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Child Tax Benefits and Labor Supply: Evidence from California

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ABSTRACT

The largest tax-based social welfare programs in the US limit their benefits to taxpayers with labor market income. Eliminating these work requirements would better target transfers to the neediest families but risks attenuating tax-based incentives to work. We study changes in labor force participation from the elimination of a work requirement in a tax credit for parents of young children, drawing on quasi-random variation in birth timing and administrative tax records. To do so, we develop and implement a novel approach for selecting an empirical specification to maximize the precision of our estimate. The unique design of the policy along with its subsequent reform allow us to isolate taxpayers' sensitivity to conditioning child tax benefits on work -- the parameter at the center of recent debates about the labor supply consequences of reforming federal tax policy for children. We estimate that eliminating the work requirement causes very few mothers to exit the labor force, with a 95% confidence interval excluding labor supply reductions of one-third of a percentage point or greater. Our results suggest expanding tax benefits for low-income children need not meaningfully reduce labor force participation.

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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] However, in the United States today, some of the largest social welfare programs focused on children exclude the lowest income families from their scope. In particular, 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 low-income parents[2] – but doing so risks attenuating the economic 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 draw upon a unique policy and its subsequent reform. In 2019, California created the Young Child Tax Credit (YCTC), a refundable state tax credit for low-income parents of children below the age of 6. Initially, like the federal CTC, the YCTC was available only to taxpayers with positive earned income during the year. However, unlike its federal analogue, the YCTC was not phased-in: the full amount of the credit (up to \$1,000 per return) was available to low-income parents who earned \$1 or more during the tax year. Then, beginning in 2022, California eliminated the work requirement altogether, allowing any taxpayer who otherwise qualified for the YCTC to claim the full credit amount even if that taxpayer had no earned income during the tax year. Focusing on mothers with young children, we estimate the effect of the work requirement by comparing

[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); Rittenhouse (2023); Bhardwaj (2023).

[2] With respect to the EITC, for example, Burman (2019) and National Taxpayer Advocate (2020) propose replacing the EITC with a flat means-tested subsidy based on family size, along with a per-worker income subsidy. With respect to the CTC, many have proposed making the credit fully refundable, so that low-income taxpayers can receive the full credit amount; the House of Representatives passed legislation along these lines in 2021.

the effect of the YCTC before versus after the reform.

To conduct our analysis, we use administrative tax records and quasi-random birth timing to compare mothers' labor force participation based on their exposure to the policy change. An important feature of our data is that we are able to measure labor force participation from wage and self-employment income reported in third party information returns; this increases the likelihood that any change in labor supply we observe reflects a real change in behavior rather than simply a change in what taxpayers report. We focus on mothers who worked in California during the prior year and whose youngest child turned six just before or just following the YCTC's age-eligibility cutoff. We select our empirical specification to minimize the (out-of-sample) estimation error of pseudo-treatment effects during a set of placebo years prior to the adoption of the policy. Using this specification, we estimate the difference in labor force participation between mothers of age-eligible children versus age-ineligible children during the time period in which the YCTC was subject to the work requirement, as well as during the time period in which it was not. Under plausible assumptions, the difference in these differences corresponds to the effect of eliminating the work requirement.

We find that eliminating the YCTC work requirement did not cause a significant number of California mothers to exit the labor force. Our main specification yields a precisely estimated but very small reduction in labor force participation (0.06 percentage points), with a 95% confidence interval ranging from a reduction of 0.35 percentage points to an increase of 0.23 percentage points.

We supplement our main analysis with a second sample consisting of California mothers whose children were enrolled in Medicaid. The Medicaid sample complements our main analysis because it includes a group of low-income mothers who did not work during the prior year; it therefore allows us to study whether the work requirement shapes the flow of nonworking mothers into the labor force. Here too, our estimated 95% confidence interval excludes substantial effects of the elimination of the work requirement on labor supply.

We validate our identifying assumptions with several placebo exercises. These replicate our main analysis for mothers in other states or time periods; for mothers whose youngest child turned five (instead of six) around the turn of the outcome year; and for mothers with a younger child who see no difference in their YCTC eligibility when their older child turn six. We find no differences in labor supply among mothers of children on either side of the age cutoff for any of these placebo groups.

We conduct several additional analyses to explore the robustness of our results. We find similar results when using reported earnings instead of third-party earnings to measure labor force participation. We also provide evidence that the effect of YCTC eligibility on labor supply during the work requirement period was stable over time, suggesting that our null results are not driven by under-awareness of the policy in the first year of its adoption relative to subsequent years. Finally, our qualitative results are robust to alternative specifications, such as including mothers of similar-aged children in other states as an additional control group or more conventional regression discontinuity specifications.

To extrapolate beyond the specific policy change we study, we translate our estimated labor supply effect into an estimate for the elasticity of labor force participation with respect to the after-tax return to work. Our reduced form results correspond to an elasticity of 0.01, or 0.06 when we focus on the lower end of our estimated 95% confidence interval. Finally, we use these elasticities to estimate the effect of a reform that would expand the federal CTC to fully cover the children of low- and zero-income parents, along the lines of a reform that was temporarily enacted for tax year 2021. Our estimates suggest this policy change would result in fewer than 155,000 parents leaving the labor force, an estimate substantially below what analyses based on prior empirical work have estimated (Goldin, Maag and Michelmore, 2022; Corinth et al., 2021; Bastian, Forthcoming), but consistent with recent empirical work that has studied the effects of the 2021 CTC expansion (Ananat et al., 2022; Enriquez, Jones and Tedeschi, 2023; Pac and Berger, 2024). We interpret our results to suggest that the labor market consequences of expanding the generosity of child benefits to low income families may be less than what would have been predicted on the basis of prior research.

Our paper builds on a long line of research studying the effect of means-tested programs on labor supply. Much of this literature investigates the extent to which the phase-out of welfare benefits reduces labor supply via high effective marginal tax rates (Moffitt, 1992; Hoynes, 1997; Ziliak, 2015). A more closely related strand investigates the effects on labor supply of programs that incentivize labor force participation, with particular focus on the EITC. Much of this literature documents a substantial effect of the EITC's work incentive on the extensive-margin of labor force participation, with little to no effect on the intensive margin (for reviews, see Eissa and Hoynes, 2006; Nichols and Rothstein, 2016; Schanzenbach and Strain, 2021).[3] In contrast, a growing literature studying the effect of work requirements in safety net programs outside of the tax code has found more mixed evidence on labor supply. In particular, several studies find no evidence of substantial labor supply effects of work requirements in SNAP (Han, 2022; Gray et al., 2023) and Medicaid (Sommers et al., 2020), whereas others do find evidence along these lines, such as Hoynes and Schanzenbach (2012) and Harris (2021) for SNAP and Falk (2023) for TANF.

Our results are also closely related to a literature studying the labor supply effects of child tax benefits, both in the United States and in other countries. These benefits usually take one of two forms: a universal child allowance (i.e., a constant per-child benefit, often with an income phase-out), such as the Canada Child Benefit or Spain's "baby-checks", or a child benefit that phases benefits in based on work, such as the federal CTC in the US. Studies focused on the first category of policies identify the income effect associated with the benefit program. These studies report mixed results, with some finding negative effects on labor supply (González, 2013; Schirle, 2015; Wingender and LaLumia, 2017; Jensen and Blundell, 2024; Lippold and Luczywek, 2024), others finding no effect (Messacar, 2021; Baker, Messacar and Stabile, 2023), and some finding positive effects (Feldman, Katuscak and Kawano, 2016). Other studies focus on policies in the second category, which condition

[3] One exception is Kleven (2023) who finds minimal labor supply effects of the EITC expansions previously studied in the literature. In general, more recent studies, as well as studies focusing on more recent policy changes, have tended to find smaller elasticities (e.g., Lin and Tong, 2017; Hoynes and Patel, 2018; Bastian and Jones, 2021).

benefits on work (Francesconi and Van der Klaauw, 2007; Sánchez-Mangas and Sánchez-Marcos, 2008; Milligan and Stabile, 2007; Mortenson et al., 2018; Lippold, 2022). Because these policies are tied to work, the studies focusing on them generally identify a labor supply response that mixes both income and substitution effects, where the specific mix varies across policies and studies. In contrast, the policy reform we study allows us to cleanly isolate the extensive-margin substitution effect associated with the change in tax incentives facing parents. This substitution effect is a key input into modeling the effect of removing a program's work requirement and is the parameter at the center of debates regarding potential federal tax policy reforms (Corinth et al., 2021; Bastian, Forthcoming).

Closest to our own 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 the 2021 tax reform (Pac and Berger, 2024; Ananat et al., 2022; Enriquez, Jones and Tedeschi, 2023). This policy change is particularly close to the one we study – both involve the elimination of a work-requirement for obtaining a child tax benefit.[4] We complement these studies – all of which rely on survey data and an event-study design comparing labor supply trends between parents and non-parents – 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 type settings must make a number of modeling choices relating to the empirical specification they employ (Lee and Lemieux, 2010). We propose and implement a data-driven method for making these choices that draws on the availability of a range of placebo samples. Specifically, we select the elements of our empirical specification (e.g., bandwidth, polynomial order) to minimize the mean squared

[4] A related set of recent papers modeled the labor supply effects of the 2021 CTC reform using previously estimated elasticities (Goldin, Maag and Michelmore, 2022; Corinth et al., 2021; Bastian, Forthcoming).

error when the estimator is applied to samples of California mothers in the years prior to the YCTC's adoption. In focusing on mean squared error, our approach shares the objective behind popular existing methods for specification selection in regression discontinuity settings (Imbens and Kalyanaraman, 2012; Calonico, Cattaneo and Titiunik, 2014; Pei et al., 2022). The novel aspect of our procedure is to evaluate the performance of each candidate specification in placebo settings that plausibly approximate the distribution of potential outcomes across the threshold in our actual sample of interest (i.e., the turn-of-the-year for samples of California mothers in years prior to the introduction of the YCTC). Doing so allows us to choose among potential specifications to maximize precision while avoiding concerns of over-fitting.[5] Jointly optimizing over bandwidth and polynomial order – as advocated by Hall and Racine (2015) in a related context – highlights important interactions between the two modeling choices. The specification we end up selecting based on this approach yields substantially more precise results compared to the specification employed in recent papers that exploit similar variation in birth-timing. More generally, our proposed approach can inform the choice of specification in regression discontinuity designs when suitable placebo datasets are available to the researcher.

