Work Requirements and Child Tax Benefits*

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December 18, 2024

**Abstract**

Many U.S. safety-net programs condition benefit eligibility on work. Eliminating work requirements would better target benefits to the neediest families but would also attenuate pro-work incentives. We study how expanding child tax credits to non-workers affects maternal labor supply, using administrative tax records and variation in state credit eligibility from quasi-random birth-timing. We employ a novel method for using placebo analyses to maximize the precision of our regression discontinuity estimator. Eliminating work requirements causes very few mothers to exit the labor force; our 95% confidence intervals exclude reductions over one-third of one percent.

(*JEL* H24, I38, J22)

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# 1 Introduction

A large and growing body of research documents substantial long-term benefits of transferring resources to children growing up in poverty (National Academies of Sciences, 2019; Aizer, Hoynes and Lleras-Muney, 2022).[1] At the same time, some of the largest social welfare programs focused on children exclude the lowest-income families from their scope. In the United States, for example, both the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC) provide no assistance to parents without income and provide only limited assistance to parents with very little income during the year. There have been many proposals to restructure these programs to provide larger benefits to the lowest-income families.[2] However, making such changes risks attenuating the financial incentive to work that these programs create (Besley and Coate, 1992). If parents can access child benefits whether or not they work, how will that affect their decisions about whether to participate in the labor force?

To shed light on this question, we study the effects of work requirements in state child tax credits on maternal labor force participation. A number of states have recently adopted credits for taxpayers claiming children below the age of six. Importantly for our purposes, these credits vary from one another in whether they condition benefit eligibility on work. We initially focus on a reform that eliminated a work requirement governing California's Young Child Tax Credit (YCTC). That is, like the federal CTC, the YCTC was initially available only to taxpayers with positive earned income during the year. After the state eliminated the work requirement, taxpayers could claim the full YCTC amount ($1,000) even if they earned no income during the year. We use this reform to isolate the effect of the work requirement

[1] Some recent examples of this literature include Dahl and Lochner (2012); Aizer et al. (2016); Bastian and Michelmore (2018); Cole (2021); Barr, Eggleston and Smith (2022); Bailey et al. (2023); Bhardwaj (2023); and Rittenhouse (2023).

[2] With respect to the EITC, reform proposals include replacing the EITC with a flat means-tested credit based on family size along with a per-worker income subsidy (Burman, 2019; Drumbl, 2019; National Taxpayer Advocate, 2020). With respect to the CTC, in 2021 Congress considered legislation to permanently make the credit fully refundable, so that low-income taxpayers could receive the full credit amount. Concerns that this reform would lead to parents dropping out of the labor force were central to the bill's opposition.

on labor force participation.

We complement this analysis by studying the labor supply effects of eligibility for young child tax credits in five other states—Colorado, Maryland, New Jersey, Oregon, and Vermont—that vary from one another in whether they condition credit eligibility on work. Although these states did not modify a work requirement during our sample period, we use them to assess the degree to which the California credit generated similar effects as other state child tax credits, both before and after its reform.

To conduct our analyses, we consider variation in mothers' exposure to the reform induced by quasi-random variation in the timing of their children's births—children who turn six just after the close of the year are eligible for the credits whereas those who turn six just prior to the end of the year are not. We use this regression discontinuity (RD) design to estimate the difference in labor force participation between mothers of age-eligible children versus age-ineligible children.

To increase the precision of our estimates, we propose and implement a novel placebo-based tuning analysis to guide the choice of specification for our RD analysis. The procedure selects the specification that minimizes the mean-squared error of estimated pseudo-treatment effects during a set of placebo years prior to the initial adoption of the policy. That is, for each placebo year before a state adopts its tax credit, we estimate the labor force participation "effect" for mothers whose children are born around the age-eligibility cutoff for the credit that will eventually be adopted. We use this approach to jointly optimize the RD bandwidth and polynomial order, as well as to determine other specification choices like the local polynomial kernel and whether to employ an "RD-Donut" design.

Our data comes from administrative Internal Revenue Service (IRS) tax records. We measure labor force participation from wage and self-employment income reported to the IRS in third party information returns, which we can observe whether or not a taxpayer files a tax return. A limitation of this data for our purpose is that we cannot observe the state of residence for mothers who are not listed on a tax or information return provided to the IRS. To address this challenge, we primarily focus on mothers who worked during the prior year; individuals in this group can be assigned to a state based on the address listed on the information return that reports their income. We supplement this analysis with two alternative samples—drawn from Medicaid enrollees and from social security records housed at the Census Bureau—to provide a comprehensive picture of flows into and out of the labor force.

We first estimate the effect of the reform to the work requirement in California. To do so, we separately estimate the effect of the California credit during the time period in which the credit was subject to the work requirement, as well as during the time period in which it was not. We find no evidence that the credit increased labor force participation when eligibility was conditioned on work, nor do we find evidence that it reduced participation when it did not; the respective estimated 95% confidence intervals are sufficiently precise to exclude increases larger than 0.09 percentage points when the work requirement was in effect and reductions larger than 0.31 percentage points when it was not. Taking the difference of these estimates suggests that eliminating the work requirement did not cause a significant number of working mothers to exit the labor force; our estimated 95% confidence interval for the effect of the reform excludes labor force participation reductions greater than 0.28 percentage points in magnitude.

We next turn from California to the other states that offer tax credits to the parents of children below the age of six. In each case, we find similar results as California. For Colorado, which offered a maximum child tax credit of $1,200 per child subject to a work requirement, we estimate that the credit did not substantially increase labor force participation; our point estimate corresponds to an increase of 0.03 percentage points and the 95% confidence interval excludes increases of greater than 0.39 percentage points. The other four states—Maryland, New Jersey, Oregon, and Vermont—provided young child tax credits that were not conditional on work during our sample period with maximum benefits ranging from $500 to $1,000 per child. We find no evidence that these unconditional tax credits substantially reduced labor force participation in any of the states in which they were adopted; on average, we estimate that they reduced labor force participation by 0.05 percentage points, with a 95% confidence interval excluding reductions greater than 0.28 percentage points. When combined with our results from California, these estimates suggest that labor force participation is relatively insensitive to changes in child tax credit work incentives; our results translate to an extensive-margin substitution elasticity of labor supply of 0.05 along with a near-zero extensive-margin income elasticity.

We validate our identifying assumptions with several placebo exercises. First, we replicate our main analysis for mothers in other time periods and in states that do not provide tax credits with a six-year-old eligibility cutoff. In addition, a distinctive feature of California's credit is that it does not depend on the number of eligible children claimed (beyond one). This motivates a placebo test comparing California mothers who have a younger child in addition to a child turning six around the turn-of-the-year, and who therefore experience no difference in credit eligibility when their older child turns six. We find no effects on labor force participation for any of these placebo groups.

By restricting our analysis to previously working mothers, our main sample primarily speaks to the effect of work requirements on exits from the labor force. We supplement this analysis with two additional samples: mothers whose children were enrolled in Medicaid and, for the California analysis, mothers of all children assigned a social security number constructed based on the Social Security Administration's Numident file. These samples complement our main analysis as they include mothers who did not work during the prior year, allowing us to study how the work requirement shapes the flow of nonworking mothers into the labor force. We observe some suggestive evidence that the availability of a tax credit may increase labor force participation for non-working mothers. However, these effects do not appear to be tied to the presence of a work requirement, and indeed our estimated 95% confidence interval for the California reform for this sample excludes substantial reductions in labor participation from the elimination of the work requirement.

We conduct several additional analyses to further explore our results. We find no substantial effect of the work requirement on the share of taxpayers reporting very low incomes or when using reported earnings instead of third-party earnings to measure labor force participation. However, we do find small but positive effects of the California reform on tax filing, consistent with increased take-up from simplified eligibility rules.

Our goal in this paper is to isolate the effect of work requirements in child tax benefits. In doing so, we contribute to a growing literature that studies the labor supply effect of work requirements in safety net programs such as SNAP (Harris, 2021; Han, 2022; Gray et al., 2023; Cook and East, 2024), Medicaid (Sommers et al., 2020), and TANF (Falk, 2023).[3] With respect to tax benefits for children, several prior papers have used administrative tax data along with quasi-random variation in birth timing to study effects on labor supply (Feldman, Katuscak and Kawano, 2016; Mortenson et al., 2018; Lippold and Luczywek, 2024). However, these papers rely on variation in benefit amount or in the timing of benefit receipt, as opposed to variation in work requirements, and so do not speak to the effects of conditioning benefit eligibility on work.[4]

Prior microsimulations that predict the effects of reforming child tax credit work requirements therefore largely rely on empirical estimates drawn from policy variation in the federal EITC (Goldin, Maag and Michelmore, 2022; Corinth et al., 2021; Bastian, 2024).[5] However, the state child tax credits we study may provide a better guide to reforms that relax existing work requirements—especially reforms that do so in the shadow of the EITC and the labor supply incentives that it generates. Moreover, policies like the EITC incentivize work by providing a benefit that is exclusively available to working taxpayers. In contrast, reforms

[3] An established literature studies the elimination of the 1990s welfare reform, which involved changes in benefits for non-workers as well as reductions in effective marginal tax rates due to benefit phase-outs (e.g., Moffitt, 1992; Hoynes, 1997; Ziliak, 2015).

[4] Wingender and LaLumia (2017) and Lippold (2022) employ a similar identification strategy as these papers in conjunction with survey data to estimate the labor supply effects of child tax benefits in the U.S. There also exists a large literature on the labor supply effects of child benefits outside the U.S. context, including Milligan and Stabile (2007); González (2013); Schirle (2015); Messacar (2021); Baker, Messacar and Stabile (2023); Jensen and Blundell (2024).

[5] Much of this literature documents large labor force participation effects of the EITC; for reviews, see Eissa and Hoynes (2006); Nichols and Rothstein (2016); Schanzenbach and Strain (2021).

that delink child benefits and work (like the ones we study) affect the return to work by expanding an existing benefit to non-workers. Although both types of policies shape the return to work, only the latter does so for taxpayers with incomes above the EITC phase-out; in some analyses, a substantial share of the predicted reduction in labor participation from expanded child tax benefits stems from the labor response of this group (e.g., Corinth et al., 2021).

Closer to our focus, several recent papers study the effect on labor supply of the elimination of the federal CTC's work requirement and phase-in structure as part of a one-year tax reform that applied to tax year 2021 (Lourie et al., 2022; Han, 2022; Enriquez, Jones and Tedeschi, 2023; Pac and Berger, 2024; Ananat et al., 2024). We complement these studies—which largely rely on survey data and an event-study design using differences in CTC eligibility (e.g., parents versus non-parents) in time periods with and without the CTC expansion[6]—by adding precision through a much larger administrative data set and an alternative identification strategy that takes advantage of a particularly close link between the treated and untreated groups. We therefore view our results as providing some of the most direct evidence to date on the labor supply effects of conditioning child tax benefits on work.

A final contribution of our paper is methodological. Researchers exploiting quasi-random treatment assignment in regression discontinuity settings must make a number of modeling choices relating to the empirical specification they employ (Lee and Lemieux, 2010; Cattaneo, Idrobo and Titiunik, 2019). We propose and implement a data-driven method for making these choices that draws on the availability of placebo periods. Specifically, we select the elements of the empirical specification (e.g., bandwidth, polynomial order) to minimize the mean squared error (MSE) of the pseudo-treatment effects estimated from years prior to a state's adoption of a credit. Popular existing methods to select RD specifications also aim to minimize MSE, but do so by estimating (asymptotic) MSE approximations (Imbens and Kalyanaraman, 2012; Calonico, Cattaneo and Titiunik, 2014; Pei et al., 2022). In con-

[6]One exception is Lourie et al. (2022) which uses bank transactions data to estimate labor force participation and CTC receipt.

trast, our procedure makes use of placebo settings to directly evaluate specifications' MSE out-of-sample, using the actual birth date cutoff that will determine credit eligibility once the credit is adopted. When the available placebos suitably approximate the relevant bias-variance trade-offs for our actual sample of interest, this method allows us to choose among potential specifications to maximize precision while avoiding concerns of over-fitting or inefficient regularization.[7] We provide evidence that our method yields substantial performance gains in settings like the one we study when compared to popular methods for bandwidth selection or to the specification employed in recent papers that exploit similar variation in birth-timing. More generally, our proposed approach can improve the precision of regression discontinuity estimators when suitable placebo datasets are available to the researcher.

## 2 Institutional Background

At the federal level, the U.S. income tax code provides a number of benefits for taxpayers who claim children on their returns. The largest federal tax benefit for children is the Child Tax Credit (CTC), which provides a tax credit of up to $2,000 for each child under the age of 17 that the taxpayer claims on his or her return.[8] The credit is partially refundable, with the refundable portion gradually phasing in once the taxpayer's earned income exceeds $2,500. In addition, the total refundable portion is capped at $1,400 per child. Because of these aspects of the credit's design, taxpayers without earned income during the year do not benefit from the CTC, and many working class taxpayers do not qualify for the full

[7] Ludwig and Miller (2007) and Imbens and Lemieux (2008) also propose bandwidth selection procedures that avoid over-fitting by evaluating performance out-of-sample. Those methods rely on an exchangeability assumption over placebo cutoffs in the running variable, whereas our approach compares specifications using the same date-of-birth cutoff relied on for identification (i.e., the turn-of-the-year), which may yield a different bias-variance trade-off. Our approach also relies on an exchangeability assumption—across the treatment and placebo periods—which we argue is plausible in our setting. Card et al. (2017) present evidence regarding the efficiency of regularization terms in bandwidth selection methods that aim to minimize asymptotic mean squared error.

[8] The CTC has been reformed a number of times since its introduction in 1997; we focus on the rules in place for recent tax years other than 2021. For tax year 2021, the federal CTC was available to taxpayers without income from work and the maximum benefit was increased to $3,600 per child younger than age 6 and $3,000 per child older than six and younger than 18.

maximum benefit (Collyer, Wimer and Harris, 2019; Goldin and Michelmore, 2022). The CTC begins to phase out for taxpayers with annual incomes over $200,000 if single and $400,000 if married.[9]

Turning from federal to state tax policy, a growing number of states provide their own child tax credits in addition to the federal benefit. We primarily focus on a policy change in the design of one such benefit: California's Young Child Tax Credit (YCTC). Beginning in 2019, the YCTC provided a maximum state tax credit of up to $1,000 per tax return for California taxpayers who meet its income requirements and who claim one or more children below the age of six.[10] From 2019-2021, an average of 407,000 California taxpayers received approximately $375 million of YCTC annually, with an average benefit amount of $922 per return.[11]

Although both the CTC and YCTC provide benefits to taxpayers with children, the two credits differ in a number of respects. First, the YCTC is only available for taxpayers with young children: taxpayers must claim at least one child under the age of 6 (versus under 17 for the federal CTC). Specifically, the dependent child must not have turned six on or before December 31st of the given tax year. Second, the YCTC targets lower-income families by phasing out at a much lower income level than the CTC ($30,000 versus $200,000 or $400,000). Third, the YCTC does not vary based on the number of young children in the household, unlike the CTC which is a per-child benefit. Finally, a key difference between the CTC and YCTC for our purposes is the relationship between credit amount and earned income for low-income taxpayers. Whereas refundability of the CTC phases in by earned income and is capped at $1,400, the YCTC is fully refundable at all income levels – benefits

[9] The other main federal income tax credit providing benefits to taxpayers with children is the Earned Income Tax Credit (EITC). The EITC phases in by income, with a maximum benefit in 2019 ranging between $3,526 and $6,557 depending on the number of children a taxpayer claims (a smaller benefit is available to working taxpayers who do not claim children). For most children, the maximum age to qualify a taxpayer for the EITC is 18, or 23 if the child is a full-time student.

[10] This amount was increased to $1,083 and $1,117 for tax year 2022 and 2023, respectively.

[11] Authors' calculations based on California Franchise Tax Board (2019) and subsequent reports. By comparison, the California EITC, which targets families in the same income range, benefited approximately 3.7 million taxpayers per year during the same years, with an average benefit amount of $197 per return, or $463 per return among the one million returns with children.

are not phased in. Thus, for California taxpayers who qualify for the credit, the YCTC is equivalent to a flat cash transfer (at least until the phase-out threshold is reached).

The aspect of the YCTC on which we focus is eligibility for taxpayers without earned income. From the program's introduction (beginning with tax year 2019) through tax year 2021, taxpayers were required to have positive earned income to qualify for the credit. We refer to this aspect of the credit's design as a work requirement. In the years that the YCTC work requirement was in place, taxpayers without earned income did not qualify for the YCTC whereas taxpayers with $1 or more of earned income qualified for the full benefit amount, assuming they were otherwise eligible (see Panel A of Figure 1).[12] Then, beginning in tax year 2022, taxpayers were no longer required to have positive earned income to qualify for the YCTC, eliminating the work requirement (see Panel B of Figure 1). We use this policy change to study how conditioning child tax benefits on work shapes labor force participation.

The YCTC benefit variation we study is comparable to the magnitude of policy variation frequently studied in the literature. For example, Feldman, Katuscak and Kawano (2016) and Lippold (2022) study the effects of a child aging out of federal CTC eligibility during a time period in which this resulted in a maximum benefit change of $1,000. With respect to the EITC, studies using variation induced by the 1986 Tax Reform (Eissa and Liebman, 1996) or changes in state EITC policy (Michelmore and Pilkauskas, 2021) focus on benefit changes that are smaller than the YCTC variation we study (roughly $300 to $700 in 2019 dollars), whereas studies focusing on the EITC's introduction (Bastian, 2020) or 1993 expansion (Meyer and Rosenbaum, 2001) involve larger benefit changes.[13] Appendix Figure A.1 plots the percentage change in the after-tax return to work from these reforms along with the change from the elimination of the YCTC work requirement. Because of the lack of a phase-

[12]During this policy period, California legislators expanded YCTC eligibility for taxpayers without social security numbers authorizing them to work; below, we consider specifications that exclude this group from our analysis.

[13]The federal EITC's introduction involved a benefit change of $1,900 in 2019 dollars. Studies of the Omnibus Budget Reconciliation Act of 1993 typically compare benefit changes of taxpayers with one versus two or more children, corresponding to up to approximately $1,700 in 2019 dollars.

in, the elimination of the YCTC work requirement led to a very large reduction in the return to work for the lowest-income working mothers, with a smaller (and monotonically declining) reduction for middle- and higher-income mothers. In contrast, the EITC reforms generated a smaller percentage change in the return to work for the lowest-income taxpayers—who did not qualify for the full EITC amount—and a larger percentage change for middle- and higher-income taxpayers.

Because California reformed its credit to eliminate the work requirement, it facilitates a clean comparison between the two policy designs. However, the child tax credits from other states are more similar to the federal CTC in certain respects and allow us to assess the degree to which the California results generalize to other settings. We therefore supplement our California analysis with analyses that study labor supply responses to child tax credits in the five other states that provide benefits to taxpayers with children below the age of six.[14]

In 2022, Colorado introduced the Colorado Child Tax Credit. Like the YCTC, the Colorado credit was targeted at lower-income families: the credit was fully phased out for single parents earning over $75,000 and married parents earning over $85,000. The Colorado credit was somewhat more generous than the YCTC in two respects. First, the maximum benefit for a taxpayer claiming one child was $1,200 in Colorado compared to $1,000 in California. Second, the Colorado credit was provided on a per-child basis (e.g., taxpayers claiming two children could receive a maximum benefit of $2,400) whereas California parents with multiple qualifying children did not qualify for a larger YCTC compared to parents with a single qualifying child. Like the federal CTC and the initial design of the YCTC, the Colorado credit had a work requirement during our sample period. However, unlike the YCTC, the Colorado CTC was pegged to the federal CTC, so that its benefits phased in

[14] A number of states provide child tax credits for parents of older children. Many of these states have eligibility thresholds that coincide with eligibility for other tax credits—the federal CTC in the case of Arizona, Idaho, Maine, New York, and Oklahoma; the Child and Dependent Care Credit in the case of Massachusetts; and the EITC in the case of New Mexico. We exclude these states from our analysis, restricting our attention to states with an eligibility cutoff at age six.

gradually with the taxpayer's earned income (see Panel A of Figure 1).

Finally, during 2022 and 2023, four states adopted child tax credits for parents of young children that were not conditioned on work: Maryland (2023), New Jersey (2022), Oregon (2023), and Vermont (2022). Like the federal CTC, these state credits were each provided on a per-child basis, with maximum credit amounts of $500 per child (Maryland, New Jersey in 2022) or $1,000 per child (Oregon, Vermont, New Jersey in 2023). Although all four credits were available to non-workers, the maximum income at which taxpayers could qualify varied widely across these credits, ranging from $15,000 (Maryland) to $175,000 in Vermont (see Panel B of Figure 1).

# 3 Data

We draw on federal administrative tax records for our analysis. This data includes the universe of children who receive a social security number as well as the mothers listed on those children's birth certificates. We focus on mothers because a higher fraction of birth certificates contain maternal social security number information (93.5%) versus paternal social security number information (79.9%).[15]

Our main sample consists of mothers who worked during the prior year and whose youngest child turns six around the start of one of our outcome years. We construct this sample as follows. The first three steps each draw on Social Security birth records. First, we identify the cohort of children for a given outcome year. This cohort consists of the universe of U.S. children who turned six years old during the final months of the outcome year as well as those children who turned six years old during the first months of the year following the outcome year. For example, the 2020 cohort consists of children born at the end of 2014 or the start of 2015. Children in the latter category are age-eligible for the credit in the outcome year whereas children in the former category are not. Second, we link children to

[15] In addition, there is substantially more consensus regarding the magnitude of labor supply elasticities for working age males, which are thought to be quite small in magnitude (Chetty et al., 2011).

the individual whose social security number is listed as the child's mother on the child's birth certificate. Third, for our California analysis, we identify other children of the same mother, and drop mothers who have given birth to a child younger than the reference child before the outcome year. This restriction excludes mothers who would continue to qualify for the YCTC despite their reference child aging out of eligibility. Fourth, we restrict the sample to the subset of mothers who received a third-party information return (Form W-2, 1099-Misc, or 1099-NEC) showing positive income for the year prior to the outcome year.[16] We assign individuals to states based on the taxpayer's residence information listed on the applicable information return. Finally, our main analyses exclude the cohort of mothers whose children turn six around the conclusion of 2021 because the temporary expansion of the federal CTC for that year provided additional benefits for taxpayer claiming children under age 6—the same eligibility cutoff we use for identification.

