute responses from earlier (1996) and later years (2005 onwards) when possible.
[20] The high-income phaseout can also affect work incentives (see "Responses to the EITC and CTC" section), but we exclude people with such high incomes from our sample.
[21] In addition to predicted earnings (E*), we include reported non-labor income from interest, dividends, rent, trusts, and Social Security (SS), each winsorized at the $99^{th}$ percentile.
[22] See https://taxsim.nber.org/taxsim35/ for more information.
[23] For example, this means we calculate T for 2019 PSID respondents during the 2018 tax year. These taxes would be paid (and any refundable benefits received) during 2019 when filing taxes for the 2018 tax year.
[24] To calculate SNAP benefits, we use data on maximum benefits by state and year from the University of Kentucky Center for Poverty Research (https://cpr.uky.edu/resources/national-welfare-data), on the FPL from the Department of Health and Human Services (https://aspe.hhs.gov/topics/poverty-economic-mobility/poverty-guidelines/prior-hhs-poverty-guidelines-federal-register-references), on BBCE from the Economic Research Service (https://www.ers.usda.gov/data-products/snap-policy-data-sets), and on SNAP standard and shelter deduction rates from the Department of Agriculture (see for example https://www.fns.usda.gov/snap/allotment/fy-2011-information). For unmarried mothers, if E* is less than the applicable gross income limit, we reduce SNAP(0, Λ) by 30% of one's net labor income for SNAP purposes, calculated as 80 percent of E* minus the standard and shelter deductions. For married parents, we take spousal earnings into account along with E* when making these calculations. We use PSID information on mortgage and utility payments to estimate the shelter deduction for each person-year (see https://www.cbpp.org/research/food-assistance/a-quick-guide-to-snap-eligibility-and-benefits).

[25] During our sample period, the transfer programs include TANF, Supplemental Security Income (SSI), "other welfare," VA/other pension, annuities, IRA and other retirement, unemployment compensation, worker's compensation, child support, alimony, help from relatives, help from others, and miscellaneous.
[26] See the Robustness Checks section for estimates that instead use a predicted measure of transfer income.
[27] Results are robust to state-level clustered standard errors (available upon request). Several states had so few observations that the state fixed effect perfectly predicted the outcome (i.e., all respondents in the state were employed). Observations in those states were automatically excluded from estimation.
[28] This is relatively rare and corresponds to the taxable and transfer income of other adult family members living in the household. Less than 3% of our sample lives with an adult family member other than their spouse (see Table 3).
[29] While CTC benefits phase in by 15 cents per dollar of income regardless of number of children, resulting in varying earned income levels necessary to qualify for maximum benefits based on one's number of children, less than 5% of the sample has more than four children. We thus capped this variable at 4+ to facilitate the regression analysis.
[30] These data come from the University of Kentucky Center for Poverty Research (https://cpr.uky.edu/resources/national-welfare-data).
[31] These are calculated using the "margins, dydx" and "margins, eyex" commands in Stata, respectively.
[32] In contrast, the p-values for this test are 0.546 and 0.357 in the cases of unmarried mothers and married fathers, respectively.
[33] For married parents, the correlations between predicted and reported transfers are weaker (less than 0.40), likely due to about three-quarters of married parents having no transfer income during the prior wave.
[34] See https://taxpolicycenter.org/briefing-book/what-earned-income-tax-credit for more information.
[35] We restrict our sample of unmarried mothers to those that are "heads/reference persons" in the PSID. This means that the individual did not report a long-term (greater than one-year) cohabitating partner.