2 Institutional Background

At the federal level, the US income tax code provides a number of benefits for taxpayers who claim children on their returns. Our focus is on 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.[6] The credit is partially refundable, with the refundable portion

[5] Ludwig and Miller (2007) and Imbens and Lemieux (2008) also propose bandwidth selection procedures that avoid over-fitting by evaluating performance out-of-sample; an important difference between those procedures and our approach is that we assess the accuracy of the specification using the actual threshold relied on for identification (i.e., the turn-of-the-year), which may yield a different bias or variance than other pseudo-cutoffs. Cattaneo, Idrobo and Titiunik (2019) discuss the inclusion of a regularization term in the bandwidth selection objective, which is another potential route for addressing over-fitting concerns when evaluating MSE in-sample.

[6] The CTC has been reformed a number of times since its introduction in 1997; we focus on the rules in place for tax year 2019.

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 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.[7]

Turning from federal to state tax policy, a growing number of states provide their own child tax credits in addition to the federal benefit.[8] Our focus is on a policy change in the design of one such benefit: California's Young Child Tax Credit (YCTC). Beginning in 2019, the YCTC provides 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.

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

[7]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.

[8]As of 2023, 15 states offer a child tax benefit. These policies vary with respect to their maximum benefit (from $100 to $1,750 per child), the age of the dependent child, the income range on which they are focused, and whether taxpayers must work in order to claim them.

are not phased in. Thus, for California taxpayers who qualify for the YCTC, 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 one dollar or more of earned income qualified for the full benefit amount, assuming they were otherwise eligible (see Panel A of Figure 1). 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).

3 Data

We draw on federal administrative tax records for our analysis. A benefit of this data is that it includes the universe of children who receive social security numbers as well as the parents listed on those children's birth certificates. The major limitation of this data for our purposes is that unless the individual is listed on a federal tax return or third-party information return, we are unable to assign that individual to a state. For this reason, we limit our analyses to various subsets of taxpayers for whom we have a recent information return linking them to a particular state, as described below.

Our main sample consists of California mothers who worked during the prior year whose youngest child turns six around the start of one of our policy 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 policy year. This cohort consists of the universe of U.S. children who turned six years old during the final months of the policy year as well as those children who turned six years old during the first months of the year following the policy 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 YCTC in the policy year whereas children in the former category are not. Second, we link children to the individual listed as the child's mother on the child's birth certificate. Third, 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 policy year. This restriction excludes mothers who would continue to qualify for the YCTC despite their reference child aging out of eligibility.[9] 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 policy year.[10] We assign individuals to states based on the taxpayer's residence information listed on this form. Finally, for our main analysis, we restrict this sample to the subset of mothers from California.

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. At the same time, a downside of this data set is that it does not allow us to study effects on the flow of non-working individuals into the labor force. To address this issue, we also consider a secondary data set consisting of California mothers whose children were enrolled in Medicaid during the prior year. This Medicaid data set includes individuals whose children were enrolled in Medicaid or the Children's Health Insurance Program (CHIP) but were themselves outside of the labor force. Crucially for our purposes, individuals in these categories receive Form 1095-B, showing their health 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.

[9] Because this restriction is based on the presence of younger children born *prior* to the policy year, mothers who give birth to a child during the policy year will remain eligible for the credit. We investigate the sensitivity of our results to this issue below.

[10] We do not otherwise limit our main sample based on income because, in principle, even individuals' with incomes above the YCTC cutoff may change their behavior in response to the policy. We present subgroup results for high- and low-income taxpayers below.

Our primary outcome is whether an individual works during the policy year. It is constructed based on whether the IRS receives an information return (Form W-2, 1099-Misc, or 1099-NEC) for the individual showing positive income during the policy year. 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, 2022). We observe these outcomes from 2000 through 2023, although some of the data is incomplete in the first few years of this time period and, as of this writing, for 2023.

4 Empirical Framework

Our empirical strategy for estimating the effect of eliminating the YCTC's work requirement is to combine quasi-random variation in birth timing with the policy change in the YCTC's design. Specifically, we compare the labor supply participation of mothers whose youngest child turns six before the end of a given year with mothers whose youngest child turns six after the start of the subsequent year. To estimate the effect of eligibility for the YCTC with a work requirement, we make this comparison for the years following the YCTC's adoption and before the policy change to its design. To estimate the effect of eligibility for the YCTC without a work requirement, we make this comparison for the years following the change in the YCTC's design. We make these comparisons using generalized 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; γ_t is a set of year fixed effects; and $g_1(\cdot)$ and $g_2(\cdot)$

are polynomials. In this specification, the effect of having a child of an age that qualifies a mother for the YCTC is given by β. The difference in the estimated values of β across policy periods (i.e., for the work requirement years versus the years without a work requirement) forms our estimate for the effect of the YCTC work requirement on labor force participation.

4.1 Empirical Specification Selection

In this subsection, we consider alternative versions of (1) for estimating the effect of the YCTC during a given policy period. We consider three specification choices: (1) the width of the birth-timing window to include in our sample (from 1 month to 24 months surrounding the turn-of-the-year); (2) whether and how to control for differences in the age-eligible and ineligible groups in birth timing (i.e., imposing that $g_1(\cdot)$ and $g_2(\cdot)$ are polynomials of degree 0, 1, or 2); and (3) whether to exclude children born around the end-of-the-year holidays through a "donut" specification (e.g., Barreca et al., 2011). 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 evaluate these trade-offs empirically based on the out-of-sample performance of each specification at estimating the effect of placebo policies in the years before the YCTC was introduced. Specifically, we estimate the effect of the pseudo-policy in each year between 2005 and 2018, using the turn-of-the-year as the assumed threshold for eligibility, and calculate the mean of the squared errors. Because the true "effect" of each pseudo-policy is zero, the squared error for each year corresponds to the square of the estimated coefficient for that year.[11] We then choose among the alternative specifications to minimize the empirical MSE of our treatment effect estimator. Under the assumption that the data generating process for

[11]We begin this analysis in 2005 rather than 2000 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.

the YCTC potential outcomes during our sample period is well approximated by the data generating process during the 2005-2018 pre-period, this analysis sheds light on the relative precision of alternative estimation strategies for our sample period.

Figure 2 presents the results of this exercise. 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. Notably, 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 correspond to common regression discontinuity specifications that respectively adjust for differences in the running variable (date of birth) with a first or second degree polynomial. 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 yield a larger RMSE than the RMSE-minimizing linear estimator.

Finally, we consider the effect on precision of analyzing a donut specification in which we exclude children born around the turn-of-the-year separately for the three specifications in Figure 2. As shown in Appendix Figure A.2, this specification choice appears to increase the variance of estimates for narrow birth-timing windows but does not otherwise appear to have an important effect on the performance of the estimator.

Based on these results, for our primary analysis we focus on the unadjusted difference in means, calculated using a four-month birth-window, and we do not employ a donut specification. This specification differs substantially from the RD specification used in recent empirical papers that employ a similar identification strategy (Barr, Eggleston and Smith, 2022; Rittenhouse, 2023; Bhardwaj, 2023; Lippold and Luczywek, 2024) but according to the results in this section, tends to yield a substantially more precise estimate.[12]

Appendix Table A.1 presents summary statistics for the sample of mothers corresponding to this specification – i.e., mothers whose youngest child turns six in the final four months of the year or the first four months of the subsequent year. The statistics in the table are calculated based on the mothers' tax records from the *prior* year, i.e., the year before their youngest child turns six. The first panel presents characteristics based solely on third-party information returns, while the second panel presents return-level information for the 95% of mothers who filed a return. In the year prior to the policy year, mothers in our sample are on average 35 years old. By construction, all mothers have positive income in the year prior to the policy year with an average (individual) income of approximately $53,000 with 12% receiving some self-employment income. As mentioned above, the vast majority (95%) filed a tax return. Of that group, 55% are married with the average household reporting an adjusted gross income of $113,000, reflecting that more than half of the sample files jointly with their spouse. The average household claims just under two children and roughly one third of those who filed claimed the federal EITC and 90% claimed the federal CTC. Since our data comes from federal administrative tax records, we do not have data on claiming rates for California's state EITC or the YCTC; however, recent work suggests that the vast majority

[12] Appendix Figure A.3 compares the distribution of estimated pseudo-effects from our preferred specification (4 month bandwidth, polynomial order 0) and from the regression discontinuity specification employed in these recent papers (4 week bandwidth, donut, polynomial order 1). We observe a similar pattern when comparing the difference in differences estimator based on our preferred specification to the difference in discontinuity estimator based on the above regression discontinuity specification (Appendix Figure A.4).

(92%) of taxpayers who claimed the federal EITC and appear eligible for California's EITC claimed the state EITC as well (Iselin, Mackay and Unrath, 2023).

Columns 2 and 3 of Appendix Table A.1 presents prior-year characteristics of mothers whose youngest child falls on either side of the age cutoff, respectively. Comparing mothers of children who were age-eligible for the YCTC during the policy year versus those who were not, we observe small differences between the groups. For example, mothers of children born before the end of the year tend to be slightly older, on average, than mothers of children born at the start of the next year.

Finally, to interpret these estimates as the causal effect of eliminating the YCTC work requirement, we impose two identifying assumptions. First, we assume that, but for the difference in policy to which they are exposed, mothers in the age-eligible and age-ineligible groups would have the same average labor force participation during each policy period.[13] This assumption would be violated, for example, 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 YCTC, such as from compositional differences in the timing of births. Second, we assume that but for the elimination of the work requirement, the effect of the YCTC 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.

5 Results

In this section, we present our main results, using the specification described in Section 4. This specification compares mean labor force participation between mothers whose youngest child turns six during the last four months of an outcome year (age-ineligible group) and mothers whose child turns six during the first four months of the subsequent year (age-eligible group). Our estimate of the effect of eliminating the YCTC work requirement is

[13] This assumption is sufficient but not necessary for our difference-in-differences estimator to be unbiased. Our main estimate would also be unbiased if potential outcomes differed across age-eligibility groups, but in a manner that is the same on average across policy periods.

given by the difference in these differences for outcome years before versus after the change in the YCTC design. As described above, our outcome of interest is maternal labor force participation, which we define as having positive wage or self-employment income reported on a third-party information return for the outcome year.

5.1 Main Results

Figure 3 shows labor force participation rates by year among California mothers in the age-eligible and age-ineligible groups. 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 most years, the labor force participation rates of the two groups appear similar to one another.[14]

Table 1 reports our main results. Columns 1 and 2 reports the estimated effect on labor force participation of age-eligibility for the YCTC, before and after the elimination of the work requirement. The reported effect corresponds to the estimate for β in the following regression:

$$Y_{it} = \alpha + \beta\, AgeEligible_i + \gamma_t + \varepsilon_{it} \tag{2}$$

where Y_{it} is an indicator for whether or not mother i had positive earned income in policy year t, $AgeEligible_i = \mathbf{1}_{\{DOB_i \geq 0\}}$ is an indicator equal to one if mother i's youngest child turned six during the first four months of the subsequent year $(t+1)$ and zero if her youngest child turns six during the last four months of policy year t, γ_t is a set of year fixed effects, and ε_{it} is an error term.