By focusing on individuals who were recently in the labor force, our primary sample sheds light on a question that has been central to recent policy debates: the degree to which expanding the refundability of child tax credits causes taxpayers to exit the labor force. A downside of this sample is that it does not allow us to study effects on the flow of non-working individuals into the labor force. However, recall that our main sample relies on prior-year income reports to classify individuals to states; a challenge in constructing samples with non-working mothers is that we lack a comprehensive source of information regarding the state in which the individual resides.

We supplement our analysis with two alternative samples that include non-working mothers; each takes a different approach to the data challenge of accurately assigning mothers to states. First, we construct samples of mothers whose children were enrolled in Medicaid or Children's Health Insurance Program (CHIP) during the prior year. Crucially for our purposes, the children in these categories are listed on Form 1095-B, showing their health

[16]We do not otherwise limit our main sample based on income because eliminating the work requirement affects the return-to-work for individuals with incomes above the credit eligibility thresholds. We present subgroup results for more granular income groups.

insurance coverage, which allows us to identify their state of residence even if they did not receive third-party reported income during the prior year. The second alternative sample we consider consists of the universe of children who were born in California. This sample is constructed based on the Social Security Administration's Numident file, which we accessed and linked to tax records through the U.S. Census Bureau.[17] Each of these alternative samples has its own benefits and limitations. The Medicaid sample has good coverage of low-income mothers but may not be representative of the uninsured or individuals who obtain health insurance through other means. In contrast, the Census sample has broader coverage, but lacks the information return data for self-employment income that our primary data set includes.[18]

Our primary outcome is whether an individual works during the outcome year. It is constructed based on whether the IRS receives an information return (Form W-2, 1099-Misc, or 1099-NEC) for the individual for the year showing positive income. Because this measure is based on third-party filed information returns, it covers individuals who did not themselves file an income tax return. Additionally, this ensures that our estimates measure real changes in labor force participation and not changes in reporting behavior in response to tax incentives (Garin, Jackson and Koustas, Forthcoming). In supplemental analyses, we also consider labor force participation measured using the income reported on individuals' filed tax returns, which is available to us for tax years 2022 and earlier.

# 4 Empirical Framework

This section describes our identification strategy and our method for selecting our empirical specification.

[17]The Numident file contains dates of birth, county of residence at time of enumeration, and basic demographic information. We link children to parents using the probabilistic matches contained in the Census Household Composition Key; see Aldana (2022) and Bernard, Drotning and Genadek (2024) for details and validation.

[18]In addition, there is a substantial time delay between when most children receive a Social Security Number and their sixth birthday, which could lead to inaccurate assignments for children who move into or out of the state in the intervening years.

## 4.1 Identification Strategy

Our empirical strategy for estimating the effect of eliminating a credit's work requirement is to combine quasi-random variation in eligibility for the credit with the policy change to the credit's design. Specifically, we compare labor force participation of mothers whose child turns six before the end of a given year with mothers whose child turns six after the start of the subsequent year.[19] We make these comparisons using regression discontinuity specifications of the following form:

$$Y_{it} = \alpha + \beta\, \mathbf{1}_{\{DOB_i \geq 0\}} + g_1(DOB_i) + g_2(DOB_i)\, \mathbf{1}_{\{DOB_i \geq 0\}} + \gamma_t + \varepsilon_{it} \tag{1}$$

where $Y_{it}$ is an indicator for whether mother $i$ had positive earned income in tax year $t$; $DOB_i$ indicates the date of birth of $i$'s youngest child, centered around the turn of the year with December 31 of year $t$ denoted by 0; $\gamma_t$ is a set of year fixed effects; and $g_1(\cdot)$ and $g_2(\cdot)$ are local polynomials. In this specification, the effect of having a child of an age that qualifies a mother for the credit is given by $\beta$.

To interpret $\beta$ as the causal effect of age-eligibility for the credit, we impose the standard RD identifying assumption that the potential outcomes (mothers' labor force participation with and without credit eligibility) are continuous in child's birth date at the turn-of-the-year cutoff, so that (1) yields an unbiased estimate for $\beta$ in each policy period given suitable choices for $g_1(\cdot)$ and $g_2(\cdot)$.[20] This assumption would be violated, for example, if the mothers of children born on either side of the age-eligibility cutoff differed systematically in their propensity to work for reasons unrelated to the credit, such as from compositional differences in the timing of births. In addition, for our analysis of the reform to California's child tax credit, the difference in the estimated values of $\beta$ across policy periods (i.e., for the work

[19] For California, we further limit our analysis to mothers whose youngest child turns six near the age-eligibility cutoff, as described in Section 3.

[20] This assumption is sufficient but not necessary for the difference-in-discontinuities estimator we apply in California to be unbiased. That estimator would also be unbiased if potential outcomes differed discontinuously for birth dates around the turn-of-the-year, but in a manner that was the same on average across policy periods.

requirement years versus the years without a work requirement) forms our estimate of the effect of eliminating the work requirement. To interpret this estimate as the causal effect of eliminating the work requirement requires an additional assumption: but for the elimination of the work requirement, the effect of the credit on labor supply would have been the same in both policy periods. Below, we provide evidence for the plausibility of both of these assumptions in our setting.

## 4.2 Placebo-Based Tuning Analysis

In this subsection, we propose and implement a data-driven method for selecting the local polynomials $g_1(\cdot)$ and $g_2(\cdot)$ in (1). Because the method selects between specifications based on performance in placebo pre-periods, we refer to it as a placebo-based tuning (PBT) analysis. We use this approach to determine the following properties of the local polynomials in children's dates of birth: (1) bandwidth (the width of the birth-timing window to include in our analysis); (2) order (linear, quadratic, or levels); (3) kernel (uniform, triangular, Epanechnikov); and (4) whether to exclude children born around the end-of-the-year holidays through a "donut" specification (e.g., Barreca et al., 2011).

To make these determinations, we evaluate the performance of each candidate specification applied to estimating the effect of placebo policies in the same state and at the same age-cutoff during the years before the credit took effect. Specifically, we separately estimate the effect of each pseudo-policy in each year before the credit is adopted, treating the turn-of-the-year as the threshold for eligibility. We evaluate performance based on the mean of the squared errors (MSE) from the estimated effects of the pseudo-policies. Because the true "effect" of each pseudo-policy is zero, the squared error for a pseudo-policy corresponds to the square of the estimated treatment effect for that pseudo-policy.[21] We then choose among the alternative specifications to minimize the empirical MSE of our treatment effect

[21] We begin this analysis in 2005 rather than 2000 (which is the first year for which we have outcome data) due to differences in the availability of W-2 data prior to that year, which could shape the bias-variance trade-off of candidate specifications.

estimator.

We first consider the optimal bandwidth and order of the local polynomials for our California analysis.[22] On the one hand, narrower birth-timing windows could reduce bias by estimating our effects from groups that are more similar to one another. Similarly, it could be that bias is reduced by flexibly adjusting for differences in children's birth dates. On the other hand, narrower birth-timing windows and more flexible functional forms could reduce the precision of our estimates by yielding an estimator with larger variance.

We empirically evaluate these trade-offs in Figure 2. The blue line plots RMSE for specifications that do not adjust for differences in birth dates between groups – i.e., for simple comparisons of means. For specifications in this category, RMSE follows a "U"-shape pattern with respect to birth-window width, consistent with the presence of a bias-variance trade-off. In particular, RMSE declines monotonically for narrow birth-windows, with larger samples reducing the variance of the estimator from sampling uncertainty. For wider birth-windows, RMSE is monotonically increasing, consistent with increasing bias as the groups of mothers being compared becomes less comparable. The RMSE of the estimator is minimized at a 4-month birth-window. Interestingly, this width corresponds to the widest birth-window that does not intersect with California's kindergarten cutoff of September 1.

The yellow and purple lines in Figure 2 respectively correspond to specifications that employ local linear or local quadratic polynomials. For specifications that use birth-windows of 8 months or less, the linear polynomial yields higher RMSE than the simple comparison of means, and the quadratic polynomial yields higher RMSE than both. For windows that include a year or more on either side of the cutoff, both the linear and quadratic specifications yield a lower RMSE than the levels comparison. Intuitively, wider windows involve comparing mothers who may differ substantially in their composition, increasing the importance of adjusting for differences in the dates of their children's births. Within the range of window lengths we consider, the RMSE-minimizing windows for the linear and quadratic estimators

[22] We apply this procedure to select specifications for our analyses for other states in Section 5.2, below.

yield a larger RMSE than the RMSE-minimizing linear estimator.

We next consider additional specification choices relating to the type of kernel employed for local polynomial estimation and of the exclusion from our sample of children born around the turn-of-the-year (a "donut" specification). We find that the relative performance of alternative kernels is closely linked to the local polynomial order and bandwidth (Appendix Figure A.2). Based on these results, we opt for a uniform kernel, which has the additional virtue of being the most straightforward to implement and interpret. As reflected in Appendix Figure A.3, donut specifications appear to weakly reduce estimator precision, especially for narrower bandwidths, so we do not employ one.

Based on these results, our preferred RD specification for California is a simple difference in means among mothers whose youngest child turns six within four months of the turn-of-the-year:

$$Y_{it} = \alpha + \beta\, \mathbf{1}_{\{DOB_i \geq 0\}} + \gamma_t + \varepsilon_{it} \tag{2}$$

where $\beta$ captures the effect of age-eligibility for the credit on labor participation. All observations within this 4-month window are included in the analysis (no donut) and evenly weighted (uniform kernel).

We have relied on two assumptions in interpreting the PBT results. First, we have assumed that the true pseudo-treatment effect in each placebo period is zero; if not, we would be mis-measuring the MSE. We are not aware of other California policies that would likely affect labor participation and that depend on a similar age cutoff during this period. To empirically assess this assumption, we again use an RD design to estimate discontinuities in labor force participation during each of the placebo years. To avoid biasing these estimates toward zero, we estimate each placebo year's pseudo-treatment effect using a leave-one-out-procedure, in which each placebo year's specification is selected to minimize MSE during the *other* placebo years. As shown in Appendix Table A.1, these estimated pseudo-effects are tightly distributed around zero (Column 1). We find similar results with two other RD specifications, described in more detail below and not based on our PBT approach, for estimating the pseudo-effects during the placebo years (Columns 2 and 3).

Second, for the results in the placebo periods to be informative about our sample period, we require that the distribution of potential outcomes during the placebo periods well-approximates the corresponding distribution during our treatment period. If this exchangeability assumption was violated, the pre-period placebo results would not help us navigate the bias-variance trade-off in the years following the credit's implementation. Although we cannot test this assumption directly, we can assess the stability of the distribution of untreated potential outcomes in this respect over time by comparing the PBT-selected specification using different placebo years within the pre-period. Appendix Figure A.4 shows that the relative performance of alternative specifications appears similar when evaluated using the first versus second halves of the pre-period.

A different potential concern with our method is that, by training it to minimize error during a placebo period in which the true effect is zero, we are inadvertently selecting an estimator that performs well at obtaining estimates that are close to zero, rather than one that performs well at obtaining estimates that are close to the true effect size. If so, this could bias our estimates towards zero even when the true effect size was non-zero. In practice, this possibility strikes us as remote given the set of specification parameters over which we are optimizing.[23] To assess this hypothesis, we conduct Monte Carlo simulations to evaluate how our method performs when the true treatment effect is non-zero. The simulation results are reported in Appendix Figure A.5, and suggest that the estimates obtained through our proposed method are unbiased, at least for the set of parameters considered here.

Our final set of analyses in this section compares the performance of our PBT analysis to alternative approaches for selecting RD specifications. One natural comparison is the specification employed in a number of recent high-quality papers that employ a similar

[23]In settings where the true effect is non-zero, there is no reason to expect that, for example, a narrower or wider bandwidth would tend to yield estimates that are mechanically closer to zero. This concern would be greater if one were applying our method to a setting in which the treatment effect was present only among observations close to the treatment threshold. This seems unlikely in our context; the labor supply incentives facing a parent whose child turns six one month after the cutoff are the same as those facing a parent whose child turns six two months after the cutoff.

identification strategy to ours, exploiting quasi-random variation in birth timing to assess the effect of eligibility of some program (including Barr, Eggleston and Smith, 2022; Garin, Jackson and Koustas, Forthcoming; Lippold and Luczywek, 2024). Those papers primarily rely on a local linear polynomial with a one-month bandwidth and a donut-design that excludes children born in the narrow window around the turn-of-the-year. We also compare PBT to the popular Calonico, Cattaneo and Titiunik (2014) ("CCT") method for bandwidth selection. Like Imbens and Kalyanaraman (2012), on which it is based, CCT relies on minimizing an approximation to the asymptotic mean-squared error, estimated from the actual RD sample rather than a placebo. Because CCT takes the local polynomial order as given, we consider three variants of that method, corresponding to local polynomials of order 0, 1, and 2.

We evaluate PBT against these alternatives using the states that did not adopt a child tax credit for children under age 6 during our sample period. Specifically, for each state, we determine the optimal specification based on minimizing the RMSE from 2005-2018, and then use that specification to estimate the pseudo-effects of a placebo credit during each of our main outcome years (2019, 2020, 2022, and 2023). Appendix Figure A.6 presents the distribution of estimates from this exercise. The distribution of estimates from each method is centered at zero, but the estimates obtained from the PBT specifications are substantially more precise than from either the popular 1-month specification or from any of the CCT variants. We obtain comparable results when we evaluate PBT against the CCT variants using the Monte Carlo simulation described above (see Appendix Figure A.5).[24]

[24] In principle, one could further improve accuracy by considering asymmetric local polynomials across the treatment cutoff by selecting the bandwidth and polynomial order for birth dates after the turn-of-the-year based on an informative placebo setting in which observations on both sides of the cutoff were age-eligible for the credit. See Arai and Ichimura (2018) for an asymptotic MSE-based proposal along these lines.

# 5 Results

In this section, we present our main empirical results. We begin with California, where we can cleanly estimate the effect of a work requirement through a change in the design of the YCTC, before turning to a range of other states that differ with respect to whether they condition child tax benefits on work. Our main sample consists of recently working mothers, and allows us to study the effect of the work requirement on exits from the labor force; we report additional results for two alternative samples that include recently non-working as well as working mothers.

## 5.1 California Results

Appendix Table A.2 presents summary statistics for our primary sample of recently working California mothers. The statistics in the table are calculated based on the mothers' tax records from the prior year, i.e., the most recent tax year in which all of the children in our sample were young enough to qualify for the YCTC. By construction, all of the mothers in our sample have positive income in this prior year, with an average (individual) income of approximately $57,000. The large majority (93%) of mothers in our sample filed a prior-year tax return; of this group, approximately one-third claimed the federal EITC and 90% claimed the federal CTC. We observe several small but statistically significant differences between mothers of children who were age-eligible for the YCTC versus those who were not; for example, mothers of children who turn six at the end of the year tend to be slightly older than mothers of children who turn six at the start of the subsequent year.

Figure 3, Panel A reports labor force participation rates by year among California mothers in the age-eligible and age-ineligible groups; Panel B reports the differences between the eligibility groups in each year. The time period covered by the figure spans three YCTC policy periods: the pre-period years without a YCTC (2005-2018); the years with a YCTC with a work requirement (2019-2021); and two years with a YCTC with no work requirement

(2022-2023). In all but one year, the labor force participation rates of the two groups are not statistically significantly different from one another.[25]

Figure 4 reports labor force participation for California mothers by child birth date, separately for outcome years in which YCTC eligibility was conditioned on work (Panel A) and for outcomes years in which it was not (Panel B). The red lines denote mean labor force participation for children born within a 4-month window around the turn-of-the-year—the PBT-selected specification—estimated separately for mothers on either side of the age-eligibility cutoff.

Table 1 reports the corresponding regression results. Column 1 estimates the labor participation effect of the YCTC with a work requirement using data from 2019 and 2020. Because the YCTC was entirely unavailable during this period to taxpayers without income from work, we can interpret this labor supply response as a pure substitution effect with respect to the extensive-margin of labor supply for taxpayers whose income would not exceed the maximum YCTC income limit of \$30,000. We find that mothers of age-eligible children are 0.11 percentage points (11 basis points) less likely to work, with a 95% confidence interval ranging from -0.32 to 0.09 percentage points.

Column 2 of Table 1 is estimated using data from the time period following the elimination of the YCTC's work requirement (2022 and 2023), and represents the effect of an unconditional child benefit for all taxpayers with income below the phase-out range. Because the YCTC was not conditioned on work during this time period, we can interpret it as generating, with respect to the extensive-margin of labor supply, a pure income effect for taxpayers with incomes below the start of the credit phase-out (\$25,000); a pure substitution effect for taxpayers with incomes above the maximum income threshold (\$30,000); and a combination of income and substitution effects for taxpayers with incomes within the credit phase-out range (\$25,000 to \$30,000). When exposed to this set of incentives, we find

[25] In 2017, we observe a small but statistically significant difference across eligibility groups. However, we are not aware of a policy cause for these differences, and a joint test of the yearly differences does not reject the null hypothesis that the eligibility groups had equal labor force participation in each pre-period year.

that mothers of age-eligible children are 0.11 percentage points less likely to work (95% CI from -0.31 to 0.08).

Column 3 reports our difference-in-discontinuities estimate of the effect of eliminating the YCTC work requirement on labor force participation. The estimate corresponds to $\delta$ in the following specification:

$$Y_{it} = \alpha + \beta\, AgeEligible_i + \delta\, AgeEligible_i * Post_t + \gamma_t + \varepsilon_{it} \tag{3}$$

where *Post* indicates a year following the elimination of the YCTC work requirement.

We estimate that the removal of the YCTC work requirement led to a reduction in mothers' labor force participation of 0.001 percentage points (0.1 basis points), with a 95% confidence interval ranging from -0.28 to 0.28 percentage points. This estimate represents the cumulative effect of eliminating the extensive-margin substitution effect associated with the pre-reform policy and imposing the extensive-margin income and substitution effects associated with the post-reform policy. Hence, the estimated effect in Column 3 provides evidence against the hypothesis that eliminating the YCTC work requirement caused a substantial number of mothers to exit the labor force.[26]

One factor that may help explain why removing the YCTC work requirement did not lead to a substantial reduction in labor force participation is that the YCTC was itself a new policy. Immediately after enactment, taxpayers may not have been aware of the credit and therefore failed to consider its work requirement when making their labor participation decisions. We assess this possibility in two ways. First, although we lack direct evidence on taxpayers' knowledge of the YCTC, we can indirectly estimate YCTC take-up based on public reports from the California agency that administers the credit. Among federal tax

[26] Appendix Table A.3 reports three robustness checks. Panel A limits our sample to the 98% of taxpayers with a valid Social Security Number to ensure that changes in YCTC eligibility for non-citizens do not bias our results. Panel B limits our sample to the 84% of mothers who claimed the child on their prior-year tax return. Third, recall that the YCTC work requirement was in effect in 2021, but the age-eligibility threshold was conflated by the federal CTC expansion. To incorporate that year into our analysis, Panel C uses mothers in states without child tax credits as a control group, i.e., a triple-difference specification. For each exercise, we obtain results similar to those from our baseline analysis.

filers (a group composing 93% of our sample), we estimate high rates of YCTC take-up, rising from approximately 84% in 2019 to 97% by 2020 (Appendix Table A.4). Although claiming a tax credit is not a perfect proxy for awareness—guided tax preparation software may result in taxpayers claiming credits of which they are unaware—taxpayers may learn about a credit through the process of claiming it on their return or through their interactions with tax preparers (Chetty and Saez, 2013).

Second, if the recency of the YCTC's adoption is an important contributor to the small effects it generates, we would expect to see larger effects of the work requirement over time as a growing number of taxpayers become aware of the credit and incorporate it into their decision-making (Chetty, Friedman and Saez, 2013; Garin, Jackson and Koustas, Forthcoming).[27] Appendix Table A.5 estimates the effect of YCTC eligibility separately for each year in which the work requirement was in effect. To account for the possibility that some taxpayers only learned about the policy in its third year, we include 2021 in this analysis. However, because the federal CTC provided different benefits for the age-eligible versus age-ineligible mothers in our sample during 2021, for this analysis we also include mothers in other states as an additional control group. In all three years, we estimate a very small effect of age-eligibility on maternal labor force participation, with no gradient that would indicate increasing effects of the work requirement over time.

A distinctive feature of the YCTC work requirement was that a taxpayer would qualify for the full credit amount as long as they earned at least $1 during the tax year. A natural question to ask is how the effects of a policy with this design would compare to the effects of a policy where benefits were conditioned on a higher earnings threshold. We formally explore this question in Appendix B. Under mild conditions, we show that the effect of a work requirement on labor force participation is weakly decreasing in the applicable earnings threshold. Intuitively, individuals who are marginal labor force participants with respect to

[27] A different factor that could contribute to a smaller effect in the credit's first year is if there are fixed costs to adjusting labor supply; parents might find it worthwhile to incur the cost to receive the credit for two years, but not for one.

a higher-dollar threshold would also be marginal with respect to a lower-dollar threshold, but not necessarily vice-versa.[28] Consequently, we interpret the effect of eliminating the YCTC's work requirement as an upper bound for the effects of alternative work requirements tied to higher earnings thresholds. In the next subsection, we report results for the Colorado child tax credit, which employs a work requirement that more closely mirrors the federal CTC.

