

Employment Responses to Earned Income Tax Benefits: Evidence from California's Young Child Tax Credit

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Abstract

Between 2019 and 2021, California provided a flat \$1,000 benefit to taxpayers with a young child and low positive earnings. Leveraging an age-based eligibility discontinuity and extensive administrative tax data, I identify the policy's impact on parents' employment and earnings. Despite a significant change in the return to work, I find limited to no evidence of a pro-employment response among treated households or single mothers. Employment effects in the subsequent year, when treated families received their larger tax refunds, are also near zero. The results suggest a lower employment elasticity and smaller income effect for parents of young children than prior estimates.

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1 Introduction

Income tax policy towards low-income families in the United States is meant to both redistribute income and incentivize employment (Hoynes and Rothstein, 2017). Both the Earned Income Tax Credit and the Child Tax Credit condition eligibility on having some earned income and phase in benefits as earnings rise. An extensive literature considers whether and to what extent these features affect eligible adults' work decisions. Most evidence suggests they draw some non-workers into employment with little effect on hours choices thereafter, but disagreement about magnitudes remains (Nichols and Rothstein, 2015; Corinth et al., 2021; Bastian, 2024a). Credible evidence about the size of these responses is critical for policymakers to appropriately weigh these programs' competing aims (Besley and Coate, 1992; Saez, 2002).

In this paper, I study a unique state tax policy that substantially altered labor supply incentives for a subset of families with young children. Between 2019 and 2021, California's Young Child Tax Credit (YCTC) provided \$1,000 to state tax filers with low positive earnings and a child younger than six years old. Unlike the EITC and current CTC, the YCTC benefit was not phased-in. Instead, its full value was available to workers with between \$1 and \$25,000 of earned income. Relative to existing policy, this structure abruptly increased the pro-work incentive for eligible non-workers. Treated parents faced an unprecedented negative marginal tax rate on their first dollar of earnings and a significant reduction in their expected average tax rate.¹

I exploit the policy's age-based eligibility cutoff to identify its causal impact on parents' employment. Specifically, I use a regression discontinuity design to compare outcomes between California parents who were just ineligible for the policy, because their youngest child turned six near the end of the relevant tax year, and those who were eligible, because their youngest child turned six at the start of the following tax year. Aside from their eligibility for this program, these families should otherwise be similar. Eligibility for no other benefit program depends on having a five- versus six-year-old, and a family's eligibility for this tax credit would have been determined five years before the policy's introduction, making it especially difficult for parents to manipulate treatment. To account for any potential differences between families located on either side of the treatment cutoff, I also estimate two difference-in-regression discontinuity designs (D-RD), using non-California families and pre-reform California families as additional control groups.

To identify relevant families and their outcomes in tax years 2019 to 2022, I use the near universe of birth records registered with the Social Security Administration (SSA) linked to IRS tax records. I associate all children in the SSA data to adults who claimed them on a 1040 and to their birth parents using the Census Household Composition Key (CHCK). I identify whether children were likely residing in California in the relevant tax years using the addresses on filed 1040s and birth locations in the Numident. I match each parent to their W-2 earnings and income fields from the

¹The benefit was phased out between \$25,000 and \$30,000 of earned income. For those who would have worked absent the policy, the program simultaneously disincentivized work through an income effect and a substitution effect in the narrow phase-out region. The program operated under the same parameters between tax years 2019 and 2021. In 2022, the earnings requirement was dropped, meaning parents with no earned income could also claim the credit. The credit amounts and maximum eligible income were also adjusted for inflation.

1040 returns for all years in my study period. My key outcomes are indicators of employment – positive W-2 wages, positive wage or salary income claimed on a 1040, and/or the presence of a Schedule C or Schedule SE attached to the 1040. I also test for intensive margin responses using W-2 wages and income reported on the 1040.

Despite the significant change in the incentive to work, I find little evidence that the policy raised employment among eligible families. Within the California sample, the share of eligible children whose parents had positive W-2 wages in the effective tax year was not statistically significantly higher than parents of ineligible children. Using a broader employment measure that includes self-employment, differences between eligible and ineligible parents remain insignificant. The D-RD models similarly yield null treatment effects. For most specifications, I can rule out employment responses greater than one percent.

I consider heterogeneous responses among female versus male and single versus married parents. Employment effects among single female parents, who comprise roughly 40 percent of the primary sample and are the focus of much prior work on this topic, are indistinguishable from zero. Employment responses among single fathers appear higher, but effects are still not statistically significant. Estimates for birth parents, which avoids a bias towards employment embedded in my definition of each child's relevant parents, are also near zero. Effects among married parents appear higher, but are less precise due to smaller sample sizes.

Given the policy's novelty, treated families may have simply been unaware of the program or that they qualified. This would imply that the boost to eligible families' subsequent tax refunds was unanticipated. The effect of such a windfall on work decisions is ambiguous. Standard labor supply models predict lower employment through an income effect, while more recent evidence suggests such assistance could allow recipients to better afford complements to work and thereby facilitate employment (Banerjee et al., 2020).² Confusion about effective tax rates might also encourage parents with higher after-tax income to increase their future labor supply (Feldman, Katuščák and Kawano, 2016). If anticipatory responses were limited, I can use the same identification strategy to test these alternative theories. I find no evidence that YCTC eligibility decreased employment in the subsequent tax year. For both households and single mothers, treatment effects using either employment measure are not statistically different than zero. One exception is single fathers, for whom I observe consistently *positive* – and, in some cases, statistically significantly higher – employment response in the next tax year.

I conclude by transforming my results into estimated employment elasticities. I show how the policy reform changed the return to work for eligible households across the earnings distribution and then, to calculate an elasticity, identify reasonable estimates of the return to work for adults on the margin of employment. I divide my estimates of the percent change in employment by the policy's effect on household's post-tax income. Weighted by the number of affected workers, plus the employment and tax policy changes in each policy year, the best estimates of the employment elasticity are 0.12 for single mothers and 0.26 for households. Both estimates are on the lower end

²Concretely, since my setting largely overlaps with the Covid-19 pandemic, parents might have used the extra income to delay a return to work or, instead, invested in childcare in order to remain working or return earlier.

of the range summarized by McClelland and Mok (2012) and Corinth and Winship (2024).

A limitation of this study is that I lack access to California state tax returns, meaning I do not observe YCTC claiming and I cannot show a first stage effect on YCTC receipt. Still, my sample likely includes nearly all YCTC claimers and take-up of the program appears reasonably high. I simulate eligibility among federal tax filers in California, count the number of likely claimants as well as their total claimed benefits, and compare these counts against official claims reported by the California Franchise Tax Board (FTB) in each tax year.³ My estimates of claimants and benefits disbursed are, on average, around five percent higher than FTB's. These differences are consistent with the share of low-income taxpayers who file a federal return but fail to claim state tax credits (Iselin, Mackay and Unrath, 2023). Given the value of the EITC and CTC, low-income parents with children who claim the YCTC are unlikely to file a state return but not a federal one.

This paper contributes to a long literature studying labor supply elasticity to taxation – in particular, studies of intertemporal substitution and low-income workers' responses to non-linear tax policies.⁴ Whereas most of the micro literature studies policy shocks that change workers' permanent expectations of after-tax wages, my setting involves only a temporary change in tax rates; neither treated nor control families are eligible for this credit the following year and should face similar tax rates in the future. My results reflect limited intertemporal substitution in line with more recent evidence (Martinez, Saez and Siegenthaler, 2021). With regard to responses to non-linear tax policy, most research finds that the participation elasticity is positive and varies by sex, education, and wage levels (Chetty, 2012; Hotz, 2003; Bastian, 2024a). Estimates of intensive margin adjustments tend to be closer to zero.⁵ However, limited variation in tax policy makes definitive conclusions about these elasticities challenging, and considerable uncertainty and disagreement about the size of these responses remains (Nichols and Rothstein, 2015; Kleven, 2022). My findings imply small extensive margin elasticities in line with or lower than prior estimates, presumably due to limited awareness of exact tax policy parameters (Phillips, 2001; Chetty and Saez, 2013; Bhargava and Manoli, 2015; Feldman, Katuščák and Kawano, 2016) and other important adjustment frictions (Chetty, 2012; Gelber, Jones and Sacks, 2020). I also separately report effects for mothers' wages and fathers' wages and within households with one or two adults. I find that lower-income single mothers exhibited limited responses to this policy, similar to the household-level estimates and consistent with evidence of declining elasticities within this population (McClelland and Mok, 2012; Heim, 2007).

My approach offers several advantages over common research designs used to estimate responses to non-linear tax policy. First, a significant share of this literature studies responses to EITC ex-

³FTB's reports related to CalEITC and YCTC claiming can be found here: <https://www.ftb.ca.gov/about-ftb/data-reports-plans/>.

⁴For a review of the literature related to the Frisch elasticity, refer to: Reichling and Whalen (2012). For reviews of labor supply elasticity with respect to non-linear income policy, refer to: Blundell and MaCurdy (1999); Hotz (2003); McClelland and Mok (2012); Saez, Slemrod and Giertz (2012).

⁵Extensive: Eissa and Liebman (1996); Meyer and Rosenbaum (2001); Grogger (2003); Hotz and Scholz (2006); Eissa and Hoynes (2006); Gelber and Mitchell (2011); Hoynes and Patel (2018); Bastian and Jones (2021); Jensen and Blundell (2024). Intensive: Meyer (2002); Chetty and Saez (2013); Eissa and Hoynes (2006); Saez (2010); Nichols and Rothstein (2015).

pensions in the 1990s, and the extent to which employment trends for low-income mothers in that era extrapolate to other periods remains unclear (Kleven, 2022). I study a more recent reform. Second, these papers leverage differential tax treatment of families based on filing status and number of children and assume these groups' outcomes would have evolved similarly if not for those reforms. My approach involves a comparison of more similarly situated families. Third, older studies tend to rely on repeated cross-sectional household surveys, but selection bias into employment confounds estimates of intensive-margin responses (Nichols and Rothstein, 2015). My sample is not affected by such composition concerns. Fourth, I observe the near universe of treatment and control children in the relevant tax years. Research using administrative rather than survey data is generally limited to tax filers and workers with positive earnings, but I identify the policy's impact on household and individual labor supply responses among the fuller population of eligible adults, including non-filers and non-workers. My primary outcome measure also relies on a third-party reported information returns, which are less prone to misreporting as survey-reported earnings and income claimed by tax filers on their returns.

Several papers use the same year-end eligibility discontinuity to study a range of outcomes – not only employment and earnings (Feldman, Katuščák and Kawano, 2016; Wingender and LaLumia, 2017; Mortenson et al., 2018; Lippold, 2019; Garin, Jackson and Kousta, 2022), but also birth timing (Meckel, 2015; LaLumia, Sallee and Turner, 2015), children's future earnings and educational achievement (Cole, 2021; Barr, Eggleston and Smith, 2022), infant health (Schulkind and Shapiro, 2014), safety net program participation (Eng and Rinz, 2020), and interactions with the child welfare system (Rittenhouse, 2022).⁶ Lippold and Luczywek (2023) use a similar age-based discontinuity to study employment and earnings responses to the expanded federal Child Tax Credit and the YCTC, and find that the additional assistance modestly reduced treated parents' wage employment in 2021 and 2022. This paper studies the YCTC in prior years, evaluates the program's pro-employment incentive, uses different family and residence definitions, and ultimately reaches different conclusions about employment effects. Finally, Goldin et al. (2024) leverage the same age-based eligibility rule to study how the removal of the YCTC's work requirement in 2022 affected employment rates among single mothers. I study the program's structure and impacts between 2019 to 2021, consider responses beyond single female parents, and use different data and an alternative sample definition to identify eligible and near-eligible parents. Despite these differences, both papers reach similar conclusions – limited to null employment responses to the

⁶Most similar to this paper, Wingender and LaLumia (2017) and Mortenson et al. (2018) study employment and earnings responses to receiving the EITC and CTC in the months after a child is born, comparing families with children born in December to families with children born in January. Using survey data, Wingender and LaLumia find treated mothers reduce earnings by nearly the same amount as their additional tax benefits. Using a panel of near universe tax returns, Mortenson et al. find zero response. This paper is distinct from these papers in at least two ways. First, unlike the first two, I do not measure labor supply responses immediately after a child's birth, an event that can have a unique and independent effect on parent's work decisions. My setting still allows me to focus on parents of young children, who exhibit the largest labor supply responses to non-linear tax policy (Michelmore and Pilkauskas, 2021). Second, these papers only test for employment and earnings response to receipt of a lump sum transfer near birth, or a shift in when this benefit is received. Expectant parents are assumed not to adjust labor supply in anticipation of being eligible for a tax credit, since they do not know exactly when their children will be born. Here, workers can know whether they're eligible for the credit in advance, meaning I can evaluate whether eligible workers substitute towards employment in the relevant tax year.

policy. In that way, they are complements and demonstrate the robustness of this finding to those methodological differences.

Finally, I contribute to a related but newer literature looking at the potential employment responses to a flat refundable tax credit like the one available to parents in 2021 (Ananat et al., 2022; Enriquez, Jones and Tedeschi, 2023). A key concern with such policies is the extent to which they might discourage work via an income effect and by replacing existing programs' phased-in structures (Corinth et al., 2021; Goldin, Maag and Michelmore, 2021; Bastian, 2024a). This paper provides important new evidence that speaks to both concerns. Continuing discussion over resurrecting the expansion and the introduction of similar programs at the state-level suggests this policy is likely to be an important anti-poverty reform in the future, and additional empirical work related to employment responses, even if in the short-term, promises to inform those debates.

The paper proceeds as follows. In Section 2, I provide a brief overview of child-based tax credits and describe the Young Child Tax Credit, in particular. In Section 3, I describe the data I use in my analysis. In Section 4, I detail my empirical strategy. In Section 5, I discuss my results. In Section 6, I conclude.

2 Background

2.1 Policy details

The presence of a qualifying child substantially reduces tax liability for most US filers and especially for lower-income workers. The maximum federal Earned Income Tax Credit (EITC) in 2023 available to families with one child was almost seven times larger than the EITC for childless adults (\$3,995 versus \$600). State supplements to the federal EITC, available in 28 states plus the District of Columbia, are often equal to a certain percentage of the federal program, meaning they also provide disproportionate benefits to filers with children. In 2023, the Child Tax Credit (CTC) reduced tax liability by an additional \$2,000 for each child under 17 for households with at least \$2,500 in earned income. The refundable portion of the CTC, known as the Additional Child Tax Credit (ACTC), is worth up to \$1,400 for each qualifying child.

Neither the EITC nor the current CTC provide support to children whose parents or guardians have zero earned income and minimal support to those with the lowest earned income. Instead, benefits are phased in as earnings rise up to a maximum amount, depending on the filer's number of children (Figure 1). Despite the clear differences in costs associated with infants versus teenagers, these programs also do not provide different levels of assistance to families with children of different ages.⁷

⁷The temporarily expanded CTC in 2021 was an exception. In addition to making the credit fully refundable, the American Rescue Plan Act (ARPA) increased the value of the credit from \$2,000 to \$3,600 for each qualifying child younger than six, and to \$3,000 for each qualifying child six or older.

To partially address for these gaps in current policy, California lawmakers introduced the Young Child Tax Credit (YCTC) in 2019.⁸ The policy was signed into law halfway through 2019 and became effective immediately, meaning households could claim the credit in the same tax year.⁹

Between 2019 and 2021, the program provided a flat \$1,000 tax benefit to eligible households. To be eligible, a California taxpayer must have had at least one qualifying child younger than age six and earned income between \$1 and \$30,000. Households were not eligible for more than \$1,000, even if they had more than one qualifying child under age six.¹⁰ The credit was fully refundable, and its full value was available to families with just \$1 in earned income. The benefit amount was reduced by \$.20 for each additional dollar of earnings between \$25,000 and \$30,000. To claim the YCTC, eligible parents had to file a state tax return and submit the same form used to claim the state's EITC supplement. As of tax year 2022, filers with no earned income are eligible to receive the credit's full value, and the benefit's value is now indexed to inflation. In tax year 2023, the maximum credit value was \$1,110.

2.2 Effects on post-tax income

Figure 1, Panel A presents the schedule of federal and state tax credits available to a single low-income parent with two young children in California in 2019. In addition to the YCTC, a low-income working parent could claim the federal EITC, federal CTC, and the California EITC. The figure illustrates how the latter three programs phase in benefits as earnings increase. In contrast, the YCTC created a large notch at \$1 in earnings. Parents eligible for the YCTC between 2019 and 2021 faced a more than 100,000% percent negative tax rate on their first dollar of earnings – a clear positive extensive margin labor supply incentive not present anywhere else in federal or state income tax policy in the US.¹¹ If the negative marginal tax rates present in the EITC and CTC's

⁸The policy was first introduced by Governor Gavin Newsom in his administration's proposed budget, released in January 2019, alongside an expansion to the state's EITC programs. Funding for the policy came from a tax conformity package that aligned state tax policies with federal tax changes under the Tax Cut and Jobs Act (TCJA). The limited funding stemming from those reforms determined the size of the policy, including the benefit amount and age restriction. Initially, the policy included only a \$500 benefit, but that was increased to \$1,000 later in the legislative cycle. Otherwise, the initial proposal had the same structure as the final version, including the earnings requirement, age six cutoff, and the \$1,000 limit per filer. An increase in the pro-work incentive for non-workers was not a stated purpose of the policy. The policy was mainly advertised as a means of addressing living costs for lower-income families with young children. According to proponents of the policy, the age restriction reflected a sense among the public and legislators about what age corresponds to early childhood.

⁹A possible explanation for the limited responses identified in 2019 is that eligible parents were unable to adjust earnings and employment within the last six months of the year. Various frictions can slow workers' responses to substantial tax incentives (Chetty et al., 2011; Gelber, Jones and Sacks, 2020; Gudgeon and Trenkle, 2020). At the same time, we might expect larger responses immediately after the law was enacted, when public communication and news coverage of the new policy was at its highest.

¹⁰Aside from the earnings and age tests, the program uses the same eligibility rules that apply to the states's EITC supplement. All members of the tax unit must have a valid SSN (or ITIN as of 2020) and have lived in California for more than half the year. Filers must be at least 18 years old. Wages, salaries, tips and other employee compensation subject to withholding in California, plus net self-employment income count toward the earned income requirement.

¹¹This tax incentive is an order of magnitude larger than the unusually large notch studied by Tazhiddinova (2020) and much larger than those studied by Kleven and Waseem (2013). However, small sample sizes and limited response at the margin of \$1 in AGI preclude using "bunching" research design to estimate a taxable income elasticity.

schedules induce some adults to enter employment, we would expect to see such a response to this policy. At the same time, the policy could discourage work for those who would otherwise earn less than \$30,000 through both an income effect and, in the phase-out region, a substitution effect.¹² Alternatively, if the additional support helps workers better afford complements to work, like childcare or transportation, the benefit might instead encourage employment.