The reported effect in Column 1 is estimated using data from before the elimination of the YCTC work requirement (2019 and 2020); we exclude 2021 because the temporary

[14] Appendix Figure A.1 reports differences in labor force participation between the eligibility groups in each year. 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.

expansion of the federal CTC provided additional benefits for taxpayer claiming children under age 6 – the same eligibility cutoff we use for identification here.[15] The reported effect in Column 2 is estimated using data from the time period following the elimination of the YCTC's work requirement (2022 and 2023). Figure 4 plots mean labor supply by child birth date for mothers in our sample during each of these periods.

For both periods, we estimate a precise zero for the difference in labor force participation across age-eligibility groups. In the work requirement period, 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 -0.31 to 0.10 percentage points. In the period without a work requirement, we find that mothers of age-eligible children are 0.17 percentage points less likely to work (95% CI from -0.38 to 0.04).

Column 3 presents our difference-in-differences estimate of the effect of eliminating the YCTC work requirement on labor force participation. The estimate corresponds to δ 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 0.06 percentage points (6 basis points), with a 95% confidence interval ranging from -0.35 to 0.23 percentage points.[16] This result provides evidence against the hypothesis that eliminating the YCTC work requirement caused a substantial reduction in mothers' labor force participation.

To interpret this reduced form effect of the policy change, we can take advantage of the

[15] Our results are largely unchanged by the inclusion of 2021, consistent with the graphical evidence in Appendix Figure A.1.

[16] In principle, to the extent our MSE-optimal specification exhibits non-zero bias, this confidence interval should be re-centered as proposed by Calonico, Cattaneo and Titiunik (2014). In practice, as discussed below, we do not find evidence that our estimator is biased, so that any bias adjustment would be close to zero.

design of the YCTC's work requirement during the years prior to the policy change that removed it. In particular, eliminating the work requirement reduced the after-tax benefits of working relative to not working. Notably, because of the unique design of the YCTC's work requirement, this substitution effect was entirely concentrated on the extensive margin of labor force participation; the policy change did not affect the relative return from positive earning amounts. In addition, for the same reason, eliminating the work requirement did not generate an income effect among those who were working at the time of the reform.[17] Thus, for our main sample composed of working mothers, the estimated labor supply response isolates the extensive-margin substitution effect associated with the policy.

5.2 Validity of Identifying Assumptions

We next conduct a range of analyses to investigate the validity of our identifying assumptions.

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 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 YCTC, such as from compositional differences in the timing of births. Additionally, this assumption would 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. Conveniently, and perhaps not coincidentally given our bandwidth selection process, children in California are eligible to attend kindergarten if their fifth birthday falls before September 1 of the given year meaning that all children in our sample become eligible for kindergarten in the same year (the year prior to losing YCTC eligibility). We are unaware of any other policies affecting California mothers that rely on this age cutoff other than the 2021 federal CTC expansion described above.

[17]Among those who were not working, eliminating the work requirement generated an income effect; however, because this group was not otherwise working, the effect of the extra income on their labor supply must have been non-negative. We present evidence relating to non-working mothers below.

Table 2 investigates the validity of this assumption. Column 1 estimates Equation (2) for California mothers in the years *prior* to the introduction of the YCTC (2000-2018) – a period for which both age-eligibility groups were exposed to the same (lack of) policy. For this time period, we find no systematic difference between the groups. Columns 2 estimates the difference-in-differences specification (Equation 3) for mothers living in states *other* than California during the same years as our main analysis: 2019-20 and 2022-23.[18] This analysis provides no evidence that labor supply differed systematically between the age-eligible and age-ineligible groups across the two policy periods. Along similar lines, Figure 5 presents the distribution of these pseudo-treatment effects by state and year and presents graphical evidence that the distribution of estimated effects in states other than California is centered at zero.

Columns 3 and 4 present two additional placebo tests for California mothers during our sample period. The first compares mothers whose youngest child turned five (instead of six) during the last four months of the outcome year to mothers whose child turns five during the first four months of the subsequent year. In this case, children on both sides of the cutoff remain age-eligible for the YCTC and so any observed differences in maternal labor supply are not due to differences in eligibility for the credit. The second placebo takes advantage of the fact that 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. Estimates from the difference-in-differences analyses show no effects of the elimination of the YCTC work requirement on maternal labor supply for either of these two placebo groups.

Our second identifying assumption 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

[18]This analysis excludes the five states (Colorado, Maryland, New Jersey, Oregon, and Vermont) that implemented their own child tax credit with the same age cutoff as the YCTC during 2022 or 2023.

our setting is that our study period overlaps with the onset of the COVID-19 pandemic in 2020. To assess this concern, we repeat the main difference-in-differences specification from Table 1 excluding 2020 (Column 1) and excluding 2020-2022 (Column 2), and find that these analyses yield nearly identical results. 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 these years.

Finally, recall that the unique design of the YCTC work requirement meant that the elimination of that requirement did not generate an income effect for already-working mothers. As such, we can approximate mothers' labor supply when facing a YCTC without a work requirement based on their labor supply when facing no YCTC at all – in neither case does the incentive created by the policy affect the decision of whether to exit the labor force. Thus, under the first identifying assumption, the effect of eliminating the YCTC work requirement exactly corresponds to the (negative of the) coefficient reported in Column 1 of Table 1. Thus, even when our second identifying assumption does not hold, this alternative identification strategy yields a similar estimated effect for our main parameter of interest.

5.3 Additional Analyses

This subsection presents additional analyses exploring heterogeneity across taxpayers, effects on reported income, and potential differences in the effect of the policy over time.

5.3.1 Heterogeneity Analyses

Our main sample includes all California mothers whose youngest child turns six around the start of one of our policy years who worked in the prior year. Although we find no effects of the YCTC work requirement on labor force participation for this population as a whole, these overall results may mask important heterogeneity. Notably, the YCTC phases out for incomes over $30,000. While the YCTC changes the return to work for all households, the labor supply incentives may be particularly strong for households with incomes below this cutoff—roughly half of our sample. Columns 1 and 2 of Table 4 repeat our main difference-in-differences specification for mothers with prior-year income below \$30,000 and \$30,000 or more, respectively. We find no evidence that eliminating the YCTC work requirement affected labor participation of low-income mothers. For higher-income mothers, the estimated effect is marginally significant but very small in magnitude, with a similar 95% confidence interval as the overall sample.

Another potential source of heterogeneity relates to marital status. During the work requirement period, taxpayers needed to have earned at least \$1 of income *per return* to receive the YCTC; as a result married mothers filing joint returns with a working spouse may have experienced less of a change in labor supply incentives upon the removal of the work requirement than 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. Columns 3 and 4 present our estimates of the effect of the removal of the YCTC work requirement for single and married mothers, respectively, and find no differences in labor force participation.[19]

5.3.2 Effects on Reported Income

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.[20] 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

[19] We measure martial status based on prior-year tax filings; since this information is only available for individuals who filed a return, this analysis is limited to the 95% of our sample who filed a prior-year return.

[20] Garin, Jackson and Koustas (2022) find that some taxpayers increase reported self-employment income to maximize tax benefits like the CTC and EITC without actually increasing their labor incomes.

is required to report it but fails to do so.[21] 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 the YCTC.

Table 5 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 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.[22] For each outcome, the measure takes a value of zero if the 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). In fact, we estimate a small, though not statistically significant, increase in the share of taxpayers reporting positive earnings from the work requirement's elimination.

Finally, Panel D reports the estimated effect on tax filing. We find that eliminating the YCTC work requirement leads to 0.4 percentage point increase in the filing rate. Perhaps surprisingly, our results provide suggestive evidence that this filing effect appears to be only partly driven by an increase in filing among taxpayers without earned income, as indicated by the increase in the share of taxpayers reporting positive earnings. 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 (c.f., Anders and Rafkin, Forthcoming).

Overall, we interpret the results in this subsection to suggest that the estimates based on information returns are unlikely to be obscuring reductions in labor income.

[21]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.

[22]For purposes of this analysis, we measure self-employment income as the sum of income reported on each Schedule C that the taxpayer files.

5.3.3 Effects over Time

One potential explanation for why we do not find a labor supply effect from removing the YCTC work requirement is that the YCTC itself is a new policy. Especially in the year immediately following its enactment, taxpayers may not have been aware of the credit and thus failed to consider its work requirement when making their labor participation decisions.

To assess this possibility, we explore differences in the effect of the YCTC's work requirement over time. If awareness is an important part of the explanation for the small effects we estimate, we would expect to see larger effects over time as a growing number of taxpayers become aware of the policy and begin to incorporate it in their decision-making. Appendix Table A.2 estimates the effect of YCTC eligibility separately for each year in which the work requirement was in effect. To account for the possibility that taxpayers only learned about the policy in its third year (i.e., 2021), 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, we also include mothers in other states in our sample for this analysis as an additional control group.

Columns 1 through 3 present results of this analysis separately for each year of the YCTC work requirement period. In all three years, we estimate a very small effect of age-eligibility on maternal labor force participation. Additionally, we do not observe a gradient in the magnitude of these coefficients across years that would indicate that taxpayers learned about the work incentives associated with the YCTC over time (Column 4).

5.4 Alternative Samples: Medicaid Sample

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 the YCTC work requirement 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 a second data set focused on California participants in Medicaid. Specifically, this data set consists of California mothers whose children were enrolled in Medicaid at some point during the prior year, regardless of whether or not the mother was working. Appendix Table A.3 presents summary statistics for the Medicaid sample. Unlike our main sample of recently working mothers, only two thirds of the mothers in this sample work and receive a substantially lower average income ($16,411 versus $52,612).[23]

Table 6 replicates Table 1 for the Medicaid sample. Here, our point estimate is slightly positive, though not statistically different from zero (95% CI from -0.38 to 0.83 percentage points). Appendix Table A.4 reports results separately for mothers who worked in the prior year and those who did not and finds no effect on labor supply for either group. We interpret these results as evidence that eliminating the YCTC work requirement did not substantially discourage labor market entry.

5.5 Alternative Specifications

Our analysis in Section 4 suggested that our preferred specification (a simple comparison of means with a four-month bandwidth) yields a more precisely estimated treatment effect than the regression discontinuity specification employed in recent papers that exploit quasi-random variation in birth timing (e.g., Barr, Eggleston and Smith, 2022). As a robustness check, Appendix Table A.5 implements this alternative birth timing RD specification, which consists of a 4-week bandwidth surrounding the turn of the year, excluding an 8-day "donut" encompassing January 1st, and includes a linear trend in child's date of birth. Columns 1 and 2 present the results of this analysis for the YCTC work requirement period and for the period in which the YCTC work requirement was eliminated, respectively. Column 3 presents a difference-in-discontinuities estimate measuring the effect of the elimination of the work requirement on labor force participation.