## 5.2 Other State Child Tax Credits

We now turn from California to other states that offer child tax credits for parents of young children. These credits differ from one another in that one state (Colorado) conditions credit eligibility on work, whereas four states (Maryland, New Jersey, Vermont, and Oregon) do not. Unlike with the California YCTC, these other state credits do not vary during our sample period with respect to whether they condition eligibility on work. For each state, we select the MSE-minimizing local polynomial order and bandwidth from applying the placebo-based tuning analysis described in Section 4 (see Appendix Figure A.7 and A.8 for details). Sample characteristics are provided in Appendix Table A.6.[29]

Table 2 reports the effect of these five state credits on labor force participation. We begin with Colorado, which conditioned credit eligibility on work during 2022 and 2023. In these years, the Colorado CTC's design mirrored the federal CTC; parents were required to work in order to qualify for its benefits, with benefit amount gradually phasing in based on the parent's earned income. As with the YCTC when a work requirement was in place, we estimate that eligibility for the Colorado CTC did not substantially increase maternal labor force participation; our estimated 95% confidence interval excludes increases of 0.39 percentage points or greater (Column 1).[30]

[28] For example, the federal CTC was initially available only to taxpayers with earned income above $10,000; any taxpayer who chose to work because of that incentive would also presumably decide to work if the required earnings threshold was reduced to $5,000. But some of those taxpayers might not choose to work if the required earnings threshold was instead raised from $10,000 to $15,000.

[29] Unlike with the California YCTC, our analyses in this section do not exclude mothers with younger children, since the other state credits are available on a per-child basis.

[30] As with California, we do not observe an increase in the effect of Colorado's work requirement over time;

Columns 2 through 5 of Table 2 report results for the four states that provided tax credits to parents of children under the age of six that were not conditioned upon work. As discussed in Section 3, these states vary from one another with respect to the maximum credit amount as well as with respect to the income range for which the credit is available. For New Jersey and Vermont, we have two years of available outcome data (2022 and 2023), whereas for Maryland and Oregon we observe outcomes for 2023 alone. In none of these states do we find evidence that the credit reduced labor force participation: the estimated reductions range from 0.05 to 0.54 percentage points, and none is statistically significant. Combining these estimates across states (via a population-weighted average) yields a 95% confidence interval of -0.28 to 0.17 percentage points.

Taken together, the estimates from the five state CTCs considered in this subsection are consistent with our finding from California that eliminating a child tax credit work requirement does not cause a substantial number of mothers to exit the labor force.

## 5.3 Robustness and Placebo Checks

We next conduct a range of analyses to investigate the validity of the identifying assumptions underlying our analyses. Our first identifying assumption requires that, but for the policy, labor force participation would be the same on average between the age-eligible and the age-ineligible mothers in each policy period. This assumption would be violated, as noted above, if the mothers of children born on either side of the age cutoff differed systematically in their propensity to work for reasons unrelated to the state CTC, such as from compositional differences in the timing of births. The assumption would also be violated if other relevant policies differed across the same age cutoff–such as age cutoffs for school entry or eligibility for other young child benefits. We are unaware of any other policies affecting the mothers in our sample that rely on this age cutoff, other than the 2021 federal CTC expansion described above.

see Appendix Table A.7.

One way to empirically assess the validity of this assumption is to investigate differences in potential outcomes across the age-eligibility threshold in pre-period years before a state adopts its child tax credit. We previously reported results for this exercise for California in Appendix Table A.1; we report corresponding results for the other states in Appendix Table A.8.

Appendix Table A.9 presents two additional placebo analyses. Panel A repeats the analysis in Table 1 for mothers living in states that did adopt a tax credit for children below the age of six.[31] A second placebo test takes advantage of the fact that, in California, the YCTC provides a $1,000 credit per tax return, not per child. As such, mothers of children near the age cutoff who also have a younger sibling see no difference in their YCTC eligibility in the year that the older child turn six. Panel B of Appendix Table A.9 replicates Table 1 for this placebo group of California mothers. The results from these two analyses do not provide evidence of differences in labor force participation across the age-eligibility threshold that are unrelated to eligibility for the credit.

The second identifying assumption underlies the interpretation of our difference-in-discontinuities analysis for California, and requires that the average effect of the work requirement on labor force participation be the same in both policy periods. This assumption would be violated, for example, if the labor force environment differed across time periods in ways that could exacerbate or mute the effects of the work requirement. Of particular concern in our setting is that our study period for our California analyses overlaps with the onset of the COVID-19 pandemic in 2020. To assess this issue, Appendix Table A.10 repeats the main difference-in-discontinuities specification from Table 1 excluding 2020 (the first year of the pandemic) or excluding 2020-2022 (the years for which the pandemic was most disruptive); these analyses yield nearly identical results as our main specification. Along similar lines, we compare the effect of the work requirement across the two years of the policy period for which it was in effect—2019 versus 2020—and find no evidence that the effect of the policy varied across

[31] Appendix Figure A.9 plots the distribution of pseudo-effect estimates from these states; the distribution is centered at zero.

these years.[32] Finally, we plot the distribution of difference-in-discontinuities estimates for the placebo states that did not adopt a child tax credit for children under the age of six. Appendix Figure A.10 shows that the distribution of these estimated pseudo-effects is centered at zero, consistent with an absence of differential trends in labor force participation between the age-eligible and age-ineligible groups during our sample period.

Finally, we consider the sensitivity of our results to alternative RD specifications with similar performance according to our placebo-based tuning analysis. For California, recall that our preferred specification entails a simple comparison of means; however, Figure 2 showed that a local linear specification with an 18-month bandwidth achieved nearly as low of a mean-squared error.[33] Despite substantial differences from our preferred specification (linear polynomial versus simple comparison of means; 18-month versus 4-month bandwidth), this alternative specification yields estimates that are very similar to our main results (see Appendix Table A.11). In contrast, the local linear specification employed in recent papers that exploit quasi-random variation in birth timing (e.g., Wingender and LaLumia, 2017; Barr, Eggleston and Smith, 2022; Garin, Jackson and Koustas, Forthcoming; Rittenhouse, 2023; Bhardwaj, 2023; Lippold and Luczywek, 2024) involve much smaller bandwidths that, according to our placebo-based tuning analysis, tend to yield larger mean-squared errors in our setting. We find that the local linear specification with a 1-month bandwidth used by Barr, Eggleston and Smith (2022), for example, yields an imprecise null for the effect of eliminating the YCTC work requirement, with an estimated confidence interval an order of magnitude larger than that obtained by our preferred specification (Appendix Table A.12).

[32]The onset of the COVID-19 pandemic is of less concern for our analysis of the tax credits from states other than California because those credits were enacted in 2022 and 2023, after the most disruptive period of the pandemic concluded.

[33]We obtain a similar result when we apply the PBT approach to two-year placebo data periods, formed from adjacent pre-period tax years (Appendix Figure A.11). An additional consideration that might support a linear time trend is that zooming out from the 4-month bandwidth employed in our main California specification provides evidence that maternal labor force participation gradually declines with respect to child birth date (Appendix Figure A.12); a linear specification that accounts for that trend may yield lower bias (even while generating higher variance).

## 5.4 Heterogeneity of Estimated Treatment Effects

This subsection explores heterogeneity in our estimated treatment effects with respect to income and marital status.

**Income** Although the tax credits we study shape labor supply incentives throughout the income distribution, the effects may be particularly strong for lower-income individuals, for whom the credit amounts constitute a larger share of after-tax income. To investigate this possibility, we replicate our main analysis separately for taxpayers who had prior-year income below or above $30,000.[34] We find no evidence of substantial labor force participation effects for low-income mothers in California (Appendix Table A.13) or in the other states we consider (Appendix Table A.14).

**Marital Status** Another potential source of heterogeneity relates to marital status. Both California and Colorado's work requirements were applied at the tax return level; as such, a non-working mother could qualify for the credit if her spouse earned positive income during the tax year. For this reason, work requirements may have a smaller effect on labor force participation for some married as compared to unmarried mothers. At the same time, prior literature suggests large differences in labor supply elasticities of married versus unmarried women, with married women being more elastic on the extensive margin (e.g., Eissa and Hoynes, 2004). Appendix Tables A.15 and A.16 replicate our main results separately for single and married mothers for California and the other states we consider. We find no systematic treatment effect differences between these groups.

## 5.5 Alternative Samples with Non-Working Mothers

Our main sample consists of mothers who, by construction, were already attached to the labor force. As such, our main estimates primarily reflect the effect of work requirements

[34] We consider other income splits, corresponding to phase-out ranges for each state credit, when we estimate elasticities in Section 6, below.

on labor force exit. However, the work requirement could also shape the degree to which non-working mothers enter the labor force. To study both of these flows, we supplement our main analysis with two alternative samples.

The first alternative sample we construct consists of mothers of children enrolled in Medicaid or CHIP at some point during the prior year, regardless of whether or not the mother was working (see Section 3 for details). Appendix Table A.17 presents summary statistics for the Medicaid sample. Across states, roughly two-third of the mothers in this sample worked during the prior year.

Panel A of Appendix Table A.18 replicates our California analyses for the Medicaid sample. As with our main sample, we estimate small and statistically insignificant effects of the YCTC on labor force participation, both before and after the elimination of the work requirement. The 95% confidence interval for the difference-in-discontinuities estimate excludes reductions in labor force participation of 0.4 percentage points or more.

For our California analysis, we additionally replicate our results with a second alternative sample, using administrative data accessed through the U.S. Census Bureau. We use this data to identify the universe of children born in California who turned six around the end of one of our outcome years. Construction of this sample relies on the Social Security Administration's Numident file, the Census Household Composition Key, filed tax returns, and third-party wage and salary income information returns (Form W-2). In the Census sample, we cannot link parents to information returns other than the W-2s, meaning our employment measure is limited to wage earnings and does not consider self-employment. Like the Medicaid sample, the Census sample has the advantage of allowing us to identify children likely residing in California during the relevant outcome years without needing to condition on mothers' prior employment and tax filing.

Panel B of Appendix Table A.18 presents results for the Census sample. As with our main sample, we observe a very small and statistically insignificant effect of the YCTC on labor force participation when the work requirement was in effect, but with this sample we observe a modest (0.27 percentage point) but statistically significant increase in labor participation after the work requirement is eliminated.

Turning from California to the other states, Appendix Table A.19 replicates the analyses in Table 2 using the Medicaid sample. In all states, the estimated effect sizes are small in magnitude (less than one percentage point). Across the four states that did not condition credit eligibility on work, we find no significant effects. In Colorado, where the credit conditioned benefit eligibility on work, we estimate that the credit led to a 0.77 percentage point increase in labor force participation, although the estimate is only marginally significant.

Overall, the results from these alternative samples are largely consistent with those from our main sample, with two potential exceptions: the positive labor participation effects from California's YCTC without a work requirement in the Census sample, and from the Colorado CTC with a work requirement in the Medicaid sample. The contrast between these results and those from our main sample raises the possibility that the tax credits may have affected the share of non-working mothers entering the labor force.

We investigate this possibility for California in Table 3 and for other states in Table 4, by splitting the sample by prior year non-workers and prior-year workers. Consistent with our main sample, we estimate small and statistically insignificant effects for mothers that were working during the prior year. In contrast, for both California and Colorado, we find some evidence that credit eligibility increases labor force participation among mothers that were not working in the prior year. Notably, the finding of a positive effect on labor force participation is more consistent in California after the work requirement is eliminated.[35] Why might credit eligibility increase labor force participation absent a work requirement? A positive effect on labor participation could be due to a positive income effect—e.g., from eliminating financial barriers to work. Alternatively, it could reflect a shift from informal to formal sources of employment associated with one's decision to start filing a tax return in

[35] For the Medicaid sample, we estimate positive and statistically significant effects both before and after the elimination of the work requirement. For the Census sample, we do so only after the elimination of the work requirement. For other states without a work requirement, we find a positive and marginally significant effect in Vermont, and no statistically significant effect in Maryland, New Jersey, or Oregon.

order to claim the credit.

Because the positive estimated effects for non-working mothers are not limited to time periods for which the work requirement was in effect, the difference-in-discontinuities results for California non-working mothers are either zero (Medicaid sample) or positive (Census sample). We interpret these results to suggest that while the availability of the tax credit may increase labor force participation for non-working mothers, the mechanism through which it does so is unlikely to be a work requirement.

### 5.6 Effects on Reported Income and Tax Filing

Our primary measure of labor force participation is based on third party information returns, rather than income reported on the taxpayer's return. This measure has the advantage of being available regardless of whether an individual files a tax return; hence, any effect we observe is likely to represent a real change in labor supply rather than change in what a taxpayer reports on her return.[36] At the same time, measuring income solely based on third-party information could lead us to miss changes in labor income that are not reported by third parties – either because no third party is required to report it or because a third party is required to report it but fails to do so.[37] A taxpayer who earns income that does not appear on an information return may be particularly inclined to report it when doing so qualifies her for a state CTC.

Beginning with California, Appendix Table A.20 replicates the analyses in Table 1 using reported income as the outcome, rather than income measured by third party information returns. Specifically, Panels A through C respectively consider the effects of YCTC age-eligibility on whether the mother (along with her spouse, if married and filing a joint return) reported any income from wages, any income from self-employment, or any income from either wages or self-employment. For each outcome, the measure takes a value of zero if the

[36]Garin, Jackson and Koustas (Forthcoming) find that some taxpayers increase reported self-employment income to maximize tax benefits like the CTC and EITC without actually increasing their labor incomes.

[37]An example of labor income unlikely to show up on third party information returns are the payments a sole proprietor receives from payors below the minimum reporting threshold of $600 per year.

mother did not file a tax return. Across measures, we find no evidence that the removal of the YCTC work requirement led to a reduction in reported income (Column 3). We observe similar results for the other state child tax credits for which reported income data is available after the credit is adopted (Colorado, New Jersey, and Vermont); see Appendix Table A.21. We interpret these results to suggest that the estimates based on information returns are unlikely to be obscuring reductions in labor income.

Appendix Table A.22 reports the estimated effect of California's YCTC on tax filing. We find that eliminating the YCTC work requirement leads to a 0.4 percentage point increase in the filing rate. Perhaps surprisingly, this filing effect appears to be only partly driven by an increase in filing among taxpayers without earned income (i.e., those newly eligible for the expanded credit), as suggested by the increase in the share of taxpayers filing and reporting positive earnings (Panel C). It may be that eliminating the work requirement increases tax filing by simplifying the YCTC eligibility rules and thereby increasing the perceived benefit to filing a return—a "woodwork effect" (c.f., Anders and Rafkin, Forthcoming). However, we do not observe evidence of an effect on tax filing when we evaluate the other state tax credits that were in place for tax year 2022 (Appendix Table A.23).

Finally, note that the YCTC work requirement provides an incentive for individuals who would otherwise drop out of the labor force to report at least a small amount of earned income, which would allow them to qualify for the full YCTC benefit. We investigate the degree to which taxpayers respond to this incentive by exploring the effect of the credit on the number of taxpayers reporting very low but positive incomes (Appendix Table A.24).[38] We observe a small but statistically significant effect of the YCTC with a work requirement on the share of taxpayers who report very low earned income (amounts below $10,000). Interestingly, Appendix Figure A.13 provides suggestive evidence that this (small) effect stems from already-working taxpayers who reduce the income they report in response to

[38]A potential concern with this analysis is that the taxpayer may report the income on a state but not federal return. In principle, taxpayers are not required to file a federal return to claim the YCTC. In practice, we expect the vast majority of state filers to report their income on a federal return due to the design of tax preparation software and the modern electronic filing system.

the credit's availability or its phase-out (i.e., an intensive-margin income or substitution effect). In particular, age-eligibility for the YCTC appears to increase the share of taxpayers reporting very low incomes and reduce the share of taxpayers reporting other positive incomes (Panels A and B). Consistent with this effect being driven by the credit's availability or phase-out, we find no evidence that the share of taxpayers reporting very low incomes is affected by the elimination of the work requirement (Panel C).[39]

# 6 Labor Supply Elasticities

To facilitate extrapolating our results to other policy settings, we use our estimates to calculate extensive-margin substitution and income elasticities. As a first step, consider the mix of income and substitution effects generated by each type of child tax credit.

First, consider a credit with a work requirement. For individuals whose income from work would fall below the start of the income phase-out, the credit raises the return to work. At the same time, the credit does not affect the after-tax income associated with not working. The credit therefore generates a pure substitution effect with respect to the extensive-margin of labor supply.[40] For individuals with income within the credit's phase-out region, the nature of the labor supply incentive effect is the same, but the magnitude is lower. In contrast, the credit generates no extensive-margin labor supply incentive effects for individuals whose income from work would fall above the top of the credit's phase-out range.[41]

The extensive-margin incentive effects are quite different for a credit that is not conditioned on work. For individuals whose income from work would fall below the start of the

[39]In a related setting, Cook and East (2024) find that SNAP recipients do not slightly increase work effort to maintain benefit eligibility when faced with work requirements.

[40]For very-low income individuals, these effects are larger for a credit with a sharp work requirement (like California) compared to a credit for which benefits gradually phase in by income (like Colorado); see Appendix B.

[41]Although not our focus, the presence of an income phase-out generates *intensive-margin* substitution effects—taxpayers with income above the start of the phase-out face an incentive to reduce their income—and income effects—working taxpayers with higher after-tax income may reduce their labor income to consume more leisure.

credit's income phase-out, the credit generates an extensive-margin income effect, in that it raises individuals baseline after-tax income whether or not they work. For individuals whose income from work would be above the start of the credit's income phase-out, the credit generates a negative extensive-margin substitution effect—the credit increases the after-tax income associated with not working compared to working. This reduction in the return to work is larger for individuals with income above the top of the phase-out range—who qualify for no credit—compared to individuals whose income would fall within the credit phase-out range.

Considering these varying incentive effects, to calculate the extensive-margin substitution elasticity, we draw on the estimated labor force participation effects for two subgroups of taxpayers for whom the credit generates a pure substitution effect with respect to the extensive-margin of labor supply: (1) individuals facing a work requirement credit whose prior-year income was below the bottom of the credit's income phase-out range; and (2) individuals facing a credit without a work requirement whose prior-year income was above the top of the credit's income phase-out range. We scale the estimated labor force participation effect for each of these groups by the percent change in the return to work induced by the applicable credit, accounting for pre-tax income, federal and state income and payroll taxes (calculated using NBER's TAXSIM program), and SNAP benefits. Details are provided in Appendix C.

The estimates used to construct the extensive-margin substitution elasticity are reported in Appendix Table A.25. On average, we estimate that the credits with work requirements increased the return to work by an average of 6.8% for taxpayers with incomes below their phase-out ranges, whereas the unconditional credits reduced the return to work by an average of 1% for taxpayers with incomes above their phase-out ranges. Scaling our estimated labor force participation effects by this percentage change in the return to work yields an average (population-weighted) extensive-margin substitution elasticity of 0.047.[42]

[42]The population-weighted average of our estimated extensive-margin substitution elasticities for the conditional credits is 0.050, compared to 0.019 for the unconditional credits.

We similarly calculate extensive-margin income elasticities by scaling our estimated labor force participation effects by the percentage change in after-tax income from not working (Appendix Table A.26). Absent the credit, we assume that non-workers receive benefits only from SNAP, abstracting from smaller safety net programs like TANF or non-market income such as charitable support or gifts from family members. As described above, for this exercise we rely on the estimated effects for the credits that are not conditioned on work, from taxpayers with incomes below the start of the applicable phase-out range. Across states, we estimate that the credits increase the after-tax income associated with non-working by a population-weighted average of 130%. Scaling our point estimates by this change yields a very small extensive-margin income elasticity, less than 0.01.

# 7 Conclusion

We study the labor supply consequences of conditioning child tax benefits on work. The question of whether to limit eligibility for child tax benefits to taxpayers with labor income is a central issue in current policy debates at both the federal and state levels. Our empirical design identifies this effect for mothers of young children—a group for whom the long-term benefits of safety net program eligibility is likely to be particularly large. The results of our analysis suggest that requiring these mothers to work in order to receive benefits is unlikely to be an effective means of increasing labor force participation in the policy setting we consider.

Our most direct evidence for the effect of eliminating a work requirement comes from California, which underwent the policy change that we are interested in. A potential concern with drawing broader conclusions from California's experience is that the design of the YCTC differs from the design of other state and federal child tax credits, particularly in terms of the starkness of its work requirement. However, the results from the other states we study suggest that the observed effects of the YCTC are not due to its distinctive design. In particular, we observe comparable results from the YCTC prior to its work requirement being eliminated as from the Colorado CTC—which has a more conventionally designed work requirement and benefit schedule. Similarly, we observe comparable results from the YCTC after the elimination of its work requirement as from the other states that never conditioned benefit eligibility on work. The similarity of the YCTC to these other states increases our confidence in extrapolating from California's experience to other policy settings.

That being said, there are several related reasons to be cautious before applying our results to policy reforms that would dramatically alter the relationship between benefit program eligibility and work in the United States. First, all of the state credits we study operate in the shadow of the EITC and the large work incentives it generates. It may be that adding or subtracting smaller incentives on top of those generated by the EITC does not much change taxpayers' perceptions about the return from work (c.f., Chetty, 2012). Consistent with this hypothesis, recent EITC reforms have been found to have had smaller effects on labor force participation compared to earlier, larger expansions (Lin and Tong, 2017; Schanzenbach and Strain, 2021).[43]

Second, taxpayers may not have responded to the credits we study because they (and their eligibility rules) were not fully salient. Our results may therefore not reflect individual behavior under perfect optimization. However, for purposes of extrapolating our results to other child tax benefits, this aspect of the credits is arguably a feature, not a bug, as many other tax benefits would also not be fully salient. Even the federal EITC's work incentives are not salient for many taxpayers (Chetty and Saez, 2013; Chetty, Friedman and Saez, 2013).

Third, our estimated (non)response to unconditional child tax credits may understate how much taxpayers would respond to much larger unconditional income grants that come

[43]With respect to the earlier EITC expansions, recent studies have come to mixed conclusions regarding the magnitude of their labor supply effects: compare Kleven (2023) and Looney (2024) with Schanzenbach and Strain (2021) and Chetty, Friedman and Saez (2013). With respect to work subsidies for taxpayers not claiming children, Miller et al. (2018) finds a labor supply effect whereas Meer and Witter (2023) and Yang et al. (2022) do not.

closer to covering the cost of living. For example, Balakrishnan et al. (2024) and Vivalt et al. (2024) document negative employment effects from a \$500 to \$1,000 *per month* universal basic income pilot, although the effects are smaller for parents than non-parents. In contrast, Jones and Marinescu (2022) and Akee et al. (2010) find no labor supply effects of Alaska's guaranteed income program or government transfers tied to casino profits, respectively, which provide unconditional benefits that are closer in size to the credits we study. In light of these considerations, our main conclusion is that marginally reducing pro-work incentives in child tax benefits is unlikely to substantially reduce labor force participation, especially if meaningful work requirements continue to exist for other safety net programs like the EITC.