I do not observe California tax filing or claiming of the YCTC, which means I cannot show the policy's direct impact on either tax credits received or post-tax income in my study sample. Instead, I use NBER's TAXSIM program (Feenber and Coutts, 1993) to simulate tax liabilities for the hundreds of thousands of lower-income California households that filed a tax return in each policy year.¹³ Figure 2 plots the average post-tax pay rate (after-tax income divided by pre-tax income, or one minus their average tax rate) for eligible and ineligible California families by predicted AGI. Since households in my sample generally face a negative income tax rate, average post-tax pay rates exceed 100 percent. The size and structure of the credit means that average tax rates decline swiftly as earnings increase and then plateau. The difference in the average tax rate at the treatment threshold decreases accordingly. For households with simulated AGI around \$5,000, YCTC eligibility increases the expected post-tax pay rate by over 10 percent. The difference is much larger for households with lower incomes. For households with simulated AGI between \$10,000 and \$30,000, eligibility increases the return to work by around two to six percent.¹⁴

2.3 Setting

My study period overlaps with the outbreak of the Covid-19 pandemic – a unique time in which to examine tax filing, employment, income, and responses to tax policy aimed at low-income families. Health and economic conditions, plus the unprecedented assistance delivered to families through the tax and unemployment insurance systems, in these years could help to explain limited employment responses to other tax policies and cash transfers (Jacob et al., 2022). While the unusual health, policy, and economic landscapes raise legitimate external validity concerns, these issues do not threaten the internal validity of my study. I expand on my identification strategy below, but the consequences of the pandemic were unlikely to affect just eligible and ineligible families differently. The one exception was the temporary expansion of the federal Child Tax Credit (CTC) through the American Rescue Plan ACT (ARPA). The CTC was made fully refundable, and the per-child benefit was raised from \$2,000 for most families to \$3,000 for children aged six to 17 and \$3,600 for children under age six (Figure 1, Panel B). This means that, in 2021, marginal YCTC eligible families received an additional \$600 from the federal CTC expansion, whereas just

¹²The larger refund received in the following year represents a non-trivial share of after-tax income for eligible families within that tax year. However, receipt generates only a very small difference in lifetime income since, in my sample, even treated families become ineligible the next year when their youngest children turns six, so there's reason to anticipate the income effect would be small.

¹³I input a select number of variables observed in my linked data for these households: tax unit composition, adults and children's ages, parents' W-2 wages and taxable investment income, and state of residence.

¹⁴Transforming my treatment effect estimates into employment elasticities will require an estimate of how the policy changed the return to work, which means selecting a reasonable earnings level at which to calculate this difference in the post-tax pay rate. Refer to subsection 5.5 for a fuller discussion of this issue.

ineligible families in my sample only received the standard CTC benefit. The federal CTC reform also eliminated the prior CTC's phase-in, which had an important effect on labor supply incentives for eligible parents (Corinth et al., 2021; Goldin, Maag and Michelmore, 2021; Bastian, 2024a). This part of the reform was not a function of children's ages, however. Aside from the additional \$600 benefit, treated and control households in my setting were similarly affected by the ARPA reforms.

3 Data

I identify eligible and near-eligible families and their respective outcomes using extensive administrative records. I begin with the nearly 12 million children born between July 2013 and June 2016 according to the Social Security Administration's (SSA) Numident file.¹⁵ These children turned six years old at the end of 2019, 2020 or 2021, or at the start of 2020, 2021 or 2022. I then identify the same children in the Census Household Composition Key (CHCK), which links the more than 100 million children born between 1997 and 2023 to their likely birth parents (Genadek, Sanders and Stevenson, 2022; Bernard, Drotning and Genadek, 2024).¹⁶ Next, I match the same children to the near universe of 1040s filed in tax years 2013 to 2022.¹⁷ Relevant to this analysis, each return contains information related to tax unit structure (i.e., unique identifiers for up to six members, including a primary filer, spouse, and up to four dependents), filing status, the state of residence, and multiple income sources, including adjusted gross income (AGI) and wage and salary income. I also observe indicators for filing a 1040 with either a Schedule C and SE, on which filers claim business and self-employment income and losses, respectively.¹⁸

I define a child's relevant parent or guardian as the adult with whom the child appears most recently in the data. For example, if a child was claimed on a tax return for 2019, I assign the adult(s) who claimed them as their parent(s) in tax year 2019 for purposes of my analysis.¹⁹ If

¹⁵The Numident contains the universe of people who registered with SSA and received a social security number. The file contains dates of birth and death, county of residence at time of enumeration, and basic demographic information.

¹⁶Unlike SSA's KIDLINK database, CHCK does not match children and parents directly using the SSNs that appear on the child's birth record. Instead, children are associated with their parent's using probabilistic matching based on the names that appear on the child's birth record and household information contained in other datasets available to the Census Bureau. I find that more than 90 percent of children are claimed by their CHCK-assigned adult on a tax return zero to five tax years after their birth. Aldana (2022) finds close correspondence between the CHCK assignments and matched California birth records. Bernard, Drotning and Genadek (2024) discuss important production issues affecting the CHCK and use linked ACS data to assess how these issues affect coverage and bias in match rates.

¹⁷The Census Bureau generally does not receive 1040s filed more than one year after the end of the relevant tax year (e.g., returns filed in 2021 for tax year 2019). This limitation is unlikely to affect my analysis, since lower-income filers with children, if they do file, tend to file early in the tax season in order to claim and receive their refunds as soon as possible.

¹⁸In the process of cleaning these 1040 data, I attach each unique person to a single federal return. Individuals may appear on multiple returns if they filed an amended return, filed a return but were claimed as a dependent or spouse on another, or were appropriately claimed as dependent on a return but also had sufficiently high unearned income such that they were required to file their own return. If an individual filed an amended return, I keep the most recently filed return. If individuals appear on multiple returns as a filer and dependent, I keep the return on which they appear as a dependent and, in case of remaining duplicates, the most recently filed one and then the one with the highest AGI.

¹⁹Throughout the rest of the paper, I refer to the adults to which children are assigned via this process as their

they were not claimed on a 2019 return, I use claiming information from their return for 2018, and so on back to 2013. If the child was never claimed on a federal return, I use their birth parents according to the CHCK file.²⁰ I use the same process for identifying whether a child resides in California during the relevant tax year. If they were claimed on a 1040 filed from California, I infer they were a California resident during that tax year. If they were not claimed on a 1040 during that tax year, but they were claimed on a California resident in the prior tax year, I assume they're still a California resident. If they were never filed on a return, I assign residency using the birth state from the Numident.²¹

I match all adults, including non-filers, to their summed annual W-2 wages in each tax year. The W-2 data contain total taxable wages and salaries from each unique job (i.e., each employee-employer combination).²² A key data limitation of my study is that I lack access to individual-level measures of non-wage earnings for most tax years in my study period.²³ I use the presence of a Schedule C or Schedule SE as indicators for self-employment income. I do not observe amounts on these forms, so I cannot verify that workers' net earnings were positive. Schedule C and SE filings also suffer from important reliability concerns as measures of self-employment.²⁴ These concerns notwithstanding, these forms allow for a broader test of employment responses beyond W-2 wages alone.

Finally, I identify the sex, race and ethnicity of children's parents from the 2000 and 2010 decennial censuses, as well as the Numident.

I link individuals across these datasets using Protected Identification Keys (PIKs) assigned through the Census Bureau's Person Identification Validation System (PVS) (Wagner and Lane, 2014). This process uses personally identifying and demographic information to associate all persons who appear in records held by the Census Bureau to a unique numerical key. Nearly all individuals represented in the administrative records are associated with a PIK.²⁵ I interpret nonmatches to

"parents", even though this label is not accurate in all cases. Children can be claimed on tax returns by adults other than their birth parents or legal guardians. Still, the shorthand will be true for many children, and it allows me to avoid using a more confusing, if more technically accurate, label.

²⁰Less than two percent of children from the initial Numident sample are unmatched to any parent using this process.

²¹Compared to using the birth records, these parent and residence definitions have the advantage of more accurately associating children with a proximate claimer and placing them in their correct state of residence. The disadvantage is that they will skew my sample towards finding higher employment, since I can only update information if parents had sufficient income to file a return. This is unlikely to bias my treatment effect estimates, unless eligible parents strategically claim eligible children to receive the credit.

²²For the workers who have multiple W-2 records from the same EIN, I keep the observation with the highest total amount.

²³I can link each adult to the Information Return Master File (IRMF), but Census only receives indicators for receipt of the 1099-G, 1099-DIV, 1099-INT, 1099-MISC, and 1099-R. For tax year 2019, the 1099-MISC captures non-employee compensation and can serve as a valuable measure of third-party reported self-employment. However, starting in 2020, reporting of non-employee compensation was transitioned to a new form: the 1099-NEC. Unfortunately, as of this writing, Census does not receive indicators for receipt of the 1099-NEC in the IRMF.

²⁴Many 1099-MISC or 1099-NEC recipients with positive non-employee compensation do not file a Schedule SE or report those earnings on their 1040 (Collins et al., 2019), while other parents with no clear record of self-employment still file a Schedule SE and claim self-employment earnings, presumably to claim work-conditioned child benefits (Garin, Jackson and Koustas, 2022).

²⁵I match around 80 percent of adults in my sample to the 2010 decennial. PIK rates for survey respondents from

the IRS data as evidence of non-filing and non-employment.

For most analyses, I restrict my sample to the set of families the policy would reasonably affect. First, for the standard regression discontinuity design, I limit to children residing in California between tax years 2019 and 2021. Second, I identify the children whose parents had total wages between \$0 and \$30,000 in nominal dollars, the maximum eligible income for the YCTC, in the prior tax year.²⁶ Third, I drop nearly three million children whose parents are associated with a parent who is linked to another child younger than age six in the relevant tax year, since these families would be eligible for the YCTC no matter when their focal child was born. My final sample contains roughly nine million children, or upwards of 250,000 children born each month. Of those children, approximately one million resided in California in their relevant tax year between 2019 and 2021. My analyses generally rely on a smaller subset of the lower-income California children (roughly 50,000 each year) whose sixth birthday falls within four months of January 1.

4 Analysis

4.1 Research Design

I compare outcomes in tax year t and $t + 1$ between parents whose youngest child was born at the end of tax year $t - 6$ and parents whose youngest child was born at the start of tax year $t - 5$. For example, I compare outcomes in 2019 and 2020 between parents whose youngest child was born at the end of 2013 versus the start of 2014, since these children turned six at the end of 2019 or the start of 2020. Appendix Figure 1 illustrates, for each cohort of children and parents, the birth years corresponding to eligibility and near eligibility and their effective and subsequent tax years. The effective tax year is the year in which eligibility for the tax credit is determined. The subsequent tax year is the following year – the one in which a return for the effective tax year is typically filed and refunds are received.²⁷

To identify the YCTC's effect on adults' employment, I use a sharp regression discontinuity design. Specifically, I estimate the following model:

$$y_{it} = \alpha + \beta \mathbb{1}\{d_i \geq D_0\}_i + \delta d_i + \gamma d_i \times \mathbb{1}\{d_i \geq D_0\}_i + \theta_t + \epsilon_{it} \quad (1)$$

Observations, i , are households or a parent with a child born near the treatment threshold.²⁸ The

lower socioeconomic backgrounds tends to be lower (Bond et al., 2014).

²⁶Barr, Eggleston and Smith (2022) impose a similar restriction to identify likely EITC-eligible sample. Mortenson et al. (2018) and Lippold and Luczywek (2023) also restrict their samples to tax filers within a certain income range.

²⁷Parents are defined as of the effective tax year, and I use their outcomes in the effective and subsequent tax year. If a child is claimed by a different adult in the subsequent tax year, I do not update my parent definition and use that adult's outcome in the following tax year, assuming that it was the adult assigned to the child in the effective tax year who received YCTC benefit, assuming they were eligible.

²⁸I refer to observations as households even though treatment is a function of children's eligibility, because outcomes are observed for the children's parents. It seems more sensible to jointly refer to households' eligibility, outcomes, and

running variable, d , is a child's birthdate relative to the January 1 cutoff, D_0 . I estimate separate linear effects, δ and γ , on either side of D_0 . I restrict my analysis to children born within the birthdate interval: $d \in [D_0 - h, D_0 + h]$. The treatment variable, $\mathbf{1}\{d_i \geq D_0\}$, is an indicator for being born at the start of 2014, 2015 or 2016, meaning the child was still 5 years old at the end of the relevant tax year and eligible for the YCTC. My parameter of interest is the coefficient on this indicator term, β . Finally, I also control for each relevant tax year, θ_t . In some models, I also include a vector of control variables, X , including being born on a weekend, having one or two parents in the effective tax year, as well as the primary parent's age and race and ethnicity.²⁹

I identify the causal effect of YCTC eligibility on multiple outcomes of interest, y . First, I test for an extensive margin employment response. I use two measures to identify employment among parents: the presence of positive W-2 earnings, and a broader measure that considers the presence of positive W-2 wages, positive wage and salary income reported on their 1040, or a Schedule SE or Schedule C filed with the 1040. Second, I test for intensive margin response: parents' W-2 earnings, wage and salary income on the 1040, and their adjusted gross income (AGI). I observe each of these outcomes in tax years t and $t + 1$, and separately estimate effects for responses in each year. In the effective tax year, t , I test for whether families adjust employment and earnings in anticipation of claiming the YCTC. In the subsequent tax year, $t + 1$, I test for responses to having potentially received the credit.

To further account for unobserved differences between children born on either side of the cutoff and isolate the effect of YCTC eligibility, I estimate two alternative models. First, I compare outcomes for families residing in California relative to those residing in other states. Specifically, I implement a difference-in-regression discontinuity design (Grembi, Nannicini and Troiano, 2016), modifying the standard RD by interacting each term with an indicator for whether the child resided in California in the relevant tax year. My parameter of interest in this model is β_2 , the coefficient on the interaction between the indicators for birthdate eligibility and California residency.

$$y_i = \alpha_1 + \beta_1 \mathbf{1}\{d_i \geq D_0\}_i + \delta_1 d_i + \gamma_1 d_i \times \mathbf{1}\{d_i \geq D_0\}_i + [\alpha_2 + \beta_2 \mathbf{1}\{d_i \geq D_0\}_i + \delta_2 d_i + \gamma_2 d_i \times \mathbf{1}\{d_i \geq D_0\}_i] CA_i + \theta_t + \varepsilon_i \quad (2)$$

Second, I use California families in the pre-reform period (2016-2018) as an additional control. In this model, I interact each standard RD term with an indicator for being an eligible or near-eligible child in the post-reform period. In this model, I exclude the year-specific fixed effects.

characteristics.

²⁹The race and ethnicity variable takes on seven values: Hispanic, and among non-Hispanics, White, Black, Asian, Native Hawaiian/Pacific Islander, American Indian/Alaskan Native and Other/Multiracial. I group parents into five age bins: less than 20, between 20 and 29, 30 and 39, 40 and 49, and 50 or older. I observe parents' race and ethnicity from the 2010 decennial. If missing, I use the 2000 decennial. If still missing, I use the Numident. I assign age using birthyear from the Numident.

$$y_i = \alpha_1 + \beta_1 \mathbb{1}\{d_i \geq D_0\}_i + \delta_1 d_i + \gamma_1 d_i \times \mathbb{1}\{d_i \geq D_0\}_i + \\ [\alpha_2 + \beta_2 \mathbb{1}\{d_i \geq D_0\}_i + \delta_2 d_i + \gamma_2 d_i \times \mathbb{1}\{d_i \geq D_0\}_i] \text{postreform}_i + \mu_i \quad (3)$$

Each model represents an intent-to-treat (ITT) design, meaning estimates reflect the effect of eligibility, as opposed to receipt, on the outcome of interest. This general "statutory parameters" ITT approach is common in much of the literature studying rollout of tax policies (Bastian and Michelmore, 2018; Hoynes and Patel, 2018; Bastian and Lochner, 2022). These estimates are arguably more relevant to policymakers than treatment-on-the-treated (TOT) estimates, since they reflect average responses across the target population.

In primary models, I set h equal to 120, including children whose birthdates are within roughly four months of January 1 in the relevant calendar years. This wider bandwidth improves precision, but introduces reasonable concerns about bias, which the the D-RD models should help to address. I also exclude households whose children were born within a eight-day window around January 1 in order to minimize selection issues. Appendix Figure 8 to Appendix Figure 12 illustrate the sensitivity of my results to the bandwidth selection, donut size, and definition of the low-income sample.³⁰

4.2 Assumptions

The key identifying assumption in the regression discontinuity setting is that an observation's distance from the treatment threshold, conditional on other covariates, is as good as randomly assigned with respect to the outcomes of interest. In this setting, the assumption means children's birthdates near the end of tax years 2013 to 2015 or start of tax years 2014 to 2016 are essentially randomly assigned, and parents' outcomes would be similar if not for the different tax treatment they receive based on their youngest child's birthdate. While this assumption is not directly testable, balance in observable characteristics between treatment and control groups and smoothness in the distribution at the threshold suggest validity (McCrary, 2008; Lee and Lemieux, 2010).

I test for balance in parents' characteristics between treated and control households. Appendix Table 1 and Appendix Table 2 present results from balance tests with the low-income, California sample and the national sample, respectively. To identify these differences, I estimate versions of Equation 1 and Equation 2 in which I use family and parental characteristics as outcome variables. Characteristics include number of assigned parents in tax year t , number of older dependents, as well as father's and mother's average ages, share who report being non-Hispanic White, W-2 employment, and W-2 wages. Only one characteristic, female parents' wages, exhibits a statistically

³⁰In these figures, I highlight estimates from models in which I use the optimal bandwidth selection proposed by Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Titiunik (2015) and Calonico, Cattaneo and Farrell (2020). In most cases, that bandwidth is between 45 and 50 days.

significant difference in the California sample, but the difference is economically small. For all others, I cannot reject the null hypothesis of no difference.

Appendix Figure 2 presents evidence of smoothness in the running variable across the threshold. I plot the number of births per day nationally (Panel A) and in the low-income, California sample (Panel B) between September and April, averaged over three birth periods in my sample. The dips reflect the relative infrequency of weekend births. I also plot the average number of births within seven and four-day birthdate bins.³¹ There does not appear to be a huge spike or dip in births around January 1. Other researchers have also investigated concerns about smoothness and manipulability in birthdates around the end of the tax year. Schulkind and Shapiro (2014), LaLumia, Sallee and Turner (2015), Barr, Eggleston and Smith (2022), and Eng and Rinz (2020) conclude the response is very small and even lower for lower socioeconomic households. The manipulability concern is arguably less of a concern in this setting because parents cannot change past birth timing. Still, to minimize concerns about selection, I exclude from my analysis any children born within eight days on either side of January 1. Appendix Figure 8 to Appendix Figure 12 illustrate how treatment effects estimates vary over donut size for various outcomes.

A final concern is that other treatments might use the same age threshold as the YCTC. Aside from the additional supplement in the 2021 CTC expansion, I am unaware of any other such policy. As discussed, eligibility for child-based tax benefits does not change discontinuously at age five or six, and eligibility for other safety net programs is not affected by children's age at the end of the tax year. WIC eligibility ends at age five, but is not determined by one's age at the end of the tax year. In California, children must be five years old by September 1 in order to enroll in Kindergarten, a rule which should affect all children in my sample similarly.

5 Results

5.1 Household-level employment responses

I first examine household-level employment responses in the low-income sample (Table 1). Panel A presents results for W-2 employment, and Panel B presents estimates using the broader employment measure. For each outcome, I present estimated treatment effects from three models. The first model presents results from the standard regression discontinuity design, limited to children residing in California (Equation 1) and without further controls. The second column presents estimates using the D-RD model and national sample, differencing out effects observed for non-California families (Equation 2). The third column summarizes results from the other D-RD model using pre-reform California families (Equation 3). Both D-RD models include controls for the child being born on a weekend and their primary parents' race, age, and filing status. Columns 1 to 3 present estimated treatment effects for the effective tax year, t , and Columns 4 to 6 present estimates for the subsequent tax year, $t + 1$.

³¹Due to disclosure rules, all counts are rounded and may appear smoother than they are.

Averaged over the three policy years, roughly two-thirds of children in this sample had parents who earned any W-2 wages, while 85 percent had either positive wages or claimed self-employment income. In the California sample, having a child eligible for the YCTC is associated with a 0.16 percentage point (0.24 percent) higher W-2 employment rate. Using non-California families or pre-reform California families as additional controls yields estimated treatment effects of 0.32 percentage points (0.45 percent) and -0.77 percentage points (-1.1 percent), respectively. The treatment effect estimates using the fuller employment measure are not meaningfully different. None of these estimates are statistically indistinguishable from zero. Using the broader employment measure, I can rule out an employment effect greater than 0.75 percent. Point estimates for treatment effects in the subsequent tax year are consistently positive, but I cannot reject that they are zero or slightly negative.