We find that the elimination of the work requirement led to a reduction in maternal labor

[23]As with our main sample of working mothers, the mothers of age-eligible and age-ineligible in the Medicaid sample have similar characteristics but for a select number of demographics that differ by construction, such as age.

force participation of 0.06 percentage points (6 basis points). This estimate is near zero and nearly identical to the estimate in our main analysis in Table 1, but as expected, is measured with substantially less precision: the 95% confidence interval includes a reduction in labor force participation of 2.14 percentage points as well as an increase 2.03 percentage points.

As an additional robustness check, Appendix Table A.6 repeats the analyses in Table 1 using mothers in states other than California as a control group, i.e., a triple-difference specification. As with our main analysis, we find that the removal of the work requirement did not lead to a significant reduction in maternal labor force participation.

6 Labor Supply Elasticities

In this section, we translate our labor supply estimates from the elimination of the YCTC work requirement into labor force participation elasticities with respect to the return to work. Doing so allows us to compare our estimates to those obtained from other policy changes.

6.1 Implied Labor Supply Elasticities from the YCTC

We calculate elasticities that correspond to both our point estimate and to the lower-bound of our estimated 95% confidence interval.

The labor supply elasticity we consider is defined as:

$$\frac{\%\Delta \text{ labor supply}}{\%\Delta \text{ return to work}} \tag{4}$$

where the numerator is calculated as:

$$\frac{\Delta \text{ labor supply}}{\text{share of sample working}} \tag{5}$$

The terms in (5) follow directly from our main results. The numerator in (5) represents the estimated change in labor supply from eliminating the YCTC work requirement (Column

3 of Table 1). We measure the denominator of (5) based on the share of mothers with positive earned income during the outcome year, averaged over the years of our analysis.

The denominator in (4) is given by:

$$\frac{\Delta \text{ return to work}}{\text{ATT(working)-ATT(non-working)}} \tag{6}$$

where ATT denotes after-tax-and-transfer income. The denominator thus represents the economic return to working.

The numerator in (6) is equal to the reform-induced change in the financial benefits of working relative to not working. In our setting, this quantity is equal -$1,000 for all individuals with children whose ages qualify them for the YCTC. Because individuals are no longer required to work to claim the credit, elimination of the work requirement increases the return to not working by $1,000. Hence, it reduces the net return to work by $1,000.

The denominator in (6) represents the average financial benefit of working relative to not working, accounting for taxes and transfers. The first term represents the average after-tax and transfer income of individuals in our sample if they choose to work, and the second term represents the average after-tax and transfer income of individuals in our sample if they choose not to work.

We calculate the after-tax-and-transfer return to working for various income levels ranging from $1 to $100,000 and calculate taxes (including refundable tax credits) using NBER's TAXSIM. We model transfers as the value of food stamps benefits that a household is eligible for given their household income and household size, using the benefit formula for federal food stamps. For the after-tax-and-transfer return to not working, we assume that if households have no earned income, they receive the maximum food stamps benefits available given their household size.[24]

The results of this exercise are reported in Table 7. The first column presents results

[24]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.

for the full sample; the remaining columns present results for three sub-groups: married mothers; single mothers who claim the EITC; and single mothers who do not claim the EITC. We split single mothers in this way because prior estimates predicting the number of parents exiting the labor force were particularly focused on the labor supply responses of single mothers with income in the EITC-eligible range (e.g. Corinth et al. 2021; Bastian Forthcoming).[25]

The first two rows of Table 7 present the change in the return to work associated with the removal of the YCTC work requirement (a $1,000 reduction in the return to work) and the mean percent change in the return to work that this $1,000 represents for each sub-sample. For our full sample, households who worked in the prior year, the $1,000 decline in the return to work corresponds to a roughly 6 percent reduction in the return to work.

The remaining rows of Table 7 present calculations for the numerator of the labor supply elasticity, using both our main point estimates as well as the lower bound of our estimated 95% confidence interval. For the full sample, we observe a 0.06 percentage point decline in employment from the elimination of the YCTC work requirement, which represents a 0.07 percent change in employment for this population. This implies an elasticity of $0.0007/0.061 = 0.011$.

Using instead our lower bound labor supply estimate, which implies a 0.352 percentage point decline in labor force participation from the removal of the YCTC work requirement, we obtain a labor supply elasticity for the full sample of 0.063.

We repeat this exercise by marital status and EITC-claiming. We focus our discussion on the lower-bound employment responses, which imply larger labor supply elasticities than our main point estimates.We estimate a labor supply elasticity of 0.08 for married mothers. We find fairly comparable labor supply elasticities for single mothers, regardless of whether they claim the EITC: we estimate a labor supply elasticity of 0.137 for single mothers who

[25] Note that single mothers who do not claim the EITC includes both single mothers with zero earnings in the tax year, as well as single mothers with earnings above the EITC-eligible range. Since we limit our sample to individuals who were working in the prior year, the number of single mothers with zero earnings is quite small.

did not claim the EITC, and 0.132 for single mothers who did claim the EITC.

Overall, these labor supply elasticities are toward the lower end of the range of previous estimates (McClelland and Mok, 2012) and consistent with other work that has found that labor supply elasticities have declined over the last several decades (see Bastian (Forthcoming) for a review). These estimates are also consistent with the empirical evidence on the lack of a substantial labor supply responses to 2021 federal CTC expansion (Ananat et al., 2022; Enriquez, Jones and Tedeschi, 2023; Pac and Berger, 2024).

6.2 Application of Labor Supply Elasticities: Federal 2021 CTC reform

We next use these labor supply elasticities to update predictions on the parental labor supply responses to the federal reforms to the CTC as part of the American Rescue Plan Act (ARPA) in 2021. In addition to increasing the maximum per-child credit amount, the ARPA reform removed the earnings requirement to claim the federal CTC as well as the phase-in rate to claim the benefit. These reforms essentially turned the credit, at least temporarily, into a near-universal child benefit. One of the primary stated concerns with making this reform permanent was that it would lead to large numbers of working parents dropping out of the labor force.

Similar to the 2022 reform to the California YCTC, the ARPA reform reduced the economic return to work for a subset of households with earnings that would have placed them on the phase-in portion of the CTC benefit schedule prior to the 2021 reform. This is because, prior to the ARPA reform, the phase-in structure of the credit provided an incentive for households to increase their earnings to receive a larger CTC benefit. With the removal of the earnings requirement and the phase-in, this work incentive was eliminated.

The change in the return to work is more complicated to calculate for the federal CTC than for the California YCTC, since the federal CTC phases-in for earnings above $2,500. This means that the change in the return to work differs depending on household income.

We can calculate the change in the return to work (RTW) for a taxpayer with income I associated with the 2021 ARPA reform as follows:

$$\Delta\ RTW(I) = [CTC_{2021}(I) - CTC_{2021}(0)] - [CTC_{2020}(I) - CTC_{2020}(0)] \tag{7}$$

The first term represents the difference in CTC benefits for a household with earnings I and the CTC benefit available in 2021 for a household with no earnings. In 2021, this term simplifies to zero for the individuals in our population, because the CTC available for households with zero earnings was the same as that available for those with positive earnings.

The second term represents the return to work due to the CTC prior to the 2021 reform. Because households were ineligible for the CTC in 2020 if they had no earnings, the reduction in the return to work can simply be expressed as the value of the CTC benefit in 2020 for a given level of income.

Previous predictions on the number of parents who would stop working because of the reduction in the return to work associated with the ARPA reform ranged from around 350,000 parents (Goldin, Maag and Michelmore 2022; Bastian Forthcoming) to 1.5 million parents (Corinth et al., 2021). The main reason for the discrepancies in these estimates was due to different assumptions about how responsive parents' labor supply would be to changes in the economic return to work, particularly among low-income single mothers.

Using the labor supply elasticities calculated based on our 95% confidence interval lower bound estimates for labor supply responses to the removal of the earnings requirement for the California YCTC, we update predictions from prior work on the expected change in the number of working parents associated with the 2021 reforms to the federal CTC. For this exercise, we use the estimates on the number of working parents from Bastian (Forthcoming), but update the predicted labor supply reduction calculated in that paper for the employment reduction calculated using the elasticities from our analysis of the YCTC reform.

Results of this exercise are presented in Table 8. We estimate that the change in the return to work due to the ARPA reforms range from 5-10%, depending on the sub-sample.

Bastian (Forthcoming) estimates that this change in the return to work would lead 367,500 parents to stop working. In contrast, applying labor supply elasticities corresponding to the lower end of our estimated 95% confidence interval, we predict that the ARPA reform would cause only 155,318 parents to stop working.

7 Conclusion

We investigate the maternal labor supply effects of the elimination of a work requirement for claiming a tax credit targeted at the parents of young, low-income children. Given the unique design of the policy, we are able to focus on working mothers to isolate the policy's extensive-margin substitution effect—a key parameter for understanding the economic consequences of conditioning child tax benefits on work. Our results imply that eliminating the work requirement does not substantially reduce maternal labor force participation.

Our results inform estimates of the labor supply response to potential changes in the design of federal tax benefits for children, including the federal CTC. Based on the elasticities implied by our results, we estimate that an expansion of the federal CTC along the lines adopted in 2021 would cause fewer exits from the labor force than prior micro-simulations suggest. An important caveat is that the extrapolation of our estimates to federal tax policy may not be as direct as with state tax policies because of differing magnitudes of benefit generosities or policy salience (Chetty, 2012). That being said, prior research has found that most taxpayers eligible for a federal benefit also claim the corresponding state benefit (Iselin, Mackay and Unrath, 2023), providing suggestive evidence against systematic differences in salience for state versus federal tax policies. Moreover, the elasticities we estimate may be a better guide to understanding the incentive effects of the federal CTC than elasticities estimated from other sources of policy variation because of the similarity between the YCTC and the federal CTC – both are benefits for children administered through the tax code and both potentially depend on the taxpayer earning other sources of income.

Finally, our results provide new evidence for states considering adopting or reforming their own child tax benefits. Fifteen states currently offer a child tax credit, including six states that have adopted a tax benefit for parents of young children since 2022 alone. A central issue in designing such policies is whether to condition benefits on work; our results suggest that doing so is unlikely to be an effective means for increasing labor force participation among taxpayers in the state.