Finally, with respect to methodology, several of our findings challenge existing best practice recommendations for RD practitioners. First, we find strong performance from simple comparisons of means, without polynomial adjustments to the running variable. This specification consistently yields the lowest MSE in our actual data and simulations, but is not typically considered in applications and is recommended against by experts (Imbens and Kalyanaraman, 2012). A related point is that we find important benefits from jointly optimizing over bandwidth and polynomial order—as advocated by Hall and Racine (2015) in a related context—which is not typically done in practice. Second, despite the theoretical advantages of triangular kernels for local polynomial estimation, we do not observe that they lead to systematic performance benefits relative to uniform kernels, which are simpler to estimate and interpret. Third, our placebo-based method consistently yields larger optimal bandwidths than those recommended by state-of-the-art approaches like Imbens and Kalyanaraman (2012) and Calonico, Cattaneo and Titiunik (2014); this aligns with simulation evidence reported in Card et al. (2017). These results suggest that researchers applying RD designs may benefit from broadening the range of empirical specifications they consider, beyond those currently recommended as best practice.

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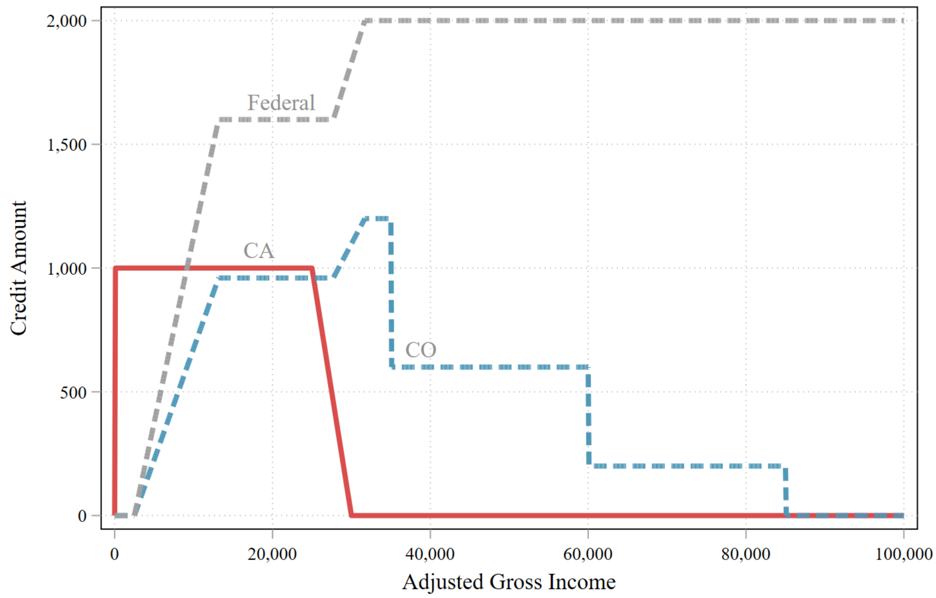
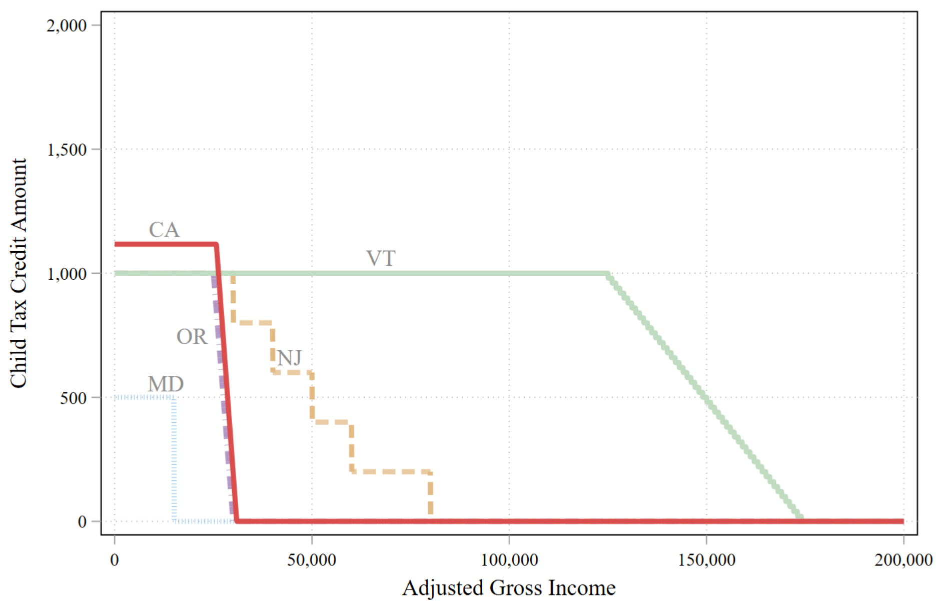
Figure 1: Benefit Schedules: Federal and State CTCs

(a) Work Requirement

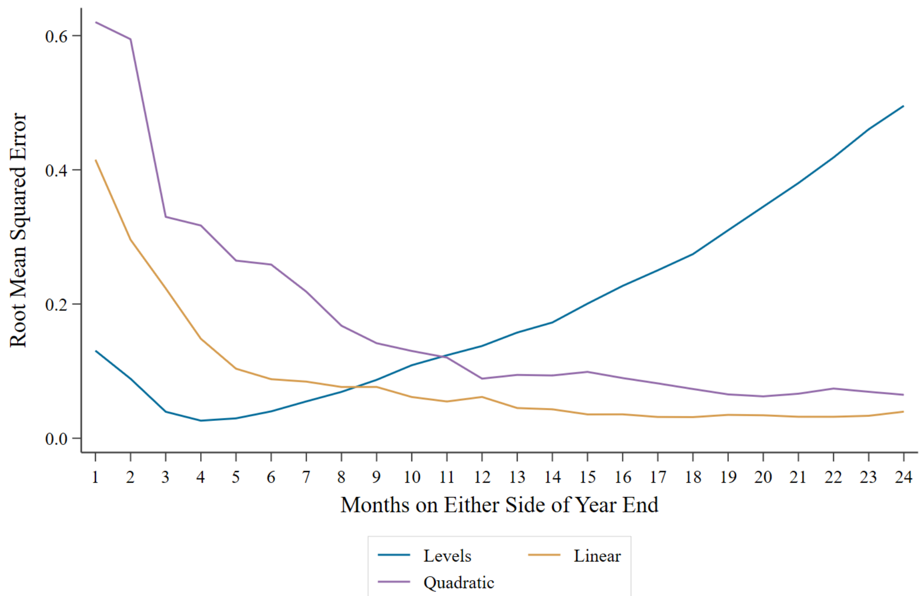



(b) No Work Requirement




Notes: The figure presents the benefit schedule for the federal CTC and the six states that provide child tax credits for parents of children under six. Panel A shows the benefit schedule for credits with work requirements: the federal CTC, the Colorado CTC, and the California YCTC prior to 2022. Panel B shows the benefit schedule for credits without work requirements: Maryland, New Jersey, Oregon, Vermont, and California after the work requirement was removed. Dollar amounts are for tax year 2023, except for the California YCTC with work requirement, for which the reported dollar amount is for 2020. Each calculation assumes a taxpayer who files a joint return, claims a single qualifying child who is under age six, and reports only earned income.

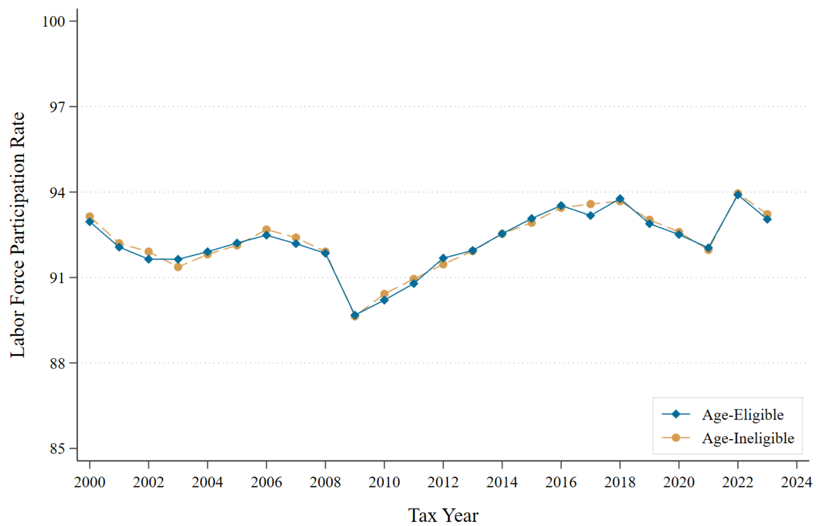
Figure 2: Placebo-Based Tuning Analysis: California



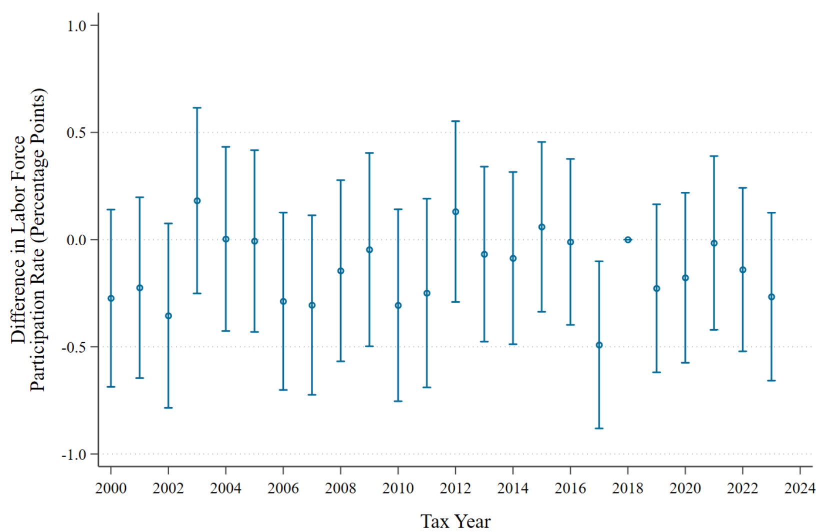

Notes: The figure reports the results of the Placebo-Based Tuning Analysis for our California analysis described in Section 4.2. It compares the root mean squared error (RMSE) of the distribution of estimated (placebo) effects for different empirical specifications. Each placebo effect is obtained from estimating the pseudo-effect of age-eligibility for the California YCTC following equation (1), for each year from 2005 through 2018, in a specification using a local polynomial with the specified bandwidth and order, with uniform weights, and with no "donut" exclusion. Each estimate is obtained from a sample composed of California mothers whose youngest child turns six within the specified bandwidth (i.e., the number of months on either side of the end of the year). The reported RMSE corresponds to the square root of the average (across years) of the square of the estimated coefficients from each year. The purple line includes a quadratic polynomial in date of birth; the yellow line includes a linear trend in date of birth; and the blue line consists of a simple comparison of means of the age-eligible and age-ineligible groups of mothers.

Figure 3: Labor Force Participation by Eligibility for California YCTC

(a) Labor Force Participation Rate



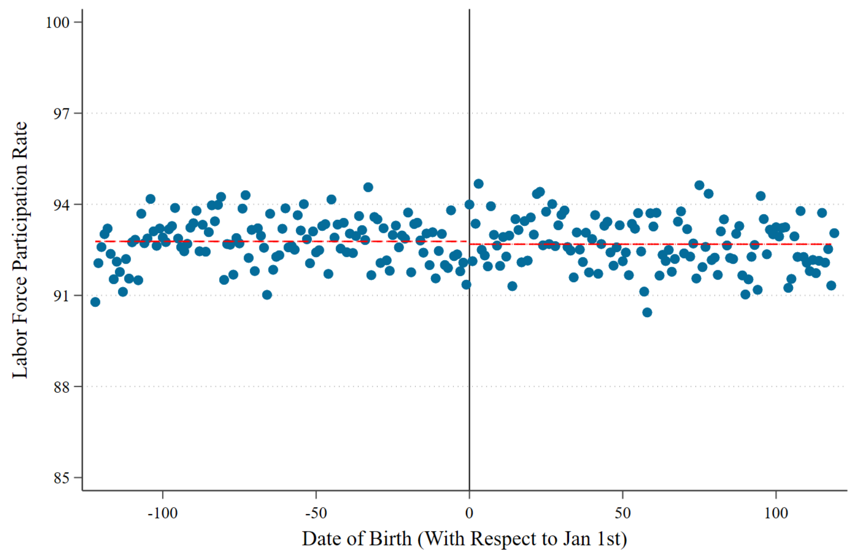

(b) Differences by Age-Eligibility



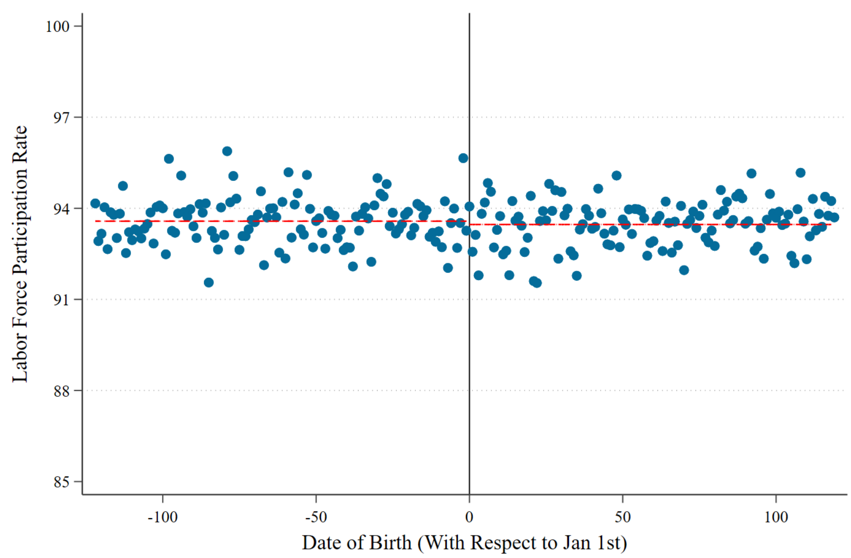

Notes: Panel A reports labor force participation rates by year for recently working California mothers by age-eligibility for the YCTC. Mothers whose youngest child turns six during the last four months of a year (age-ineligible group) are reported in yellow; mothers whose youngest child turns six during the first four months of the subsequent year (age-eligible group) are reported in blue. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of the specified year. Labor force participation is defined as having positive wage or self-employment income reported on third-party information returns. Panel B reports the differences in maternal labor force participation rates between age-eligible and age-ineligible groups. Coefficient estimates are reported in percentage points (0-100). Bars represent estimated 95% confidence intervals, derived from heteroskedasticity-robust standard errors.

Figure 4: Labor Force Participation of California Mothers by Child's Date of Birth

(a) Work Requirement Period (2019-2020)




(b) No Work Requirement Period (2022-2023)




Notes: The figure reports mean labor force participation rates binned by child's date of birth for recently working California mothers. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of the year (2019 or 2020 in Panel A; 2022 or 2023 in Panel B). Mothers whose youngest child turns six during the last four months of a year (age-ineligible group) are assigned a negative value for date of birth; mothers whose youngest child turns six during the first four months of the subsequent year (age-eligible group) are assigned a positive value for date of birth. For example, a child born on January 10 would have a date of birth value of 9. Labor force participation is defined as having positive wage or self-employment income reported on third-party information returns. The horizontal lines correspond to the estimated means for the age-eligible and age-ineligible groups.

Table 1: California YCTC Eligibility and Labor Force Participation

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| Coefficient | -0.113 | -0.114 | -0.001 |
| | (0.103) | (0.099) | (0.143) |
| 95% CI | [-0.32, 0.09] | [-0.31, 0.08] | [-0.28, 0.28] |
| Control Mean | 92.8 | 93.6 | 93.2 |
| Observations | 251,699 | 244,828 | 496,527 |

*Notes:* The table reports the estimated effect of eligibility for the California YCTC separately during the work requirement period (Column 1) and period without a work requirement (Column 2). Column 3 corresponds to the difference in estimated effects between Columns 1 and 2. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100). The sample consists of recently working mothers in California whose youngest child's sixth birthday falls within the four-month window around the end of the specified year. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table 2: Other State CTC Eligibility and Labor Force Participation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| Coefficient | 0.032 | -0.052 | -0.033 | -0.069 | -0.535 |
| | (0.184) | (0.237) | (0.128) | (0.330) | (0.393) |
| 95% CI | [-0.33, 0.39] | [-0.52, 0.41] | [-0.28, 0.22] | [-0.72, 0.58] | [-1.30, 0.23] |
| Control Mean | 93.2 | 94.8 | 95.3 | 93.4 | 94.7 |
| Observations | 74,873 | 138,541 | 441,346 | 91,310 | 13,746 |
| Bandwidth (Months) | 5 | 19 | 24 | 24 | 9 |
| Polynomial Order | 0 | 1 | 1 | 1 | 0 |
| Work Req | Yes | No | No | No | No |
| Max Credit ($) | 1,200 | 500 | 500-1,000 | 1,000 | 1,000 |
| Years | 2022-2023 | 2023 | 2022-2023 | 2023 | 2022-2023 |

*Notes:* The table reports the estimated effect of eligibility for the state child tax credits for states other than California. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100). The sample consists of recently working mothers in the specified state who have a child whose sixth birthday falls within the specified bandwidth around the end of the specified year. Polynomial order 0 refers to a simple comparison of means; polynomial order 1 refers to a specification that includes a linear trend in the child's date of birth. Work req indicates whether eligibility for the credit is limited to parents with income from work. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table 3: California YCTC Eligibility and Labor Force Participation by Prior-Year Employment

| | Medicaid Sample | | | Census Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(5) - (4)] |
| **Panel A: Worked in Prior Year** | | | | | | |
| Coefficient | -0.200 | -0.241 | -0.040 | 0.029 | 0.021 | -0.008 |
| | (0.167) | (0.166) | (0.235) | (0.097) | (0.088) | (0.130) |
| 95% CI | [-0.53, 0.13] | [-0.57, 0.08] | [-0.50, 0.42] | [-0.16, 0.22] | [-0.15, 0.19] | [-0.26, 0.25] |
| Control Mean | 90.3 | 90.8 | 90.5 | 92.9 | 92.7 | 92.8 |
| Observations | 127,145 | 123,325 | 250,470 | 280,000 | 350,000 | 630,000 |
| **Panel B: Did Not Work in Prior Year** | | | | | | |
| Coefficient | 0.645** | 0.823** | 0.178 | 0.025 | 0.655*** | 0.630** |
| | (0.310) | (0.343) | (0.462) | (0.217) | (0.190) | (0.288) |
| 95% CI | [0.04, 1.25] | [0.15, 1.49] | [-0.73, 1.08] | [-0.40, 0.45] | [0.28, 1.03] | [0.07, 1.19] |
| Control Mean | 19.1 | 22.0 | 20.5 | 16.3 | 18.9 | 17.8 |
| Observations | 65,089 | 59,088 | 124,177 | 120,000 | 170,000 | 290,000 |

*Notes:* The table replicates the analysis from Table 1 separately for mothers who worked during the prior year (Panel A) and for mothers who did not work during the prior year (Panel B). A mother is treated as having worked during the prior year if she has positive wage or self-employment income for that year reported on third-party information returns. Columns 1-3 report results from the Medicaid sample, which is comprised of mothers of children enrolled in Medicaid in the state of California at any point during the prior year. Columns 4-6 report results from the Census sample, which is comprised of birth mothers of children born in California; birth mothers are identified from the Census Household Composition Key (CHCK) and birth location is according to the Social Security Administration (SSA) Numident file. For the Medicaid sample, the outcome in each column is an indicator for having positive wage or self-employment income reported on a third-party information return. For the Census sample, the outcome is an indicator for positive wage income reported on a W-2 form. All counts and estimates are rounded per Census's disclosure rules governing administrative records. The Census Bureau has reviewed results from the Census sample to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product (Data Management System (DMS) Number: P-7503840, Disclosure Review Board (DRB) approval number: CBDRB-FY24-SEHSD003-066.) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Other State CTC Eligibility and Labor Force Participation by Prior-Year Employment

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| Panel A: Worked in Prior Year | | | | | |
| Coefficient | 0.081 | -0.155 | 0.096 | -0.207 | -0.723 |
| | (0.317) | (0.407) | (0.242) | (0.507) | (0.670) |
| 95% CI | [-0.54, 0.70] | [-0.95, 0.64] | [-0.38, 0.57] | [-1.20, 0.79] | [-2.04, 0.59] |
| Control Mean | 90.9 | 92.2 | 92.8 | 91.3 | 91.7 |
| Observations | 32,744 | 67,485 | 178,270 | 49,693 | 7,052 |
| Panel B: Did Not Work in Prior Year | | | | | |
| Coefficient | 0.959 | -1.400 | -0.352 | -0.074 | 3.055* |
| | (0.727) | (1.010) | (0.603) | (1.074) | (1.562) |
| 95% CI | [-0.47, 2.38] | [-3.38, 0.58] | [-1.53, 0.83] | [-2.18, 2.03] | [-0.01, 6.12] |
| Control Mean | 26.2 | 20.8 | 24.1 | 22.2 | 23.7 |
| Observations | 14,842 | 24,466 | 77,164 | 23,488 | 3,091 |