Figure 3 and Figure 4 illustrate the treatment effect estimates from the first and second models using regression discontinuity plots. The blue and gold dots represent averages for each outcome variable within four-day birthdate bins for children residing in California and children residing in any other state, respectively. I include the estimates and standard errors of the corresponding discontinuity from Equation 1 and Equation 2 within each plot.³²

Appendix Table 5 summarizes treatment effects for the set of households in which parents had no W-2 wages in the prior tax year. If the policy increased labor market entry, we would expect to observe more positive estimates in this sample. In fact, effects appear lower, though they're still indistinguishable from zero and not significantly different than the results summarized in Table 1. They're also less precise due to the smaller sample size. Appendix Table 6 summarizes treatment effects for the full set of households, not conditioning on parents' prior year income. As expected, effects are more attenuated, since they presumably include non-responses among less affected higher-income households.

5.2 Employment responses by parents' sex

Table 2 summarizes estimated employment effects for single female (Panel A) and male parents (Panel B). As in Table 1, I present treatment effect estimates for positive W-2 wages and the broader employment measure in both the effective and subsequent tax years across three specifications.

The point estimates for the employment effect in the effective tax year among single mothers range from -0.17 to 0.38 percentage points (-0.1 to 0.5 percent) depending on the outcome measure and specification. Effects in the subsequent year are of a similar magnitude. Effects among single fathers appear more positive, though less precise and still insignificant. In the effective tax year, the estimated treatment effects on W-2 employment are approximately 0.6 percentage points (1 percent) and for the broader employment measure, range from -0.09 to 0.16 percentage points (-2.0 to 0.5 percent). Treatment effects in the subsequent tax year are higher and, in some instances,

³²Following the guidance for such figures summarized in (Korting et al., 2023), I do not present best-fit lines.

statistically significant (1.2 to 1.4 percentage points, or 2.0 percent).³³

Appendix Table 7 summarizes effects on wage employment for single and married parents.³⁴ How non-linear tax policy affects employment among secondary earners is a particular policy concern given the steeper marginal rates typically applied to their wages (Kearney and Turner, 2013; Michelmore, 2018). My findings point to positive extensive margin responses, especially among female parents, but I still cannot reject the null that they're zero. Due to the smaller sample sizes, these estimates are especially sensitive to bandwidth selection. Appendix Table 8 presents results for single female and male parents using alternative bandwidths. Appendix Table 9 does the same, but uses the full sample of age-eligible children, instead of conditioning on parents' prior income.

5.3 Employment responses by policy year

Figure 5 summarizes treatment effect estimates separately by policy year.³⁵ Despite the markedly different policy incentives and labor markets facing these families, treatment effect estimates are fairly consistent across specifications and tax years. Using the California sample, treatment effects are centered at zero. Using the national sample and the D-RD design, all point estimates of employment effects, both in the effective and subsequent tax year appear positive, but most are not statistically significantly different than zero. Differences across year within outcome are not statistically different, and only a handful are statistically different than zero. These largely consistent results also point against the policy's novelty as an explanation for limited effects. Parents who had two years to receive the credit and learn about its parameters still exhibited null responses to incentives in 2021.

An exception is the effect on W-2 employment in 2021, which is positive and just significant in the California-only sample. This effect is notable, because this is the year in which treated families also received an additional \$600 from the expanded Child Tax Credit. Lippold and Luczywek (2023) find that this supplement reduced wage employment among eligible parents, suggesting the estimated positive effect in the California could be understated. However, when using the D-RD design and the national sample, which should difference out any effects of the CTC expansion, the estimated treatment effect of the YCTC is attenuated and becomes insignificant.

³³Instead of evidence of a positive income effect, it's possible that this result is attributable to a positive, but imprecisely estimated, *substitution* effect from the prior tax year, which continues into the subsequent year. This is unlikely for two reasons. First, if parents were aware of the policy and responded in the effective year, one might expect such a response to a time-limited tax incentive to involve substitution across time, meaning a shift from the next year (the higher tax period) to the effective one. Second, it's not clear why a lingering effect from a positive response in the prior year would be so much larger than the initial response, especially given the size of the lump sum transfer and the higher tax rate applied to their subsequent earnings.

³⁴I use "married" as a shorthand for whether the focal child is associated with two adults in a given policy year. In many instances, these parents are each linked to the child because they filed together and claimed the child on a 1040. For a smaller share of families, the parents may have filed together in a prior year and I assume they're still together and residing with the child in the relevant tax year, or they're the child's birth parents and the child has never been claimed on a 1040. In the latter cases, the parents might not in fact be married.

³⁵To identify these effects, I re-estimate each model described in Section 4 excluding the fixed effects for policy year.

5.4 Intensive margin responses

I also identify intensive margin responses within the low-income sample. Recall that expected effects for these outcomes are more ambiguous. Anticipated eligibility or actual receipt of the benefit in the subsequent year could discourage work among incumbent workers through an income effect. Alternatively, if additional income facilitates employment, eligibility could be associated with higher earnings.

Table 4 summarizes my results. Panel A presents estimates for AGI, Panel B presents estimates for total W-2 wages, and Panel C presents total W-2 wages among filers. Average AGI among control California parents who filed a return is approximately \$33,000, and eligibility for the YCTC is associated with a \$560 decrease. The D-RD model using non-California families as a control also suggests a negative effect, while the point estimate for the pre-reform D-RD is positive. None of these estimates are statistically significant. Average baseline wages for California parents were approximately \$23,000, and YCTC eligibility is associated with an average increase of \$100. D-RD models also yield marginally positive effects. Effects on wages among filers are largely similar. None of these effects are statistically significant.

Effects on AGI in the subsequent year appear negative. Looking at the California-only and national D-RD model, eligibility seems associated with a decrease in AGI on the order of the YCTC benefit itself, consistent with a negative income effect. However, this effect is not robust to using the pre-reform D-RD model. The decrease in AGI also seems to be occurring through a channel other than wages, since we observe no significant decline in total wages for treated families in the subsequent year.

Appendix Figure 6 presents estimates for intensive margin outcomes separately by policy year. Consistent with the results summarized in Figure 5, treatment effects do not exhibit significant variation over the study period.

Employment entrances can confound studies of intensive margin responses. Even though I find limited to no positive extensive margin responses, I account for this possibility by also identifying effects among households with positive prior year earnings (Appendix Table 4). YCTC eligibility seems associated with substantial reductions in AGI and wages within the California-only sample, but this result is not robust to using the D-RD models.

5.5 Employment elasticity

I conclude by converting extensive margin effects for households and single mothers into estimates of employment elasticities: the percent change in the employment rate, E , divided by the percent change in the expected return to work, R : $\frac{\% \Delta E}{\% \Delta R}$.³⁶ The percent change in employment is

³⁶I focus on the extensive margin response because that is where the tax incentives are greatest, it's where workers are most sensitive to these tax incentives (Saez, 2002; Eissa and Hoynes, 2006; Nichols and Rothstein, 2015), and it's the margin which has drawn the most interest in debates over the expanded CTC (Corinth et al., 2021; Goldin, Maag and Michelmore, 2021; Bastian, 2024a).

the difference in the share of households or single mothers with earned income in the effective tax year that's attributable to YCTC eligibility, which I calculate by dividing my treatment effect estimates from Panels A and B in Table 1 by the respective control group's average employment in the effective tax year. The percent change in the expected return to work is captured by the difference in the post-tax pay rate attributable to YCTC eligibility. As discussed in Section 2, that difference declines as earnings increase. To identify a single estimate, I must select an earnings level that reflects what adults on the margin of employment might expect to earn if they work. I use the average W-2 earnings among adults in my sample who worked in a given tax year but had no earnings in the prior year.

Table 5 summarizes my estimates of each component of this calculation. Panel A presents estimates at the household-level, and Panel B presents estimates among single female parents. I calculate elasticities by policy year since employment effects can vary and, more importantly, estimates of the return to work due to the YCTC change each year due to the broader policy landscape. Column 1 identifies the policy year, and Column 2 identifies the number of California households or single female parents who enter into my estimation. Column 3 summarizes average annual earnings in each policy year among household and mothers in my sample who were not employed in the prior year. Column 4 summarizes estimates of the percent change in the return to work attributable to YCTC eligibility corresponding to that earnings level in each policy year. Column 5 summarizes estimated employment effects in each year, which are the point estimates from the national D-RD model using wage employment as the outcome and summarized in Figure 5.³⁷ Column 4 reports the percent change in employment, which is the percentage point treatment effect divided by the baseline employment rate. And the final column presents the best estimates of the implied extensive margin elasticity, which again is the ratio of the percent change in employment (Column 4) and the percent change in the return to work (Column 2) within each year.³⁸

Using these estimates, the employment elasticity is 0.26 for households and 0.12 for single mothers. These estimates are on the lower end of the range of elasticities for single mothers reported in the recent EITC literature (Chetty et al., 2011; Hoynes and Patel, 2018; Bastian and Jones, 2021), and used by Bastian (2024a), Goldin, Maag and Michelmore (2021), and Corinth et al. (2021) in their simulations of the employment response to the CTC. In his simulation of the potential employment effects of the expanded CTC, Bastian (2024a) uses an elasticity of 0.4, which is informed largely by estimates of the same elasticity for EITC-eligible single mothers. And in their simulation of the likely employment effects of the expanded CTC in 2021, Corinth et al. (2021) use an

³⁷I use estimates from the D-RD models assuming that other states as an additional control decreases any lingering bias from birth timing. Results from these models also tend to not only be more precise, but higher, meaning they can be interpreted as reasonable upper bounds. Still, my finding that elasticity tends to be small is largely robust to alternative models and outcomes. Using the standard RD and the California sample, my best estimates of the employment elasticity for households is -.025 and for single mothers is 0.158. When using the combined wage and self-employment measure, my estimate of the extensive margin response for households is 0.014, and the corresponding elasticity estimate is 0.035.

³⁸The averages in the final row of each panel are weighted according to the number of households and parents in each policy year. This means that the ratio of those average percent change in employment and percent change in the return to work will not equal the weighted average of the within-year elasticities in the final column.

employment elasticity of 0.75, which was based on the midpoint of a range of estimates collected by McClelland and Mok (2012). My results generally rule out an elasticity as high as 0.75, even for low-income single mothers. Only by taking the upper bound of my estimated extensive margin responses and assuming the lowest return to work can I arrive at an estimate that high.

6 Conclusion

In this paper, I study a unique state tax policy that provided a flat \$1,000 benefit to California parents with a young child and low positive earned income. The policy produced a significant change in the return to work for eligible families, including an unprecedented negative marginal tax rate on their first dollar of earnings. I use rich administrative data to identify eligible and near-eligible families and to observe their short-run employment responses.

Despite the substantial change in tax incentives, I find limited evidence of effects on claiming, employment, or earnings among eligible families. Eligible families appear just as likely to be employed in the effective policy year, according to linked tax records, as ineligible families. My finding of limited employment effects is robust to differencing out responses from non-California families in the study period and California families before the policy took effect. I also find limited to no evidence that receipt of the credit affected treated families' employment in the subsequent tax year. Nearly all of the estimated employment results, across multiple specifications and sample definitions, are statistically indistinguishable from zero.

These null results underscore the extent to which adults may not have full information about tax incentives or are prevented from responding to them by important labor market and tax filing frictions. They also imply small disemployment effects from policy reforms that eliminate or reduce phased-in tax benefits.

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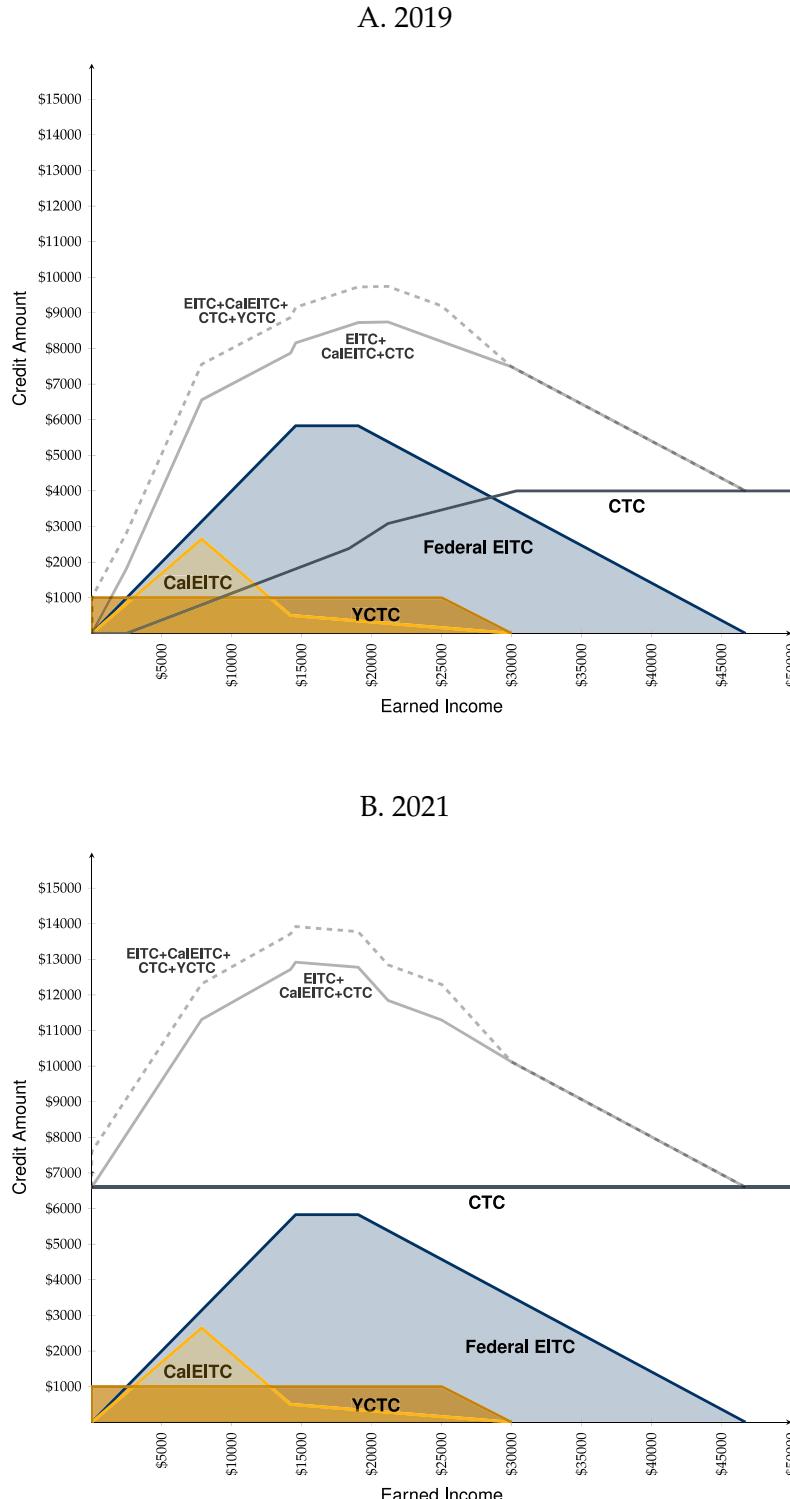
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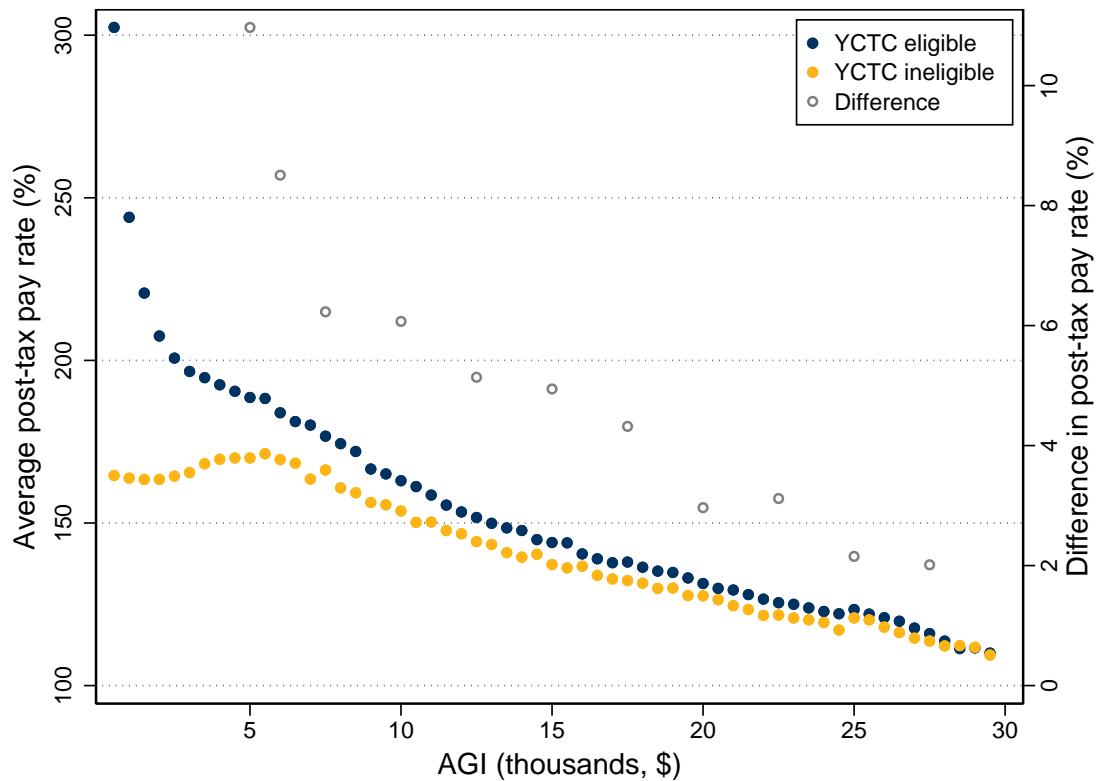
7 Figures and Tables

Figure 1: Benefit schedule for Federal EITC, California EITC, and California Young Child Tax Credit for single filers with two children



Notes. Figure 1 presents the benefit schedule for the Federal EITC, CalEITC and YCTC in tax years 2019 and 2021 for a single filer with two children, one younger than age six and the other between the age of 6 and 17. The shaded blue area represents the schedule for the Federal EITC. The light gold area represents the schedule for the CalEITC. The dark grey line represents federal CTC schedule. The darker gold area represents the schedule for the YCTC. The dotted gray line represents the total benefits for which taxpayers might be eligible, and the solid grey line represents the same sum minus the new YCTC.