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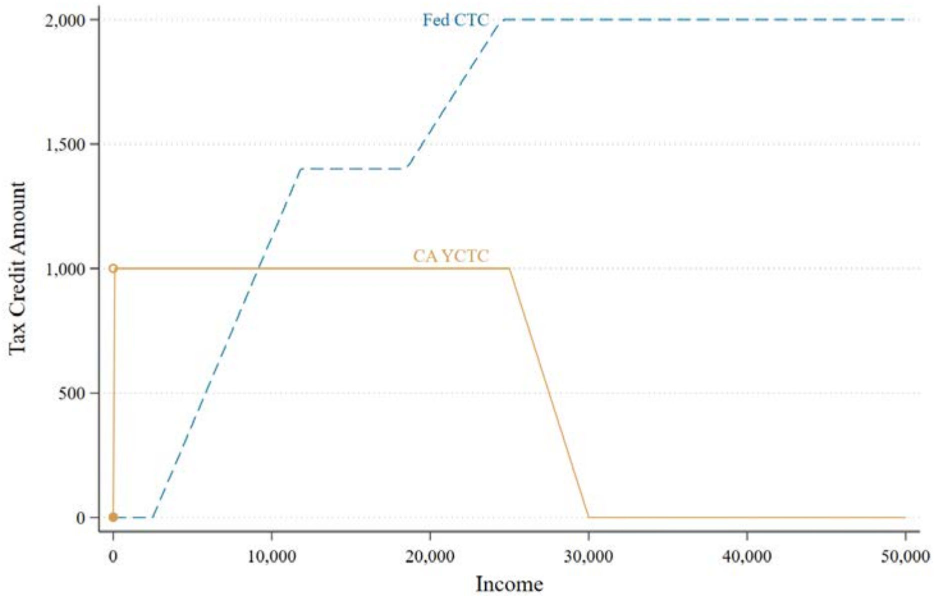
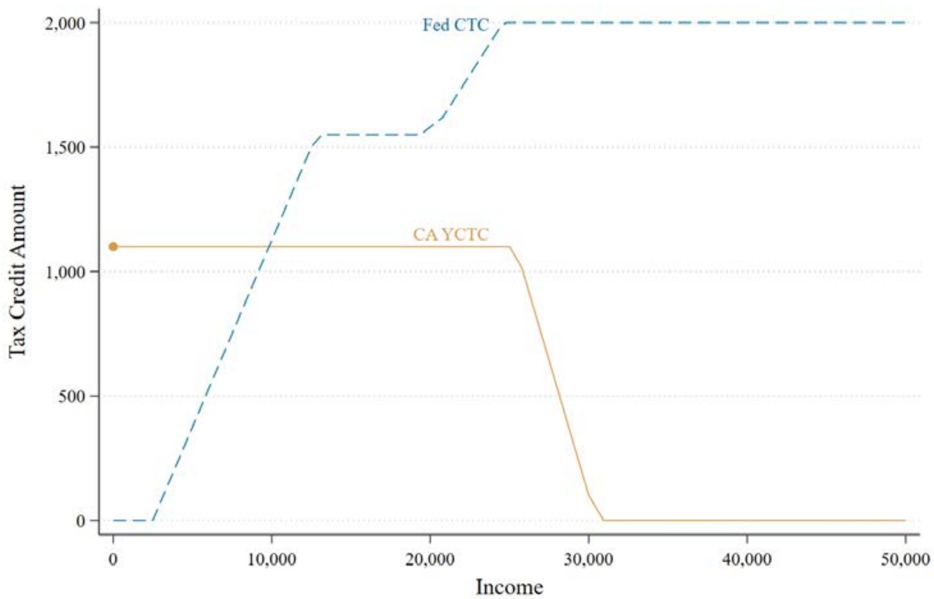
Figure 1: Child Tax Credit and Young Child Tax Credit Benefit Schedule

(a) 2019-2020

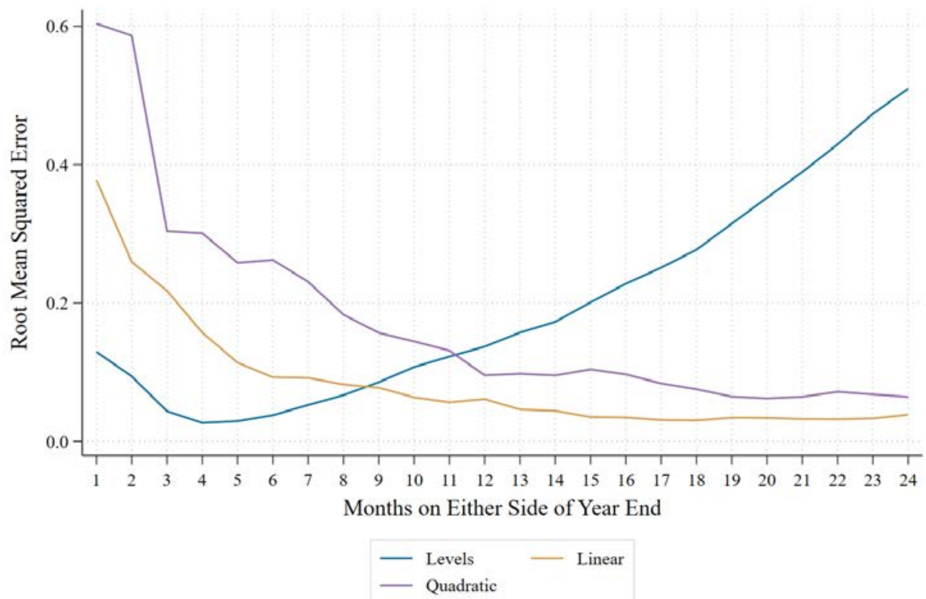


(b) 2022-2023



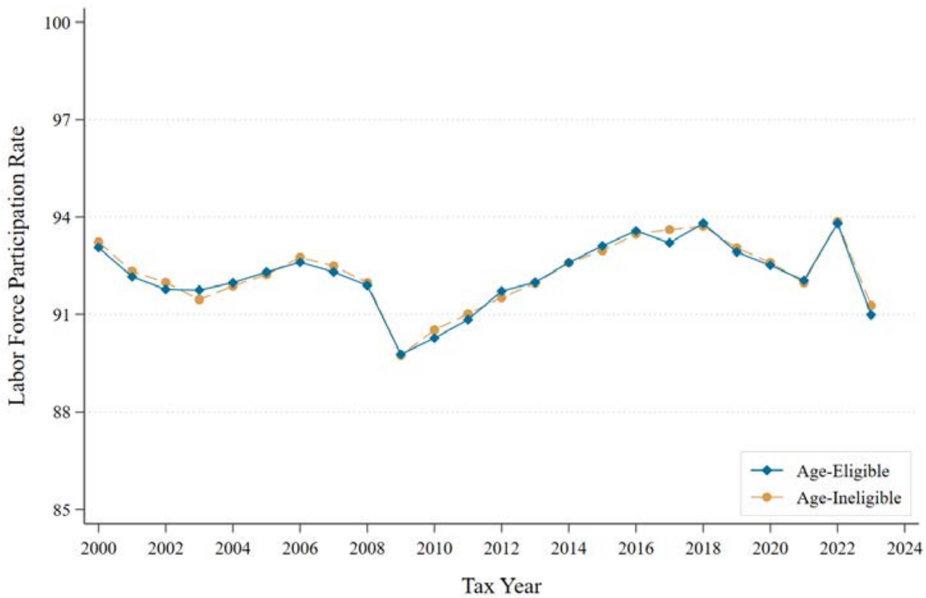
Notes: The figure shows the benefit amount for which taxpayers of varying income levels would qualify, for the California Young Child Tax Credit (yellow) and federal Child Tax Credit (blue). Panel A shows the benefit schedule that applied in tax years 2019 and 2020 (averaged across years). Panel B shows the benefit schedule that applied in tax years 2022 and 2023 (averaged across years). Each calculation assumes a taxpayer filing a joint return, claiming a single qualifying child under the age of six, and reporting only earned income.

Figure 2: Root Mean Squared Error by Specification and Bandwidth



Notes: The figure reports the results of the exercise described in Section 4.1, comparing 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 effect of age-eligibility for the YCTC following equation (1), for each year from 2005 through 2018. Each estimate is obtained from a sample composed of California mothers whose youngest child turns six within the specified number of months on either side of the end of the specified 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 three lines correspond to estimating equations that vary in the date of birth polynomial included in the implementation of equation (1). 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.

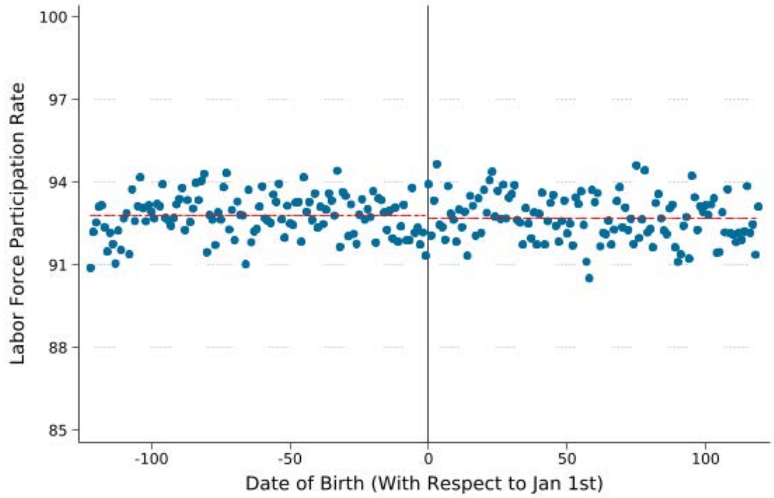
Figure 3: Maternal Labor Force Participation by YCTC Age-Eligibility and Year



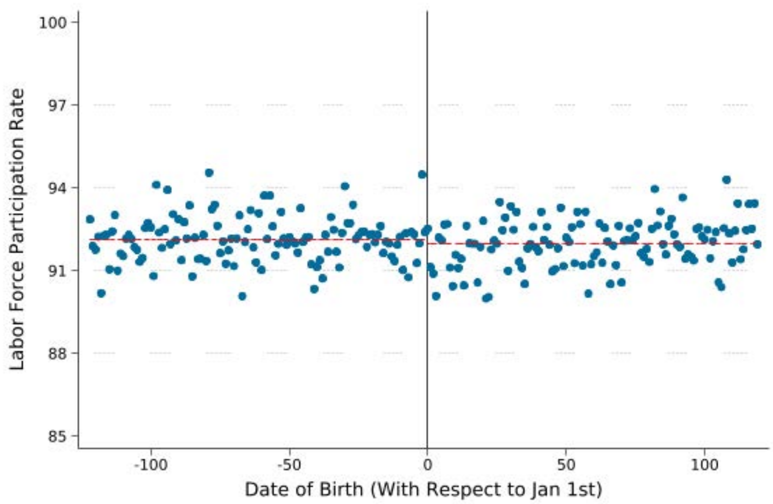
Notes: The figure reports labor force participation rates by year for our main sample. The sample consists of California mothers who had positive positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of the specified year. 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. Labor force participation is defined as having positive wage or self-employment income reported on third-party information returns.