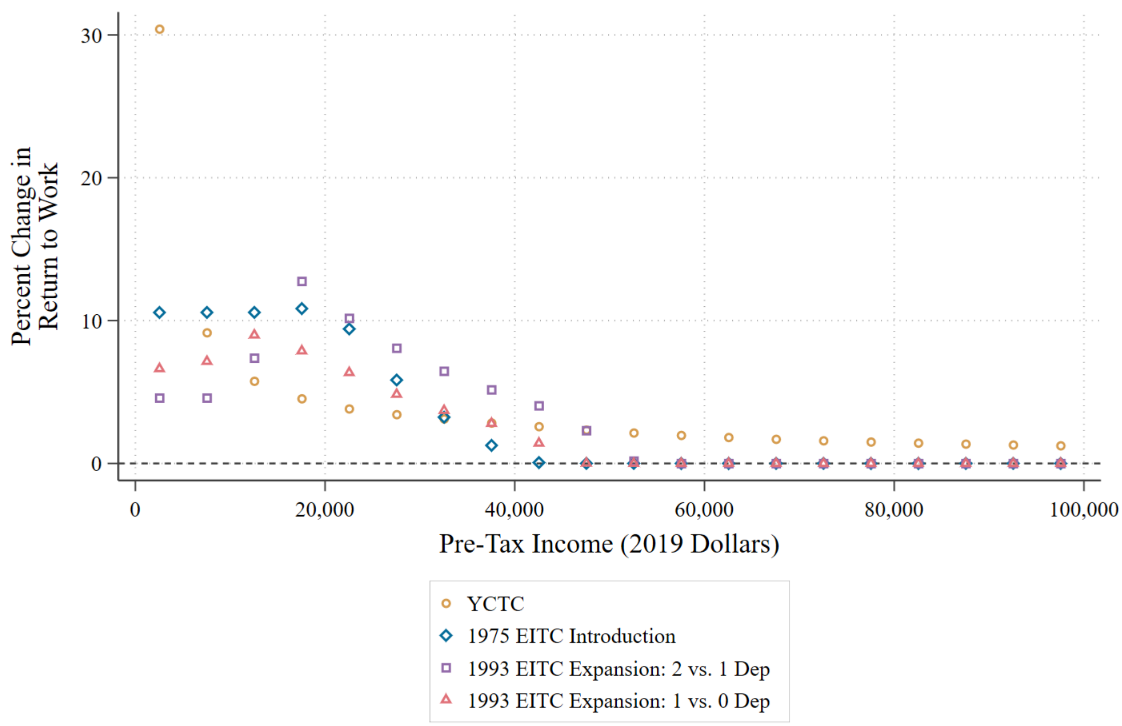
*Notes:* The table replicates the analysis from Table 2 separately for mothers who worked during the prior year and for mothers who did not work during the prior year. The analysis is conducted using the Medicaid sample, which is comprised of mothers of children enrolled in Medicaid in the specified state at any point during the prior year. A mother is treated as having worked during the prior year if she has positive wage or self-employment income for that year reported on a third-party information return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# Online Appendix to Work Requirements and Child Tax Benefits

Jacob Goldin Tatiana Homonoff Neel Lal
Ithai Lurie Katherine Michelmore Matthew Unrath

# A Appendix Tables and Figures

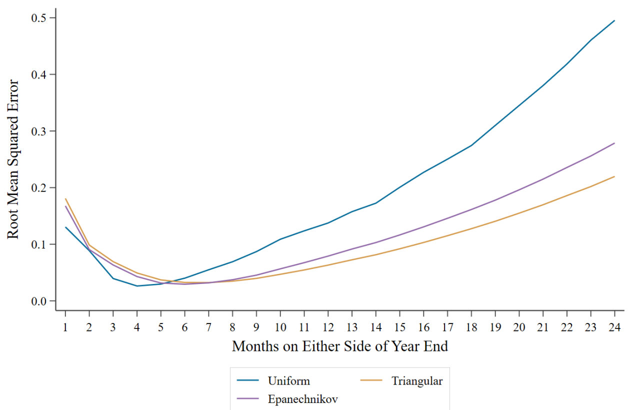
Figure A.1: Percent Change in Return to Work



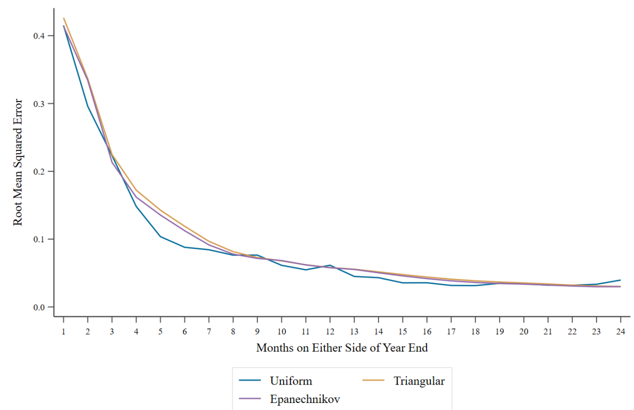

Notes: The figure plots the percentage change in the return to work for a range of policy reforms. For each reform, the percentage change in the return to work is calculated as the difference between the post-reform return to work minus the pre-reform return to work, divided by the pre-reform return to work. The return to work at a given level of pre-tax income is defined as the after-tax income at the specified pre-tax income minus the after-tax income for which the taxpayer would be eligible if the taxpayer were not to work. After-tax income is calculated net of federal income taxes and payroll taxes, using the NBER TAXSIM model. The plotted points are derived from the calculated percentage change in the return to work at $100 intervals, averaged into $5,000 bins. Pre-tax income is based on 2019 dollars; earlier policy changes are inflation-adjusted to 2019 dollars for comparability. Unless otherwise specified, tax liability is calculated assuming an unmarried taxpayer with one child. The figure considers three policy reforms: the elimination of the YCTC work requirement, the introduction of the EITC in 1975, and the expansion of EITC generosity from the Omnibus Budget Reconciliation Act of 1993 (OBRA93). For OBRA93, we separately plot the change in the return to work for taxpayers with one child versus no children, and for taxpayers with two children versus one. For each reform, the pre-reform return to work is calculated based on the tax law in place for the tax year prior to the reform; we assume that the only change in policy is the specified reform, so we do not account for other contemporaneous changes in policies such as changes in statutory tax rates in the year that the reform first applies.

Figure A.2: Placebo-Based Tuning Analysis: Optimal Kernel

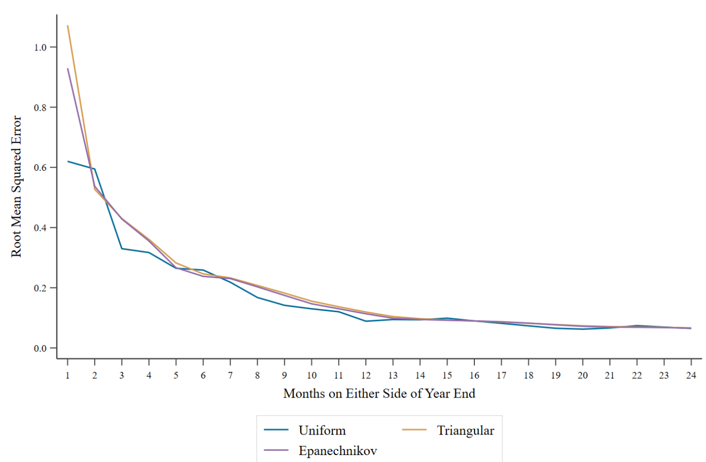
(a) Levels




(b) Linear



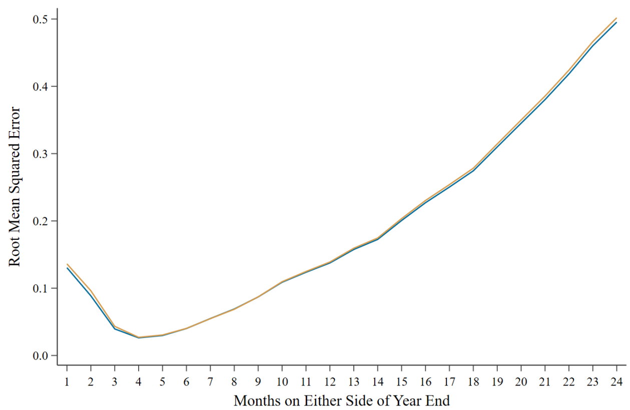

(c) Quadratic



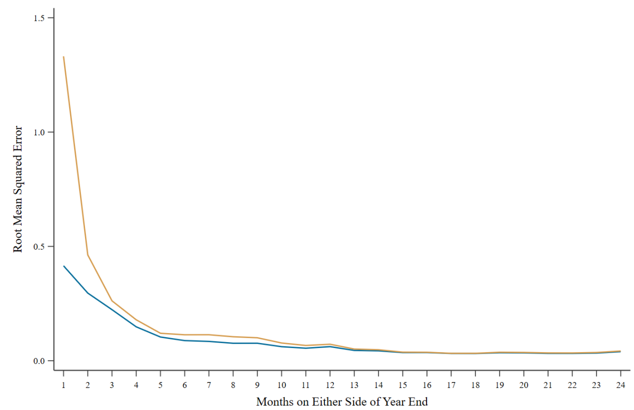

Notes: The figure reports the results of the Placebo-Based Tuning Analysis to determine the optimal local polynomial kernel for the regression discontinuity specification for our California analysis, as described in Section 4.2. The figure repeats the analyses in Figure 2, but varies the choice of kernel. The kernels considered are uniform (blue), triangular (yellow), and Epanechnikov (purple). The three panels correspond to a simple comparison of means (Panel A), a linear trend in child's date of birth (Panel B), or a quadratic trend in child's date of birth (Panel C).

Figure A.3: Placebo-Based Tuning Analysis: Donut Exclusion

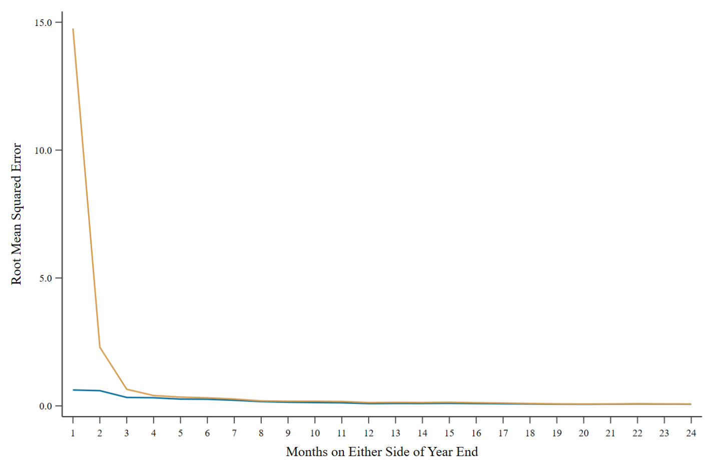
(a) Levels




(b) Linear



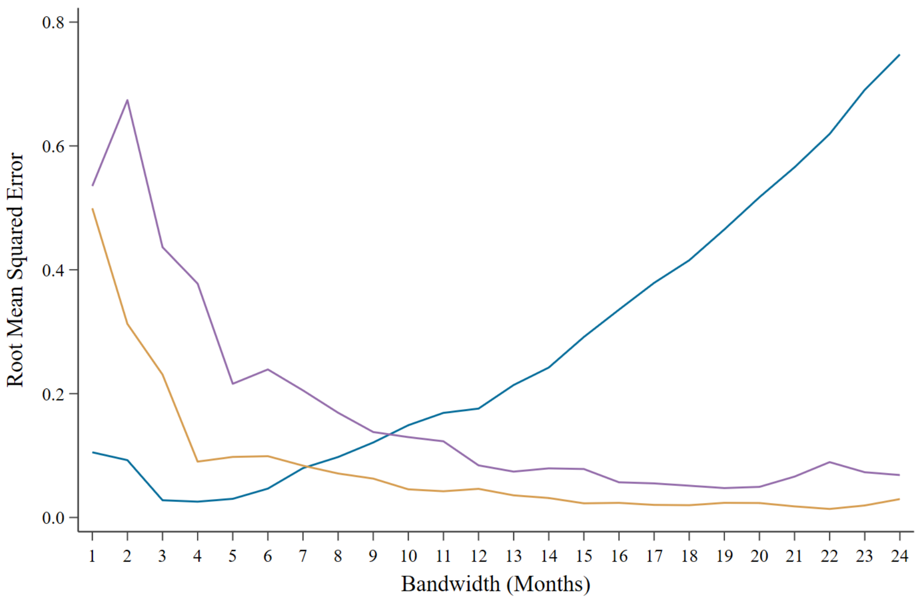

(c) Quadratic



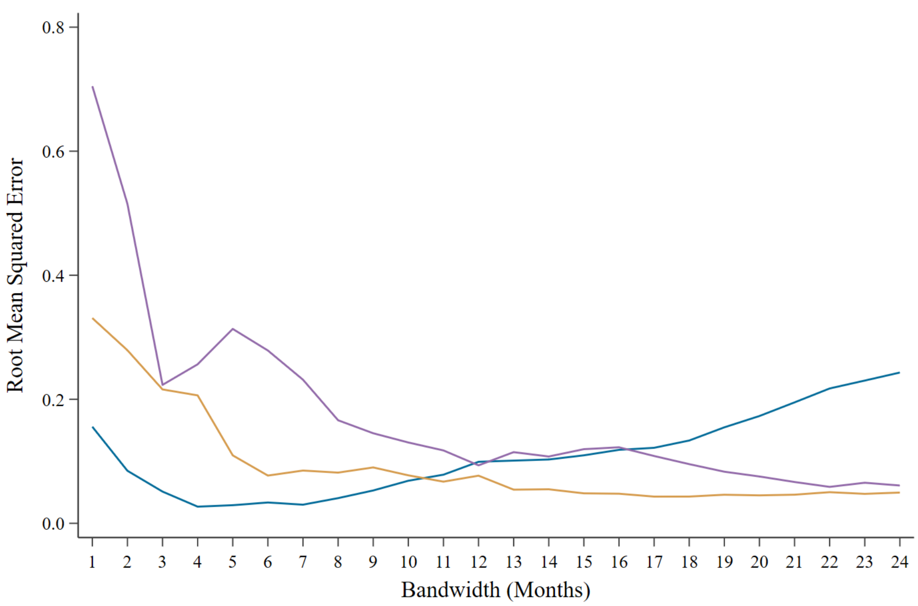

Notes: The figure reports the results of the Placebo-Based Tuning Analysis to determine whether to exclude an 8-day "donut" of births around the turn of the year for the regression discontinuity specification for our California analysis, as described in Section 4.2. The figure repeats the analyses in Figure 2, but varies whether a donut of births is excluded. The yellow lines reflect analyses that exclude the donut of births; the blue lines reflect analyses that do not. The three panels correspond to a simple comparison of means (Panel A), a linear trend in child's date of birth (Panel B), or a quadratic trend in child's date of birth (Panel C).

Figure A.4: Placebo-Based Tuning Analysis by Pre-Period Segment

(a) 2005-2011

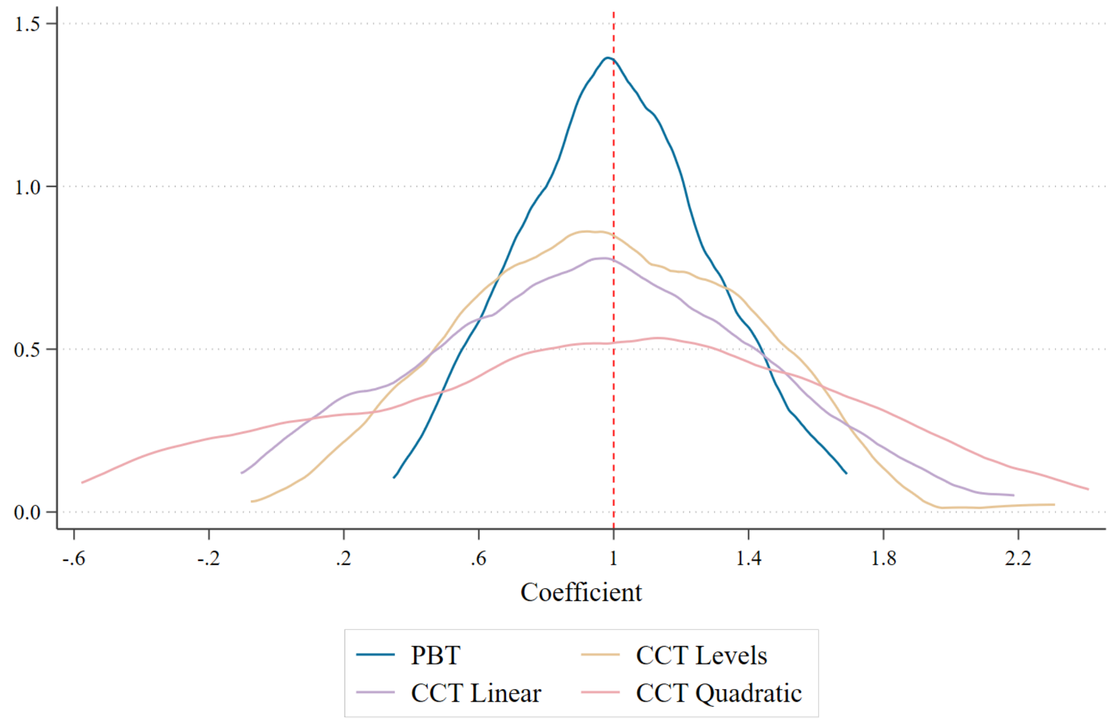



(b) 2012-2018




Notes: The figure replicates the analysis in Figure 2 separately for the first and second halves of the pre-period. Panel A reports results for 2005-2011; Panel B reports results for 2012-2018.

Figure A.5: Simulation Analysis for PBT and Other Specification Selection Methods

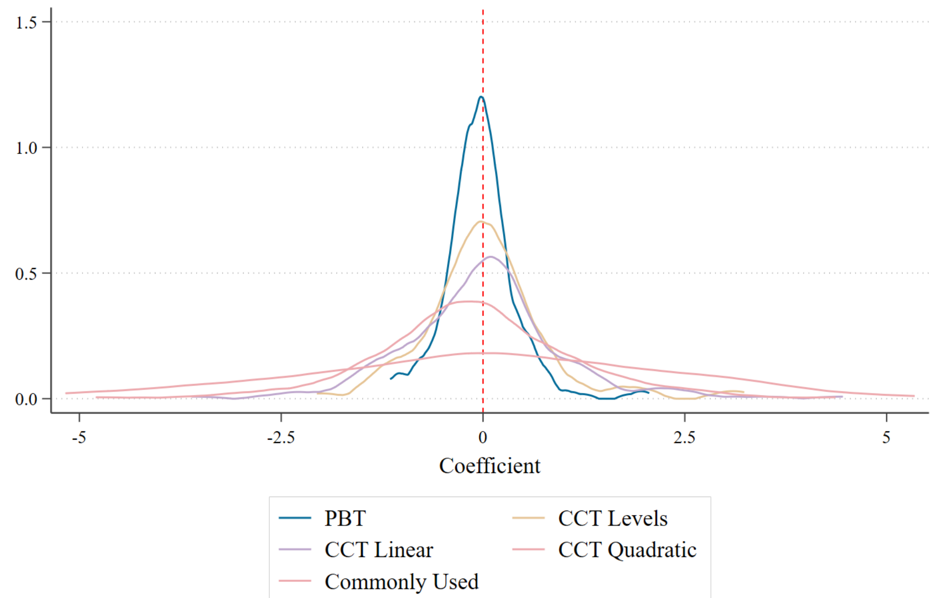



Notes: The figure reports results from a Monte Carlo simulation analysis comparing the performance of alternative approaches for selecting local polynomial order and bandwidth for regression discontinuity designs. PBT refers to the placebo-based tuning analysis described in Section 4. CCT refers to the optimal bandwidth selection method described in Calonico, Cattaneo and Titiunik (2014), implemented using the RDRobust Stata package; the levels, linear, and quadratic variants refer to the assumed local polynomial order. Each simulation iteration involves 14 placebo pre-periods ($t \in \{-13, -12, \ldots, 0\}$) and one post-period ($t = 1$). The running variable ($X$) ranges from -1 to 1 and is observed at 0.01 increments, so that there are 201 observations per period, indexed by $i$, each corresponding to a different value of $X$. The treatment ($T \in \{0, 1\}$) is applied to values of $X > 0$. The outcome ($Y_{it}$) is determined according to the following equation:

$$Y_{it} = \alpha_1 X_{it} + \alpha_2 X_{it}^2 + \alpha_3 X_{it}^3 + \alpha_4 X_{it}^4 + \beta_t T_{it} + \varepsilon_{it}$$

where $\beta$ reflects the treatment effect and $\varepsilon_{it} \sim N(0, 1)$. The treatment effect is assumed to be zero for the placebo pre-periods, $\beta_t = 0$ for $t < 1$, and is equal to 1 for the post-period, $\beta_t = 1$ for $t = 1$. Each iteration of the simulation involves randomly drawing the vector of parameters that govern the relationship between the running variable and the outcome, $\alpha_k \sim U[-1, 1]$ for $k \in \{1, 2, 3, 4\}$. The plotted distributions are obtained as follows. For PBT, the optimal local polynomial order and bandwidth are determined based on the placebo pre-periods, following the procedure described in the notes for Figure 2, considering bandwidths from 0.1 to 1.0 in 0.1 step intervals. The estimated pseudo-treatment effect for each simulation is then obtained by applying the PBT-selected specification to the post-treatment year. The CCT estimates are obtained by applying the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth selection procedure for the post-period, implemented using the RDRobust Stata package, and assuming the specified polynomial order. The plotted distributions correspond to the distribution of estimated pseudo-treatment effects under the specified approach for selecting the empirical distribution.