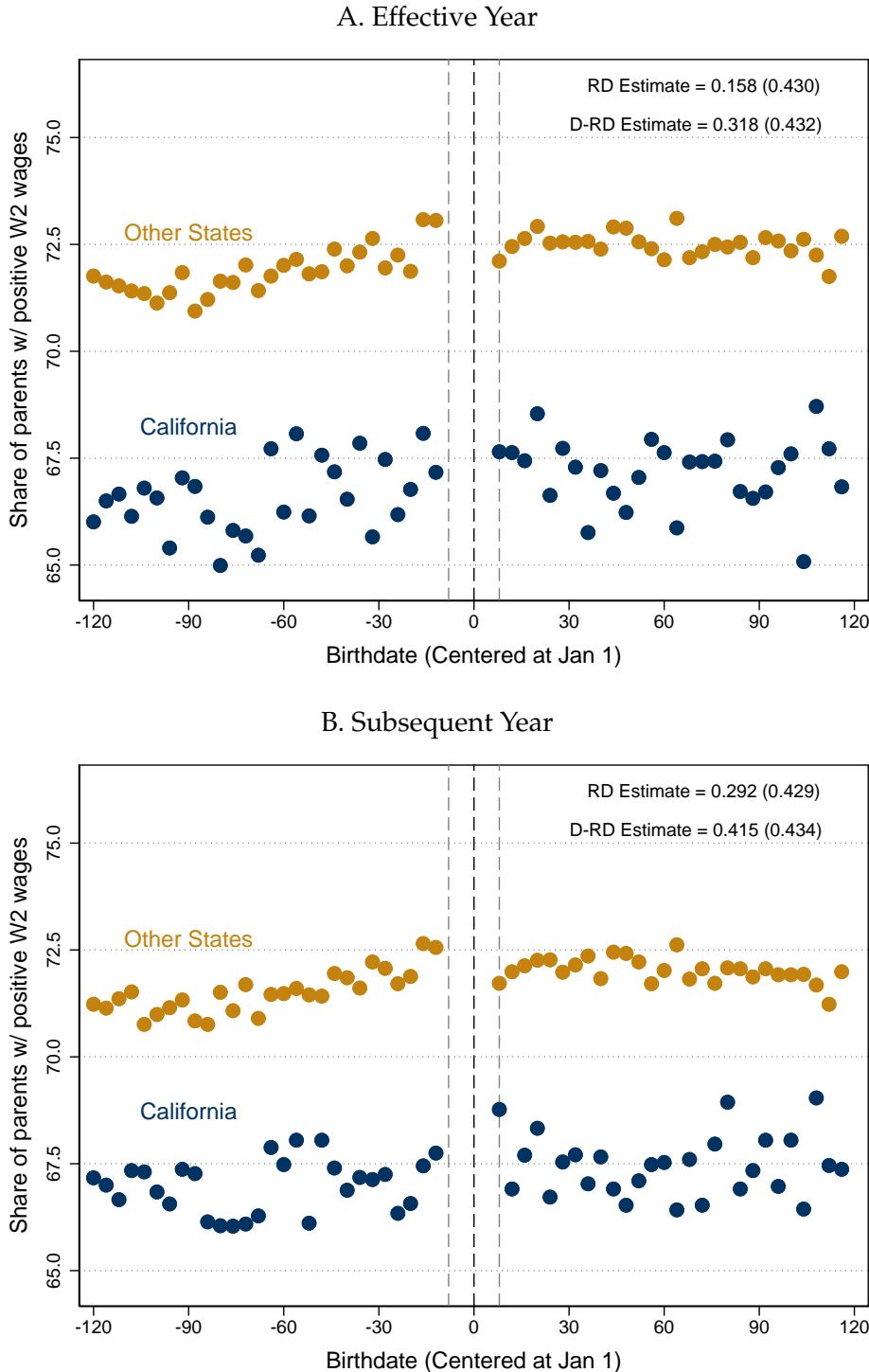
Figure 2: Estimated impact of YCTC eligibility on post-tax pay rate



Notes. Figure 2 plots the average post-tax pay rate for YCTC eligible and ineligible California families from the low-income sample within \$500 AGI bins. The grey markers identify the estimated percent change in the post-tax pay rate at select AGI levels. Averages are rounded per Census Bureau's disclosure rules.

Source. IRS 1040s and W-2 TY 2019, SSA Numident.

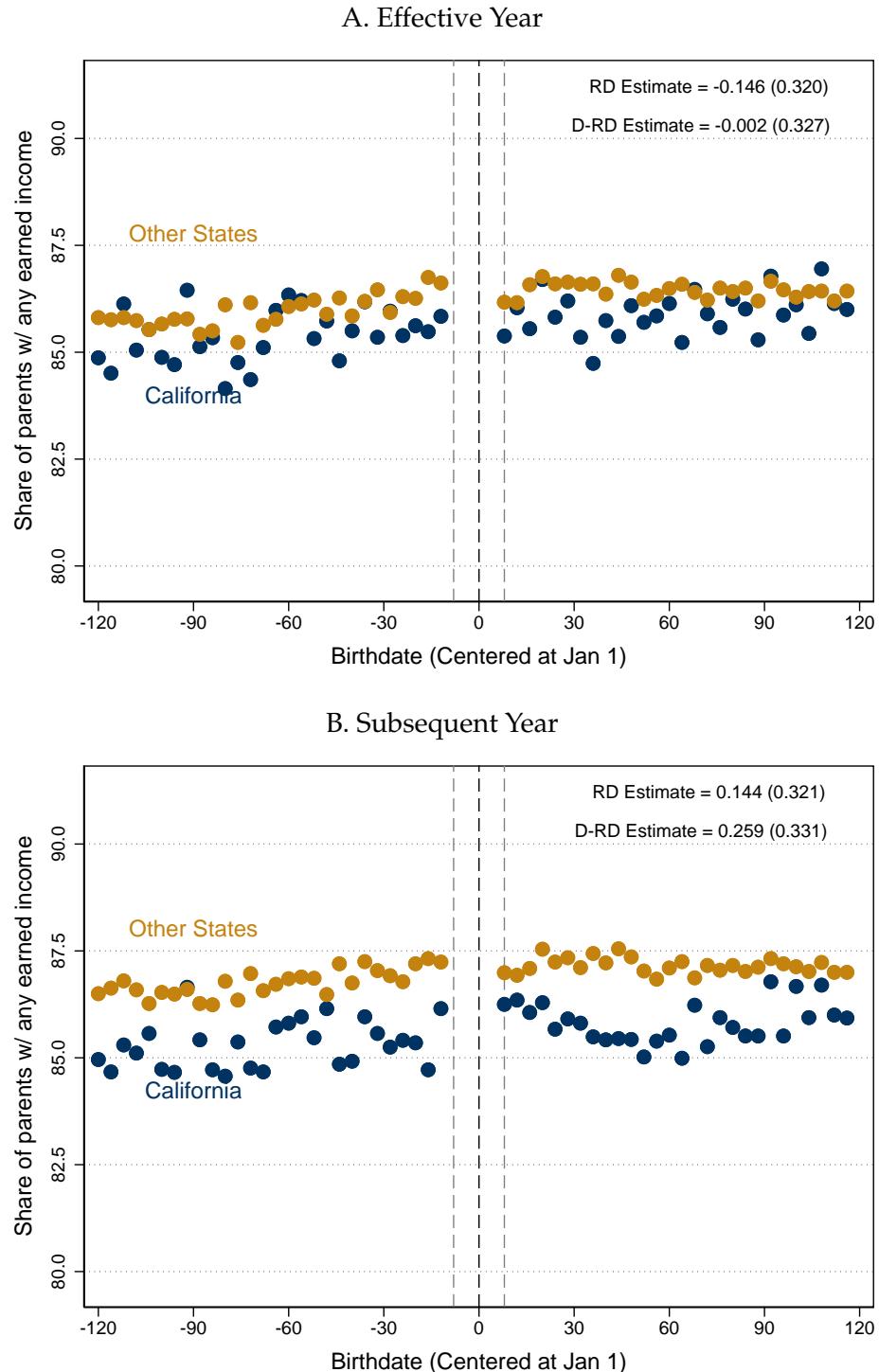
Figure 3: Regression discontinuity graph of birthdate on parents' W-2 employment rate in low-income sample



Notes. Figure 3 plots the share of children within 4-day birthdate bins residing in (blue dots) or outside of (gold dots) California whose parents had positive W-2 wages in the effective tax year (Panel A) and the subsequent tax year (Panel B), averaged across the three policy years. The dotted vertical lines indicate the eight-day donut excluded from all estimations. Estimates of the discontinuities are included in the upper-right hand corner, and correspond to the estimates reported in Table 3. All estimates are rounded per the Census Bureau's disclosure rules.

Sources. IRS 1040 and W-2 returns, tax years 2018-2022; SSA Numident; Census Household Composition Key.

Figure 4: Regression discontinuity graph of birthdate on parents' W-2 and/or self-employment rate in low-income sample

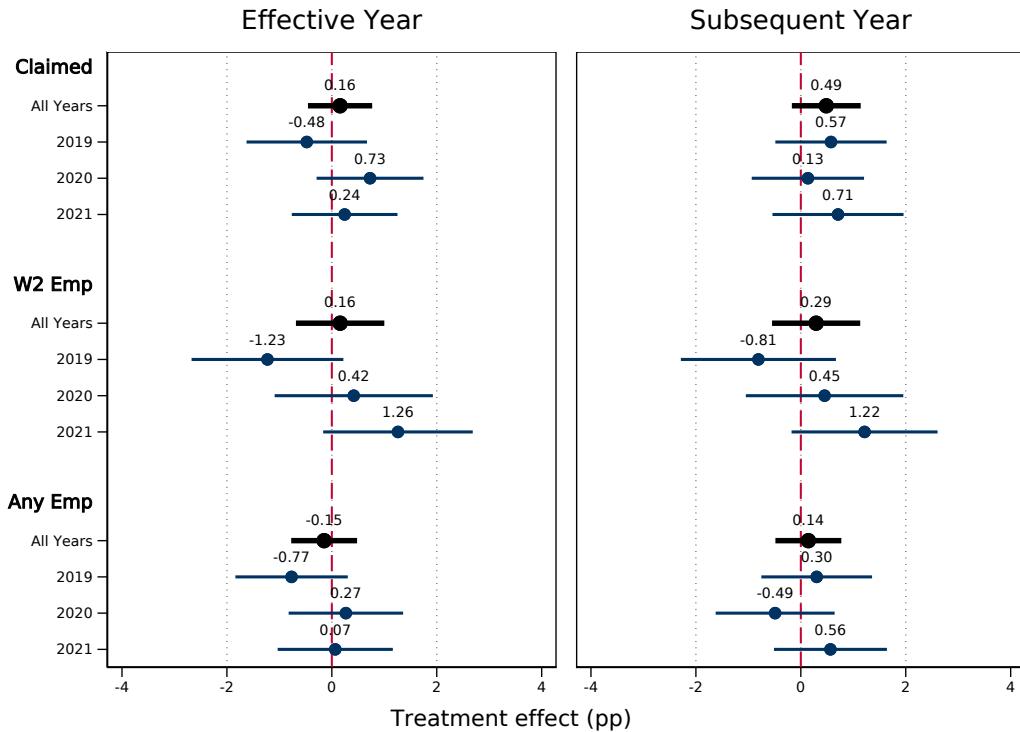


Notes. Figure 4 plots the share of children within 4-day birthdate bins residing in (blue dots) or outside (gold dots) of California whose parents had positive W-2 wages, positive wage and salary income on their 1040, or seemed to claim positive self-employment income in the effective tax year (Panel A) and the subsequent tax year (Panel B), averaged across the three policy years. The dotted vertical lines indicate the eight-day donut excluded from all estimations. Estimates of the discontinuities are included in the upper-right hand corner, and correspond to the estimates reported in Table 3. All estimates are rounded per the Census Bureau's disclosure rules.

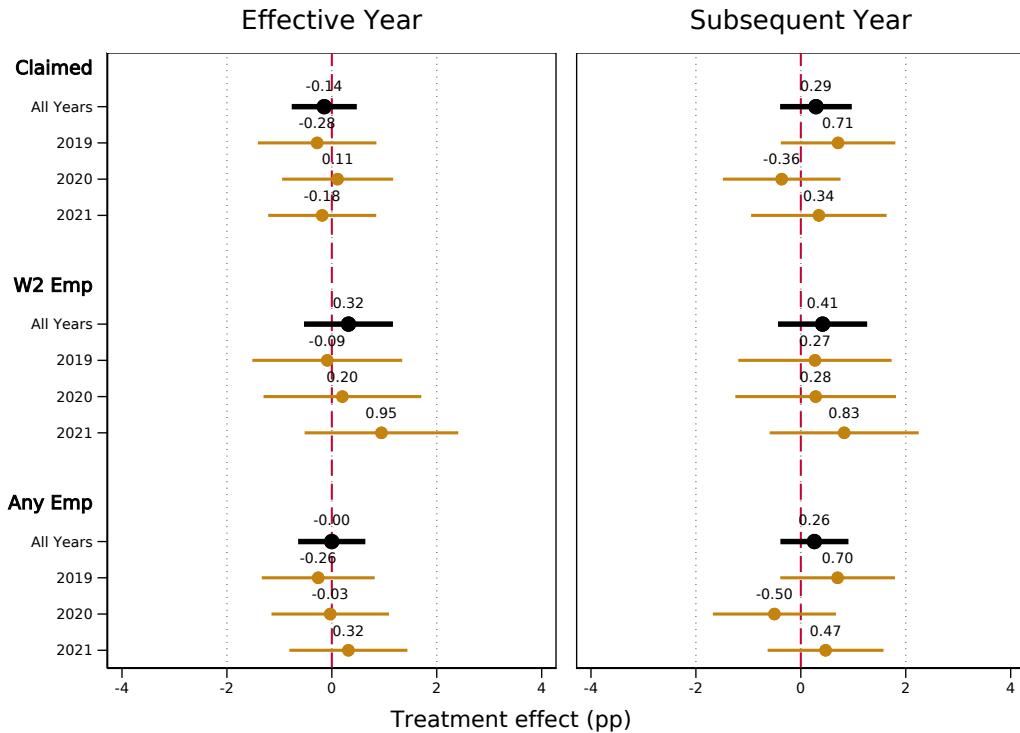
Sources. IRS 1040 and W-2 returns, tax years 2018-2022; SSA Numident; Census Household Composition Key.

Figure 5: Treatment effect estimates for extensive margin outcomes by policy year

A. California sample



B. National sample



Notes. Figure 5 plots treatment effect estimates for extensive margin outcomes overall and by policy year using the California-only sample (Equation 1) and the D-RD estimates from the national sample (Equation 2). All estimates are rounded per the Census Bureau's disclosure rules.

Sources. IRS 1040 and W-2 returns, tax years 2019 to 2022; SSA Numident; Census Household Composition Key.

Table 1: Treatment effects on parents' employment within low-income sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any W-2 Employment						
Treatment	0.16 (0.43)	0.32 (0.43)	-0.77 (0.60)	0.29 (0.43)	0.41 (0.43)	0.07 (0.60)
Control Mean	66.6	71.3	67.9	67.0	71.0	68.2
N	240,000	2,093,000	480,000	240,000	2,093,000	480,000
Bandwidth	120	120	120	120	120	120
Panel B: Any Employment						
Treatment	-0.15 (0.32)	-0.00 (0.33)	-0.30 (0.44)	0.14 (0.32)	0.26 (0.33)	0.38 (0.44)
Control Mean	85.4	85.9	86.2	85.3	86.6	86.4
N	240,000	2,093,000	480,000	240,000	2,093,000	480,000
Bandwidth	120	120	120	120	120	120
Covariates		X	X		X	X
All States		X			X	
Prior Years			X			X

Notes. Table 1 summarizes treatment effect estimates on parents' employment between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p< 0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Table 2: Treatment effects on employment among parents by sex and family status within low-income sample

	Effective Tax Year				Subsequent Tax Year			
	(1) Any Wages	(2) Any Wages	(3) Any Emp	(4) Any Emp	(5) Any Wages	(6) Any Wages	(7) Any Emp	(8) Any Emp
Panel A: Single female parents								
Treatment	0.38 (0.57)	0.30 (0.58)	-0.17 (0.44)	-0.06 (0.44)	0.09 (0.58)	-0.05 (0.59)	-0.20 (0.44)	-0.17 (0.45)
Control Mean	74.2	74.2	86.7	86.7	73.5	73.5	86.9	86.9
N	110,000	1,158,000	110,000	1,158,000	110,000	1,158,000	110,000	1,158,000
Using BW	120	120	120	120	120	120	120	120
Panel B: Single male parents								
Treatment	0.60 (0.98)	0.56 (0.97)	0.16 (0.89)	-0.09 (0.90)	1.28 (0.97)	1.50 (0.97)	1.77** (0.84)	1.69* (0.87)
Control Mean	57.2	57.2	71.4	71.4	57.0	57.0	75.0	75.0
N	50,500	440,000	50,500	440,000	50,500	440,000	50,500	440,000
Using BW	120	120	120	120	120	120	120	120
All States	X		X		X		X	
Covariates	X		X		X		X	

Notes. Table 2 summarizes treatment effect estimates on parents' employment and earnings by marital status. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 for tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Table 3: Treatment effects on birth parents' employment within low-income sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Birth mothers						
Treatment	0.50 (0.46)	0.13 (0.47)	-0.56 (0.64)	0.45 (0.46)	0.11 (0.47)	-0.18 (0.64)
Control Mean	61.9	66.7	63.0	62.8	67.0	64.0
N	220,000	2,003,000	440,000	220,000	2,003,000	440,000
Bandwidth	120	120	120	120	120	120
Panel B: Birth fathers						
Treatment	0.38 (0.53)	-0.04 (0.55)	-1.34* (0.75)	0.96* (0.53)	0.70 (0.55)	0.55 (0.75)
Control Mean	60.5	62.1	60.9	61.0	62.3	61.7
N	170,000	1,461,000	330,000	170,000	1,461,000	330,000
Bandwidth	120	120	120	120	120	120
Covariates		X	X		X	X
All States		X			X	
Prior Years			X			X

Notes. Table 3 summarizes treatment effect estimates on parents' employment between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p< 0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Table 4: Treatment effects on parents' income within low-income sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AGI						
Treatment	-561 (387)	-361 (372)	401 (495)	-1,629** (652)	-1,156* (653)	-132 (856)
Control Mean	32,920	28,320	31,500	40,380	35,130	39,150
N	200,000	1,803,000	400,000	200,000	1,770,000	400,000
Bandwidth	120	120	120	120	120	120
Panel B: Total W-2 Wages						
Treatment	107 (226)	178 (228)	428 (290)	316 (343)	546 (344)	816* (430)
Control Mean	22,980	21,420	22,430	28,830	26,130	27,420
N	160,000	1,498,000	330,000	160,000	1,491,000	330,000
Bandwidth	120	120	120	120	120	120
Panel C: Total W-2 Wages, Conditional on Filing						
Treatment	99 (234)	145 (235)	453 (300)	312 (337)	453 (338)	765* (431)
Control Mean	23,290	21,670	22,870	28,740	26,130	27,600
N	150,000	1,388,000	300,000	140,000	1,364,000	300,000
Bandwidth	120	120	120	120	120	120
Covariates	X		X		X	X
All States	X				X	
Prior Years		X				X

Notes. Table 4 summarizes treatment effect estimates on parents' income between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p< 0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Table 5: Calculations of employment elasticity estimates

Year	N	Marginal Earnings	% ΔR	ΔE	% ΔE	ε
<i>Panel A: Households</i>						
2019	78,500	\$20,000	3.9	-0.09	-0.12	-0.031
2020	74,500	\$23,000	2.6	0.20	0.28	0.108
2021	82,000	\$26,000	2.0	0.95	1.36	0.681
Average		\$23,040	2.8	0.37	0.52	0.261
<i>Panel B: Single Female Parents</i>						
2019	36,500	\$9,000	6.2	-0.18	-0.23	-0.037
2020	36,500	\$9,600	6.7	0.30	0.41	0.061
2021	40,000	\$10,500	3.4	0.76	1.06	0.310
Average		\$9,720	5.4	0.31	0.43	0.118

Notes. Table 5 reports estimates of extensive margin elasticities for households (Panel A) and single female parents (Panel B) given estimates of the percent change in the return to work from YCTC eligibility and the estimated percent change in employment within each population. Refer to Section 5 for discussion of each value's source and derivation. All estimates are rounded per the Census Bureau's disclosure rules.

Source. IRS 1040 and W-2s TYs 2018-2022. SSA Numident, Census Household Composition Key, NBER TAXSIM.