Figure 4: YCTC Eligibility and Labor Supply: Binned Scatter Plot

(a) 2019-2020

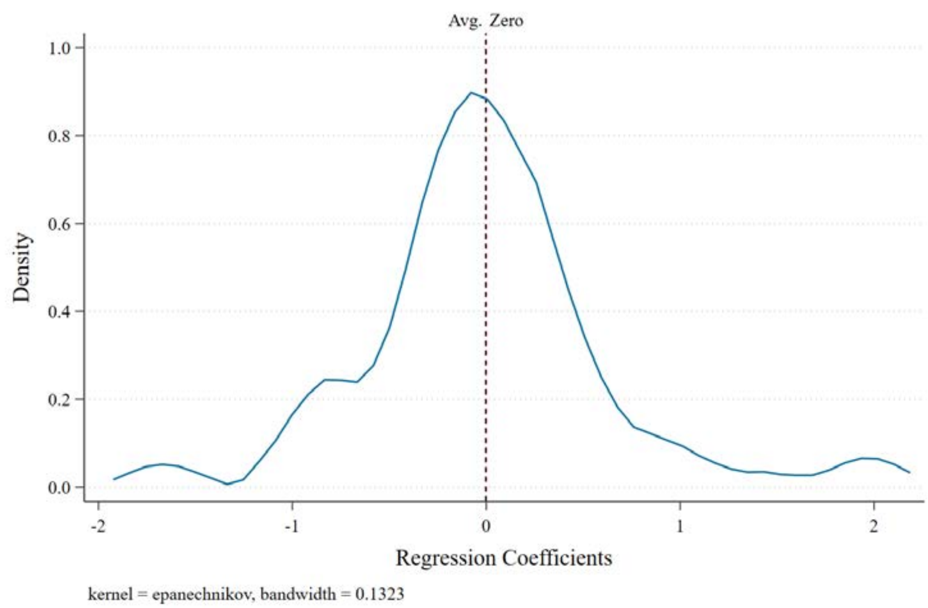


(b) 2022-2023



Notes: The figure reports mean labor force participation rates by child's date of birth for our main sample. The sample consists of California mothers 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.

Figure 5: Distribution of Pseudo-Treatment Effects from Other States



Notes: The figure reports the distribution of regression coefficients obtained from comparing labor force participation (placebo) effects for mothers in states other than California. For each state and year, a coefficient 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 bandwith of 0.1323. The dashed vertical lines denote the sample mean of the distribution (black) and zero (red), as labeled in the figure.

Table 1: YCTC Eligibility and Maternal Labor Supply

	(1)	(2)	(3)
	Work Req. (2019-2020)	No Work Req. (2022-2023)	Diff-in-Diff [(2) - (1)]
Coefficient	-0.107	-0.168	-0.061
	(0.103)	(0.107)	(0.148)
95% CI	[-0.309,0.096]	[-0.377,0.041]	[-0.352,0.230]
Control Mean	92.822	92.564	92.694
Observations	251,645	244,759	496,404

Notes: 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 and 2 compare labor force participation among mothers whose youngest child is below the age of six in the specified year to mothers whose youngest child is above the age of six in that year. Column 1 is estimated for years in which the YCTC contained a work requirement; Column 2 is estimated for years in which it did not. Column 3 corresponds to the difference in estimated effects between Columns 1 and 2. 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: Placebo Tests: YCTC Eligibility and Labor Supply

	(1)	(2)	(3)	(4)
	CA Pre-Period (2000-2018)	National Sample (Diff-in-Diff)	Age 4/5 Cohort (Diff-in-Diff)	Younger Sibling Cohort (Diff-in-Diff)
Coefficient	-0.036	0.006	0.055	0.102
	(0.037)	(0.052)	(0.148)	(0.231)
95% CI	[-0.109,0.036]	[-0.096,0.108]	[-0.234,0.344]	[-0.351,0.556]
Control Mean	92.217	93.658	92.227	89.686
Observations	2,097,327	3,485,515	533,333	277,169

Notes: This table compares labor force participation between various (placebo) groups of mothers who do not differ in their eligibility for the YCTC. 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 compares mothers of age-eligible versus age-ineligible children in California in the years before the implementation of the YCTC. Columns 2 to 4 present difference-in-differences estimates from equation 3 for three different placebo populations: mothers outside of California, excluding Colorado, Maryland, New Jersey, Oregon, and Vermont (Column 2); mothers of children whose youngest child's *fifth* birthday falls within the four-month window around the turn of the specified year (Column 3); and mothers of children near the age cutoff who also have a younger sibling (Column 4). The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table 3: Investigating Time Variation in Effect of YCTC Work Requirement

	(1)	(2)	(3)
	2022-23 vs 2019	2023 vs 2019	2020 vs 2019
Coefficient	-0.043	-0.161	0.037
	(0.179)	(0.217)	(0.207)
95% CI	[-0.394,0.309]	[-0.586,0.265]	[-0.368,0.442]
Control Mean	92.727	92.165	92.822
Observations	369,945	247,932	251,645

Notes: This table compares labor force participation of mothers with children born around the turn of the year for different policy 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-differences estimate from equation 3 excluding 2020. Column 2 reports these results excluding 2020 and 2022. Column 3 compares mothers of age-eligible versus age-ineligible children in California in across the two years within the YCTC work requirement period. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table 4: Heterogeneity Analyses: YCTC Eligibility and Labor Supply

	(1)	(2)	(3)	(4)
	Low Income	High Income	Single	Married
Coefficient	-0.062	-0.162*	-0.132	-0.057
	(0.303)	(0.095)	(0.203)	(0.204)
95% CI	[-0.657,0.532]	[-0.348,0.025]	[-0.530,0.267]	[-0.456,0.342]
Control Mean	85.589	98.383	94.275	92.746
Observations	220,331	276,073	212,493	260,481

Notes: This table reports difference-in-differences estimates from equation 3 for different subgroups. 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 and Column 2 limit to mothers with prior year earnings below or above $30,000, respectively. Columns 3 and 4 limit the analysis to single and married mothers, respectively. include individuals who file taxes as married filing jointly or married filing separately, with or without a spousal exemption. includes mothers with any other tax filing status. These classifications are based on third-party information returns (Columns 1 and 2) and tax filing status (Columns 3 and 4) from the previous year; as a result, Columns 3 and 4 include only the 95% of mothers who filed a prior-year return. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: YCTC and Reporting Outcome

	(1)	(2)	(3)
	Work Req. (2019-2020)	No Work Req. (2022)	Diff-in-Diff [(2) - (1)]
Panel A: Reported Wages			
Coefficient	0.054	0.366**	0.312
	(0.108)	(0.185)	(0.214)
95% CI	[-0.158,0.266]	[0.004,0.728]	[-0.107,0.732]
Control Mean	91.967	88.025	90.669
Observations	251,645	122,013	373,658
Panel B: Reported Schedule C Income			
Coefficient	0.086	-0.027	-0.113
	(0.139)	(0.201)	(0.244)
95% CI	[-0.185,0.358]	[-0.422,0.368]	[-0.592,0.366]
Control Mean	13.995	14.462	14.148
Observations	251,645	122,013	373,658
Panel C: Reported Earned Income			
Coefficient	-0.032	0.255	0.287
	(0.090)	(0.163)	(0.187)
95% CI	[-0.210,0.145]	[-0.065,0.576]	[-0.079,0.654]
Control Mean	94.579	90.925	93.376
Observations	251,645	122,013	373,658
Panel D: Filed			
Coefficient	-0.037	0.359**	0.396**
	(0.074)	(0.160)	(0.176)
95% CI	[-0.183,0.109]	[0.046,0.672]	[0.051,0.741]
Control Mean	96.404	91.340	94.736
Observations	251,645	122,013	373,658

Notes: 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), an indicator for reporting positive earned income, wage or self-employment (Panel C), an indicator for filing a tax return (Panel D). Outcomes in Panels A-C take on a value of zero if the individual did not file a tax return. Units are percentage points (0-100). Columns 1 and 2 compare labor force participation among mothers whose youngest child is below the age of six in the specified year to mothers whose youngest child is above the age of six in that year. Column 1 is estimated for years in which the YCTC contained a work requirement; Column 2 is estimated for years in which it did not. Column 3 corresponds to the difference in estimated effects between Columns 1 and 2. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Medicaid Sample: YCTC Eligibility and Labor Force Participation

	(1)	(2)	(3)
	Work Req. (2019-2020)	No Work Req. (2022-2023)	Diff-in-Diff [(2) - (1)]
Coefficient	0.052	0.278	0.226
	(0.217)	(0.219)	(0.308)
95% CI	[-0.373,0.476]	[-0.152,0.707]	[-0.378,0.830]
Control Mean	65.888	67.582	66.722
Observations	191,766	182,273	374,039

Notes: This table repeats the analyses in Table 1 for the Medicaid sample. This sample includes mothers of children enrolled in Medicaid in the state of California at any point during the prior 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). The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table 7: Elasticity of Labor Force Participation with Respect to Return to Work

	(1)	(2)	(3)	(4)
	Full Sample	Married	EITC & Single	No EITC & Single
Mean Return to Work				
Change in Return to Work	-1,000	-1,000	-1,000	-1,000
Percent Change in Return to Work	-6.021	-6.425	-6.442	-3.170
Point Estimate				
Employment Effect	-0.061	-0.057	-0.286	0.209
Percent Change in Employment	-0.066	-0.062	-0.305	0.217
Elasticity	0.011	0.010	0.047	-0.069
95% Confidence Interval Lower Bounds				
Employment Effect	-0.352	-0.456	-0.795	-0.418
Percent Change in Employment	-0.377	-0.492	-0.847	-0.435
Elasticity	0.063	0.077	0.132	0.137

Notes: This table reports estimated labor supply elasticities based on the point estimates and lower bound of the estimated 95% confidence intervals from Table 1, as described in Section 6 of the text.