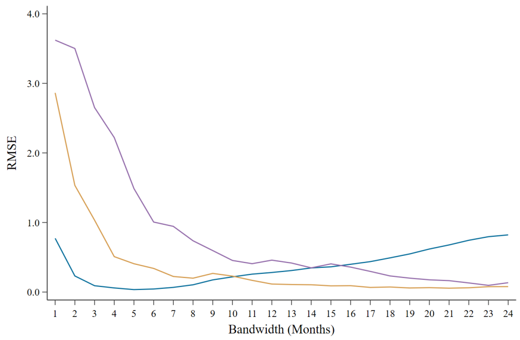
Figure A.6: Distribution of Pseudo-Treatment Effects for Placebo State-Years by Specification Selection Method



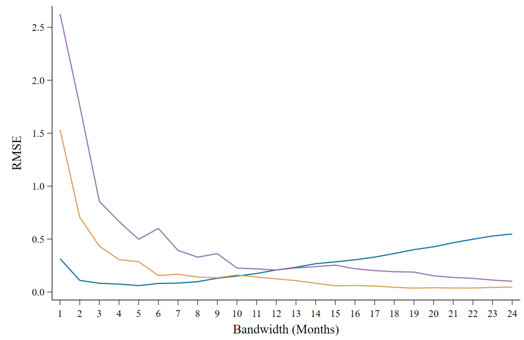

Notes: The figure reports the results of a placebo exercise comparing alternative methods for selecting the RD specification. The placebo states consist of each state that did not provide a young child tax credit during our sample period (i.e., all states and the District of Columbia other than California, Colorado, Maryland, New Jersey, Oregon, and Vermont). For each placebo state, we apply the placebo-based tuning analysis to 2005-2018 to select that state's optimal RD specification. We then use the optimal specification from each state to obtain estimates for that state for each outcome year (2019, 2020, 2022, and 2023). We separately obtain a distribution of estimates for each placebo state-year based on the optimal bandwidth selection method proposed in Calonico, Cattaneo and Titiunik (2014), implemented using the RDRobust Stata package. Because that method takes the local polynomial order as given, we alternatively apply the CCT method to local polynomials of order 0 (levels), 1 (linear), and 2 (quadratic). The "commonly used" distribution refers to placebo state year estimates drawn from an RD specification with a one-month bandwidth, a local linear polynomial, and a donut exclusion of births 8 days around the turn-of-the-year.

Figure A.7: Placebo-Based Tuning Analysis: Other State CTCs

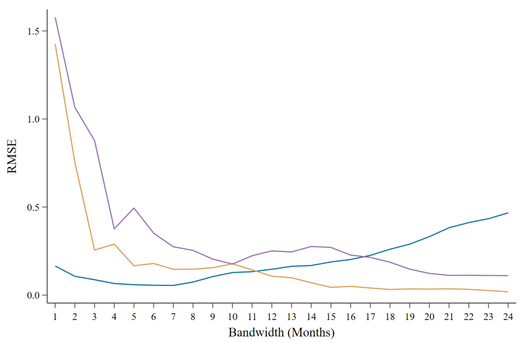
(a) Colorado



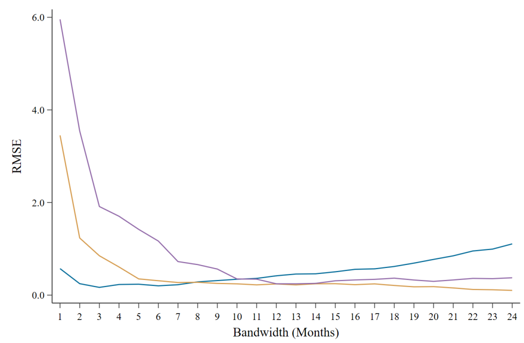

(b) Maryland



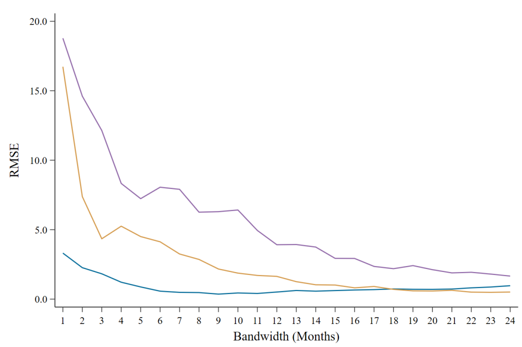

(c) New Jersey




(d) Oregon



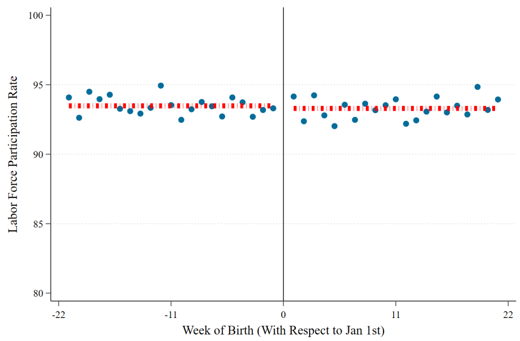

(e) Vermont



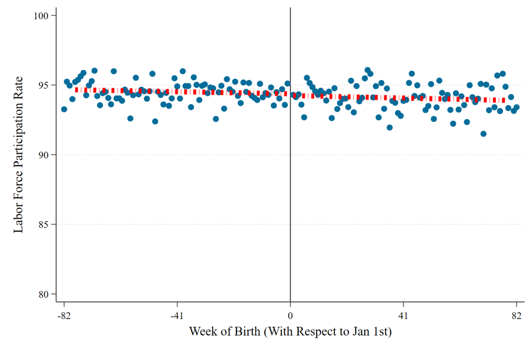

Notes: The figure reports the results of the Placebo-Based Tuning Analysis for Colorado, Maryland, New Jersey, Oregon, and Vermont described in Section 4.2. It compares the root mean squared error (RMSE) of the distribution of estimated (placebo) effects for different empirical specifications. Each placebo effect is obtained from estimating the pseudo-effect of age-eligibility for the given state's child tax credit following equation (1), for each year from 2005 through 2018, in a specification using a local polynomial with the specified bandwidth and order, with uniform weights, and with no "donut" exclusion. Each estimate is obtained from a sample composed of mothers whose focal child turns six within the specified bandwidth (i.e., the number of months on either side of the end of the year). The reported RMSE corresponds to the square root of the average (across years) of the square of the estimated coefficients from each year. The purple line includes a quadratic polynomial in date of birth; the yellow line includes a linear trend in date of birth; and the blue line consists of a simple comparison of means of the age-eligible and age-ineligible groups of mothers.

Figure A.8: Labor Force Participation by Child's Date of Birth: Other State CTCs

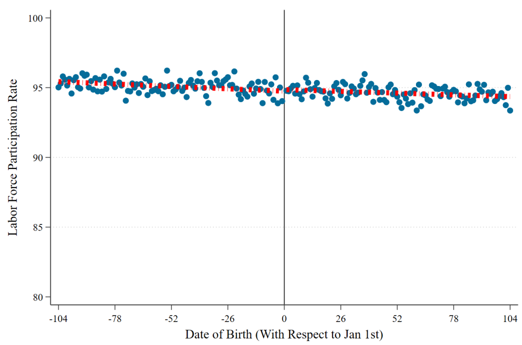
(a) Colorado



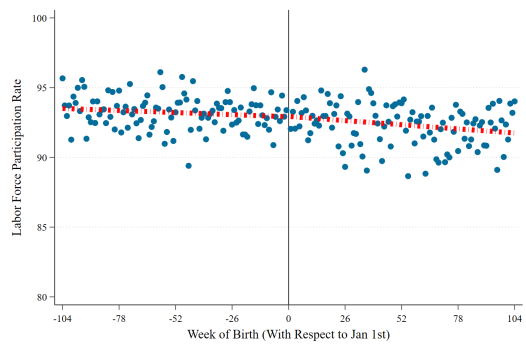

(b) Maryland



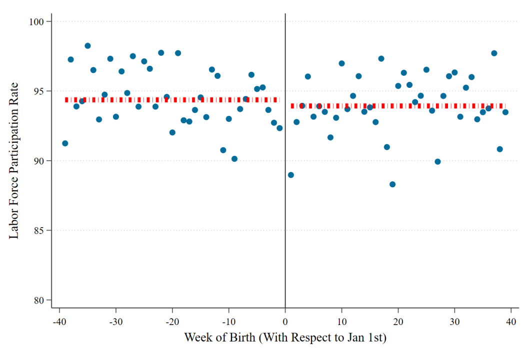

(c) New Jersey




(d) Oregon

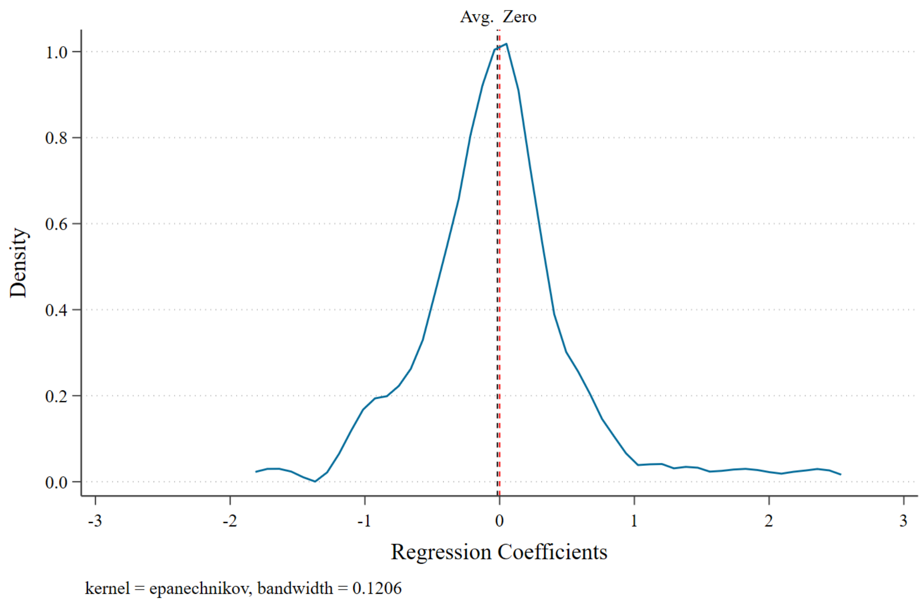



(e) Vermont



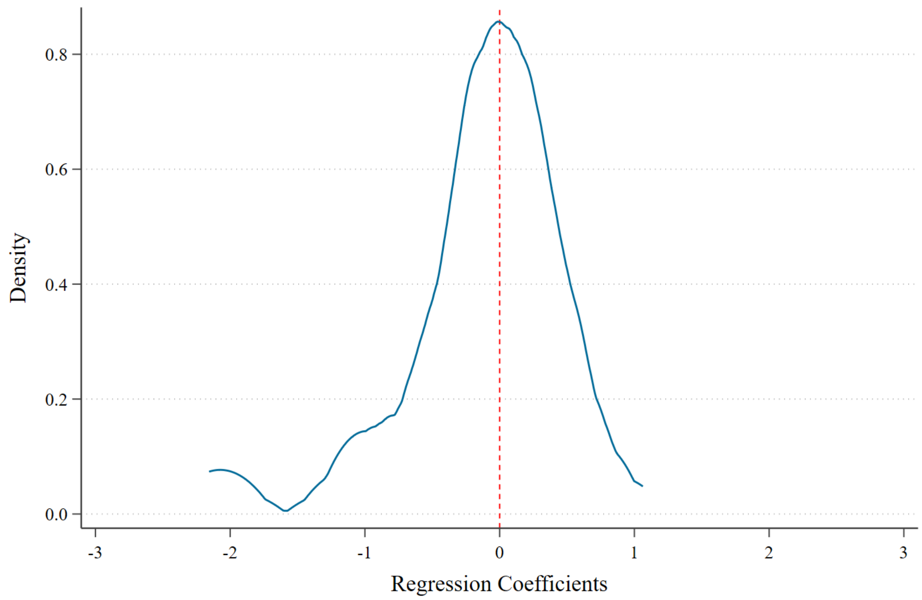

Notes: The figure reports mean labor force participation rates binned by child's week of birth for recently working mothers. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and who has a child whose sixth birthday falls within the PBT-selected bandwidth for the state. The dashed lines correspond to the state's local polynomial, estimated separately for dates before versus after the turn of-the-year (normalized to zero). See Table 2 for state-specific bandwidths and polynomial orders.

Figure A.9: Distribution of Pseudo-Effects for Placebo State-Years



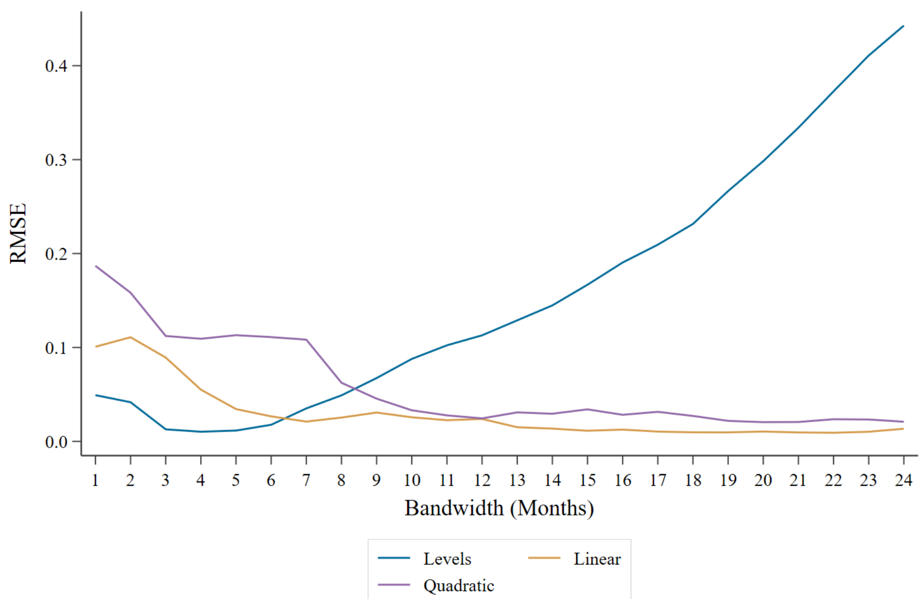

Notes: The figure reports the distribution of estimated pseudo-treatment effects for mothers in state-years that do not have a young child tax credit during the sample period using the PBT-optimal specification for the California analysis. For each state and year, the estimated effect is obtained by comparing labor force participation in the given year among mothers who worked in the given state in the year prior to the given year, and whose youngest child's sixth birthday falls within the four-month window around the turn of the given year. The analysis includes coefficients from 2019, 2020, 2022, and 2023, and from all states and the District of Columbia other than California, Colorado, Maryland, New Jersey, Oregon, and Vermont. The figure plots a kernel-density figure with an epanechnikov kernel and bandwidth of 0.1206. The dashed vertical lines denote the sample mean of the distribution (black) and zero (red), as labeled in the figure.

Figure A.10: Distribution of Difference-in-Discontinuities Pseudo-Effects for Placebo States




Notes: The figure reports the distribution of estimated pseudo-treatment effects from the difference-in-discontinuities specification (Column 3 in Table 1) for states that do not have a young child tax credit during the sample period using the PBT-optimal specification for the California analysis. The specification compares labor force participation among age-eligible versus age-ineligible mothers during the period with no YCTC work requirement (2022-2023) and the YCTC work requirement period (2019-2020). The distribution includes one estimated treatment effect for each placebo state (i.e., all states and the District of Columbia other than California, Colorado, Maryland, New Jersey, Oregon, and Vermont).

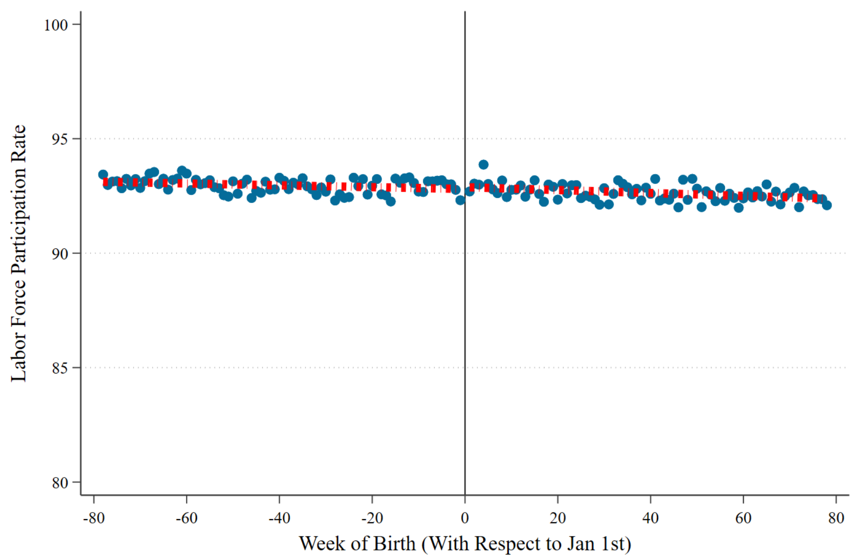
Figure A.11: Placebo-Based Tuning Analysis: Two-Year Specification



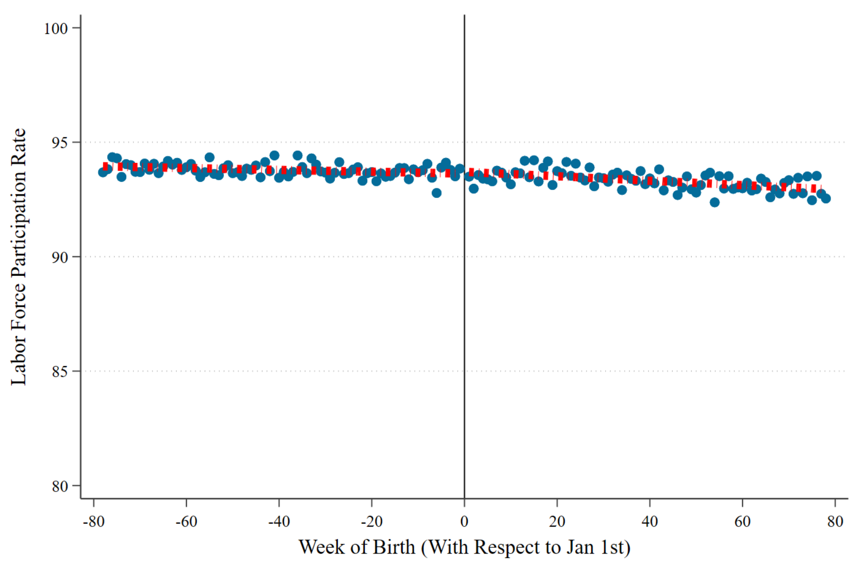

Notes: The figure replicates the analysis reported in Figure 2, but each placebo period spans two years rather than one. Each candidate specification includes a calendar year fixed effect. The placebo periods considered are: 2005 and 2006; 2006 and 2007; ...; 2017 and 2018.

Figure A.12: Labor Force Participation by Child's Date of Birth: Alternative Specification

(a) 2019-2020



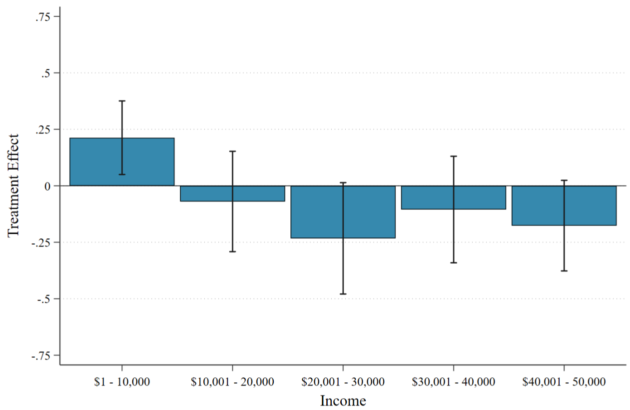

(b) 2022-2023



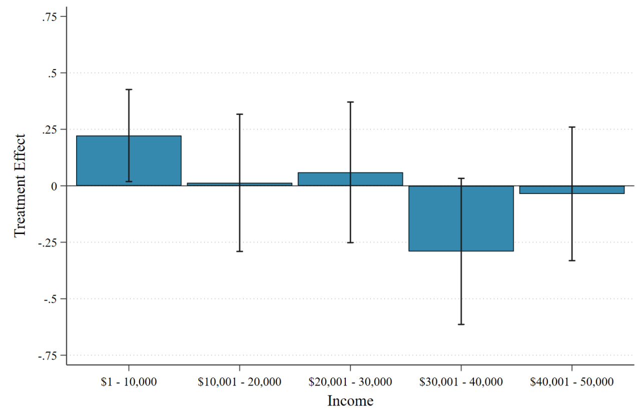

Notes: The figure reports mean labor force participation rates binned by child's week of birth for recently working California mothers. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the 18-month window around the turn of the year (2019 or 2020 in Panel A; 2022 or 2023 in Panel B). The dashed lines correspond to the best linear fit, estimated separately for dates before versus after the turn of-the-year (normalized to zero).

Figure A.13: Intensive Margin Effect of YCTC Eligibility

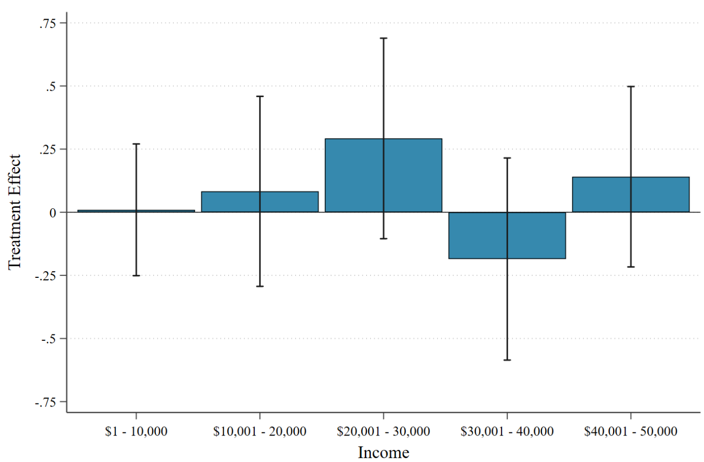
(a) Work Requirement (2019-2020)




(b) No Work Requirement (2022-2023)




(c) Difference-in-Discontinuities




Notes: The figure reports the estimated treatment effect of YCTC age-eligibility on the likelihood that a taxpayer reports income within the specified income bin. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of the specified year. The sample is additionally limited to individuals who file a tax return reporting positive income. For each income bin, panels A, B, and C report specifications based on Columns 1, 2, and 3 (respectively) of Table 1. Treatment effects are reported in percentage points (0-100). Bars represent estimate 95% confidence intervals, derived from heteroskedasticity-robust standard errors.