Appendix

Employment Responses to Earned Income Tax Benefits: Evidence from California's Young Child Tax Credit

Appendix Table 1: Balance table for parents' characteristics within California sample

	Control Average	Post Jan 1 Diff	P-value	N
Number of parents	1.27 (0.00)	-0.00 (0.00)	0.28	240,000
Number of older dependents	0.67 (0.01)	0.00 (0.01)	0.63	230,000
Female Parent				
Age	34.76 (0.07)	-0.15 (0.10)	0.11	170,000
Percent White	16.64 (0.28)	0.72 (0.41)	0.07	170,000
Percent with W-2 wages in prior TY	63.02 (0.36)	0.85 (0.52)	0.10	170,000
Total wages in prior TY	9,252 (75)	236 (108)	0.03	170,000
Male Parent				
Age	38.88 (0.10)	-0.20 (0.15)	0.18	99,000
Percent White	19.99 (0.38)	0.78 (0.55)	0.15	110,000
Percent with W2 wages in prior TY	50.63 (0.47)	1.24 (0.68)	0.07	110,000
Total wages in Prior TY	7,897 (96)	211 (137)	0.12	110,000

Notes. Appendix Table 1 presents results from balance tests of key characteristics for parents of children born in at the end or beginning of tax years 2013 to 2016. Differences are identified by estimating Equation 1. Column 1 reports the average value for that characteristic among ineligible parents. Column 2 contains estimate of β , or the discontinuity at the treatment threshold. Column 3 contains the p-value associated with the coefficient.

Source. IRS 1040 TY2018-2022, 2000 and 2010 Decennial, SSA Numident.

Appendix Table 2: Balance table for parents' characteristics within national sample

	Control Average	Post Jan 1 Diff	P-value	N
Number of parents	1.21 (0.00)	-0.00 (0.00)	0.38	2,093,000
Number of older dependents	0.70 (0.00)	0.01 (0.01)	0.30	2,058,000
Female Parent				
Age	34.25 (0.02)	-0.05 (0.10)	0.61	1,573,000
Percent White	38.12 (0.12)	0.62 (0.54)	0.25	1,592,000
Percent with W-2 wages in prior TY	71.22 (0.12)	0.46 (0.52)	0.38	1,592,000
Total wages in prior TY	10,610 (26)	144 (113)	0.20	1,592,000
Male Parent				
Age	38.02 (0.04)	-0.10 (0.16)	0.52	850,000
Percent White	43.00 (0.17)	0.96 (0.70)	0.17	870,000
Percent with W2 wages in prior TY	56.94 (0.18)	1.24 (0.72)	0.08	870,000
Total wages in Prior TY	9,311 (37)	234 (151)	0.12	870,000

Notes. Appendix Table 2 presents results from balance tests of key characteristics for parents of children born in at the end or beginning of tax years 2013 to 2016. Differences are identified by estimating Equation 2. Column 1 reports the average value for that characteristic among ineligible parents. Column 2 contains estimate of β_2 , or the discontinuity at the treatment threshold. Column 3 contains the p-value associated with the coefficient.

Source. IRS 1040 TY2018-2022, 2000 and 2010 Decennial, SSA Numident.

Appendix Table 3: Treatment effects on parents' employment within low-income sample

	Effective Tax Year				Subsequent Tax Year			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Any W-2 Employment								
Treatment	1.19 (0.80)	1.22* (0.70)	1.48* (0.87)	-0.69 (1.37)	1.17 (0.73)	1.41* (0.73)	1.23 (0.92)	0.07 (1.40)
Control Mean	67.0	67.0	71.8	68.1	67.1	67.1	71.5	68.3
N	90,500	100,000	730,000	140,000	100,000	96,000	680,000	140,000
Bandwidth	51	56	47	41	57	54	44	40
Panel B: Any Employment								
Treatment	0.33 (0.53)	0.24 (0.60)	1.20 (1.14)	-1.24 (1.09)	1.27** (0.63)	1.16* (0.63)	2.41** (1.09)	1.12 (0.93)
Control Mean	85.7	85.5	86.3	86.3	85.4	85.4	86.9	86.5
N	110,000	86,000	380,000	130,000	83,500	81,500	400,000	150,000
Bandwidth	59	49	28	38	48	47	29	44
Covariates	X	X	X	X	X	X	X	X
All States		X				X		X
Prior Years			X				X	

Notes. Appendix Table 3 summarizes treatment effect estimates on parents' employment between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Table 4: Treatment effects on parents' income within positive earnings sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: AGI						
Treatment	-1,888*** (547)	-176 (450)	-528 (726)	-1,327 (932)	793 (843)	416 (1,231)
Control Mean	93,770	93,770	93,770	113,400	113,400	113,400
N	470,000	4,090,000	890,000	450,000	4,014,000	870,000
Bandwidth	120	120	120	120	120	120
Panel B: Total W-2 Wages						
Treatment	-1,439*** (476)	-40 (399)	-312 (636)	-1,843** (788)	-108 (703)	-309 (1,026)
Control Mean	85,400	85,400	85,400	106,600	106,600	106,600
N	480,000	4,221,000	920,000	470,000	4,137,000	900,000
Bandwidth	120	120	120	120	120	120
Panel C: Total W-2 Wages, Conditional on Filing						
Treatment	-1,560*** (493)	56 (411)	-483 (662)	-1,394* (800)	253 (711)	126 (1,055)
Control Mean	86,640	86,640	86,640	103,500	103,500	103,500
N	460,000	3,976,000	860,000	430,000	3,804,000	820,000
Bandwidth	120	120	120	120	120	120
Covariates		X	X		X	X
All States		X			X	
Prior Years			X			X

Notes. Appendix Table 4 summarizes treatment effect estimates on parents' income between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Table 5: Treatment effects on parents' employment within zero-wage sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any W-2 Employment						
Treatment	-0.68 (0.67)	-0.14 (0.71)	-1.26 (0.95)	0.31 (0.72)	0.23 (0.76)	0.18 (1.02)
Control Mean	24.7	24.7	24.7	31.9	31.9	31.9
N	82,000	600,000	160,000	82,000	600,000	160,000
Bandwidth	120	120	120	120	120	120
Panel B: Any Employment						
Treatment	-1.01 (0.73)	-0.41 (0.73)	-1.05 (1.03)	0.06 (0.71)	0.16 (0.74)	0.96 (0.99)
Control Mean	67.5	67.5	67.5	70.1	70.1	70.1
N	82,000	600,000	160,000	82,000	600,000	160,000
Bandwidth	120	120	120	120	120	120
Covariates		X	X		X	X
All States		X			X	
Prior Years			X			X

Notes. Appendix Table 5 summarizes treatment effect estimates on parents' employment between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Table 6: Treatment effects on parents' employment within full sample

	Effective Tax Year			Subsequent Tax Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any W-2 Employment						
Treatment	-0.04 (0.20)	0.14 (0.20)	-0.41 (0.29)	-0.07 (0.21)	0.12 (0.21)	-0.04 (0.30)
Control Mean	85.9	85.9	85.9	85.3	85.3	85.3
N	590,000	4,974,000	1,115,000	590,000	4,974,000	1,115,000
Bandwidth	120	120	120	120	120	120
Panel B: Any Employment						
Treatment	-0.09 (0.14)	0.01 (0.14)	-0.15 (0.20)	-0.06 (0.14)	0.05 (0.15)	0.15 (0.20)
Control Mean	93.9	93.9	93.9	93.4	93.4	93.4
N	590,000	4,974,000	1,115,000	590,000	4,974,000	1,115,000
Bandwidth	120	120	120	120	120	120
Covariates	X		X		X	X
All States	X				X	
Prior Years			X			X

Notes. Appendix Table 6 summarizes treatment effect estimates on parents' employment between tax years 2019 to 2022. Refer to Section 4 a discussion of the empirical design. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 records from tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Table 7: Treatment effects on parents' W-2 employment by sex and marital status, low-income sample

	Effective tax year		Subsequent tax year	
	(1) CA Sample	(2) National Sample	(3) CA Sample	(4) National Sample
Panel A: Single female parents				
Treatment	0.38 (0.57)	0.30 (0.58)	0.09 (0.58)	-0.05 (0.59)
Control Mean	74.2	74.2	73.5	73.5
N	110,000	1,158,000	110,000	1,158,000
Using BW	120	120	120	120
Panel B: Single male parents				
Treatment	0.60 (0.98)	0.56 (0.97)	1.28 (0.97)	1.50 (0.97)
Control Mean	57.2	57.2	57.0	57.0
N	50,500	440,000	50,500	440,000
Using BW	120	120	120	120
Panel C: Married female parents				
Treatment	0.89 (0.91)	1.05 (0.97)	0.32 (0.92)	0.41 (0.98)
Control Mean	43.9	43.9	46.2	46.2
N	58,500	430,000	58,500	430,000
Using BW	120	120	120	120
Panel D: Married male parents				
Treatment	-0.13 (0.93)	0.25 (0.98)	1.22 (0.93)	1.43 (0.98)
Control Mean	48.9	48.9	51.7	51.7
N	57,000	430,000	57,000	430,000
Using BW	120	120	120	120

Notes. Appendix Table 7 summarizes treatment effect estimates on parents' employment by sex and marital status. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 for tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Table 8: Treatment effects on employment among parents by sex and family status within low-income sample

	Any Wages		Any Emp	
	(1)	(2)	(3)	(4)
Panel A: Single female parents, Effective tax year				
Treatment	0.09 (1.22)	0.55 (1.37)	-0.55 (0.87)	0.93 (1.47)
Control Mean	74.5	78.2	86.9	88.4
N	36,500	330,000	40,500	220,000
Optimal BW	44	40	48	29
Panel B: Single female parents, Subsequent tax year				
Treatment	-0.71 (1.40)	-0.71 (1.42)	-0.41 (1.06)	-0.77 (1.22)
Control Mean	73.7	77.2	87.0	88.9
N	31,000	320,000	31,000	280,000
Optimal BW	39	39	39	35
Panel C: Single male parents, Effective tax year				
Treatment	0.09 (1.22)	0.55 (1.37)	-0.55 (0.87)	0.93 (1.47)
Control Mean	74.5	78.2	86.9	88.4
N	36,500	330,000	40,500	220,000
Optimal BW	44	40	48	29
Panel D: Single male parents, Subsequent tax year				
Treatment	2.14 (2.24)	4.28** (1.74)	3.96** (1.54)	6.12*** (2.21)
Control Mean	56.9	60.2	75.2	77.2
N	14,500	180,000	20,000	110,000
Optimal BW	41	54	52	37
All States		X		X
Covariates		X		X

Notes. Appendix Table 8 summarizes treatment effect estimates on parents' employment and earnings by marital status. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

Source. IRS 1040 and W-2 for tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

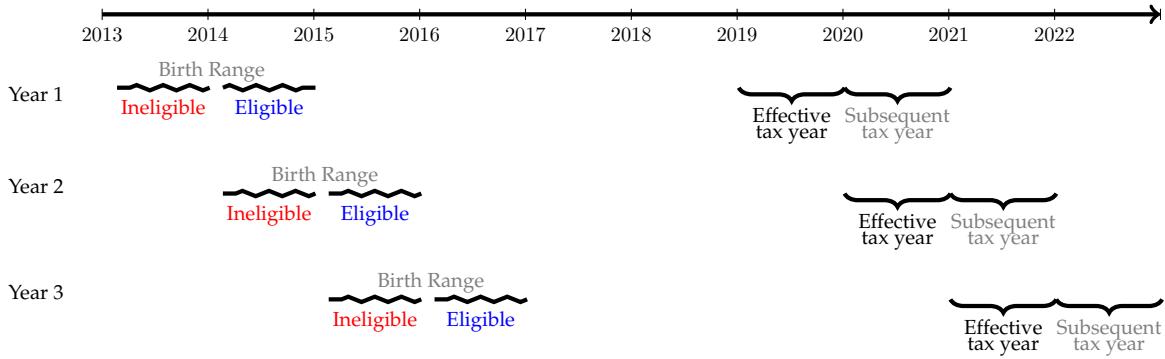
Appendix Table 9: Treatment effects on parents' W-2 employment by sex and marital status, full sample

	Effective tax year		Subsequent tax year	
	(1) CA Sample	(2) National Sample	(3) CA Sample	(4) National Sample
Panel A: Single female parents				
Treatment	0.34 (0.38)	0.22 (0.40)	0.01 (0.40)	-0.11 (0.41)
Control Mean	83.5	83.5	82.3	82.3
N	180,000	1,717,000	180,000	1,717,000
Using BW	120	120	120	120
Panel B: Single male parents				
Treatment	0.48 (0.56)	0.50 (0.58)	0.30 (0.57)	0.42 (0.59)
Control Mean	78.4	78.4	77.1	77.1
N	110,000	880,000	110,000	880,000
Using BW	120	120	120	120
Panel C: Married female parents				
Treatment	0.13 (0.41)	0.31 (0.43)	-0.18 (0.40)	-0.08 (0.43)
Control Mean	64.0	64.0	65.2	65.2
N	280,000	2,285,000	280,000	2,285,000
Using BW	120	120	120	120
Panel D: Married male parents				
Treatment	-0.41 (0.31)	-0.29 (0.33)	0.05 (0.31)	0.13 (0.33)
Control Mean	83.6	83.6	83.5	83.5
N	280,000	2,282,000	280,000	2,282,000
Using BW	120	120	120	120

Notes. Appendix Table 9 summarizes treatment effect estimates on parents' employment by sex and marital status. All counts and estimates are rounded per Census's disclosure rules governing administrative records. Heteroskedasticity-robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p < 0.01.

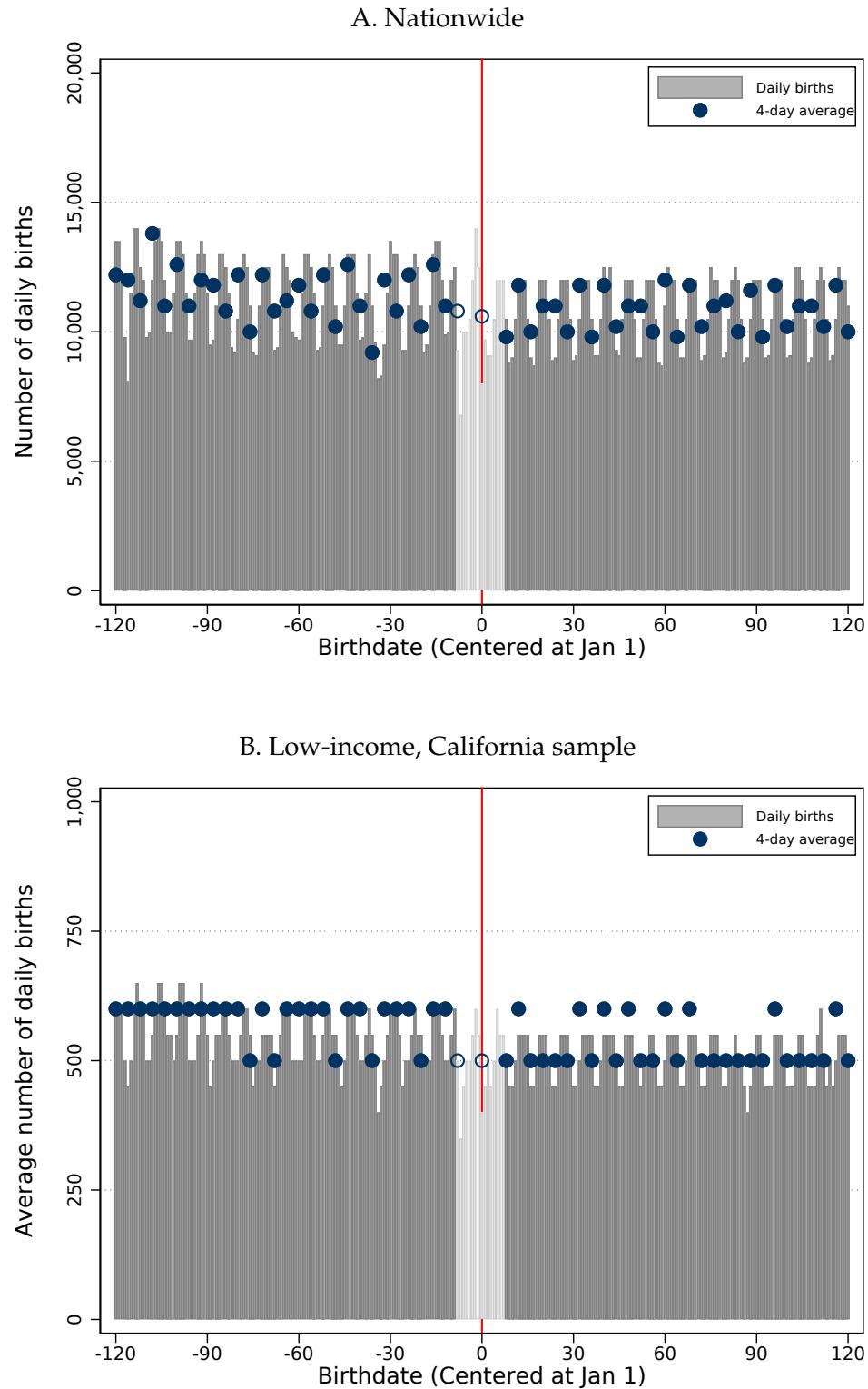
Source. IRS 1040 and W-2 for tax years 2013 through 2022, SSA Numident, Census Household Composition Key.

Appendix Figure 1: Birth ranges for eligible and near-eligible children and relevant tax years



Notes. Appendix Figure 1 illustrates the birth windows from which I construct my sample, and the effective and subsequent tax years for each birth cohort. The first birth cohort, for example, is comprised of children born at the end of 2013 and start of 2014, since these children turned six at the end of 2019, meaning they were ineligible for the YCTC in 2019, or the start of 2020, meaning they were still five at the end of 2019 and were eligible. The effective tax or policy year for this cohort is 2019, and their subsequent tax year is 2020. The birth cohorts are distinct, meaning each child and parent is eligible or near eligible with respect to a single policy year and only appears in my sample once.

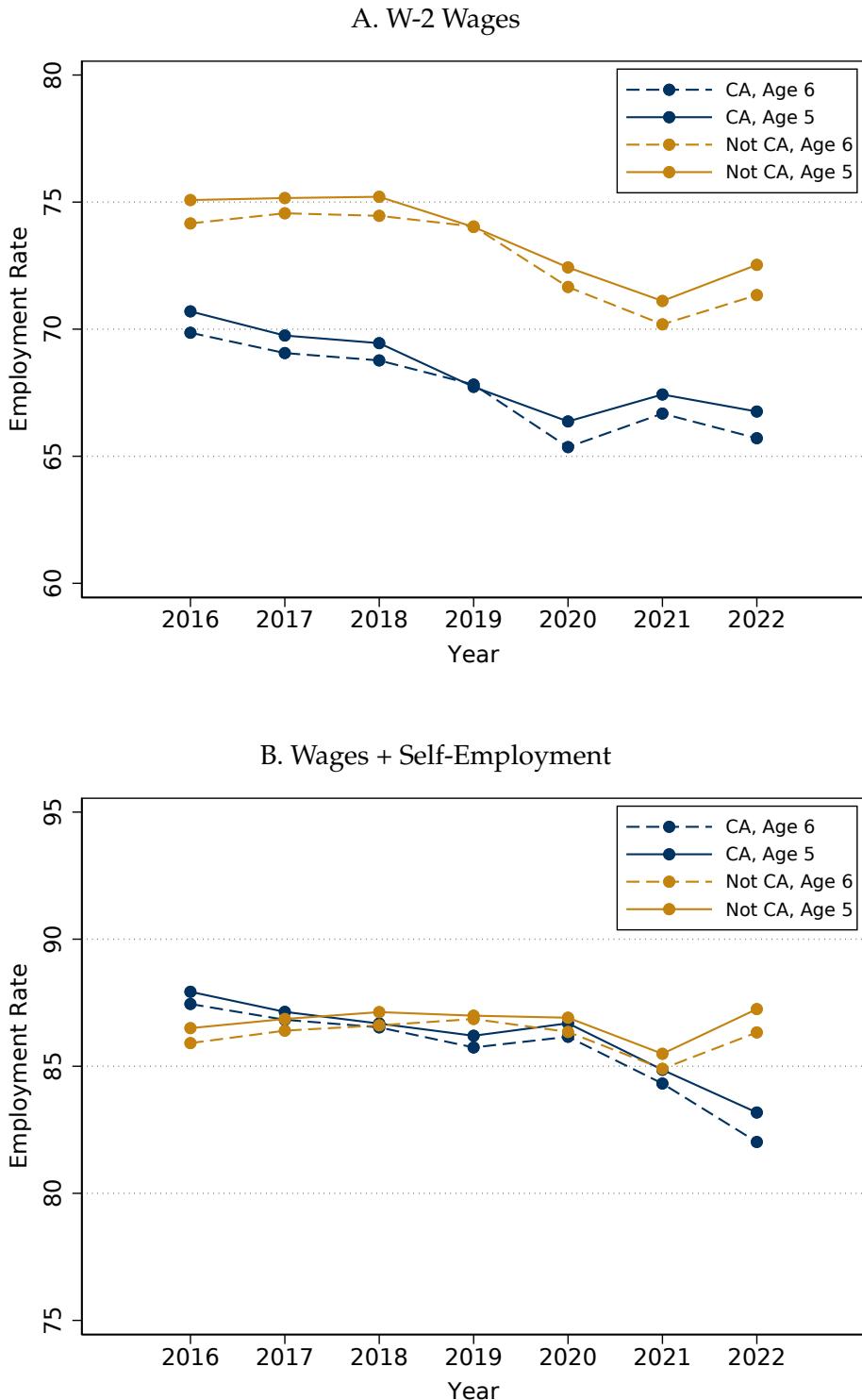
Appendix Figure 2: Distribution of average daily births



Notes. Appendix Figure 2 plots the average number of births each day within 120 days of January 1 between 2020 and 2022, as well as the average number of daily births within four day birthdate bins in the full, national sample (Panel A) and the low-income, California sample (Panel B). Counts and averages are rounded per Census Bureau's disclosure rules.