Table 8: Modeled Labor Force Exits from the 2021 CTC Expansion

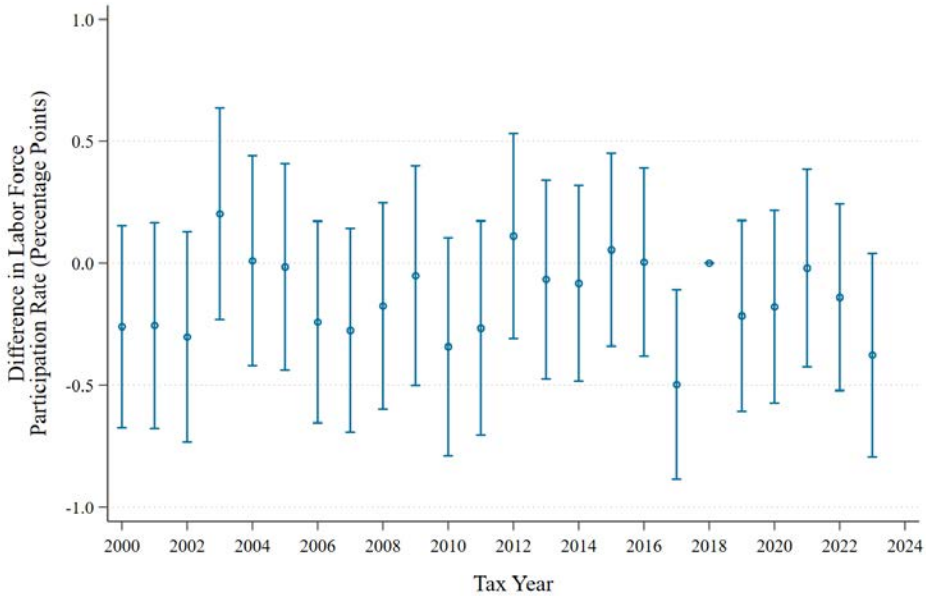
	Bastian (2023) Estimates				Our Estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
	Population	Change in RTW	Elasticity	Labor Force Exit	Elasticity	Labor Force Exit
Married Mothers	5,654,115	0.04	0.20	48,524	0.08	18,584
Single						
Single Mothers & Did Not Claim EITC	1,784,789	0.05	0.20	17,860	0.14	12,260
Single Mothers & Claimed EITC	6,760,784	0.10	0.40	263,213	0.13	86,552
Married Fathers	8,469,542	0.07	0.05	27,610	.	27,610
Single Fathers & Others	2,873,704	0.07	0.05	10,312	.	10,312
Total	25,542,934			367,518		155,318

Notes: This table reports predicted labor market exits among parents associated with the 2021 reforms to the federal CTC. Columns (1)-(4) come from Bastian (Forthcoming); elasticities in column (5) come from the lower end of our estimated 95% confidence interval, reported in Table 7. For groups for which we do not calculate an elasticity, we apply the corresponding elasticity from Bastian (Forthcoming). refers to the return-to-work due to the CTC, as described in the text of Section 6. The labor force reduction reported in column (6) is obtained by multiplying the population in (1) by the change in return to work in (2) by the elasticities we calculate from our analysis in (5).

A Online Appendix to Child Allowances and Labor Supply: Evidence from the California Young Child Tax Credit

Jacob Goldin Tatiana Homonoff Neel Lal
Ithai Lurie Katherine Michelmore

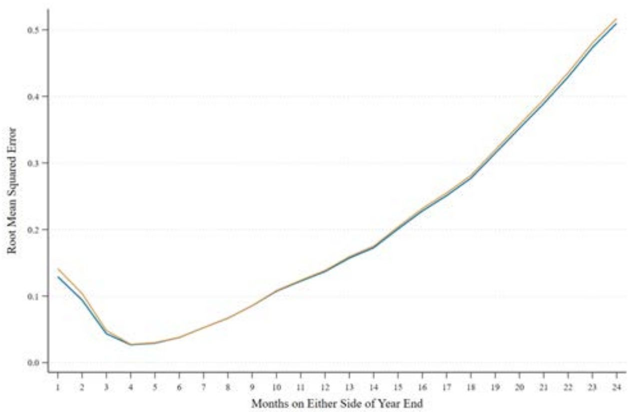
Figure A.1: Difference in Maternal Labor Force Participation by YCTC Age-Eligibility and Year



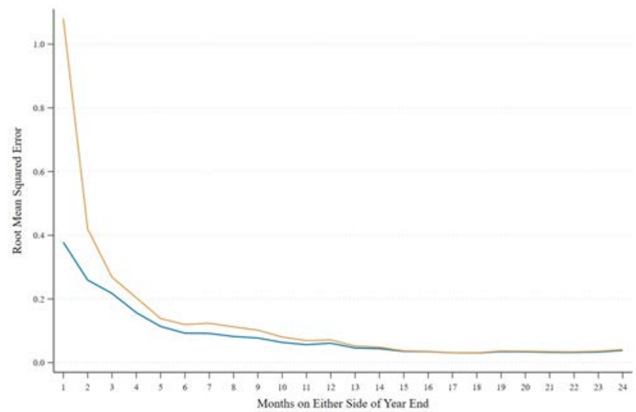
Notes: The figure reports yearly differences in labor force participation rates between age-eligible and age-ineligible mothers in our main sample. The sample consists of California mothers who had positive wage or self-employment income reported on third-party information returns during the previous tax year and whose youngest child's sixth birthday falls within the four-month window around the turn of the specified year. The age-ineligible group consists of mothers whose youngest child turns six during the last four months of a year; the age-eligible group consists of mothers whose youngest child turns six during the first four months of the subsequent year. Labor force participation is defined as having positive wage or self-employment income reported on third-party information returns. Coefficient estimates are reported in percentage points (0-100). Bars represent the estimated 95% confidence intervals.

Figure A.2: RMSE by Donut Specification and Bandwidth

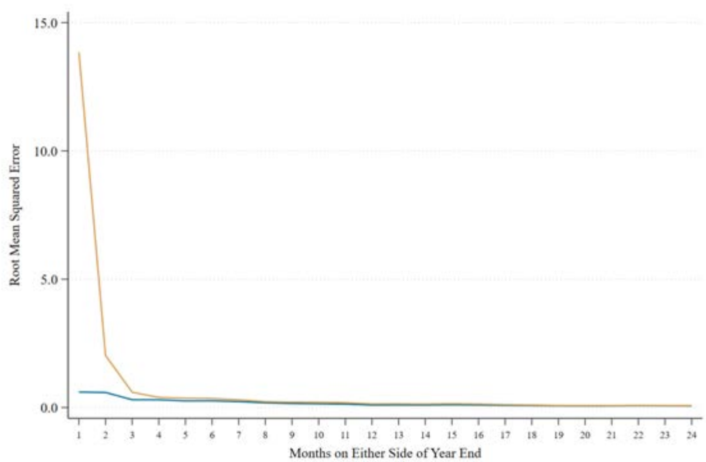
(a) Levels



(b) Linear



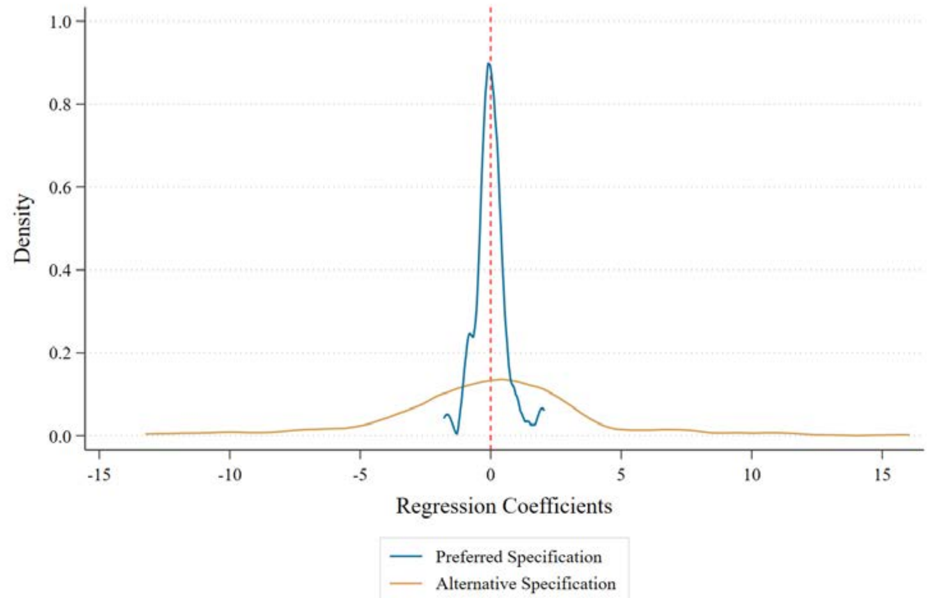
(c) Quadratic



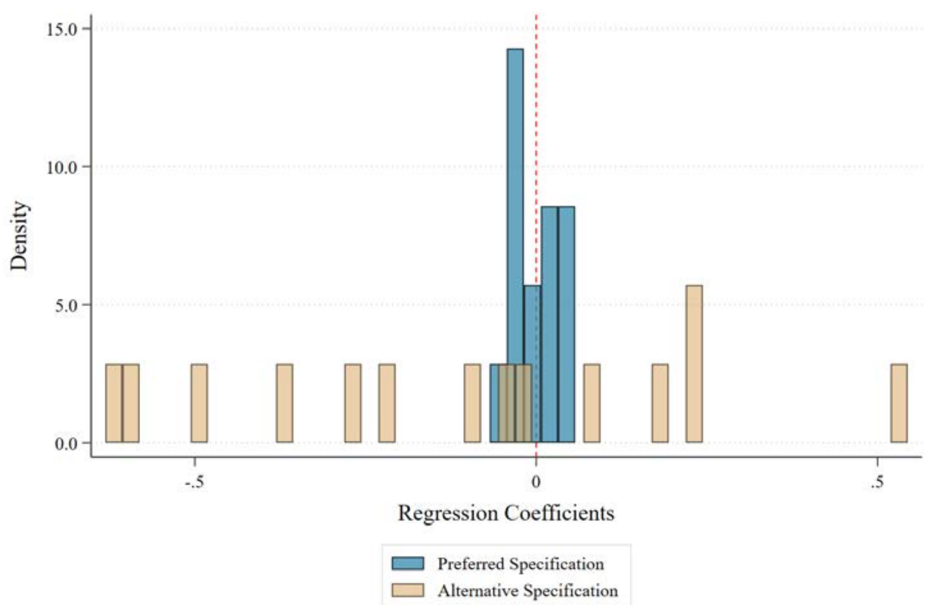
Notes: The figure repeats the analyses in Figure 2 (blue) as well as a corresponding analysis that excludes an 8-day donut around the turn of the year (yellow). The three panels correspond to estimating equations that vary in the date of birth polynomial included in the implementation of equation (1). Panel A is a simple comparison of means of the age-eligible and age-ineligible groups of mothers; Panel B includes a linear trend in date of birth; Panel C includes a quadratic polynomial in date of birth.