Table A.1: Estimated Pseudo Treatment Effects from the California Pre-Period

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Leave-One-Out | CCT Levels | CCT Linear | CCT Quadratic | Commonly Used Specification |
| Coefficient | -0.012 | -0.088 | -0.014 | -0.206 | 0.440 |
| | (0.216) | (0.232) | (0.344) | (0.476) | (1.050) |
| Control Mean | 92.3 | 92.2 | 92.3 | 92.3 | 92.1 |

*Notes:* The table reports the mean and standard deviation of the estimated effect of YCTC age-eligibility on labor force participation for each year from 2005 through 2018 (prior to the YCTC's introduction). Units are percentage points (0-100). The columns report alternative approaches for estimating the yearly treatment effects. In Column 1, a leave-one-out procedure is employed, in which each year's specification is selected by applying the placebo-based tuning analysis to the other 13 pre-period years. In Columns 2-4, the specification is selected using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth method, implemented using the RDRobust Stata package, imposing a local polynomial in child date of birth with the specified order. In Column 5, the specification is a regression discontinuity design with a one-month bandwidth, a local linear polynomial with uniform bandwidth, and an 8-day donut around the turn-of-the-year.

Table A.2: Summary Statistics by Eligibility for California YCTC

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | All | Age-Eligible (Jan-Apr) | Age-Ineligible (Sep-Dec) | p-value |
| *Individual-Level* | | | | |
| Age | 35.201 | 35.058 | 35.336 | 0.000 |
| Any Income | 1.000 | 1.000 | 1.000 | . |
| Total Income | 57,127 | 56,998 | 57,249 | 0.670 |
| Self-Employed | 0.119 | 0.119 | 0.118 | 0.550 |
| Filed a Tax Return | 0.934 | 0.935 | 0.933 | 0.034 |
| *Return-Level, if Filed* | | | | |
| Married | 0.550 | 0.554 | 0.546 | 0.000 |
| Num. Claimed Children | 1.818 | 1.816 | 1.819 | 0.334 |
| AGI | 129,376 | 131,259 | 127,593 | 0.260 |
| Claimed Federal EITC | 0.374 | 0.370 | 0.377 | 0.000 |
| Claimed Federal CTC | 0.904 | 0.903 | 0.905 | 0.034 |
| Observations | 496,527 | 241,349 | 255,178 | |

*Notes:* The table reports lagged (prior-year) characteristics for recently working California mothers by age-eligibility for the YCTC. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of one of the following years: 2019, 2020, 2022, and 2023. Column 1 reports statistics for the full sample; Column 2 reports statistics for mothers of children whose birthday falls after the turn-of-the-year (age-eligible); Column 3 reports statistics for mothers of children whose birthday falls before the turn-of-the-year (age-ineligible); Column 4 reports the p-value for the test of equality between Columns 2 and 3. Individual-level characteristics are based on third-party information returns; return-level characteristics are based on tax return data and are only presented for individuals who filed a prior-year tax return.

Table A.3: California YCTC Eligibility and Labor Force Participation: Robustness

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| **Panel A: Has a Social Security Number** | | | |
| Coefficient | -0.005 | -0.124 | -0.119 |
| | (0.106) | (0.102) | (0.147) |
| 95% CI | [-0.21, 0.20] | [-0.32, 0.08] | [-0.41, 0.17] |
| Control Mean | 93.8 | 94.4 | 94.1 |
| Observations | 210,661 | 205,769 | 416,430 |
| **Panel B: Claimed Child** | | | |
| Coefficient | -0.137 | -0.092 | 0.045 |
| | (0.101) | (0.097) | (0.140) |
| 95% CI | [-0.33, 0.06] | [-0.28, 0.10] | [-0.23, 0.32] |
| Control Mean | 93.4 | 94.1 | 93.7 |
| Observations | 246,066 | 238,559 | 484,625 |
| | Work Req. DiD (2019-2021) | No Work Req. DiD (2022-2023) | Triple Difference [(2) - (1)] |
| **Panel C: Triple Difference** | | | |
| Coefficient | -0.007 | -0.065 | -0.058 |
| | (0.091) | (0.106) | (0.140) |
| 95% CI | [-0.19, 0.17] | [-0.27, 0.14] | [-0.33, 0.22] |
| Control Mean | 93.7 | 94.6 | 94.0 |
| Observations | 3,006,951 | 1,974,592 | 4,981,543 |

*Notes:* The table replicates the California YCTC analysis in Table 1 using alternative sample restrictions or time periods. Panel A restricts the sample to mothers assigned a social security number. Panel B restricts the sample to mothers who claimed the focal child on their prior-year tax return. Panel C is a triple-difference model including 2021, the year of the federal CTC expansion, in the analysis. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100).

Table A.4: Estimated YCTC Take-Up Among Federal Filers

| | Number of Claimants | | | Total Credit Amount ($ in Mil) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Claimed | Eligible | Takeup (%) | Claimed | Eligible | Takeup (%) |
| 2019 | 428,857 | 517,406 | 82.9 | 389 | 465 | 83.7 |
| 2020 | 416,980 | 453,675 | 91.9 | 388 | 401 | 96.7 |
| 2019-2020 | 845,837 | 971,081 | 87.1 | 777 | 866 | 89.7 |

*Notes:* The table reports estimates of take-up of the California YCTC among federal tax filers. Take-up is estimated by comparing reported YCTC claiming statistics reported by the California Franchise Tax Board (FTB) to simulated YCTC eligibility for California federal tax filers claiming one or more children under the age of six. Calculations are conducted at the tax return level. Actual claim data regarding the number of YCTC claimants and the total dollars of YCTC claimed is derived from California Franchise Tax Board (2019) and the subsequent report for 2020. Simulated YCTC eligibility among federal filers is based on the YCTC credit formula and information reported on the federal return. For purposes of this exercise, we treat as eligible for the YCTC any federal filer satisfying the following criteria: had a California address; claimed at least one child below the age of six for either the federal CTC or EITC; reported Adjusted Gross Income under $30,000; reported positive wages or Schedule C profit; reported taxable interest and dividends below $3,828 for 2019 returns or $3,882 for 2020 returns; did not file as married filing separately; filed the federal return before the close of the calendar year following the tax year; and both the filer and child possessed a valid SSN. These criteria apply to the most recent tax filing. In the rare occasion that multiple taxpayers claimed the same child, the child was assigned to the taxpayer with higher income.

Table A.5: Yearly Estimates of California YCTC Eligibility on Labor Force Participation

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2019 | 2020 | 2021 | 2019-2021 |
| Age-Eligibility x California | -0.069 | -0.075 | 0.114 | -0.101 |
| | (0.153) | (0.156) | (0.163) | (0.141) |
| Age-Eligibility x California x Year | | | | 0.091 |
| | | | | (0.112) |
| Control Mean | 94.1 | 93.7 | 93.2 | 93.7 |
| Observations | 1,077,818 | 1,095,161 | 1,082,917 | 3,255,896 |

*Notes:* The table reports the estimated effect of eligibility for the California YCTC separately for each year in which the credit was subject to a work requirement (2019-2021). The sample consists of recently working mothers whose youngest child's sixth birthday falls within the four-month window around the end of the specified year. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100). Columns 1 through 3 present the effect of YCTC age-eligibility by comparing labor force participation of California mothers of age-eligible versus age-ineligible children to mothers of same-aged children in different states in 2019 through 2021, respectively. Column 4 includes the samples for 2019 through 2021 adding an interaction between the indicator for YCTC age-eligibility (i.e., age-eligible and living in California) and a continuous year variable along with year by California fixed effects and year by age-eligibility fixed effects. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table A.6: Summary Statistics for Other States with Young Child Tax Credits

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| *Individual-Level* | | | | | |
| Age | 34.341 | 35.023 | 35.628 | 34.527 | 34.912 |
| Any Income | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| Total Income | 50,357 | 62,064 | 64,764 | 50,929 | 48,823 |
| Self-Employed | 0.169 | 0.168 | 0.128 | 0.141 | 0.130 |
| Filed a Tax Return | 0.942 | 0.933 | 0.947 | 0.922 | 0.954 |
| *Return-Level, if Filed* | | | | | |
| Married | 0.670 | 0.558 | 0.603 | 0.628 | 0.670 |
| Num. Claimed Children | 2.078 | 1.937 | 1.896 | 1.902 | 1.976 |
| AGI | 119,170 | 122,765 | 145,269 | 111,190 | 108,217 |
| Claimed Federal EITC | 0.319 | 0.344 | 0.321 | 0.341 | 0.317 |
| Claimed Federal CTC | 0.914 | 0.928 | 0.907 | 0.922 | 0.924 |
| Observations | 74,873 | 138,541 | 441,346 | 91,310 | 13,746 |

*Notes:* The table reports lagged (prior-year) characteristics for recently working mothers in states other than California with state-level young child tax credits. The sample consists of individuals who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose child's sixth birthday falls within the PBT-specified bandwidth around the turn of the year for the years in which the applicable state credit was available. Individual-level characteristics are based on third-party information returns; return-level characteristics are based on tax return data and are only presented for individuals who filed a prior-year tax return.

Table A.7: Yearly Estimates of Colorado CTC Eligibility on Labor Force Participation

| | (1) | (2) |
|---|---|---|
| | 2022 | 2023 |
| Coefficient | 0.185 | -0.121 |
| | (0.257) | (0.262) |
| 95% CI | [-0.32, 0.69] | [-0.63, 0.39] |
| Control Mean | 93.3 | 93.1 |
| Observations | 37,382 | 37,491 |

*Notes:* The table reports the estimated effect of eligibility for the Colorado CTC separately for each year that the credit was in effect (2022 and 2023). The sample consists of recently working mothers with a child whose sixth birthday falls within the five-month window around the end of the specified year. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100).

Table A.8: Other State Pseudo Effects During the Pre-Period

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Leave-One-Out | CCT Levels | CCT Linear | CCT Quadratic | Commonly Used Specification |
| Colorado | -0.018 | 0.147 | 0.328 | -0.044 | -0.486 |
| | (0.193) | (0.394) | (0.688) | (0.987) | (3.514) |
| New Jersey | 0.028 | 0.014 | 0.101 | 0.034 | 0.980 |
| | (0.138) | (0.329) | (0.413) | (0.572) | (1.932) |
| Maryland | -0.033 | 0.132 | 0.170 | 0.146 | -0.707 |
| | (0.196) | (0.327) | (0.385) | (0.723) | (2.323) |
| Oregon | -0.035 | -0.159 | -0.152 | -0.389 | 1.210 |
| | (0.362) | (0.379) | (0.731) | (1.017) | (3.667) |
| Vermont | -0.276 | 0.474 | 0.336 | 0.211 | 0.427 |
| | (0.552) | (1.805) | (2.239) | (2.983) | (8.546) |

*Notes:* The table replicates the analysis in Table A.1 for states other than California.

Table A.9: Placebo Tests: California YCTC Eligibility and Labor Force Participation

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| | Panel A: All States Sample | | |
| Coefficient | -0.036 | -0.053 | -0.017 |
| | (0.036) | (0.034) | (0.049) |
| 95% CI | [-0.11, 0.03] | [-0.12, 0.01] | [-0.11, 0.08] |
| Control Mean | 94.0 | 94.7 | 94.4 |
| Observations | 1,757,313 | 1,733,838 | 3,491,151 |
| | Panel B: Younger Child Sample | | |
| Coefficient | 0.029 | 0.084 | 0.055 |
| | (0.160) | (0.160) | (0.226) |
| 95% CI | [-0.28, 0.34] | [-0.23, 0.40] | [-0.39, 0.50] |
| Control Mean | 89.7 | 90.8 | 90.2 |
| Observations | 145,447 | 131,776 | 277,223 |

*Notes:* Panel A replicates Table 1 but for individuals living in placebo states that did not offer a young child tax credit during our sample period (i.e., all states and the District of Columbia other than California, Colorado, Maryland, New Jersey, Oregon, and Vermont). Panel B replicates Table 1 for California mothers with a child that turns six within the four months around the turn-of-the-year *in addition to* a younger child who would continue to qualify the mother for the California YCTC during the outcome year.

Table A.10: Investigating Time Variation in the Effect of YCTC Eligibility

| | (1) | (2) | (3) |
|---|---|---|---|
| | 2022-23 vs 2019 | 2023 vs 2019 | 2020 vs 2019 |
| Coefficient | 0.024 | -0.039 | 0.049 |
| | (0.176) | (0.204) | (0.207) |
| 95% CI | [-0.32, 0.37] | [-0.44, 0.36] | [-0.36, 0.45] |
| Control Mean | 93.4 | 93.1 | 92.8 |
| Observations | 370,060 | 248,043 | 251,699 |

*Notes:* The table compares labor force participation of recently working California mothers with children born around the turn of the year for various combinations of outcome years. The outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns; units are percentage points (0-100). Column 1 reports the difference-in-discontinuities estimate from equation 3 excluding 2020. Column 2 reports the same specification but excludes both 2020 and 2022. Column 3 compares mothers of age-eligible versus age-ineligible children in California across the two years within the YCTC work requirement period.

Table A.11: PBT-Optimal Linear RD Specification: California

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| Coefficient | 0.072 | 0.069 | -0.003 |
| | (0.104) | (0.100) | (0.144) |
| 95% CI | [-0.13, 0.27] | [-0.13, 0.27] | [-0.29, 0.28] |
| Control Mean | 93.0 | 93.8 | 93.4 |
| Observations | 988,326 | 958,470 | 1,946,796 |

*Notes:* The table replicates Table 1 under an alternative regression discontinuity specification with a local linear polynomial in child's date-of-birth and a bandwidth of 18 months. The specification uses a uniform kernel and does not exclude a donut of birthdates near the turn-of-the-year.

Table A.12: Commonly Used RD Specification: California

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| Coefficient | -0.285 | -0.172 | 0.113 |
| | (0.738) | (0.711) | (1.025) |
| 95% CI | [-1.73, 1.16] | [-1.57, 1.22] | [-1.90, 2.12] |
| Control Mean | 92.8 | 93.7 | 93.2 |
| Observations | 47,547 | 46,177 | 93,724 |

*Notes:* The table replicates Table 1 under a commonly used alternative regression discontinuity specification with a local linear polynomial in child's date-of-birth, a bandwidth of one month, a uniform kernel, and a donut exclusion of 8 days around the turn-of-the-year.

Table A.13: Heterogeneity by Income: California

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| Panel A: Lower Income (< \$30,000) | | | |
| Coefficient | -0.203 | -0.234 | -0.031 |
| | (0.219) | (0.242) | (0.326) |
| 95% CI | [-0.63, 0.23] | [-0.71, 0.24] | [-0.67, 0.61] |
| Control Mean | 88.4 | 88.4 | 88.4 |
| Observations | 86,036 | 70,890 | 156,926 |
| Panel B: Higher Income ($\geq \$30,000$) | | | |
| Coefficient | -0.075 | -0.099 | -0.025 |
| | (0.106) | (0.098) | (0.144) |
| 95% CI | [-0.28, 0.13] | [-0.29, 0.09] | [-0.31, 0.26] |
| Control Mean | 95.1 | 95.7 | 95.4 |
| Observations | 165,663 | 173,938 | 339,601 |

*Notes:* Notes: The table replicates Table 1 separately for taxpayers with prior-year reported AGI below versus above \$30,000. Taxpayers who did not file during the prior-year are treated as having reported an AGI of \$0.

Table A.14: Heterogeneity by Income: Other States

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| Panel A: Lower Income (< $30,000) | | | | | |
| Coefficient | 0.398 | -0.857 | 0.287 | -0.491 | -1.286 |
| | (0.460) | (0.601) | (0.339) | (0.822) | (1.149) |
| 95% CI | [-0.50, 1.30] | [-2.03, 0.32] | [-0.38, 0.95] | [-2.10, 1.12] | [-3.54, 0.97] |
| Control Mean | 88.2 | 89.6 | 90.7 | 87.5 | 88.0 |
| Observations | 19,419 | 40,558 | 116,461 | 25,992 | 3,357 |
| Panel B: Higher Income ($\geq$ $30,000) | | | | | |
| Coefficient | -0.116 | 0.270 | -0.174 | 0.089 | -0.174 |
| | (0.186) | (0.222) | (0.124) | (0.321) | (0.352) |
| 95% CI | [-0.48, 0.25] | [-0.16, 0.71] | [-0.42, 0.07] | [-0.54, 0.72] | [-0.86, 0.51] |
| Control Mean | 95.0 | 96.9 | 96.8 | 95.7 | 96.8 |
| Observations | 55,454 | 97,983 | 324,885 | 65,318 | 10,389 |

*Notes:* The table replicates Table 2 separately for taxpayers with prior-year reported AGI below versus above $30,000. Taxpayers who did not file during the prior-year are treated as having reported an AGI of $0.

Table A.15: Heterogeneity by Marital Status: California

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| **Panel A: Married** | | | |
| Coefficient | 0.046 | -0.025 | -0.071 |
| | (0.142) | (0.137) | (0.197) |
| 95% CI | [-0.23, 0.32] | [-0.29, 0.24] | [-0.46, 0.32] |
| Control Mean | 93.0 | 93.7 | 93.3 |
| Observations | 129,239 | 125,786 | 255,025 |
| **Panel B: Unmarried** | | | |
| Coefficient | -0.145 | -0.204 | -0.060 |
| | (0.142) | (0.135) | (0.196) |
| 95% CI | [-0.42, 0.13] | [-0.47, 0.06] | [-0.44, 0.32] |
| Control Mean | 94.4 | 95.2 | 94.8 |
| Observations | 106,635 | 102,130 | 208,765 |

*Notes:* The table replicates Table 1 separately for married versus unmarried taxpayers. Marital status is measured based on taxpayers' prior-year filing status. Taxpayers who filed as married filing jointly or married filing separately are treated as married. Taxpayers who filed with a different filing status are treated as unmarried. Taxpayers who did not file a prior-year tax return are excluded from this analysis.

Table A.16: Heterogeneity by Marital Status: Other States

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| Panel A: Married | | | | | |
| Coefficient | -0.075 | 0.418 | -0.070 | -0.014 | -0.070 |
| | (0.227) | (0.303) | (0.162) | (0.408) | (0.429) |
| 95% CI | [-0.52, 0.37] | [-0.18, 1.01] | [-0.39, 0.25] | [-0.81, 0.79] | [-0.91, 0.77] |
| Control Mean | 93.5 | 95.5 | 95.6 | 94.2 | 95.8 |
| Observations | 47,221 | 72,151 | 251,963 | 52,890 | 8,785 |
| Panel B: Unmarried | | | | | |
| Coefficient | 0.090 | -0.412 | 0.115 | 0.387 | -1.823** |
| | (0.295) | (0.355) | (0.198) | (0.501) | (0.730) |
| 95% CI | [-0.49, 0.67] | [-1.11, 0.28] | [-0.27, 0.50] | [-0.60, 1.37] | [-3.26, -0.39] |
| Control Mean | 94.6 | 95.3 | 95.9 | 95.0 | 94.8 |
| Observations | 23,273 | 57,090 | 166,140 | 31,324 | 4,329 |

*Notes:* The table replicates Table 2 separately for married versus unmarried taxpayers. Marital status is measured based on taxpayers' prior-year filing status. Taxpayers who filed as married filing jointly or married filing separately are treated as married. Taxpayers who filed with a different filing status are treated as unmarried. Taxpayers who did not file a prior-year tax return are excluded from this analysis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Medicaid Sample: Summary Statistics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | California | Colorado | Maryland | New Jersey | Oregon | Vermont |
| *Individual-Level* | | | | | | |
| Age | 33.605 | 32.233 | 33.042 | 33.481 | 32.929 | 33.097 |
| Any Income | 0.669 | 0.688 | 0.734 | 0.698 | 0.679 | 0.695 |
| Total Income | 16,394 | 17,195 | 22,819 | 18,482 | 18,650 | 18,635 |
| Self-Employed | 0.083 | 0.128 | 0.156 | 0.109 | 0.110 | 0.100 |
| Filed a Tax Return | 0.839 | 0.857 | 0.836 | 0.865 | 0.791 | 0.838 |
| *Return-Level, if Filed* | | | | | | |
| Married | 0.439 | 0.507 | 0.353 | 0.397 | 0.518 | 0.513 |
| Num. Claimed Children | 1.932 | 2.260 | 2.025 | 2.034 | 2.052 | 2.102 |
| AGI | 38,516 | 44,581 | 42,027 | 39,212 | 48,757 | 46,773 |
| Claimed Federal EITC | 0.594 | 0.586 | 0.621 | 0.626 | 0.573 | 0.569 |
| Claimed Federal CTC | 0.921 | 0.927 | 0.934 | 0.938 | 0.921 | 0.915 |
| Observations | 374,647 | 47,586 | 91,951 | 255,434 | 73,181 | 10,143 |

*Notes:* The table reports lagged (prior-year) characteristics for mothers who were enrolled in Medicaid during the prior-year in states with young child tax credits. The sample for each state consists of mothers whose child's sixth birthday falls within the PBT-specified bandwidth around the turn-of-the-year for the years in which the applicable state credit was available; PBT-selected bandwidths by state for the Medicaid sample are reported in Table 2. Individual-level characteristics are based on third-party information returns; return-level characteristics are based on tax return data and are only presented for individuals who filed a prior-year tax return.