Source. SSA Numident, IRS 1040 and W-2 records, Census Household Composition Key.

Appendix Figure 3: Average household-level employment rates by age eligibility in California and national samples between 2016 and 2021

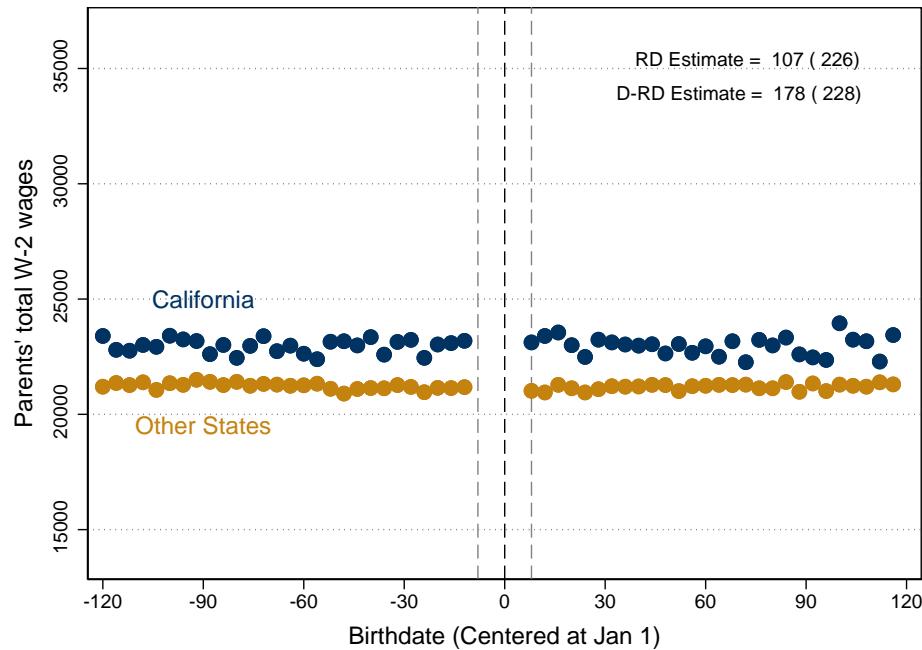


Notes. Appendix Figure 3 plots the share of parents who were employed in each tax year according to two employment measures between 2016 and 2021 by whether they resided in California and their youngest child was five or six at the end of the year. YCTC policy was not in effect for tax years before 2019.

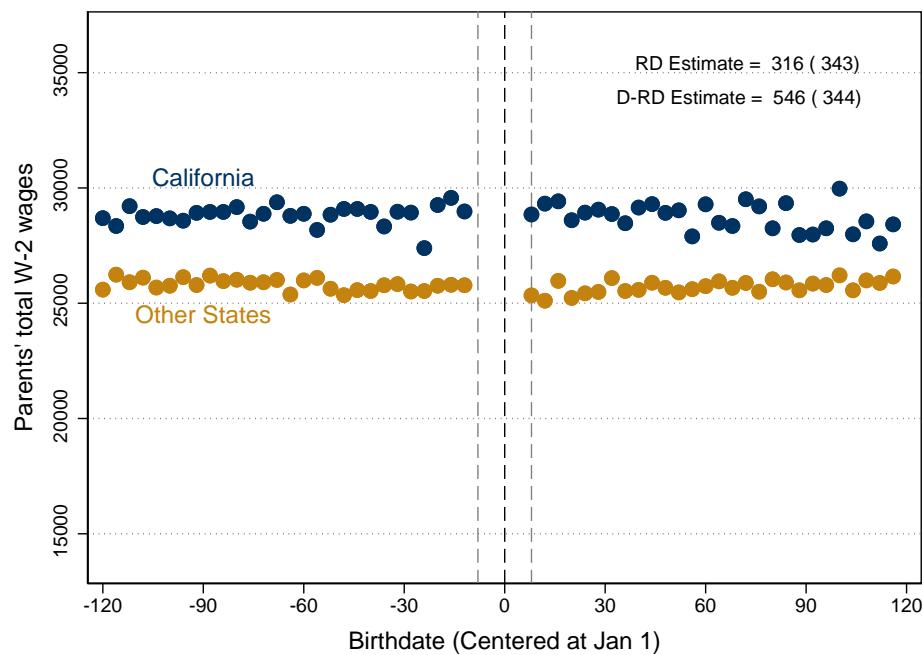
Source. SSA Numident, IRS 1040 and W-2 records, Census Household Composition Key.

Appendix Figure 4: Estimated effect of birthdate on parents' W-2 wages in low-income sample

A. Effective Year



B. Subsequent Year

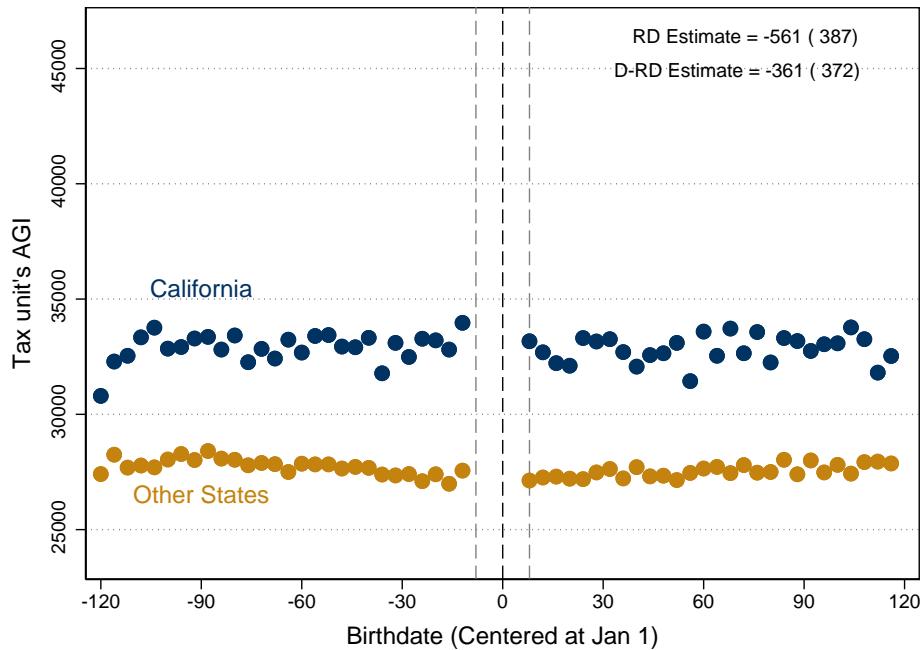


Notes. Appendix Figure 4 plots the average total W-2 wages for parents of children within four-day birthdate bins residing in (blue dots) or outside (gold dots) of California in the effective tax year (Panel A) and the subsequent tax year (Panel B), averaged across the three policy years. The dotted vertical lines indicate the eight-day donut excluded from all estimations. Estimates of the discontinuities are included in the upper-right hand corner, and correspond to the estimates reported in Table 4. All estimates are rounded per the Census Bureau's disclosure rules.

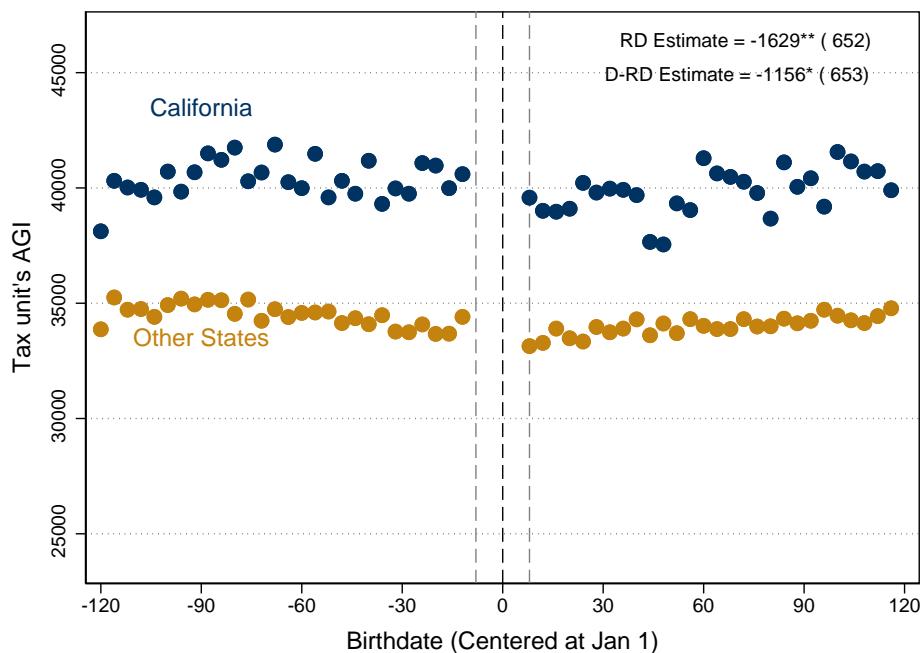
Sources. IRS 1040 and W-2 returns, tax years 2018-2022; SSA Numident; Census Household Composition Key.

Appendix Figure 5: Estimated effect of birthdate on parents' AGI in low-income sample

A. Effective Year



B. Subsequent Year

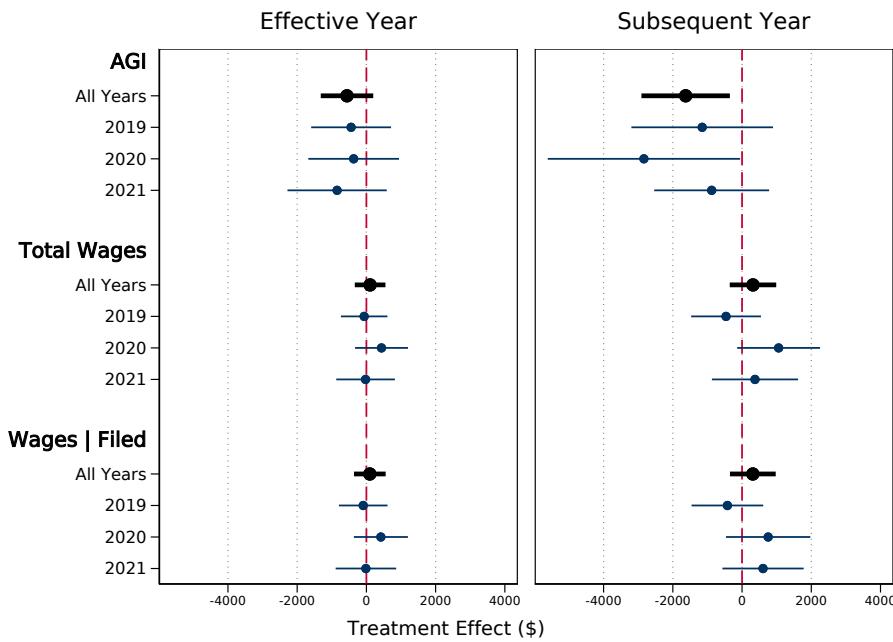


Notes. Appendix Figure 5 plots the average AGI for parents of children within four-day birthdate bins residing in (blue dots) or outside (gold dots) of California in the effective tax year (Panel A) and the subsequent tax year (Panel B), averaged across the three policy years. The dotted vertical lines indicate the eight-day donut excluded from all estimations. Estimates of the discontinuities are included in the upper-right hand corner, and correspond to the estimates reported in Table 4. All estimates are rounded per the Census Bureau's disclosure rules.

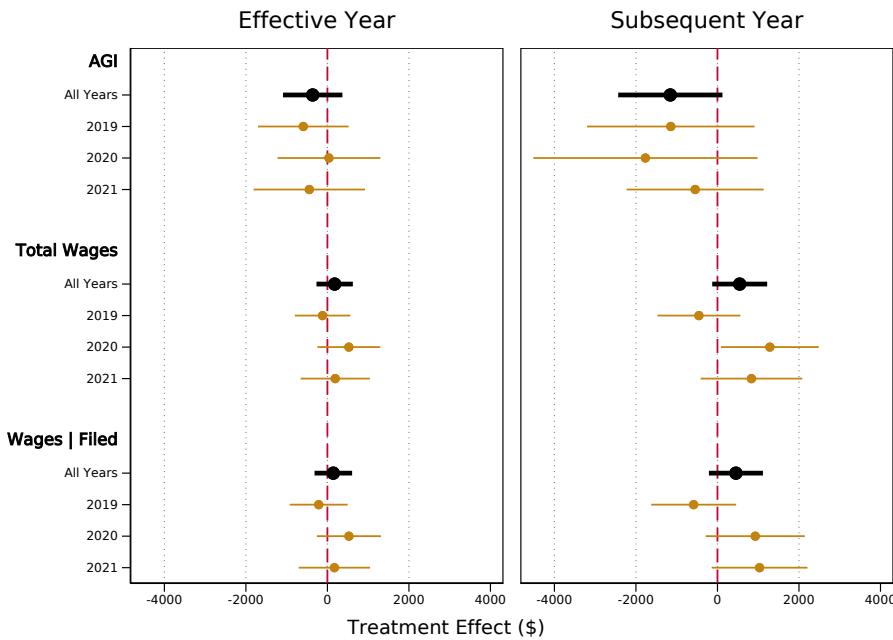
Sources. IRS 1040 and W-2 returns, tax years 2018-2022; SSA Numident; Census Household Composition Key.

Appendix Figure 6: Treatment effect estimates for intensive margin outcomes by tax year

A. California sample



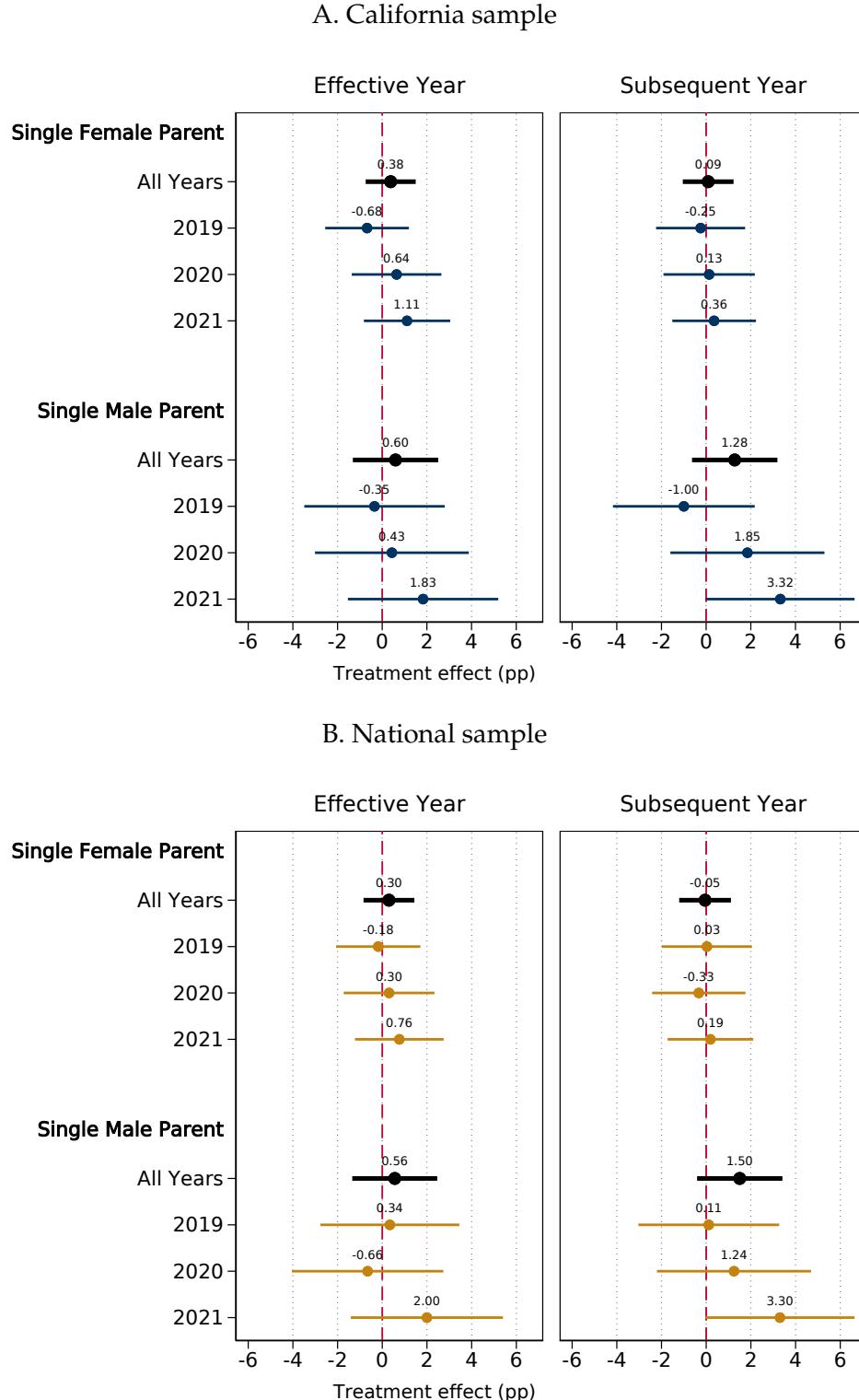
B. National sample



Notes. Appendix Figure 6 plots treatment effect estimates for intensive margin outcomes overall and by tax year from the California sample (Equation 1) and national sample (Equation 2).

Sources. IRS 1040 and W-2 returns, tax years 2018 to 2022; SSA Numident; Census Household Composition Key.