Figure A.3: Distribution of Pseudo-Treatment Effects: Preferred versus Alternative Specification

(a) Other States, 2019-20 and 2022-23

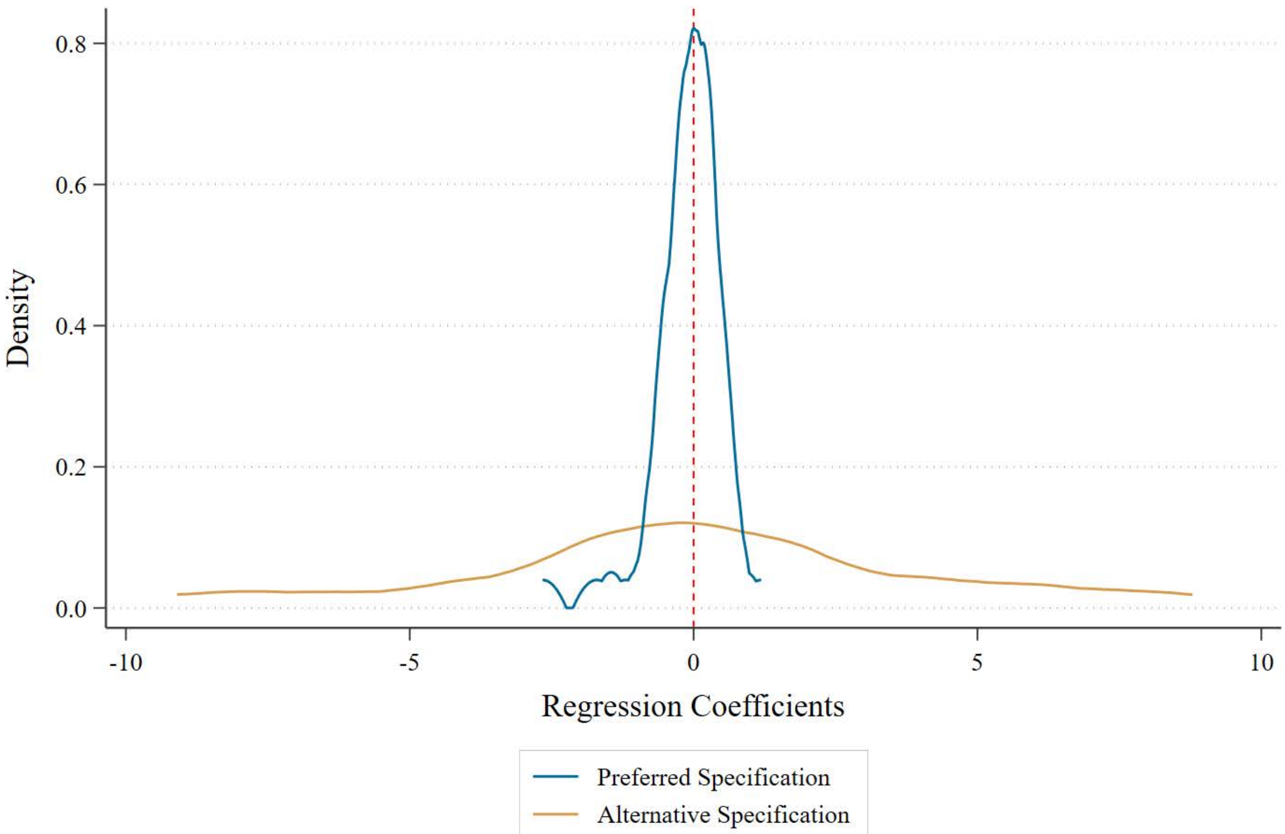


(b) California, 2000-2018



Notes: The figure compares the distributions of (placebo) regression coefficients obtained from our preferred specification, a four-month bandwidth with polynomial order zero (blue) and those obtained from a common alternative regression discontinuity specification with 4-week bandwidth (excluding an 8-day donut around the turn of the year) with a first order polynomial (yellow). Panel A reports the distribution of estimates obtained from other states for years 2019, 2020, 2021, and 2022. Both specifications are separately estimated for each state other than California, excluding states that adopted a child tax policy with an overlapping age cutoff during this sample period (Colorado, Maryland, New Jersey, Oregon, and Vermont). Panel B reports the distribution of estimates obtained from California for years 2000 through 2018.

Figure A.4: Distribution of Pseudo-Treatment Effects: Preferred versus Alternative Differences-in-Discontinuity Design



Notes: The figure compares the distributions of (placebo) regression coefficients obtained from difference-in-differences specification comparing 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 blue line estimates the first difference using our preferred specification, a four-month bandwidth with polynomial order zero. The yellow line estimates the first difference using a common alternative regression discontinuity specification with 4-week bandwidth (excluding an 8-day donut around the turn of the year) with a first order polynomial. Both specifications are separately estimated for each state other than California, excluding states that adopted a child tax policy with an overlapping age cutoff during this sample period (Colorado, Maryland, New Jersey, Oregon, and Vermont).

Table A.1: Summary Statistics by YCTC Age-Eligibility

	(1) All	(2) Age-Eligible (Jan-Apr)	(3) Age-Ineligible (Sep-Dec)	(4) p-value
Individual-Level				
Age	35.201	35.057	35.337	0.000
Any Income	1.000	1.000	1.000	.
Total Income	52,728	53,103	52,374	0.000
Self-Employed	0.118	0.118	0.118	0.575
Filed a Tax Return	0.953	0.953	0.953	0.523
Return-Level, if Filed				
Married	0.551	0.555	0.547	0.000
Num. Claimed Children	1.815	1.814	1.816	0.568
AGI	113,069	114,312	111,893	0.000
Claimed Federal EITC	0.372	0.368	0.375	0.000
Claimed Federal CTC	0.899	0.898	0.900	0.128
Observations	496,404	241,288	255,116	

Notes: This table reports demographic and prior-year tax return characteristics for our main sample of recently working California mothers whose youngest child's sixth birthday falls within the four-month window around the end of the given year. Data includes cohorts for the following policy 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 within the first four months of the subsequent year (age-eligible); Column 3 reports statistics for mothers of children whose birthday falls within the last four months of the year (age-ineligible); Column 4 reports the p-value for the test of equality between Columns 2 and 3. employment characteristics are based on third-party information returns; characteristics are based on tax return data and are only presented for individuals who filed a prior-year tax return.

Table A.2: YCTC Eligibility and Labor Supply by Year During the Work Requirement

	(1)	(2)	(3)	(4)
	2019	2020	2021	2019-2021
Age-Eligibile x CA	-0.055	-0.097	0.125	-0.099
	(0.153)	(0.157)	(0.163)	(0.141)
Age-Eligibile x CA x Year				0.090
				(0.112)
Control Mean	94.159	93.688	93.248	93.696
Observations	1,073,539	1,094,902	1,082,668	3,251,109

Notes: 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 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 control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table A.3: Summary Statistics by YCTC Age-Eligibility

	(1)	(2)	(3)	(4)
	All	Age-Eligible (Jan-Apr)	Age-Ineligible (Sep-Dec)	p-value
Individual-Level				
Age	33.604	33.406	33.788	0.000
Any Income	0.669	0.670	0.669	0.647
Total Income	16,411	16,307	16,506	0.002
Self-Employed	0.082	0.082	0.083	0.592
Filed a Tax Return	0.861	0.861	0.862	0.266
Return-Level, if Filed				
Married	0.441	0.441	0.442	0.398
Num. Claimed Children	1.930	1.929	1.930	0.786
AGI	38,489	38,295	38,668	0.000
Claimed Federal EITC	0.589	0.589	0.589	0.614
Claimed Federal CTC	0.915	0.915	0.916	0.483
Observations	374,039	179,868	194,171	

Notes: This table reports demographic and prior-year tax return characteristics for our Medicaid sample of California mothers whose youngest child's sixth birthday falls within the four-month window around the end of the given year. Data includes cohorts for the following policy 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 within the first four months of the subsequent year (age-eligible); Column 3 reports statistics for mothers of children whose birthday falls within the last four months of the year (age-ineligible); Column 4 reports the p-value for the test of equality between Columns 2 and 3. employment characteristics are based on third-party information returns; characteristics are based on tax return data and are only presented for individuals who filed a prior-year tax return.

Table A.4: Heterogeneity Analyses: Medicaid Sample

	(1)	(2)
	Worked in Prior Year	Did Not Work in Prior Year
Coefficient	-0.064	0.238
	(0.240)	(0.455)
95% CI	[-0.535,0.406]	[-0.653,1.130]
Control Mean	90.107	19.470
Observations	250,335	123,704

Notes: This table reports difference-in-differences estimates from equation 3 for the Medicaid sample, separately for mothers who had positive earnings income in the prior year (Column 1) and for those who did not (Column 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). This sample includes mothers of children enrolled in Medicaid in the state of California at any point during the prior year. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table A.5: Regression Discontinuity Design: YCTC Eligibility and Labor Supply

	(1)	(2)	(3)
	Work Req. (2019-2020)	No Work Req. (2022-2023)	Diff-in-Discontinuity [(2) - (1)]
Coefficient	-0.303	-0.361	-0.058
	(0.738)	(0.766)	(1.064)
95% CI	[-1.750,1.144]	[-1.862,1.140]	[-2.143,2.027]
Control Mean	92.808	92.674	92.742
Observations	47,546	46,165	93,711

Notes: This table presents regression discontinuity estimates for the labor market impacts of eligibility for the YCTC among recently working California mothers. The sample consists of children born in last four weeks of the year and the first four weeks of the subsequent year, excluding those whose dates of birth fall within an 8-day period surrounding January 1st. Regressions include a linear time trend and an interaction between a linear time trend and an indicator for whether the mother is age-eligible for the YCTC during the specified year. Columns 1 and 2 compare mothers of age-eligible versus age-ineligible children in the years before and after the elimination of the YCTC work requirement, respectively. Column 3 presents the differences of these two estimates, i.e., the difference-in-discontinuities estimate. 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 control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.

Table A.6: YCTC Eligibility and Labor Supply: California vs. Other States

	(1)	(2)	(3)
	Work Req. DiD (2019-2020)	No Work Req. DiD (2022-2023)	Triple Difference [(2) - (1)]
Coefficient	-0.067	-0.161	-0.094
	(0.110)	(0.117)	(0.160)
95% CI	[-0.283,0.149]	[-0.389,0.067]	[-0.408,0.220]
Control Mean	93.91	93.11	93.51
Observations	2,002,380	1,972,164	3,974,544

Notes: 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 and 2 compare labor force participation of California mothers of age-eligible versus age-ineligible children to mothers of same-aged children in different states. Column 1 is estimated for years in which the YCTC contained a work requirement; Column 2 is estimated for years in which it did not. Column 3 presents the triple-difference estimator, i.e., the interaction of indicators for being in the no work requirement period, having an age-eligible child, and living in California residency, controlling for the corresponding main effects, two-way interactions, and year fixed effects. 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. Analyses exclude mothers in Colorado, Maryland, New Jersey, Oregon, and Vermont. The control mean corresponds to mothers of age-ineligible children. Parentheses report heteroskedasticity-robust standard errors.