Table A.18: California YCTC Eligibility and Labor Force Participation: Alternative Samples

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022-2023) | Diff-in-Disc [(2) - (1)] |
| **Panel A: Medicaid Sample** | | | |
| Coefficient | 0.031 | 0.265 | 0.234 |
| | (0.216) | (0.218) | (0.306) |
| 95% CI | [-0.39, 0.45] | [-0.16, 0.69] | [-0.37, 0.83] |
| Control Mean | 66.2 | 68.4 | 67.3 |
| Observations | 192,234 | 182,413 | 374,647 |
| **Panel B: Census Sample** | | | |
| Coefficient | 0.072 | 0.268** | 0.196 |
| | (0.145) | (0.128) | (0.194) |
| 95% CI | [-0.21, 0.36] | [0.02, 0.52] | [-0.18, 0.58] |
| Control Mean | 70.5 | 68.6 | 69.4 |
| Observations | 400,000 | 530,000 | 920,000 |

*Notes:* The table replicates the California YCTC analysis in Table 1 for the Medicaid sample (Panel A) and Census sample (Panel B). The Medicaid sample includes mothers of children enrolled in Medicaid in the state of California at any point during the prior year. The Census sample is comprised of birth mothers of children born in California; birth parents are identified from the Census Household Composition Key (CHCK) and birth location is according to the Social Security Administration (SSA) Numident file. For the Medicaid sample, the outcome in each column is an indicator for having positive wage or self-employment income reported on third-party information returns. For the Census sample, the outcome is an indicator for positive wage income reported on a W-2 form; all counts and estimates are rounded per Census's disclosure rules governing administrative records. Units are percentage points (0-100). Parentheses report heteroskedasticity-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The Census Bureau has reviewed results from the Census sample to ensure ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product (Data Management System (DMS) Number: P-7503840, Disclosure Review Board (DRB) approval number: CBDRB-FY24-SEHSD003-066.)

Table A.19: Other State CTC Eligibility and Labor Force Participation: Alternative Sample

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Colorado | Maryland | New Jersey | Oregon | Vermont |
| Coefficient | 0.770* | -0.763 | -0.040 | 0.205 | -0.843 |
| | (0.417) | (0.574) | (0.348) | (0.674) | (0.899) |
| 95% CI | [-0.05, 1.59] | [-1.89, 0.36] | [-0.72, 0.64] | [-1.12, 1.53] | [-2.61, 0.92] |
| Control Mean | 70.5 | 73.3 | 71.9 | 69.3 | 71.6 |
| Observations | 47,586 | 91,951 | 255,434 | 73,181 | 10,143 |

*Notes:* The table replicates the analysis from Table 2 for the Medicaid sample. The Medicaid sample is comprised of mothers of children enrolled in Medicaid in the specified state at any point during the prior year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.20: California YCTC Eligibility and Income Reporting

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022) | Diff-in-Disc [(2) - (1)] |
| **Panel A: Reported Wages** | | | |
| Coefficient | 0.076 | 0.362** | 0.287 |
| | (0.124) | (0.185) | (0.222) |
| 95% CI | [-0.17, 0.32] | [0.00, 0.72] | [-0.15, 0.72] |
| Control Mean | 89.2 | 88.0 | 88.8 |
| Observations | 251,699 | 122,017 | 373,716 |
| **Panel B: Reported Schedule SE Income** | | | |
| Coefficient | 0.024 | -0.091 | -0.115 |
| | (0.136) | (0.203) | (0.244) |
| 95% CI | [-0.24, 0.29] | [-0.49, 0.31] | [-0.59, 0.36] |
| Control Mean | 13.4 | 14.8 | 13.8 |
| Observations | 251,699 | 122,017 | 373,716 |
| **Panel C: Reported Earned Income** | | | |
| Coefficient | -0.006 | 0.303* | 0.309 |
| | (0.110) | (0.163) | (0.196) |
| 95% CI | [-0.22, 0.21] | [-0.02, 0.62] | [-0.08, 0.69] |
| Control Mean | 91.7 | 91.0 | 91.5 |
| Observations | 251,699 | 122,017 | 373,716 |

*Notes:* The table replicates the analysis in Table 1 for outcomes based on the income that taxpayers report on their federal tax returns. The outcomes considered in each panel are: an indicator for reporting positive wage income (Panel A); an indicator for reporting positive self-employment income (Panel B); and an indicator for reporting positive earned income (defined as the sum of wage and self-employment income) (Panel C). Units are percentage points (0-100). Each outcome takes on a value of zero if the individual does not file a tax return. Self-employment income is measured as the sum of net profits reported on each Schedule SE that the taxpayer files. The analysis excludes filing outcomes for tax year 2023, for which filing data was not available to us at the time of our analysis. Tax returns were excluded from the analysis if they were filed after the end of the calendar year following the tax year covered by the return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.21: Other State CTC Eligibility and Income Reporting

| | (1) | (2) | (3) |
|---|---|---|---|
| | Colorado | New Jersey | Vermont |
| **Panel A: Reported Wages** | | | |
| Coefficient | 0.194 | -0.077 | 0.074 |
| | (0.316) | (0.240) | (0.659) |
| 95% CI | [-0.43, 0.81] | [-0.55, 0.39] | [-1.22, 1.37] |
| Control Mean | 89.5 | 91.1 | 91.8 |
| Observations | 37,382 | 217,986 | 6,937 |
| **Panel B: Reported Schedule SE Income** | | | |
| Coefficient | -0.522 | 0.062 | -0.151 |
| | (0.409) | (0.322) | (0.951) |
| 95% CI | [-1.32, 0.28] | [-0.57, 0.69] | [-2.02, 1.71] |
| Control Mean | 19.6 | 17.6 | 19.6 |
| Observations | 37,382 | 217,986 | 6,937 |
| **Panel C: Reported Earned Income** | | | |
| Coefficient | 0.208 | 0.005 | 0.062 |
| | (0.275) | (0.206) | (0.578) |
| 95% CI | [-0.33, 0.75] | [-0.40, 0.41] | [-1.07, 1.20] |
| Control Mean | 92.2 | 93.6 | 93.8 |
| Observations | 37,382 | 217,986 | 6,937 |

*Notes:* The table replicates the analysis in Table 2 for outcomes based on the income that taxpayers report on their federal tax returns. The analysis is limited to Colorado, New Jersey, and Vermont, which provided a state-level CTC for tax year 2022 – the most recent year for which reported income data is available to us at the time of our analysis. For the same reason, the analysis excludes reporting outcomes for tax year 2023. The outcomes considered in each panel are: an indicator for reporting positive wage income (Panel A); an indicator for reporting positive self-employment income (Panel B); and an indicator for reporting positive earned income (defined as the sum of wage and self-employment income) (Panel C). Units are percentage points (0-100). Each outcome takes on a value of zero if the individual does not file a tax return. Self-employment income is measured as the sum of net profits reported on each Schedule SE that the taxpayer files. Tax returns were excluded from the analysis if they were filed after the end of the calendar year following the tax year covered by the return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.22: California YCTC Eligibility and Tax Filing

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022) | Diff-in-Disc [(2) - (1)] |
| Coefficient | 0.014 | 0.365** | 0.351* |
| | (0.100) | (0.160) | (0.188) |
| 95% CI | [-0.18, 0.21] | [0.05, 0.68] | [-0.02, 0.72] |
| Control Mean | 93.3 | 91.3 | 92.6 |
| Observations | 251,699 | 122,017 | 373,716 |

*Notes:* The table replicates the analysis in Table 1 for the outcome of filing a federal tax return. The analysis excludes filing outcomes for tax year 2023, for which filing data was not available to us at the time of our analysis. Units are percentage points (0-100). Tax returns were excluded from the analysis if they were filed after the end of the calendar year following the tax year covered by the return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.23: Other State CTC Eligibility and Tax Filing

| | (1) | (2) | (3) |
|---|---|---|---|
| | Colorado | New Jersey | Vermont |
| Coefficient | 0.220 | -0.002 | -0.157 |
| | (0.268) | (0.202) | (0.564) |
| 95% CI | [-0.31, 0.75] | [-0.40, 0.39] | [-1.26, 0.95] |
| Control Mean | 92.7 | 93.9 | 94.2 |
| Observations | 37,382 | 217,986 | 6,937 |

*Notes:* The table replicates the analysis in Table 2 for the outcome of filing a federal tax return. The analysis is limited to Colorado, New Jersey, and Vermont, which provided a state-level CTC for tax year 2022 – the most recent year for which tax filing data is available to us at the time of our analysis. For the same reason, the analysis excludes filing outcomes for tax year 2023. Units are percentage points (0-100). Tax returns were excluded from the analysis if they were filed after the end of the calendar year following the tax year covered by the return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.24: California YCTC Eligibility and Small-Dollar Income Reporting

| | (1) | (2) | (3) |
|---|---|---|---|
| | Work Req. (2019-2020) | No Work Req. (2022) | Diff-in-Disc [(2) - (1)] |
| **Panel A: Earned Income between 1 and 1,000** | | | |
| Coefficient | 0.044* | 0.022 | -0.022 |
| | (0.024) | (0.024) | (0.034) |
| 95% CI | [-0.00, 0.09] | [-0.02, 0.07] | [-0.09, 0.04] |
| Control Mean | 0.3 | 0.2 | 0.3 |
| Observations | 251,699 | 122,017 | 373,716 |
| **Panel B: Earned Income between 1 and 5,000** | | | |
| Coefficient | 0.178*** | 0.137** | -0.041 |
| | (0.061) | (0.066) | (0.090) |
| 95% CI | [0.06, 0.30] | [0.01, 0.27] | [-0.22, 0.13] |
| Control Mean | 2.4 | 1.3 | 2.0 |
| Observations | 251,699 | 122,017 | 373,716 |
| **Panel C: Earned Income between 1 and 10,000** | | | |
| Coefficient | 0.152 | 0.327*** | 0.175 |
| | (0.096) | (0.110) | (0.146) |
| 95% CI | [-0.04, 0.34] | [0.11, 0.54] | [-0.11, 0.46] |
| Control Mean | 6.2 | 3.7 | 5.3 |
| Observations | 251,699 | 122,017 | 373,716 |

*Notes:* The table replicates the analysis in Table 1 for the outcome of filing a tax return reporting low dollar amounts of earned income. The outcomes considered are indicators for reporting positive earned income less than \$1,000 (Panel A), \$5,000 (Panel B), and \$10,000 (Panel C). Earned income is defined as reported wages and self-employment income. Outcomes take on a value of zero if the individual did not file a tax return. Units are percentage points (0-100). The analysis excludes reporting outcomes for tax year 2023, for which filing data was not available to us at the time of our analysis. Tax returns were excluded from the analysis if they were filed after the end of the calendar year following the tax year covered by the return. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.25: Substitution Elasticities By State

| | Work Requirement, Below Phase-Out | | No Work Requirement, Above Phase-Out | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | California | Colorado | California | Maryland | New Jersey | Oregon | Vermont |
| *Labor Force Participation* | | | | | | | |
| Effect | 0.045 | 0.247 | -0.078 | 0.243 | -0.124 | -0.076 | -1.725** |
| | (0.307) | (0.439) | (0.103) | (0.231) | (0.149) | (0.334) | (0.875) |
| Control Mean | 91.5 | 93.3 | 95.9 | 96.6 | 97.3 | 96.0 | 98.0 |
| Percent Change | 0.0 | 0.3 | -0.1 | 0.3 | -0.1 | -0.1 | -1.8 |
| *Return to Work* | | | | | | | |
| Change in Return to Work | 1,000 | 1,450 | -1,099 | -500 | -743 | -1,000 | -1,000 |
| Control Mean | 23,116 | 25,618 | 90,876 | 86,732 | 126,635 | 86,505 | 150,887 |
| Percent Change | 7.4 | 5.2 | -1.6 | -0.8 | -0.6 | -1.5 | -0.7 |
| Elasticity | 0.01 | 0.05 | 0.05 | -0.31 | 0.21 | 0.05 | 2.65 |
| | (0.05) | (0.09) | (0.07) | (0.29) | (0.25) | (0.24) | (1.35) |
| 95% CI | [-0.08, 0.10] | [-0.13, 0.23] | [-0.08, 0.19] | [-0.88, 0.26] | [-0.28, 0.69] | [-0.42, 0.52] | [0.02, 5.29] |
| Income Range | $1 - $25,000 | $1 - $35,000 | > $30,000 | > $15,000 | > $80,000 | > $30,000 | > $175,000 |
| Work Requirement | Yes | Yes | No | No | No | No | No |

*Notes:* The table reports estimated extensive-margin substitution elasticities, as described in Section 6. Columns 1 and 2 correspond to state-years for which the applicable credit is conditioned on work; Columns 3-7 correspond to state-years for which the applicable credit is available to non-workers. Column 1 includes California years for 2019 and 2020; Column 3 includes California years for 2022 and 2023. Each column is limited to individuals in the sample for the specified state (reported in 1 and Table 2) who filed a prior-year tax return listing AGI within the specified income range. refers to the estimated percentage point effect of age-eligibility for the applicable credit on labor force participation, following the applicable specification from Table 1 or Table 2. is estimated for mothers in the sample whose children are not age-eligible for the credit for their state. is obtained from dividing the estimated labor force participation effect by the control mean. The change in return to work refers to the simulated effect of age-eligibility for the credit on the net financial gain from working relative to not working, accounting for pre-tax income (measured as prior-year AGI), state and federal income taxes and credits, and SNAP benefits. The simulations are based on prior-year AGI, filing status, and number of dependents. is calculated by dividing the percent change in labor force participation by the percent change in the return to work. The reported standard error and 95% confidence interval for the elasticity are calculated assuming that uncertainty in the labor force participation effect is the only source of uncertainty. For additional details, refer to Appendix C.

Table A.26: Income Elasticities By State

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | California | Maryland | New Jersey | Oregon | Vermont |
| *Labor Force Participation* | | | | | |
| Effect | -0.094<br>(0.345) | 0.534<br>(1.206) | 0.187<br>(0.363) | 0.508<br>(1.019) | -0.222<br>(0.448) |
| Control Mean | 92.1 | 92.2 | 94.1 | 93.7 | 96.1 |
| Percent Change | -0.1 | 0.6 | 0.2 | 0.5 | -0.2 |
| *Non-Working Benefits* | | | | | |
| Change in Benefits | 1,100 | 500 | 752 | 1,000 | 1,000 |
| Control Mean | 684 | 698 | 681 | 735 | 817 |
| Percent Change | 174.3 | 76.8 | 116.3 | 147.0 | 131.6 |
| Elasticity | -0.00<br>(0.00) | 0.01<br>(0.02) | 0.00<br>(0.00) | 0.00<br>(0.01) | -0.00<br>(0.00) |
| 95% CI | [-0.00, 0.00] | [-0.03, 0.04] | [-0.00, 0.01] | [-0.01, 0.02] | [-0.01, 0.01] |
| Income Range | $1 - $25,000 | $1 - $15,000 | $1 - $30,000 | $1 - $25,000 | $1 - $125,000 |
| Work Requirement | No | No | No | No | No |

*Notes:* The table reports estimated extensive-margin income elasticities, as described in Section 6. The analysis is limited to state-years for which the applicable credit was not conditioned on work and to individuals who filed a prior-year tax return reporting AGI within the specified income range. Each column is limited to individuals in the sample for the specified state (reported in 1 and Table 2). refers to the estimated percentage point effect of age-eligibility for the applicable credit on labor force participation, following the applicable specification from Table 1 or Table 2. is estimated for mothers in the sample whose children are not age-eligible for the credit for their state. is obtained from dividing the estimated labor force participation effect by the control mean. The change in Non-Working Benefits refers to the simulated effect of age-eligibility for the credit on the after-tax and transfer income associated with not working, accounting for state and federal refundable tax credits and SNAP benefits. The simulations are based on prior-year AGI, filing status, and number of dependents. is calculated by dividing the percent change in labor force participation by the percent change in the benefits associated with not working. The reported standard error and 95% confidence interval for the elasticity are calculated assuming that uncertainty in the labor force participation effect is the only source of uncertainty. For additional details, refer to Appendix C.

# B Work Requirement Earnings Threshold and Labor Participation Response

This Appendix section considers a simple labor supply model to study how taxpayers respond to the \$1 YCTC work requirement as compared to a work requirement tied to a higher earned income threshold.

Individuals choose whether and how much to work. Labor effort is indexed by $l \in \{0\} \cup [1, \infty)$, with $l \geq 1$ denoting work and $l = 0$ denoting not working. Let $u(l)$ refer to the utility in money-metric units to an individual from working $l \in [1, \infty)$ units, accounting for the disutility of work and the utility benefits from earning income, as well as any tax implications or other program benefits other than the program whose work requirement is being considered (e.g., the YCTC).

Absent any new program, an individual will choose to work if the utility from working exceeds the utility from not working, for some positive amount of work $l \geq 1$. Thus, the individual will choose to work if and only if the utility associated with the optimal amount of positive work, $u_1$, exceeds the utility from not working, $u_0$, where

$$u_1 := \max_l u(l) \text{ s.t. } l \geq 1$$

and $u_0$ is normalized to zero. The share of the population that works (absent the work requirement under consideration) is therefore given by $Pr(u_1 > 0)$.

Consider a benefit program with a work requirement that provides $Y$ dollars if an individual works at least $k \geq 1$ units, and provides no benefit otherwise. We will refer to this policy as $B_k$.

Under this policy, if an individual works at least $k$ units, the individual's utility is given by $u_k + Y$, where

$$u_k := \max_l u(l) \text{ s.t. } l \geq k$$

Note that by construction, $k' > k$ implies $u_k \geq u_{k'}$. This follows from the fact that $u_{k'}$ and $u_k$ are defined to equal the maximum of the same function, but the set over which the former is evaluated is a strict subset of the set over which the latter is evaluated: $\{l : l \geq k'\} \subset \{l : l \geq k\}$.

Under policy $B_k$, an individual's utility from working is the maximum of $u_1$ and $u_k + Y$. Intuitively, either the individual chooses to work at least $k$, in which case they obtain utility $u_k + Y$, or they work some positive amount less than $k$, in which case they would continue to receive $u_1$ (i.e., the best they could obtain absent the benefit program with the work requirement). Thus, under policy $B_k$, the individual will work if and only if $u_1 > 0$ or $u_k + Y > 0$. The share of the population that works is thus given by: $Pr(u_1 > 0) + Pr(u_1 \leq 0 \;\&\; u_k \geq -Y)$.

Thus, the effect of introducing policy $B_k$ on the share of the population that works, $\beta_k$, is given by:

$$\begin{aligned}
\beta_k &= \underbrace{Pr(u_1 > 0) + Pr(u_1 \leq 0 \ \& \ u_k + Y \geq 0)}_{\text{Share Working Under } B_k} - \underbrace{Pr(u_1 > 0)}_{\text{Share Working Absent } B_k} \\
&= Pr(u_1 \leq 0 \ \& \ u_k + Y \geq 0) \\
&= Pr(u_k + Y \geq 0 \mid u_1 \leq 0)\, Pr(u_1 \leq 0)
\end{aligned}$$

Finally, note that for any $k' > k$, we have:

$$\begin{aligned}
\beta_{k'} &= Pr(u_{k'} + Y \geq 0 \mid u_1 \leq 0)\, Pr(u_1 \leq 0) \\
&\leq Pr(u_k + Y \geq 0 \mid u_1 \leq 0)\, Pr(u_1 \leq 0) \\
&= \beta_k
\end{aligned}$$

where the inequality follows from the fact (discussed above) that for each individual $k' > k$ implies $u_k \geq u_{k'}$.

# C Labor Supply Elasticity Calculations

This Appendix provides additional detail on the elasticity calculations based on our estimates of the effect of child tax credit eligibility on labor force participation.

The outcome of interest is mean labor force participation, denoted by $Y$. Let $Y(1)$ denote mean labor force participation among those whose children are age-eligible for the applicable credit, and let $Y(0)$ denote mean labor force participation among those who children are not age-eligible for the applicable credit.

The extensive-margin substitution elasticity we consider is defined as:

$$\frac{\%\Delta \text{ labor force participation}}{\%\Delta \text{ return to work}}. \tag{1}$$

The numerator of (1) reflects the percent change in the fraction of the population that works based upon age-eligibility for the credit, defined as:

$$\%\Delta \text{ labor force participation} = \frac{Y(1) - Y(0)}{Y(0)} \tag{2}$$

The denominator of (1) reflects the percent change in the return-to-work, $RTW$, accounting for taxes and transfers:

$$\%\Delta \text{ return to work} = \frac{RTW(1) - RTW(0)}{RTW(0)} \tag{3}$$

where $RTW(1)$ and $RTW(0)$ denote the mean after-tax-and-transfer return-to-work with and without age-eligibility for the credit. In turn, the after-tax-and-transfer return-to-work is defined as the difference in income one would receive from working compared to not working, accounting for pre-tax income, federal and state income taxes (including credits), and SNAP benefits.

We calculate the after-tax-and-transfer return to work for various income levels ranging from \$100 to \$200,000 (in \$100 increments) and calculate federal and state taxes (including refundable tax credits) using NBER's TAXSIM. In some cases we manually model state child tax credits that have not yet been fully incorporated into TAXSIM. Because the California YCTC work requirement induced a very large return to work in percentage terms for individuals with near-zero income, we group individuals with income below \$100 into the \$100 bin to limit the influence of these outliers on the mean return-to-work for our sample. We calculate SNAP benefits based on household (assumed to be the tax unit) income and size, using the benefit formula for the federal SNAP program. We assume that households with no earned income would receive the maximum SNAP benefits available given their household size. Households that do not work are technically eligible for other programs such as cash welfare. Because take up of those benefits tend to hover in the 20-30% range, we disregard the value of cash welfare for this exercise.

The extensive-margin income elasticity we consider is defined as

$$\frac{\%\Delta \text{ labor force participation}}{\%\Delta \text{ benefits from not working}}. \tag{4}$$

As in (2), the numerator of (4) reflects the percent change in the fraction of the population that works based on age-eligibility for the credit.

The denominator of (4) reflects the percent change in the after-tax and transfer benefits one would obtain from not working based on age-eligibility for the credit:

$$\%\Delta \text{ benefits from not working} = \frac{BNW(1) - BNW(0)}{BNW(0)} \tag{5}$$

where $BNW(1)$ and $BNW(0)$ denote the mean after-tax-and-transfer benefits one would receive from not working with and without age-eligibility for the credit. As above, the benefits from not working account for federal and state refundable tax credits as well as SNAP benefits.