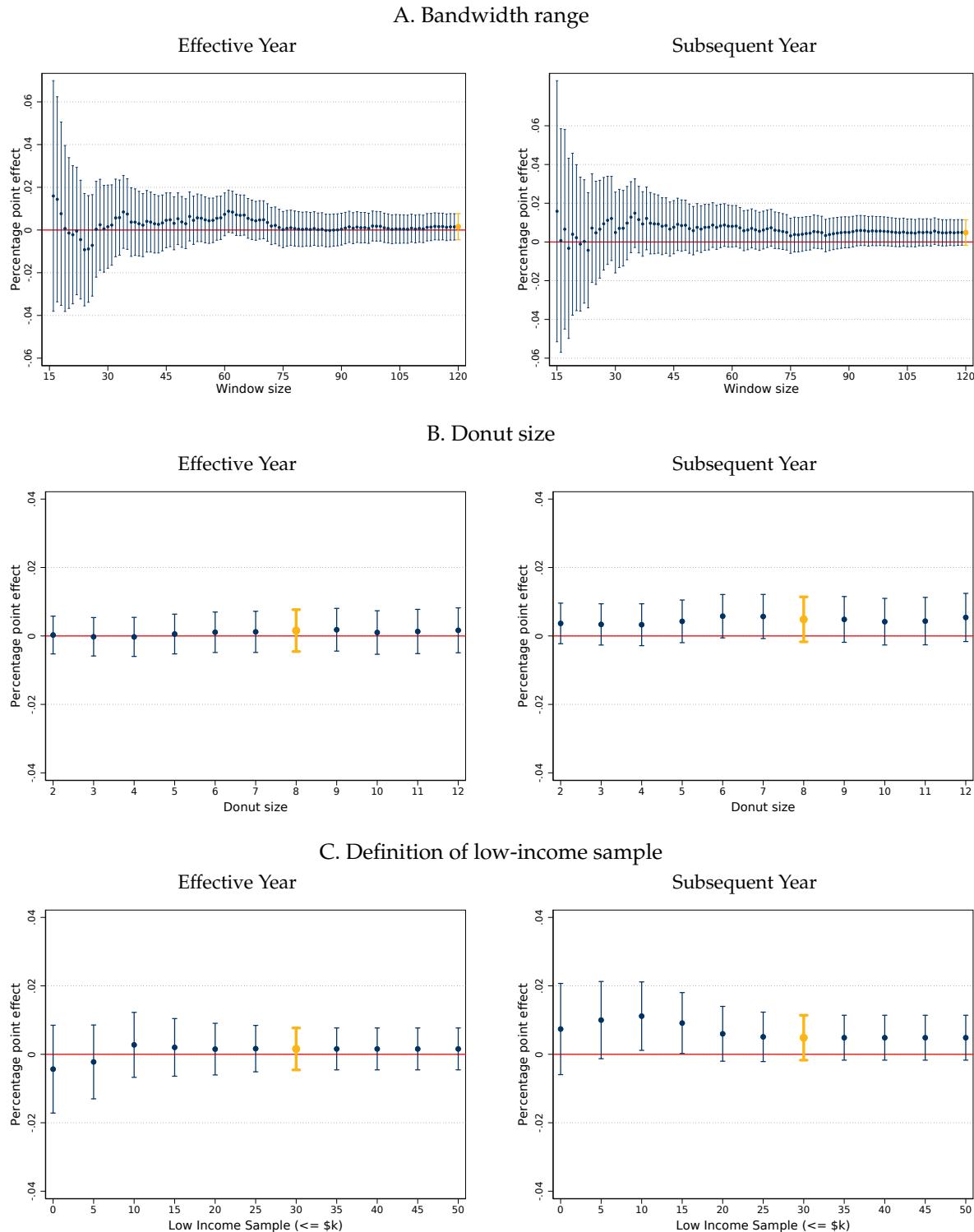
Appendix Figure 7: Treatment effect estimates for wage employment by parent sex and tax year



Notes. Appendix Figure 7 plots treatment effect estimates for employment outcomes by parent sex and policy year from the California sample (Equation 1) and national sample (Equation 2).

Sources. IRS 1040 and W-2 returns, tax years 2019 to 2022; SSA Numident; Census Household Composition Key.

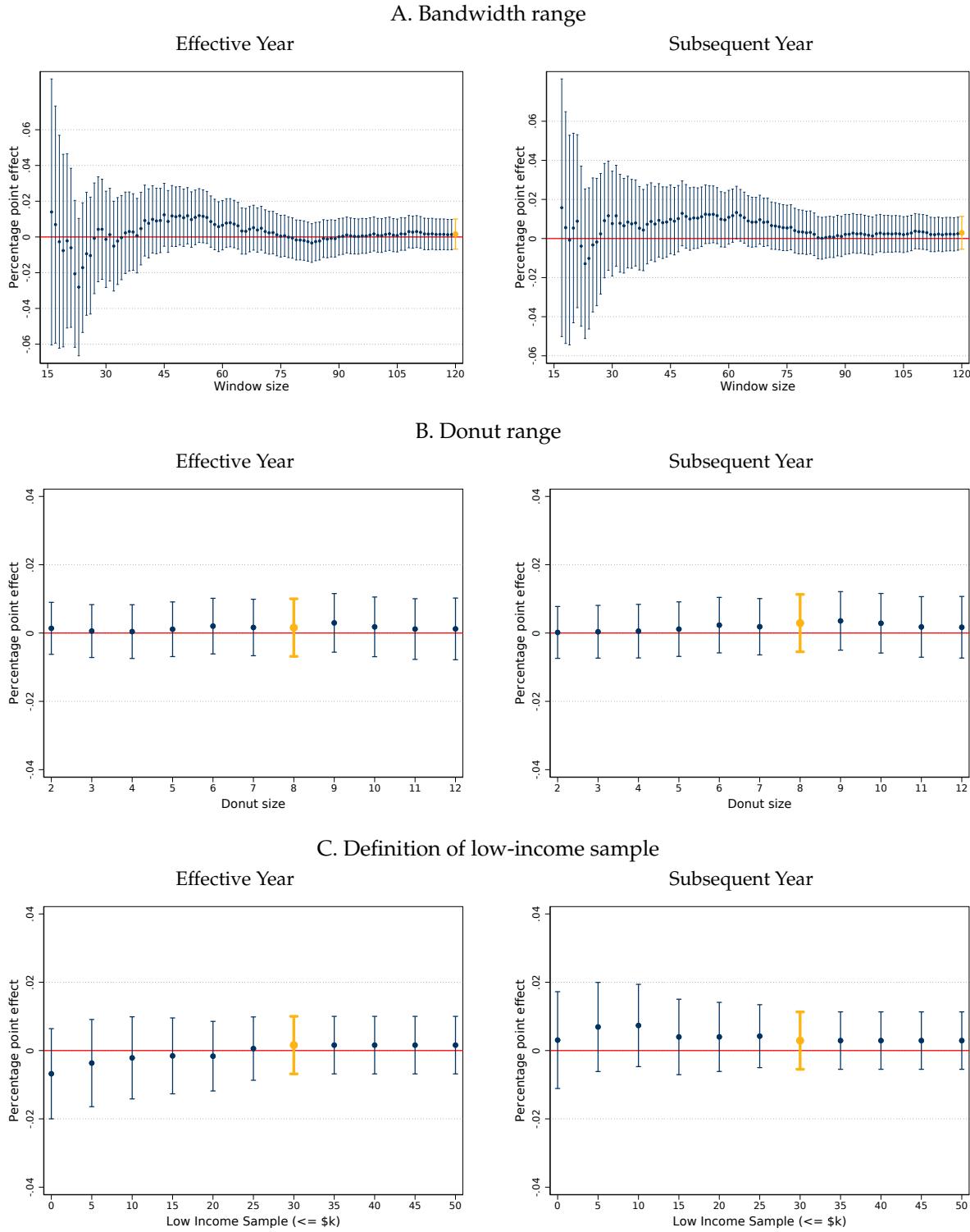
Appendix Figure 8: Estimated effects on claiming rate from standard RD and California sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 8 plots treatment effect estimates on the claiming rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the claiming rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

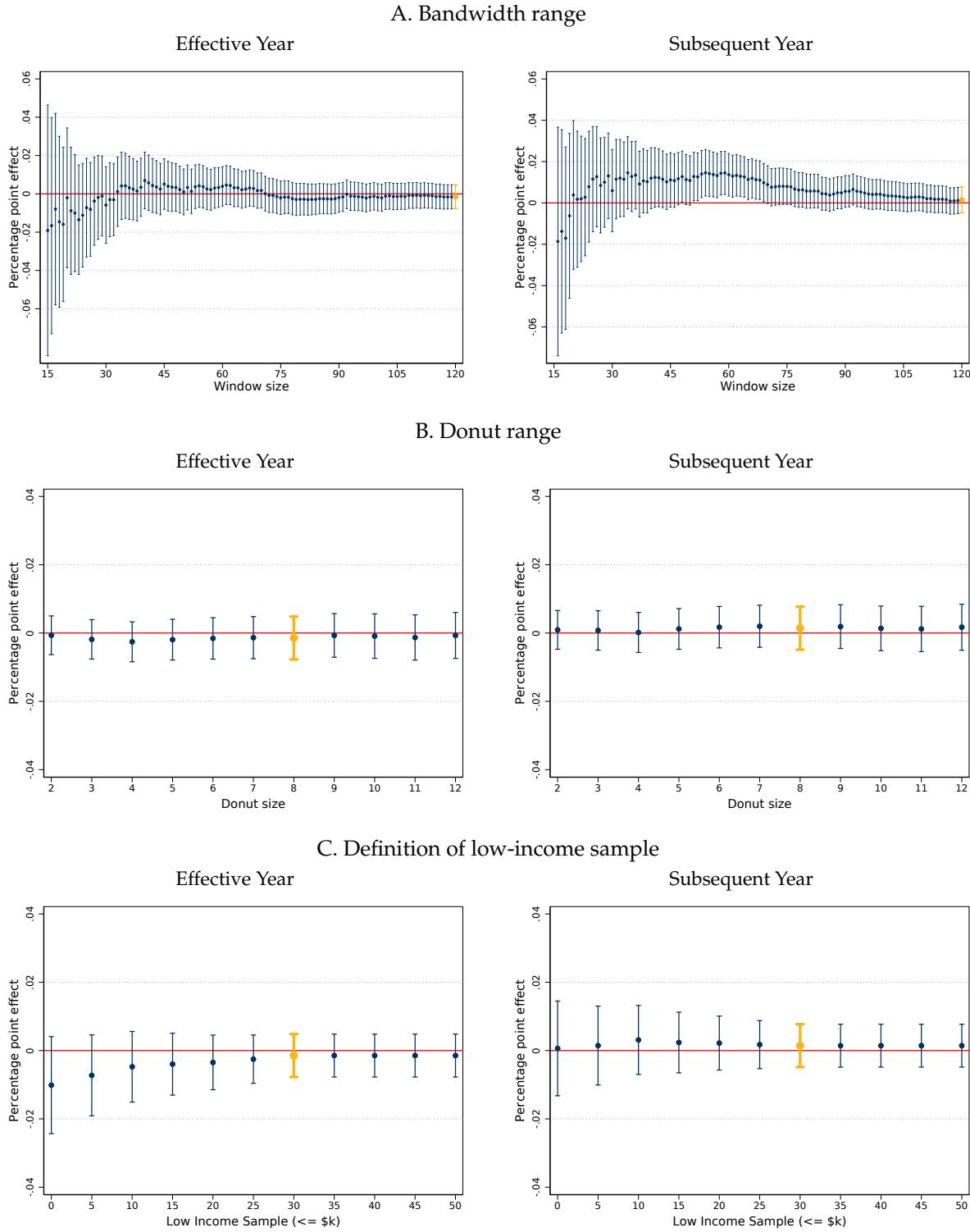
Appendix Figure 9: Estimated effects on parents' W-2 employment rate from standard RD and California sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 9 plots treatment effect estimates on the parents' W2 employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 1 of Table 1. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the W2 employment rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

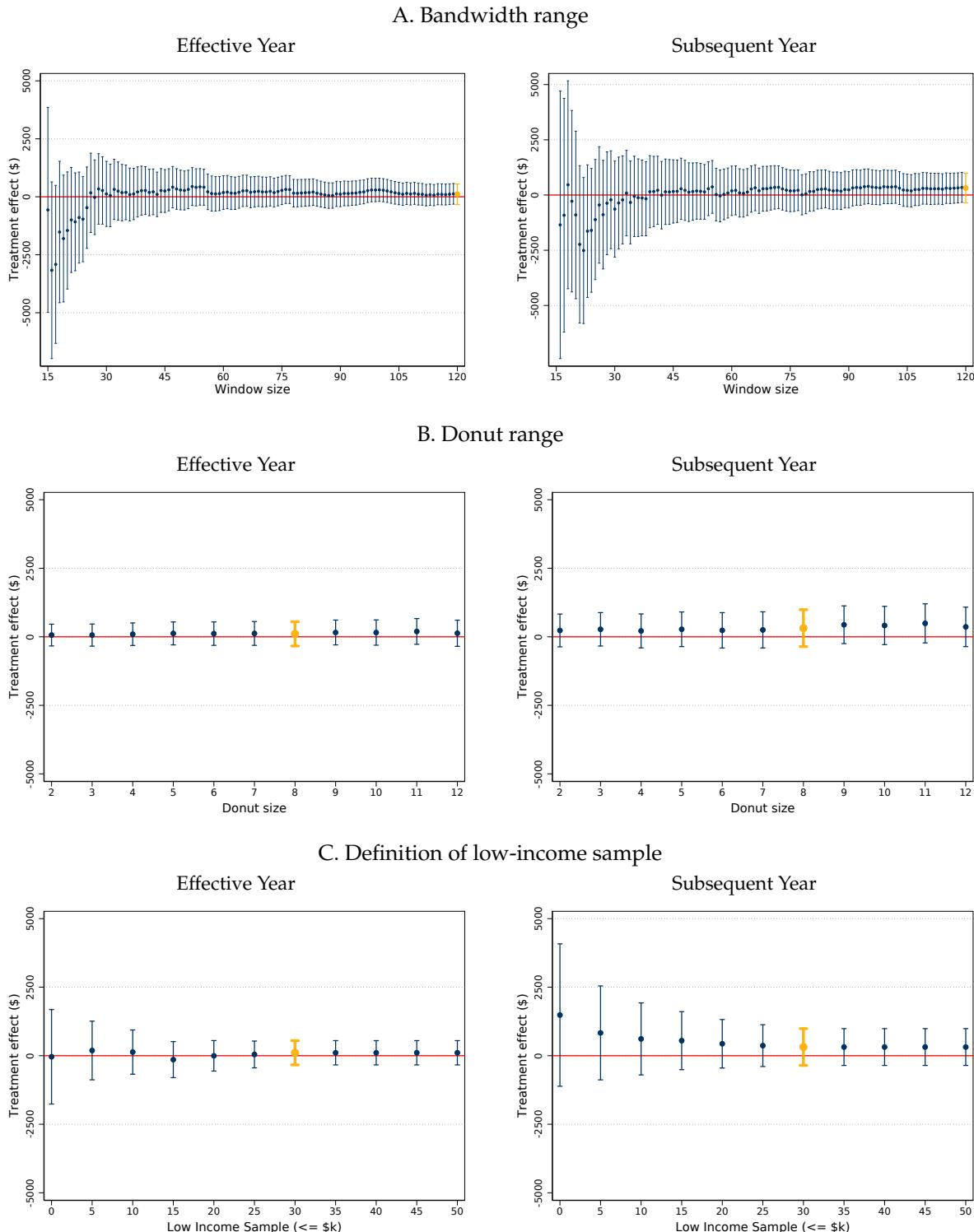
Appendix Figure 10: Estimated effects on parents' W-2 and/or self-employment rate from standard RD and California sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 10 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 1 of Table 1. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the W2 or self-employment rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident, NCensus Household Composition Key.

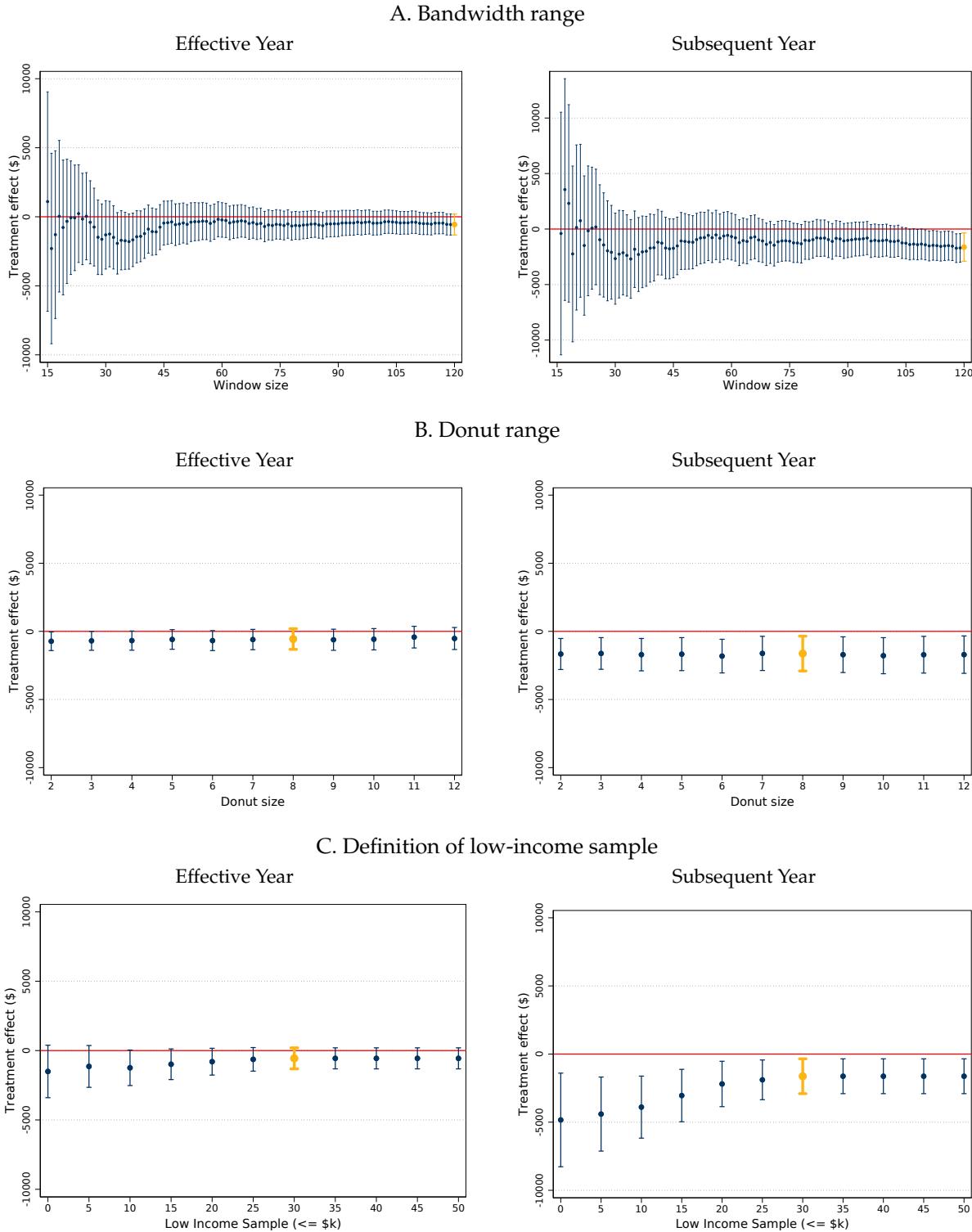
Appendix Figure 11: Estimated effects on W-2 wages from standard RD and California sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 11 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 1 of Table 4. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for total W2 wages in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

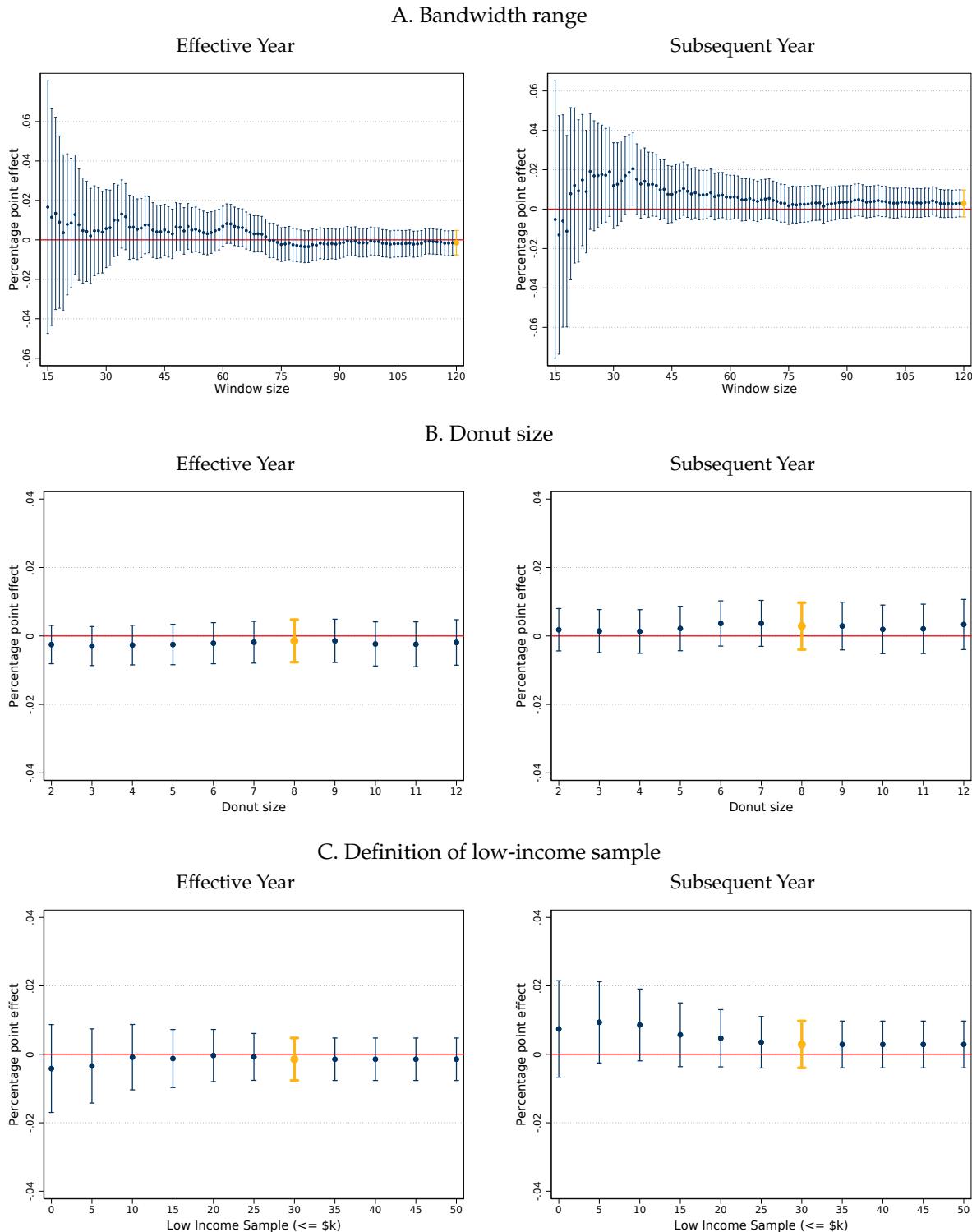
Appendix Figure 12: Estimated treatment effects on AGI from standard RD and California sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 12 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 1 of Table 4. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for AGI in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

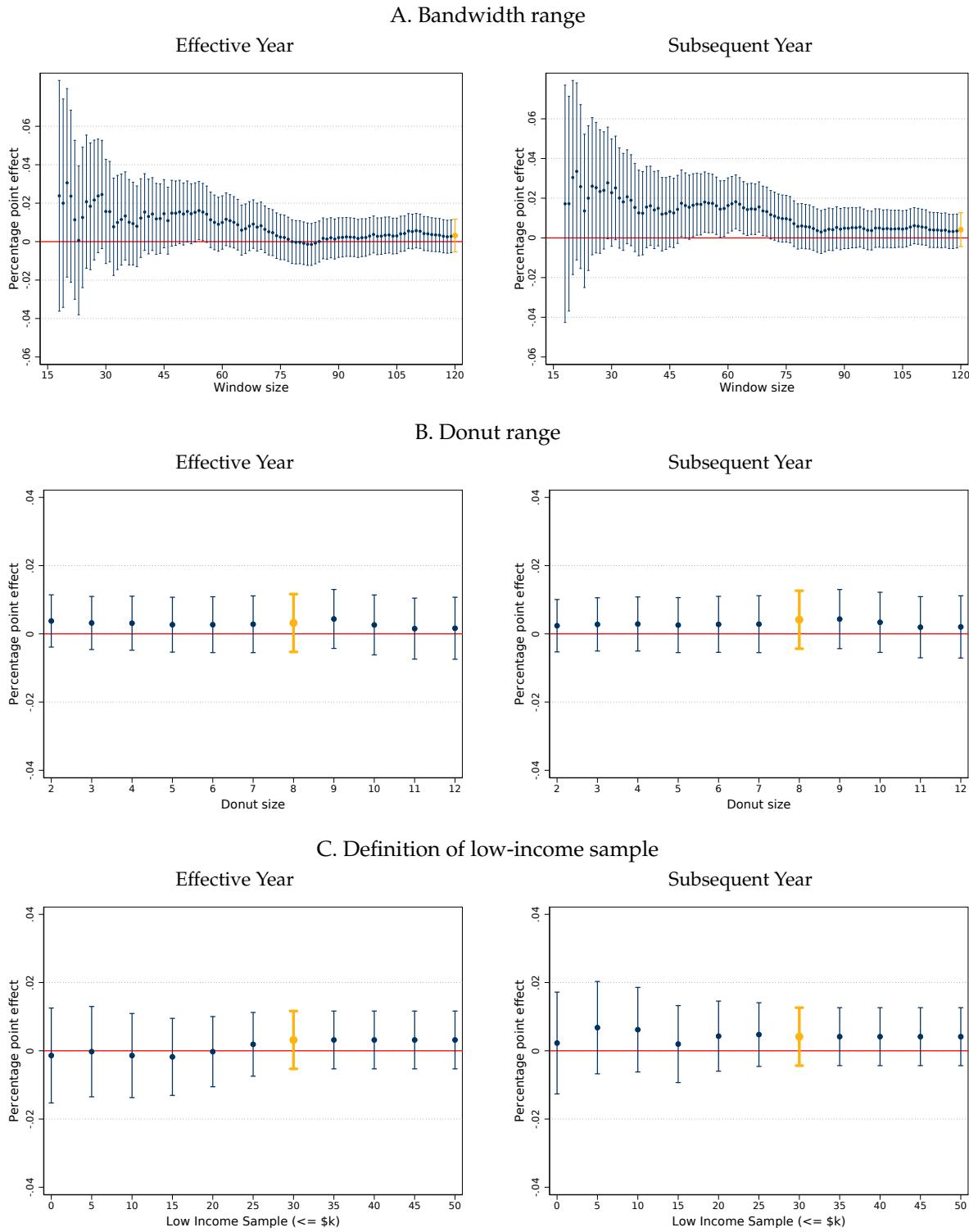
Appendix Figure 13: Estimated treatment effects on claiming rate using D-RD and national sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 13 plots treatment effect estimates on the claiming rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the claiming rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

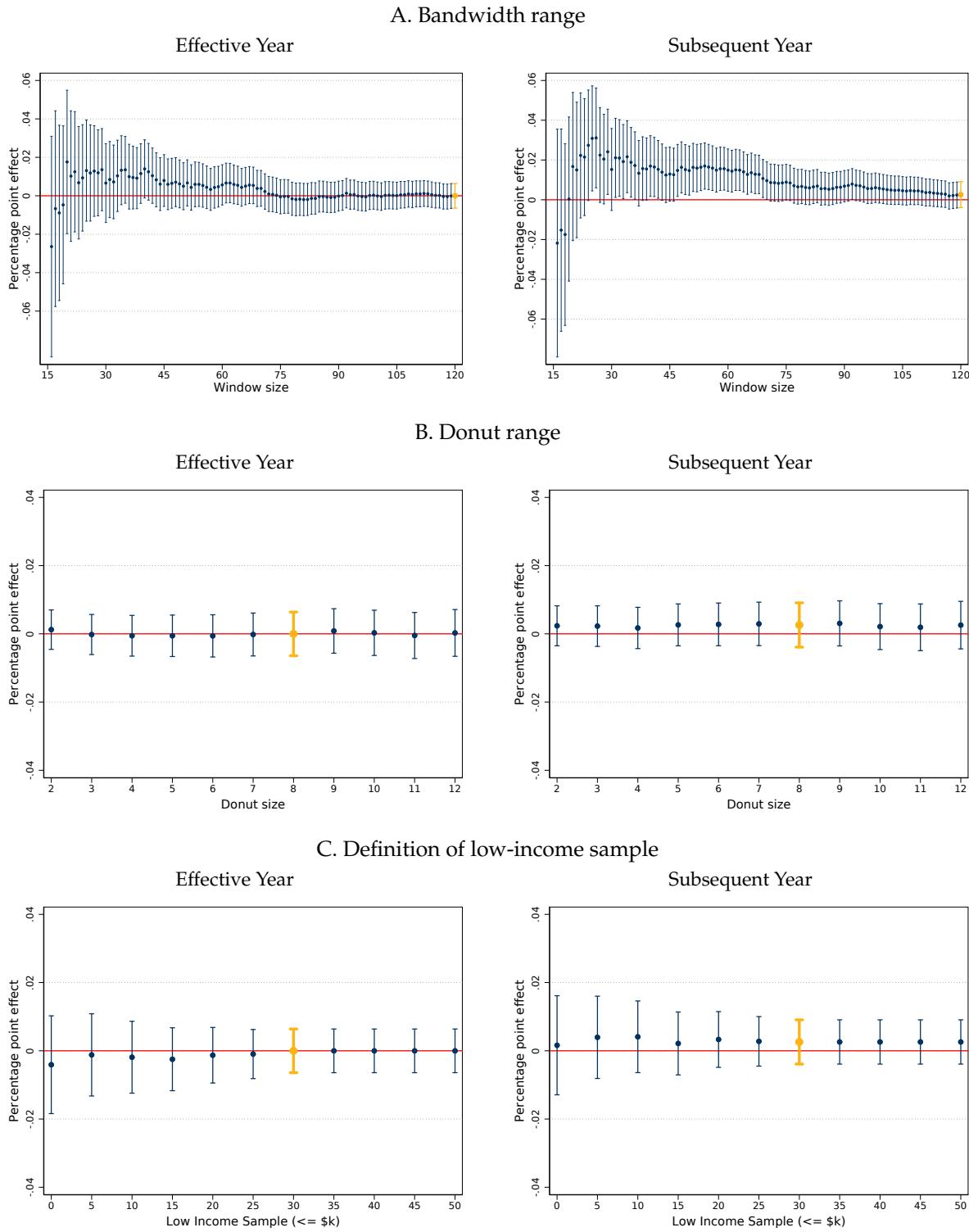
Appendix Figure 14: Estimated effects on parents' W-2 employment rate using D-RD and national sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 14 plots treatment effect estimates on the parents' W2 employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 2 of Table 1. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the W2 employment rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numidex; Census Household Composition Key.

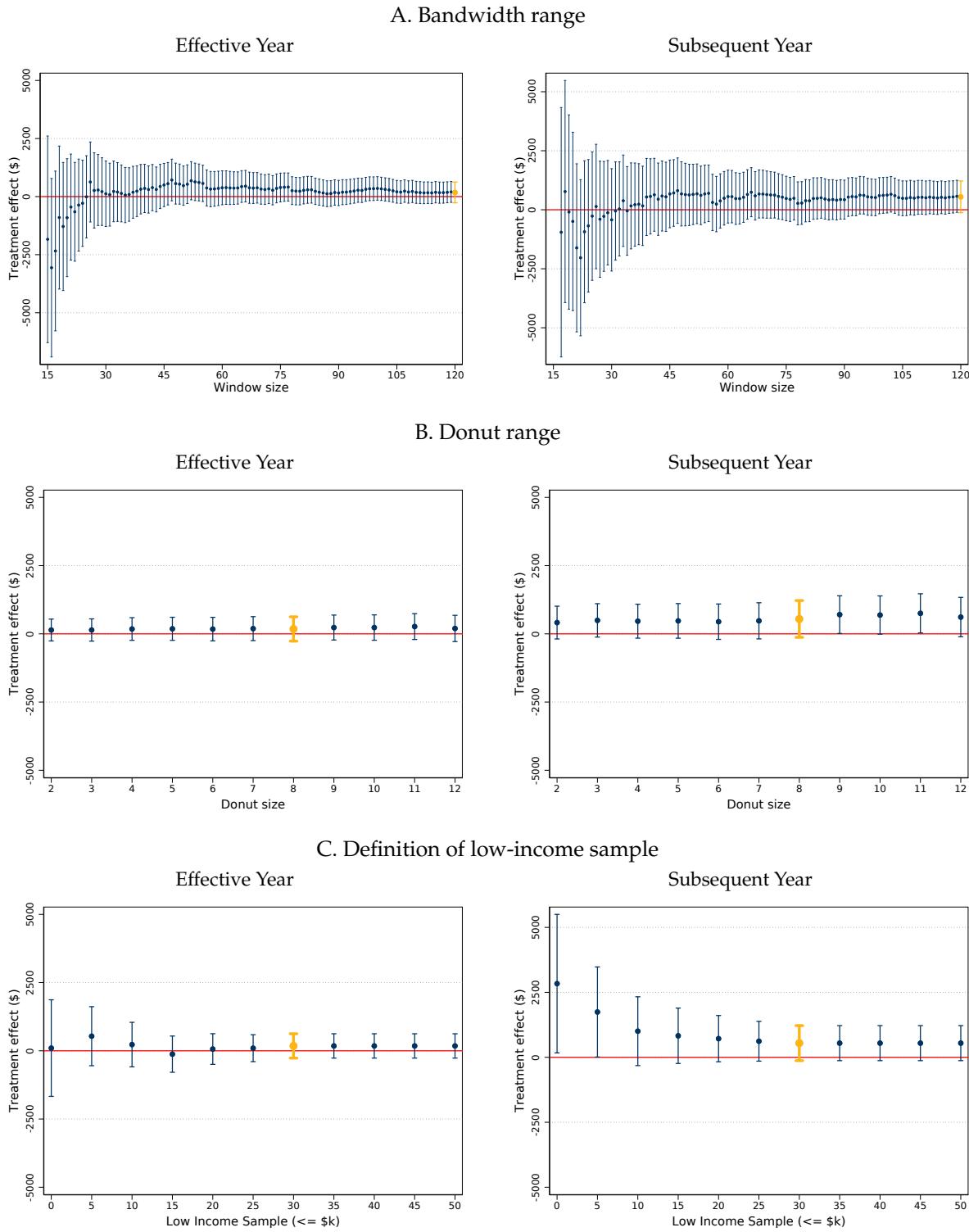
Appendix Figure 15: Estimated effects on parents' W-2 and/or self-employment rate using D-RD and national sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 15 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 2 of Table 1. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for the W2 or self-employment rate in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numidex Census Household Composition Key.

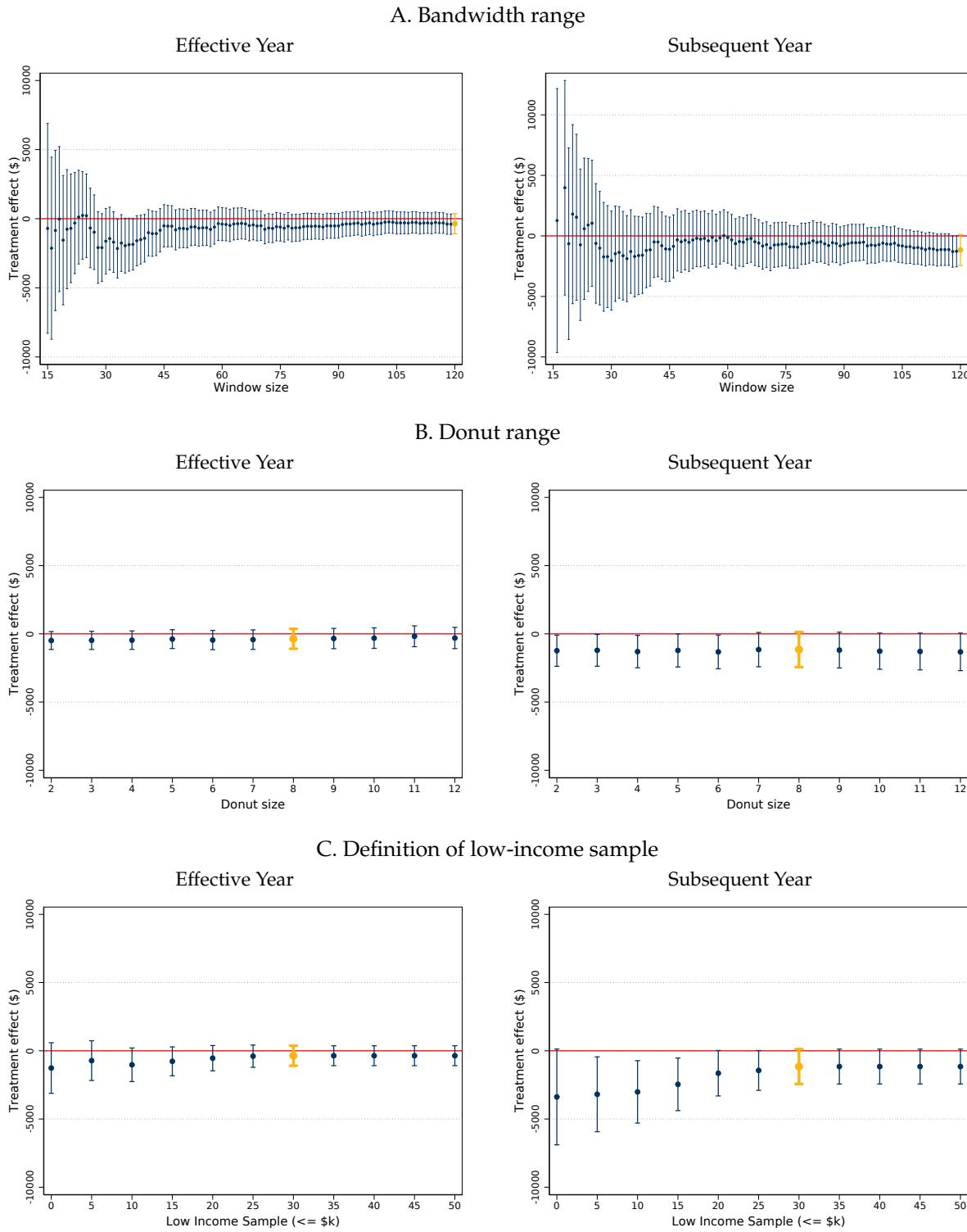
Appendix Figure 16: Estimated treatment effects on parents' W-2 wages using D-RD and national sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 16 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 2 of Table 4. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for total W2 wages in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.

Appendix Figure 17: Estimated treatment effects on parents' AGI using D-RD and national sample by bandwidth range, donut size, and low-income definition



Notes. Appendix Figure 17 plots treatment effect estimates on the parents' W2 or self-employment rate using Equation 1, the standard regression discontinuity design and limited to the sample of low-income California children. The preferred estimate is in Column 2 of Table 4. Here, I vary the bandwidth selection (Panel A), donut size (Panel B), and definition of low-income status (Panel C). The left and right columns represent treatment effect estimates for AGI in the effective and subsequent tax year, respectively.

Sources. IRS 1040 and W-2 returns, TYs 2019-2022; SSA Numident; Census Household Composition Key.